The effect of a supply shock in the production of cocaine on violence: Evidence from Colombia and Venezuela

by

Monica Beeder

Master's thesis

Master's thesis handed in the 15th of June 2020

Master's Degree in Economics

The University of Bergen, Department of Economics [June 2020]

UNIVERSITETET I BERGEN

Acknowledgments

First, I want to thank my supervisor, Rune Jansen Hagen, for valuable guidance in writing this thesis. I thank my second supervisor, Eirik Andre Strømland, for helping me with the econometric methodology and analysis, for useful comments and advice, and for always being available to answer my endless series of questions. Without the two of you, I could never have finished this thesis.

Furthermore, I want to thank Chr. Michelsen Institute (CMI) for accepting me as an affiliated master student and allowing me to be a part of an inspiring research environment. In particular, I would like to thank Magnus Hatlebakk for useful comments and encouragement.

I like to thank Catalina Franco Buitrago at the Norwegian School of Economics (NHH) for helping me with data sources in Colombia.

I would also like to thank Amanda Kvarven, Nina Serdarevic and, Ragnar Alne at the University of Bergen (UiB) for providing useful STATA tips and for taking the time to answer my many questions.

In addition, I would like to thank my fellow students at the University of Bergen and at CMI for providing good company, exciting discussions, and entertaining coffee breaks.

I would also like to thank Sigri Wind for proofreading and for giving valuable comments on my drafts.

Finally, I would like to thank Siri Reinzt, Charlotte Ringdal, Anna Oleynik for useful comments on my drafts, and moral support.

Abstract

Using data on coca cultivation and homicides, this paper analyzes an otherwise little researched topic, linking cocaine production and violence in Colombia. I use an exogenous supply shock in gasoline, an input factor needed to produce cocaine, and analyze the effect on violence in coca-producing areas compared to non-producing areas using a differences-in-differences strategy.

The price of gasoline decreases in 2016, because of an exchange rate shock between Colombia and Venezuela. The results indicate that the positive supply shock leads to more violence in coca producing areas. The main results are robust to various tests, such as controlling for immigration, excluding big cities and distance from the border.

This paper contributes to the literature by showing that when it becomes cheaper to produce cocaine, there is more violence in production areas. By looking at a purely economic effect on the drug market, instead of a drug enforcement effect, the paper also show that there is an effect of price changes on the cocaine market that goes beyond the drug enforcement. The paper also contributes to the literature by studying the interaction between two illegal markets: the smuggling of gasoline and cocaine production.

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1. Introduction

Latin America is the world′s most violent region not at war, with 45 of the 50 most murderous cities in the world, and eight of the top 10 most murderous countries (Igarapé Institute 2017). In Colombia, interpersonal violence causes more premature deaths than heart disease and traffic accidents (Global Burden of Disease 2017). One major mechanism thought to be behind the extensive violence is the prevalence of cocaine production throughout Colombia.

Colombia is currently the most important cocaine producer (coca bush cultivation) in the world (United Nations Office of Drugs and Crime (UNODC) 2019). The most recent estimates show that the global production of cocaine reached an all-time high of 1,976 tons in 2017, which was more than double the level recorded in 2013. Coca cultivation in Colombia is the main driver of this increase.

Despite the strong correlation, there is little research on the causal relationship between the cultivation, production, and trafficking of drugs in Latin America and the violence. Evidence from Afghanistan suggests that violence can lead to more drug production; hence, the direction of causality is unclear (Lind, Moene & Willumsen 2014). 95 percent of all scientific knowledge on effective violence prevention relates exclusively to the United States and wealthy European countries, where homicide rates are low (Eisner & Nivette 2012). Thus, more research is needed in low- and middle-income countries to advance local knowledge on the causes of violence (Eisner 2015).

In this thesis, I study the relationship between violence and cocaine production in Colombia. I use an exogenous price shock in the cocaine market to study the effect on violence in cocaine-producing areas. The price shock originates from a shock to the exchange rate between the currencies of Colombia and Venezuela in 2016. In turn, this shock stems from hyperinflation in Venezuela caused by the decrease in international oil prices and poor monetary policy. The shock affects the price of an input into the cocaine production, trafficked gasoline. This shock allows a quasi-experimental research design to study the impact of the supply shock on violence. I perform a Differences-in-differences (DiD) analysis between areas with high-intensity and low-intensity coca cultivation, assuming (and testing for) similar trends before and after the economic shock. I survey the existing literature about the cocaine production chain to assure that cocaine production and the input of gasoline, the variable of interest, are located in the areas of cultivation. I use data on coca cultivation and homicides, two reliable data sources in a field of research with many unknowns, and a general lack of information. The results indicate that the positive supply shock leads to more violence in coca producing areas than in non-producing areas. Lastly, the paper discusses alternative mechanisms and find that the results are robust to various tests, as controlling for immigration, excluding big cities and distance from the border.

This paper contributes to the literature by looking at the effects of a pure economic shock and by studying a supply shock instead of a demand shock. Most of the previous literature studies economic shocks that stem from law enforcement campaigns against drugs, and is studying change in demand. Both Angrist and Kugler (2008) and Mejia and Restrepo (2013) have studied demand shocks in the Colombian coca production as a consequence of drug enforcement campaigns. Abadie et al. (2014) have looked at the impact of drug eradication programs in Colombia. Castillo, Mejia, and Restrepo (2020) have studied the effects of a negative supply shock from drug enforcement in Colombia and the impact of violence along Mexican trafficking routes. Dell (2015) has examined areas in Mexico with vigorous drug enforcement. Drug enforcement is violent, and therefore it is challenging to distinguish the effect on violence from law enforcement campaigns from "pure" changes in demand. By looking at a pure economic shock on the drug market, instead of a drug enforcement intervention, one is more likely to establish a causal relationship where price changes affect the cocaine market, which in turn affects the level of violence. Another contribution to the literature is to study a supply shock and show that when it becomes cheaper to produce cocaine, there is more violence in production areas. This knowledge is valuable for policies. Finally**,** the paper also contribute to the literature by studying the interaction between two illegal markets: the smuggling of gasoline and cocaine production.

This rest of the thesis proceeds as follows. First, I give background information on cocaine production in Colombia, violence in Colombia, and the exchange rate shock and import of gasoline from Venezuela. Then, I look at related research and discuss the potential mechanism linking a price shock to cocaine production and violence. I argue that purely positive economic shocks to drug production will lead to more violence, even though no preexistence literature have studied it. Then, I describe the data before presenting my main analysis. This is followed by various robustness tests. Finally, I conclude.

2. Background

2.1 Cocaine production in Colombia

Cocaine is a natural product extracted from the leaves of *Erythroxylum coca* and *Erythroxylum novogranatese,* better known as coca leaves (European Monitoring Centre for Drugs and Drug Addiction (EMCDDA) and Europol 2020). Coca leaves are almost exclusively cultivated in Colombia, Peru, and Bolivia. The extraction of cocaine alkaloids from the coca leaves also almost exclusively takes place in these three countries, and the majority of the global production of cocaine hydrochloride takes place in the same countries. Colombia is the major producer of the three countries, both in terms of coca leaves and cocaine production (UNODC 2019).

To produce cocaine, the coca leaves go through various chemical processes. First, the coca leaves are cultivated and harvested. It is important to note that the leaf is marketed in a fresh state and is a perishable good, as the leaf tends to rot about two days after harvest (UNODC & Government of Colombia 2017). Then in the extraction process, the leaves are crushed with sulfuric, acid, calcium carbonate, and gasoline (EMCDDA and Europol 2020). The leaves are soaked in barrels of gasoline and then drained, which creates the coca (base) paste. Coca (base) paste has about one-hundredth of the volume of coca leaves, and the transition from leaf to paste is where most of the weight reduction in cocaine production occurs (Angrist and Kugler 2008). Later, in the purification stage, potassium permanganate is added to the paste, and the resulting mixture is filtered, creating the cocaine base (EMCDDA and Europol 2020). Then, in the crystallization stage, Ammonium hydroxide, acetone, and hydrochloric acid are added to the cocaine base to create cocaine hydrochloride. Lastly, the cocaine hydrochloride is divided into user dosage and mixed (cut) with other ingredients. This last step is typically done in consumer countries.

The first two stages, the cultivation and extracting, where the coca base paste is created, usually are taking place at the local farmer level (Mejia & Rico 2010). Approximately 2/3 of the peasant coca growers do not directly sell the coca leaf but transform it through a relatively simple and artisanal process into coca paste, and then sell it as an input to large-scale cocaine producers (Mejia & Rico 2010). This thesis will focus on the second step, the extraction, where the gasoline is used. This process takes place close to the cultivation area for two reasons; the perishable nature of the leaves and the transportation cost. In order to produce the cocaine (base) paste, the quantity of coca leaves required is so large that transportation of the leaves becomes problematic.

There is no single method for producing cocaine, and many of the ingredients have substitutes (though they often contain the same core components that are necessary to create the chemical processes) (Mejia & Rico 2010; EMCDDA and Europol 2020). In the case of gasoline, the input of interest in this paper, it is possible to substitute with kerosene (paraffin) and oil. However, price and availability make gasoline the most common ingredient.

Daniel Mejia and Daniel M. Rico have estimated the economics of the supply chain for producing cocaine based on the different chemicals needed in the process (2010). Even though the calculations are to be used with caution, as the researchers suggest, it gives a good indication of the ratios of the different inputs needed in the production. They estimate that to produce one kilogram of cocaine base, 382 liters of gasoline, 0.85 liters of Ammonia, 0.10 liters of Sulfuric Acid, 0.35 liters of Caustic Soda, 360 kg of Cement and 1.01 kg of Potassium permanganate are needed. When they adjust the estimation for prices of the different inputs they calculate that to produce one kilogram of base cocaine it costs (in Colombian pesos in 2008): 752,703 pesos for gasoline, 12,546 pesos for Ammonia, 2,318 pesos for Sulfuric Acid, 532 pesos for Caustic Soda, 189,000 pesos for Cement and 120,190 pesos for Potassium permanganate. Consequently about 70 % of the costs of these inputs (if one makes one kilogram) stem from the gasoline. Part of the gasoline used in the production is reusable, so for large scale operation, there are efficiency gains, where the gasoline can be about one fifth (22%) of the cost of chemicals. The estimations for gasoline are used with the prices from Colombia, and not from the smuggled gasoline.

Gasoline is a relatively cheap ingredient. It is the quantity and location that makes it relevant. The amount needed in the production makes it an essential component in the production, also cost-wise. As it is used in the first steps of the production, it is an input for farmers that have small and unstable incomes to start with, making it a critical factor.

As explained in the next chapter, earlier research has studied the effect of changes in demand for coca leaves on violence and finds significant results. Coca leaves are also relatively cheap; even though it is the only fundamental ingredient in cocaine, it also has a minor cost. In 2016, it was estimated that the average price of a kg of fresh coca leaves was 0.95 US dollars, while the average estimated price for a kg of cocaine paste was 621 US dollars, the average estimated price for a kg of cocaine base was 814 US dollars, and the average estimated price for a kg of cocaine hydrochloride (cocaine) was 1,633 US dollars (UNODC & Government of Colombia 2017). Since earlier research has found significant effects of a price shock on coca leaves, which seems to have a lower cost share, it should be possible to detect the effects of a price shock on gasoline.

2.2 Shock to gasoline prices

In neighboring country Venezuela, there is a highly subsidized gasoline market, intended for its inhabitants: everyone with a Venezuelan identity card can go to any gasoline station and buy gasoline for 1 bolivar/liter (El País Cali 2017; BBC 2018). The subsidy was implemented as one of the many services provided to the population at a time when Venezuela was a prosperous country due to its oil reserves. When former president Carlos Andres Perez tried to end gasoline subsidy in 1989, it caused a big riot (Pozzebon 2019). Therefore, this service has persisted through the country's political and economic turmoil because of Venezuela's oil reserves.

An unintended consequence of the subsidy is that many Colombians either travel themselves across the border to buy gasoline or buy smuggled cheap gasoline from Venezuela (BBC 2018, Joshua Collins 2019). Part of this smuggled gasoline is then used in Colombia to produce cocaine (see the chapter above on cocaine production) (Mejia & Rico 2010). The Venezuelan president has addressed this problem on various occasions, but with little effect (El País Cali 2017; BBC 2018).

Since the price of gasoline in Venezuela is fixed, the price for Colombians wanting to buy their gasoline will vary with the fluctuation in the currency between Colombian pesos and Venezuelan bolivars. When Venezuela was hit by hyperinflation, it became cheaper for Colombians to buy Venezuelan bolivars and gasoline from Venezuela. The closer to the Colombian border, the more expensive the gasoline becomes (El País Cali 2017). The price differences remain important even though different actors require payments along the different smuggling routes. The Initiative for Investigative Journalism in the Americas, of the International Center for Journalists (ICFJ) has reported on the increase in illegal import of gasoline due to hyperinflation in Venezuela (El País Cali 2017).

Figure 1 Exchange rate between Venezuelan Bolivar Fuerte Venezolano and Colombian Pesos

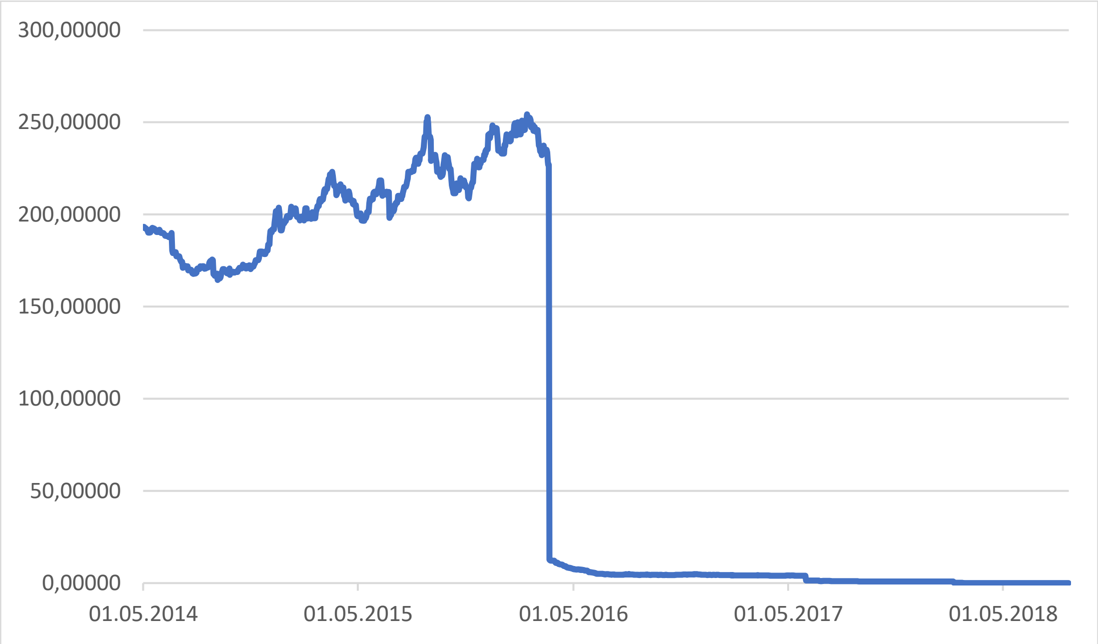


Figure produced with data from the Central Bank of Colombia (2014-2018)

Venezuela is an oil-exporting and import-dependent economy with repressed markets for foreign exchange and intermediate and consumption goods (Cerra 2016). The oil export earnings cover the primary source of foreign exchange, which are used to import various foods and consumer goods. Venezuelan authorities tightly regulates foreign exchange rates, and its system for rationing foreign exchange creates a repressed goods market for import. When the international oil prices fell in 2014, this led to a drop in oil revenues, which again led to a massive reduction in the provision of foreign exchange to importers. This, in turn, led to a sharp decrease in the supply of goods to retail markets that drove the rise in inflation well beyond money growth. Together with a system that allowed different businesses to buy US dollars at different exchange rates, these factors led to a surge in inflation and the black market premium that led to hyperinflation in Venezuela in 2016.

