Title
Essays on the Economics of Crime

Author
Rozo Villarraga, Sandra V.

Publication Date
2015

Peer reviewed|Thesis/dissertation

University of California
Los Angeles

Essays on the Economics of Crime

A dissertation submitted in partial satisfaction
of the requirements for the degree
Doctor of Philosophy in Economics

by

Sandra Viviana Rozo Villarraga

2015

Abstract of the Dissertation

Essays on the Economics of Crime

by

Sandra Viviana Rozo Villarraga

Doctor of Philosophy in Economics

University of California, Los Angeles, 2015

Professor Adriana Lleras-Muney, Chair

These essays contribute towards our understanding of the consequences of illegal behavior on economic outcomes and of the role of public policy in addressing and containing those consequences more effectively. This dissertation is composed of three chapters. **Chapter 1 – Is murder bad for business and real income? The effects of violent crime on local economic activity**: studies the channels through which violence (measured by the homicide rate) impacts economic outcomes, and thus whether investments in violence reduction have significant economic returns. I estimate the effects of violent crime on local wages, prices, and production using unique firm-level panel data and rich information on consumer prices in Colombia. To estimate causal effects, I exploit exogenous reductions in violent crime driven by U.S. international anti-drug expenditures; these resulted in greater violence reductions in municipalities with higher political competition (namely closely contested elections) in the past. I find that higher homicide rates lower housing rents and increase prices. Wages also increase, but only for white-collar workers. Putting all these forces together, real wages fall for both types of worker, but more so for blue-collar workers. These estimates, in combination with a theoretical model, allow me to compute that when homicide rates increase 10%, white- and blue-collar workers' welfare (measured as utility of consumption) is reduced 2.8% and 6.3%, respectively. Consequently, violent crime increases in-

equality as measured by real incomes or by welfare. Aggregate production also falls 2.1%, mostly because firms reduce production, although there is also a small decrease in the number of firms.

Chapter 2 – On the consequences of enforcement on illegal drug production: investigates the effects of the biggest antidrug program ever applied in a drug producing country. I use satellite information on the exact location of coca crops between 2000 and 2010 in Colombia to identify the effects of spraying herbicides on coca production. I exploit the variation created by restrictions to spraying in protected areas (i.e., indigenous territories and natural parks) and the time variation of U.S. international antidrug expenditures to identify the effects of the program. My results suggest that coca cultivation is reduced by 0.07 hectares per additional hectare sprayed. However, spraying induces unintended negative effects on the welfare conditions of the treated areas and spillover effects in neighboring countries. Despite the reduction under coca cultivation, cocaine production remains steady due to a sharp increase on cocaine yields. In sum, the program's costs are by far higher than its potential benefits.

Chapter 3 – On the effects of enforcement on illegal markets: Evidence from a quasi-experiment: studies the effects of enforcement on illegal behavior in the context of a large aerial spraying program designed to curb coca cultivation in Colombia. In 2006, Colombia pledged not to spray a 10km band around the frontier with Ecuador due to diplomatic frictions. We exploit this variation to estimate the effect of spraying on cultivation by regression discontinuity around the 10 km threshold and conditional differences in differences, using satellite data. Our results suggest that spraying one hectare reduces coca cultivation by 0.018 to 0.034 hectares, but these effects are too small to make spraying a cost-effective policy.

The dissertation of Sandra Viviana Rozo Villarraga is approved.

Maria Casanova

Paola Giuliano

Leah Boustan

Adriana Lleras-Muney, Committee Chair

University of California, Los Angeles

2015

To Fer, my rock and world. Thank you for your unwavering love, encouragement, patience, and support.

Table of Contents

List of Figures

List of Tables

Acknowledgments

I am particularly indebted to my dissertation chair, Adriana Lleras-Muney, for her invaluable support and guidance through my graduate studies. I am also extremely grateful to Leah Boustan, Aprajit Mahajan, Till von Wachter, Walker Hanlon, Paola Guiliano, and Maria Casanova for all their feedback and immense support during my studies and in my different research projects. I also thank the United Nations Office on Drugs and Crime in Bogota and the *Departamento Administrativo Nacional de Estadística* in Colombia [Colombian Statistics Department] for providing the data for my research projects. I also thank Daniel Mejia and Pascual Restrepo my co-authors of the last chapter of this dissertation. This chapter is reprinted here with their permission.

Vita

Education

2011-2015 Doctoral Student, University of California, Los Angeles.

2010-2011 M.A., Economics, University of California, Los Angeles.

2007-2008 M.A., Economics, Universidad de los Andes. *Cum Laude.*

2002-2007 B.A., Economics, Universidad de los Andes. *Magna Cum Laude.*

Professional Experience

2012 Summer Junior Researcher, CESED, Universidad de los Andes.

2011 Summer/Winter Consultant, Inter-American Development Bank.

2008-2010 Research Fellow, Inter-American Development Bank.

2007 Junior Research Economist, Fedesarrollo.

2006 Junior Analyst, Colombian Central Bank.

Honors, Scholarships and Fellowships

2014-2015 UCLA Dissertation Year Fellowship.

2013 UCLA Welton Prize for Best Graduate Student Paper.

2012-2013 UCLA Award for Best Teaching Assistant.

2013 ISSDP Award for Best Presentation of an Early Career Scholar.

2011-2013 UCLA Economics Department TA Fellowship.

2012 UCLA. Honors Field Comprehensive Exams.

2010 Colombian Central Bank Lauchlin Currie Scholarship.

2007 Portafolio, El Tiempo. Award to Best Student in Economics.

2007 Universidad de los Andes. M. A. Academic Excellence Scholarship.

2002/2004 Universidad de los Andes. B. A. Academic Excellence Scholarship

CHAPTER 1

Is Murder Bad for Business and Real Income? The Effects of Violent Crime on Economic Activity

1.1 Introduction

Violence remains a development challenge today. Every year, approximately 11% of global GDP is spent to address and contain violence (IEP, 2013).[1] Despite the fact that more than 65% of these resources are spend in developing countries, the most violent countries and regions, based on the homicide rate,[2] are also the poorest and most unequal, as shown in Figure 1.1. Violent crime not only imposes direct costs on society through mortality,[3] but also induces indirect economic costs by distorting workers' and firms' decisions. These distortions are reflected in market prices and market size, and ultimately affect consumer welfare. However, with the exception of housing prices and GDP,[4] there is limited evidence on the

[1]Of these amount 51% are accounted for by military expenditures.

[2]Violent deaths per 100,000 inhabitants are the most consistent measure of violent crime available over time and space. In 2012, Southern Africa and Central America were the sub-regions with the highest homicide rates on record with averages over 25 victims per 100,000 inhabitants, followed by South America, Middle Africa, and the Caribbean with average rates between 16 and 23 homicides per 100,000 inhabitants.

[3]See Soares (2006) for a review of the mortality costs of violence.

[4]A large group of studies uses hedonic pricing models to identify the effects of urban crime on property prices–e.g., Thaler (1978), Hellmand and Naroff (1979), Lynch and Rasmussen (2001), Bowes and Ihlanfeldt (2001), Gibbons (2004), and Linden and Rockoff (2008). They find a negative elasticity of housing prices with respect to urban crime. See Appendix B for a detailed list of the point estimates of these studies. Another group of literature investigates the effects of violence on aggregate economic activity. Cross country studies find negative effects of violence on economic activity (e.g., Organski and Kugler, 1977, Alesina and Perotti, 1996, Collier, 1999,

effects of violent crime on local markets, and no work that investigates its effects on inequality.

This paper estimates the effects of violent crime on market prices (non-housing prices, wages, and housing rents) and market size (average production and firm exit) by examining unique firm-level and rich consumer pricing data matched to homicide rates in Colombia. With the exception of the elasticities of housing rents to violent crime,[5] these elasticities have not been identified before.[6] It also investigates the extent to which homicide rates have heterogeneous effects, specifically whether it affects high- and low-skilled workers equally (blue- and white-collar). This facilitates the characterization of the types of agents who are more vulnerable to violent crime and the analysis of the effects of violent crime in inequality.

To estimate causal effects of violent crime, I make use of the large reductions in violence caused by large U.S. transfers (measured through the intensity of the U.S. international anti-drug expenditures) sent in the late 1990s to improve security conditions in Colombia. These expenditures were disbursed across areas based on population alone, but they affected municipalities differently depending on the original location of illegally armed groups. According to most historical accounts the illegal armed groups were originally located in areas that had high political competition through an episode known as *La Violencia* (1948-1958).[7]

Imai and Weinstein, 2000, Murdoch and Sandler, 2004, Hoeffler and Reynal- Querol, 2003, Blomberg and Mody, 2005, Busse and Hefeker, 2007, Abadie and Gardeazabal, 2008, Justino and Verwimp, 2008, and Cerra and Saxena, 2008). Within-country studies find mixed results depending on the type of violence analyzed. Evidence on the effects of terrorism and internal conflict points to negative effects on economic growth (e.g., D´Addario, 2006, Arunatilake et al., 2001, Abadie and Gardeazabal, 2003, Deininger et al., 2003, and Pshiva and Suarez, 2006). Evidence on the effects of international wars points to insignificant long term effects on economic activity (e.g., Davis and Weinsten, 2002 and Miguel and Roland, 2011).

[5]See Appendix B for a detailed list of the 16 papers that identify this elasticity.

[6]It most be mentioned however that using similar data Camacho and Rodriguez (2013) study the effects of conflict on the probability of firm exit. The authors find a positive relation between conflict and the probability of firm exit.

[7]There is strong historical evidence supporting this argument (see Guzman et al., 2006, Sarmiento, 1985, Henderson, 1984, Pecaut, 2001, and Roldan, 2002)). The political competition index was created with information from the previous presidential elections to the period of *La Violencia* (1948-1958).

As a consequence of this episode, illegal armed groups were created and first located in areas with higher political competition, proliferating all forms of violence, and dis-empowering the local governments. I use the interaction of local political competition in 1946 with the level of U.S. transfers as my instrument for violence. Consequently, my variation comes from the fact that when security conditions improve across the country, areas with higher political competition in 1946 (namely areas with more contested elections), reduced violence more proportionally when security transfers from the U.S. were higher.

I find large effects of violent crime on a series of market prices, including wages, rents, and non-housing living costs. In particular, I find evidence of a small wage compensation for violent crime; however, it is only statistically significant for white-collar workers. My estimates suggest that when homicide rates increase in 10%, white-collar workers nominal wages increase by about 1%. Data on internal migration from the 2005 Colombian population census suggests that one of the reasons only white-collar workers see compensating wage rises is that they have lower geographic mobility costs (as found by Cullen and Levitt, 1999, and Malamud and Wozniak, 2010). Additionally, I find that higher violent crime induces firms to increase output prices, and that in turn, non-housing living costs are drastically increased in more violent areas. This result is confirmed by the behavior of local food prices: when homicide rates increase 10%, retail food prices increase 6%. The increase in food prices, coupled with firms' pricing behavior, strongly suggests an increase in non-housing living costs in more violent areas. I also find that housing rents decrease in response to higher levels of homicide rates. Specifically, when homicide rates increase 10%, housing rents decrease 4%. However, the effects are too small to compensate for the increase in non-housing living costs. Overall, a 10% increase in homicide rates causes real income for blue- and white-collar workers to decrease 1.3% and 0.6%, respectively. Consequently, violent crime increases income inequality.

With regard to the effects on market size, I find that higher input costs, higher wages, and workers' migration (which reduces output demand) drive firms to reduce production, and ultimately, causes some firms to exit the market. Specifically, when homicide rates increase 10%, firms' production declines 1.7%, and the number of firms in the market is reduced 0.4%.[8]

I then propose a theoretical framework that allows me to compute welfare effects using the estimated elasticities.[9] The model presents an economy divided into municipalities that face different levels of violence. In the model, violence reduces workers utility by acting as a local disamenity–e.g, by increasing the probability of being harmed and the stress of living in more dangerous environments. Additionally, violence increases firms' marginal cost through additional expenditures on security. The model predicts that when violence increases, workers move to areas with lower violence, thereby pushing up wages. Higher wages and higher security-costs induce firms to increase output prices in a setting with monopolistic competition. In turn, higher prices coupled with workers' migration reduce local demand. Hence, aggregate production falls generating negative profits until some firms exit the market. The overall effects of violence on workers' welfare and firms' aggregate production can be expressed as a function of the elasticities of market prices and size with respect to violence. The model can also be used to compute willingness to pay for a violence reduction.

I find negative effects of violent crime on workers' welfare and firms' production, but with some degree of heterogeneity on their magnitude. The overall elasticity of workers' welfare with respect to violent crime is -0.46, about -0.28 for white-collar workers, and -0.63 for blue-collar workers.[10] Consequently, blue-collar

[8] These results are in-line with Camacho and Rodriguez (2013) who identify a negative effect of violence on the probability of firm exit using the same data.

[9] The model combines recent frameworks of multiple regions proposed by Redding (2012) and traditional local labor models formulated by Roback (1982) and Rosen (1979), and extends them to include violence.

[10] These elasticities are statistically significant.

workers are twice as affected to changes in homicide rates and are willing to pay a higher percentage of their income to reduce violent crime (relative to white-collar workers). The elasticity of aggregate production with respect to violent crime is estimated to be -0.21; only half as large as the elasticity of welfare with respect to violent crime (-0.47). Hence, by only considering the effects of violent crime on aggregate production, the negative effects of homicide rates are underestimated.

I address concerns related to the validity of my identification strategy. Specifically, my estimates are only valid if there are no time-varying covariates correlated with U.S. international anti-drug expenditures that also have heterogeneous effects across areas with different levels of political competition in 1946. For example, this occurs if an increase in U.S. international anti-drug expenditures induces the local governments or central governments to change their behavior in different ways within areas with different degrees of political competition. For instance, they could reduce expenditures in areas that received relatively large transfers, or instead choose to complement external funds with more internal funds. I address these concerns by showing that there is no correlation between public expenditures from local governments (as a total and by type) and my instrument, and no correlation between the central government's transfers to municipalities (as a total and by type) and my instrument. I also show my results are robust to controlling for the variation in 45 observable covariates. These observables comprise all the information available at the municipality level in Colombia.[11]

This paper is structured as follows. Section 2 presents the theoretical model, section 3 describes the data, section 4 presents the empirical strategy, sections 5 and 6 present the results, section 7 presents some robustness checks, and finally, the last section offers some concluding remarks.

[11] A municipality is a small political subdivision akin to a county in the U.S. There are 1,119 municipalities in Colombia, about 300 are included in this study since 87% of the sample is located in these areas.

1.2 Model Setup

This section presents the theoretical framework for understanding the effects of violence on local markets, which I use to derive the welfare and aggregate production consequences of violence. The purpose of the model is to provide a link between the estimated (observable) effects of violence and its welfare effects. The model combines simple ingredients previously presented in models of multiple regions by Redding (2012) with labor supply models formulated by Roback (1982) and Rosen (1979), and extends them to include violence.[12] It describes an economy divided into municipalities, indexed by m, that face different levels of violence.[13] Each municipality is composed of workers and firms and is endowed with a fixed stock of quality-adjusted housing.[14]

1.2.1 Workers' Problem

Each worker has one unit of labor that is supplied inelastically with zero disutility.[15] Workers face different levels of violence according to their location. For workers, violence acts as a municipality disamenity and reduces utility. There is ample empirical evidence on the negative effects of violence on workers' utility. For example, Youngstrom et al. (2003), ODonnell et al. (2011), Ramirez (2012), and Leavitt et al. (2014) show that all forms of exposure to violence, including witnessing, being a victim, and knowing victims are correlated with several types of behavioral disorders.[16]

[12] For a more recent application see Serrato and Zidar (2014).

[13] Violence is assumed to be exogenous for modeling purposes, but the empirical section will account for this issue.

[14] Housing is included in the model because extensive previous work has shown that violent crime negatively impacts housing prices, which in turn will affect workers' welfare. See appendix B for a review of the point estimates of the 16 studies that identify the effects of violent crime on housing rents.

[15] Inelastically supplied labor is a common assumption in local labor markets such as Rosen (1979) and Roback (1982). More recent examples can be found in Moretti (2011).

[16] Similar results are presented by Ghobarah et al. (2003), Camacho (2008), Bundervoet et al. (2009), and Akresh et al. (2011), who find negative impacts of civil war exposure on height-

Additionally, workers are imperfectly mobile across locations. Following Redding (2012) restrictions to mobility are introduced assuming that workers have idiosyncratic preferences for each location (x_{im}) drawn from a known distribution. Idiosyncratic preferences can also be understood as idiosyncratic mobility costs for each location, which are independently drawn across workers and locations.[17]

In sum, each worker i, located in municipality m, maximizes utility over housing (h_{im}) and a composite good of tradable varieties (C_{im}) facing wages (w_m), rents (r_m), a non-housing good price index (P_m), violence (v_{mt}), and an idiosyncratic mobility cost (x_{im}). Specifically, workers solve the following problem:

$$\max_{C_{im}>0, h_{im}>0} \Big[\Big(\frac{C_{im}}{\alpha}\Big)^{\alpha}\Big(\frac{h_{im}}{1-\alpha}\Big)^{1-\alpha}\Big]^{\beta}\Big[\frac{1}{v_m}\Big]^{1-\beta} x_{im}$$
$$s.t \qquad P_m C_{im} + r_m h_{im} = w_m + T \tag{1.1}$$

with:

$$C_{im} = \sum_{k \epsilon M}\Big[\int_0^{N_k} c_{jmk}^{\rho} dj\Big]^{1/\rho}$$

where N_k denotes the number of firms and T represents non-labor income, which comes via lump-sum transfers of the total revenue collected through housing rents.[18] Given this setup each worker chooses the region that offers him the highest utility. Moreover, wages and utility differ across locations. The corresponding indirect utility function that describes workers' maximum welfare given market prices and violence is then given by:

$$V_{im}(P_m, w_m, r_m, v_m) = [(w_m + T)P_m^{-\alpha} r_m^{\alpha-1}]^{\beta}[1/v_m]^{1-\beta} x_{im} \tag{1.2}$$

for-age-z-scores for children, prenatal stress, and future health risks (even several years after the end of the conflict).

[17] This assumption is necessary to guarantee different levels of welfare across locations. Otherwise, in the case of perfect mobility workers will move between locations until the welfare is equalized across locations.

[18] Ownership is symmetrical across individuals as in Helpman (1998).

Hence, higher violence reduces workers indirect utility in each location inducing some workers to migrate to other areas. In turn, migration flows are reflected in changes in wages, non-housing prices, and housing rents. Thus, by affecting workers' migration decision, violence indirectly affects market prices.

1.2.2 Firms' Problem

Each firm j acts as a monopolistic competitor producing a unique and differentiated product.[19] Firms are immobile across locations and face different violence intensity (v_m) depending on the municipality in which they are located.[20] Violence increases firms' marginal costs as found by Goldberg et al. (2014), who compiles strong evidence on the higher security costs that firms face when violence is higher for multiple cities and countries in the world.[21] In addition, firms produce their outputs using only labor.[22]

Following Redding (2012), to produce a variety, a firm must incur a fixed cost of F units of labor and a variable costs that is increasing in violence.[23] Hence, the amount of labor ($l_m(j)$) required to produce $y_m(j)$ units of variety j in municipality m is given by:

$$l_m(j) = F + MC(v_m)y_m(j) \tag{1.3}$$

[19]This assumption allows to test whether violence has an effect on the extensive (average production) and intensive margin (firm exit) of production. If firms are assumed to act as price takers, then violence will only affect the extensive margin of production (firm exit).

[20]This assumption follows the behavior observed in Colombian data where firms' mobility between municipalities occurs for only 2% of the sample. However, the results of the model hold as long as there are some restrictions to firms' mobility, which in practice is always the case, given that firms have invested on infrastructure in each location.

[21]Their city studies include Ciudad Juarez, Medellin, Mexico City, Rio de Janeiro, and Tijuana, while, their country-level studies include Jamaica, Nepal, and Rwanda.

[22]This assumptions was imposed for simplicity. However, a more complicated of the version in which firms produce using labor and other firms outputs as their inputs of production yields similar results.

[23]Firms can sell locally or to other municipalities. As long as there are some barriers to trade the results of the model hold. In practice, this is always the case since there are always transportation costs between regions.

where $MC(v_m)$ represents the marginal costs incurred by the firm which are an increasing function of violence. Profit maximization implies that equilibrium prices are a constant mark-up over marginal costs so that the prices offered by firm j at municipality m are given by:

$$P_m(j) = [\frac{\epsilon}{1+\epsilon}]MC(v_m) \tag{1.4}$$

where ϵ denotes the elasticity of demand. Replacing the constant mark-up condition into the free entry condition yields the equilibrium output. Thus in this context, by increasing firms' marginal costs, violence reduces the equilibrium intensive margin of production:

$$y^*_m(j) = \frac{F(\epsilon - 1)}{MC(v_m)}$$

Given $y^*_m(j)$, the labor market clearing condition implies that the total number of firms in each municipality (N_m), is proportional to the endogenous supply of workers. Thereby, violence also affects the extensive margin of production:

$$N^*_m = \frac{L_m(v_m)}{F\epsilon} \tag{1.5}$$

Consequently, the number of firms in each municipality is a decreasing function of violence because higher violence induces workers migration. In brief, violence reduces the aggregate production within each municipality by reducing the production of the firms that stay in the market (intensive margin) and by driving firms to exit (extensive margin).

1.2.3 Violence Incidence on Welfare and Aggregate Production

The effects of violence on workers' welfare can be approximated by the effects on their indirectly in their indirect utility of consumption, which captures the direct effects of violence on utility as well as the indirect effects through changes in prices. Additionally, the effects on firms' aggregate production in each municipality can be estimated as the sum of the effects of violence on firms' intensive and extensive margin of production. Specifically:

Proposition 1 *Given the indirect utility function presented in equation (1.2). The effects of violence on the utility of consumption of worker i at municipality m can be expressed as:*

$$\frac{dV_{im}}{dv_m} = \underbrace{\frac{\partial V_{im}}{\partial P_m}\frac{\partial P_m}{\partial v_m} + \frac{\partial V_{im}}{\partial w_m}\frac{\partial w_m}{\partial v_m} + \frac{\partial V_{im}}{\partial r_m}\frac{\partial r_m}{\partial v_m}}_{\textit{Market/Indirect effects}} + \underbrace{\frac{\partial V_{im}}{\partial v_m}}_{\textit{Direct disutility effects}} \tag{1.6}$$

Moreover, the elasticity of firms' aggregate production with respect to violence within each municipality m is given by:

$$\epsilon_{Yv} = \frac{dlog(Y_m)}{dlog(v_m)} = \underbrace{\frac{\partial log(\bar{y_m})}{\partial log(v_m)}}_{\textit{intensive margin effects}} + \underbrace{\frac{\partial log(N_m)}{\partial log(v_m)}}_{\textit{extensive margin effects}} = \epsilon_{yv} + \epsilon_{Nv} \tag{1.7}$$

where $Y_m = N_m \bar{y_m}$ denotes the aggregate production in municipality m at period t given the total number of firms (N_m) and the average production per firm ($\bar{y_m}$).

Equations (1.6) and (1.7) are at the center of my empirical analysis. In the next sections I identify empirically the effects of violence on market prices (i.e., non-housing prices, rents, and wages) and market size (i.e. firm exit and production) to then quantify equations (1.6) and (1.7).

The model predicts the direction in which violence should affect market prices and size. When violence is higher firms are expected to increase output prices to offset the higher costs they face. Additionally, workers' migration will induce wages to increase and housing prices to fall given the fixed housing supply within each municipality. Higher costs and lower sales due to workers' migration reduce firms' profits, so that ultimately, the number of firms in the market is reduced.

1.3 Data

I use Colombian annual data between 1995 and 2010 to carry out my empirical analysis. Colombia offers an ideal setting to identify the effects of homicide rates for at least three reasons. First, there was drastic municipal and annual variation in homicide rates during the period of analysis. Second, in the early 90s there was intense violent crime, making Colombia the second most violent country in the world after El Salvador. However, this violent episode was followed by a remarkable recovery in security conditions. In particular, homicide rates dropped in 48% during the period of analysis. Third, it is a developing country with excellent micro data on firms' behavior and consumer retail food prices.

1.3.1 Data on Violent Crime

Data on violent crime by municipality is available from the Observatory of Human Rights of the Colombian Vice Presidency. I use intentional homicide rates per 100,000 inhabitants as a measure of violence because they are available for the whole period of study and for all of the municipalities in the country. This is not the case for any other available indicators. Moreover, this is the only available measure of violent crime consistently measured across countries and regions of the world, which facilitates the interpretations and comparison on the results of this study.

Figure 1.2 (right panel) presents the time evolution of intentional homicide rates for the period of interest. It shows that violent crime was drastically reduced after 2002 with the election of Álvaro Uribe as president. Uribe's primary policy objective was to improve security conditions across the country. Consequently, homicide rates declined by 48% between 1995 and 2010, from 65.8 to 33.97 homicides per 100,000 inhabitants, respectively.

Figure 1.3 presents the geographic distribution of intentional homicide rates for 2002 and 2011, the years before and after the sharp decline in violent crime. The figure suggests that the violent crime reduction was focused on the center of the country (around the capital city). Additionally, the figure shows the strong geographic variation on homicide rates during the period of analysis. I exploit the annual and municipal variation in homicide rates to identify the effects of violent crime.

When considering the correlation between homicide rates and other measures of violence more representative of the Colombian armed conflict (such as armed actions, attacks, clashes between groups, and deaths in battle),[24] I find that both types of violence are correlated; however, this correlation is low. The highest correlation occurs between homicide rates and armed actions, with a value of 0.2. This suggests that, although the intensity of the armed conflict influences homicide rates, the main variation in homicide rates is driven by other variables. Moreover, because more than 87% of the firm-level data is located in urban areas and the Colombian armed conflict mainly takes place in the rural areas, this study identifies the effects of more general forms of violent crime, rather than violence induced by conflict.

[24]Available through the Conflict Analysis Resource Center (CERAC).

1.3.2 Data on Market Prices and Size

I use three main sources of information. The first source of information is the *Encuesta Anual Manufacturera* [Annual Manufacturing Survey], collected by the *Departamento Nacional de Estadística, DANE*, the Colombian statistical agency. The data set is a census of all the manufacturing plants with ten or more workers or value of total output larger than 65 million of 1992 Colombian pesos (approximately USD$95,000). Once a plant is included in the survey, it is followed over time until it goes out of business. Moreover, all multi-plant firms are included even if only one of them satisfies the selection criteria. The data set is an unbalanced panel data of approximately 16,016 firms (16,776 plants) for the period between 1995 and 2010, which amounts to a total of 124,247 observations.

In conjunction with the standard plant information, the census contains information on all physical quantities and prices (valued at factory-gate prices) of each output and input used or produced by each plant. In this paper, firms' prices are defined as the plant-product-year observation estimated by dividing the value of revenues or expenditures by physical quantities. Appendix A presents the descriptive statistics of the survey for 1995 and 2010.[25] I use this data to estimate the effects of homicide rates on firms' output prices, wages, production, and exit decision.[26]

A secondary source of information are local retail food prices collected by the Colombian Ministry of Agriculture. The data covers average annual retail prices of the 500 most consumed food products (according to sales) within 53 municipalities located in 20 departments from 1996 to 2010. I use this data to study the effects

[25] The tables report a drop in average production and wages between 1995 and 2010. This is only observed when the variables are expressed in dollars given the drastic depreciation of the Colombian peso between those years. Yet, the values increased in Colombian pesos during this period.

[26] All variables expressed in monetary terms (except wages) were transformed into real values using a producer price index generated for each firm using 1995 as a base year. The index was constructed using a Laspeyres methodology.

of homicide rates on retail food prices.

The third source of information are the Colombian National Household Surveys between 1995 and 2010. These surveys are representative at the national level and correspond to cross sections collected annually. They are also collected by the Colombian statistical agency and contain information on workers' and households' socioeconomic characteristics. I use this data to estimate the effects of homicide rates on housing rents and recover key parameters of the indirect utility function.

1.4 Identification Strategy

My empirical analysis proceeds in two steps. First, I estimate the effects of homicide rates on market prices (non-housing prices, housing rents, and wages) and market size (firm production and exit). Then I combine the estimated elasticities with equations (1.6) and (1.7) to quantify the effects of violent crime on workers' welfare (measured as utility of consumption) and firms' aggregate production.

To identify the effects of violent crime I exploit the municipal and annual variation in homicide rates between 1995 and 2010.[27] The municipal standard deviation of homicide rates across years is presented in Figure 1.2 (left panel). It confirms that there is a large geographic variation in violent crime during the period of interest. Firms' exposure to violence corresponds to the homicide rates observed in the municipality where they are located. The specification of interest is given by:

$$log(y_{jmt}) = \gamma_0 + \gamma_1 log(v_{mt}) + k_j + g_t + \epsilon_{jmt} \tag{1.8}$$

where y_{jtm} represents market prices or size observed for firm j, at year t, and located at municipality m, v_{mt} are homicide rates, ϵ_{jmt} is the error term, and k_j

[27]Colombia is divided into 1,119 municipalities, they are approximately equivalent to a U.S. county.

and g_t are fixed effects by firm (or municipality) and year.[28]

The identification of γ_1 is challenging given the endogeneity concerns between homicide rates and market outcomes, even after controlling for firm or municipality fixed effects. Specifically, firm or municipality fixed effects only solve issues of cross section endogeneity that correspond to static differences between areas with high and low levels of violent crime. However, time-feedback effects may be still taking place. For example, in the case of firms' production, time-feedback effects may take place in two different directions. First, when production is high, economic conditions may improve, inducing less poverty, less violent crime, and better economic conditions as documented by Miguel et al. (2004) and Miguel and Satyanath (2011). This is the so-called *grievance* channel, as defined by Collier and Hoeffler (2004). It implies that high-production areas tend to be less violent, whereas low-production areas tend to be more violent. Hence, the gap in production increases with time across areas with different levels of violent crime. In this setting, γ_1 will be upward biased. In contrast, as suggested by Dube and Vargas (2013), a rise in contestable income via an increase in production, may also increase violent crime by raising gains from income appropriation. This is the so-called *greed or rapacity* channel, which suggests that violent crime may be equally significant in areas with high and low production. In this situation, γ_1 will be biased towards zero.[29]

To address for endogeneity concerns, I use a panel-instrumental variable methodology. Firm or municipality fixed effects solve the cross section endogeneity prob-

[28]Specifically, for the estimates of the effects of homicide rates on firm exit decision and rents the model only includes fixed effects at the municipal level.

[29]Time-feedback effects also increase prices in areas with low and high violent crime, which complicates the identification of its effects. For example, in areas with high levels of violent crime firms' costs are higher given the additional security expenditures. In turn, higher prices and reduce purchasing power, which further fuels violent crime. However, areas with low violent crime also face higher prices given they likely also have high agglomeration. Higher prices induce selection so that only the wealthiest individuals tend to stay. Because the wealthiest individuals are also likely to be the most educated, violent crime is further reduced. Hence, the estimates of γ_1 will be biased towards zero.

lems, whereas the instrument for homicide rates addresses the time-feedback effects between homicide rates and market outcomes. In sum, I estimate the effects of homicide rates through the following specification:

$$log(y_{jmt}) = \gamma_0 + \gamma_1 log(v_{mt}) + k_j + g_t + \epsilon_{jmt} \tag{1.9}$$

$$log(v_{mt}) = \theta_0 + \theta_1 PC_{mt} + k_j + g_t + u_{jmt} \tag{1.10}$$

where y_{jmt} represents the outcomes of firm j, located in municipality m, at year t, v_{mt} represents homicide rates, PC_{mt} is the instrument for violent crime (explained in detail in the next section), and k_j and g_t represent firm (or municipality) and year fixed effects. In this specification, γ_1 will identify the elasticity of the firms' outcomes with respect to violent crime. The identification strategy is valid so long as the exclusion restriction (i.e., $corr(PC_{mt}, \epsilon_{jmt}|k_j, g_t) = 0$) and relevance assumption are satisfied.[30]

1.4.1 Instrumenting for Homicide Rate

The time variation of the instrument is driven by changes in U.S. international anti-drug expenditures. Around the mid 1990s Colombia became the top producer of cocaine, which was mainly exported to the U.S.[31] Consequently, beginning in 1995 the U.S. began sending approximately 30% of its international anti-drug expenditures each year to improve security conditions in Colombia.[32] Conditional on local population, these resources were to be spent evenly across municipalities.[33] According to the annual budget of the Office of National Drug Control Policy of the White House (ONDCP), between 1995 and 2010 the U.S. disbursed

[30]The relevance assumption, as defined by Imbens and Angrist (1994), Abadie (2003) and Angrist et al. (1996) requires a strong correlation between violent crime and the instrument.

[31]See the annual World Drug Reports by the United Nations Office of Drugs and Crime.

[32]According to official documents this condition was imposed to guarantee that the resources reached the poorest and most politically unrepresented regions of Colombia.

[33]Yet, within each municipality these resources could be spent in different ways.

18.27 billion dollars to reduce the international supply of illegal drugs.[34] The time evolution of these expenditures is presented in Panel (B) of Figure 1.4.[35] Panel (A) of the same figure shows the changes in homicide rates per 100,000 inhabitants compared to the changes in U.S. international anti-drug expenditures. It suggests that there is a negative contemporaneous correlation between the changes in both variables.

The resources disbursed by the U.S. had different levels of effectiveness within different municipalities: they reduced violent crime more proportionally in areas where violent groups were more clearly established by targeting those groups and recovering the monopoly of authority. To identify these areas, I use a political competition index for 1946, which corresponds to the presidential elections previous to the episode of *La Violencia*, which historians point as the origin of the current illegally armed groups–e.g., Guzman et al. (2006), Sarmiento (1985), Henderson (1984), Pecaut (2001), and Roldan (2002). *La Violencia* was a historical episode that took place between 1948 and 1958. It was period of strong violence between the two traditional political parties. In April of 1948, political competition between the *Liberales* [liberals] and *Conservadores* [conservatives] escalated dramatically to armed actions as the liberals' leader, Jorge Eliecer Gaitán, was assassinated. Although an amnesty was declared between parties in 1953, after which most of the armed groups were disarmed, poor economic conditions for former combatants coupled with low support for reintegration, facilitated the creation of new illegally armed groups.[36] Hence, the illegally armed groups were

[34]There is no record of U.S. international anti-drug expenditures to Colombia before 1995 in the official documents of ONDCP or Colombian data. For this reason, I assume they are very small or nearly zero.

[35]To whether the variation on my instrument is driven by a time trend I run all my estimates also restricted my sample for the period between 1998 and 2002 when there was a sharp increase and decline in U.S. international anti-drug expenditures. My results are robust to this test, they are not reported to save space, but the estimates are available upon request.