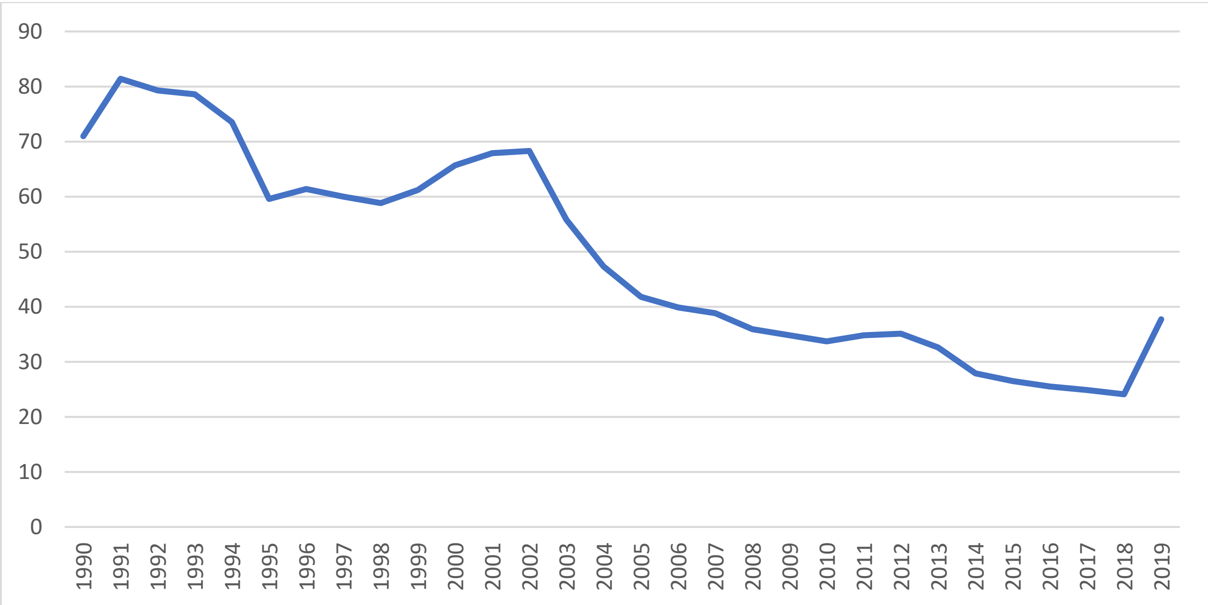
The inflation led to a dramatic fall of the Venezuelan bolivar compared to Colombian pesos (and other currencies), as shown in Figure 1. The depreciation of the Venezuelan bolivar to Colombian pesos makes the illegal gasoline cheaper for Colombians, thus creating a shift in the cost of cocaine production in Colombia. As shown, the reduction in gasoline costs in Columbia were due to hyperinflation in Venezuela and not related to the Colombian cocaine market, and therefore this can be considered an exogenous shock on gasoline prices in Columbia. This paper uses this exogenous price shock on the cocaine market, to study the effect of the cocaine market on violence in cocaine-producing areas.

2.3 Violence in Colombia

Colombia has a long history of violence and civil wars since its independence in 1810 (Angrist and Kugler 2008). There were high levels of violence in Colombia long before they started producing and trafficking drugs.

The country experienced six major civil wars during the 19th century, and during *La Violencia* from 1948 to 1957, more than 200,000 Colombians were killed (Angrist & Kugler 2008; Vargas & Caruso 2014). Drugs did not cause all violence in Colombia, but it does not mean that it did not perpetuate it. The incredibly high level of violence in the 1990s, when the homicide rate reached 70 homicides per 100,000 inhabitants, coincided with a shift in coca cultivation towards Colombia (Mejia & Restrepo 2013). Below, in figure 2, the evolution of homicides in Colombia is graphed for the last 30 years (homicide rate is defined as homicide per 100,000). As one can see, the homicide rate is, on average, decreasing and has dramatically fallen since the early 1990s. It is also possible to notice a small increase in the violence in the last years.

Figure 2 Evolution of homicides in Colombia in the last 30 years



Graph produced with data from UN Office on Drugs and Crime's International Homicide Statistics database, Departamento Administrativo Nacional de Estadística (DANE) and Policía Nacional de Colombia

Most of the homicides in Colombia are committed with firearms coming from at least 20 countries (Open Democracy 2017). Although the peace agreement in 2016 between Fuerzas Armadas Revolucionarias de Colombia (FARC) and the government, forced FARC to hand in (some of) their weapons, there is no reason to believe that there is any shortage of firearms in the country (Ray Mark Rinaldi 2019).

3. Related research

There is little research on cocaine markets, despite their importance (Storti, Grauwe & Reuter 2011). Only in the E.U. it is estimated that 18 million adults have tried cocaine during their lives (EMCDDA and Europol 2020). Cocaine accounts for nearly one-third of the illicit market in drugs, which makes it the second-largest, after cannabis, and the global consumption is increasing. There is also little research on the causal mechanisms between drug markets and violence. As Mejia and Restrepo (2013) point out: “Anecdotal evidence linking cocaine production to violence is not enough to establish a causal relation”.

Most of the research on the topic studies the relationship between legal enforcement of interventions against drugs that may lead to shifts in the market and their effects on violence. Castillo, Mejia, and Restrepo (2020) have studied the impact of a negative supply shock for cocaine from drug enforcement in Colombia and the effect of violence in areas in Mexico that were used for trafficking drugs into the U.S. They found that Mexican cartel violence increased in periods of reduced cocaine supply caused by Colombian government seizures. Dell (2015) shows that in areas with vigorous drug enforcement caused by a shift in political leaders, there was an increase in violence (homicide rate) in Mexico. Abadie et al. (2014) looked at the effects of drug eradication programs in Colombia on violence and found that the eradications led to more violence in the short and long-term. Both Angrist and Kugler (2008) and Mejia and Restrepo (2013) have studied demand shocks in the Colombian coca production as a consequence of drug enforcement and its effect on violence and find that enforcement that leads to higher demand for coca leaves in Colombia, generates more violence. Mejia and Restrepo (2013) studied the effect of shifts in demand for cocaine in the U.S. on violence in Colombia. Since these shifts in demand occurred at the same time as Plan Colombia, the largest law enforcement intervention against drugs in the western hemisphere, it is unclear if their estimates capture the shift in demand or just the shift in drug enforcement. As drug enforcement is violent in its nature, it is difficult to distinguish the effect of law enforcement from the change in demand.

As there is no research, that I am aware of, that looks at a purely economic shock to illegal markets and its effect on violence, it is relevant to investigate the literature on legal commodities and examine the link between price shocks and violence. In the last 10 to 15 years, this literature has changed from analyzing one homogenous effect at a country level to the micro-level and studying the underlying mechanisms, where research points out several competing mechanisms might dominate under different circumstances (Rigterink 2020). Therefore, there is no clear positive or negative correlation between price shocks, income, and violence.

Dube and Vargas (2013) have looked at how income shocks affect armed conflict and violence, with a focus on Colombia. They show that two different mechanisms can lead to opposite effects. The first is the opportunity cost effect, which exhibits a negative relationship between income shocks and violence. The second is the rapacity effect that shows a positive relationship between income shocks and violence. If prices for a labor-intensive natural resource increase, the wages for its worker should rise, which would lead to an upward shift in income for the households, which would increase the opportunity cost of conflict and recruitment to illegal actives (Dal Bo and Dal Bo 2011). However, the rapacity mechanism, also called "natural resources as a prize" or "greed," would raise the return to conflict related to natural resources since there is more money to be earned (Rigterink 2020).

There are different theories on what makes the various mechanisms dominant (Dal Bo and Dal Bo 2011, Dube and Vargas 2013, Rigterink 2020). However, for an illegal good like coca, the mechanisms should work in the same direction, at least for a positive shock. Parallel to the opportunity cost effect, a positive shock to the coca market would increase the household income from coca and give them the incentive to join these illegal actives, which can cause more violence. For the rapacity effect, a positive shock to the coca market would increase the incentives to overtake production that belongs to others, either vertically (by taking over more of the production chain) or horizontally (by taking over coca leaves farms from others). The rapacity mechanism often leads to turf wars between the gangs (Lessing 2015). It is even possible that the two mechanisms might reinforce each other. If the opportunity cost leads more people into the market, and with more workers in the market, their greed may lead them to take over different areas. Or if farmers earn more on the production, they can afford to do more of the production themselves, and thereby increase their income, which again can lead to more violence. In conclusion, a positive supply shock that results from cheaper gasoline should likely lead to more violence in the areas producing coca than in the ones that do not produce it.

4. Data

My dataset includes data on the cultivation and production of coca and cocaine, data on violence, and data on exchange rates between Colombia and Venezuela.

4.1 Data on cultivation and production

To estimate the causal effect of cocaine production on violence, I would ideally use data on cocaine production; however, the information on cocaine production is not available since it is an illegal industry. Fortunately, I use can data on coca production, which is an indirect way to measure the effects of cocaine production. As mentioned in 2.1 Cocaine production in Colombia, the first stages of the cocaine production take place physically close by the cultivation areas.

The optimal data source on the coca cultivation would have been the Integrated Monitoring System of Illicit Crops (SIMCI) of the United Nations Office on Drugs and Crime (UNODC). SIMCI is a satellite-based monitoring system that estimates the extension of coca crops annually (Abadie et al. 2014). It uses satellite imagery of Colombia, and based on these satellite pictures, SIMCI experts will geo-reference the area that they interpret as coca producing, based on visual inspection. Then these areas interpreted as coca producing are confirmed via high definition photographs through helicopter flights. Unfortunately, I did not get access to these data in time[1]. Instead, I use seizure data. The problem with seizure data is that it might not be perfectly correlated with the actual cultivation data. The police might not always do big seizures in areas with large cultivation either because of fear of violent confrontation or because of corruption. Since Colombia has access to good quality data on coca cultivation, it is still likely that the police do seizures regularly in areas with a high density of cultivation. I have verified that all the top producing municipalities are part of the seizure data. Consequently, the correlation should be high between the two datasets. A preliminary study of the geo-referenced data shows that nearly all municipalities in the treatment group had cultivation in 2016 and 2018.

The data I use is at a yearly level, and the data is at the municipality level. In Colombia, there are 1,123 municipalities grouped into 33 departments. Municipalities are analogous to counties in the U.S., whereas departments are analogous to states (Dube & Vargas 2013).

[1] It will be interesting, in the future, to check whether my findings are robust to this type of data.

4.2 Data on violence

My main dependent variable is the homicide rate per 100,000 inhabitants from 2010 through 2019, which is constructed from homicide data from National Police Statistical Contravention Crime and Operational Information System - SIEDCO. The data provide information on the cause, location, circumstance of death, date, gender, and age. I use municipality-level population projections to compute death rates based on the National Census of 1985 and 2020 from the Colombian National Statistics Department (DANE).

Homicides are often used as a proxy for violence because it is highly correlated with other violence and are accurately measured (Soares 2004). I use the normalized variable, homicides per 100,000 inhabitants, as this is the most common practice and allows for comparison across time and space.

4.3 Data on exchange rates

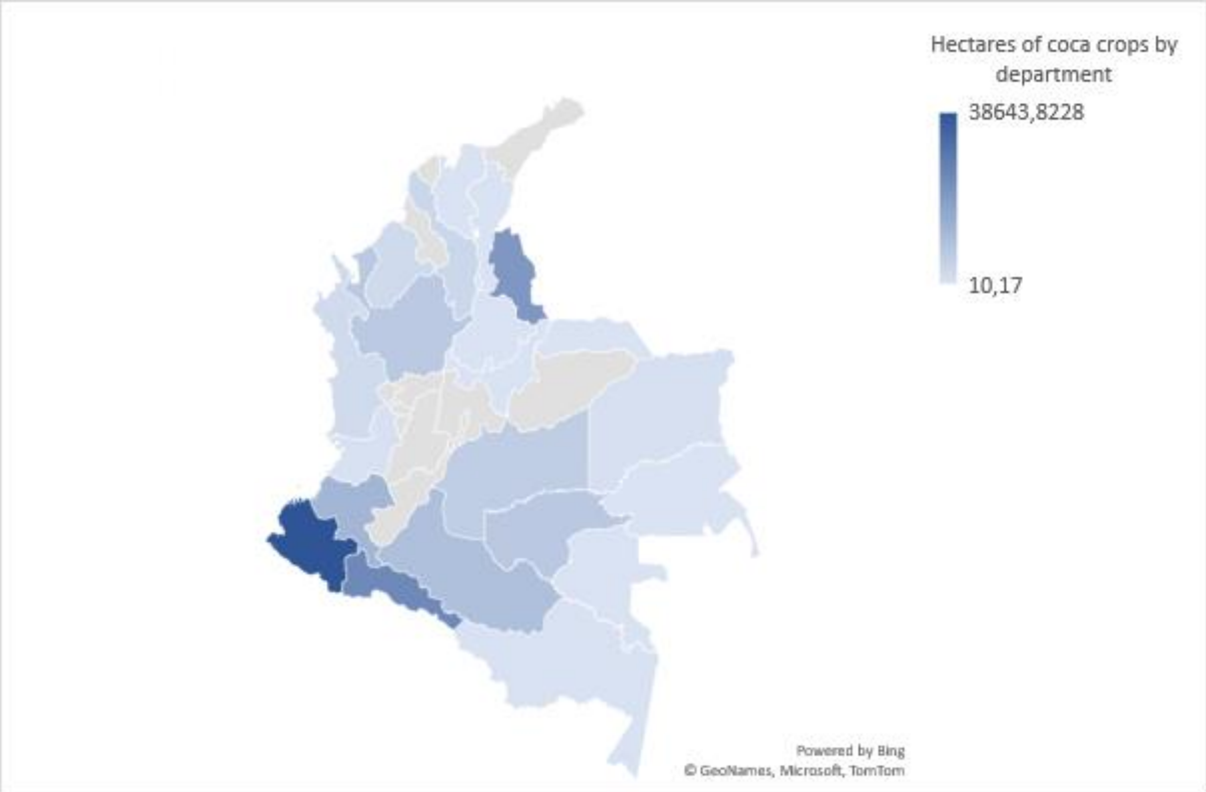
I use official currency data on exchange rates between Colombia and Venezuela from the Colombian Central Bank (Banco de la República Colombia 2020) to model the price shock. I use Colombian pesos for Colombia and Bolívar Fuerte Venezolano for Venezuela. Venezuela has several currencies due to their high inflation. I use Bolívar Fuerte Venezolano because it was the official currency from 2008 until August of 2018. The price shock, as one can see in Figure 1, shows a massive devaluation of Bolívar Fuerte Venezolano to Colombian peso in 2016.

5. Identification strategy: Differences-in-differences design

I will estimate the effect of the cocaine price shock on violence. However, it is challenging to estimate causal effects on violence in a country like Colombia due to the high number of instability factors (war, peace processes, economic instability, and income inequality). Many factors can affect violence, and drugs do not cause all violence. Therefore, I use the differences-in-differences (DiD) design to exploit the geographic variation in coca cultivation intensity. I also exploit exogenous time variation in gasoline prices, an input in the cocaine production induced by a currency shock between Venezuela and Colombia. The strategy is similar to the one Dube and Vargas (2013) use to look at the effects of economic shocks and change in violence in Colombia for legal goods, and the one Sviatschi (2018) uses to estimate the impact of a demand shock for coca leaves on children's long-term outcomes in Peru.

There is a high concentration of coca cultivation within a few areas in Colombia, and this was also the case in 2016 when the gasoline price shock occurred (UNDOC 2017, 2019). The concentration of cultivation is shown on the map below (Map 1). The map displays the coca cultivation by share of land area covered by coca plants, with darker colors indicating a higher density of coca cultivation. One can easily see the concentration of coca cultivation; there are relatively small areas in colors, and only a few places are represented by dark blue, which indicates a high concentration of coca crop cultivation. The high concentration of coca in a few areas make the scenario suitable for a difference-in-differences (henceforth referred to as DiD) analysis, where one compares the changes in violence in the "treated" areas where there is a high concentration of coca cultivation with the areas with low (or no) cultivation of coca. The areas with a high concentration of coca cultivation will be the treatment group that will be affected by the price shock, while the areas with low coca cultivation will be the control group. The map only shows the cultivation areas and not the production plants. These areas are also where the first part of the production chain of cocaine, where gasoline is used as an input, is located.