[36]Consequently, in 1964 adherents of the Cuban-style revolution founded the National Liberation Army (ELN, for its initials in Spanish). Later, in 1966, a second left-wing group called the Revolutionary Armed Forces of Colombia (FARC, for its Spanish name) was founded as the union of all the remaining communist guerillas. Initially, both groups claimed to defend the

created and originally located in areas with higher political competition around *La Violencia*.

Once created, illegally armed groups spread all forms of violence where they were initially located. Consequently, governments with higher political competition in 1946 became less empowered and violent groups were also more clearly established in these areas. Hence, areas with a higher political competition during *La Violencia* have higher violent crime today. Moreover, when these areas received higher security expenditures, driven by higher U.S. international anti-drug expenditures, they were able to reduce violent crime more effectively by recovering the monopoly of authority and targeting these violent groups more precisely.

Exploiting this idea, I construct the instrument for homicide rates as the interaction of the U.S. international anti-drug expenditures and a political competition index for 1946–which corresponds to the presidential elections prior to the crisis of *La Violencia*.[37] The political competition index that I use was constructed by Chacon et al. (2011) with information from the results of the 1946 presidential elections by municipality as:

$$PC_m = 1 - \frac{|\%\text{Liberal votes}_m - \%\text{Conservative Votes}_m|}{100} \tag{1.11}$$

thus, PC_{mt} takes values between zero and one. Low values of the index cor-

interests of the rural poor, aiming to overthrow the government and install a Marxist regime. However, with time, both groups became primarily economically motivated (Dube and Vargas, 2013). Paramilitarism began in the late 1980s as an anti-insurgent response by land-owners and drug traffickers to the left-wing guerillas' actions in areas where the state was unable to provide security. In 1997, the paramilitary forces coalesced into the United Self-Defense Organization of Colombia (AUC, for its Spanish name). By 2003, the AUC declared a partial ceasefire, and some paramilitary blocks agreed to participate in a 'disarming program' that concluded in 2005. However, many of the combatants that were part of the AUC fused later into new criminal groups that are known today as *Bandas Criminales* (BACRIM, for its name in Spanish).

[37] The theoretical relation between political competition and violence has been studied recently by Chacon et al. (2011) and Dunning (2011). The authors show that when institutions are weak and several groups fight for power, democracy in peace is easier to achieve when one group is dominant. Otherwise, although both groups have a higher chance of winning elections, there is also a higher likelihood of success in challenging election results through armed action.

responds to the case where one of the political parties had the absolute majority within a municipality. On contrast, high values correspond to cases of extreme political competition (equal vote share in each party). The index is available for 755 of the 1,119 Colombian municipalities and has a mean of 0.5. Specifically, the instrument for homicide rates is constructed as:

$$PC_{mt} = PC_m * \text{US-IAE}_t \tag{1.12}$$

where US-IAE_t represents the U.S. international anti-drug expenditures in millions of dollars of 1995. In sum, my identification comes from the fact that areas with higher political competition in 1946 had violent groups more clearly established and less empowered local governments. Hence, they were also more responsive to higher expenditures in security. The higher responsiveness of areas with higher past political competition to transfers in security is explained because these funds were used to recover the monopoly of authority. Suggestive evidence on this idea is presented in Panel (B) of Figure 1.4.[38] The figure shows the time evolution of homicide rate in areas with high and low political competition for 1946. It suggests that U.S. international anti-drug expenditures induce a reversal of fortune between the areas that had different levels of political competition. In other words, the gap in violence between these areas shrinks as U.S. international anti-drug expenditures are higher.

1.4.2 Correlation between Homicide Rates and the Instrument

Evidence on the correlation between homicide rates and PC_{mt} is presented in Figures 1.5, 1.6, and 1.7. Figure 1.5 presents a fitted linear regression of the mean value of homicide rates against deciles of political competition for 1946.

[38] A similar graph which divides municipalities in two groups according to the median level of violence only shows that both areas reduce violence in the same proportion and the gap between regions is the same across time.

The sample used to construct this figure includes the homicide rates across the whole period of study (the same behavior can be replicated for each year between 1995 and 2010). The figure suggests that municipalities with higher political competition in 1946 have higher homicide rates today.

Figure 1.6 presents the same exercise for years with different levels of U.S. international anti-drug expenditures. The level of U.S. anti-drug expenditures is reported in the label in parentheses. The figure suggests that the positive correlation between homicide rates and past political competition is positive for every year. Moreover, it suggests that areas with higher political competition in 1946 reduce violent crime for proportionally when U.S. transfers were higher.

In addition, Figure 1.7 presents the absolute change in homicide rates from 1995 (the year with the lowest U.S. anti-drug expenditures) and 2010 (the year with the highest U.S. anti-drug expenditures). It confirms that the areas with the highest political competition index in 1946 reduced homicide rates more proportionally relative to the other areas in response to higher U.S. transfers.

A formal test for the correlation between the instrument and homicide rates is presented in Table 1.2. The table presents the results of the first stage regression of the logarithm of homicide rates on PC_{mt} including fixed effects by firm and year as described in equation (1.10). Column (1) presents the first stage regression using PC_{mt} as an instrument, and columns (2) and (3) present the results of the regression using each PC_m and US-IAE$_t$ as instruments. The last two columns are presented as evidence of the individual contribution of each variable towards the instrument.

The results for column (1) confirm that there is a strong correlation between the instrument and homicide rates. The coefficient on the instrument has a negative sign and is statistically significant. Thus, as predicted, municipalities with a higher political competition index for 1946 reduce violent crime more proportionally when there are higher U.S. security transfers. The partial R^2 is 8% and the F-test for excluded instruments takes a value of 86.07,[39] alleviating concerns of finite sample bias due to weak instruments (as defined by Bound et al., 1995). Moreover, the estimates in columns (2) and (3) confirm that each of the variables has a strong correlation with violence and affect it in the expected direction. In particular, homicide rates are higher today in municipalities that had a higher political competition index in 1946, whereas higher U.S. anti-drug expenditures have a negative correlation with homicide rates.

The last section of the paper presents several robustness checks that support the validity of the exclusion restriction.

1.5 Incidence of Violent Crime in Local Markets

This section presents the estimates of the elasticities of homicide rates on market size (average production and exit) and prices (non-housing prices, housing rents, and nominal wages). The estimates correspond to the results of equations (1.9) and (1.10), where (v_{mt}) is homicide rates per 100,000 inhabitants.

1.5.1 Effects on the Intensive and Extensive Margin of Production

Table 1.3 presents the estimates of equations (1.9) and (1.10) using the logarithm of real production as dependent variable (otherwise referred to as the intensive margin of production). The results presented in column (1) suggest that the OLS estimates of the effects of homicide rates on real production are biased towards zero, in-line with the *rapacity* channel discussed in section 4. The results on columns (2) and (3) suggest negative effects of violence on firms' production, which is consistent with the previous results by Alesina and Perotti (1996), Abadie and Gardeazabal (2003), Fielding (2003), Singh (2013), Collier and Duponchel (2010),

[39]For the case of a single endogenous regressor, Staiger and Stock (1997) suggest rejecting the hypothesis of weak instrument if this F-statistic is higher than 10.

and Klapper et al. (2013).[40] When endogeneity is addressed the coefficients increase in absolute value. My preferred estimates, presented in column (3), suggest that when homicide rates increase 10%, firms' production declines 1.7%.[41]

To estimate the effects of homicide rates on the total number of firms (the extensive margin of production), I aggregate the firm data by municipality. The results are presented in Table 1.4 and suggest that when homicide rates increase 10%, the total number of firms in a given municipality declines 0.4%.[42] This result is in-line with Camacho and Rodriguez (2013), who use the same data to study the effects of guerrilla and paramilitary attacks on the probability of firm exit.[43]

Following equation (1.7), the aggregate production-violence elasticity corresponds to the sum of the elasticities of the number of firms and firms' production with respect to homicide rates. The values for these elasticities were identified in Tables 1.3 and 1.4. They suggest that when violence increases 10%, aggregate production falls 2.1%;[44] this implies that the 48% decline in Colombia's homicide

[40]These papers find negative effects of violence on firms' stock market returns and productivity. However, there is evidence of heterogeneous effects. For example, Guidolin and La Ferrara (2010) study the effects of the end of the Angolan civil war on stock market returns of firms operating in the diamond sector. The authors find that the sudden death of the rebels' leaders, which marked the end of the civil war, was detrimental for incumbent firms because violence acted as a barrier to international competition.

[41]I also check for the effects of violence across the distribution of production in Appendix C-using quantile regressions. For this purpose, I combine the methodology proposed by Buchinsky (1998) to control for selection and Lee (2007) to control for endogeneity. A detailed description of the methodology is presented in Appendix C with the results. I find that the effects of violence are similar across the distribution of real production, so small and big firms are equally affected by violence.

[42]I find similar results using the same specification and information on the total number of firms registered at the Chambers of Commerce and collected by *Confecamaras*, the association of Chamber of Commerce in Colombia. The institution collected data on the total number of firms registered across the country from 2000 to 2005 for 996 municipalities in Colombia.

[43]They find that a one standard deviation increase in the number of guerrilla or paramilitary attacks increases the probability of plant exit 5.5 percentage points.

[44]My estimates are a lower bound of the effects of violent crime on aggregate production because $E(log(y_{jm})) \leq log(E(y_{jm}))$. To test whether this problem was important, I estimate equations (1.9) and (1.10) using aggregate production by municipality as the dependent variable. Aggregate production is constructed as the sum of the production of all firms within each location and time period. The results are similar and suggest that when homicide rates increase 10%, aggregate production within each municipality falls 2.31%. I report the elasticity using firm-level data because it allows to control for time varying industry fixed effects as well as for firm time invariant characteristics.

rates from 1995 to 2010, increased aggregate production 9.96%.

1.5.2 Effects of Violent Crime on Housing and Non-housing Prices

Table 1.5 presents the estimates of equations (1.9) and (1.10), taking the logarithm of firms' real output prices as the dependent variable. In these estimates, I test for the sensitivity of results to the inclusion of firm fixed effects, year fixed effects, product classification fixed effects,[45] product-time fixed effects, and controls for other municipality covariates listed in Appendix D.[46]

The estimates are presented in Table 1.5 and suggest a positive and sizable effect of homicide rates on firms' real output prices. As expected, the effects of violence on firms' prices grow when correcting for endogeneity. My preferred estimates are presented in column (4), which account for endogeneity for the sample of firms that stays in the market. They suggest that when homicide rates increase 10%, real output prices increase 5.3%.

Since in practice there are input-output linkages, firms may possibly face higher input prices when violence is higher. To check if this is true, I estimate the same specification using the logarithm of real input prices as the dependent variable. The results are presented in Table 1.6 and show a similar behavior. Column (4) suggest that when homicide rates increase 10%, the input prices faced by firms increase 2.0%. Thereby, firms increase output prices disproportionately more than the increase in input prices that they face.

These changes in prices represent a sizable effect of homicide rates. For example, Kugler and Verhoogen (2012) use the same data between 1982 and 2005 to estimate the elasticity of real output and input prices to firms' size. They find

[45] I use the four first digits of the International Standard Industry Classification codes to create the fixed effects; they include around 115 products.

[46] According to Kugler and Verhoogen (2012) the inclusion of controls by product classification and time-product trends is crucial to exclude the variation in prices explained by the dynamics of each product's industry.

that a 10% increase in employment results in 0.26% and 0.12% higher real output and input prices, respectively.

The behavior of local retail food prices shows a similar behavior. In Table 1.7 I estimate equations (1.9) and (1.10) using data on retail food prices. The results in column (4) suggest that when homicide rates increase 10%, real food prices increase 5.9%. This represents further evidence that non-housing living costs are higher in areas with higher violent crime given food prices have the largest weight in the Colombian CPI. [47]

To estimate the effects of homicide rates on nominal housing prices I use data from the National Household Surveys.[48] Specifically, I estimate equations (1.9) and (1.10) for the cities available on the National Household Survey including fixed effects by year and municipality and controlling for individuals age, education, gender, number of children, and marital status. As shown in Table 1.8, I identify an elasticity of housing prices with respect to violence of -0.38 (s.e. 0.15), in line with the 15 studies that have identified this parameter (their point estimates are reported in Appendix B).[49]

1.5.3 Effects of Violent Crime on Wages

The theoretical model predicts that higher levels of violence increase wages because workers leave more violent areas. This section estimates the wage-violence elasticity, which corresponds to running a hedonic wage equation in the spirit of

[47]Particularly, according to the *Departamento Administrativo Nacional de Estadística*, the Colombian statistical agency, food consumption represents approximately 42% of the consumer price index and is the most relevant item for living costs, excluding housing prices. I exclude housing prices in these calculations because they have a separate term in the welfare estimates in equation 1.6. The second biggest item in terms of weight is transportation with 25%, followed by education with 5%, clothing with 2%, and other categories with smaller shares.

[48]During the period of analysis these surveys had several methodological changes. From 1995 to 2005 the surveys are available for the 13 cities and from 2006 and 2010 they are available for the main 24 cities of the country.

[49]Specifically, all the studies identify a negative effect of violent crime on housing prices with an elasticity range between -0.1 and -3, and an average of -1.16.

Rosen (1986).

As mentioned by Kniesner et al. (2010) and Lavetti (2012), the estimation of a hedonic equation on wages should ideally include information by individual and by firm.[50] However, despite the richness of the data used in this paper, information on both firms' and workers' characteristics is unavailable. Hence, I only include fixed effects by firm. Although most estimates in the literature use workers' heterogenity, recent studies have called attention to the relevance of firms' heterogeneity in explaining wage variation (e.g., Card et al.,2013).[51]

Table 1.9 reports the estimates of equations (1.9) and (1.10) using the logarithm of nominal average wages as the dependent variable. I find evidence of a small but positive wage compensation to violence. As for the previous cases, the elasticity of violence on wages grows in absolute value when corrections for endogeneity. The estimates in column (2) suggest that when homicide rates increase 10%, nominal wages increase 0.7%.

To test for heterogeneous effects of violence by type of worker, I use the logarithm of nominal average wages for white- and blue-collar workers. Table 1.10 present the results, which suggest that only white-collar workers are compensated for higher violence.[52] In particular, when violence increases 10%, white-collar workers' wages increase 0.9%.

[50]Some studies also include fixed effects for matching effects between firms and workers which solves the endogeneity caused by endogenous switching. This is only relevant when there is an idiosyncratic productivity component associated with potential job match in the theoretical model, which is not the case in this paper.

[51]For instance, Frias et al. (2012) suggest that two thirds of wage variation can be explained by firm heterogeneity, and Abowd et al. (2002) show that workers' and firms' heterogeneity have equal importance in explaining wage variation. Estimates by Lavetti (2012) show that a wage's hedonic equation that only includes firms' heterogeneity can explain as much as 66% of the wage variation in a linear or a non-linear model. See Table 6, 7, and 10 of Lavetti (2012).

[52]My estimates mainly correspond to the urban areas of Colombia where 87% of my sample is located. To check whether migration from the rural to urban areas was accounting for the results observed for blue-collar workers I run the estimates excluding the 13 main cities of the country where 92.3% of the registered migration from rural to urban places takes place. The results are robust to this exercise, they are not reported to save space, but the estimates are available upon request.

I use IPUMS census data to test whether heterogeneous effects of violence on wages may be partially driven by differential mobility costs for individuals with higher skill levels. I use the Colombian population census for 2005, the only census available during the period of analysis. In the census, households are asked for their location five years ago. Despite the fact that there is no information on the type of work each individual performs, I use years of schooling and complete secondary as measures of types of skill.

In brief, I run a probit model for the probability of migrating in 2005 on mean homicide rates from 2000 to 2005 in the municipality where the individual was located in 2000, a measure of the education observed in 2005, the interaction of the former two variables, gender, age, and regional controls (i.e., department controls). Table 1.11 reports the results of this exercise, suggesting that workers with higher education have a higher probability of migrating. Additionally, workers that lived at municipalities with higher homicide rates between 2000 and 2005 also have a higher probability of migrating. Moreover, all the interactions for violence and education are significant and have a positive sign, which present strong evidence of higher mobility restrictions for lower skilled workers when facing a violence shock.

Since I am using the level of education observed in 2005 (after migrating), it may be argued that workers may have increased their education after migrating. To address this threat, I re-estimate the probit model only for workers that had more than 25 and 30 years in 2000 (before migrating). This group of individuals has lower chances of increasing their education in their new location. The results are reported in columns (3) and (4) and show a very similar behavior.[53] These

[53]Data on international out-migration from Colombia supports this claim. According to the International Organization for Migration, in 2005 there were around 3.3 million Colombians living abroad (Ramirez et al.,2010). This estimate was obtained by using the population Census of 2005, which recorded whether a member of a household was living abroad permanently and in which country. The 2005 U.S. Census suggests that, around 1 million of these Colombians were living on the U.S. and 37% of these immigrants have graduated from college (before migrating). In contrast, that same year only 14% of Colombian residents graduated from college (Medina

results are in-line with empirical evidence found by Cullen and Levitt (1999) and Malamud and Wozniak (2010) for the U.S.[54]

When considering the size of the effects of violence on wages, they seem small relative to the effects of other wage shocks.[55]

1.6 Measuring the Welfare Consequences of Violent Crime

1.6.1 Effects of Violent Crime on Workers' Real Income

Before making any parametric assumptions, it is worth considering what could be the effects of violence on real income based on the elasticities identified in the last section. Let PI represent an aggregate price index of housing and non-housing goods. Then, a simple accounting exercise of the effects of violence on real income is given by:

and Posso, 2009).

[54]Cullen and Levitt (1999) examine the relationship between crime and urban flight in the main cities of the U.S. by type of worker. The authors use city-level data covering the last three decennial census years for 127 U.S. cities with populations greater than 100,000 in 1970. They find that migration decisions of highly educated households are particularly responsive to changes in crime. Similar evidence has been presented by Malamud and Wozniak (2010). They examine whether or not higher education is a causal determinant of geographic mobility using the 1980 U.S. Census. The authors use state-cohort level variation in college completion arising from draft avoidance behavior among men at risk for conscription into the Armed Forces during the Vietnam conflict as a source of exogenous variation in the probability that a man completed college. They show that this variation increased migration rates substantially among affected cohorts. They find that college education increases the probability of a long-distance move for the marginal college graduate significantly. One of the mechanisms at hand is that college education increases the set of possible occupations available for recent graduates. This result is in line with a large empirical literature that has documented that the local labor supply elasticity is larger for high-skill workers than for low-skill workers. For example, Bound and Holzer (2000) find that in response to demand shifts less educated workers drop substantially.

[55]For example, Cortes (2008) uses U.S. data to study the effects of low-skilled immigration on wages. Her results suggest that when there is a 10% increase in the share of low skilled immigrants in the labor force, blue-collar wages decrease 2%. Moreover, Dustmann et al. (2013) uses U.K. data to study the effects of immigration on the wage distribution. They find that an additional inflow of immigrants of 1% of the native population reduces wages in the low percentiles (i.e., 5th percentile) 0.6%, but increases the median wage 0.6%.

$$\frac{\partial ln(w/PI)}{\partial ln(v)} = \frac{\partial ln(w)}{\partial ln(v)} - \frac{\partial ln(PI)}{\partial ln(v)} \tag{1.13}$$

an approximation of the effects of violent crime on the price index (PI) can be obtained using the estimated elasticities of the effects of homicide rates on retail food prices (0.59) and housing rents (-0.38) and the weights of non-housing and housing expenditures in the Colombian CPI. The weights of non-housing and housing goods on the Colombian CPI take values of 42% and 30.1%, respectively.[56] Replacing these values into equation (1.13) I find that when homicide rates increase 10%, the real income of white- and blue-collar workers is reduced 0.6% and 1.3%, respectively. Consequently, higher homicide rates increase income inequality. Moreover, blue-collar workers are two times as sensitive to the effects of homicide rates relative to white-collar workers.

1.6.2 Effects on Workers' Welfare

The effects of homicide rates on real income ignores that violence not only affects workers indirectly through changes in market prices, but also by inducing direct effects on utility. For example, workers may be losing utility when they are exposed to more dangerous environments. I use equation (1.6) to estimate the elasticity of workers' welfare with respect to homicide rates. This elasticity accounts for the direct and indirect effects of homicide rates. In this context, the welfare effects of violent crime are approximated through its effects on workers' utility of consumption.

I use the estimates identified in the last section to recover the partial derivatives on the effects of homicide rates on market prices.[57] The other terms of equation (1.6) are derived using the average indirect utility function that solves the workers'

[56] The other categories included in the CPI correspond to health, education, transportation, communications, and entertainment. Together they account for 28% of the CPI.

[57] By combining the estimated elasticities with the observed mean values of P_m, w_m, and r_m.

problem as stated in equation (1.2) across all individuals in each municipality.[58] The values for α and β are set to 0.82 (s.e. 0.021) and 0.98 (s.e. 0.15) based on the identification strategy described on Appendix F.[59]

Table 1.12 presents the results of this exercise.[60] They suggest that when homicide rates increase 10%, welfare declines for all workers 4.6%. However, there are heterogeneous effects of violence by type of worker. Specifically, the welfare-violence elasticity for white-collar workers is 0.28 and for blue-collar workers is 0.63. Consequently, blue-collar workers are two times more responsive to violence than white-collar workers. Thus, higher violence increases welfare inequality.[61] The heterogeneous effects of violence on welfare are mainly induced by the differential effects that violence has on workers' wages by type of skill given all workers face similar living costs. Thus, by increasing the wag gap, violence increases welfare inequality.

When decomposing of the effects of violence into the direct disutility created by violence and the welfare losses due the indirect effects caused by changes in

[58]Which will be given by:

$$V_m(P_m, w_m, r_m, v_m) = [(w_m + F)P_m^{-\alpha} r_m^{\alpha-1}]^{\beta}[1/v_m]^{1-\beta}\bar{x_m}$$

The value for the average locality shock by municipality was set to 1. From this expression, I derive the four missing partial derivatives and use observed mean values of P_m, w_m, r_m, and v_m to estimate their magnitudes. The specific values I use are presented in Appendix E.

[59]Similar values were obtained by Davis and Ortalo-Magne (2011) for the share of housing consumption expenditures using U.S. data. Standard errors were computed using the delta method.

[60]I also computed the welfare effects of violent crime by assuming a CES utility function. Although there are some changes in the order of magnitudes the ordering of the effects is the same. In particular, the effects of violent crime on welfare are always larger than the effects of violent crime on aggregate production. In addition, blue-collar workers are always more affected by violent crime. This result remains of changed for any utility function in which violence affects the marginal utility of housing and non-housing consumption, that is where violence is a multiplicative term to consumption or housing and acts as a local disamenity for workers. The results are available upon request, yet they are not reported to save space.

[61]Tests on the sensitivity of the results to changes on the parameter values suggest that the magnitude of the effects changes with different values of β. Nevertheless, the order of the effects is always the same as long as violence reduced the marginal utility of consumption. Specifically, the welfare effects of homicide rates are always bigger than the effects identified for real income, and blue-collar workers are two times more responsive to violence than their white-collar counterparts.

market prices according to equation (1.6), I find that the indirect effect of homicide rates account for the majority of the total welfare losses induced by violence.[62]

Additionally, my results suggest that the elasticity of welfare with respect to violence (i.e, -0.46) is at least twice as big as the elasticity of aggregate production on violence (i.e., 0.22). Thus, by only considering the effects of violence on GDP, the incidence of violence is underestimated. Hence, welfare effects are more informative because they incorporate the indirect costs of violence, caused by changes in market prices.

1.6.3 Willingness to Pay for a Reduction in Violent Crime

Following Just et al. (2005), Fleurbaey (2009), and Fleurbaey and Blanchet (2013), workers' willingness to pay for a reduction in violence could be approximated by solving:

$$V(P_0, (w_0 + T), r_0, v_0) = V(P_1, [(w_1 + T) - A], r_1, v_1) \tag{1.14}$$

where 0 and 1 represent two municipalities such that $v_0 > v_1$ and A represents the amount of income taken away from a worker to restore his original welfare level. It is a measurement of the willingness to pay to reduce violence from v_0 to v_1. Given the indirect utility function described in equation (1.2), A can be estimated as:

$$A = (w_1 + T) - (w_0 + T)[\frac{P_1}{P_0}]^{\alpha}[\frac{r_1}{r_0}]^{1-\alpha}[\frac{v_1}{v_0}]^{1-\beta} \tag{1.15}$$

The results of this exercise are presented in Figure 1.8.[63] The graph presents

[62] However, the estimates are not including the costs of human lives lost.

[63] I use the mean values of the observed variables described in Appendix E and the elasticities estimated in previous sections to recover the implied wages, prices, and rents for each value of homicide rates. Thus, I observe a different value of wages, rents and local prices for each level of violence. I combine these values with the estimates for α and β obtained following the

the percentage of income that a worker is willing to give up to reduce homicide rates to 1 per 100,000 inhabitants.[64] For example, the figure shows that at a value of 20 homicides per 100,000 people, workers' are willing to pay 27% of their income to reduce violence to 1 homicide per 100,000 inhabitants.

The figure shows that both white- and blue-collar workers have similar willingness to pay to reduce violent crime when violence is low. However, as violent crime increases, blue-collar workers have a higher willingness to pay, relative to white-collar workers. This gap eventually converges to a difference of around 20% of income. The figure shows that when violent crime is higher than 60 homicides per 100,000 people, blue- and white-collar workers are willing to pay 50% and 30% of their income to reduce violent crime, respectively. For the Colombian case, where homicide rates were 32.2 per 100,000 people in 2012, the estimates suggest that workers will be willing to pay on average 33.5% of their income to have homicide rates drop to 1 per 100,000 people.

In sum, blue-collar workers are willing to pay a higher percentage of their income to reduce violence relative to white-collar workers. This occurs because blue-collar workers do not receive a wage compensation when violence is high, but still they face higher living costs. Moreover, higher mobility costs for blue-collar workers and worst outside options for this population may be a relevant driving factor of this result. This suggest that collection of resources to reduce violence may be more problematic in areas with more violence given blue-collar workers have lower resources. This points to the importance of international aid to support countries that face very high levels of violence and also may partly explains the persistence of violence in poor countries.

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methodology presented in Appendix F. I calculate A fixing homicide rates in municipality 1 at 1 homicides per 100,000 inhabitants (i.e., $v_1 = 1$) and allowing the value of v_2 to vary between 2 and 70 homicides per 100,000 inhabitants.

[64] The willingness to pay to reduce homicide rates to zero, is not presented because in that case the second term of equation (1.15) is zero, which implies that workers are willing to give their whole income to be in that situation, which in itself is not a very useful result.

1.7 Robustness Checks

My estimates are valid as long as the exclusion restriction is satisfied. In the context of equations 1.9 and 1.2 this occurs if $E[\epsilon_{jmt} PC_{mt} | k_j, g_t] = 0$. Because the estimates include fixed effects by firm (or municipality) and year, the identification is not threatened by static differences between areas with different political competition or by aggregate time trends. A violation of the exclusion restriction will only occur if there are time-varying covariates correlated with the U.S. international anti-drug expenditures, that have differential effects within areas with different political competition. For example, the exclusion restriction would be violated if when U.S. international anti-drug expenditures are high, the local or central governments change their behavior, crowding-out other expenditures in different proportions in areas with different political competition.

I address these concerns by showing no correlation between the instrument and the behavior of local and central governments. Table 1.13 presents the results of a regression of the municipal income or expenditures (as a total and by type) in the instrument, which suggests no correlation of the instrument with behavior of the municipal government. Additionally, I repeat the same exercise on the transfers send by the central government to each municipality (as a total and by type) in Table 1.14. The results also suggest no correlation of the behavior of the central government with the instrument.

To present further evidence on the validity of the exclusion restriction, I control for 45 covariates available by municipality in the final estimates and find no sensitivity of the results. The covariates can be grouped into: i) demographics (e.g., population by sex and age and interactions between these variables), ii) public income (e.g., tax and non-tax income collected by municipalities and by type), iii) public expenditures, and iv) other variables (e.g., school enrollment and rain). A detailed list of the 45 covariates used as controls is presented in Appendix D. They comprise all the information available at the municipality level. The estimates including the controls are presented in Tables 1.3, 1.4, 1.5, and 1.9.

1.7.1 Ruling Out Differential Time Pre-trends

Another threat to the identification strategy is the existence of time pre-trends between areas with different political competition that may explain the effects observed today. I address this concern in Figure 1.9 by showing there are no systematic differences in population growth between areas with different political competition in 1946. For this purpose, I use information from the population censuses of 1912, 1918, 1928, and 1938.[65] This is a strong test, since no differences in population growth will indicate no comparative advantages of living in one of these areas, assuming small mobility restrictions.

I also check for differences in time pre-trends on ten other covariates available between 1940 and 1945 by municipality.[66] For this purpose, I run a regression of each of these covariates onto the political competition index and year interactions. In the absence of pre-time trends, these interactions should not be significant. Table 1.15 presents the results, which confirm the expected behavior. These findings are not surprising. Specifically, historians have pointed out that political violence around 1946 was not correlated to socio-economic or geographic characteristics. For instance, after compiling evidence on the causes of *La Violencia*, Guzman et al. (2006) mention that:"...the violence during those years did not respect race or economic status, it took place in regions of minifundia or latifundia, among the prosperous and the miserable, in deserts and plains, and in the valleys and mountains."

[65]The data was digitized from the *Anuarios de Estadística General* collected by the *Contraloría General de La República* and published in 1932 and 1946 (see DCG, 1932 and DCG, 1946).

[66]They were also digitized from *Anuarios de Estadística General* collected by the *Contraloría General de La República.*

1.8 Conclusions

This paper studies the effects of violent crime on local markets. For this purpose, I exploit the annual and municipal variation of homicide rates in Colombia between 1995 and 2010, and employ rich and unique firm-level and consumer food prices panel data. I instrument for homicide rates using the interaction between a political competition index for 1946 and U.S. international anti-drug expenditures. The utilization of the index is motivated by ample historical evidence suggesting that the current violence spell originated in a previous violent episode that took place between 1948 and 1958–i.e., *La Violencia*. As a consequence of this episode, illegal armed groups were created and first located in areas with higher political competition, proliferating all forms of violence, and dis-empowering the local governments. Consequently, my variation comes from the fact that when security conditions improve across the country, areas with higher political competition in 1946 (namely areas with more contested elections), reduced violence more proportionally when security transfers from the U.S. were higher.

I find that firms respond to violent crime by increasing their output prices. Hence, areas with higher violent crime also have higher non-housing living costs. Additionally, I find that when homicide rates increase workers leave, which results in lower housing rents and a small wage increase; however the wage increase is statistically significant only for white-collar workers. Empirical evidence suggests that higher mobility restrictions for workers of lower skill prevent their wages from rising. Altogether, when violence increases 10%, real income for blue- and white-collar workers decreases 1.3% and 0.6%, respectively. Additionally, higher input costs, higher wages, and lower local demand (through an increase in workers migration) lead firms to reduce their average production, and ultimately a few firms exit the market.

By combining the estimated elasticities with a theoretical model I find that when homicide rates increase, blue-collar workers' welfare losses are two times as high as the ones experienced by white-collar workers. This points to a relevant channel through which higher violence reinforces the inequality: if intense violence increases living costs, and wages are only partially compensated for white-collar workers, violence increases inequality, potentially fueling further social unrest and violence.

Moreover, I find that blue-collar workers are willing to pay a higher percentage of their income to reduce violence relative to white-collar workers; blue-collar workers' wage does not increase when violence is high, but still they face higher living costs. This suggest that collection of resources to reduce violence may be more difficult in areas with more violence, given the poorest workers are the ones willing to pay more. This points to the importance of international aid to support countries that face very high levels of violence.

In sum, I find that reductions in violent crime have large economic returns. A back-of-the-envelope calculation suggests that the 48% decline on homicide rates that took place between 1995 and 2010 in Colombia, increased aggregate production by 8.1% and worker's welfare 22.5%. However, my estimates are a lower bound of the total social costs of violent crime. Specifically, my estimates do not measure the costs of violent crime on mortality. They only account for the effects of violent crime on the population that survives violent episodes. Other studies have dealt with the mortality costs associated with violence. For instance, by using cross country data for 73 countries Soares (2006) estimates that, on average, one year of life expectancy lost to violence is associated with a yearly social cost of 3.8% of GDP. The author estimates that the health dimension of the welfare costs of violence corresponds to a yearly value of 9.7% of the Colombian GDP.

Additionally, this paper can only identify the short term local effects of violence. It does not account for the long-term effects of violent crime on foreign direct investment as studied by Pshiva and Suarez (2006). This type of analysis is constrained by the unavailability of micro data on domestic or foreign direct investment. Yet, violence reduction are expected to have a positive impact on the country's risk perception, which in turn, should spike investment.

Despite the fact that this paper uses unique and rich data on firms' and consumer food prices, there are still some limitations to the data. My estimates mainly deal with the effects of violent crime on the most populated cities of the country where the majority of the economic activity is concentrated (317 municipalities in Colombia). Yet, there is no available data to asses the effects of violent crime on rural areas, which restricts the analysis of the general equilibrium effects of violent crime in Colombia. In addition, there is no data on the extent in which violence increases informality since there is no data for the size of this sector. This is a relevant constraint, specially for developing countries were the size of the informal sector is so significant. For instance, according to estimates of the Colombian statistical department in the last 10 years, approximately 50% of all the Colombian employed population was informal.

Next steps in these research agenda include identifying the main determinants on the global drop on violence, which corresponds to one of the most significant developments of humanity (Pinker, 2011; Goldstein, 2011; and HSR, 2011).[67] In addition, fruitful insights may be gained by testing the results of this paper on a developed country.

[67]The number of people killed in battle has dropped by a thousandfold over the last centuries (Pinker, 2011). Similarly, homicide rates in Europe, the only continent with available data from the beginning of the millennium, declined from 100 to 0.8 per 100,000 people between the year 1200 and 2000 (Eisner, 2003). The violence reduction has been more pronounced in the second half of the 20th century, both in the number and intensity of international wars and internal conflicts (Pinker, 2011 and Goldstein, 2011). In the 1950s, there were around six international wars fought per year, with approximately 20,000 people killed on average per year. In contrast, since the beginning of the 21st century, there was only one war per year and the number of individuals killed fell to 3,000 people/year. Moreover, since the end of the Cold War, the number of civil conflicts has declined sharply, and between 1970 and 2008, the number of battle deaths of countries with civil wars fell 90% (HSR, 2011). Global homicide rates, which have only been consistently measured since 1995, have steadily decreased to 8.9 per 100,000 inhabitants for 2011.