Map 1 Coca cultivation in Colombia in 2016



Notes: The darker blue color indicates high density of coca cultivation and one can observe that the cultivation is highly concentrated in a few areas. Map produced with data from Observatorio de Drogas de Colombia.

Formally, the Differences-in-Differences (DiD) model may be expressed as:

$$y_{it} = \alpha_i + \alpha_2 post_t + \alpha_3 treat_i + \beta(treat_i \times post_t) + \lambda_t + \varepsilon_{it} \quad (1)$$

Where the subscript *i* specify the municipality, and *t* represents time measured in years. y_{it} is the homicide rate of municipality *i* in year *t* and is the outcome variable of interest. *treat* is the treatment variable taking the value 1 if the municipality is in the treatment group (with high cultivation of coca) and 0 if the municipality is in the control group (low cultivation of coca). *Post* is a binary variable taking the value 0 if the year is 2010 to 2015 and the value 1 if the year is 2016 to 2019 since the shock happened in 2016. $\lambda_t$is a vector of year-fixed effects, and α_i is the municipality fixed effect. Like Dube and Vargas (2013), I employ the municipality fixed effects to control for time-invariant municipal characteristics that may be correlated with economic conditions that may affect the conflict outcome. ε_{it} is a time-varying error term.

The coefficient of interest, β measures the average causal effect of the positive price shock in gasoline prices on the outcome variable, homicide rate. The identifying assumption is that the change in the outcome variable would have been the same in both the treatment and control group in the absence of the price shock. β, our parameter of interest, estimates the average change in violence for municipalities that produce coca compared with municipalities that do not produce coca.

To avoid overstating the precision of the estimates, I cluster standard errors (Cameron & Miller 2015). I cluster standard errors at the department level to account for potential serial correlation over time and across municipalities within a department in violence. Although the treatment status is at the municipality level, I believe there can be a correlation within departments, so I cluster at the department level. Dube and Vargas (2013) do the same when studying the effects of different price shocks on other commodities on violence in Colombia. There are 33 departments in Colombia. There is no clear consensus on the exact number of clusters needed; some may say 33 is enough and others suggest that less than 42 is too little (Angrist & Pischke 2008). The problem is that cluster-robust standard errors are potentially downward biased with a small number of clusters (Cameron, Gelbach & Miller 2008). Therefore, I also use wild cluster bootstrap, a strategy that has been shown to perform well with small numbers of clusters.

Definition of treatment and control groups

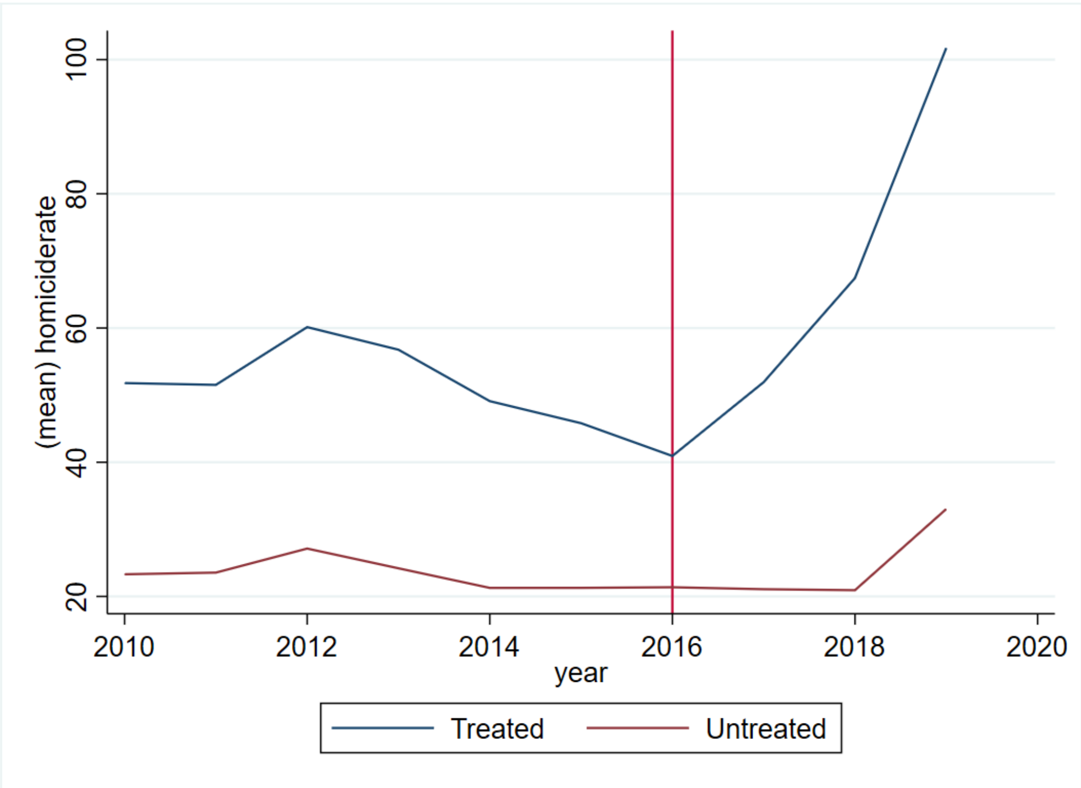
I define the treatment status based on coca cultivation. Specifically, I define the treatment status based on coca cultivation status in the years before the shock in 2016. I define the treatment group as the municipalities that had registered coca cultivation all the 4 years before the shock. This definition implies a treatment group of 76 municipalities, while the control group is the remaining 971 municipalities (see Appendix B for the list of municipalities). After the main analysis, I will conduct a series of robustness checks with alternative definitions of the treatment variable.

6. Results

6.1 Graphical representations

Figure 3 graphs the trends, including the pre-trends, for the homicide rate by treatment and control group. An important assumption for the DiD design to hold, is the assumption of a parallel trend between the treatment and control groups before the shock (Angrist & Pischke 2009). The assumption is that in the absence of a shock, the two groups would continue with a parallel trend; this is impossible to test since one cannot see the counterfactual outcome. The estimated treatment effect relies on the assumption of parallel trends. The graph shows a parallel trend before the shock in 2016 (indicated by the red vertical line). The graph also indicates distinct developments in the treatment and control groups after the shock in 2016. Whereas the mean homicide rate (homicide per 100,000) in the treatment group increases after the shock, the mean homicide rate in the control group is stable after the shock until 2018, where it also increases.

Figure 3 Graphical representation of pre-trend



6.2 Main findings

Below in table 1, are the main results from the DiD analysis. I present the results with and without the different fixed effects and the standard errors clustered at the municipality level and department level. The standard errors become bigger once I start clustering at a higher level, and consequently, the results become less significant. The main result is displayed in column 6 and shows a positive statistically significant effect of 12.34, which is robust across all specifications. The wild clustering of the standard errors shows similar results, with a p-value of 0.03 versus 0.024 in the main analysis where I cluster at the department level (see appendix A). The result indicates that, on average, the impact of the shock in the treatment group (the areas with high cultivation) is an increase of 12.34 homicides per 100,000 inhabitants. Even for a violent country like Colombia, the number is quite high. The average homicide rate in the whole sample is about 26 homicides per 100,000 inhabitants, which implies that the effect of the supply shock is equivalent to a 50% increase in the number of murders in the average municipality. The parameter has a positive sign, suggesting that the positive supply shock to cocaine production (the drop in the price of imported gasoline) leads to more killings as hypothesized.

Table 1 Differences-in-differences analysis of the effect of the price shock on homicide rates

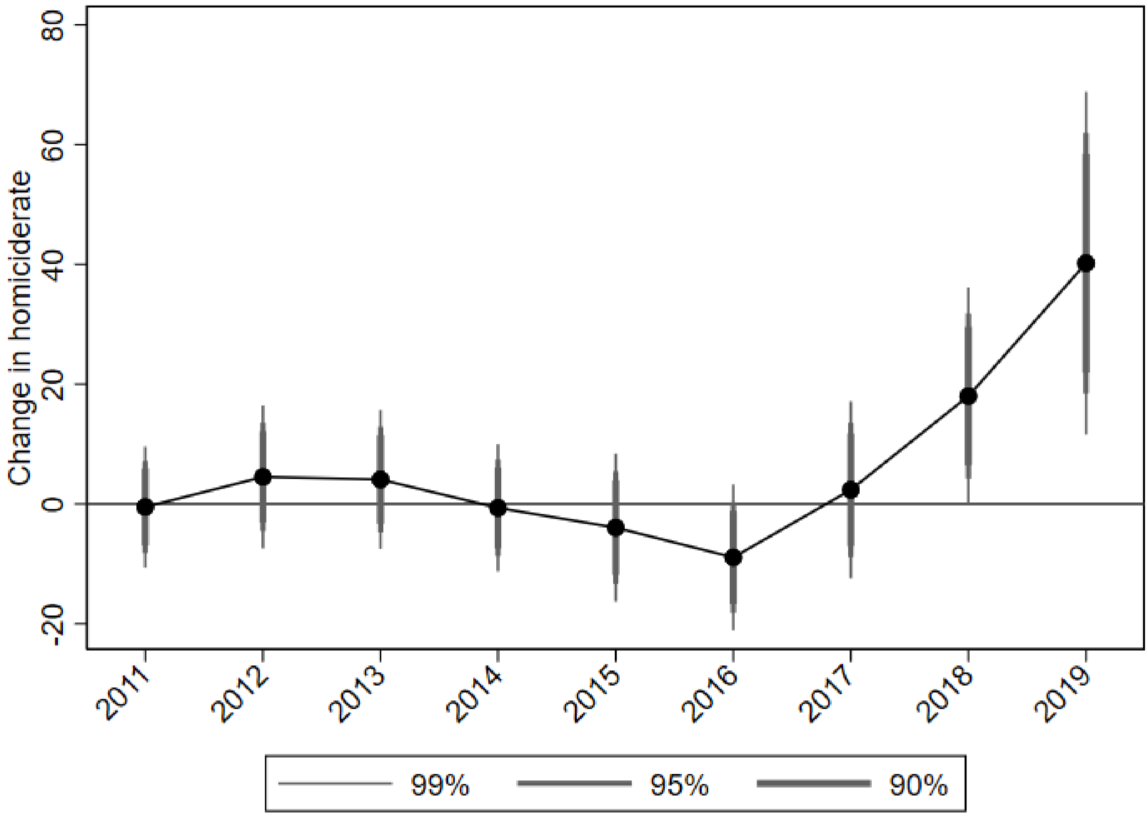
	(1)	(2)	(3)	(4)
DiD	12.34**	12.34**	12.34**	12.34**
	(5.459)	(5.461)	(5.461)	(5.469)
Constant	23.46***	23.27***	25.23***	44.45***
	(3.760)	(3.384)	(2.232)	(2.294)
Observations	11,220	11,220	11,220	11,220
Number of muni	1,122	1,122	1,122	1,122
Municipality FE	NO	NO	YES	NO
Year FE	NO	YES	YES	YES
Department FE	NO	NO	NO	YES
Cluster	Department	Department	Department	Department

Cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

6.3 Alternative graphical representation

Below, in Figure 4, I have graphed the developments for the treatment and control groups using an alternative technique. The graph presents the difference in the change in the average homicide rate in the treatment and the control group from one year to another, and one can see that for the first years the change is small and not significantly different from zero, while after 2016 there is a larger and significant positive change in the treatment group. The graph confirms the parallel trend shown in Figure 3.

Figure 4 Alternative graphical representation



6.4 Placebo test

I have run a differences-in-differences placebo test. The idea with a placebo test is to pretend that the shock happened earlier than it happened. One can thus "test" the untestable parallel trend assumption, which is necessary for the DiD design (Gertler 2016). The DiD design relies on the idea that in the absence of the shock (treatment), the treatment and control groups would continue to move in parallel. This assumption is impossible to test for, as we will never see the counterfactual (which in this case would be the absence of the price shock in 2016). It is still possible to test the validity of the parallel trend assumption with a placebo test.

A placebo test is run by using data from the pre-shock period between 2010 and 2015. For the different placebo estimations, I will assume that the shock happens in another year than the actual shock. In the first estimation, I assume the shock was in 2011, in the second estimation, I assume the shock was in 2012 and so forth. Commonly, the placebo test uses one point in time for the test, however here I have done a placebo test for all available time points.

If there were significant effects in the placebo test, the parallel trend assumption would not be valid. Below in Table 2, one can see the results of the placebo tests. Standard errors are clustered at the department level. The placebo tests show no significant effects at a 5 or 1 % level. 2014 and 2015 only show a statistically significant effect at the 10 % level and with the opposite sign of the main findings. The negative sign reflects the drop we see for homicides for the treatment group before the price shock. As illustrated in Figure 3 this small drop is not likely to affect my findings. The wild clustered errors show no statistically significant effect (see Appendix A).

Table 2 Differences-in-differences placebo-estimation

VARIABLES	(1) placebo year 2011	(2) placebo year 2012	(3) placebo year 2013	(4) placebo year 2014	(5) placebo year 2015
DiD	0.692	1.251	-1.512	-4.330*	-5.440*
	(4.255)	(2.550)	(2.450)	(2.542)	(3.077)
Constant	25.23***	25.23***	25.23***	25.23***	25.23***
	(1.616)	(1.616)	(1.618)	(1.619)	(1.618)
Observations	6,732	6,732	6,732	6,732	6,732
R-squared	0.012	0.012	0.012	0.013	0.013
Number of muni	1,122	1,122	1,122	1,122	1,122
Municipality FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Cluster	Department	Department	Department	Department	Department

Clustered robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

7. Potential threats to the Differences-in-differences design

7.1 Immigration from Venezuela

One potential threat to the validity of the differences-in-differences design (DiD), is the increasing immigration from Venezuela to Colombia. The assumption for DiD to hold is that the treatment group and the control group would have experienced identical trends in the absence of the treatment, and migration from Venezuela could invalidate this assumption.

In the data, there is an increase in the number of Venezuelans that are victims of homicide. In 2010-2012, the number of Venezuelans killed was less than 20, in 2017, the number jumped to 80, and in 2019, 439 were reported killed. The concern is not that these homicides would bias the results, as they constitute only 0.62 % of the murders, and it is possible to remove them from the data. The concern is that the Venezuelans might be victims of crime and also cause crimes since they are vulnerable, with little money, escaping a difficult situation in their home country. If Venezuelan immigrants could disproportionately move to the areas which are defined as treatment municipalities, this could bias the DiD estimates. As mentioned earlier, by using a DiD design, the objective is not to explain all the changes in violence in the country, just the different trends between the treatment and control groups. Nevertheless, if there is a disproportional flow of Venezuelans that move the treatment areas, this could bias the estimations.

In 2014, only 23,573 Venezuelans were living in Colombia, while in 2019, 1,488,373 Venezuelans were living in Colombia (Migración Colombia 2020).

The map below (*Map 2*) shows the estimations of the concentration of immigrants from Venezuela at a municipality level in Colombia. The red color indicates more than 10,000 immigrants per municipalities, dark orange indicates between 1,000 and 10,000 immigrants, light orange indicates between 500 and 1,000, dark gray indicates between 100 and 500 immigrants and the light gray color indicates that there are less than 100 immigrants from Venezuela in the municipality.

To test whether immigration could affect the DiD analysis, I redo the analysis without the municipalities with a large number of immigrants from Venezuela. I first redo the analysis without the municipalities with more than 40,000 immigrants from Venezuela (the ones listed as the top 8 municipalities in Table 3).

Secondly, I repeat the analysis without the municipalities with more than 10,000 immigrants (the once that are in red in figure 1 and are listed in Table 3). Finally, I redo the analysis without a bigger sample of municipalities in Colombia with immigration from Venezuela. I now use the top 60 municipalities in Colombia with Venezuelan immigration, all the municipalities with noticeable migration, and exclude them from the analysis (see the table in Appendix C for list).

I redo both the main DiD analysis and the pre-analysis to test the parallel trend assumption as the main analysis.

Map 2 Immigration from Venezuela in Colombia

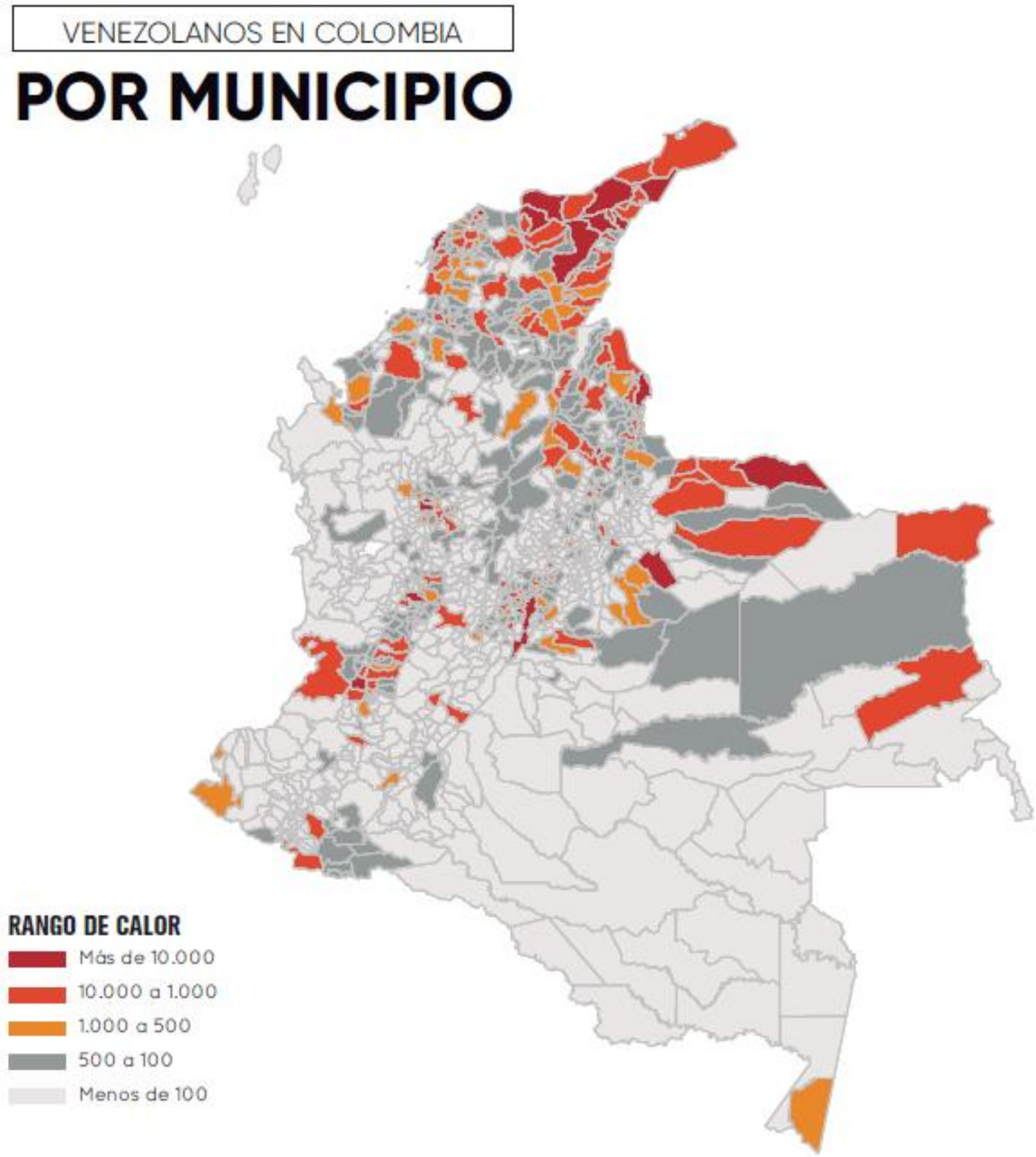


Figure from Migración Colombia (2020)

Table 3 Municipalities with many immigrants from Venezuela

Municipalities	number of immigrants	population	% immigrants to population
Bogotá,D,C,	357667	8281030	4.32
Cúcuta	93461	674831	13.85
Barranquilla	86918	1236202	7.03
Medellín	86201	2549537	3.38
Cali	55884	2470852	2.26
Maicao	44251	166603	26.56
Riohacha	42278	295984	14.28
Cartagena de Indias	40798	1047005	3.90
Bucaramanga	37094	528610	7.02
Santa Marta	35166	515717	6.82
Valledupar	29165	493367	5.91
Villa del Rosario	28147	96953	29.03
Soacha	25159	556268	4.52
Soledad	23589	683580	3.45
Arauca	17187	93261	18.43
Pereira	12156	478892	2.54
Bello	11812	491182	2.40
Yopal	10732	152655	7.03
Floridablanca	10721	267538	4.01
San Juan del Cesar	1036	39472	2.62
Fonseca	1013	35205	2.88
Ciénaga	10128	105510	9.60

I start by redoing the graphical representation of the pre-trend, as shown in Figure 5, 6 and 7. The pre-trends are quite similar to the main analysis. They are remarkably parallel, and they display, as in the main analysis, a jump in the treated sample after the shock in 2016.

Figure 5 Graphical representation of pre-trend using a restricted sample 1

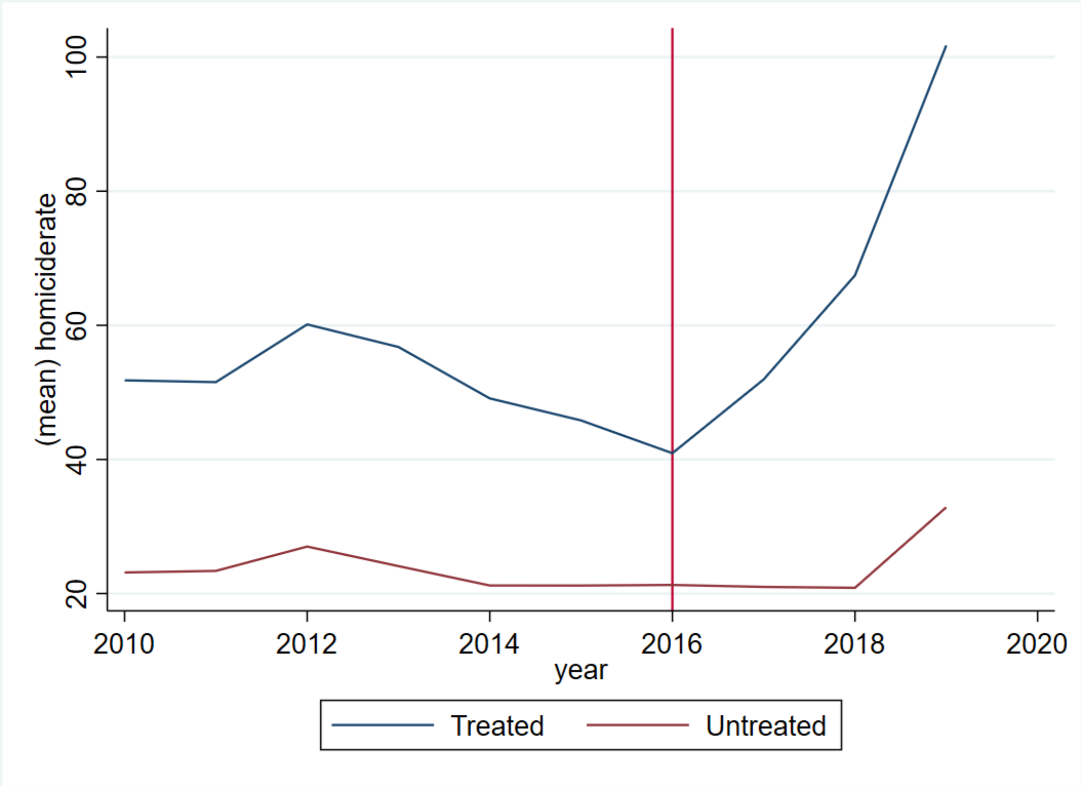


Figure 6 Graphical representation of pre-trend using a restricted sample 2

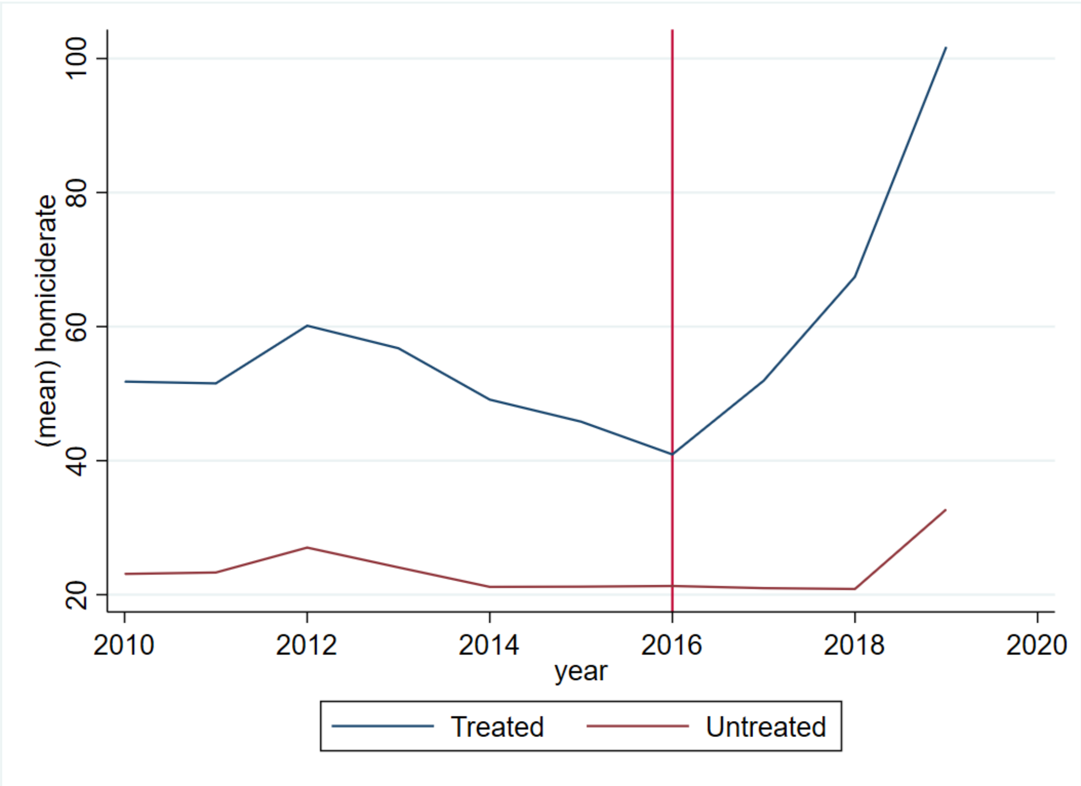
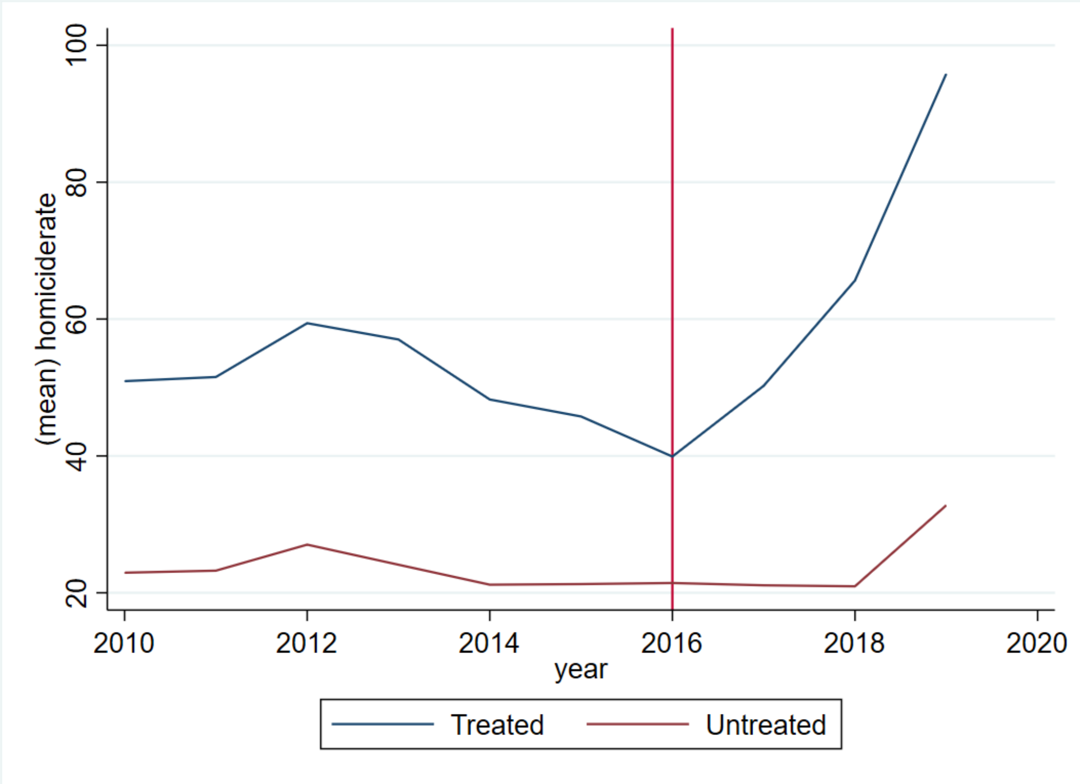


Figure 7 Graphical representation of pre-trend using a restricted sample 3



I then redo the main differences-in-differences analysis, as one can see in Table 4. All the estimations are positive and statistically significant, as in the main analysis. The estimations are a bit smaller in size, 9.99 compared to 12.34, for the most restricted sample, which is natural since I have excluded a large part of the population (the areas with most immigrants tend to be the bigger cities, with some exceptions). This extra analysis shows that the effect that is measured in the main analysis cannot be explained by immigration from Venezuela.

I also do placebo tests and the alternative graphical representation on these restricted samples, as one can see in Appendix C. The placebo tests all show results that are similar to the main analysis and not statistically significant. The graphical representation confirmed this similar pattern.

Table 4 Differences-in-differences analysis of homicide rate without municipalities with many immigrants from Venezuela

VARIABLES	(1) full sample	(2) restricted sample 1	(3) restricted sample 2	(4) restricted sample 3
DiD	12.34**	12.32**	12.32**	9.991**
	(5.461)	(5.452)	(5.452)	(4.652)
Constant	25.23***	25.10***	25.10***	24.83***
	(2.232)	(2.264)	(2.264)	(2.391)
Observations	11,220	11,140	11,140	10,660
R-squared	0.037	0.037	0.037	0.033
Number of muni	1,122	1,114	1,114	1,066
Municipality FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Cluster	Department	Department	Department	Department

Clustered robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

7.2 Definition of treatment

The treatment definition is based on cultivation status, and despite the fact that some areas have more cultivation than others, I still have to choose a cutoff for what is included in the treatment and control groups. This definition is especially important since I am using seizure data that might not be perfectly correlated with the actual cultivation, particularly on the lower end of cultivation. To ensure that the cutoff choice is not essential for the results, I perform robustness tests where I change the cutoff for the treatment status. I will start by widening the definition of treatment status. In the main analysis, I defined the treatment group as the municipalities that had registered coca cultivation all the 4 years before the shock. I will now use broader definitions where it is sufficient that there was registered coca cultivation in at least some of the 4 years before the shock.

Group 2 has a treatment group that is somewhat larger than the treatment group in the main analysis. For Group 2 there only has to been registered coca cultivation in 3 of the 4 last years before the shock (see Appendix B for the full list of municipalities). Group 2 treatment contains 119 municipalities, while the corresponding control group contains 1,004 municipalities.

Group 3 is a bigger treatment group where there has to be registered coca cultivation in 2 of the 4 last years before the shock (see appendix for the full list of municipalities). Group 3 treatment contains 169 municipalities and group 2 control contains 954 municipalities.

Group 4 is the largest treatment group containing 237 municipalities (886 municipalities in the control group) where there has only been registered coca cultivation in one of the 4 last years before the shock (see appendix for the full list of municipalities). As the treatment groups get larger, more municipalities with little cultivation are included, so the effect size will likely be smaller.