Table 1.1: Violent Crime in Colombia Relative to Other Regions of the World

Intentional Homicide rate per 100,000 people (average)			
Region	1995-2000	2001-2005	2006-2010
Latin America	20.39	22.97	19.26
North America	6.82	6.92	7.39
Eastern Africa	5.48	11.18	4.66
Northern Africa	1.75	5.35	1.20
Southern Africa	38.76	28.77	24.44
Asia	4.77	3.62	3.49
Europe	3.84	3.47	2.47
Oceania	1.43	2.96	3.56
Top 10% (Most Violent)	32.00	34.10	38.40
Lowest 10% (Least Violent)	0.70	0.70	0.70
Colombia	**66.29**	**57.04**	**35.98**
World Total	8.85	7.97	9.60

Note: Intentional homicide rates per 100,000 people is defined as all the unlawful deaths purposefully inflicted on a person by another person per 100,000 inhabitants. Source: Data from the Global Study on Homicides of the United Nations Office of Drugs and Crime.

Table 1.2: First Stage Regression of homicide rates on PC_{mt}

Dependent Variable: Log ($Hom\,Rate_{mt}$)			
	(1)	(2)	(3)
PC_{mt}	-0.05***		
	(0.00)		
PC_m		1.42***	
		(0.08)	
$U.S.-IAE_t$			-0.03***
			(0.00)
Firm FE	Y		Y
Year FE	Y	Y	
Clustered errors (mun)	Y	Y	Y
Partial R-squared	0.08	0.18	0.04
F-test excluded inst.	86.07	23.50	256.16
Obs.		124,247	
N. of Clusters		317	

Note: The table presents the results of the first stage regression for the specification presented in equations (1.9) and (1.10) and given by:

$$log(v_{mt}) = \theta_0 + \theta_1 PC_{mt} + k_j + g_t + u_{jmt}$$

where m represents municipality, t year, and j firm. Violent crime (v_{mt}) is measured through homicide rates per 100,000 people. PC_{mt} is defined as the interaction of the political competition index of 1946 and U.S. international anti-drug expenditures in millions of dollars of 1995 ($US-IAE_t$). Standard errors clustered at the municipality level are presented in parentheses. *** Significant at 1% level, ** Significant at 5% level, and * Significant at 10% level.

Table 1.3: Effects of Violent Crime on Firms' Real Production

Dependent Variable: Log ($Real\ Production_{jmt}$)			
	OLS	2SLS	2SLS
	(1)	(2)	(3)
Log ($Hom\ Rate_{mt}$)	-0.01***	-0.18***	-0.17***
	(0.00)	(0.03)	(0.04)
Year FE	Y	Y	Y
Firm FE	Y	Y	Y
Municipality's characteristics			Y
R-squared	0.08	0.11	0.14
Obs.		124,247	
N. of Clusters		317	
First Stage. Dependent Variable: Log ($Hom\ Rate_{mt}$)			
PC_{mt}		-0.05**	-0.05**
		(0.00)	(0.00)
F-excluded instrument		86.07	96.14
Partial R-squared		0.08	0.11

Note: The table presents the results of the specification presented in equations (1.9) and (1.10) and given by:

$$log(y_{jmt}) = \gamma_0 + \gamma_1 log(v_{mt}) + k_j + g_t + \epsilon_{jmt}$$

$$log(v_{mt}) = \theta_0 + \theta_1 PC_{mt} + k_j + g_t + u_{jmt}$$

where y_{jmt} represents real production of firm j, located in municipality m, at year t, v_{mt} is measured as homicide rates per 100,000 people, and k_j and g_t represent firm and year fixed effects. PC_{mt} is defined according to equations (1.11) and (1.12). It corresponds to an interaction of the political competition index of 1946 and U.S. international anti-drug expenditures in millions of 1995. The other covariates included as municipality's characteristics are described in Appendix D. Real values were obtained using a municipality price index with base year 1995. Standard errors clustered at the municipality level are presented in parentheses. *** Significant at 1% level, ** Significant at 5% level, and * Significant at 10% level.

Table 1.4: Effects of Violent Crime on the Total Number of Firms by Municipality

Dependent Variable: Log (Number of firms)			
	OLS	2SLS	2SLS
	(1)	(2)	(3)
Log ($Hom\,Rate_{mt}$)	-0.02**	-0.04*	-0.04*
	(0.01)	(0.02)	(0.02)
Year FE	Y	Y	Y
Mun FE	Y	Y	Y
Municipality's characteristics			Y
R-squared	0.05	0.06	0.09
Observations		4,620	
Clusters (by muncod)		308	
First Stage. Dependent Variable: $Log(Homicide\,Rate_{mt})$			
PC_{mt}		-0.05***	-0.05***
		(0.01)	(0.01)
F-test excluded instrument		15.98	16.21
Partial R-squared		0.04	0.05

Note: The table presents the results of the following specification:

$$log(N_{mt}) = \gamma_0 + \gamma_1 log(v_{mt}) + k_m + g_t + \epsilon_{jmt}$$

$$log(v_{mt}) = \theta_0 + \theta_1 PC_{mt} + k_m + g_t + u_{jmt}$$

where N_{mt} represents aggregate number of firms at municipality m, at year t, v_{mt} is measured according to homicide rates per 100,000 people, and k_m and g_t represent municipality and year fixed effects. PC_{mt} is the instrument for homicide rates and is defined according to equations (1.11) and (1.12). It corresponds to an interaction of the political competition index of 1946 and U.S. anti-drug expenditures in millions of dollars of 1995. The other covariates included as municipality's characteristics are described in Appendix D. Standard errors clustered at the municipality level are presented in parentheses. *** Significant at 1% level, ** Significant at 5% level, and * Significant at 10% level.

Table 1.5: Effects of Violent Crime on Firms' Output Prices

Dependent Variable: Log ($Real\,Output\,Prices_{jmt}$)				
	OLS	OLS	2SLS	2SLS
	(1)	(2)	(3)	(4)
Log ($Hom\,Rate_{mt}$)	0.07***	0.07***	0.51**	0.53**
	(0.02)	(0.02)	(0.22)	(0.26)
FE firm	Y	Y	Y	Y
FE year	Y	Y	Y	Y
Industry FE		Y	Y	Y
Industry-Year FE		Y		Y
Municipality's characteristics		Y		Y
R-squared	0.57	0.71	0.57	0.63
Obs.	116,468			
N. of Clusters	317			
First Stage. Dependent Variable: Log ($Hom\,Rate_{mt}$)				
PC_{mt}			-0.05***	-0.05***
			(0.01)	(0.01)
F-test excluded instrument			17.89	16.05
Partial R-squared			0.04	0.05

Note: The table presents the results of the specification presented in equations (1.9) and (1.10) and given by:

$$log(p_{jmt}) = \gamma_0 + \gamma_1 log(v_{mt}) + k_j + g_t + \epsilon_{jmt}$$

$$log(v_{mt}) = \theta_0 + \theta_1 PC_{mt} + k_j + g_t + u_{jmt}$$

where p_{jmt} represents real output prices of firm j, located in municipality m, at year t, v_{mt} is measured as homicide rates per 100,000 people, and k_j and g_t represent firm and year fixed effects. PC_{mt} is defined according to equations (1.11) and (1.12). It corresponds to an interaction of the political competition index of 1946 and U.S. international anti-drug expenditures in millions of 1995. Each observation corresponds to a plant-product-year unit. Industry fixed effects correspond to the four-digit classification of the International Standard Industry Classification (ISIC) for each product-plant-year observation. There are 115 four-digit codes and 29 departments in the sample. The other covariates included as municipality's characteristics are described in Appendix D. Real values were obtained using a municipality price index with base year 1995. Standard errors clustered at the municipality level are presented in parentheses. *** Significant at 1% level, ** Significant at 5% level, and * Significant at 10% level.

Table 1.6: Effects of Violent Crime on Firms' Input Prices

Dependent Variable: Log ($Real\,Input\,Prices_{jmt}$)				
	OLS	OLS	2SLS	2SLS
	(1)	(2)	(3)	(4)
Log ($Hom\,Rate_{mt}$)	0.03***	0.09***	0.20**	0.20**
	(0.01)	(0.00)	(0.08)	(0.08)
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Industry FE		Y		Y
Industry-Year FE		Y		Y
Other covariates		Y		Y
R-squared	0.28	0.56	0.3	0.51
Obs.	395,523			
N. of Clusters	317			
First Stage. Dependent Variable: Log ($Hom\,Rate_{mt}$)				
PC_{mt}			-0.06***	-0.06***
			(0.01)	(0.01)
F-test excluded instrument			18.28	18.72
Partial R-squared			0.05	0.05

Note: The table presents the results of the specification presented in equations (1.9) and (1.10) and given by:

$$log(p_{jmt}) = \gamma_0 + \gamma_1 log(v_{mt}) + k_j + g_t + \epsilon_{jmt}$$

$$log(v_{mt}) = \theta_0 + \theta_1 PC_{mt} + k_j + g_t + u_{jmt}$$

where p_{jmt} represents real input prices of firm j, located in municipality m, at year t, v_{mt} is measured as homicide rates per 100,000 people, and k_j and g_t represent firm and year fixed effects. PC_{mt} is defined according to equations (1.11) and (1.12). It corresponds to an interaction of the political competition index of 1946 and U.S. international anti-drug expenditures in millions of 1995. Each observation corresponds to a plant-product-year unit. Industry fixed effects correspond to the four-digit classification of the International Standard Industry Classification (ISIC) for each product-plant-year observation. There are 115 four-digit codes and 29 departments in the sample. The other covariates included as municipality's characteristics are described in Appendix D. Real values were obtained using a municipality price index with base year 1995. Standard errors clustered at the municipality level are presented in parentheses. *** Significant at 1% level, ** Significant at 5% level, and * Significant at 10% level.

Table 1.7: Effects of Violent Crime on Average Retail Food Prices

Dependent Variable: Log ($Real\,Food\,Price_{qmt}$)				
	OLS	OLS	2SLS	2SLS
	(1)	(2)	(3)	(4)
Log ($Hom\,Rate_{mt}$)	0.09**	0.1***	0.56**	0.59**
	(0.04)	(0.02)	(0.31)	(0.35)
Municipality FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Product FE	Y	Y	Y	Y
Product-Year FE		Y		Y
Other Covariates				Y
R-squared	0.31	0.51	0.48	0.43
Obs.	44,724			
N. of Clusters	53			
First Stage. Dep Variable: Log ($Hom\,Rate_{mt}$)				
PC_{mt}			-0.01***	-0.01***
			(0.00)	(0.00)
F-test excluded instrument			11.89	14.08
Partial R-squared			0.06	0.06

Note: The table presents the results of the following specification:

$$log(f_{qmt}) = \gamma_0 + \gamma_1 log(v_{mt}) + \gamma_m + \gamma_t + \gamma_q + \gamma_{qt} + \epsilon_{jmt}$$

$$log(v_{mt}) = \theta_0 + \theta_1 PC_{mt} + \theta_m + \theta_t + \theta_q + \theta_{qt} + u_{jmt}$$

where f_{qmt} represents the average retail price of product q, at municipality m, at year t, and v_{mt} is measured according to homicide rates per 100,000 people. PC_{mt} is defined according to equations (1.11) and (1.12). It corresponds to an interaction of the political competition index of 1946 and U.S. international anti-drug expenditures in real values of 1995. Each observation on this sample corresponds to the real prices of the 500 most consumed products in the 53 municipalities, located in 20 different departments. The other covariates included as municipality's characteristics are described in Appendix D. Real values were obtained using a municipality price index with base year 1995. Standard errors clustered at the municipality level are presented in parentheses. *** Significant at 1% level, ** Significant at 5% level, and * Significant at 10% level.

Table 1.8: Effects of Violent Crime on Housing Rents

Dependent Variable: Log ($Nominal\, Rents_{imt}$)				
	OLS		2SLS	
	(1)	(2)	(3)	(4)
Log (Hom R)	-0.04***	-0.05**	-0.31**	-0.38**
	(0.01)	(0.02)	(0.13)	(0.15)
Municipality Controls	Y	Y	Y	Y
Year Controls	Y	Y	Y	Y
Individual Controls: age, sex, education		Y		Y
R-squared	0.19	0.32	0.23	0.48
Obs.	22,913			

Note: The table presents the results of the following specification:

$$log(r_{imt}) = \gamma_0 + \gamma_1 log(v_{mt}) + \gamma_m + \gamma_t + \gamma_m + \Gamma' X_{imt} + \epsilon_{imt}$$

$$log(v_{mt}) = \theta_0 + \theta_1 PC_{mt} + \theta_m + \theta_t + \Theta' X_{imt} + u_{imt}$$

where r_{imt} represents the average nominal housing rents paid by individual i, at municipality m, at year t, and v_{mt} is measured according to homicide rates per 100,000 people. PC_{mt} is defined according to equations (1.11) and (1.12). It corresponds to an interaction of the political competition index of 1946 and U.S. international anti-drug expenditures in real values of 1995. The specification was estimated useing data from the National Household Surveys between 1885 and 2010. The other covariates included as municipality's characteristics are described in Appendix D. Robust standard errors are presented in parentheses. *** Significant at 1% level, ** Significant at 5% level, and * Significant at 10% level.

Table 1.9: Effects of Violent Crime on Nominal Wages

Dependent Variable: Log ($Average\,Nominal\,Wages_{jmt}$)		
	OLS	2SLS
	(1)	(2)
Log ($Hom\,Rate_{mt}$)	0.05***	0.07***
	(0.01)	(0.01)
Year FE	Y	Y
Firm FE	Y	Y
Municipality's characteristics	Y	Y
R-squared	0.41	0.42
Obs.	124,247	
N. of Clusters	317	

Note: The table presents the results of the specification presented in equations (1.9) and (1.10) and given by:

$$log(w_{jmt}) = \gamma_0 + \gamma_1 log(v_{mt}) + k_j + g_t + \epsilon_{jmt}$$

$$log(v_{mt}) = \theta_0 + \theta_1 PC_{mt} + k_j + g_t + u_{jmt}$$

where w_{jmt} represents nominal average wage of firm j, located in municipality m, at year t, v_{mt} is measured as homicide rates per 100,000 people, and k_j and g_t represent firm and year fixed effects. PC_{mt} is defined according to equations (1.11) and (1.12). It corresponds to an interaction of the political competition index of 1946 and U.S. international anti-drug expenditures in millions of dollars of 1995. The other covariates included as municipality's characteristics are described in Appendix D. Standard errors clustered at the municipality level are presented in parentheses. *** Significant at 1% level, ** Significant at 5% level, and * Significant at 10% level.

Table 1.10: Effects of Violent Crime on Nominal Wages by Type of Worker

Dep Variable: Log ($Average\,Nominal\,Wages_{jmt}$)		
	White-Collar	Blue-Collar
	2SLS	2SLS
	(1)	(2)
Log ($Hom\,Rates_{mt}$)	0.09***	0.03
	(0.01)	(0.02)
Year FE	Y	Y
Firm FE	Y	Y
Municipality's characteristics	Y	Y
R-squared	0.33	0.34
Obs.	40,048	
N. of Clusters	206	
First Stage. Dep Variable: Log ($Hom\,Rates_{mt}$)		
PC_{mt}	0.89***	0.86***
	(0.05)	(0.06)
F-excluded instrument	17.09	15.78
Partial R-squared	0.08	0.07

Note: The table presents the results of the specification presented in equations (1.9) and (1.10) and given by:

$$log(w_{jmt}) = \gamma_0 + \gamma_1 log(v_{mt}) + k_j + g_t + \epsilon_{jmt}$$

$$log(v_{mt}) = \theta_0 + \theta_1 PC_{mt} + k_j + g_t + u_{jmt}$$

where w_{jmt} represents nominal average wage of firm j, located in municipality m, at year t, v_{mt} is measured according to homicide rates per 100,000 people, and k_j and g_t represent firm and year fixed effects. The other covariates included as municipality's characteristics are described in Appendix D. Standard errors clustered at the municipality level are presented in parentheses. *** Significant at 1% level, ** Significant at 5% level, and * Significant at 10% level.

Table 1.11: Lower Mobility For Workers with Lower Levels of Skill

Dependent Variable = 1 if internal migration between 2000 and 2005				
	Workers in Urban Areas			
	All workers		25> years	30> years
	(1)	(2)	(3)	(4)
Education (Years)	0.001***			
	(0.000)			
Secondary Education (complete)		0.008***	0.010***	0.004***
		(0.001)	(0.002)	(0.001)
Homicide Rate at municipality of origin	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Education* Homicide rate in mun of origin	0.0002***			
	(0.000)			
Secondary Education x Homicide rate in mun of origin		0.001***	0.001***	0.001***
		(0.000)	(0.000)	(0.000)
Controls: Gender, Age	Y	Y	Y	Y
Regional Controls (by department)	Y	Y	Y	Y
Pseudo R-squared	0.253	0.207	0.133	0.091
Obs.	692,700		572,957	475,549
Base Probability of Migration	0.099		0.092	

Note: The table reports the marginal effects of a probit model using a 10% extract from the Colombian population census of 2005. The municipality of origin corresponds to the location of the individual in the year 2000. The data was obtained from IPUMS. Robust standard errors are reported in parentheses. *** Significant at 1% level, ** Significant at 5% level, and * Significant at 10% level.

Table 1.12: Effects of Violent Crime on Workers' Welfare (Measured as utility of consumption)

Workers	Elasticity Welfare-Violence	Decomposition of Total Effects	
		Market Effects	Direct Disutility
All Workers	-0.46***	95.72%	4.28%
	(0.05)		
White-collar	-0.28*	94.65%	5.35%
	(0.05)		
Blue-collar	-0.63***	96.41%	3.59%
	(0.05)		

Note: the table presents the estimates of equation (1.6) where the partial derivatives on the effects violent crime to market prices were derived from the previous estimates and the mean values of each variable are reported in Appendix E. The values of α and β were set to 0.82 and 0.98 according to the identification strategy described in Appendix F. Standard errors were computed using the delta method. *** Significant at 1% level, ** Significant at 5% level, and * Significant at 10% level.

Table 1.13: Ruling out the Correlation between the Instrument and the Municipal Government Behavior

	Dependent Variables [expressed in real billions of pesos (1995=100)]							
	Income (Inc.)	Tax-Inc.	Non-tax Inc.	Expenditures (Exp.)	Admin. Exp.	Debt Exp.	*'Regalias'*	Fiscal Balance
PC_{mt}	0.005	-0.002	0.002	0.001	-0.001	0.002	-0.003	0.001
	(0.010)	(0.007)	(0.011)	(0.006)	(0.003)	(0.007)	(0.005)	(0.002)
Mun FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
R-squared	0.944	0.942	0.943	0.944	0.944	0.944	0.942	0.941
Clusters				317				
Obs.				4,755				

Note: the table presents a regression of municipal public expenditures (as a total and by type) on the instrument (PC_{mt}). PC_{mt} is defined according to equations (1.11) and (1.12), and corresponds to the interaction of the political competition index of 1946 (PC_m) and U.S. international anti-drug expenditures in millions of dollars of 1995 ($US - IAE_t$). *'Regalias'* represent the income collected through taxes on mineral and oil exploitation. Standard errors clustered at the municipality level are presented in parentheses. *** Significant at 1% level, ** Significant at 5% level, and * Significant at 10% level.

Table 1.14: Ruling out the Correlation between the Instrument and the Central Government Behavior

	Dep. Variable: Transfers from the Central Government to the Municipalities [Real billions of pesos (1995=100)]			
	Education Transfers	Health Transfers	Other Purposes	Total Transfers
PC_{mt}	0.000	-0.002	-0.013	0.000
	(0.002)	(0.005)	(0.047)	(0.001)
Mun FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
R-squared	0.960	0.960	0.960	0.960
N. of Clusters	317			
Obs.	4,755			

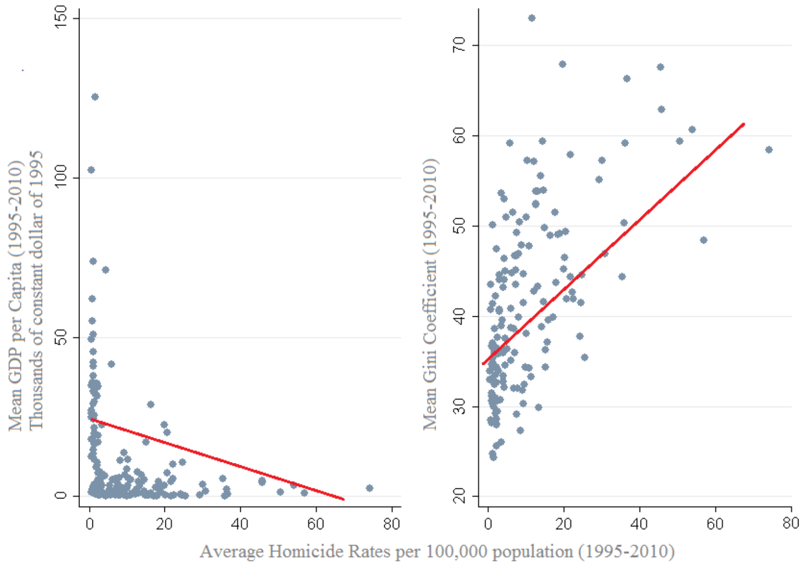
Note: the table presents a regression of the central government transfers to municipalities (as a total and by type) on the instrument (PC_{mt}). PC_{mt} is defined according to equations (1.11) and (1.12), and corresponds to the interaction of the political competition index of 1946 (PC_m) and U.S. international anti-drug expenditures in millions of dollars of 1995 ($US - IAE_t$). Standard errors clustered at the municipality level are presented in parentheses. *** Significant at 1% level, ** Significant at 5% level, and * Significant at 10% level.

Table 1.15: Excluding Differential Time Pre-trends between Areas with Different Political Competition Index Before 1946

	Dependent Variables									
	GPI	Rice Price	Meat Price	Egg Price	Milk Price	Potatoes Price	Coffee Price	Exports	Imports	Cattle Value
$PC_m * I(1941)_t$							0.04	-2.45	0.53	
							(0.23)	(3.70)	(3.90)	
$PC_m * I(1942)_t$	-0.26	0.05	-0.18	0.28	-0.15	-0.16	-0.15	3.84	4.3	
	(0.2)	(0.12)	(0.23)	(0.23)	(0.18)	(0.12)	(0.19)	(3.69)	(3.90)	
$PC_m * I(1943)_t$	0.12	0.02	-0.43	0.44	0.37	-0.23	0.08	-3.64	-1.09	
	(0.15)	(0.14)	(0.29)	(0.36)	(0.23)	(0.26)	(0.19)	(3.23)	(3.33)	
$PC_m * I(1944)_t$	-0.03	0.004	-0.52	0.88	-0.04	-0.09	-0.003	-3.12	-3.15	
	(0.13)	(0.18)	(0.43)	(0.33)	(0.29)	(0.14)	(0.18)	(3.16)	(3.33)	
$PC_m * I(1945)_t$	0.16	-0.05	0.11	0.41	0.02	0.05	-0.17	-3.67	-3.07	-2.18
	(0.22)	(0.22)	(0.65)	(0.39)	(0.28)	(0.24)	(0.18)	(3.15)	(3.33)	(3.10)
High FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
R-squared	0.71	0.66	0.57	0.58	0.21	0.69	0.97	0.07	0.068	0.07
Years available	1941-1945	1941-1945	1941-1945	1941-1945	1941-1945	1941-1945	1940-1945	1940-1945	1940-1945	1944-1945
Obs.	420	420	420	420	420	420	114	63	64	1269

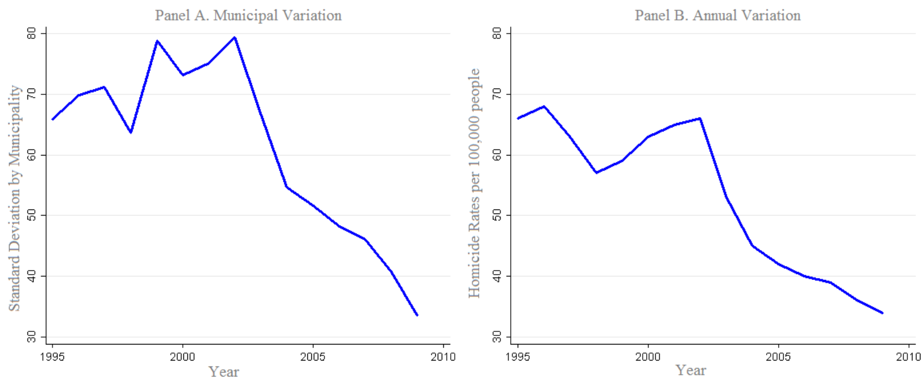
Note: the table presents a regression of the dependent variables observed before 1946 on the interaction of political competition in 1946 (PC_m) and dummy variables by years. GPI stands for General Price Index. Exports and Imports are expressed in millions and cattle value in hundreds of Colombian pesos. Standard errors clustered at the municipality level are presented in parentheses. *** Significant at 1% level, ** Significant at 5% level, and * Significant at 10% level.

Figure 1.1: The Poorest and Most Unequal Countries are also the Most Violent



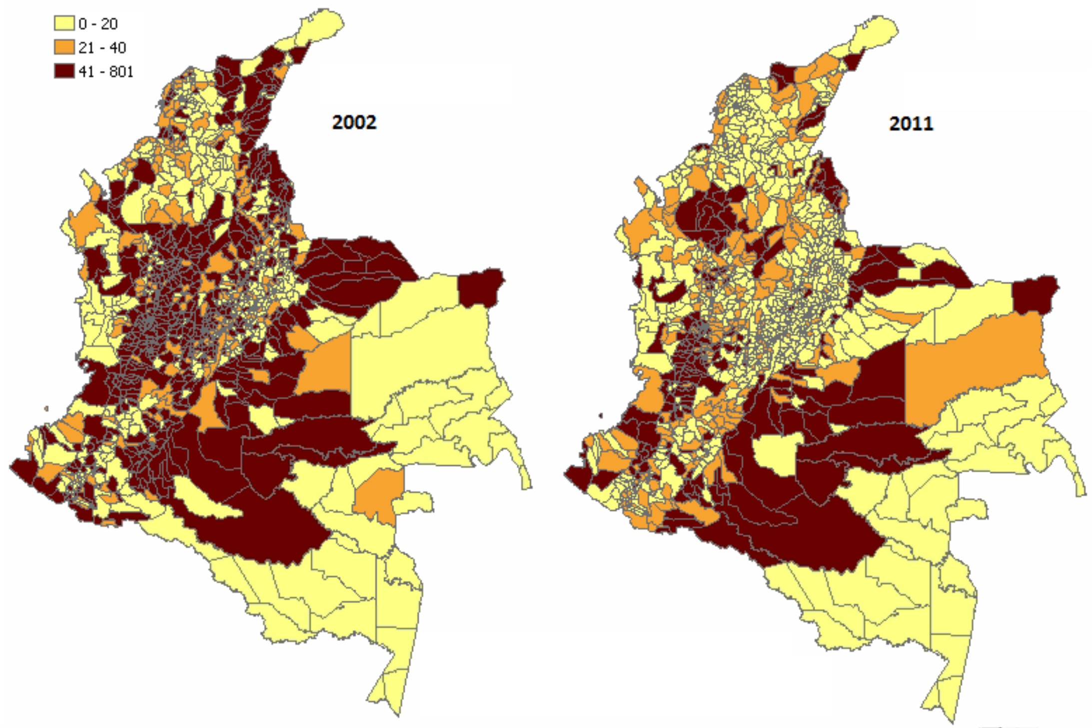
Note: The figure presents the correlation between: (i) the average GDP per capita and the average homicide rates (left panel), and (ii) the Gini coefficient and the average homicide rates (right panel). Averages were estimated between 1995 and 2010 for 194 countries of the world. Each dot in the figure represents a country and the line presents the fitted values of a regression of GDP per capita (left panel) and the Gini coefficient (right panel) on homicide rates. homicide rates come from the Global Homicide Study of the United Nations Office of Drugs and Crime. GDP per capita comes from the World Development Indicators of the World Bank. Finally, the Gini coefficient corresponds to a standardized index produced by the World Bank data from eight original sources: Luxembourg Income Study (LIS), Socio-Economic Database for Latin America (SEDLAC), Survey of Living Conditions (SILC) by Eurostat, World Income Distribution (WYD; the full data set is available here), World Bank Europe and Central Asia dataset, World Institute for Development Research (WIDER), World Bank Povcal, and Ginis from individual long-term inequality studies.

Figure 1.2: Municipal and Annual Variation of Homicide Rates in Colombia



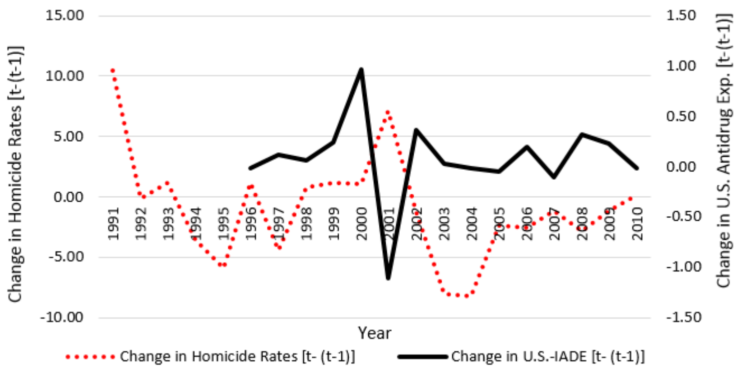
Note: The left panel presents the standard deviation of homicide rates per 100,000 people across municipalities and the right panel presents the time evolution of homicide rates per 100,000 people. Source: Observatory for Human Rights of the Colombian Vice Presidency.

Figure 1.3: Geographic Distribution of Homicide Rates by Municipality in 2002 and 2011

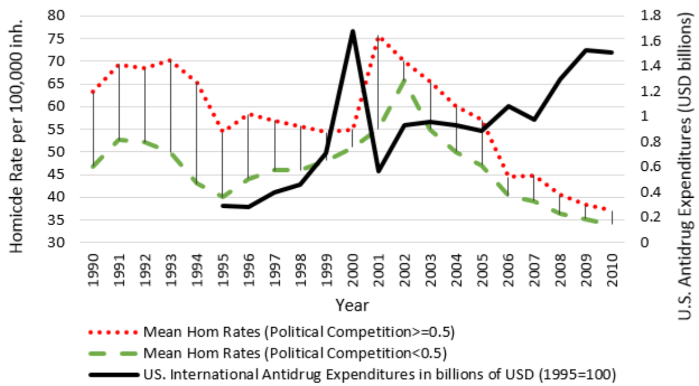


Note: The maps were elaborated with data from the *Instituto Colombiano Agustín Codazzi*, the Colombian geography agency. Data on homicide rates per 100,000 inhabitants comes from the Observatory for Human Rights of the Colombian Vice Presidency.

Figure 1.4: U.S. International anti-drug Expenditures and Homicide Rate in Areas with Different Political Competition in 1946



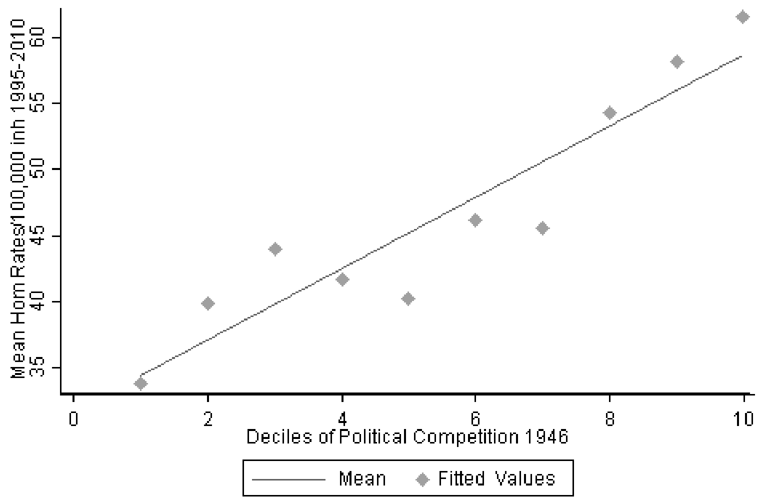
Panel (A)



Panel (B)

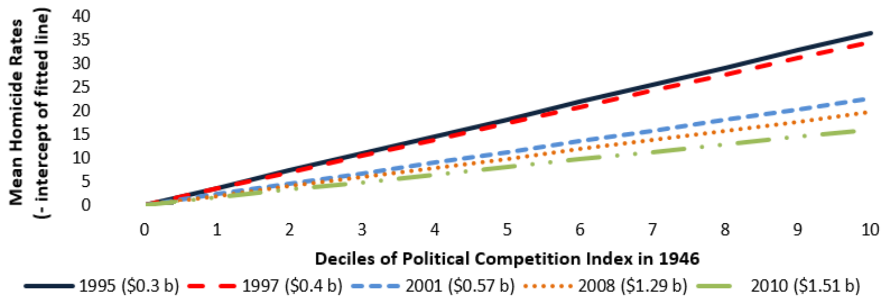
Note: Panel (A) compares the time evolution of changes in real U.S. international anti-drug expenditures and changes in homicide rates per 100,000 inhabitants. It suggests that both variables have a negative correlation. Panel (B) presents the time evolution of real U.S. international anti-drug expenditures and the average homicide rate per 100,000 inhabitants in areas with different levels of political competition in 1946. It suggests that U.S. transfers in security induced a reversal of fortune between areas with different levels of political competition in 1946. In particular, violent crime is reduced more quickly in areas with higher political competition for 1946 where illegally armed groups were originally located, and hence, where local governments were less empowered today.

Figure 1.5: Cross Section Correlation between Political Competition in 1946 and Homicide Rates Today



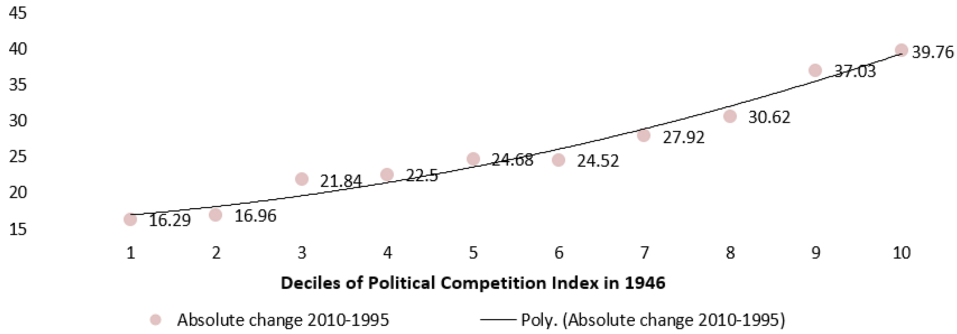
Note: The figure presents fitted values of a linear regression of mean homicide rates per 100,000 inhabitants for all municipalities between 1995 and 2010 on deciles of the political competition index in 1946.