I re-run the DiD analysis using the alternative treatment groups with the same outcome variable and equation (1) as the main analysis. The results are shown in the table below (Table 5). The estimations are shown both when clustering at the municipality level and the department level. The estimations are statistically significant (at 1 % or 5 % level) and positive, as in the main analysis. The results show that the choice of cutoff is not essential for the results. As expected, the effects are slightly smaller; whereas the main estimation is 12.34, the estimates for group 2 are 11.34, for group 3 are 10.46 and for group 4 are 7.11. The smaller effects correspond the wider treatment definitions that now include municipalities with lower presences of coca cultivation.

Table 5 Differences-in-differences estimation using alternative treatment definitions

VARIABLES	(1) group 2	(2) group 3	(3) group 4
DiD	11.34*** (3.611)		
DiD		10.46*** (3.090)	
DiD			7.108** (2.647)
Constant	25.23*** (2.217)	25.23*** (2.206)	25.23*** (2.205)
Observations	11,220	11,220	11,220
R-squared	0.038	0.039	0.036
Number of muni	1,122	1,122	1,122
Municipality FE	YES	YES	YES
Year FE	YES	YES	YES
Cluster	Department	Department	Department

Clustered robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

I redo the graphical analysis for the different treatment and control groups to see if visually they have parallel pre-trends. Below I have graphed the trends, including the pre-trends for the homicide rate by treatment and control group (Figure 8, 9 and 10). All three different definitions of treatment and control groups show a parallel trend before the shock in 2016 (indicated by the red vertical line) as the original treatment group does. I have graphed the treatment and control groups using an alternative technique, as one can see in Appendix D. The graph presents the change in the average homicide rate in the treatment and the control group from one year to another. The graphs present a similar result to the main analysis where one can see that for the first years, the change is small, and from 2016 there is a more considerable positive change for all the three groups.

Figure 8 Graphical representation of pre-trend group 2

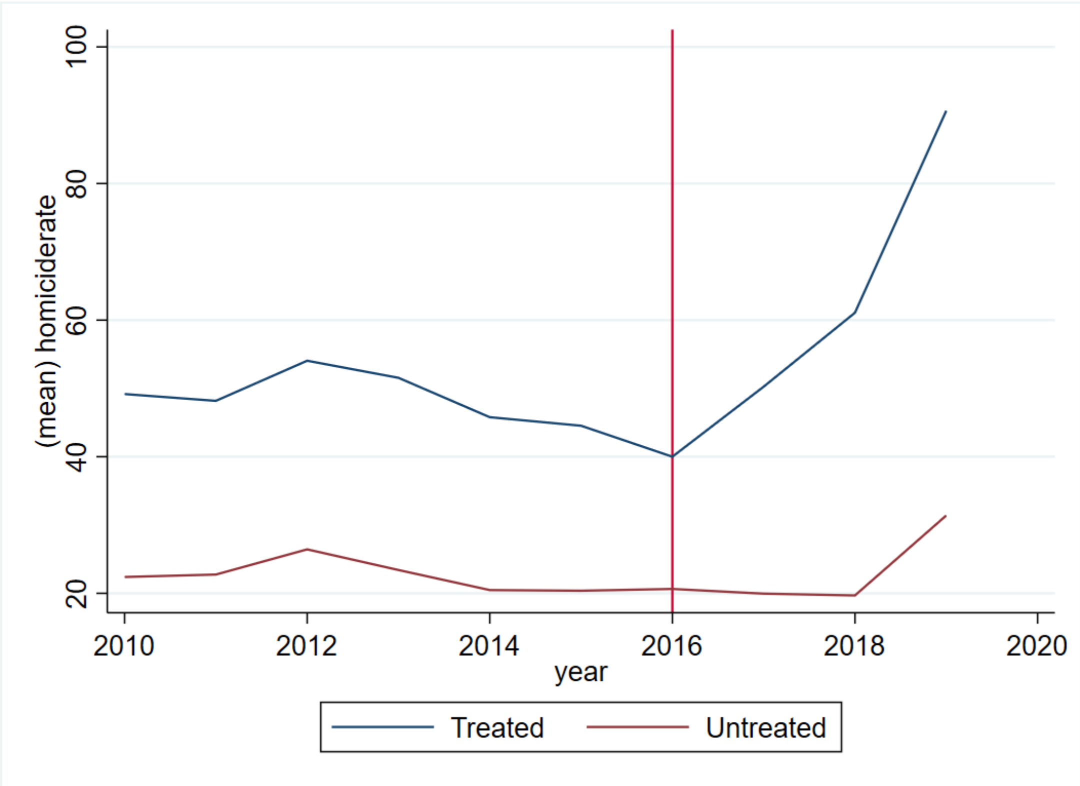


Figure 9 Graphical representation of pre-trend group 3

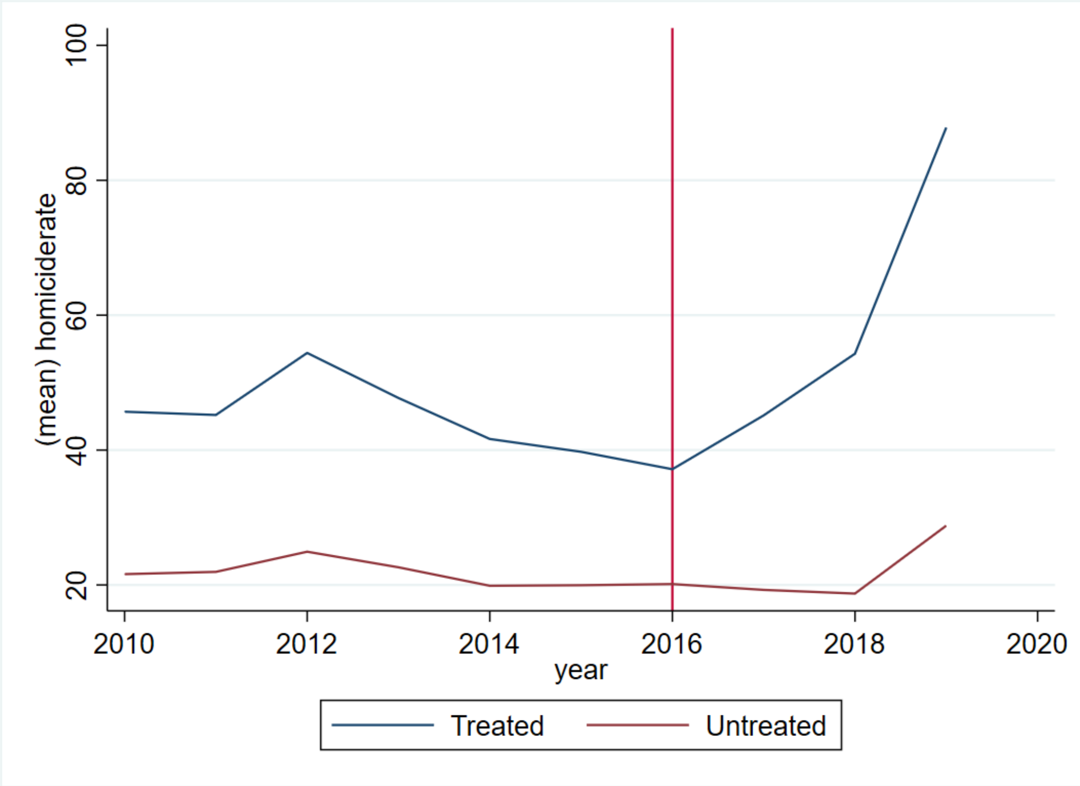
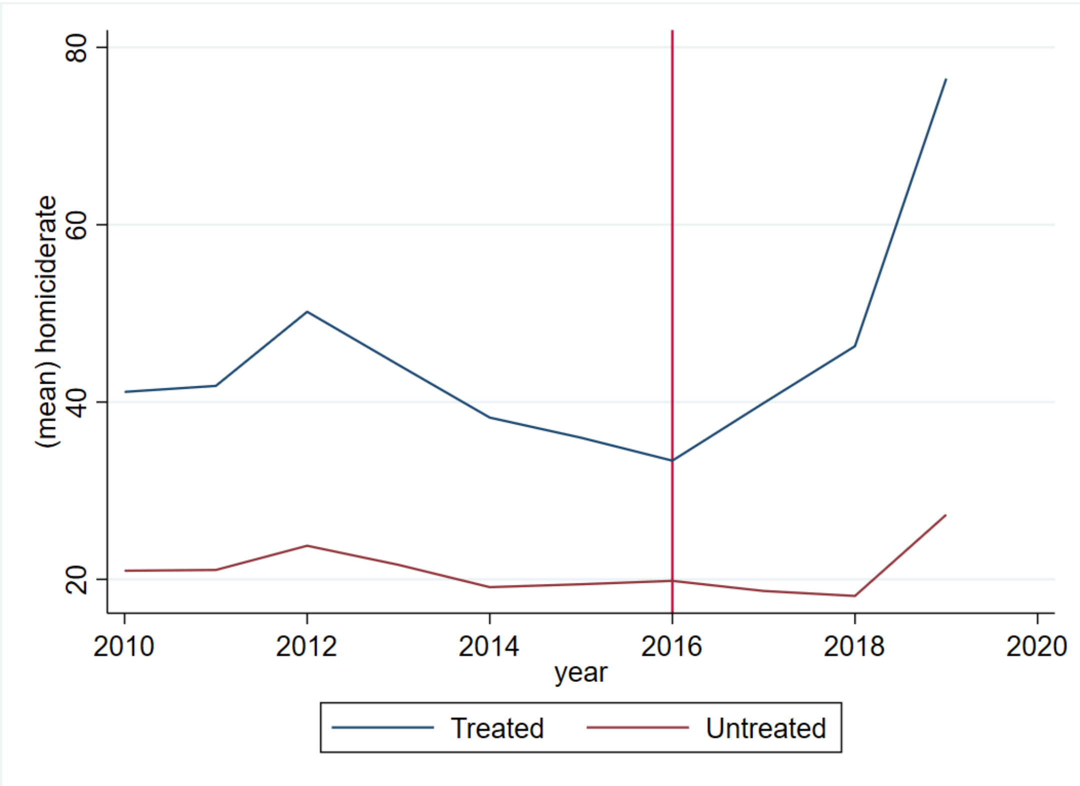


Figure 10 Graphical representation of pre-trend group 4



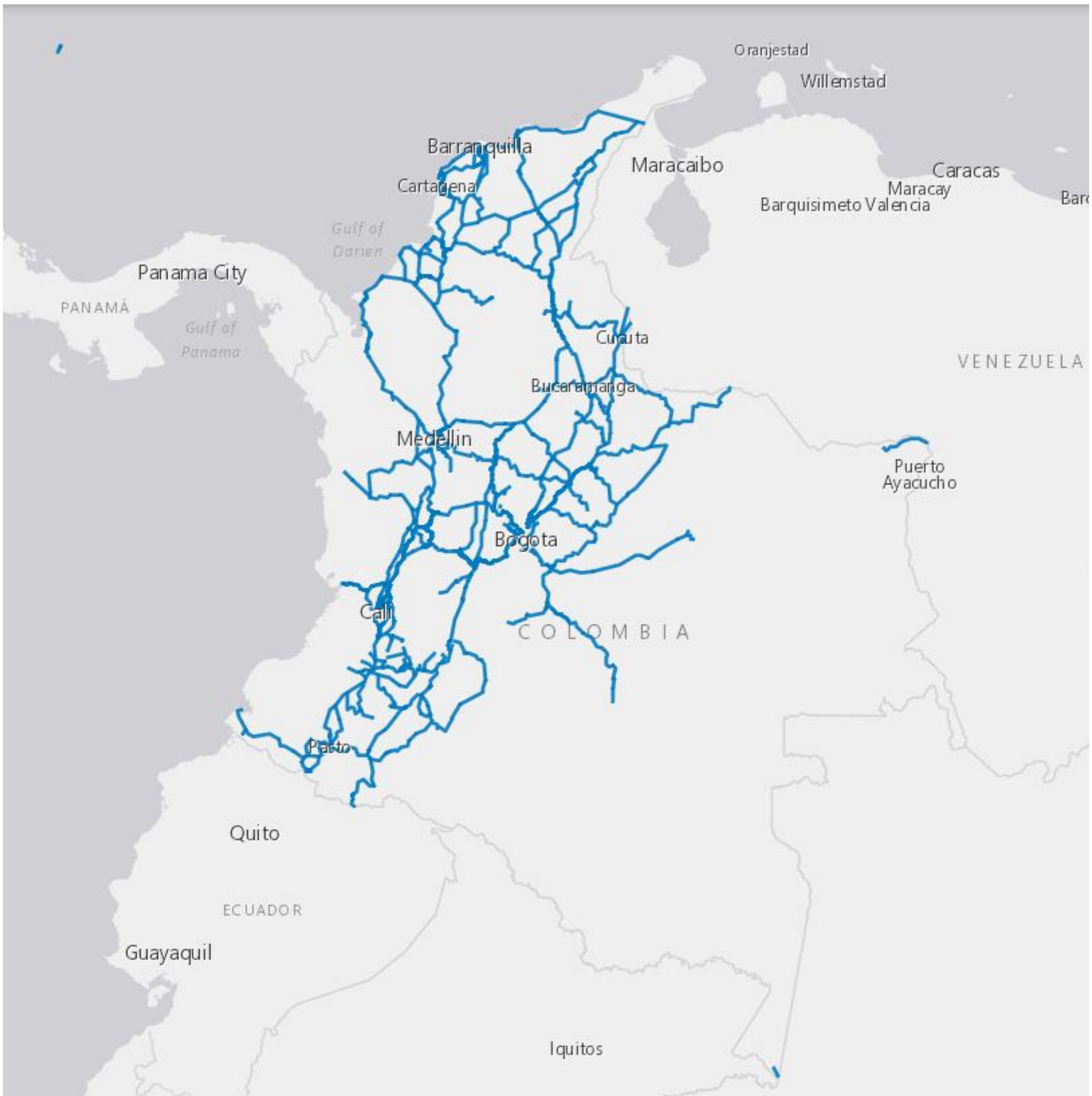
7.3 Study of areas close to the border with Venezuela

An alternative method for testing the robustness of the analysis, is to decrease and concentrate the full sample (while keeping the original definition of treatment and control). The assumption behind a concentrated sample is that municipalities that are close to the border with Venezuela should be more affected by the positive shock from Venezuela due to transportation costs (it takes time, money, and risk to transport the illegal goods).

Looking at the road network in Colombia, it is not apparent how much the transportation time (and cost) increases in the different areas. As one can see from the map (Map 3), there are no main roads in the eastern part of the country, and therefore it is likely that the commodities from Venezuela arrive from the North and North East and will pass through the country to arrive in the South. Since Colombia is a large country, there are big differences in transportation distances. Whereas Cúcuta is only a 2-hour drive from El Tachira, Venezuela, Bogotá is 14 hours away and Pasto 28 hours away (Google calculation 2020).

It is possible to calculate the travel distance from the border to each city, but most production occurs in rural areas, where road quality can be poor. Therefore, it is not sure that it is faster to get to a little village far outside Bogotá, than a rural area outside of Calí because the speed of transportation is faster and easier on highways. I will assume that everything else being equal, on average, a municipality in the South West is further away from the Venezuelan border than a municipality in the North East. With this assumption, I simplify by comparing the departments that are adjacent to the border with the departments that are adjacent to departments that are adjacent to the border.