Figure 1.6: Cross Section Correlation between Political Competition in 1946 and Homicide Rates Today for Years with Different U.S. anti-drug International Expenditures



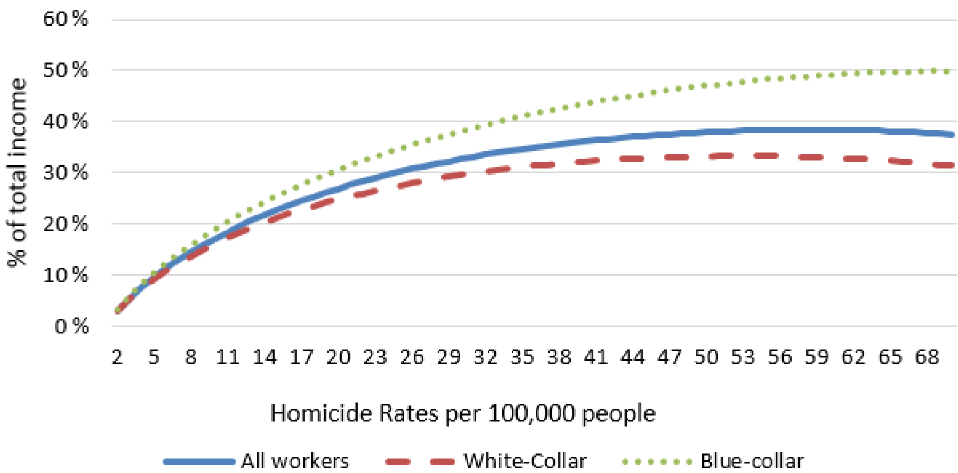
Note: The figure presents fitted values of a linear regression of homicide rates on deciles of the political competition index for years with different U.S. international anti-drug expenditures. The intercept was subtracted from each fitted line for comparison purposes. The level of U.S. international expenditures are reported in the label in parentheses. The figure suggests that: (i) there is a positive correlation between past political competition and homicide rates today for all years, and (ii) areas with higher political competition reduce violent crime more quickly when U.S. transfers are higher.

Figure 1.7: Areas with Higher Political Competition Reduced Violent Crime more Proportionally when U.S. anti-drug Expenditures are Higher



Note: The figure reports the absolute value of the change in homicide rates between 1995 (year with the lowest U.S. anti-drug International Expenditures) and 2010 (year with the highest U.S. anti-drug International Expenditures). They figure suggests that the areas with higher past political competition reduced violent crime more quickly when U.S. international anti-drug expenditures are higher.

Figure 1.8: Workers' Willingness to Pay for a Decline in Violence

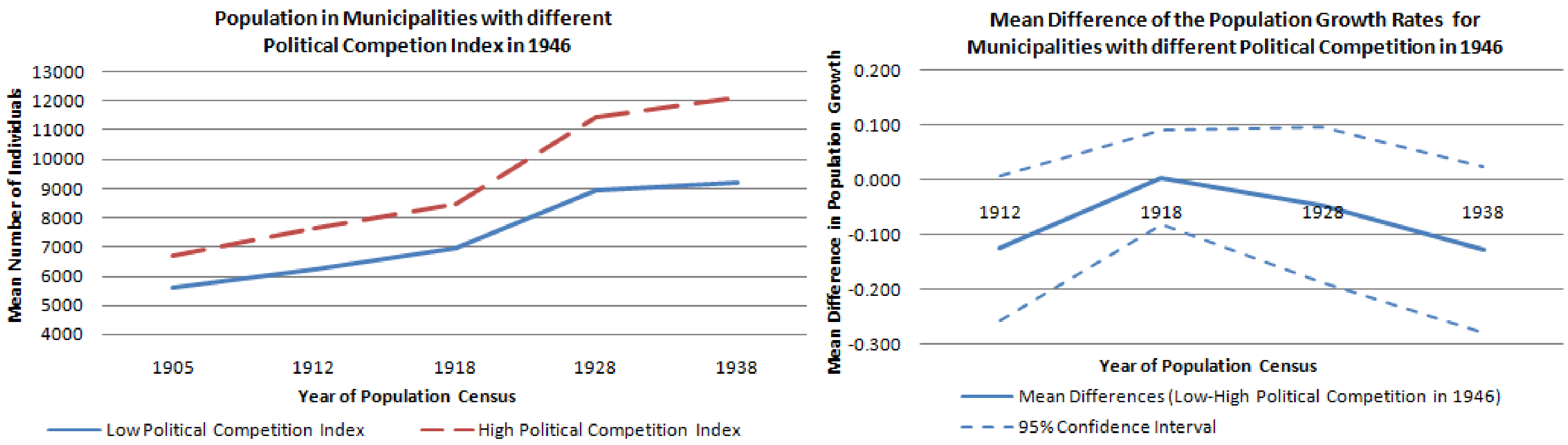


Note: The figure presents the estimates of the share of income that a worker with a level of violent crime reported in the horizontal axis will be willing to pay to have homicide rates equal to 1 per 100,000 people. The estimates are carried out using equation (1.15), which is derived by solving for T in the following expression:

$$V(P_0,(w_0+T),r_0,v_0) = V(P_1,[(w_1+T)-A],r_1,v_1)$$

where 0 and 1 represent two municipalities such that $v_0 > v_1$. In particular, for this case $v_1 = 1$. Moreover, $V(.)$ represents the indirect utility of the worker, P stands for the non-housing prices, $w+F$ represent labor and non-labor income, and r stands for housing rents.

Figure 1.9: Population Parallel Time Pre-trends Across Areas with Different Political Competition



Note: All municipalities with a political competition index higher than the median were classified as areas with high political competition index and vice versa. The information was digitized from the information on the population census available at the *Anuarios de Estadística General* collected by the *Contraloría General de La República* published between 1932 and 1946.

1.9 Appendices

A- Descriptive Statistics

Table A.1: Descriptive Statistics for the Annual Manufacturing Survey

	1995		2010	
Variable	Mean	St. Deviation	Mean	St. Deviation
Age (years)	18.69	14.90	19.76	15.50
Multiplant	0.07	0.25	0.07	0.26
N of employees	82.08	180.51	66.79	155.82
Share male (% of total employees)	0.62	0.27	0.62	0.25
Real Average Monthly Wage (USD)	419.75	324.26	402.53	253.27
Number of inputs used	13.02	9.26	12.94	13.55
Number of final products	5.53	3.45	4.91	4.34
Real Annual sales (millions of USD)	8.20	4.40	7.90	8.05
Labor Share	0.34	0.19	0.32	0.19
Share blue (% total employees)	–	–	0.62	0.24
Exports (% of n. of plants)	–	–	0.21	0.41
Obs. (N. of Plants)	7,909		9,944	

Source: *Encuesta Anual Manufacturera* [Annual Manufacturing Survey] collected by the *Departamento Nacional de Estadística*–the Colombian statistical agency. The survey includes all manufacturing firms with more than 10 employees, with detailed information on all prices and physical quantities (valued at factory-gate prices) on inputs and outputs used/produced by each firm. The data is available for the period between 1995 and 2010. Note: * Exchange rates correspond to the average annual rates published by the Colombian central bank (it takes a value of $906 and $1912 Colombian pesos per U.S. dollar in 1995 and 2010, respectively).

B- Literature on Rent-Violence Elasticity

An exhaustive review of the literature on the effects of crime on housing prices identifies 15 studies that present point estimates of the elasticity of rents with respect to crime. They are cited in the table below:

Authors	Year of publication	Location	Elasticity
Ihlanfeldt and Mayock	2010	Miami	-0.15
Naroff et al.	1980	Boston	-1.67
Burmel	1988	Chicago	-0.1
Gibbons	2004	London	-1
Pope	2008	Florida	-2.3
Linden and Rockoff	2008	North Carolina	-4
Buonnano et al.	2012	Spain	-1.27
Thaler	1978	Rochester	-3
Bowes and Ihlanfeldt	2001	Atlanta	-3
Hellman and Narrof	1979	Boston	-0.63
Clark and Cosgrove	1990	Mulitple locations	-0.125
Schwartz, Susin, and Voicu	2003	New York	-0.12
Ceccato and Wilhelmsson	2011	Sweeden	-0.04
Braakmann	2012	England and Wahles	-2%
Pope and Pope	2012	US, whole country	-0.35%

C- Quantile Regression Estimates

To obtain the estimates of Panel B on Table 6 I combine the methodologies by Buchinsky (1996) to control for selection and by Lee (2007) to control for endogeneity. For all the steps where the inclusion of a power series of an inverse Mills ratio was necessary it was approximated through a second order polynomial following Staneva et al. (2010). Specifically, the following process was used:

1. Estimate the probability of exit and entry through a probit model. For the entry equation the independent variables include the three instruments (i.e., the dummy for CAEs, the interaction between the chambers of commerce location and the days needed to close a business, and the Bartik instrument for violence), lagged rural and urban population (by municipality), and lagged real per capita GDP (by department) obtained from DANE, the Colombian national statistical agency. The exit equation includes the same independent variables plus the lagged values of sales. Define the estimates of this step as $\hat{\beta}^o$.

2. Use the semiparametric least-square estimator used by Buchinsky (1998) and first formulated by Ichimura (1993) and given by:

$$\hat{\beta} = Arg\, Min_{\beta} \frac{1}{n} \sum_{i=1}^{n} (d_i - \hat{E}(d_i|X\beta)^2] \tag{1.16}$$

   to obtain the estimates for the coefficients in the selection equations, where $\hat{E}(d_i|X, \beta)$

$$\hat{E}(d_i|Z, \beta) = \frac{\sum_{j \neq i} y_j k((X_i'\beta - X_j'\beta)/h_n)}{\sum_{j \neq i} k((X_i'\beta - X_j'\beta)/h_h)} \tag{1.17}$$

   where $k(.)$ is the truncated normal kernel function. In the first round the truncation point is set at the standard errors of $X'\hat{\beta}^o$ (the estimates of step 1), and the kernel bandwidth is set to $n^{-1/5}$ to obtain $\hat{\beta}^1$.

3. Reset the symmetric truncation point to the standard errors of $X'\hat{\beta}^1 n^{-1/3}$ and h_n $X'\hat{\beta}^1 n^{-1/5}$ and solve again (1.16) to obtain the final $\hat{\beta}$.

4. Predict $X'\hat{\beta}$ and obtain the inverse Mills ratio of each equation.

5. Estimate the quantile regression of equation (12) including a second order polynomial of the inverse Mills ratio predicted for the entry and exit selection equations and predict the residuals.

6. Predict the inverse Mills ratio of the residuals of the previous step.

Dep Variable: Log ($Real\, Production_{jmt}$)					
	0.1	0.25	0.5	0.75	0.9
Log ($Hom\, R_{mt}$)	-0.25***	-0.24***	-0.24***	-0.25***	-0.24***
[Boot-clust-er]	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Year dummies	Y	Y	Y	Y	Y
Selection Correction	Y	Y	Y	Y	Y
Pseudo R-squared	0.12	0.11	0.10	0.11	0.10
Obs.			124247		
N. of Clusters			317		

7. Estimate the quantile regression of equation (11) including the second order polynomials for the exit inverse Mills ratio, entry inverse Mills ratio, and residuals inverse Mills ratio from the last step.

8. Estimate the standard errors by bootstrap clustering by municipality.

The estimates of this process are reported in the table. They suggest uniform negative effects of violence on firms across the distribution function of real production.

D- List of Covariates

Number	Variables	Available	Source
1	Population 11 to 20 years	1995-2010	DANE
2	Population 21 to 30 years	1995-2010	DANE
3	Population 31 to 40 years	1995-2010	DANE
4	Population 41 to 50 years	1995-2010	DANE
5	Population 51 to 60 years	1995-2010	DANE
6	Population 61 to 70 years	1995-2010	DANE
7	Population 71 to +years	1995-2010	DANE
8	Male Population	1995-2010	DANE
9	Male-11 to 20 years Pop	1995-2010	DANE
10	Male-21 to 30 years Pop	1995-2010	DANE
11	Male-31 to 40 years Pop	1995-2010	DANE
12	Male-41 to 50 years Pop	1995-2010	DANE
13	Male-51 to 60 years Pop	1995-2010	DANE
14	Male-61 to 70 years Pop	1995-2010	DANE
15	Male-71+ years Pop	1995-2010	DANE
16	Tax Income	1995-2010	DNP
17	Non-Tax Income	1995-2010	DNP
18	Transfers Income	1995-2010	DNP
19	Capital Income	1995-2010	DNP
20	Income from 'Regalias'	1995-2010	DNP
21	Gov. Operational Expenditures	1995-2010	DNP
22	Debt Interest Expenditures	1995-2010	DNP
23	Other Expenditures	1995-2010	DNP
24	Capital Investment	1995-2010	DNP
26	Education Inv.	1995-2010	DNP
27	Health Inv.	1995-2010	DNP
28	Housing Inv.	1995-2010	DNP
29	Other Public Services Inv.	1995-2010	DNP
30	Transportation Inv.	1995-2010	DNP
31	Cultural Inv.	1995-2010	DNP
32	Agricultural Inv.	1995-2010	DNP
33	Environmental Inv.	1995-2010	DNP
34	Justice Inv.	1995-2010	DNP
35	Recreational Inv.	1995-2010	DNP
36	Vulnerable groups Inv.	1995-2010	DNP
37	Disaster prevention Inv.	1995-2010	DNP
38	Education Inv.*Public Services Inv	1995-2010	DNP
39	Education Inv*Justice Inv	1995-2010	DNP
40	Education Inv* Health Inv	1995-2010	DNP
41	Public Debt	1995-2010	DNP
42	Rain	1995-2010	CEDE
43	Primary Enrollment	1995-2010	Ministry of Educ
44	Secondary Enrollment	1995-2010	Ministry of Educ

E- List of Parameter Values Used for Welfare Estimation

Variables	Values	Source	Period
% change in homicide rates 1995-2010	-0.48	Human Rights Observatory	1995-2010
Elasticity r and v	-1.16	Review of studies	–
α	0.80	National Household Survey	2000-2010
β	0.98	National Household Survey	2000-2010
v	51.86	Human Rights Observatory	1995-2010
r	5040.96	Colombian Statistical Department	1995-2010
P	238.87	Colombian Statistical Department	1995-2010
l	192.00	National Household Survey	2000-2010
w	6085.00	National Household Survey	2000-2010
F	91920.00	National Household Survey	2000-2010
w (white-collar)	8214.75	AMS and National Household Survey	1995-2010
w (blue-collar)	4868.00	AMS Household Survey	1995-2010

Note: AMS stands for Annual Manufacturing Survey.

F- Estimates for α and β

The welfare estimates require the estimation of the parameters of the utility function. In order to do so I use the information available in the *Gran Encuesta Integrada de Hogares* [Colombian National Household Survey] between 2006 and 2010. These surveys are representative at the National level and correspond to cross sections collected annually and contain information on workers and households socioeconomic characteristics. They are collected and processed by the Colombian Statistical Department (DANE, for its initials in Spanish).

According to the theoretical model presented in section 2, the indirect utility of a worker i living at municipality m is given by:

$$V_{im}(P_m, w_m, r_m, v_m) = \left[(w_m + F)P_m^{-\alpha} r_m^{\alpha-1}\right]^{\beta} [1/v_m]^{1-\beta} x_{im} \tag{1.18}$$

which could also be expressed as:

$$V_{im} = V_m x_{im} \tag{1.19}$$

where:

$$V_m = \left[\frac{w_m + F}{P_m^{\alpha} r_m^{1-\alpha}}\right]^{\beta} [1/v_m]^{1-\beta} \tag{1.20}$$

Following, Redding (2012) I assume that the workers idiosyncratic shocks x_{im} are distributed frechet so that:

$$Pr(x_{im} < a) = e^{-a^{-\delta}} \tag{1.21}$$

From these assumptions, the amount of people living at m as a share of total population is given by:

$$s_m = \left[\frac{V_m}{V}\right]^{\delta} \tag{1.22}$$

where:

$$V = \left[\sum V_m^{\delta}\right]^{1/\delta} \tag{1.23}$$

Adding time subscripts and taking logs we can write:

$$ln(s_{mt}) = \underbrace{\delta\beta}_{a_1} ln(I_{mt}) - \underbrace{(\delta\alpha)}_{a_2} ln(P_{mt}) - \underbrace{\delta(1-\alpha)}_{a_3} ln(r_{mt}) - \underbrace{\delta(1-\beta)}_{a_3} log(v_{mt}) + \underbrace{\delta lnV}_{\gamma_t} + \epsilon_{mt} \tag{1.24}$$

In particular, the right hand side of the equation is lagged to reduce endogeneity problems. I estimate the previous specification for the 24 cities available on the National Household Survey from 2006 to 2010. The population shares s_{mt}

come from the Colombian National Statistical Agency and are constructed with information from the population census of 2005 and the National Household Surveys. Based on this methodology I identify values of α=0.82 (s.e=0.021), β=0.98 (s.e.=0.15)

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CHAPTER 2

On the Unintended Consequences of Enforcement on Illegal Drug Producing Countries

2.1 Introduction

As of 2013, the total expenditures by the United States on the war against illegal drugs accounts for approximately $40 billion dollars per year[1]. According to information of the Office of National Drug Control Policy of the White House, on average, 12% of these resources where spent on international initiatives to reduce illegal drug supply. However, few efforts have been directed at studying supply side antidrug policies[2]. This paper investigates the effectiveness and welfare consequences of aerially spraying herbicides on coca crops in Colombia.

According to data from the United Nations Office of Drugs and Crime (UNODC), of the 18 countries that have implemented supply antidrug interventions in the last two decades, Colombia has applied the most aggressive strategy in terms of resources invested. In particular, data by UNODC indicates that by 2000, 74% of the world's supply of cocaine was produced in Colombia. This facilitated the direction of a vast amount of financial resources from the Colombian and the U.S. governments towards reducing the cocaine supply. Between 2000 and 2010, the U.S. government spent around 6 billion dollars on international supply control in Colombia (Office of National Drug Control Policy), making Colombia the third largest recipient of military foreign aid from the U.S. (after Israel and and Egypt)[3]. In addition, between 2000 and 2010 the Colombian government disbursed US$668 million/year in its war against illegal drug production. Combined, these expenses account for approximately 1.1% of the country's GDP (Mejía and Restrepo (2011)).

Despite the huge amount of resources invested, as of today, there is little empirical evidence at the micro level on the impact of these programs. Most of the related work consists of theoretical models calibrated with aggregate data to

[1]As estimated by Becker and Murphy in the Wall Street Journal article of January 4, 2013.

[2]According to the World Drug Report of 2012, by the year 2011, 18 countries were implementing supply interventions mainly focused on the forced eradication of opium poppy and coca leaf crops—the main inputs of heroin and cocaine production, respectively.

[3]The data on top recipients of U.S. foreign assistance is available at: http://www.fas.org/sgp/crs/row/R40213.pdf

simulate the effect of antidrug policies on drug trafficking or econometric analysis based on aggregate time series (see for example Rydell et al. (1996), Moreno-Sanchez at al. (2003), Diaz and Sanchez (2004), Mejía (2008), Chumacero (2008), Costa-Storti and De Grauwe (2008), Grossman and Mejía (2008), Tragler et al. (2008), Dion and Russel (2008), and Mejía and Restrepo (2011)). These studies conclude that the forced destruction of coca and opium crops is an ineffective strategy for drug control. The main limitations of these studies is that they use aggregate data, which possess a considerable threat of endogeneity; their results are driven by theoretical assumptions; and they ignore other unintended effects of these programs.

Two recent studies that attempt to address the endogeneity concerns between spraying and coca cultivation are Reyes (2014) and Mejía et al. (2014). The former instruments spraying with the distance between sprayed areas and the closest military base finding a positive correlation between coca cultivation and the treatment. The main limitation of this analysis is that the location of military bases is likely endogenous to the one of coca crops. Mejía et al. (2014) exploit the variation induced by a diplomatic friction between the governments of Colombia and Ecuador that resulted in a free-spraying zone within a 10 km band along the border with Ecuador and inside Colombia beginning in 2006. The authors employ regression discontinuity and conditional differences in differences to identify the effects of the program on coca cultivation. Their results suggest that spraying one additional hectare reduces coca cultivation by about 0.02 to 0.065 hectares.

This paper contributes to the existing literature by employing a unique and rich data set with 1-square-km cells collected through satellite data to study the effects of an antidrug supply policy, by studying the effects of the an antidrug program over the whole territory where the program was implemented (on contrast to the Mejia et al.(2014) where the analysis is focused in a region of the country), and by investigating not only the effect of spraying on coca production, but also, on the welfare conditions of coca-producing areas and its spillover effects on other non-treated areas (including neighboring countries).

The data collection is done by the Integrated Monitoring System of Illicit Crops of the United Nations of Drugs and Crime to guarantee that there is no data manipulation. The data includes information on all the areas that had coca crops between 2000 and 2010. I use this data set to study the effect of spraying on coca production in the short (12 months) and medium term (24 to 36 months), and to check if spraying spreads coca production into neighbouring areas that were not treated (i.e., spillover effects). Moreover, I aggregate these data on municipality units and combine them with other governmental sources to identify the effects of the program on violence outcomes (homicide rates and forced displacement), education outcomes (enrollment rates and school dropout), infant mortality, and poverty rates.

The identification of the causal effects of aerial spraying is challenging given that treatment is not randomly assigned, but is targeted through satellite images. The targeting mechanism creates two types of endogeneity issues. *Cross-section endogeneity* in coca production arises since the targeted areas have more hectares of coca. It also arises for the socioeconomic indicators because coca growing is illegal in the country and so, coca-producing areas are the ones with the lowest governmental presence (hence, the ones with the worst socioeconomic outcomes). *Panel endogeneity* or time feedback effects arise because areas with increasing coca cultivation have more spraying, which may lead to worsening socio-economic conditions and more coca cultivation.

To identify the effects of spraying on coca cultivation and social outcomes, I instrument spraying with the exogenous variation created by governmental restrictions to spraying in protected areas (i.e., natural parks and indigenous territories) and the time variation in financial resources available for aerial spraying induced by U.S. antidrug international expenditures. In particular, my instrument is constructed as the interaction of these two variables. Since aerial spraying is forbidden in protected areas, and I show that this rule in enforced in Colombia, coca crops outside these areas face a higher likelihood of being treated. Moreover, the likelihood of spraying increases more proportionally for unprotected areas when U.S. international antidrug expenditures are higher, relative to protected areas. This last is my source of variation.

My results suggest that when the likelihood of being sprayed increases by 1%, coca production decreases by 0.07 hectares per square kilometer. This suggests that eradicate 1 hectare of coca per square km (1 square km=100 hectares) spraying will have to increase by 14.3 hectares per square km (to increase the likelihood of being sprayed by 14.3%). I obtain similar results when I use a random sample collected at the producer level. These results are persistent 12 and 36 months after the treatment implementation. I also check for evidence of spillovers from the program and find no evidence that coca production increases in the non-treated areas neighboring the treated ones. This may suggest that if producers are changing locations, they may be going to areas farther away from the treated ones, or even to other countries with similar coca-growing conditions and less enforcement (i.e., Peru and Bolivia). The aggregate figures support this hypothesis.

I also find that spraying worsens the welfare conditions in treated areas. Specifically, when the share of area sprayed increases by 1% in each municipality, poverty rates increase by 0.22 percentage points. These effects persist 2 years after the fumigations. Moreover, spraying is reflected in worse education and health conditions of coca producers. A 1% increase in the share of area sprayed reduced secondary school enrollment by 0.11 percentage points and increases dropout rates by 0.04 percentage points. This suggests that as a result of the program, older children may be pulled out of school to work and help compensate for the income shock caused by the fumigations. The negative effect of the program on education outcomes reverts 1 year after the treatment implementation. This is in line with the results of Beegle et al.(2006), who document the impact of a loss in the crop's value on child labor.

Related to health outcomes, I find that when the share of area sprayed increases by 1%, infant mortality increases by 0.07 percentage points. This effect may be explained by a combination of a direct effect of the herbicide on health outcomes as documented by Mejía and Camacho (2012) and an indirect effect of the program caused by the income shock. This effect persists 2 years after the fumigations.

I also find evidence of an increase in violence outcomes 1 year after treatment implementation. My results indicate that when the share of area sprayed increases by 1% in each municipality, homicide rates increase by 0.67 percentage points and the number of individuals displaced increases by 4.97. Local authorities suggested the negative effect of aerial spraying on violence may be explained by the military check-ups that take place on the ground before the aircraft begin their flights. These inspections may be increasing the likelihood of a confrontation between the authorities and the drug traffickers, which increases violence in the treated areas in the short run. Moreover, this effect may be explained by drug traffickers retaliating in response to the crop eradication. These explanations are consistent with the fact that these effects seem to disappear in the long term.

Despite the reduction on the total area under coca cultivation in Colombia (it stood at three quarters of its level in 1990), the quantity of cocaine manufactured was at least as high as the one manufactured in 2001 (UNODC, 2013). This was due to a sharp increase of cocaine yields in Colombia[4]. In fact, based on data on cocaine seizures from the Antinarcotics Colombian Police and data collected at coca farms from UNODC in 2001 it was possible to produce 4.2 kg of cocaine per hectare of coca in 2001, whereas this yields increased to an average range of 5.1 to 6.8 kg per hectare in 2010. In other words, coca-producers and cocaine traffickers are also actively modifying their behavior in response to higher levels of enforcement which has resulted maintained cocaine's supply stable.

In sum, considering its sizable financial cost, the small effects on coca cultivation and cocaine supply, the negative unintended consequences on the population living in coca-producing areas (who are the poorest and most vulnerable in Colombia), and the negative spillover effects on neighboring countries, it can be concluded that the program's costs are by far higher that its potential benefits. This calls to the urgency of applying other policy alternatives such as supporting the development and implementation of alternative legal crops and strengthening governmental presence in coca-producing areas.

In the next section, I describe the existing involuntary eradication programs; section 3 describes the data; section 4 presents the identification strategy; section 5 presents the results; section 6 presents some robustness checks; and section 8 presents a brief cost-effectiveness analysis of the program. Finally, section 7 offers concluding remarks.

[4]Cocaine yields are measured as total kilograms of cocaine per hectare of coca leaf.

2.2 Forced Eradication antidrug Programs

Currently, the only types of forced eradication programs implemented in the world are manual eradication and aerial spraying. Manual eradication is performed by a group of men who destroy coca or opium poppy crops by hand (UNODC (2012)). Aerial spraying is executed with an herbicide called glyphosate, which small aircraft spray as close as possible to the ground. For 2010, Colombia, Mexico, Peru, Morocco, Myanmar, Bolivia and Afghanistan were the countries most actively involved in these initiatives.

In terms of scale, of the 18 countries that implement these programs, Colombia applies the most aggressive eradication strategy. Data from the Colombian Antinarcotics Police (DIRAN) suggest that between 2000 and 2010, 787,096 ha (or 3,039 mi^2) were sprayed in Colombia. This is more than double the size of Mexico's eradication program, which takes second place in terms of the number of hectares eradicated (UNODC (2012)). Aerial spraying began to be implemented in Colombia in 1978 (Gaviria and Mejia (2011)), and it is the biggest forced eradication program in the world (UNODC (2012)). Yet, data on the size of the program began to be collected only in 1986. Since that year, the program has grow extensively. The total area sprayed increased from 870 to 103,302 hectares between 1986 and 2010.

Figure 2.1 presents the evolution of the hectares eradicated by type of program and hectares grown during the last decade. The time series show that the rise in hectares sprayed has been coupled with a reduction in coca production in the last decade. However, the causality of the program on the total hectares of coca cultivated cannot be inferred from these aggregate figures alone.

Aerial spraying is mainly targeted through satellite images produced and processed by UNODC. These satellite pictures are taken in the last months of the year and are processed with great detail to identify the exact location of the crops. This information is then passed to the Antinarcotics National Police (DIRAN), in charge of executing the fumigations. Before the fumigations are performed, DIRAN confirms the location of the crops through flight inspections. Due to the magnitude of the area cultivated in Colombia and the governmental financial restrictions, not all the coca crops are sprayed in Colombia. Thus, the program concentrates on areas where there is a higher crop density.

The manual eradication program began in 2007 and maintains a modest size given its high costs in terms of human lives[5]. Reports from DIRAN estimate that since its implementation, 135 men have been killed through explosions of mines hidden in the ground to prevent the eradication. In 2010, 32,140 hectares were eradicated through this program. Hence, the aerial spraying program was 5 times as large as the manual eradication program for that year.

[5]This program was being implemented in 18 countries in 2010.

Unlike the manual eradication program, aerial spraying has been implemented for more than 30 years and has a known targeting mechanism. Thus, this study will focus on identifying the effectiveness and welfare consequences of the aerial spraying program[6].

2.3 The Data

Over the years, the scarcity of good quality data has been the main limitation in studying the effectiveness of antidrug programs in producer countries. Around 1999, UNODC launched the Illicit Crop Monitoring Programme. It aimed at collecting satellite images of the countries the most coca, opium and cannabis, including Colombia, Peru, Bolivia, Afghanistan, Lao People's Democratic Republic, Myanmar and Morocco. These images allow identifying the exact location and size of the coca, opium, or cannabis crops, and are collected annually. UNODC not only processes the satellite images to determine the size of crops but verifies this information by flying in areas that are chosen randomly throughout each country. Thus, this is the highest quality available data on the location of illicit crops.

Despite the great efforts by UNODC, evaluating the effectiveness of antidrug programs in producer countries remains constrained by the lack of data on treatment recipients and by the unclear targeting mechanisms different governments use. The aerial spraying program in Colombia is a unique exception since the Antinarcotics Police (DIRAN) records the exact location where the small aircraft open their valves to start spraying glyphosate and close them to stop.

I combine these unique sources of information and construct two data sets to identify the impact of aerial spraying on coca-producing areas. The first one is balanced panel data at the grid level, which corresponds to an area of 1 square km or 100 hectares. It includes all grids that had at least 1 hectare of coca between 2000 and 2010. For each unit of observation I observe the hectares of coca grown, the hectares aerially sprayed, the hectares manually eradicated, and the exact location of each of the 1,115,840 grids in the sample. I use this sample to identify the effect of aerial spraying on coca production. Table A.1 of Appendix G presents descriptive statistics for this data set. The table shows that on average each grid had 0.11 hectares manually eradicated, 0.54 hectares aerially sprayed, and 0.84 hectares of coca.

The second data set aggregates the grid data by municipality and combines it with other governmental information on welfare outcomes. This results in a balanced panel that contains the 288 municipalities with at least 1 hectare of coca between 2001 and 2010[7]. This data set includes information on violence-related

[6]This paper excludes all the observations that were treated by both programs (this accounts for 0.52% of the grid sample.)

[7]Colombia is divided into 1,123 municipalities.

outcomes (i.e., homicide rates per 100,000 inhabitants and forced displacement), education outcomes (i.e., enrollment rates and school dropout); infant mortality rates, and poverty rates.

Table G.4 in Appendix G presents the descriptive statistics for this sample. The table shows that the municipalities in the sample have low levels of socioeconomic development and high levels of violence. This is because coca crops are illegal in the country and thus are cultivated only in remote areas with very low governmental presence. I use this data set to assess the welfare consequences of aerial spraying on coca producer municipalities in Colombia. Appendix G also presents the data sources and the definition of each variable in this data set.

Finally, Table 2.1 presents a summary of the information available in both data sets.

2.4 Estimation Framework

To address the endogeneity issues of spraying with coca production and with the socioeconomic conditions, I estimate the effect of the program using instrumental variables. In particular, I use the following specification:

$$Y_{it} = \alpha_0 + \alpha_1 Spr_{it} + g_t + k_i + e_{it} \tag{2.1}$$

$$Spr_{it} = \beta_0 + \beta_1 Outside\, PA_i * U.S.\, Exp_t + g_t + k_i + u_{it} \tag{2.2}$$

where Y_{it} represents coca production or welfare indicators by grid or municipality i in year t; Spr_{it} is the treatment intensity measured as an indicator dummy for being sprayed (for the grid sample) or the share of area sprayed (for the municipality sample); g_t are time fixed effects; k_i are grid or municipality fixed effects; $Outside\, PA_i$ is an indicator variable that takes the value of 1 if the grid is located outside protected areas, and it corresponds to the number of hectares outside protected areas for the municipality sample; and $U.S.\, Exp_t$ are the U.S. international antidrug expenditures in real billions of dollars of 2010. For the municipality data, I scale hectares grown, sprayed, and lying outside the protected areas by the total area. This is necessary due to the diverse size of municipalities in Colombia. In this specification the coefficient of interest is α_1, which identifies the local average treatment effect of the program for the group of compliers.

In equations 1 and 2, I instrument the treatment assignment with an interaction of the exogenous variation created by governmental restrictions to spraying in protected areas and U.S. international supply antidrug expenditures. By governmental mandate, protected areas—i.e., natural parks and indigenous territories—cannot be sprayed in Colombia[8]. According to the National Geographical Institu-

[8]According to Decree 143 of 1991, aerial spraying is prohibited in indigenous territories and

tion in Colombia (i.e., Instituto Geográfico Agustin Codazzi), natural parks and indigenous territories comprise 12% and 27.6% of Colombia, respectively. Moreover, around 5% of the total population lives in these areas. Figure 2.2 presents the exact location of these areas throughout the country. It is worth noting that there are coca crops inside these areas. For instance, in 2010, 18% of the total hectares of coca were located in protected areas.

To create time variation on the instrument, I interact protection areas with the U.S. international antidrug expenditures in real billions of dollars of 2010. According to the annual budget of the Office of National Drug Control Policy of the White House (ONDCP) between 2000 and 2010 the U.S. disbursed 17.6 real billion of dollars of 2010, to reduce the international supply of illegal drugs. The time evolution of these expenditures is presented in Figure 2.3, Panel B. Because between 1990 and 2000 Colombia produced more than 50% of the world's cocaine[9], the country received 30% of those resources throughout 2000 and 2010. In particular, according to the data published in the annual budget summary of ONDCP between 2000 and 2010 Colombia received 5.3 real billions of dollars of 2010 to improve security conditions and reduce drug supply. Hence, it should be expected that higher U.S. expenditures would induce a higher treatment intensity in non-protected areas.

Because non-protected areas have a higher likelihood of being treated and treatment intensity should increase when there are higher U.S. international antidrug expenditures, the correlation between the instrument and the treatment intensity should be positive.

2.4.1 Assessing the instrument's quality

I begin by presenting some evidence on the correlation between the instrument and the treatment intensity. Figure 2.3 presents the hectares sprayed by deciles of the share of area outside protected areas at the municipality level—$Outside\, PA_i$. Panel A of Figure 2.3 presents fitted values of hectares sprayed on deciles of $Outside\, PA_i$ for years with different levels of U.S. international antidrug expenditures. The figure suggests that: (i) municipalities with a higher share of non-protected areas had a higher number of hectares sprayed, and (ii) in years when the U.S. international antidrug expenditures were higher (as shown in Panel B), the intensity of treatment increased more for non-protected areas; in other words, the slope of the fitted lines increases when U.S. antidrug expenditures are higher.

A formal test on the correlation between the instrument and spraying intensity, the so-called relevance assumption, as defined by Imbens and Angrist (1994),

natural parks. The decree also establishes a 100 meter band around these areas for which aerial spraying is also forbidden. Resolution 0015, approved the 5th of August of 2005, allows aerial spraying in natural parks if several requirements are fulfilled. However, to date, these conditions have not been met and aerial spraying has never been done in protected areas.

[9]See the annual World Drug Reports by the United Nations Office of Drugs and Crime.

Abadie (2003) and Angrist et al. (1996), is presented in Tables 2.2 and 2.3. The tables present the results of the first stage of the instrumental variables regression as specified in equation (2) for the samples with units by grid and municipality. Both tables show the estimates of three regressions: column (1) presents the first stage regression using the interaction of the area outside protected areas and the U.S. international antidrug expenditures, and columns (2) and (3) present the results of the regression using each of these variables individually.

The results for column (1) confirm that the relevance assumption is satisfied. The coefficient on the instrument has a positive sign and is statistically significant. The R^2 is 14% and 17% for the grid and municipality sample, respectively. In addition, the partial R^2 is higher than 5% for both samples, and the F-test for excluded instruments takes a value of 60.00 for the grid and 21.71 for the municipality data. For the case of a single endogenous regressor, Staiger and Stock (1997) suggest rejecting the hypothesis of weak instrument if this F-statistic is higher than 10. Hence, these estimates rule out concerns of having the finite sample bias of IV (as defined by Bound, Jaeger and Baker (1995)). Moreover, the estimates in columns (2) and (3) confirm that each of the variables has predictive power on the treatment intensity and affect it in the expected direction.

The second assumption that must be satisfied for the validity of my identification strategy is the exclusion restriction. There will be a violation in the exclusion restriction only if the $corr(Instrument_{it}, u_{it}|k_i, g_t) \neq 0$. In other words, the exclusion restriction requires that the instrument only affects the outcomes through aerial spraying. Since the estimates of equations (1) and (2) include year and grid or municipality fixed effects, my identification strategy is not threatened by the static potential differences between protected and non-protected areas, nor by changes in aggregate time trends across years[10].

The instrument is effectively comparing non-protected areas with a high change in enforcement expenditures with protected areas with a low change in enforcement expenditures. The identifying assumption will be violated if there are variables changing in time correlated with U.S. international antidrug expenditures that affect protected and non-protected areas in different ways. For example, a violation to the exclusion restriction might take place if when U.S. international antidrug expenditures change the local (i.e., municipality) or central governments modify their behavior in different ways in municipalities with a different share of unprotected areas.

I address this concern through two exercises that rule out any differential changes in behavior for the local or the central governments in areas with different shares of unprotected areas. Table 2.4 presents a regression of each municipality's public income and expenditures (as total and by type) on the instrument. These variables represent the local government's behavior and they are presented in real

[10]This rules out any business cycle variation at the aggregate level for Colombia or the U.S., as well as any variation on international commodity prices.

billions of Colombian pesos of 2010. The regressions include fixed effects by year and municipality and all standard error are clustered at the municipality level. The table suggests that there is no correlation between the instrument and any of the variables. Hence, the local governments show no differential response to changes in U.S. international antidrug expenditures between areas with a different share of protected areas.

Table 2.5 presents a similar exercise, but now the independent variables correspond to the transfers made by the central government to each municipality. The table presents the regressions of total transfers and transfers by type (i.e., health, education, and other purposes) on the instrument. The results rule out any different response of the central government to changes in U.S. antidrug expenditures between municipalities with different share of protected areas.

Finally, in order to interpret α_1 in equation (1) as the local average treatment effect of aerial spraying on the outcomes, I need to rule out the existence of defiers; this is reasonable since protected areas should be less exposed to aerial spraying throughout the period of analysis. Figure 2.4 shows evidence that supports the validity of this assumption. As can be seen, those municipalities with a higher share of protected areas have very low levels of aerial spraying.

2.4.2 Other threats to internal validity

An important threat to my identification strategy is potential possible manipulation of the treatment by producers. If producers are aware of the governmental restrictions on aerial spraying in protected areas and they do not face restrictions in changing locations, it could be expected that they would move their coca crops to protected areas to prevent fumigation. If that were the case, the instrument could no longer be used as a plausibly exogenous variation for treatment assignment. Figure 2.5 presents deciles of the percentage of area that is non-protected against the percentage of area that is covered by coca crops in each municipality. The figure suggests that there is not a concentration of coca crops in protected areas throughout the period of analysis.

Another concern with the validity of the results is that the government may have been substituting the aerial spraying program with manual eradication in the protected areas. Figure 2.6 presents the deciles of the area that is unprotected areas against the mean hectares that are manually eradicated (both as a percentage of total area). The figure suggests that the government is not increasing the number of hectares manually eradicated in protected areas. In fact, Decree 143 of 1991 in Colombia imposes restrictions on any involuntary eradication program implemented in protected areas.

2.5 Empirical Results

Tables 2.6 and 2.7 present the estimates of equations (1) and (2). I only use the grid sample to identify the impact of the program on drug production since it is the only outcome available at this level; the municipality data is used to assess the effects of the program on the welfare outcomes[11]. To identify the medium-term effect of the program, I lag the treatment in equation 2 one and two years[12]

2.5.1 Impact on Drug Production

Table 2.6 presents the estimates for the effect of spraying on hectares of coca. They point to small effects of the program. As expected the OLS estimates overestimate the effects of the program since the sprayed areas tend to have more coca. My most preferred estimates presented in column (3) take a value of -1.19 ha per square km (1 square km =100 hectares). Given the estimates on column (1) from table 2.2, when U.S international antidrug expenditures increase in $1 billion the grids that are inside an unprotected area the likelihood of being sprayed by 18%. Hence, on average, increasing the likelihood of being aerially sprayed by 1% reduces the hectares of coca cropped by 0.07 ha per square km (i.e., 1.19 over 18). Thus, to reduce 1 hectare of coca per grid (1 grid=100 hectares) the likelihood of being sprayed will have to increase in 14.3%, that is, 14.3 additional hectares will need to be sprayed.

The medium-term estimates present a similar pattern, showing a sustained negative effect of the program in the medium term (i.e., 1 or 2 years after the fumigations)[13].

There are several reasons why aerial spraying may not have a higher impact on coca leaf production. For instance, Dávalos et al. (2009), Caulkins and Hao (2008), and Mejía and Restrepo (2011), suggest that some of the ways producers may reduce the effect of the herbicides on coca are: (1) applying manual defoliation, (2) selecting highly productive coca varieties with more resistance to the herbicides, or (3) switching to agroforestry coca, which mixes tall plants such as plantains or fruits with coca to prevent the effect of fumigations.

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[11] For all the estimates I calculated clustered standard errors at the grid or municipality level. Moreover, for the grid level estimates I also verified the robustness of the the results to spatial correlation between grids. For this purpose, I verified the sensitivity of the results to the estimation of Conley (1999) spatial standard errors assuming that: i) the correlation between grid-level cells is zero for areas bigger than 300x300 hectares (which groups 6 one square km cells) and ii) the correlation between grid-level cells is zero for areas bigger than 500x500 hectares (which groups 25 one square km cells). Since the standard errors are almost exactly the same as the ones obtained by clustering at the grid level, I only report the latter to save space.

[12] It was not possible to assess the impact of the program after more than 2 years given the sample size restrictions in the municipality panel data.

[13] I do not identify heterogeneous effects of the program on coca production by region.

2.5.2 Are there spillover effects on coca production?

In this subsection, I check whether the program is creating spillover effects. These effects will occur if, for example, when the hectares of coca cultivated drops in the treated areas, if increases in nearby untreated areas. I use the following specification to test for spillovers:

$$Coca_{-it} = \alpha_0 + \alpha_1 Spr_{it-1} + g_t + k_i + e_{it} \tag{2.3}$$

where Spr_{it-1} represents the total ha sprayed in municipality i in $t-1$; $Coca_{-it}$ represents the total hectares of coca grown in the municipalities that belong to the same department as municipality i but which were not treated in $t-1$ or in t[14]; and g_t and k_i stand for year and municipality fixed effects. Standard errors were clustered at the municipality level in the estimates. Appendix H presents the estimates of equation (3), which suggest no evidence of a spillover effect of the program on coca production. In particular, the effects show the opposite sign, suggesting that coca production decreased in the municipalities not treated by the program, too. I also estimate this specification with the grid sample, analyzing the effect around the adjacent grids that were not treated in the previous period. The results are not statistically significant for any specification[15].

This may indicate that if coca producers are changing locations as a result of the program, they may be moving to areas farther away from the treated areas or to other countries with similar coca-growing conditions (e.g., Peru or Bolivia). In fact, the aggregate series of coca production by country gathered and processed by UNODC support this argument. While coca production fell in Colombia by 60.81% (from 163,300 to 64,000 hectares) between 2000 and 2010, it increased by 136% in Peru (from 43,400 to 62,500 hectares) and by 44% in Bolivia (from 14,600 to 34,500 hectares) during this period. However, despite the increase of hectares grown in Peru and Bolivia, the world's coca production decreased from 221,300 to 151,200 hectares between 2000 and 2010.

2.5.3 Impact on Welfare Outcomes

Table 2.7 assess the effect of the program on the welfare indicators of coca-producing areas. Specifically, the table presents the effects of the program on: poverty rates, education outcomes, infant mortality, and violence. Given the estimates of column (1) of table 2.3, when U.S. antidrug expenditures increase in \$1 billion and the share of area in an unprotected area increases by 1% the share of sprayed area increases at least by 18%. This implies that, to be interpreted correctly, all the coefficients in table 2.7 need to be divided by 18.

[14]Colombia is divided into 1123 municipalities, which can be grouped into 32 departments.

[15]I also checked for the spillover effects of the program in all of the other socioeconomic indicators at the municipality level and find no statistical evidence of spillovers for any of them.

Poverty rates are constructed based on the percentage of the rural population under the poverty line[16]. Since poverty rates were constructed with the information available in the population census of 2005, they are available only for that year. Hence, the estimates will not include fixed effects by municipality. The estimates suggest that the areas that had a 1% higher share of area aerially sprayed had rural poverty rates 0.22 percentage points higher in the short term. More strikingly, these effects seem to be maintained 1 and 2 years after the treatment implementation. These effects are moderate since, according to the Food and Agriculture Organization of the United Nations, rural poverty rates in Latin America only fell by 7% between 1980 and 2010, from 60 to 53%.

For the education outcomes, I find a significant effect of the program on secondary enrollment and school dropout only in the short term. The results suggest that when the share of area sprayed increases by 1%, secondary enrollment rates decrease by 0.11 percentage points and school dropout rates increase by 0.04 percentage points. When compared to the changes in these variables across time, the effects of the program on secondary enrollment rates are small, and the effect on school dropout rates is large. In particular, during the period of analysis secondary enrollment rates increased from 58.49 to 84.16 and school dropout rates fell by from 11.80 to 11.34[17]. I do not find any effect on primary enrollment rates.

Together these results indicate that since a relevant part of the household's income is reduced by aerial spraying the older children are being pulled out of school to work and compensate for the income shock (as suggested in a theoretical model by Basu and Van (1998)). Similar responses to negative income shocks on the probability that children enter employment, leave school, and fail to advance have been documented by Jacoby and Skoufias (1997) in rural India, Duryea et al. (2007) in Brazil, and Beegle et al. (2006) in Tanzania. For example, Beegle et al.(2006) find that when hit by a transitory negative shock in the value of crops, rural households tend to increase their use of child labor by 30%. This is in line with the permanent income hypothesis that suggests households that lack buffer stocks and are credit constrained tend to use other mechanisms to smooth consumption. Indeed, this is the case in coca-producing areas that have rural poverty rates of nearly 60% of the total population.

The estimates also point to a negative and significant effect of the program on infant mortality in both the short and medium term. The coefficients indicate that when the share of area treated increases by 1% infant mortality increases by 0.07, 0.05, and 0.05 percentage points, the same, one, and two years after the fumigations. This is a relevant effect considering infant mortality rates changed only 0.50 percentage points between 2006 and 2007, the two years for which there

[16] The poverty line is 60% of the median household income, from data published by the Colombian Statistical Department in the population census of 2005.

[17] For secondary enrollment rates this corresponds to the change between 2005 and 2010, and for school dropout this corresponds to the change between 2007 and 2009. These are the only years for which these variables are available in coca-producing areas.

is available information of this outcome.

The increase in infant mortality in the treated areas may be explained by the direct effect of the herbicide on human health and the indirect effect of spraying through the increase in rural poverty rates. Unfortunately, there is not enough data at the individual level to identify precisely the size of the direct and indirect effects. Yet, other studies that have analysed the direct effect of glyphosate on human health suggest that it generates a negative effect on health outcomes. For example, Mejía and Camacho (2012) use daily panel data on the individual-level registers of medical consultations, emergency room visits, hospitalizations, and procedures that took place in any health service institution in Colombia between 2003 and 2007, and daily data on spraying intensity to identify the effects of the program. In particular, they check for different patterns in the reported pathologies 15 days after a fumigation in the treated municipalities. They find that, on average, a 1 square km increase in the area sprayed increases by 0.2 percentage points the probability of having a skin pathology 15 days after the treatment, and that an increase in one standard deviation in the area sprayed in the municipality of residence increases the probability of an abortion by 0.025 of a standard deviation. Given that the standard deviation of aerial spraying takes a value of 1651 in my sample[18], and that the standard deviation of abortion in their sample takes a value of 0.2, these represent a very small effect.

The results by Mejía and Camacho (2012) suggest that a significant portion of the negative effect that I identify on infant mortality may be driven by the indirect effects of spraying on rural poverty. However, more data is needed to provide a more precise decomposition of the direct and indirect effects of the program on health outcomes. Other evidence of the effect of negative income shocks on health outcomes has been found by Adda et al. (2009) and Ferreira and Schady (2009).

Finally, table 2.7 also reports the effects of aerial spraying on homicide rates per 100,000 inhabitants and number of individuals displaced by force in each municipality. The estimates in column (1) suggest that when the share of area sprayed increases by 1%, the homicide rates increase by 0.67 percentage points and the number of displaced individuals increases to around 4.97. These are small relative to the change in these variables between 2000 and 2010. Specifically, homicide rates and forced displacement fell by 20.95 percentage points and 509 individuals, respectively, during this period.

In the past, several studies have shown the relation between drug trafficking and violence (see for instance Angrist and Kugler (2008), Dube and Vargas, (2008) and Dell (2011)), but the role that antidrug involuntary eradication programs have on violence has never been studied before from the micro perspective. Local authorities suggested the negative effect of aerial spraying on violence may be explained by the military check-ups that take place on the ground before the aircraft begin their flights. To guarantee the security of the pilots, aerial spraying

[18]This information is not available in their paper.

only begins once a group of men from the military or the police check the aircraft trajectory to prevent any retaliation of drug traffickers against the aircraft. These check-ups may be increasing the violence level in the treated areas in the short run by increasing the likelihood that authorities have more confrontations with drug traffickers.

An alternative explanation for this effect may be a retaliation response from drug traffickers as a consequence of the eradication. Both of these explanations are consistent with the fact that these effects seem to disappear in the long-term estimates.

2.6 Robustness Check

In this section, I use a sample collected by SIMCI-UNODC at the producer level to check the effects of the program on drug production outcomes. The sample consists of two rounds of cross sections: the first collected between 2005 and 2006, and the second between 2007 and 2010. The producers to be surveyed were chosen by dividing the country into seven regions according to geographical characteristics. Each of the regions was divided into areas of 1 square km, and all those grids with coca production were identified through the satellite images. The producers that were surveyed were selected randomly from the areas with coca.

The surveys contain information on the socioeconomic characteristics of producers, productivity related variables (i.e., number of harvests and kgs/ha), and the geographic location of rural producers. In the survey, I observe which producers were aerially sprayed within the last 12 months. The sample has 2535 observations. Appendix I presents the descriptive statistics of this sample. For the productivity variables, the information was collected directly on the coca crops by field workers of UNODC and not only self-reported by coca producers.

I use this sample to run equations (1) and (2) for three outcomes related to drug production: (i) hectares cultivated, (ii) kilograms of coca per hectare, and (iii) number of harvests per year. Given that there are few observations where producers are located inside protected areas, I use the distance from the location of coca producers to the border of the nearest protected area as an instrument for aerial spraying. It is expected that those producers near or within protected areas face a lower probability of being aerially sprayed. Figure 2.7 presents some graphical evidence on the relation between the distance to the nearest protected area and aerial spraying.

As I did for the grid and municipality sample, here I multiplied the instrument by total U.S. international antidrug expenditures. Table 2.8 presents the estimates of the first stage equation. The estimates include the producer's age, education, and gender as well as dummies for year, region, department, and municipality. They confirm a positive effect of the instrument on the treatment assignment and reject the possibility of weak instruments. The results in column (1) suggest that when U.S. international antidrug expenditures increase in $1 billion and the minimum distance from a protected area decreases in 1 km the likelihood of being sprayed increases by at least 3% for coca-producers.

Table 2.9 presents the results of the OLS and 2SLS estimates of equation (1). For both, the effect of aerial spraying is negative. Yet, the impact of the program increases in absolute value for the 2SLS coefficients. This is in line with the idea that OLS estimates were biased in absolute value towards zero in the cross section.

Considering the estimates of column (3) in table 2.8, the results suggest that when the likelihood of being sprayed increases in 1% each producer crops 0.10 less hectares of coca (i.e., -0.31 over 3), the kilograms per hectare are reduced in 27.32 kg/ha (i.e., 81.98 over 3), and the number of harvests collected by producers are reduced in 0.39 (i.e., 1.17 over 3).

These results are reassuring since they point to negative effects of the program on coca bush cultivation. Although I cannot address the panel endogeneity for this case, and the coefficients may be underestimating the effect of the program, they point to the same signs.

2.7 On the Program's Cost-Effectiveness

My results suggest that coca cultivation is reduced in 0.07 ha when the likelihood of being sprayed increases by 1%. Hence, to reduce a hectare of coca per square km (1 square km=100 ha) the likelihood of being sprayed will have to increase 14.3%, that is, hence spraying will have to increase by 14.3 hectares per square km. According to Walsh et al. (2008) the average cost per additional hectare sprayed for the U.S. is $750 and for every dollar spend by the U.S. Colombia spends about 2.2 dollars (see Mejia, Restrepo, and Rozo, (2014)). Hence, these numbers suggest that the approximate direct cost or eradicating one hectare of coca is $120,000. As a result of the higher enforcement, Colombia has decreased coca cultivation in 74,532 hectares between 2001 and 2010, which amounts to an approximate financial cost of $2.55 billions of dollars.

Despite the fact that higher enforcement in Colombia has displaced coca cultivation to Bolivia and Peru, coca cultivation in the Andean region (which accounts for the world's supply of coca leaf) as a whole fell by 59,700 hectares between 2001 and 2010. However, as area under coca cultivation stood at three quarters of the level in 1990, the quantity of cocaine manufactured was at least as high as the one manufactured in 2001 (UNODC, 2013). This was due to a sharp increase of cocaine yields in Colombia. In fact, based on data on cocaine seizures from the Antinarcotics Colombian Police and data collected at coca farms from UNODC, in 2001 it was possible to produce 4.2 kg of cocaine per hectare of coca, whereas this yields increased to an average range of 5.1 to 6.8 kg per hectare in 2010.

In other words, coca-producers and cocaine traffickers are also actively modifying their behavior in response to higher levels of enforcement which resulted in a stable cocaine supply throughout the period of analysis.

Considering, its financial cost, the small effects on coca cultivation and cocaine supply, the negative unintended consequences of aerial spraying on the population living in coca-producing areas (who are the poorest and most vulnerable in Colombia), and the negative spillover effects on neighboring countries, it can be concluded that the program's costs are by far higher that its potential benefits. In fact, there are other policy alternatives that are less harmful for the population living in coca-producing areas such as supporting the development and implementation of alternative legal crops and strengthening governmental presence in those areas.

2.8 Conclusions

This paper identifies the impact of aerial spraying on coca-producing areas in Colombia. In general, previous studies that assess the effects of antidrug policies in producer countries have focused on theoretical models and aggregate time series. Moreover, these studies have traditionally focused on the effects that these programs have on drug production; yet, to the best of my knowledge, none of them has ever assessed how these programs affect the socioeconomic conditions of coca-producing areas (with the exception of health outcomes) or its spillover effects on non-treated areas (including neighboring countries).

This paper contributes in this direction by presenting a clean identification strategy that uses micro data to offer a complete overview of the effects that these programs generate on drug production, poverty, education, health, and violence.

Since aerial spaying is targeted through satellite images, there are various concerns when trying to identify its effect. Most of these are related with the endogeneity between aerial spraying and the outcomes. Specifically, that: (i) since coca crops are illegal in Colombia they are located in the poorest and most remote areas with the lowest governmental presence (what I called *cross-section* endogeneity), and (ii) changes in socioeconomic indicators across time make some areas more susceptible to beginning to cultivate coca (what I called *panel* endogeneity). To correct for these issues, I identify the effect of the program using instrumental variables.

The instrument exploits the plausible exogenous variation created by governmental restrictions in protected areas and the time variation in U.S. international supply antidrug expenditures. I show that since protected areas cannot be sprayed, the likelihood of being sprayed increases outside of these areas. Hence, in years when U.S. international supply antidrug expenditures are higher, aerial spraying increases in non-protected areas while it remains the same in protected areas.

I study the effects of the program in the short term (12 months after treatment implementation) and in the medium term (24 and 36 months after treatment reception). The results are striking: coca cultivation is reduced only by 0.07 ha per square km when the likelihood of being sprayed increases by 1% (hence, to eradicate 1 hectare of coca per square km spraying needs to increase by 14.3 hectares per square km) and there is a deterioration of the socioeconomic indicators in the treated areas. In particular, I find negative effects of the program on all rural welfare indicators. This is of great concern taking into account that the coca-producing regions are already the poorest areas of Colombia. These individuals may perceive that these effects are caused by the government, which in turn, may generate political unrest in coca-producing areas, further fueling the Colombian civil conflict.

Moreover, although I find no evidence of spillovers in the non-treated areas near the treated ones, this may suggest that if producers are changing locations, they may be going to areas farther away from the treated ones, or even to other countries with similar coca-growing conditions and less enforcement (i.e., Peru and Bolivia). The aggregate figures support this hypothesis.

In addition, although aerial spraying is inducing a reduction in coca production, it has also increased cocaine yields, and hence, cocaine's supply is at least as high as it was in 2000. In sum, the costs of the program are too high to be justified by its potential benefits. This points to the urgency of exploring new alternatives for controlling illicit crop production in producer countries.

Although this paper is able to cleanly identify the effectiveness of aerial spraying in Colombia, its main limitation is that the mechanisms that explain these effects cannot be distinguished. This may be overcome in the future if better information becomes available in coca-producing areas.

Table 2.1: Summary of Data Sets

	Data Set 1	Data Set 2
Units	Grid (1 squared km=100 ha)	Municipality
Years	2000-2010	2001-2010
Frequency	Yearly	Yearly
Type of Data	Panel	Panel
Observations	1,115,840	2880
Coca (ha)	Yes	Yes
Aerial Spraying (Ha)	Yes	Yes
Manual Eradication(Ha)	Yes	Yes
Other Variables	–	Violence, Education, Health, Poverty, Geographic Characteristics, Area, Rural Population, Government Expenditures, and Authorities Presence.

Note: The data on hectares of coca was processed by the United Nations Office of Drugs and Crime (UNODC) through satellite images collected every December. Data on hectares aerially sprayed comes from the Colombian Antinarcotics National Police (DIRAN). All other variables come from diverse agencies of the Colombian government. See Appendix G for the specific sources.

Table 2.2: First Stage Results (Grid-point sample)

Dependent Variable: $I(Sprayed > 0)$			
Independent Variables	(1)	(2)	(3)
$Instrument_{it}$	0.18***		
	(0.01)		
$I(Outside\,Protected\,Areas)_i$		0.07***	
		(0.00)	
$U.S.\,Exp_t$			0.2***
			(0.00)
Year FE	X	X	
Grid FE	X		X
R-squared	0.14	0.1	0.11
F-Test (excluded instruments)	60	269.52	95.66
Partial R-squared	0.08	0.09	0.11
N. of Clusters		101440	
Observations		1115840	

Note: The table presents the first stage estimates of the specification presented on equations (1) and (2) for the data with grid units. Each grid corresponds to an area of 1 square kilometer. The sample includes all the grids in Colombia that had a positive number of hectares of coca cultivated between 2000 and 2010. U.S. international antidrug expenditures are expressed in real billions of 2010 dollars. $I(Outside\,Protected\,Areas)_i$ is an indicator variable that takes the value of one if the grid is outside indigenous territories and natural parks. Clustered standard errors at the grid level are presented in parentheses. *** Significant at 1% level.

Table 2.3: First Stage Results (Municipality Sample)

Dependent Variable: Area Sprayed (% of Total Area)			
Independent Variables	(1)	(2)	(3)
$Instrument_{it}$	0.18***		
	(0.03)		
$Share\, Outside\, Protected\, Areas_i$		0.32***	
		(0.07)	
$U.S.\, Exp_t$			2.04***
			(0.05)
Year FE	X	X	
Municipality FE	X		X
R-squared	0.17	0.2	0.11
F-Test (excluded instruments)	21.71	19.91	17.96
Partial R-squared	0.05	0.06	0.04
N. of Clusters		288	
Observations		2880	

Note: The table presents the first stage estimates of the specification presented on equations (1) and (2). The sample includes all the Colombian municipalities that had a positive number of hectares of coca cultivated between 2001 and 2010. Since municipalities vary in size, all variables expressed in hectares were scaled by total area. U.S. international antidrug expenditures are expressed in real billions of 2010 dollars. $Share\, Outside\, Protected\, Areas_i$ corresponds to the percentage of total area outside indigenous territories and natural parks in each municipality. Clustered standard errors at the municipality level are presented in parentheses. *** Significant at 1% level.

Table 2.4: Ruling out the Correlation between the Instrument and the Local Government's Behavior

	Dependent Variables in real billions of Colombian pesos (1995=100)			
	Public Expenditures	Education PE	Health PE	Other PE
PC_{mt}	-0.02	0.03	-0.05	-0.08
[clust-err]	(0.04)	(0.03)	(0.11)	(0.09)
Mun FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
R-squared	0.01	0.01	0.01	0.01
N. of Clusters		755		
Obs.		11,325		

Note: The table presents a regression of fiscal variables on the instrument. The instrument corresponds to an interaction of U.S. international antidrug expenditures ($US\,Exp_t$) expressed in real billions of dollars of 2010 and the share of unprotected areas ($Outside\,PA_i$) which corresponds to the percentage of total area outside indigenous territories and natural parks in each municipality. All fiscal variables are expressed in billions of Colombian pesos of 2010. *Regalías* denotes the share of income received by a municipality due to exploitation of natural resources such as oil and other minerals. The sample includes all the Colombian municipalities that had a positive number of hectares of coca cultivated between 2001 and 2010. Clustered standard errors at the municipality level are presented in parentheses. *** Significant at 1% level.

Table 2.5: Ruling out the Correlation between the Instrument and the Central Government's Behavior

	Dependent Variable expressed in real billions of pesos (2010=100)			
	Education Transfers	Health Transfers	Other Purposes Transfers	Total Transfers
$Outside\, PA_i * U.S.\, Exp_t$	-15.19	1.60	3.22	18.62
	(12.59)	(1.76)	(2.75)	(18.70)
Mun FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
R-squared	0.83	0.32	0.94	0.69
N. of Clusters	261			
Years available	2002-2010			
Obs.	2349			

Note: The table presents a regression of the public transfers from the Central Colombian government to each of the municipalities on the instrument. The instrument corresponds to an interaction of U.S. international antidrug expenditures ($US\, Exp_t$) expressed in real billions of dollars of 2010 and the share of unprotected areas ($Ouside\, PA_i$) which corresponds to the percentage of total area outside indigenous territories and natural parks in each municipality. All fiscal variables are expressed in billions of Colombian pesos of 2010. The sample includes all the Colombian municipalities that had a positive number of hectares of coca cultivated between 2002 and 2010. Clustered standard errors at the municipality level are presented in parentheses. *** Significant at 1% level.

Table 2.6: Impact of Spraying on Coca Production (Grid-point Sample)

	Dependent Variable: Coca (ha per square km)				
	OLS	OLS	2SLS	2SLS	2SLS
	Short-term	Short-term	Short-term	1 year after treatment	2 years after treatment
	(1)	(2)	(3)	(4)	(5)
Ha Sprayed at t	0.66**	0.21**	-1.19***		
	(0.01)	(0.01)	(0.09)		
Ha Sprayed at t-1				-1.51***	
				(0.11)	
Ha Sprayed at t-2					-1.92***
					(0.14)
Year FE	X	X	X	X	X
Grid FE	X	X	X	X	X
N. of Clusters			101440		
Observations	1115840	1115840	1115840	1014400	912960

Note: The table presents the estimates of the structural equation of the specification presented in equations (1) and (2) by 2SLS using $I(Outside\,Protected\,Areas)_i * U.S.\,Exp_t$ as an instrument. The estimates correspond to the data set by grid units. Each grid corresponds to an area of 1 square kilometer. The sample includes all the grids in Colombia that had a positive number of hectares of coca cultivated between 2000 and 2010. Columns (1) through (3) presents the effect of the program 1 to 12 months after the treatment reception, column (4) presents the effect 13 to 24 months after the treatment reception, and column (5) presents the effect of the program 25 to 36 months after the treatment implementation. *Coca* represents the total hectares of coca cultivated observed through satellite images. Clustered standard errors at the grid level are presented in parentheses. *** Significant at 1% level and ** Significant at 5% level.

Table 2.7: Impact on Welfare Indicators (Municipality Sample)

	1 year after	2 years after	3 years after
	(1)	(2)	(3)
Poverty Rates (d)	0.04***	0.03***	0.03***
	(0.01)	(0.01)	(0.01)
Primary Enrollment (b)	-0.71	-1.18	-1.93
	(3.23)	(5.75)	(4.28)
Secondary Enrollment (b)	-2.13***	-1.75	-1.09
	(0.43)	(4.3)	(4.2)
School Dropout (c)	0.82***	0.36	0.34
	(0.26)	(0.67)	(3.45)
Infant Mortality (c)	1.26***	0.97*	0.94***
	(0.29)	(0.31)	(0.26)
Homicide Rates (a)	12.23***	-5.1	-3.56
	(1.60)	(5.62)	(3.45)
Forced Displacement (a)	89.52***	37.26	41.99
	(15.79)	(39.95)	(90.95)
Mean Values			
Poverty Rates (Percentage of rural pop under poverty line)			0.56
Primary Enrollment (Registered students/Pop in age)			128.93
Secondary Enrollment (Registered students/Pop in age)			71.21
School Dropout (Registered students/students finishing year)			10.8
Infant Mortality (Deaths of ind. younger than 1 year / Ind. born alive)			44.1
Homicide Rate (Homicides /100,000 inh)			55.85
Forced Displacement (N. of individuals)			592.7
Area Sprayed (% of Total Area)			0.26
N of Clusters		288	
Observations (a)	2880	2592	2304
Observations (b)	1440	1440	1440
Observations (c)	576	576	576
Observations (d)	288	288	288

Note: The table presents the estimates of the structural equation of the specification presented in equations (1) and (2) by 2SLS using $Share\,Outside\,Protected\,Areas_i$ * $U.S.\,antidrug\,Expenditures_t$ as an instrument. Each row in the table reports the results of a separate regression that studies the impact of spraying on each of the independent variables listed above. The estimates correspond to the data set by municipality units. The sample includes all Colombian municipalities that had a positive number of hectares of coca cultivated between 2001 and 2010. Each regression included fixed effects by municipality and year except the regression in which poverty rates are used as a independent variable. Column (1) presents the effect of the program 1 to 12 months after the treatment reception, column (2) presents the effect 13 to 24 months after the treatment reception, and column (3) presents the effect of the program 25 to 36 months after the treatment implementation. Clustered errors at the municipality level are presented in parentheses.* Significant at 10%, ** Significant at 5%, and *** Significant at 1%.

Table 2.8: First Stage Results (Producer Sample)

Independent Variables	Dependent Variable: $I(Sprayed > 0)$ (1)	(2)	(3)
$Instrument_{it}$	0.03***		
	(0.00)		
$Min\,Distance\,to\,Protected\,Areas_i$		0.02***	
		(0.00)	
$U.S.\,International\,Supply\,Anti\text{-}drug\,Expenditures_t$			0.73***
			(0.05)
Covariates	X	X	X
R-squared	0.46	0.45	0.43
Partial R-squared	0.1	0.08	0.13
F (excluded instrument)	29.3	13.77	160.9
Observations	2102	2102	2102

Note: The table presents the first stage regression of the equations (1) and (2). The estimates correspond to the data collected at the producer level by the United Nations Office of Drugs and Crime (UNODC). The sample consists of two rounds of cross sections, one collected between 2005 and 2006, and the second between 2007 and 2010. The producers that were surveyed were selected randomly from the areas with coca. $I(Sprayed > 0)$ corresponds to an indicator variable that takes the value of one if the producer was sprayed 12 months before the survey. $Min\,Distance\,to\,Protected\,Areas$ represents the minimum distance between each producer and the nearest border to a protected area. U.S. international antidrug expenditures are expressed in real billions of dollars of 2010, and $Instrument_{it} = Min\,Distance\,to\,Protected\,Areas_i * U.S.\,antidrug\,Expenditures_t$. The covariates included in the regressions were age, education, and gender. The estimates also included dummies for year, region, department, and municipality. Only the estimations with the U.S. Expenditures do not included dummies for year. Robust standard errors are presented in parentheses. * Significant at 10%, ** Significant at 5%, and *** Significant at 1%.