Map 3 Main road network in Colombia

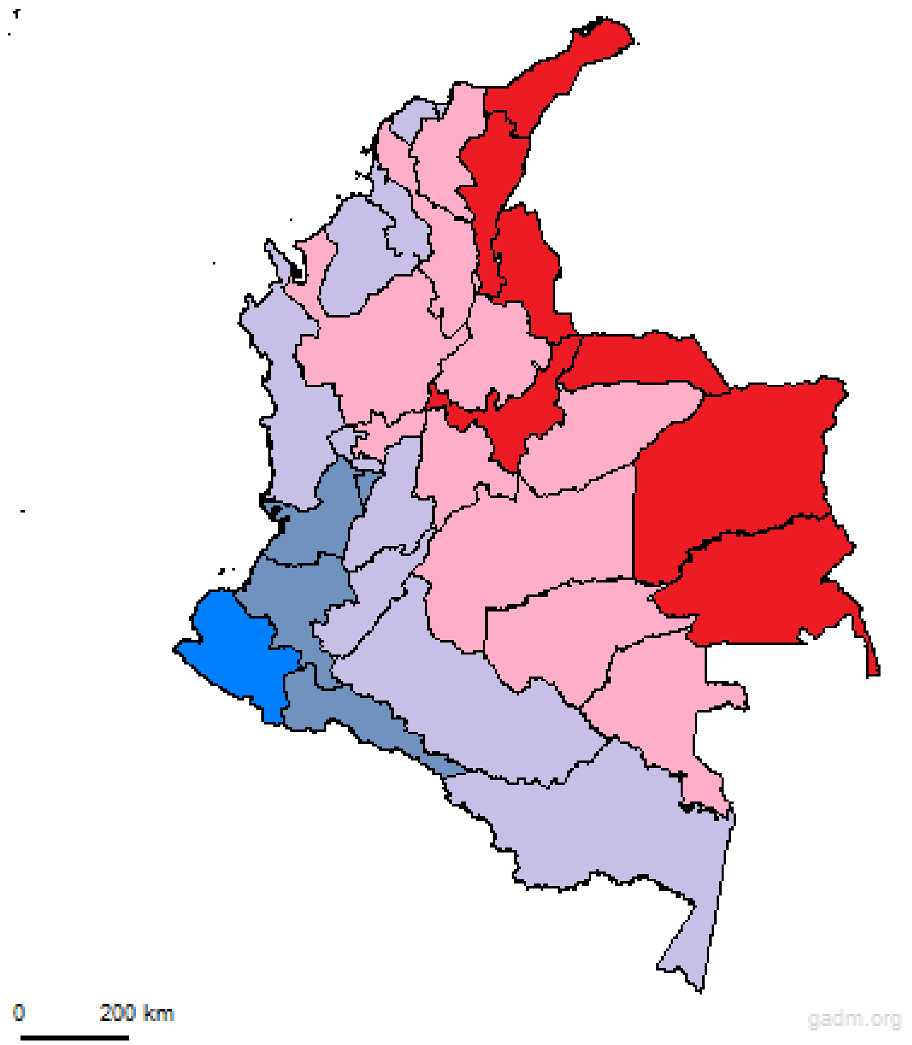


Map from Instituto Nacional de Vías (Colombian Ministry of Transportation)

On the map below (Map 4), it is possible to see the departments' proximity by color code: the areas in red are nearest and adjacent to the Venezuelan border, and the once in blue color is further away. The departments adjacent to the Venezuelan border are La Guajira, Cesar, Norte de Santander, Boyacá, Arauca, Vichada, and Guainia (in red on the map below). The departments adjacent to these departments are Magdalena, Bolivar, Antioquia, Santander, Caldas, Cundinamarca, Casanare, Meta, Guaviare, Vaupés (in pink on the map below).

I will now redo the analysis using the different subsamples, first, with the departments adjacent to the border hereafter, the red sample. Then the departments that are adjacent to the border and the once that are adjacent to departments that are adjacent to the border, the red and pink sample. I start with the red sample, and I then do the pink and red sample.

Map 4 Departments in Colombia by proximity to the Venezuelan border



Notes: The map display Colombia with department borders. The color represents the distance to the Venezuelan border. The red color represents the department directly adjacent to the Venezuelan border. Blue represents departments with less proximity to the Venezuelan border.

I redo the DiD analysis for the different sub-samples. The analysis is done by clustering both at the municipality level and department level. See table 6 below for the results. All the effects are positive. The effects are statistically significant for the red and pink samples, but not for the red sample, where the sample consisted of only the departments adjacent to the border. The effect is nearly double the original analysis's size from 12.34 to 20.23 for the pink and red sample. It makes sense that the effect increases as the sample get more concentrated on the area affected. Still, this does not explain why the sample nearest the border, the red sample is not statistically significant (or only statistically significant at 10 %). The smaller the sample, the less power there is in the analysis, and this may explain why the analysis done to the red sample is not statically significant. Due to the small numbers of cluster, I re-run the analysis with wild clustering errors. These results (as one can see in appendix), show no statistical significant effects, with p-values of 0.12 and 0.232 for the pink and red and red sample respectively. Therefore, this sub-analysis does not seem to be suitable for inference. One potential reason for the violation of the DID assumption could be that the areas close to the border generally can experience more violence, for other reasons than cocaine production, such as smuggling.

Table 6 Differences-in-differences analysis of the homicide rate on red and pink subsample

	(1)	(2)
VARIABLES	red and pink	red
DiD	20.23**	37.82*
	(9.368)	(16.82)
Constant	19.72***	17.31***
	(3.090)	(1.972)
Observations	7,120	2,230
R-squared	0.029	0.048
Number of muni	712	223
Municipality FE	YES	YES
Year FE	YES	YES
Cluster	Department	Department

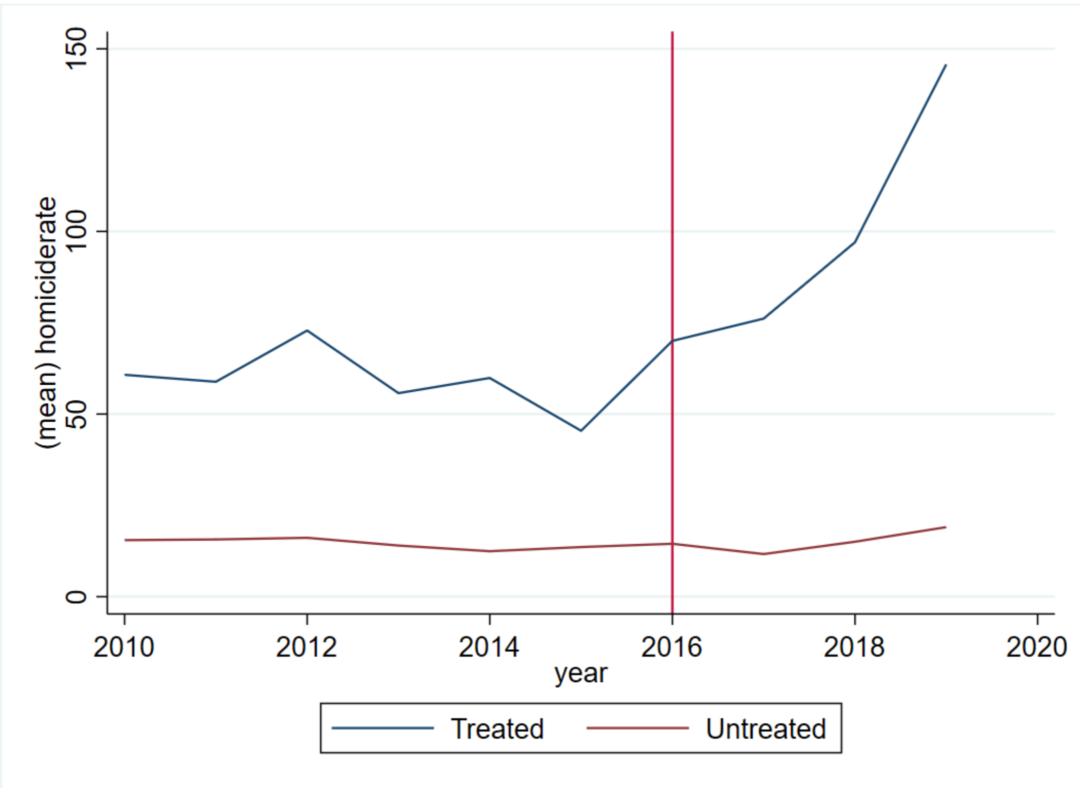
Cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Below I have graphed the trends, including the pre-trends for the homicide rate by treatment and control group for the red sample. Visually the graph does not show a parallel trend before the shock in 2016 (indicated by the red vertical line). The graph does show a change between the treatment and control groups after the shock in 2016. Whereas the mean homicide rate in the treatment group increases after the shock, the mean homicide rate in the control group is stable after the shock until 2018, where it increases a bit. The lack of similar trends can also explain why the DiD estimation was not significant; it did not have a prerequired parallel trend.

In the alternative graphical representation, as one can see in Appendix E, I have graphed the treatment and control groups using an alternative technique. The graph presents the change in the average homicide rate in the treatment and the control group from one year to another. One cannot see the evident change as in the main analysis. However, one can see that for the first years the change is small and from 2015 there is a more considerable positive change, though not statically significant.

The placebo test, as one can see in Appendix E, does not show any significant results. Yet, the visual representation in figure 11 still violates the required parallel trends.

Figure 11 Graphical representation of pre-trend for red sample

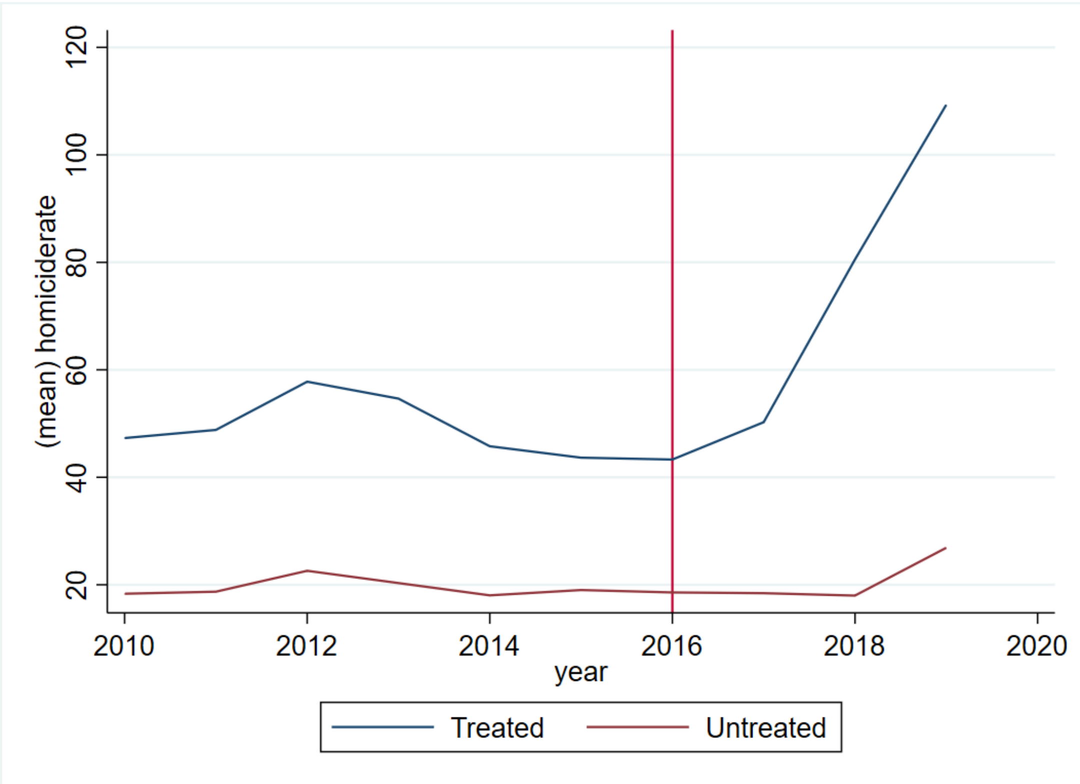


I then redo the same tests for the red and pink sample. Below I have graphed the trends, including the pre-trends for the homicide rate by treatment and control group for the red and pink sample (Figure 12). The pre-trends are quite similar to the main analysis, and they are parallel. They display, as in the main analysis, a jump in the treated sample after the shock in 2016. It is interesting to note that the sample that had a more similar pre-trend was also the sample with an effect.

In the alternative graphical representation, as one can see in Appendix E, I have graphed the treatment and control groups using an alternative technique. The graph presents the change in the average homicide rate in the treatment and the control group from one year to another, and one can see that for the first years, the change is small, and from 2016 there is a more considerable positive change, as in the main analysis.

The placebo test, as one can see in Appendix E, does not show any significant results.

Figure 12 Graphical representation of pre-trend for red and pink sample



8. Conclusion

In this thesis, I have studied the relationship between violence and cocaine production to investigate whether a positive shock to cocaine production leads to more violence. Using an exogenous price shock in the cocaine market, I have investigated the effect on violence in cocaine-producing areas. The price shock originates from a shock to the exchange rate between the currencies of Colombia and Venezuela, which in turn is caused by hyperinflation in Venezuela due to oil shock and poorly manipulation of exchange rates. This shock affects the price of an input into the cocaine production, the price of trafficked gasoline.

I have used a quasi-experimental research design to study the impact of the supply shock on violence. I am performing a Differences-in-differences (DiD) analysis between areas with high-intensity and low-intensity coca cultivation, assuming (and testing for) similar trends before and after the economic shock. I combine data on coca cultivation and homicides, two relatively reliable data sources in a field of research with many unknowns, and a general lack of information. The positive supply shock leads to more violence in coca producing areas compared to non-producing areas. The impact of the shock in the treatment group is an increase of 12.34 homicides per 100,000 inhabitants. Even for a violent country like Colombia, the number is quite high. The average homicide rate in the whole sample is about 26 homicides per 100,000 inhabitants, which implies that the effect of the supply shock is equivalent to a 50% increase in the number of murders in the average municipality. The results are robust to various tests, as controlling for immigration, excluding big cities and distance from the border.

The results indicate that when it becomes cheaper to produce cocaine, there is more violence in the production areas. Since violence and drug production are both highly unwanted, the implication should be to make sure that it does not become cheaper to produce cocaine. It also implies that the decriminalization of drug production would lead to more violence. However, legalization would still be an option because it would acquire an entirely new set of intuitions and regulations that could prevent violence. Still, "turning a blind eye" on drug production would not be productive. It also means that the government should, in the future, be watchful for price changes that could affect the production to avoid more unnecessary violence.

The thesis also highlights the underlying poor economic conditions for the people involved in the industry, such as the farmers. Improving the actual economic conditions for poor people would be a start to avoid such high levels of violence. If one could use some of the vast sums of money allocated to fighting drugs into the education system, more children could get a decent education and jobs with modest salaries. Fair salaries in legal activities would increase the opportunity cost and thus making fewer people prone to get into the illegal business, and thereby decrease the level of violence.

For future research, it would be interesting to study the peace agreement, which was another major phenomenon in Colombia that happened in 2016 (UNODC & Government of Colombia 2017). One could ask if there is any way the peace agreement could be the leading cause of more violence. It is clear that this is not the intention of the agreement; the intention is of course peace, the opposite of violence. However, the peace agreement might have some unintended consequences that could lead to more violence. One crucial factor of the peace agreement was that the FARC guerrillas had do give up the territories that they had used to produce coca and cocaine, and they did (UNODC & Government of Colombia 2017). The abandoning of territory might lead to violence in the competition over territories, either between the government and the illegal armed groups or between different illegal armed groups. Nevertheless, these potential fights over vacant sites cannot explain the substantial results for the complete analysis, as the FARC guerilla only occupied some of the counties. In the future, it would be interesting to study the regions occupied by FARC and its effects on violence.

Furthermore, I did not get access to geo-referenced data in time, so I used seizure data. It would be interesting, in the future, to check whether my findings are robust to this type of data.

It would also be useful to study the shifts in demand and supply from the corona crisis in the future. The corona crisis has shut down many countries, including Colombia, and the travel and commerce restriction makes it hard to transport drugs to consumer countries. Since there is a lag in time from cultivation and production to consumption (about two years from the cultivation of coca leaves in Colombia to consumption in the U.S.), it should be possible to study the different shifts in the market.

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Appendix

Appendix A

Table A.1 Differences-in- differences analysis of the effect of the effect of the price shock on homicide rates cluster at municipality level

VARIABLES	(1)	(2)	(3)	(4)
DiD	12.34***	12.34***	12.34***	12.34***
	(4.326)	(4.328)	(4.326)	(4.327)
Constant	23.46***	23.27***	25.43***	25.23***
	(0.711)	(0.928)	(0.246)	(0.701)
Observations	11,220	11,220	11,220	11,220
Number of muni	1,122	1,122	1,122	1,122
Municipality FE	NO	NO	YES	YES
Year FE	NO	YES	NO	YES
Cluster	Municipality	Municipality	Municipality	Municipality

Cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A. 2 Differences-in-differences analysis of the effect of the effect of the price shock on homicide rates with wild clustering

	without controls	some controls	some controls	Main result
DiD	12.33859***	12.33859**	12.33859**	12.33859**
	(1.932392)	(5.459306)	(5.461011)	(5.461254)
	[0.000]	[0.024]	[0.031]	[0.024]
		{0.04004004}		{0.03003003}
Observations	11220	11220	11220	11220
Municipality FE	NO	NO	YES	NO
Year FE	NO	NO	YES	YES
Cluster	NO	YES	YES	YES

Notes: I cluster at department level (clustered standard errors in (), clustered p-values in [], and wild clustered p-values in {}). *** p<0.01, ** p<0.05, * p<0.1

Table A. 3 Placebo tests with wild clustering

	placebo year 2011	placebo year 2012	placebo year 2013	placebo year 2014	placebo year 2015
DiD	0.692	1.251	-1.512	-4.330*	-5.440*
	(4.255)	(2.550)	(2.450)	(2.542)	(3.077)
	[0.872]	[0.627]	[0.542]	[0.098]	[0.087]
	{0.822}	{0.606}	{0.534}	{0.126}	{0.104}
Observations	6,732	6,732	6,732	6,732	6,732

Notes: I cluster at department level (clustered standard errors in (), clustered p-values in [], and wild clustered p-values in {}). Year fixed effects included. Municipality fixed effects included, but not for wild cluster

∗ $p < 0.10$.

∗∗ $p < 0.05$.

∗∗∗ $p < 0.01$.

Appendix B

Table B. 1 List of municipalities in the main treatment sample

List of municipalities in main treatment sample
Anorí
Apartadó
Arauquita
Balboa
Barbacoas
Bolívar (Cauca)
Bolívar (Santander)
Buenaventura
Cajibío
Cantagallo
Cartagena del Chairá
Convención
Corinto
Cumaribo
Cáceres
Dagua
El Charco
El Doncello
El Paujil
El Retorno
El Tambo
El Tarra
Francisco Pizarro
Ituango
Jamundí
La Llanada
La Macarena
La Tola
Landázuri
Mapiripán
Mercaderes
Miraflores
Montelíbano
Morales
Mutatá
Ocaña
Olaya Herrera
Orito
Páez

Policarpa
Puerto Asís
Puerto Caicedo
Puerto Concordia
Puerto Guzmán
Puerto Leguízamo
Puerto Rico(Caquetá)
Puerto Rico(Meta)
Ricaurte
Riosucio
Roberto Payán
Río de Oro
Samaniego
San Andres de Tumaco
San Francisco
San José del Guaviare
San José del Palmar
San Luis
San Miguel
San Pablo de Borbur
San Vicente del Caguán
Santa Bárbara
Santa Rosa del Sur
Santacruz
Sardinata
Tarazá
Teorama
Tibú
Tierralta
Timbiquí
Turbo
Uribe
Valdivia
Valle Del Guamuez
Villagarzón
Vista Hermosa
Yarumal

Table B. 2 List of municipalities in different treatment groups

List of municipalities in different treatment groups by municipality code			
Group 1	Group 2	Group 3	Group 4
5040	5040	5031	5031
5045	5045	5040	5040
5120	5107	5045	5045
5361	5120	5107	5107
5480	5134	5120	5120
5652	5172	5134	5134
5660	5250	5172	5154
5790	5361	5234	5172
5837	5480	5250	5234
5854	5495	5361	5250
5887	5585	5480	5284
13160	5628	5495	5315
13670	5652	5585	5361
13688	5660	5628	5380
18150	5790	5652	5425
18247	5837	5660	5475
18256	5854	5736	5480
18592	5887	5756	5495
18753	13160	5790	5585
19075	13458	5819	5591
19100	13670	5837	5628
19130	13688	5854	5649
19212	15572	5858	5652
19256	18150	5885	5660
19450	18247	5887	5679
19473	18256	5890	5736
19517	18410	13160	5756
19809	18592	13458	5790
20614	18610	13473	5819
23466	18753	13490	5837
23807	19001	13670	5854
27615	19022	13688	5858
27660	19050	13744	5885
50325	19075	15572	5887
50350	19100	18094	5890
50370	19110	18150	5893
50450	19130	18205	5895
50590	19212	18247	13006

50711	19256	18256	13030
52079	19318	18410	13042
52250	19450	18460	13160
52385	19473	18592	13458
52390	19517	18610	13473
52490	19532	18753	13490
52520	19533	18756	13600
52540	19622	19001	13654
52612	19780	19022	13667
52621	19809	19050	13670
52678	20614	19075	13683
52696	23466	19100	13688
52699	23682	19110	13744
52835	23807	19130	13810
54206	23855	19142	15572
54250	27361	19212	15681
54498	27413	19256	17662
54720	27430	19318	18001
54800	27615	19355	18094
54810	27660	19364	18150
68101	27800	19418	18205
68385	50325	19450	18247
76109	50330	19473	18256
76233	50350	19517	18410
76364	50370	19532	18460
81065	50400	19533	18592
86320	50450	19548	18610
86568	50590	19622	18753
86569	50683	19693	18756
86571	50711	19698	19001
86573	52079	19780	19022
86757	52250	19807	19050
86865	52256	19809	19075
86885	52385	19821	19100
95001	52390	20011	19110
95025	52399	20178	19130
95200	52427	20614	19137
99773	52473	20770	19142
	52490	23466	19212
	52520	23682	19256
	52540	23807	19290
	52612	23855	19318
	52621	27025	19355
	52678	27077	19364

	52696	27361	19392
	52699	27413	19397
	52835	27430	19418
	54001	27615	19450
	54003	27660	19455
	54128	27800	19473
	54206	27810	19517
	54245	47001	19532
	54250	50325	19533
	54344	50330	19548
	54385	50350	19622
	54498	50370	19693
	54670	50400	19698
	54720	50450	19743
	54800	50590	19780
	54810	50683	19807
	68101	50711	19809
	68250	52036	19821
	68385	52079	20011
	68773	52227	20013
	76109	52233	20178
	76233	52250	20310
	76364	52256	20550
	81065	52260	20614
	86320	52385	20621
	86568	52390	20710
	86569	52399	20770
	86571	52411	23466
	86573	52427	23580
	86757	52435	23682
	86865	52473	23807
	86885	52490	23855
	95001	52520	27025
	95015	52540	27077
	95025	52612	27150
	95200	52621	27250
	99773	52678	27361
		52696	27413
		52699	27425
		52835	27430
		54001	27450
		54003	27491
		54128	27580
		54206	27615

54245	27660
54250	27745
54261	27800
54344	27810
54385	41001
54498	41006
54553	47001
54670	50251
54720	50325
54800	50330
54810	50350
68101	50370
68190	50400
68250	50450
68255	50568
68385	50577
68573	50590
68615	50683
68773	50689
68861	50711
76109	52036
76126	52079
76233	52227
76364	52233
76834	52240
81065	52250
86001	52254
86320	52256
86568	52260
86569	52356
86571	52385
86573	52390
86757	52399
86865	52411
86885	52418
95001	52427
95015	52435
95025	52473
95200	52490
97161	52520
99001	52540
99624	52573
99773	52612
	52621

52678
52687
52696
52699
52786
52835
54001
54003
54128
54206
54245
54250
54261
54344
54385
54498
54553
54670
54720
54800
54810
66572
68101
68190
68250
68255
68385
68397
68573
68615
68720
68745
68773
68861
70265
73168
76100
76109
76126
76233
76250
76275
76364
76670

	76834
	81065
	86001
	86320
	86568
	86569
	86571
	86573
	86757
	86865
	86885
	91001
	94001
	94663
	95001
	95015
	95025
	95200
	97161
	99001
	99524
	99624
	99773

Appendix C

Table C. 1 Top 60 municipalities in Colombia after number of immigrants from Venezuela

Municipalities	number of immigrants
Bogotá,D,C,	357667
Cúcuta	93461
Barranquilla	86918
Medellín	86201
Cali	55884
Maicao	44251
Riohacha	42278
Cartagena de Indias	40798
Bucaramanga	37094
Santa Marta	35166
Valledupar	29165
Villa del Rosario	28147
Soacha	25159
Soledad	23589
Arauca	17187
Pereira	12156
Bello	11812
Yopal	10732
Floridablanca	10721
San Juan del Cesar	1036
Fonseca	1013
Ciénaga	10128
Sincelejo	9130
Saravena	8927
Chía	7800
Itagüi	6940
Armenia	5974
Rionegro	5930

Envigado	5849
Fundación	5810
Tibú	5656
Pamplona	5644
Piedecuesta	5236
Los Patios	5144
Puerto Colombia	5113
Palmira	4895
Uribia	4746
Facatativa	4724
Chinácota	4659
Inírida	4560
Jamundí	4541
Barrancabermeja	4534
Villavicencio	4453
Zipaquirá	4418
Ibagué	4416
Mosquera	4157
Tunja	4063
El Banco	3912
Ocaña	3889
Manizales	3885
Dosquebradas	3845
Montería	3845
Magangué	3821
Malambo	3691
Arauquita	3585
Madrid	3504
Girón	3403
Barrancas	3113
Sabanalarga	2951
Cajicá	2748

Placebo test for restricted samples by of immigration from Venezuela

Table C. 2 Differences-in- differences placebo-estimation restricted sample 1

VARIABLES	(1) placebo year 2011	(2) placebo year 2012	(3) placebo year 2013	(4) placebo year 2014	(5) placebo year 2015
DiD	0.646	1.185	-1.576	-4.389*	-5.481*
	(4.240)	(2.530)	(2.433)	(2.530)	(3.071)
Constant	25.10***	25.10***	25.10***	25.10***	25.10***
	(1.638)	(1.638)	(1.640)	(1.641)	(1.640)
Observations	6,684	6,684	6,684	6,684	6,684
R-squared	0.012	0.012	0.012	0.013	0.013
Number of muni	1,114	1,114	1,114	1,114	1,114
Municipality FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

Clustered robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table C. 3 Differences-in- differences placebo-estimation restricted sample 2

VARIABLES	(1) placebo year 2011	(2) placebo year 2012	(3) placebo year 2013	(4) placebo year 2014	(5) placebo year 2015
DiD	0.619	1.133	-1.591	-4.405*	-5.517*
	(4.229)	(2.519)	(2.425)	(2.529)	(3.069)
Constant	25.08***	25.08***	25.08***	25.08***	25.08***
	(1.656)	(1.655)	(1.658)	(1.659)	(1.658)
Observations	6,600	6,600	6,600	6,600	6,600
R-squared	0.012	0.012	0.012	0.013	0.013
Number of muni	1,100	1,100	1,100	1,100	1,100
Municipality FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

Clustered robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table C. 4 Differences-in- differences placebo-estimation restricted sample 3

VARIABLES	(1) placebo year 2011	(2) placebo year 2012	(3) placebo year 2013	(4) placebo year 2014	(5) placebo year 2015
DiD	1.032	1.058	-1.395	-4.614	-5.234
	(4.419)	(2.706)	(2.600)	(2.729)	(3.220)
Constant	24.83***	24.83***	24.83***	24.83***	24.83***
	(1.720)	(1.720)	(1.723)	(1.725)	(1.723)
Observations	6,396	6,396	6,396	6,396	6,396
R-squared	0.012	0.012	0.012	0.012	0.012
Number of muni	1,066	1,066	1,066	1,066	1,066
Municipality FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

Clustered robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Alternative graphical representations

Figure C. 1 Change in the average homicide rate in the treatment and the control group restricted sample 1

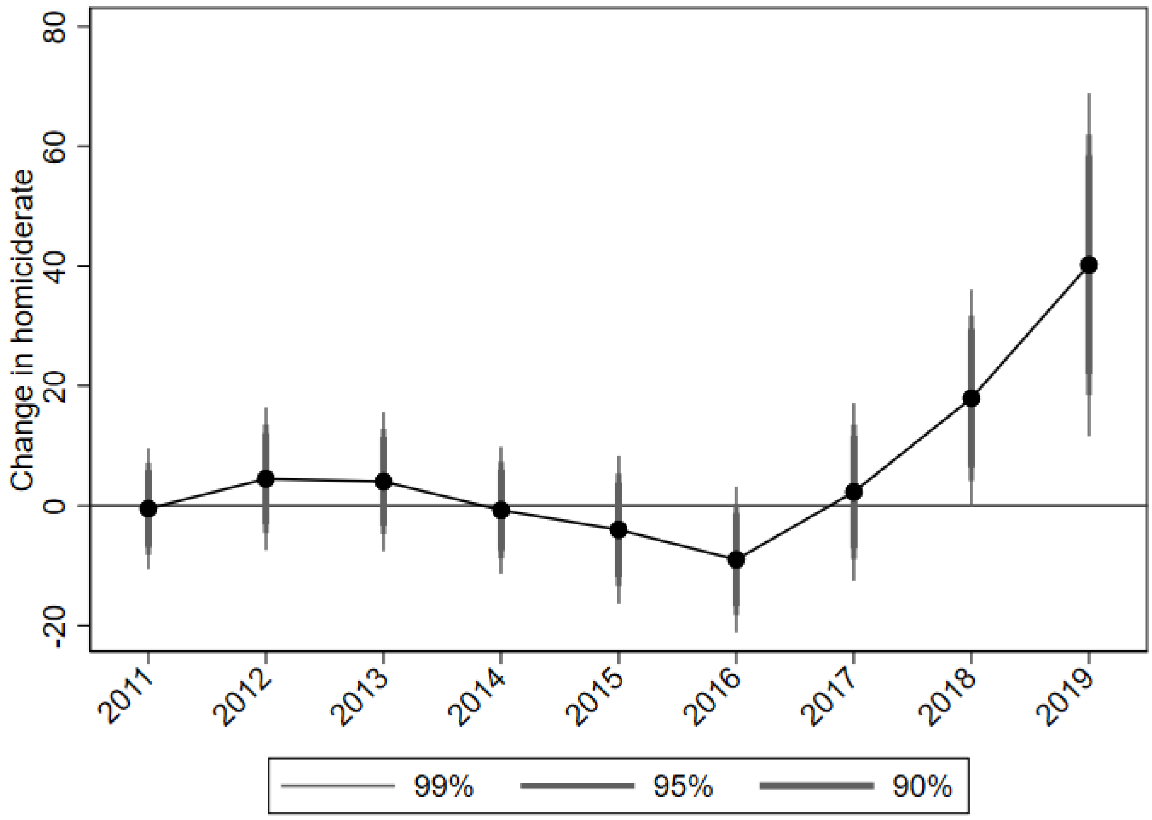


Figure C. 2 Change in the average homicide rate in the treatment and the control group restricted sample 2