Table 2.9: Impact of Spraying on Drug Production (Producer Sample)

	Dependent Variables					
	Coca (ha)		Kgs/ Ha		N. Harvest	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
Indp. Variable	(1)	(2)	(3)	(4)	(5)	(6)
$I(Sprayed > 0)$	-0.04**	-0.31***	-76.60**	-81.63**	-0.93***	-1.17***
	(0.01)	(0.02)	(34.22)	(37.70)	(0.22)	(0.36)
Covariates	X	X	X	X	X	X
R-squared	0.35	0.18	0.48	0.40	0.60	0.60
Observations	2099	2099	2099	2099	2099	2099
	Mean Values					
Coca (ha)	1.15					
Kgs/ Ha	1022.41					
N of Harvests	4.48					
$I(Sprayed > 0)$	0.23					

Note: The table reports the estimates of equation (1) and (2) by OLS and 2SLS. The estimates correspond to the micro data collected at the producer level by the United Nations Office of Drugs and Crime (UNODC).The sample consists of two rounds of cross sections, one collected between 2005 and 2006, and the second between 2007 and 2010. The producers that were surveyed were selected randomly from the areas with coca. $I(Sprayed > 0)$ corresponds to an indicator variable that takes the value of one if the producer was sprayed 12 months before the survey. Columns (2), (4) and (6) report the results of an instrumental variables regression using $Min\, Distance\, to\, Protected\, Areas_i * U.S.\, antidrug\, Expenditures_t$ as an instrument. $Coca$ represents the number of hectares of coca cultivated by each producer, Kgs/Ha is a proxy for productivity that measures the total kilograms of coca produced per hectare cultivated, and $N.\, Harvest$ measures the number of times producers collect the coca crops per year. The covariates included at the producer level were age, education and gender. The estimates included dummies for year, region, department, and municipality. Robust standard errors are presented in parentheses. * Significant at 10%, ** Significant at 5%, and *** Significant at 1%.

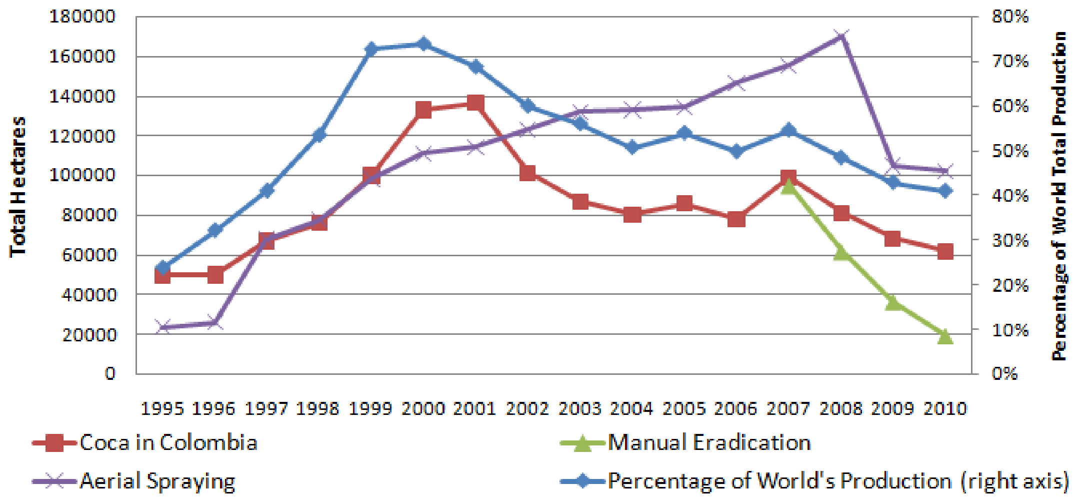

Figure 2.1: Coca Production, Aerial Spraying and Manual Eradication in Hectares

Note: Hectares of coca cultivated and hectares manually eradicated come from UNODC. The data on total hectares aerially sprayed comes from the Colombian Antinarcotics Police. The 'percentage of world's production' corresponds to the Colombian coca production as a percentage of the world's production, which amounts to the aggregate coca production of Bolivia, Peru, and Colombia.

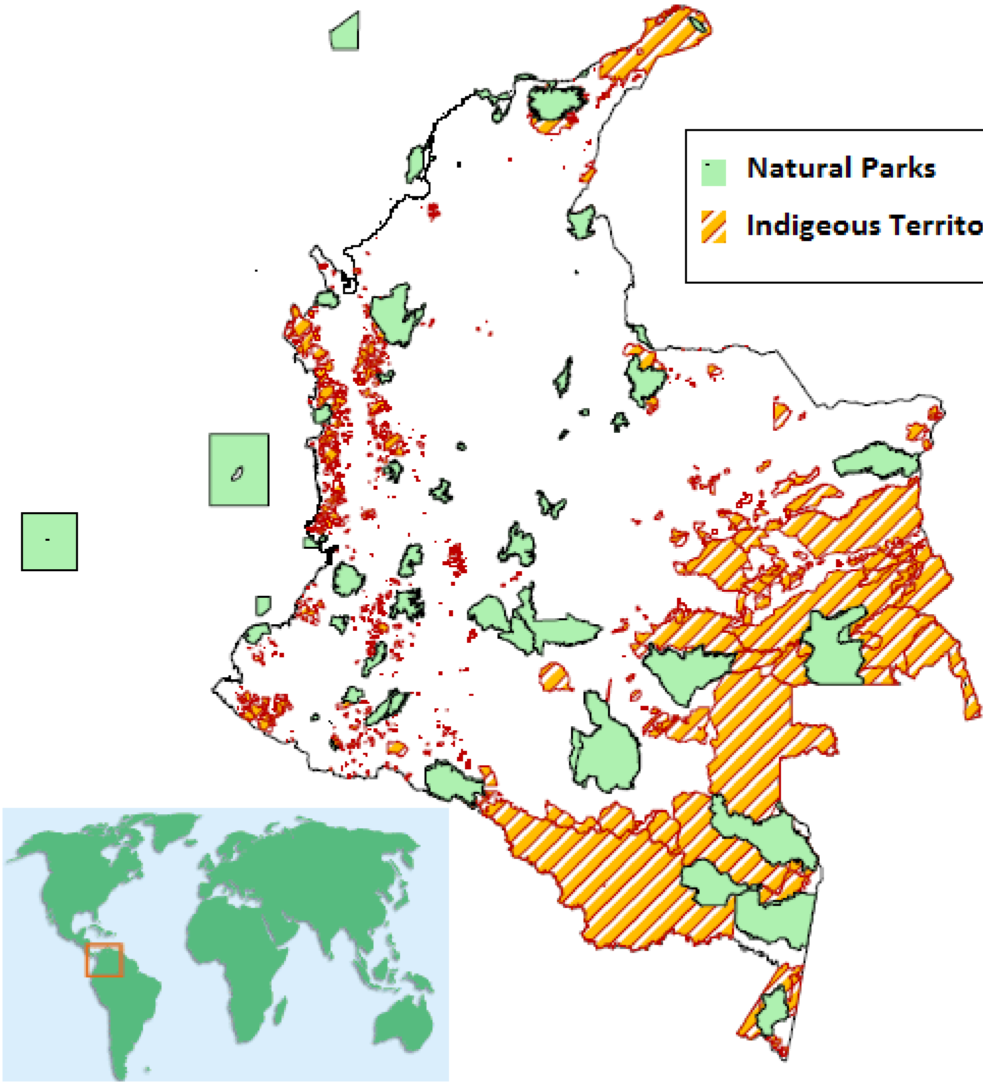

Figure 2.2: Location of Protected Areas in Colombia

Note: This figure presents the geographic location of natural parks and indigenous territories in Colombia. By governmental mandate, natural parks and indigenous territories cannot be sprayed in Colombia. Natural parks and indigenous territories comprise 12% and 27.6% of the Colombian territory, respectively. The source of the geographical location of protected areas is the National Geographical Institution in Colombia (i.e., Instituto Geografico Agustin Codazzi).

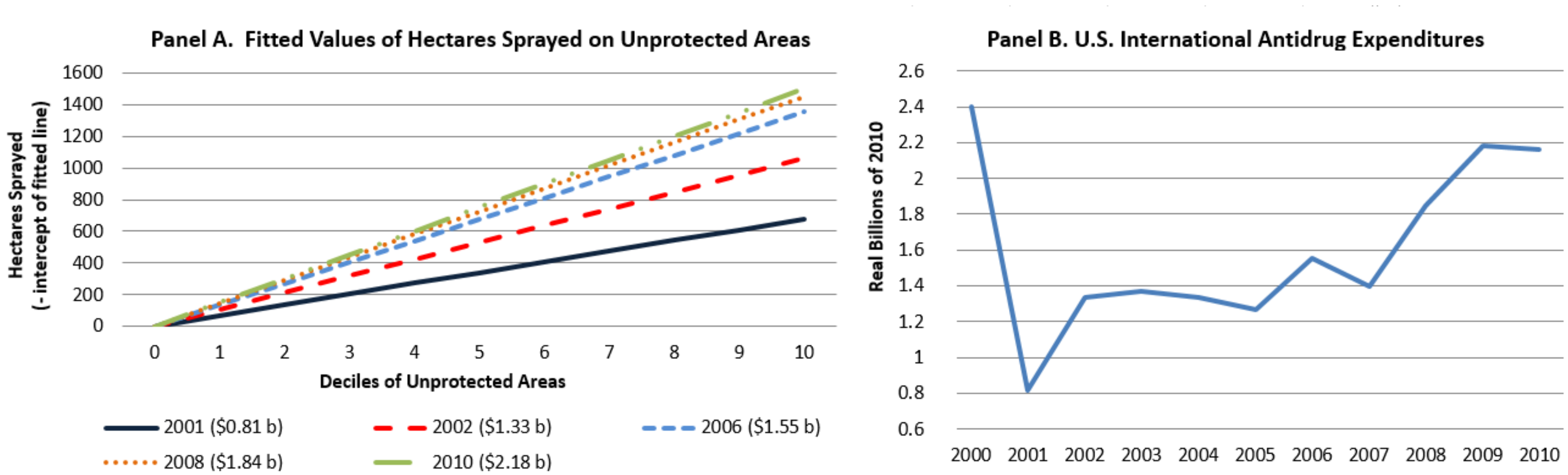

Figure 2.3: Instrument Strength and Time Evolution of U.S. Antidrug Expenditures

Note: Panel A was constructed by aggregating the grid data by municipality. It presents a fitted line of the total number of hectares sprayed by deciles of the share of unprotected area in each municipality. Higher deciles of *Unprotected Areas* correspond to municipalities with a lower share of protected areas in its territory. The panel presents a fitted line for years with different U.S. international antidrug expenditures (values are presented in the legend on parentheses in Panel A). During these years U.S. international antidrug expenditures expressed in real billions of dollars of 2010 were increasing (see Panel B). The figure suggests that: (i) municipalities with a higher share of non-protected areas had a higher number of hectares sprayed, and (ii) in years when the U.S. antidrug expenditures were higher (as shown in Panel B), the intensity of treatment increased more for non-protected areas.

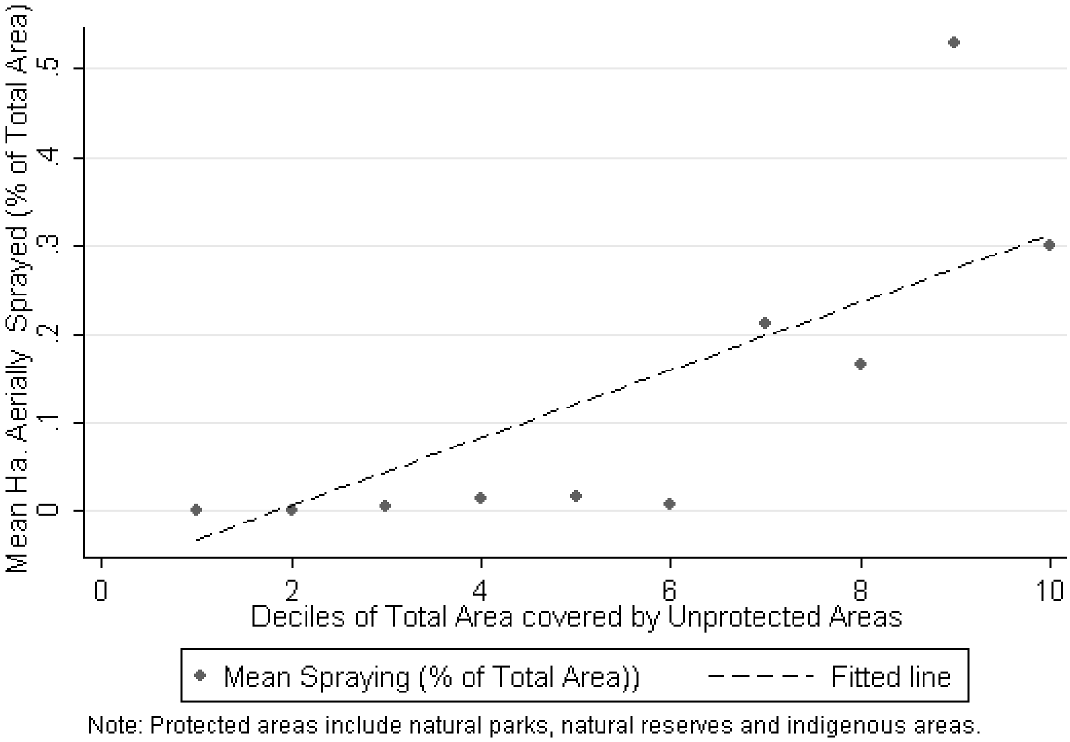

Figure 2.4: Aerial Spraying in Unprotected Areas

Note: This figure was constructed with data at the municipality level. It shows the mean hectares of area sprayed as a percentage of total area in each municipality against deciles of the share of area covered by unprotected areas. It confirms that municipalities with a lower share of protected areas have a higher number of hectares aerially sprayed.

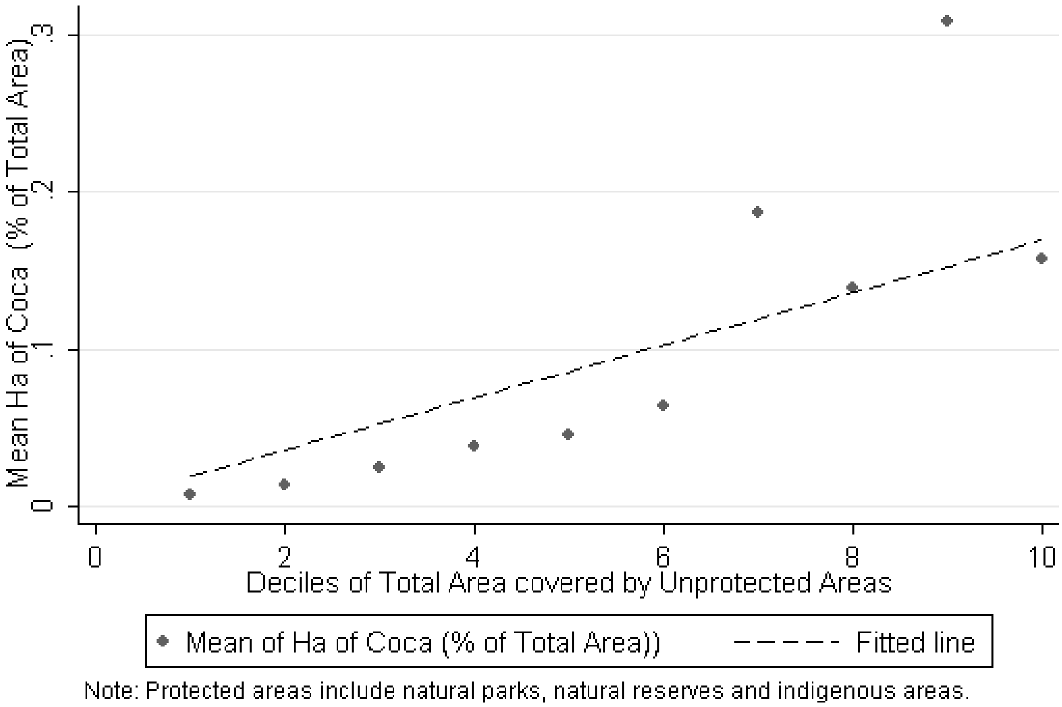

Figure 2.5: Coca Cultivation in Unprotected Areas

Note: This figure was constructed with data at the municipality level. It shows the mean hectares of coca cultivated as a percentage of total area in each municipality against deciles of the share of area covered by unprotected areas. It confirms that municipalities with a higher share of protected areas do not have a higher number of hectares of coca cultivated.

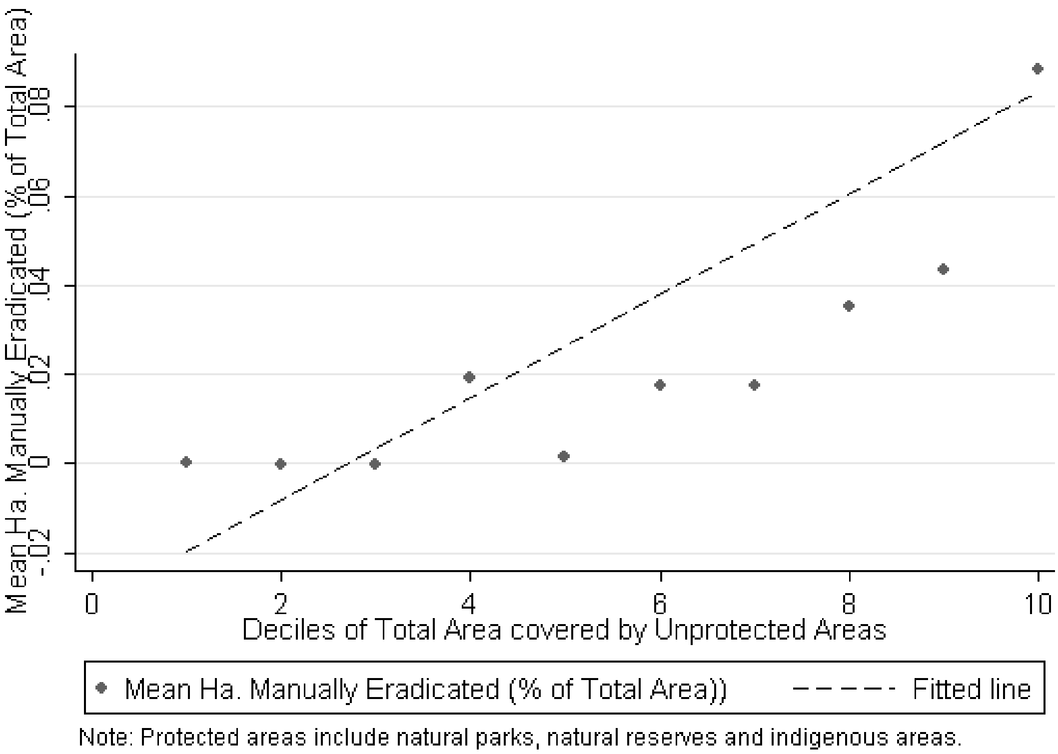

Figure 2.6: Manual Eradication in Unprotected Areas

Note: This figure was constructed with data at the municipality level. It shows the mean hectares manually eradicated as a percentage of total area in each municipality against deciles of the share of area covered by unprotected areas. It confirms that municipalities with a higher share of protected areas do not have a higher number of hectares manually eradicated.

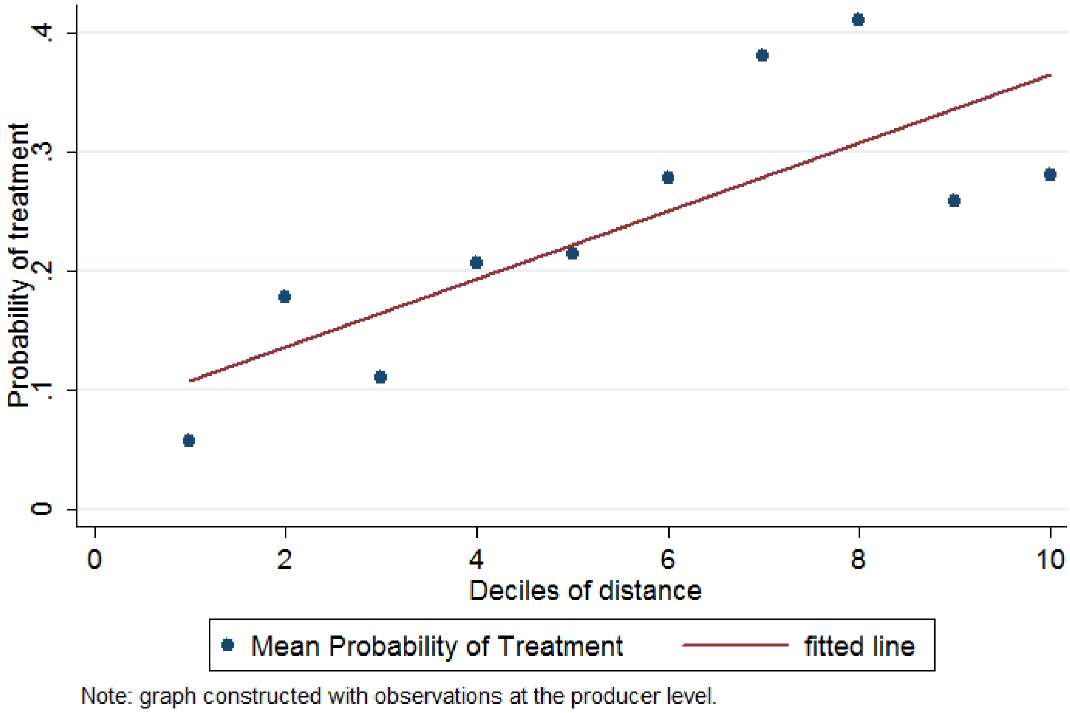

Figure 2.7: Distance to Nearest Protected Area and Probability of Treatment

Note: This figure was constructed with data collected at the producer level. It shows the probability that a producer was aerially sprayed against deciles of the minimum distance of each producer to the nearest protected area. It confirms that producers located farther away from protected areas have a higher probability of being sprayed.

2.9 Appendices

G- Descriptive Statistics and Sources

Table G.1: Descriptive Statistics - Grid Sample

	Mean	St Deviation
Manually Eradicated (Ha)	0.11	1.51
Aerial Spraying (Ha)	0.54	26.89
Coca	0.84	2.46
N of Observations	1115840	
N of Groups	101440	
Years	11	
Period	2000 to 2010	

Note: this table presents the descriptive statistics of a panel data set with grid units. Each grid corresponds to an area of 1 km^2. The sample includes all the grids in Colombia that had a positive number of hectares of coca cropped between 2000 and 2010.

Table G.2: Data Sources - Municipality Sample

Outcome	Variable	Source
Drugs	Aerial Spraying	Antinarcotics National Police (DIRAN)
	Manual Eradication	UNODC
	Hectares of Coca	UNODC
Violence	Homicide Rates	Vicepresidency
	Armed Confrontations	Vicepresidency
	Displaced Individuals	Administrative Dep. For Social Prosperity
Education	Primary Enrollment Rate	Ministry of Education
	Secondary Enrollment Rate	Ministry of Education
	School Drop-Out Rate	Ministry of Education
Health	Infant Mortality	National Statistical Department (DANE)
Poverty	Unsatisfied Basic Needs	National Statistical Department (DANE)
	Quality of Life Index	National Planning Department
	Poverty Rate	Constructed with data from the 2005 (CEDE)

Note: this table describes the sources of the variables available in the sample by municipality. The sample includes all the Colombian municipalities that had a positive number of hectares of coca cropped between 2001 and 2010. They account for 288 muncipalities.

Table G.3: Variable Definitions- Municipality Sample

Variable	Definition	Years
Homicide Rates	Homicides /100,000 pop	2001-2010
Armed Confrontations	Number of actions	2001-2010
Displaced Individuals	Number of individuals	2001-2010
Primary Enrollment Rate	(Registered students/Pop in age)*100	2005-2010
Secondary Enrollment Rate	(Registered students/Pop in age)*100	2005-2010
School Dropout Rate	(Registered students/students that finish academic year)*100	2007-2009
Infant Mortality	(Deaths of ind. younger than 1 year / Ind. born alive)*100	2006, 2007
Unsatisfied Basic Needs	(Indv with unsatisfied need/Total pop)*100	2005 and 2010
Quality of Life Index	Maximum Value (excellent conditions)=100, Min Value=0	2005
Poverty Rate	Percentage of rural pop under poverty line*	2005

Note: this table describes the definitions and years of availability of the variables included in the sample by municipality. The sample includes all the Colombian municipalities that had a positive number of hectares of coca cropped between 2001 and 2010. They account for 288 muncipalities.

Table G.4: Descriptive Statistics - Municipality Sample

	Observations	Mean	St Dev
Sprayed	2680	429.6385	1615.627
Manual Eradication	1072	70.24467	1058.197
Coca	2680	290.6657	868.6115
Homicide Rates	2680	54.90541	66.80186
Displaced Individuals	2680	582.6216	1242.691
Primary Enrollment Rate	1340	129.3728	37.45113
Secondary Enrollment Rate	1340	71.43532	29.17269
School Drop-Out Rate	804	10.69174	5.798444
Infant Mortality	536	44.03243	18.23138
Poverty Rate	268	0.5698644	0.093297

Note: this table presents the descriptive statistics of a panel data set by municipality. The sample includes all the municipalities in Colombia that had a positive number of hectares of coca cropped between 2000 and 2010.

H- Spillover Effects

Table H.1: Results of Equation (3)- (Municipality Sample)

Dependent Variable: Ha of Coca in Area not Sprayed in t-1			
Independent Variable	(1)	(2)	(3)
Ha Sprayed in t-1	0.1***	0.1***	-0.11***
	(0.01)	(0.01)	(0.03)
R-squared	0.02	0.04	0.005
Observations	2880	2880	2880
N of Clusters	288	288	288
Year FE		X	X
Mun FE			X

Note: this table presents the results of the regression of equation (3) by OLS. The estimates correspond to the micro data set by municipality units. The sample includes all Colombian municipalities that had a positive number of hectares of coca cropped between 2001 and 2010. $Ha\,Sprayed\,in\,t-1$ represents the total ha sprayed in municipality i in $t-1$, and the dependent variable is the total hectares of coca cropped in the municipalities that belong to the same department as municipality i but which were not treated in $t-1$ or in t. Clustered standard errors at the municipality level are presented in parentheses. Regressions include dummies for region. *** Significant at 1% level.

I- Descriptive Statistics for Producer's Sample

Table I.1: Descriptive Statistics

	2005-2006 - Total		2007-2010 - Total	
Variable	Mean	St Dev	Mean	St Dev
Gender	0.9087222	0.2881076	0.936095	0.2446904
Age	38.34148	11.35844	40.6126	11.69249
Education (Years)	3.582412	1.497889	4.064167	1.996461
Experience	6.644788	4.298623	6.771643	3.579531
N. Household Members	5.102483	2.250969	5.016029	3.34812
Coca 1st Eco. Activity	0.9698634	0.1710246	0.8681664	0.3384575
Sell Coca Leaf	0.3406667	0.4741041	0.5041651	0.5002009
Area of Farm (Ha)	19.88769	38.68512	16.6291	32.21931
N. of Workers /Ha of coca	4.880347	4.663753	3.95868	4.822073
N. Workers / Ha of coca	6.053402	7.929141	9.868221	8.04295
Harvested Area	1.071285	0.864355	1.081115	0.953343
N. Harvest/Year	4.360391	2.039785	4.33752	1.383656
Kgs of Coca/Ha coca	1097.494	398.098	928.2207	410.5222
Number of obs	1389		1146	

Note: this table presents the descriptive statistics of the micro data set collected at the producer level by the United Nations Office of Drugs and Crime (UNODC). The sample consists of two rounds of cross sections, one collected between 2005 and 2006, and the second between 2007 and 2010. The coca-producers that were surveyed were selected randomly from the areas with coca.

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CHAPTER 3

On the Effects of Enforcement on Illegal Markets: Evidence from a Quasi-experiment

3.1 Introduction

Illegal activities such as counterfeiting, tax evasion, and the operation of illegal drug markets remain a serious problem throughout the world. Yet, there is still open debate on how to design and implement public policies to reduce their extent. The economics analysis of crime suggests that the decision to engage in illegal activities is rational and, as such, is shaped by incentives and penalties (see Becker, 1968 and Stigler, 1970). The central prediction is that enforcement reduces crime by increasing its costs. Despite its theoretical appeal, social scientists and pundits have raised several concerns about this view. In particular, critics have argued that criminals may be irrational, myopic, or predisposed to illegal behavior (Menninger, 1968); that extrinsic penalties crowd out intrinsic motivations (Frey, 1997); or that enforcement may backfire if it conveys information about widespread illegal behavior (Benabou and Tirole, 2003, 2006). Apart from the theoretical controversies, evidence on the role of enforcement in reducing illegal behavior is not abundant in part, due to the lack of the exogenous sources of variation in enforcement required to uncover its causal impact.

Our paper contributes to the growing literature that attempts to estimate the causal effect of enforcement on illegal activities. We use the war on drugs in Colombia as a case study, focusing on the role of aerial spraying with herbicides in curbing illegal coca cultivation. At least since 1996, Colombia has been the world's largest cocaine producer and grower of coca crops (the raw input for cocaine production). Coca cultivation takes place in remote areas of the country with little institutional presence, where farmers face the risk of being detected and sprayed with herbicides by the government. When coca crops are sprayed with herbicides they are partially lost, which increases the cost of this illegal activity and reduces the farmers incentive to pursue it. Our goal in this paper is to assess the effectiveness of this form of enforcement, and explore how it affects farmers' behavior.

For this purpose, we exploit the geographic and time variation on aerial spraying induced by a diplomatic friction between the governments of Ecuador and Colombia around 2006. Around the year 2000, the Ecuadorian government alleged that Colombian aerial spraying campaigns near the frontier were causing health problems, productivity losses, and environmental damage in their territory. In response, the Colombian government committed to completely stop aerial spraying campaigns within a 10 km band around the international frontier with Ecuador at the beginning of 2006. The Colombian government broke its commitment at the end of 2006 and continued spraying within the band throughout 2007. However, this aerial spraying stopped in 2008 in response to a lawsuit filed by the Ecuadorian government in international courts.

We use satellite and geo-referenced data of coca cultivation and aerial spraying on 1-square-km (100 hectares) cells between 2000 to 2010, and estimate the effect of aerial spraying using two methodologies. First, we use a fuzzy regression discontinuity design and compare coca cultivation in cells near both sides of the 10 km threshold. We show that aerial spraying changes discontinuously at the 10 km threshold during the years in which Colombia agreed not to spray the exclusion area (except in 2009), while all other covariates, including coca cultivation do not. This provides us with a discontinuous change in the likelihood of enforcement that we can use to identify its effect on illegal coca cultivation. Importantly, we document that the reduction in spraying since 2006 near Ecuador was not compensated for by other types of enforcement (i.e., manual eradication of coca crops).

Additionally, we report results obtained by conditional differences in differences. In particular, we compare the cultivation of illicit crops in cells within the exclusion area to that in similar cells located 10 to 20 km away from the frontier – the area that continued to be sprayed throughout the years in our sample. Both groups of cells were exposed to aerial spraying before 2006, but after that, only the latter cells continued to be sprayed. Thus, the difference in coca cultivation between both regions since 2006 can be attributed to the change in enforcement. To guarantee the comparability of both groups, we control for coca cultivation and spraying before the intervention, and use a variety of techniques to control non-parametrically for these predetermined characteristics.

Consistent with the view that illegal behavior is a rational choice, we find significant (but small) deterrent effects of spraying on coca cultivation. The regression discontinuity estimates imply that cells in the sprayed area near the cutoff had approximately 10% higher likelihood of being sprayed than cells in the exclusion area near the cutoff. As a result, cultivation was reduced from 0.18 to 0.34 hectares per square kilometer in the former group relative to the latter. Similarly, our estimates using the conditional differences in differences methodology suggest that the areas that were exposed to aerial spraying after 2006 faced approximately 10% higher likelihood of being sprayed and, as a result, had on average 0.25 fewer hectares of coca per square kilometer (relative to cells in the region not sprayed). Both methodologies suggest that spraying an additional hectare (a 1% increase in spraying campaigns) reduces coca cultivation by between 0.018 and 0.034 hectares in a given year.

Our findings confirm the key insight from the economics of crime. Namely that enforcement in the form of a higher likelihood of being sprayed with herbicides dissuades farmers from growing illegal crops. However, these effects are too small when compared to the costs of this policy. In particular, our largest point estimates suggest that to reduce coca cultivation by 1 hectare, approximately 30 additional hectares must be sprayed every year. Moreover, it is possible that coca cultivation is in part displaced by aerial spraying campaigns, making the 30 hectares a lower bound. The average direct cost to the U.S. of spraying one hectare of coca crops in Colombia is estimated to be about $750 dollars (DNE, 2004, cited in Walsh et al., 2008). According to official sources, for each dollar the U.S. spends on the spraying program, the Colombian government spends about 2.2 dollars protecting the spraying crews and cleaning up the area before they carry out these campaigns. Thus, the joint cost of spraying 30 hectares of coca, and reducing cultivation by 1 hectare per year, is about $72,000 dollars, out of which the U.S. pays at least $22,500. As we show in greater detail in the paper, these numbers imply that the marginal cost to the U.S. of reducing coca supply in retail markets by 1 kg through subsidizing aerial spraying policies in Colombia is at least $0.9 million dollars, which is in the ballpark of the costs reported by Mejia and Restrepo (2013) using a different methodology. This is significantly higher than the same cost for other policies, such as interdiction in Colombia ($181,000 dollars; see Mejia and Restrepo, 2013), or treatment and prevention in the U.S. ($8,250 and $68,750 dollars, respectively; see MacCoun and Reuter, 2001). It is also high when compared to the retail price of 1 kg of pure cocaine in U.S. retail markets, which ranges from $100,000 to $150,000 dollars.

In addition to providing evidence on the link between enforcement and illegal behavior, estimating the impact of aerial spraying on coca cultivation is important for several reasons. First, Colombia is a key case in terms of anti-narcotic policy. During our period of analysis, it was the largest cocaine producer nation, covering nearly 70% of the total supply and a similar proportion of total coca cultivation in the Andean region. Second, effective supply reduction policies in Colombia have the potential to reduce the availability of cocaine and its associated harms throughout the world. In fact, most of the cocaine produced in Colombia is exported, and between 60% and 70% of the cocaine consumed worldwide is produced in Colombia (UNODC, 2012). Third, aerial spraying is the largest anti-drug program implemented in Colombia. It entails not only resources from the local government but also from the U.S. In particular, since the beginning of *Plan Colombia* in 2000 – the largest cooperative effort between the U.S. and a source country to curb drug supply and improve security conditions – aerial spraying has been the most significant component, with both countries spending more than $3 billion dollars. Finally, illegal behavior in Colombia is pervasive, and understanding how to reduce it a key policy challenge.

The rest of the paper is organized as follows. Section 2 describes the related literature; section 3 describes the Colombian context and the natural experiment used to identify the causal impact of aerial spraying on coca cultivation. Section 4 presents the data and estimates the effects of spraying. Section 5 discusses the main results and presents a cost-benefit analysis of the aerial spraying program. Finally, section 6 concludes.

3.2 Related literature

Our paper is related to two branches of economics literature. First, it is related to the literature on the effects of enforcement on crime. This topic goes back to the seminal contributions of Becker (1969), Stigler (1970), and Ehrlich (1973). The main implication of these models is that enforcement– in the form of fines, tighter punishments, or a higher probability of detection– reduces crime and illegal behavior. Yet, testing this proposition is challenging as it requires credible sources of exogenous variation in enforcement. Otherwise, the fact that enforcement reacts to crime creates a misleading upward bias in the estimated effect of enforcement on crime. Initially, the economics literature failed to find empirical support for this proposition (see Cameron, 1988, Marvell and Moody, 1996, and Eck and Maguire, 2000, for surveys of the early literature), but many of these contributions were plagued with endogeneity issues.

Recent studies have addressed identification more carefully. For instance, Marvell and Moody (1996) find that within-state increases in the number of police officers reduce crime in the U.S. Levitt (1997) uses electoral cycles as an instrument for police hiring and finds significant reductions in crime when more policemen are hired[1]. Corman and Mocan (2000) use high frequency changes in the number of police officers, arguing that the variation is exogenous due to administrative burdens in the hiring process of police officers. They find that more police officers causes a reduction in burglaries, but no effect on other crime categories. Di Tella and Schargrodsky (2004) exploit the exogenous reallocation of police forces across Buenos Aires that came as a result of a terrorist attack on a Jewish Center. They find a large and localized deterrent effect of more police presence on car thefts. A similar strategy is used by Draca, Machin, and Witt (2011), who also find evidence of deterrence effects by exploiting police reallocation in London after the terrorist attacks of 2005. Evans and Owens (2007) use state grants to fund Community Oriented Policing Services (COPS) as an instrument for the number of police officers. They find that higher police presence reduces auto thefts, burglaries, robberies, and aggravated assaults. Buonanno and Mastrobuoni (2012) exploit delays created by a centralized police hiring system in Italy to estimate the effect of police officers on local crime, finding deterrence effects in some crime categories. Finally, Garcia, Mejia, and Ortega (2012) study the randomized introduction of a police training program among small localities in Bogota, Colombia. They find that the intervention significantly reduces crime, not by increasing the

[1]See also the criticism by McCrary, (2002), and the reply by Levitt, (2002).

police force, but by improving its quality and engagement with the community.

Another body of literature focuses on the effects of enforcement, or characteristics related to the likelihood of detection, on soft crime or tax evasion.[2] For example, Bar-Ilan and Sacerdote (2001) find that the introduction of traffic cameras and changes in fines reduced driving infractions. Dubin, Graetz, and Wilde (1987) and Beron, Tauchen, and Witte (1992) present evidence that higher audit rates modestly increase reported income for some groups of taxpayers. In this same area, Klepper and Nagin (1989) find that noncompliance rates are related to the traceability, deniability, and ambiguity of the items being declared, which are in turn related to the probability that evasion will be detected and punished; and Kagan (1989) presents evidence that compliance is greater among people whose income is directly reported to the IRS and who therefore have fewer opportunities to cheat.

We contribute to this literature by cleanly identifying the effect of enforcement on illegal behavior in the context of the war on drugs and illicit crop cultivation in Colombia. The strength of our empirical exercise relies not only on our identification strategy, but also on the precision of our data on illicit crop cultivation and enforcement activities. In particular, we observe satellite data on coca cultivation in small 1-squared-km cells, and information on the exact location of spraying and manual eradication policies is recovered from GPS devices. Consistent with the previous findings on the literature, our results suggest that enforcement, on the form of a greater likelihood of being aerially sprayed, reduces illicit coca cultivation by farmers.

Our paper is also related to the branch of applied economic literature on the cost-effectiveness of anti-drug policies. The main challenge in this area is that anti-drug interventions typically take place on a large scale; hence, it is difficult to obtain appropriate counterfactuals. One approach, followed by Meja and Restrepo (2011, 2013), is to construct and calibrate economic models of illegal drug markets to understand and quantify the main forces and determinants of the cost, effectiveness, and efficiency of different anti-drug strategies in producer and transit countries. Their main result is that spraying illicit crops is costly and ineffective relative to policies aimed at seizing drug shipments. However, both strategies are costly relative to demand reduction policies in consumer countries.

Other papers in the literature have focused on estimating the impact of spraying campaigns on coca cultivation by using geographic and time variation. For example, Moreno-Sanchez et al. (2003) and Dion and Russler (2008) use departmental data from Colombia and find a positive correlation between the levels of spraying and the presence of coca crops. However, these results are likely to be driven by simultaneity bias in their estimates. In particular, spraying is higher in areas with more cultivation, creating an upward bias in OLS estimates. Recent studies have attempted to address these endogeneity concerns. For example,

[2]For a thorough review of the empirical literature, see Andreoni, Erard, and Feinstein (1998).

Moya (2005) uses matching techniques employing municipal data from Colombia and finds spraying does not have a significant effect on coca crops. Yet, the comparison between municipalities may still be subject to omitted variable bias even after matching on observables. Reyes (2011) instruments spraying with the distance between sprayed areas and the closest military base, and finds evidence that aerial spraying increases illicit crops. However, the main limitation of this study is that the exogeneity of the location of military bases is hard to justify. Finally, Rozo (2014) instruments spraying with the interaction between the distance between each 1-square-km cell (or coca producer) and the nearest border of a protected area and the U.S. international anti-drug expenditures. The author exploits the fact that by governmental mandate protected areas cannot be sprayed with herbicides due to environmental and social concerns. Her results indicate that aerial spraying has a negative and significant effect on the hectares of coca cropped and coca producers productivity, but that, at the same time, it causes negative unintended consequences on the socio-economic conditions of coca-producing areas.

This paper contributes to the existing evidence by estimating the effects of aerial spraying programs using a sharp natural experiment. We also use cost figures to back up a lower bound for the cost effectiveness of these programs. We find that despite reducing cultivation, aerial spraying is too costly to be a useful anti-narcotic policy. In particular, demand reduction policies in the U.S. or interdiction campaigns provide the same benefits in terms of supply reduction at much lower costs.

3.3 The Colombian context and the natural experiment

Following the large increase in coca cultivation that took place in Colombia after 1994 and the increasing involvement of illegal armed groups in these activities, in September of 1999 the governments of Colombia and the U.S. launched a joint strategy which would come to be known as the *Plan Colombia*. According to official figures, 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* (see DNP, 2006). Between 2000 and 2008 total expenditures on the military component of *Plan Colombia* represented close to $1.2 billion dollars per year, corresponding to 1.1% of the country's annual GDP, making it the largest anti-drug intervention in a producing country.

The strategies implemented under *Plan Colombia* included aerial spraying campaigns, manual eradication campaigns, control of chemical precursors used in the processing of coca leaf into cocaine, detection and destruction of cocaine processing laboratories, and seizure of drug shipments en route to foreign countries. Aerial spraying has been by far the main anti-drug strategy in terms of financial 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 (states) bordering Ecuador, where our empirical analysis is centered. Figure 3.1 shows the evolution of hectares with coca cultivation, aerial spraying with herbicides, and manual eradication for the whole country and for the departments of Nariño and Putumayo in the last years. About a third of total coca cultivation and half of overall aerial spraying in Colombia between 2000 and 2010 took place in the departments of Putumayo and Nariño.

Spraying campaigns are carried out by American contractors, such as DynCorp, using small aircraft. Coca crops are sprayed with substances such as Roundup, whose main active ingredient is glyphosate. Glyphosate is absorbed through the plant foliage and is effective only on growing plants (e.g., it is not effective in preventing seeds from germinating). It kills the plant by inhibiting its growth. Though Roundup was designed to kill weeds and grasses, including coca bushes, it may also affect other legal crops that are not glyphosate-resistant. Aerial spraying with glyphosate is targeted at areas where coca crops have been detected using satellite images, implying that areas with coca crops are much more likely to be sprayed and destroyed by this enforcement strategy.

Hence, farmers that grow coca bushes face the risk of having their crops destroyed by herbicides used in aerial spraying campaigns. Given this risk, they may still grow coca bushes and play their luck, or mitigate the effects of the herbicide using a variety of techniques. For instance, farmers can spray molasses on the coca bushes to prevent the herbicide from penetrating the foliage and killing the plant. In addition, they can cut the stem of the plant a few hours after the fumigation event, enabling the plant to grow back a few months later. Finally, farmers can reallocate their crops to areas less likely to be sprayed. However, these alternatives are costly, which forces some farmers to start cultivating solely legal crops that are not targeted by spraying campaigns. This is the intended effect of aerial spraying that we measure in this paper.

Because aerial spraying campaigns typically target areas with a high prevalence of coca plantations, traditional estimates of the effect of spraying on cultivation are biased upwards. In this paper, we solve this problem and identify the effects of aerial spraying using a natural experiment. In particular, we exploit a natural experiment resulting from a diplomatic friction between the governments of Colombia and Ecuador. The friction concluded in the compromise by the Colombian government not to carry out spraying campaigns within a 10 km strip along the international border with Ecuador starting in 2006.

From the beginning of fumigation under *Plan Colombia*, the Ecuadorian government complained of alleged adverse effects of spraying on the health of its population, the environment, livestock, and legal crops near the bordering area.

In 2006, the Colombian government announced that it would discontinue aerial spraying within a 10 km band along the international frontier with Ecuador within Colombian territory. However, the Colombian government recanted at the end of 2006 and continued the spraying campaigns in the area. As a result of this non-compliance with the initial agreement, the Ecuadorian government filed a lawsuit against Colombia in the International Court of Justice in The Hague. Since, the suit was filed, on March 31st, 2008, the Colombian government has stopped all spraying campaigns within the 10 km strip.

The implementation of this exclusion area generated geographical and time variation that we exploit to identify the effects of aerial spraying. Figure 3.2 shows a map of the exclusion strip and its location in Colombia.

3.4 Data

We employ unique panel data on the location of coca crops within 1-square-kilometer (or 100 hectares) cells from 2000 to 2010. The data is collected and processed by the United Nations Office for Drugs and Crime (UNODC) in Colombia, and comes from satellite images. The satellite images show the number of hectares with coca cultivation detected on each grid by the end of that year. We also use cell level data on the number of hectares sprayed for the same period. The data is collected from GPS devices installed in the aircraft used in aerial spraying campaigns, and it records the exact location of the plane when the spraying valves are open. Using these observations we code a dummy of whether a grid was sprayed or not, for each year from 2000 to 2010. Moreover, we use data on whether manual eradication campaigns took place on each grid, covering the 2007-2010 sub-period. These data are obtained from GPS devices used by manual eradication teams.

We restrict our sample to all grid points with centroids located within 20 km of the international frontier with Ecuador. Our sample includes 10,880 cells. We refer to the 5,613 cells within 10 km of the frontier as the exclusion region, since this is the area that Colombia agreed not to spray. In contrast, we refer to the 5,275 cells located 10 km to 20 km from the frontier as the sprayed area, as these cells were sprayed throughout our period of analysis. Both regions are depicted in Figure 3.2.

To summarize the data, Figure 3.3 presents the likelihood of aerial spraying and coca crops per square kilometer in both regions from 2000 to 2010. As anticipated above, the figure reveals similar patterns in aerial spraying until 2003 and in 2005. A significant gap opens beginning in 2006, when the Colombian government first agreed to reduce the spraying campaigns in the exclusion area. The difference becomes after 2007, when the likelihood of spraying is reduced to zero in the exclusion region (though some cells in this region were sprayed) and increased in the sprayed area. The data on cultivation reveals a sharp decline from 2000 to

2004, during the first years of *Plan Colombia*, from about 3 hectares per square kilometer to about 0.5. However, in 2006, and later in 2009 and 2010, cultivation increased in the exclusion region relative to the sprayed area.

3.5 Fuzzy regression discontinuity approach

In this section we employ a fuzzy regression discontinuity design to evaluate the impact of aerial spraying on coca cultivation. We exploit the exogenous rule applied by the Colombian government in 2006, and implemented again from 2008 onwards, to stop aerial spraying 10 kms around the international frontier.

In our setting, the forcing variable is the distance from the centroid of each cell i to the international frontier with Ecuador. We normalize the forcing variable to take the value of zero at the 10 km cutoff, and denote it by $\widehat{D}_i$, where $\widehat{D}_i = D_i - 10km$. In this exercise, we exclude from our sample all cells that had their centroid in the first 500 m around the cutoff value, since they have a significant portion of their territory in both the exclusion and the sprayed area. Thus, we only compare cells near the 10 km cutoff lying entirely on one side or the other, and exploit the discontinuity in enforcement created by the Colombian commitment not to spray the cells entirely in the exclusion region.[3] Our discussion above implies that in the remaining sample of cells, there should be a discontinuity in aerial spraying around $\widehat{D}_i = 0$ in 2006, and from 2008 onwards, assuming that the Colombian government fulfilled its commitment strictly during those years. On the contrary, there should be no discontinuity for years before 2006. For 2007, although a formal agreement was not in place, spraying may have been lower in the exclusion area due to the diplomatic friction between Colombia and Ecuador.

Let S_{it} be a dummy equal to 1 if grid i was sprayed during year t. Our discussion above implies that after 2006, all coca plantations with $\widehat{D}_i < 0$ have a probability of treatment near zero, whereas for those coca plantations located above $\widehat{D}_i > 0$ the probability of treatment jumps to positive values. That is:

$$\lim_{d \downarrow -500} \Pr[S_{it} = 1 | \widehat{D} = d] < \lim_{d \uparrow 500} \Pr[S_{it} = 1 | \widehat{D} = d] \forall t \geq 2006 \tag{3.1}$$

The existence of a discontinuity during these years also depends on the precise implementation of the exclusion area. For instance, there may be imperfect compliance by Colombian authorities around the cutoff, or spraying on the sprayed area near the cutoff may be reduced as well. In both cases, cells in the exclusion area will be less likely to be sprayed, but there need not be a discontinuity.

[3]Alternatively, we also experimented with models that use the cells within 500 m of the cutoff to estimate the conditional expectation of cultivation and the likelihood of spraying as a function of the distance to the cutoff. In these models, we add separate dummies for cells within 0 to 500 m away from the cutoff in the exclusion area, and cells within 0 to 500 m away from the cutoff in the sprayed area. We obtained estimates of similar magnitude and more precise.

We start by exploring whether the policy was implemented in such a way as to create a discontinuity in aerial spraying. We investigate this question by restricting our sample to several bands around the cutoff, including cells with centroids 2.5 km, 2.75 km or 3 km away from the cutoff. We refer to these samples as "discontinuity samples," following Angrist and Pischke. For each discontinuity sample, we estimate the model:

$$S_{it} = \pi_{0t} + \pi_{1t}1\{\widehat{D}_i > 0\} + f_t(\widehat{D}_i) + \varepsilon_{it}, \tag{3.2}$$

for different years, t, or pooling different years. Here, f_t is a polynomial in the forcing variable. We compute standard errors clustering at the cell level and robust against hetereoskedasticity. We also computed standard errors that are robust against some forms of spatial correlation for our main estimates, and obtained slightly larger standard errors. However, these are not reported since they do not change our conclusions and are computationally cumbersome.[4] The coefficient on π_{1t} measures any discontinuity around the cutoff. Our discussion above implies that we expect $\pi_{1t} = 0$ for $t = 2001, \ldots, 2005$, and $\pi_{1t} > 0$ for $t \geq 2006$.

The left panel of Table 3.1 (columns 1 to 3) presents estimates of the difference in enforcement at the discontinuity, π_{it}, for several years and pooled years. The results in each column correspond to different discontinuity samples specified in the top row. In all the models in this table we use a cubic polynomial to approximate the underlying behavior in enforcement as a continuous function of the distance to the cutoff.

The estimates in column 1 use the largest discontinuity sample (grids with centroids $\pm 3km$ around the cutoff) show that before 2006 there is no significant discontinuity. In contrast, during 2006, cells in the sprayed area near the 10 km cutoff were approximately 9% more likely to be sprayed than similar cells in the exclusion region. The discontinuity in the likelihood of spraying is a robust finding for for all years after 2006. When we pool the years 2008 to 2010 – when the spraying campaigns were reduced to nearly zero in the exclusion area – we find that the likelihood of spraying was approximately 10% higher in the sprayed region, relative to close cells in the exclusion area. The results are robust across the different discontinuity samples presented in columns 1, 2, and 3. When using the quadratic polynomial (which seems enough as one moves closer to the cutoff

[4] We used Conley (1999) standard errors that allow for spatial correlation between a cell and the 8 cells located in a 3×3 square of grids around it; or the 24 cells located in a 5×5 square of grids around it. These standard errors were almost identical to our errors that assumed no spatial correlation. We also computed standard errors robust against spatial correlation between a cell and all other cells in a 2.5km radius around it following Hsiang (2010). This has the advantage that also allows us to control for first and second order serial correlation in the errors simultaneously. We obtained slightly larger standard errors (about 4% larger in the worst cases) that did not change any of our conclusions. We also computed standard errors robust against spatial correlation for our main estimates obtained via conditional differences in differences. In this case the standard errors were larger, but they did not change any of the conclusions outlined in that section where we find highly significant effects.

to approximate the underlying CEF), we find estimates of roughly the same size as those reported in Table 3.1. However, these estimates are very imprecise and we do not report them here. Table 3.1 also presents the estimates of the difference in enforcement at the discontinuity when we pool the years 2006 to 2010 – when the diplomatic friction began. They further support our findings.

The previous results can be seen graphically. Figure 3.4 shows the local behavior of the likelihood of aerial spraying on both sides of the 10 km cutoff and for each discontinuity sample (which can be seen on the horizontal axis of each figure). To ease the graphical analysis, we plot cells by the distance of their border to the cutoff, defined as $\widehat{D}_i - 500$ on the right of the cutoff and $\widehat{D}_i + 500$ on the left. By doing so, we remove from the figure the 500 meter band around the cutoff that we excluded from the estimation sample. We use a cubic polynomial to approximate the local behavior on each side of the 10 km cutoff. The plots reveal a clear discontinuity in the likelihood of spraying after 2008, and less so for 2006 and 2007. When we pool the years 2006 to 2010 (after the diplomatic friction began), we find a clear graphical discontinuity in the likelihood of spraying. In contrast, in the top panel of Figure 3.6, which is constructed in a similar way, we see no apparent discontinuity in the likelihood of enforcement for all years before 2006: years during which Colombia sprayed both regions.

We now investigate the consequences of the discontinuity in enforcement on coca cultivation. Let Y_{it} be the hectares with coca crops in cell i in year t, measured with satellite images at the end of the year. We estimate the following specification:

$$Y_{it} = \gamma_{0t} + \gamma_{1t} 1\{\widehat{D}_i > 0\} + f_t(\widehat{D}_i) + \epsilon_{it}, \tag{3.3}$$

for different years, t. Here, f_t is a polynomial. The coefficient on γ_{1t} measures any discontinuity around the cutoff. As for the previous estimates, we compute standard errors clustering at the grid level and robust against hetereoskedasticity.

The right panel of Table 3.1 (columns 4 to 6) presents estimates of the difference in coca cultivation at the discontinuity, γ_{it}, for several years and pooled years. Each column presents estimates obtained with a different discontinuity sample, indicated in the top row. Consistent with the results on spraying, we find no significant difference in cultivation from 2000 to 2005 in the first row, when there was regular spraying in the exclusion area. In contrast, for the years with lower spraying in the exclusion area (after 2006), we find overall evidence of reductions in cultivation in the sprayed area. The results are not very precise when the sample is divided by years, but they are mostly significant at the 10% confidence level, or near significant. We obtain more precise estimates by pooling the years 2008, 2009, and 2010 when the spraying campaigns were reduced to nearly zero on the exclusion area. In this case, the estimates in column 4 suggest that cultivation was reduced by about 0.33 hectares per square kilometer in cells near the cutoff in the sprayed region relative to the exclusion area. The estimates in columns 5 and 6 confirm our findings, but are less precise, given that they are obtained for more narrow discontinuity samples.

Figure 3.5 and the bottom panels of Figure 3.6 also present these results graphically (the construction of these figures is analogous to that of Figure 3.4). Though it is hard to see the discontinuity in cultivation graphically during the years in which Colombia agreed not to spray the exclusion area, the figures show some decline in cultivation in the sprayed region near the discontinuity (Figure 3.5). In contrast, the bottom panel in Figure 3.6 shows no apparent discontinuity in cultivation during other years in which both areas were sprayed equally. The above results suggest that the enforcement of the 10 km exclusion area created a discontinuity in enforcement around the cutoff after and during 2008, and less so for 2006 and 2007. The discontinuity in spraying caused divergent illegal behavior on both sides of the cutoff.

The fact that we do not find any discontinuity before 2005 is reassuring: It implies that time invariant characteristics do not vary discontinuously around the cutoff, and the dynamics of cultivation on both sides of the 10 km line were balanced before 2005. This suggests that the particular choice of the exclusion area was rather arbitrary and was not done strategically aiming at certain cells with particular cultivation dynamics.

To quantify the exact effect of spraying on illegal coca cultivation, we compute a 2SLS estimate using the discontinuity as the instrument. That is, we estimate equation:

$$Y_{it} = \beta_{0t} + \beta_{1t} S_{it} + f_t(\widehat{D}_i) + \upsilon_{it}, \tag{3.4}$$

for different years separately, instrumenting S_{it} with the dummy $1\{\widehat{D}_i > 0\}$ (so that equation 3.2 corresponds to the first stage).

Table 3.2 presents the fuzzy RD estimates for different discontinuity samples presented in the different columns, and different degrees of the polynomial f_t in different panels. The estimates in the left panel (columns 1 to 3) pool the years 2006 to 2010, while the right panel (columns 4 to 6) pools the years between 2008 and 2010. Our estimates indicate a negative LATE of the likelihood of aerial spraying on total hectares of coca planted per square km. Our estimates suggest that a 10 percentage point increase in the likelihood of aerial spraying reduces cultivation from 0.18 to 0.34 hectares, depending on the different choices of bandwidth or degree of the polynomial used to approximate the local behavior of spraying and cultivation near the cutoff.

To ensure the consistency of our estimates, we require other covariates determining cultivation to vary smoothly around the 10 km cutoff. We have shown this is indeed the case for coca cultivation and spraying before 2006, which is reassuring. Table 3.3 shows that the same holds for manual eradication since 2007 (we have data only on manual eradication beginning this year). These results imply that the decrease of aerial spraying on the exclusion area was not compensated for by an increase in manual eradication, and hence our estimates reflect only the causal effect of the policy change in spraying. Moreover, though not reported to save space, we find no discontinuity in terms of altitude around the cutoff.[5] 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.[6]

The evidence in this section suggests that increases in the likelihood of enforcement do result in less illegal behavior. We take the estimates in this section as evidence that an increase of 12 percentage points in the likelihood of spraying causes a reduction of between 0.18 and 0.34 hectares of coca per square kilometer in a given year. In practice, we believe that part of our estimate captures the possibility that coca farmers reallocate their crops to the exclusion region, which seems reasonable given the proximity between cells. However, this simply implies we are over-stating the real effect of spraying on overall cultivation, and does not rule out our conclusion that farmers respond rationally to the increase in enforcement; it simply suggests another margin of response. However, one additional piece of evidence suggests that reallocation may not be that pervasive. When we estimate the effect of the discontinuity in enforcement on the likelihood of cultivation (the extensive margin, rather than the intensive margin), we find no effects (not reported to save space). This suggests that cultivation in the exclusion region increases within cells, and not because farmers grow coca bushes in new cells. This suggests little reallocation of farmers from the sprayed region to cells in the exclusion area that did not have coca before. In any case, we cannot entirely rule out the extent of the reallocation of coca crops, and our point estimates remain an upper bound on the effects of enforcement.

3.6 Conditional differences in differences estimates

In the previous section we exploited the geographical discontinuity in enforcement around the 10 km cutoff. In this section, we exploit within-cell variation to estimate the causal effect of aerial spraying on coca cultivation using a conditional differences in differences strategy. Figure 3.7 plots the difference in spraying and cultivation between the sprayed and exclusion areas for each year. As can be seen, there is a differential increase in the likelihood of spraying in the sprayed region in 2004 and after 2006 relative to the exclusion area. However, the problem of directly exploiting this variation is that there are unbalanced dynamics in cultivation before 2006 that may confound our estimates, as can be seen in the right panel.

[5] As a last robustness check on our results we estimate a placebo test using a fake cutoff value of the forcing variable of 15 kms around the international border and including all the observations 5 to 25 kms away from the international frontier with Ecuador. Given that this is an arbitrary threshold, and no policy change was being implemented around it, we expect no significant discontinuities. Though not reported to save space, we did not find any discontinuity in spraying or cultivation in this case. These results are available upon request.

[6] In fact, coca is more productive at medium altitudes; see Mejia and Restrepo, 2013b.

Formally, let $T_i = 1\{\widehat{D}_i > 0\}$, and let $\overline{Y_i}$ be the average cultivation in grid i from 2000 to 2005. We are interested in estimating the treatment effect of T_i on $Y_{it} - \overline{Y_i}$, for t >2005 (we will also present the results for t ≥2008 when the spraying campaigns fell to nearly zero in the exclusion area). The traditional differences in differences methodology estimates it by a regression of $Y_{it} - \overline{Y_i}$ on T_i. The problem with the traditional estimate is that, as shown in Figure 3.7, T_i is correlated with $Y_{it'}$ for t' ≤2005. If there are dynamic linkages in cultivation (persistence or mean reversion), this would bias the traditional differences in differences estimate.

Thus, a consistent estimate exploiting within-cell variation and comparing both regions must control for the dynamics of cultivation and spraying before the diplomatic friction (that is, the years 2000 to 2005). Our regression discontinuity setup did not face this issue because there was no discontinuity in cultivation and spraying from 2000 to 2005. The difference only appears when comparing both regions as a whole, and not simply cells around the cutoff.

In this section, instead, we posit the following conditional independence assumption:

$$Y_{it} - \overline{Y_i} \perp T_i | Z_i, \{Y_{it'}, S_{it'}\}_{t' \leq 2005}, \tag{3.5}$$

where Y_{itd} is the potential cultivation in cell i and year t, for a fixed enforcement regime d. Here $d = 1$ means the cell is in a sprayed area, and $d = 0$ means it is not. This assumption states that once we condition on the whole history of cultivation and spraying in a cell ($\{Y_{it'}, S_{it'}\}_{t' \leq 2005}$), and cell covariates Z_i (including a polynomial in altitude and municipality fixed effects), potential cultivation would be equal for cells in the sprayed and exclusion areas (in the absence of a difference in enforcement). We use the change in potential cultivation instead of its level to remove any permanent difference between cells not captured by the conditioning set.[7] We believe this is a plausible assumption, as coca cultivation in two cells with the exact same path of cultivation, spraying, and manual eradication from 2000 to 2005, should follow very similar trajectories in the absence of a difference in enforcement, even if they are on different sides of the 10 km threshold.

We exploit the above CIA in several ways to estimate the effect of being in the sprayed region during years in which Colombia agreed not to spray the exclusion area. First, we start by running the regression:

$$Y_{it} - \overline{Y_i} = \beta_t T_i + \delta_t + \sum_{t' \leq 2006} \Gamma \cdot (Y_{it'}, S_{it'})' + \Theta Z_i' + \varepsilon_{it}, \forall t \geq 2006 \tag{3.6}$$

Here, β_t identifies the effect of being in the sprayed region (relative to the exclusion region) during year t as long as the conditional expectation of the outcome is linear in the covariates. We compute standard errors clustering at the cell level and robust to heteroskedasticity. We also computed standard errors robust against

[7]In theory, we do not even have to remove the average cultivation, as this is already in the conditioning set. In practice, this helps to control for potential sources of misspecification.

some forms of spatial correlation. We do not report them to save space and because they do not change our conclusions about the significance of our estimates (see footnote 4 for details).

Column 1 in the top panel of Table 3.4 presents the regression estimates of being in the sprayed region on cultivation. We present the estimates separately by year, and also pool together the years 2006, 2008, 2009, and 2010, when Colombia agreed not to spray the exclusion area. We refer to these years as post-treatment years. Our estimates show that cultivation was reduced in all years after 2006; the effects are all significant at all traditional levels; and the average effect pooling the post-treatment years together is a reduction of 0.26 hectares per square km (s.e=0.015). The bottom panel presents estimates using the likelihood of spraying as a dependent variable. Our estimates show a large increase in the likelihood of spraying in the sprayed region, especially after 2008, and a small increase in 2006 and 2007, consistent with the fact that the diplomatic friction began in 2006. When pooling the post-treatment years together (2006, 2008, 2009, and 2010), we find that the likelihood of aerial spraying was 9.2 percentage points higher in the sprayed area, which is similar to the estimate obtained using our regression discontinuity design. We do not estimate pre-treatment behavior in these variables to check for equal trends because these are mechanically zero once we control for pre-treatment characteristics. Our estimates reveal a significant decline in cultivation for all years.

Consistency of the previous estimates requires the conditional expectation of cultivation and aerial spraying to be linear in the covariates. To relax this assumption, we follow several strategies in which we control non-parametrically for the propensity score $\lambda_i = P[T_i = 1|Z_i, \{Y_{it'}, S_{it'}\}_{t' \leq 2005}$. We estimate the propensity score, $\widehat{\lambda}_i$, using a probit model not reported to save space.

In column 2 we reweight the regression in equation 3.6 by the propensity score (see Hirano, Imbens, and Ridder, 2003, for more on this approach). In particular, we weight observations in the sprayed area by $p/(1-p)$, where p is the fraction of cells in this area, and observations in the exclusion area by $\widehat{\lambda}_i/(1-\widehat{\lambda}_i)$, with $\widehat{\lambda}_i$ the estimated propensity score of the grid. This method ensures that all covariates are balanced and set to the distribution of the sprayed region. Once reweighted, the regression estimate equals the average treatment effect on the treated (that is, the sprayed area).[8] Besides reweighting by the propensity score, we also control linearly for all covariates in the regression. This is known as a double-robust regression: on the one hand, reweighting the data controls non-parametrically for the influence of covariates; on the other, the covariates in the regression control linearly for any source of misspecification in the propensity score. As can be seen from the results in column 2, the results change little relative to column 1,

[8]We can also estimate the average treatment effect, but this requires weighting by $1/\widehat{\lambda}_i$ the observations in the sprayed area. However, there are values with very low estimated propensity scores that make this exercise imprecise. In any case, our results are similar.

suggesting that the linear controls were already capturing most of the relevant heterogeneity in cultivation and spraying dynamics.[9]

The role of weighting the data by the propensity score can be seen graphically in Figure 3.8. We plot estimates of β_t for $t = 2000, \ldots, 2010$ after reweighting the data using the propensity score as described above. The right panel shows that now, cultivation is balanced between the sprayed and exclusion areas before 2006. By contrast, the raw data on cultivation presented in Figure 3.7 exhibited unbalanced dynamics before 2006 that could confound our estimates. A similar pattern emerges for spraying, although dynamics were already roughly balanced in the raw data. When computing the estimates used in this figure, we do not control linearly for the covariates in the regression presented in equation 3.6. Doing so mechanically sets the estimates of $\beta_t = 0$ for $t \leq 2005$.

In column 3 we follow another strategy and stratify on the propensity score as in Angrist (1998) and Dehejia and Wahba (1999). In particular, we group observations by their propensity score in 20 equal bins covering the $(0, 1)$ interval. The j-th bin contains grids with an estimated propensity score between $(j-1) \times 0.05$ and $j \times 0.05$. For each bin, we estimate equation 3.6 separately, and use weighted averages of all these estimates to obtain an estimate for β_t. We obtained the variance of β_t as a weighted sum of the variances for each bin as well.[10] We weight each estimate by the number of observations in the bin from the sprayed region. This guarantees that we estimate the average treatment effect on the treated (results for the ATE were similar). This approach has the advantage of not imposing any functional form on the conditional expectation as a function of the propensity score, but of course is limited by the size of our bins. Again, we control locally for all covariates when estimating equation 3.6 for each bin. This partly controls for differences in the propensity score within bins and misspecification of the propensity score. Our results are similar to the basic regression estimates in column 1, though we find a smaller reduction in cultivation.

Finally, in column 4 we do Kernel matching on the propensity score. This works by finding, for each grid in the sprayed region, others in the exclusion area within a band around its estimated propensity score, and weighting them by a Kernel that assigns less weight to distant grids. Reweighting the regression using these weights produces an estimate of the average treatment effect on the treated. The reweighting guarantees that every grid in the sprayed region is compared to an average of grids with similar propensity score in the exclusion region, and thus

[9] We report the usual regression standard errors clustering at the grid level. These errors ignore the fact that the propensity score is estimated in a previous stage. However, as suggested by Hirano, Imbens, and Ridder (2003), these standard errors are actually conservative, relative to adjusted ones. We computed an alternative set of bootstrapped standard errors taking into account the estimation of the propensity score and obtained slightly smaller standard errors not reported.

[10] Again, bootstrapping the whole procedure resulted in similar standard errors. Thus, we present standard errors that assume the propensity score is known.

controls non-parametrically for the propensity score. We present estimates of the standard errors assuming that the propensity score is known.[11] Again, we also include the covariates in the regression linearly, which control partly for differences in the propensity score within the kernel of an observation. Our results vary little with respect to the traditional regression estimates in column 1.

In columns 5 to 8 of Table 3.4 we conduct another exercise. Instead of focusing on the period since 2006, when Colombia first agreed not to spray the exclusion area, we focus on the period since 2008, when it was forced not to do so by international pressure. In this case, we condition on all covariates until 2007, and replicate the estimates from columns 1 to 4. We do not report the estimates for 2006 and 2007 because these are mechanically zero, showing that this methodology weights the data in such a way that, by construction, the sprayed and exclusion areas have equal cultivation and spraying trends until 2007. These estimates also reveal a negative effect on cultivation of being in the sprayed area, and a similar positive effect on the likelihood of spraying of around 9 percentage points. The fact that the estimate on cultivation is less negative could reflect the possibility of weaker responses to enforcement during these years. However, we prefer our first set of estimates in columns 1 to 4, since arguably including the endogenous response of cultivation in 2006 as a control is not entirely satisfactory.

All the same, the estimates in this section suggest that grids in the sprayed region were approximately 10 percentage point more likely to be sprayed during years in which Colombia committed not to spray the exclusion area. As a consequence, farmers in the region reduced cultivation between by approximately 0.25 hectares per square kilometer. The fact that this is smaller than our regression discontinuity estimates could be due to two things: first, because they are, in theory, different objects. In columns 2 to 4 of Table 3.4 we estimate an average treatment effect on the sprayed grids (and the regression in column 1 produces a mix between the ATT and ATE), while regression discontinuity estimates a local effect. Second, there may be more reallocation of crops to the exclusion region near the 10 km cutoff. This implies that this indirect margin is more relevant for the regression discontinuity estimates, making them overstate the deterrent effect. In any case, both methodologies reveal significant responses by households to a differential likelihood of spraying, consistent with the view that enforcement reduces illegal behavior.

3.7 Cost benefit analysis of aerial spraying

As discussed in the introduction, the aerial spraying program is the largest component among the supply reduction efforts implemented under *Plan Colombia*. Between 2000 and 2008, \$585 millions were allocated to the eradication program,

[11] In general, it is not known whether bootstrapping them produces consistent estimates, so we keep the naive ones. In any case, bootstrapped standard errors were very similar.

whereas $62.5 were allocated to air interdiction and $89.3 to coastal and river interdiction by the military forces, and $152.7 to interdiction activities carried out by the Colombian Police (see the U.S. Government Accountability Office - GAO, 2008).

Our regression discontinuity estimates suggest that for a 10 percentage increase in the likelihood of aerial spraying, coca cultivation is reduced approximately by between 0.18 and 0.34 hectares per square kilometer. Our conditional differences in differences points to a reduction of approximately 0.25 hectares. Since 1 square kilometer contains 100 hectares, these estimates imply that to reduce cultivation by 1 hectare during a given year, the Colombian government has to spray 30-55.5 additional hectares.

It is estimated that the average direct cost to the U.S. per hectare sprayed is about $750 (see Walsh et al., 2008). Thus, reducing cultivation by 1 hectare through financing spraying campaigns costs the U.S. $22,500-$41,625 dollars. Additionally, for every dollar spent by the U.S., Colombia spends about 2.2 dollars (aerial spraying campaigns are jointly financed by the countries), making the overall cost $72,000-$133,200. To put these numbers in perspective, that hectare is able to produce enough coca bushes to synthesize 6 kilograms of cocaine per year, which would cost $12,000 dollars to buy from Colombian farms.

From a drug policy perspective, it is more informative to calculate the benefits in terms of the reduction of kilograms of cocaine in consumer markets. We do not have estimates of the social benefits of such reductions, but at least we can compare the cost to that of other policies achieving a similar objective. To do so, we use the estimates in Mejia and Restrepo (2013) obtained by calibrating a model of downstream cocaine markets. The authors find that a 1% reduction in coca cultivation reduces cocaine in consumer markets by 0.004%.[12] This elasticity is small for several reasons. First, cultivation represents only a small fraction of the total market value of cocaine in consumer markets. Thus, an increase in the price of coca bushes caused by spraying translates into a small increase in consumer prices. Second, demand is inelastic, so the small increase in prices barely affects consumption. Finally, downstream markets adjust to the shock by substituting towards cheaper inputs of production, such as more chemical precursors and technology to produce more cocaine per hectare, by demanding more cocaine from other source countries, or by switching to better transportation techniques, partially offsetting the effect of the shock on the supply of cocaine.

Total coca cultivation in Colombia was about 80,000 hectares during our period of analysis. Reducing this by 1% (800 hectares), would cost the U.S. $18-$33.3 million dollars per year. However, this investment would reduce the supply of cocaine in its territory only by 0.004%, which equals 20 kg. This implies that

[12] We use the estimate they report of $\Lambda_q = 0.004$, which corresponds to the elasticity of final supply with respect to reductions in cultivated area in Colombia. We take the estimate for an elasticity of demand of 0.75 in Table 5.

the marginal cost to the U.S. of reducing retail quantities of cocaine by 1 kg by subsidizing aerial spraying in Colombia is $0.9- $1.66 million dollars. These are large magnitudes, but are similar to the estimates reported by Mejia and Restrepo (2013) using an entirely different methodology. To put them in perspective, the price of 1 kg of cocaine in retail markets is about $150,000 per kilogram.

The conclusion from this exercise is that aerial spraying is a very costly policy from a supply-reduction perspective. In particular, the policy is significantly more costly than other alternatives achieving the same objective. The estimated marginal cost to the U.S. of reducing retail quantities of cocaine by 1 kg is estimated at $181,000 dollars by subsidizing interdiction policies in Colombia (Mejia and Restrepo, 2013), or $8,250 and $68,750 dollars by funding treatment and prevention efforts, respectively, in the U.S. (MacCoun and Reuter, 2001). Thus, despite being able to reduce coca cultivation by affecting farmers incentives, aerial spraying has only small effects on cultivation. These effects translate to even smaller effects on downstream markets for the reasons emphasized above, making it a costly supply-reduction policy. If on top of that we add the share of the costs paid by Colombia and the alleged negative effects on health (Camacho and Mejia, 2014), other legal crops, the environment (see Relyea, 2005 and Dvalos et al., 2011), and the socio-economic conditions of coca-producing areas (see Rozo (2014)), the policy looks even less favorable.

3.8 Conclusion

In this paper we explored the deterrent effects of enforcement on illegal behavior. We did so in the context of illegal coca cultivation in Colombia. We find that aerial spraying of coca crops – a particular type of enforcement aimed at partially destroying the illicit goods – induces farmers to reduce coca cultivation. Our findings are aligned with the key insight from the economic analysis of crime, suggesting that the decision to engage in illegal activities is rational and, as such, responds to the likelihood of enforcement.

Our main contribution is to present a clean and credible source of identification for the effects of enforcement on illegal markets. In particular, we exploit a diplomatic friction between the governments of Colombia and Ecuador over the possible negative effects of spraying campaigns in the Colombian territory bordering Ecuador. This diplomatic friction ended in a compromise by the Colombian government to stop spraying campaigns with glyphosate within a 10 km band along the border with Ecuador in 2006.

We use a regression discontinuity design, exploiting the arbitrary 10 km line and a conditional differences in differences estimator comparing similar cells with different treatment probabilities to uncover the causal effects of spraying on coca cultivation. Both methodologies point to a negative and significant effect of the program on coca production. In particular, both methodologies show that cells in the region that continued to be sprayed were approximately 10 percentage points more likely to be sprayed than cells in the exclusion area. In consequence, coca cultivation decreased in this region by about 0.18 to 0.34 hectares (regression discontinuity estimates) or 0.25 hectares (conditional differences in differences estimate) per squared kilometer.

Despite reducing coca cultivation, aerial spraying in Colombia has only small effects in downstream markets. We estimate that reducing the Colombian coca cultivation by 1% (about 800 hectares) would cost the U.S. between \$18 and \$33.3 million dollars per year. However, this investment would reduce the supply of cocaine in its territory by only 0.004%, which equals 20 kg of cocaine less per year. Hence, the cost of reducing cocaine retail supply by 1 kg is at least \$0.9 million dollars per year if resources are used to subsidize spraying campaigns in Colombia. Other policies, such as treatment and prevention, or even subsidizing interdiction efforts in Colombia, would be significantly more cost effective in curbing drug supply.

Table 3.1: Estimates of the local difference in spraying and coca cultivation around the 10km cutoff (sprayed minus exclusion area).

	Difference in spraying			*Difference in cultivation*		
	(1)	(2)	(3)	(4)	(5)	(6)
Discontinuity sample:	$\pm 3km$	$\pm 2.75km$	$\pm 2.5km$	$\pm 3km$	$\pm 2.75km$	$\pm 2.5km$
Difference before 2006:	-0.013	-0.010	-0.025	0.405	0.515	0.308
	(0.028)	(0.031)	(0.035)	(0.458)	(0.537)	(0.592)
Observations	15918	14262	12672	15918	14262	12672
Difference in 2006:	0.090*	0.090	0.088	-0.732	-0.847	-1.275**
	(0.052)	(0.058)	(0.067)	(0.512)	(0.575)	(0.646)
Observations	2653	2377	2112	2653	2377	2112
Difference in 2007:	0.140**	0.114*	0.072	-0.422	-0.317	-0.419
	(0.062)	(0.069)	(0.078)	(0.318)	(0.388)	(0.387)
Observations	2653	2377	2112	2653	2377	2112
Difference in 2008:	0.126***	0.088*	0.091	-0.405*	-0.613**	-0.598**
	(0.047)	(0.052)	(0.059)	(0.218)	(0.240)	(0.254)
Observations	2653	2377	2112	2653	2377	2112
Difference in 2009:	0.099***	0.078**	0.079*	-0.172	-0.250	-0.229
	(0.036)	(0.040)	(0.044)	(0.167)	(0.184)	(0.200)
Observations	2653	2377	2112	2653	2377	2112
Difference in 2010:	0.133***	0.114**	0.136**	-0.419*	-0.409	-0.410
	(0.046)	(0.052)	(0.059)	(0.234)	(0.266)	(0.290)
Observations	2653	2377	2112	2653	2377	2112
Difference after 2006 (inclusive):	0.118***	0.097***	0.093**	-0.430**	-0.487**	-0.586**
	(0.030)	(0.034)	(0.038)	(0.212)	(0.241)	(0.259)
Observations	13265	11885	10560	13265	11885	10560
Difference after 2008 (inclusive):	0.120***	0.093***	0.102***	-0.332*	-0.424**	-0.412*
	(0.030)	(0.034)	(0.038)	(0.177)	(0.197)	(0.211)
Observations	7959	7131	6336	7959	7131	6336

Notes: The table presents regression discontinuity estimates of the difference in spraying (left panel) and cultivation (right panel) around the 10 km cutoff. Each row has a different model estimated for different years or pooled years. The discontinuity sample varies from $\pm 3km$ (columns 1 and 4), $\pm 2.75km$ (columns 2 and 5) and $\pm 2.5km$ (columns 3 and 6). In all models, we control for a cubic polynomial in the forcing variable and include municipality and year specific intercepts. Standard errors robust against heteroskedasticity and serial correlation within cells are reported in parenthesis. Estimates with *** are significant at the 1%, those with ** are significant at the 5%, and those with * are significant at the 10%.

Table 3.2: fuzzy RD estimates of the local average treatment effect of spraying on cultivation around the 10 km cutoff.

	Years 2006 to 2010			Years 2008 to 2010		
	(1)	(2)	(3)	(4)	(5)	(6)
Discontinuity sample:	$\pm 3km$	$\pm 2.75km$	$\pm 2.5km$	$\pm 3km$	$\pm 2.75km$	$\pm 2.5km$
			Using linear function			
Effect of spraying:	-3.472**	-3.028**	-2.815*	-2.255**	-1.861*	-2.259*
	(1.405)	(1.366)	(1.450)	(1.048)	(1.011)	(1.199)
Observations	13265	11885	10560	7959	7131	6336
Instrument F-stat.	70.4	71.6	57.7	63.6	65.7	46.3
			Using quadratic polynomial			
Effect of spraying:	-3.473**	-3.031**	-2.820*	-2.258**	-1.881*	-2.301*
	(1.406)	(1.374)	(1.465)	(1.048)	(1.019)	(1.211)
Observations	13265	11885	10560	7959	7131	6336
Instrument F-stat.	70.5	71.8	57.8	63.8	66.7	47.2
			Using cubic polynomial			
Effect of spraying:	-3.652	-5.024	-6.282	-2.780	-4.541	-4.035
	(2.241)	(3.430)	(4.270)	(1.729)	(2.884)	(2.755)
Observations	13265	11885	10560	7959	7131	6336
Instrument F-stat.	25.8	14.0	10.2	22.0	10.8	10.2
			Using quartic polynomial			
Effect of spraying:	-3.660	-5.180	-6.464	-2.794	-4.679	-4.091
	(2.247)	(3.501)	(4.366)	(1.736)	(2.919)	(2.779)
Observations	13265	11885	10560	7959	7131	6336
Instrument F-stat.	25.8	14.0	10.2	22.0	11.2	10.5

Notes: The table presents fuzzy regression discontinuity estimates of the effect of differential aerial spraying on cultivation around the 10 km cutoff. Columns 1 to 3 pool the years 2006, 2008, and 2010; while columns 4 to 6 add 2009. Each panel presents estimates controlling for a different polynomial in the forcing variable. In all models we include municipality and year specific intercepts. The discontinuity sample varies from $\pm 3km$ (columns 1 and 4), $\pm 2.75km$ (columns 2 and 5) and $\pm 2.5km$ (columns 3 and 6). Standard errors robust against heteroskedasticity and serial correlation within cells are reported in parenthesis. Estimates with *** are significant at the 1%, those with ** are significant at the 5%, and those with * are significant at the 10%.

Table 3.3: Estimates of the local difference in manual eradication around the 10 km cutoff (sprayed minus exclusion area).

	(1)	(2)	(3)
Discontinuity sample:	3km	2.75km	2.5km
Difference in 2007:	0.018	0.004	-0.002
	(0.036)	(0.040)	(0.044)
Observations	2653	2377	2112
Difference in 2008:	0.065	0.046	0.019
	(0.058)	(0.064)	(0.072)
Observations	2653	2377	2112
Difference in 2009:	-0.044	-0.063	-0.061
	(0.048)	(0.053)	(0.060)
Observations	2653	2377	2112
Difference in 2010:	0.015	0.013	0.010
	(0.017)	(0.018)	(0.020)
Observations	2653	2377	2112
Difference after 2006 (inclusive):	0.013	0.000	-0.009
	(0.024)	(0.027)	(0.029)
Observations	10612	9508	8448
Difference after 2008 (inclusive):	0.012	-0.001	-0.011
	(0.028)	(0.031)	(0.034)
Observations	7959	7131	6336

Notes: The table presents regression discontinuity estimates of the difference in manual eradication around the 10 km cutoff. Each row has a different model estimated for different years or pooled years. The discontinuity sample varies from $\pm 3km$ (columns 1 and 4), $\pm 2.75km$ (columns 2 and 5) and $\pm 2.5km$ (columns 3 and 6). In all models, we control for a cubic polynomial in the forcing variable and include municipality and year specific intercepts. Standard errors robust against heteroskedasticity and serial correlation within cells are reported in parenthesis. Estimates with *** are significant at the 1%, those with ** are significant at the 5%, and those with * are significant at the 10%.

Table 3.4: Conditional differences in differences estimate of being in sprayed region

Covariates:	Before 2005				Before 2007			
		Propensity score methods				Propensity score methods		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent variable: coca cultivation.							
Estimate for 2006:	-0.581***	-0.571***	-0.517***	-0.579***	.	.	.	.
	(0.045)	(0.051)	(0.048)	(0.050)	.	.	.	.
Estimate for 2007:	-0.154***	-0.146***	-0.032	-0.137***	.	.	.	.
	(0.042)	(0.043)	(0.039)	(0.040)	.	.	.	.
Estimate for 2008:	-0.091***	-0.059**	0.022	-0.049*	-0.097***	-0.039	0.034	-0.045
	(0.023)	(0.029)	(0.033)	(0.028)	(0.023)	(0.030)	(0.036)	(0.031)
Estimate for 2009:	-0.204***	-0.218***	-0.156***	-0.206***	-0.155***	-0.139***	-0.111***	-0.138***
	(0.017)	(0.022)	(0.022)	(0.020)	(0.016)	(0.021)	(0.023)	(0.021)
Estimate for 2010:	-0.163***	-0.143***	-0.066**	-0.141***	-0.094***	-0.028	0.011	-0.034
	(0.022)	(0.026)	(0.027)	(0.025)	(0.021)	(0.023)	(0.028)	(0.023)
Pooling post-treatment years:	-0.260***	-0.248***	-0.179***	-0.244***	-0.115***	-0.069***	-0.022	-0.072***
	(0.015)	(0.022)	(0.021)	(0.021)	(0.012)	(0.019)	(0.021)	(0.018)
	Dependent variable: aerial spraying.							
Estimate for 2006:	0.057***	0.072***	0.082***	0.072***	0.000	0.000	0.000	0.000
	(0.005)	(0.006)	(0.007)	(0.006)	(0.000)	(0.000)	(0.000)	(0.000)
Estimate for 2007:	0.019***	0.054***	0.073***	0.047***	0.000*	0.000*	0.000	0.000**
	(0.006)	(0.009)	(0.009)	(0.008)	(0.000)	(0.000)	(0.000)	(0.000)
Estimate for 2008:	0.129***	0.159***	0.155***	0.150***	0.109***	0.128***	0.111***	0.120***
	(0.005)	(0.007)	(0.008)	(0.006)	(0.005)	(0.008)	(0.011)	(0.007)
Estimate for 2009:	0.079***	0.105***	0.101***	0.100***	0.067***	0.093***	0.088***	0.088***
	(0.004)	(0.005)	(0.006)	(0.005)	(0.004)	(0.005)	(0.008)	(0.005)
Estimate for 2010:	0.102***	0.110***	0.111***	0.108***	0.091***	0.099***	0.100***	0.097***
	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.008)	(0.005)
Pooling post-treatment years:	0.092***	0.111***	0.112***	0.107***	0.089***	0.107***	0.100***	0.102***
	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.006)	(0.004)

Notes: The table presents conditional differences in differences estimates of the effect of being in the sprayed region (relative to the exclusion areas) on cultivation and spraying. Each row has a different model estimated for different years or pooled years. Columns 1 and 5 are usual linear regressions. In columns 2 and 6 we estimate the ATT by reweighting the regression using the estimated propensity score. In columns 3 and 7 we estimate the ATT by stratifying on the estimated propensity score. Finally, in columns 4 and 8 we match observations on the propensity score. Columns 1 to 4 condition on cultivation and spraying from 2000 to 2005. Columns 5 to 7 condition on cultivation and spraying from 2000 to 2007, and hence no estimate is reported for 2006 and 2007. Each year has 10,880 observations. Standard errors robust against heteroskedasticity and serial correlation within cells are reported in parenthesis (however, the errors ignore the estimation of the propensity score in a previous stage). Estimates with *** are significant at the 1%, those with ** are significant at the 5%, and those with * are significant at the 10%.

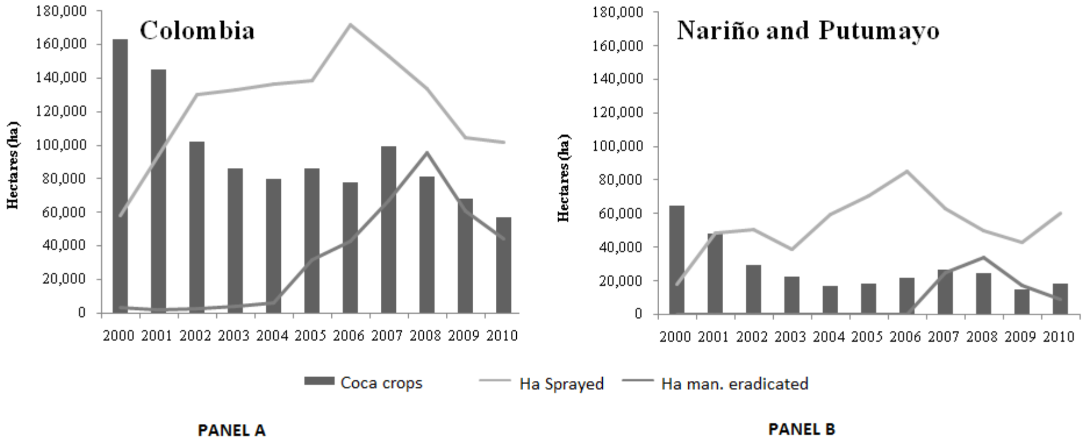

Figure 3.1: Coca cultivation and aerial spraying in Colombia (left panel) and Narino and Putumayo (right panel). These are the limiting states with Ecuador. Data from the United Nations Office of Drugs and Crime, UNODC.

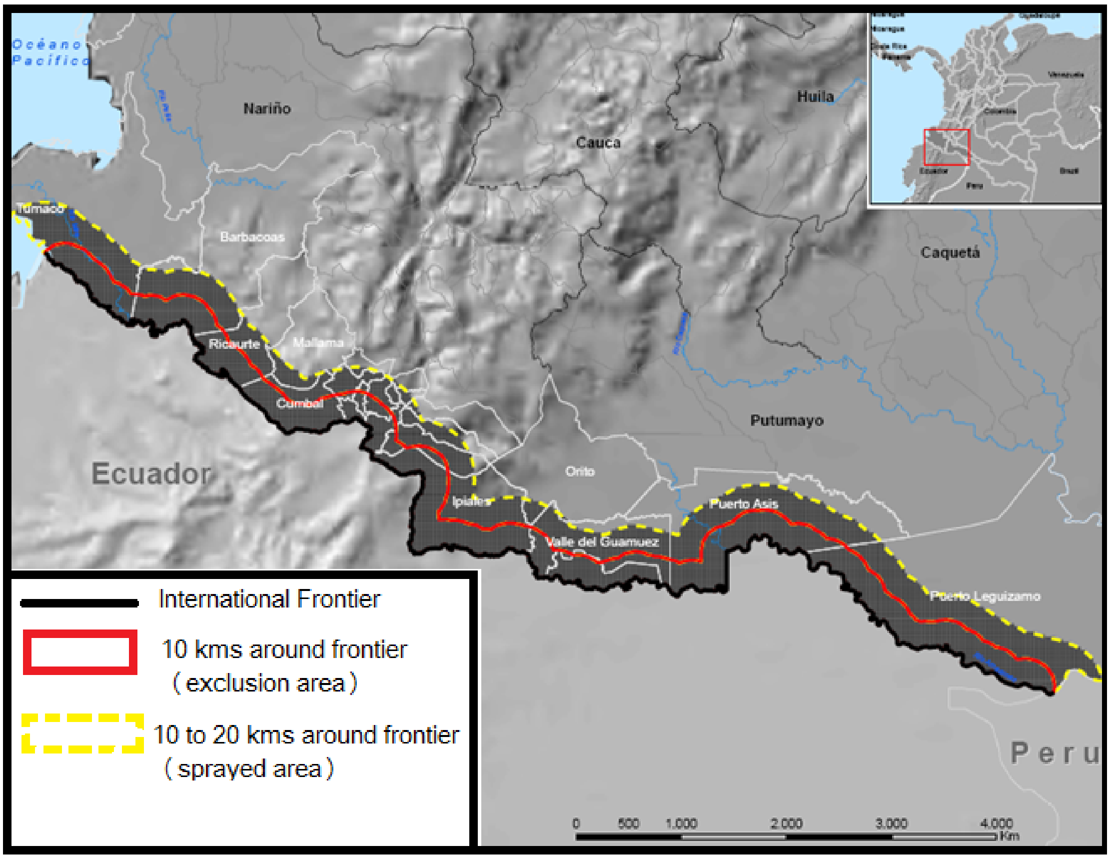

Figure 3.2: Map of the frontier between Colombia and Ecuador illustrating the sprayed and exclusion areas.

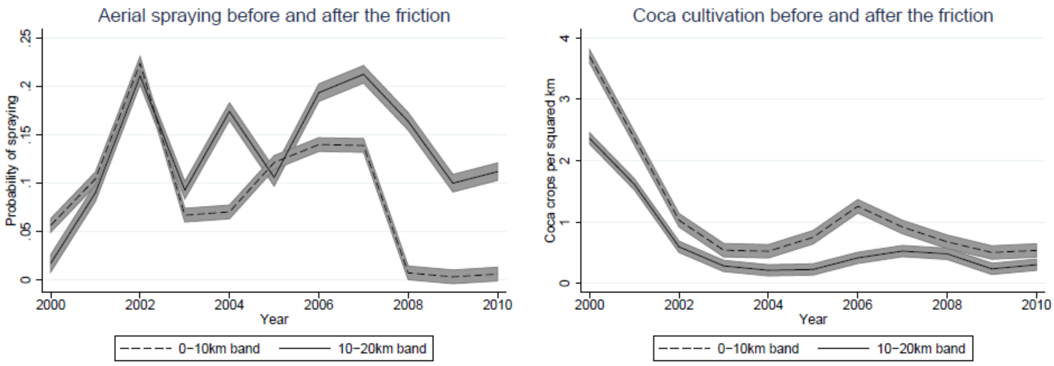

Figure 3.3: Coca cultivation and likelihood of aerial spraying in the exclusion (dotted line) and sprayed areas (solid line) from 2000 to 2010. 95% confidence intervals for the averages in each group are presented as the gray area for each year.

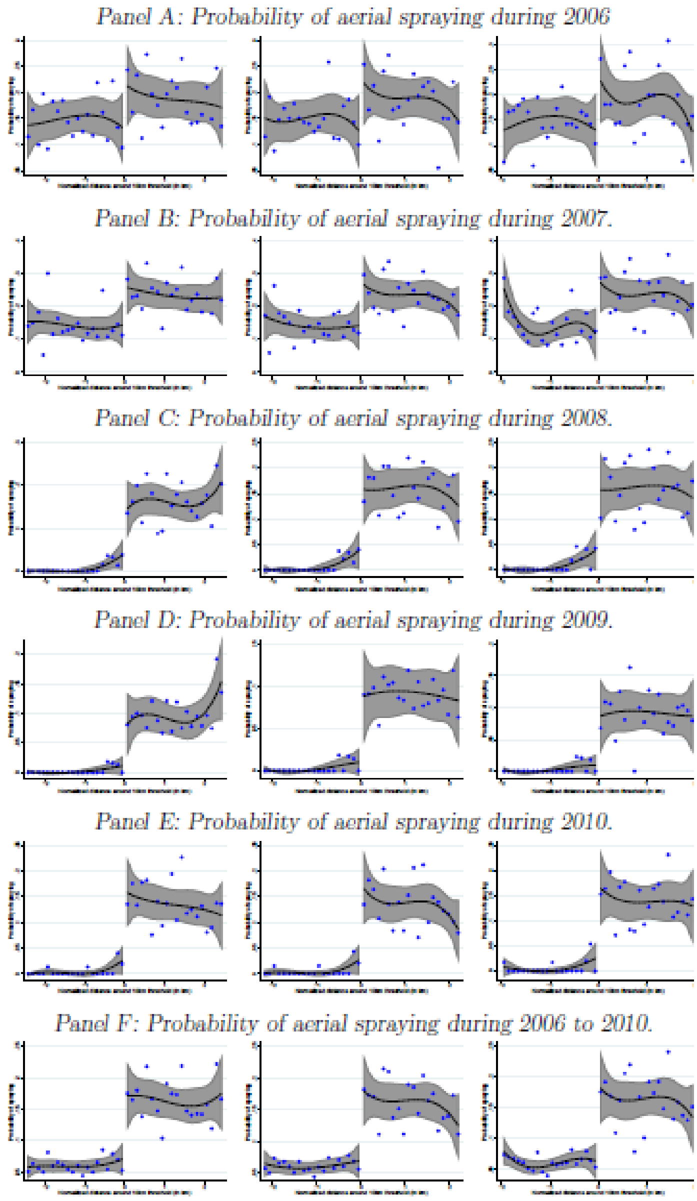

Figure 3.4: Probability of Aerial Spraying.

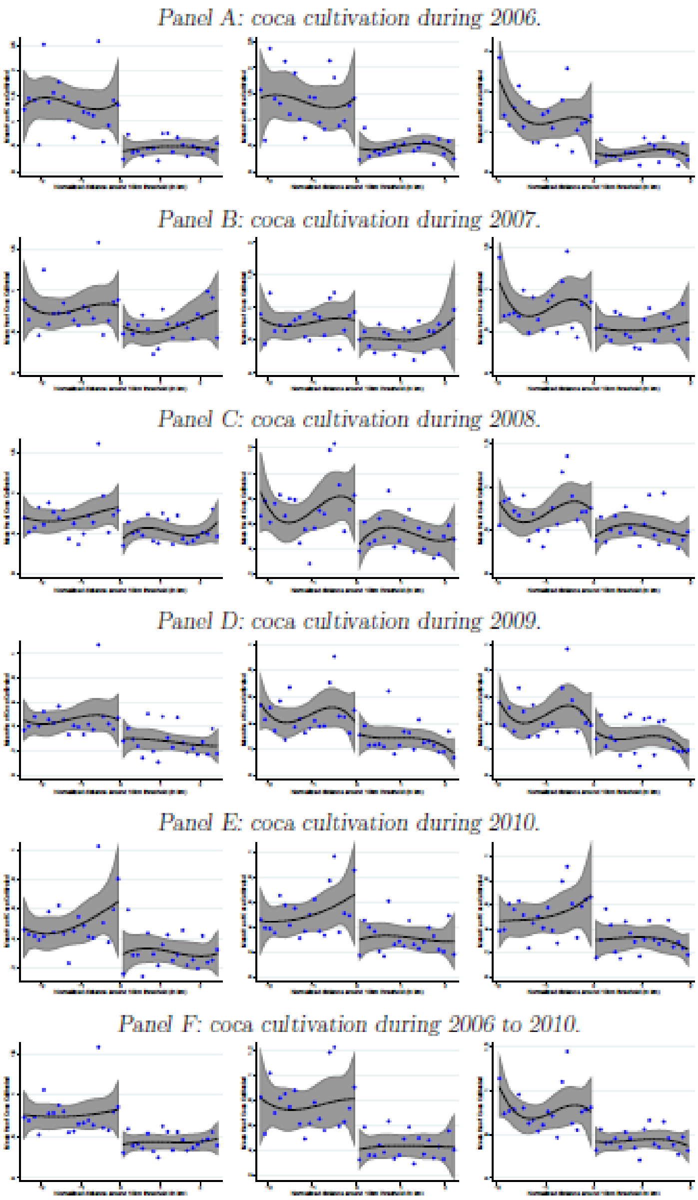

Figure 3.5: Coca cultivation.

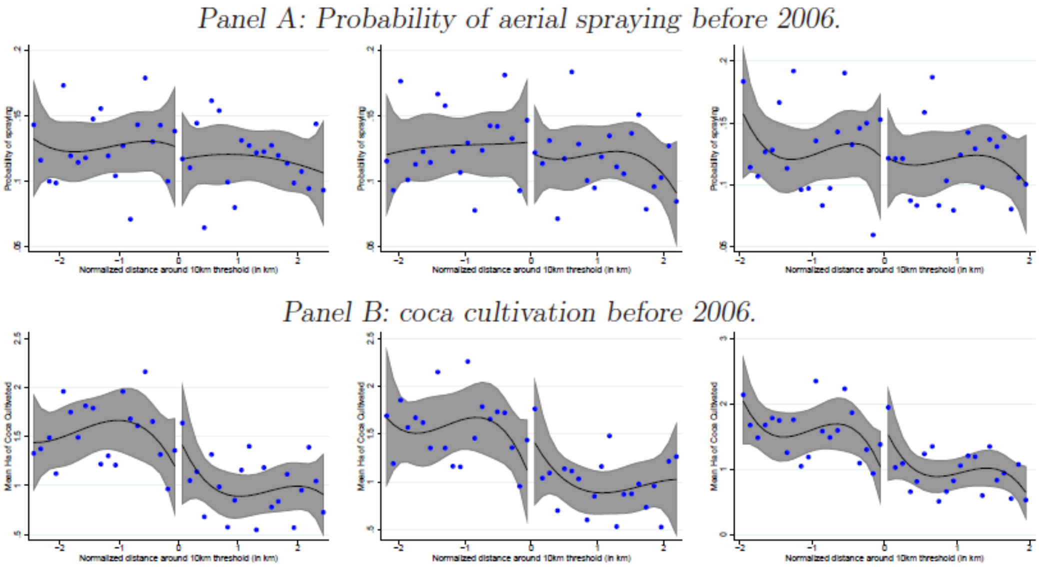

Figure 3.6: Probability of aerial spraying and coca cultivation around the 10 km cutoff during years in which Colombia formally sprayed the exclusion area.

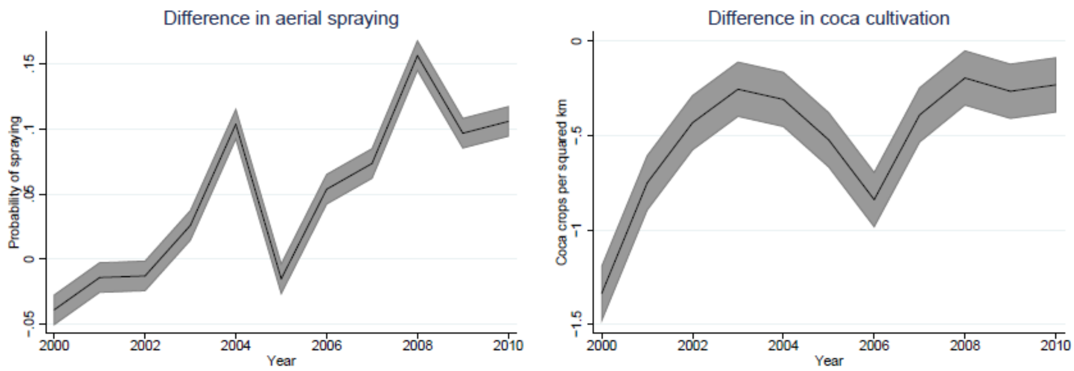

Figure 3.7: Difference in coca cultivation and spraying between the sprayed and exclusion areas from 2000 to 2010.

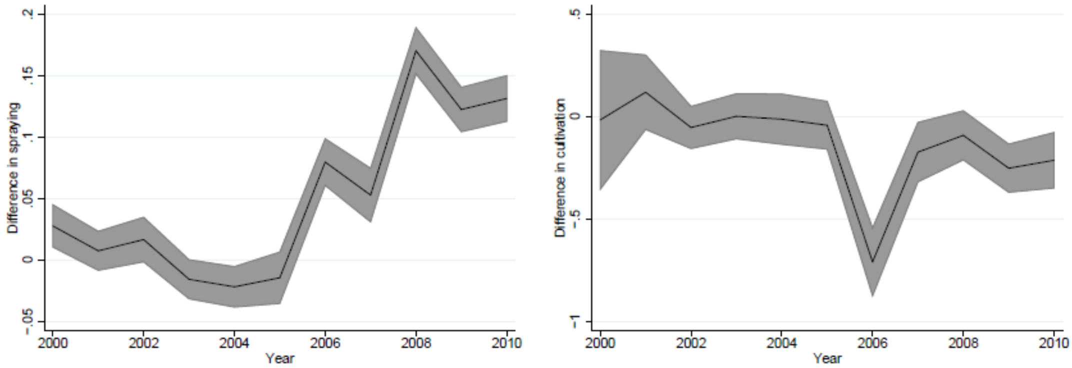

Figure 3.8: Re-weighted difference in coca cultivation and spraying between the sprayed and exclusion areas from 2000 to 2010 relative to the average before 2006. We weight observations in the exclusion area by the estimated odds ratio based on cultivation and spraying from 2000 to 2005, so that the distribution of these covariates in the exclusion area matches that of the sprayed area.

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