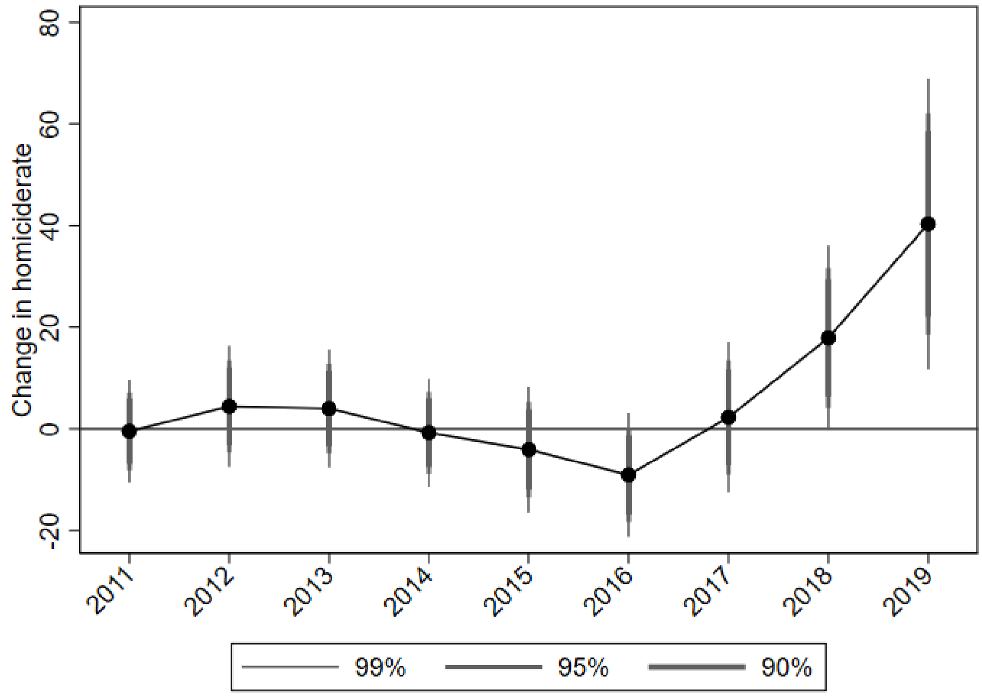
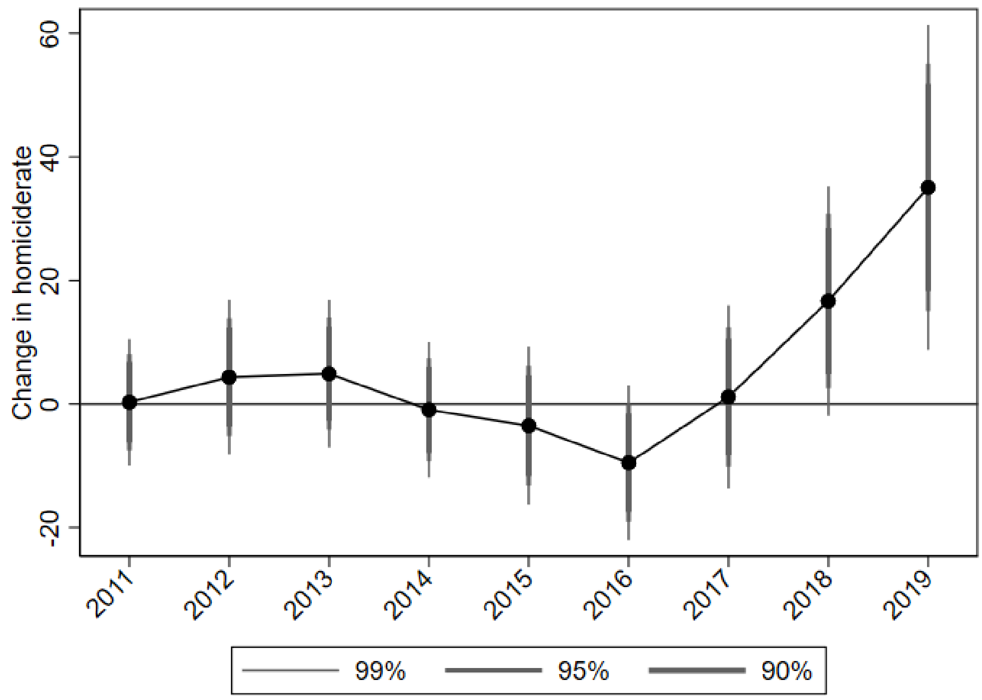


Figure C. 3 Change in the average homicide rate in the treatment and the control group restricted sample 3



Appendix D

Table D. 1 Differences-in- differences placebo-estimation group 2

VARIABLES	(1) placebo year 2011	(2) placebo year 2012	(3) placebo year 2013	(4) placebo year 2014	(5) placebo year 2015
DiD	-0.649	0.200	-0.741	-2.258	-2.498
	(2.627)	(1.998)	(2.175)	(2.181)	(3.527)
Constant	25.23***	25.23***	25.23***	25.23***	25.23***
	(1.619)	(1.616)	(1.618)	(1.620)	(1.619)
Observations	6,732	6,732	6,732	6,732	6,732
R-squared	0.012	0.012	0.012	0.012	0.012
Number of muni	1,122	1,122	1,122	1,122	1,122
Municipality FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Cluster	Department	Department	Department	Department	Department

Clustered robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D. 2 Differences-in- differences placebo-estimation group 3

VARIABLES	(1) placebo year 2011	(2) placebo year 2012	(3) placebo year 2013	(4) placebo year 2014	(5) placebo year 2015
DiD	-0.206	0.358	-3.391**	-4.708**	-4.959*
	(2.704)	(2.751)	(1.573)	(1.833)	(2.852)
Constant	25.23***	25.23***	25.23***	25.23***	25.23***
	(1.618)	(1.616)	(1.628)	(1.627)	(1.622)
Observations	6,732	6,732	6,732	6,732	6,732
R-squared	0.012	0.012	0.013	0.014	0.013
Number of muni	1,122	1,122	1,122	1,122	1,122
Municipality FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Cluster	Department	Department	Department	Department	Department

Clustered robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D. 3 Differences-in- differences placebo-estimation group 4

VARIABLES	(1) placebo year 2011	(2) placebo year 2012	(3) placebo year 2013	(4) placebo year 2014	(5) placebo year 2015
DiD	0.896	0.680	-3.042**	-4.645***	-5.276*
	(1.896)	(1.974)	(1.279)	(1.544)	(2.709)
Constant	25.23***	25.23***	25.23***	25.23***	25.23***
	(1.610)	(1.613)	(1.633)	(1.634)	(1.626)
Observations	6,732	6,732	6,732	6,732	6,732
R-squared	0.012	0.012	0.013	0.014	0.014
Number of muni	1,122	1,122	1,122	1,122	1,122
Municipality FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Cluster	Department	Department	Department	Department	Department

Clustered robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Alternative graphical representations

Figure D. 1 Change in the average homicide rate in the treatment and the control group using group 2

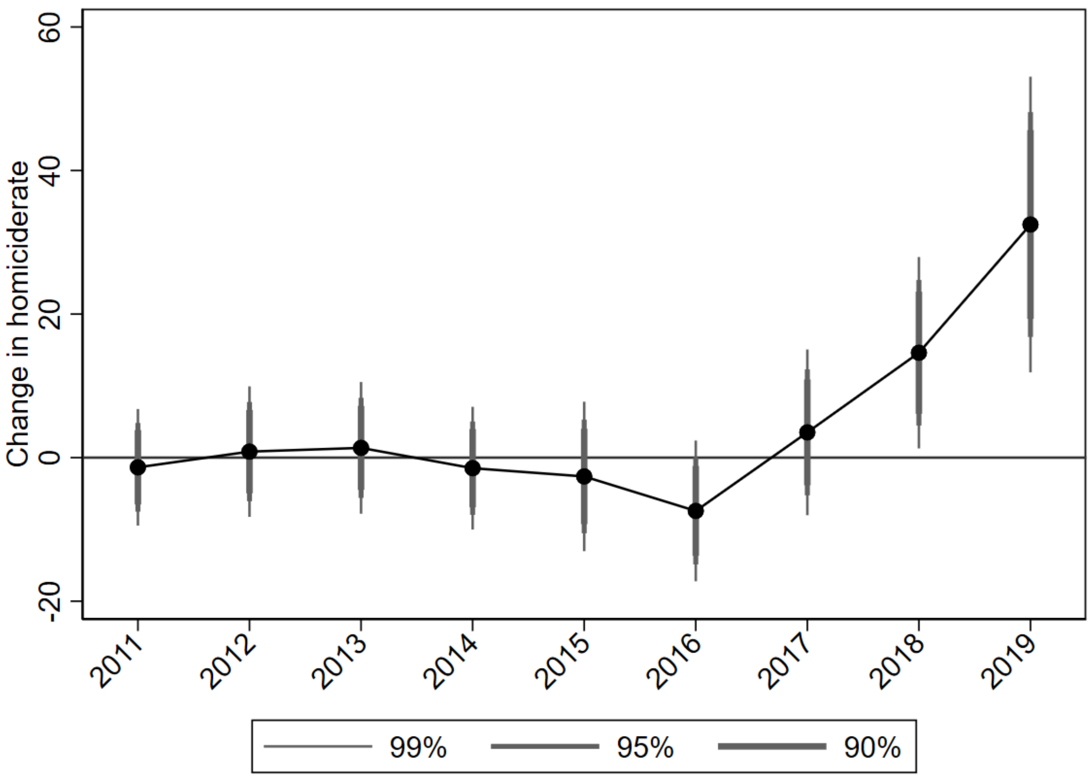


Figure D. 2 Change in the average homicide rate in the treatment and the control group using group 3

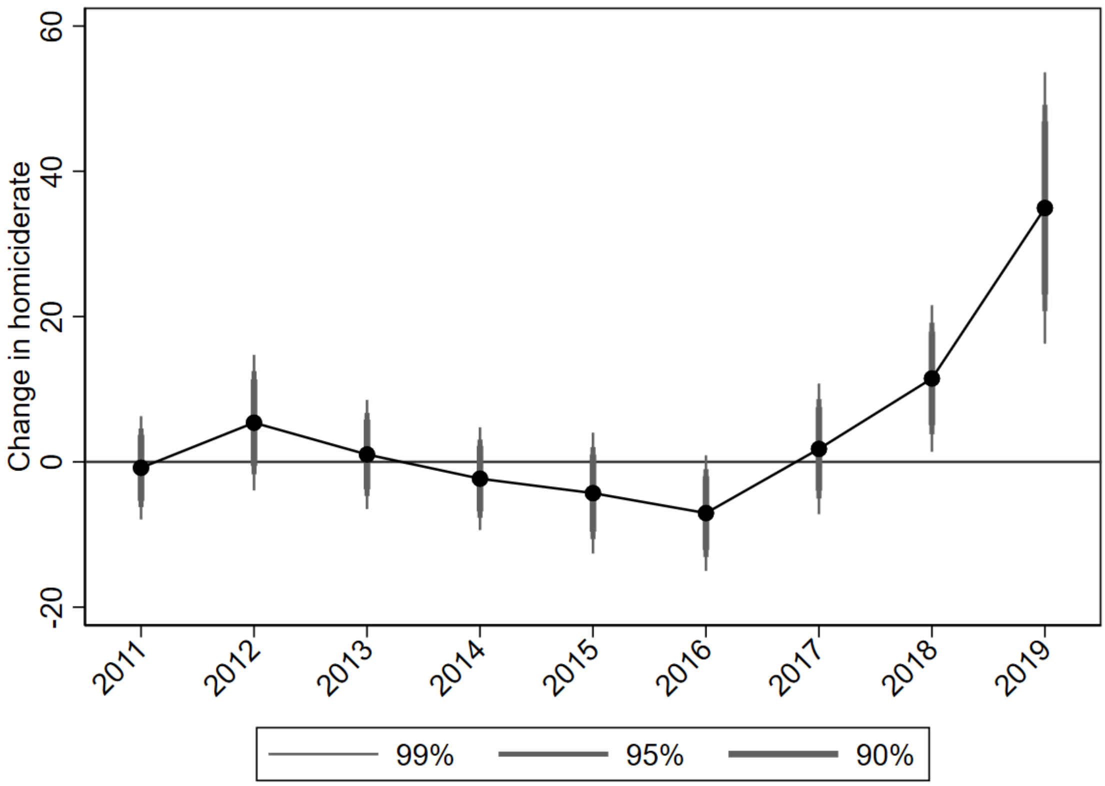
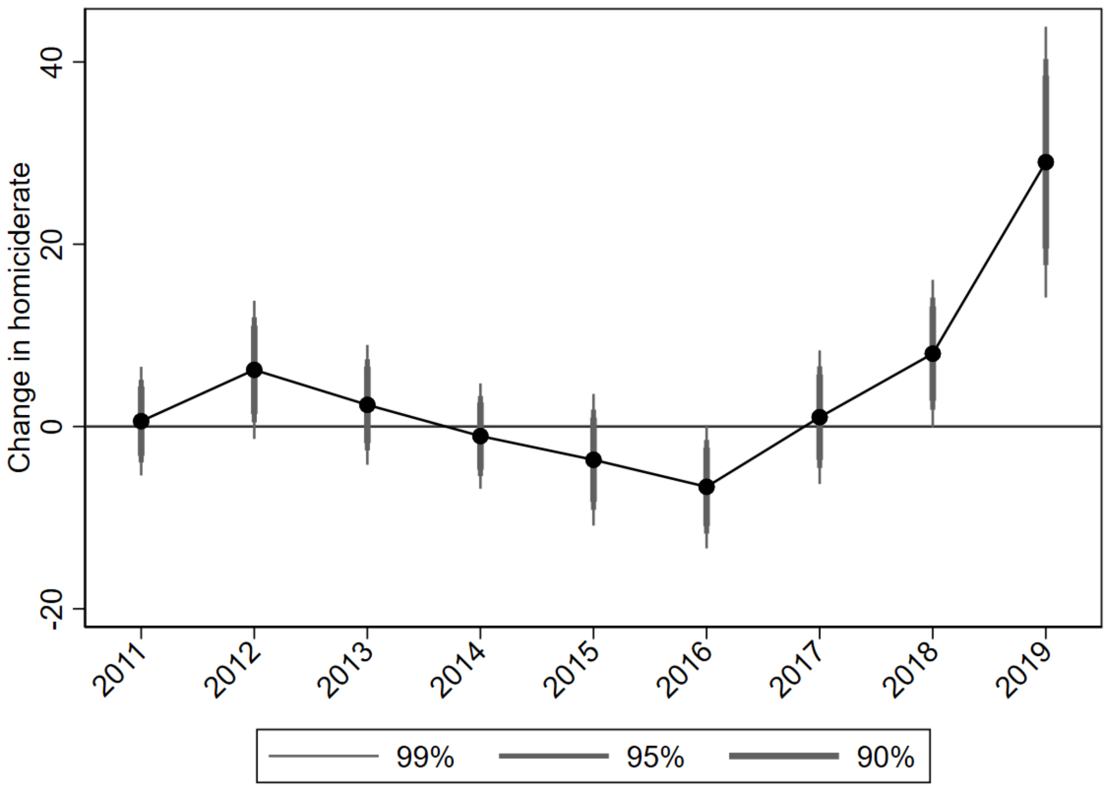


Figure D.3 Change in the average homicide rate in the treatment and the control group using group 4



Appendix E

Table E. 1 Differences-in- differences analysis of the homicide rate on red and pink subsample

	red and pink	red
DiD	20.23**	37.82*
	(9.368)	(16.82)
	[0.046]	[0.066]
	{0 .12}	{0.232}
Observations	7120	2230

Notes: I cluster at department level (clustered standard errors in (), clustered p-values in [], and wild clustered p-values in {}). Year fixed effects included. Municipality fixed effects included, but not for wild cluster

* $p < 0.10$.

** $p < 0.05$.

*** $p < 0.01$.

Placebo test for restricted samples by proximity to border

Table E. 2 Differences-in-differences placebo-estimation on the red subsample

VARIABLES	(1) placebo year 2011	(2) placebo year 2012	(3) placebo year 2013	(4) placebo year 2014	(5) placebo year 2015
DiD	15.81	19.09*	15.85	21.27	25.24
	(13.47)	(9.024)	(10.89)	(12.59)	(13.34)
Constant	17.31***	17.31***	17.31***	17.31***	17.31***
	(1.971)	(1.972)	(1.973)	(1.971)	(1.971)
Observations	2,230	2,230	2,230	2,230	2,230
R-squared	0.019	0.023	0.022	0.027	0.032
Number of muni	223	223	223	223	223
Municipality FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Cluster	Department	Department	Department	Department	Department

Clustered robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table E. 3 Differences-in-differences placebo-estimation on red and pink subsample

VARIABLES	(1) placebo year 2011	(2) placebo year 2012	(3) placebo year 2013	(4) placebo year 2014	(5) placebo year 2015
DiD	10.32	10.90	9.762	10.18	13.97*
	(10.67)	(8.167)	(7.474)	(7.572)	(7.783)
Constant	19.72***	19.72***	19.72***	19.72***	19.72***
	(3.079)	(3.084)	(3.101)	(3.108)	(3.100)
Observations	7,120	7,120	7,120	7,120	7,120
R-squared	0.021	0.022	0.022	0.022	0.024
Number of muni	712	712	712	712	712
Municipality FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Cluster	Department	Department	Department	Department	Department

Clustered robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure E. 1 Alternative graphical representation for red subsample

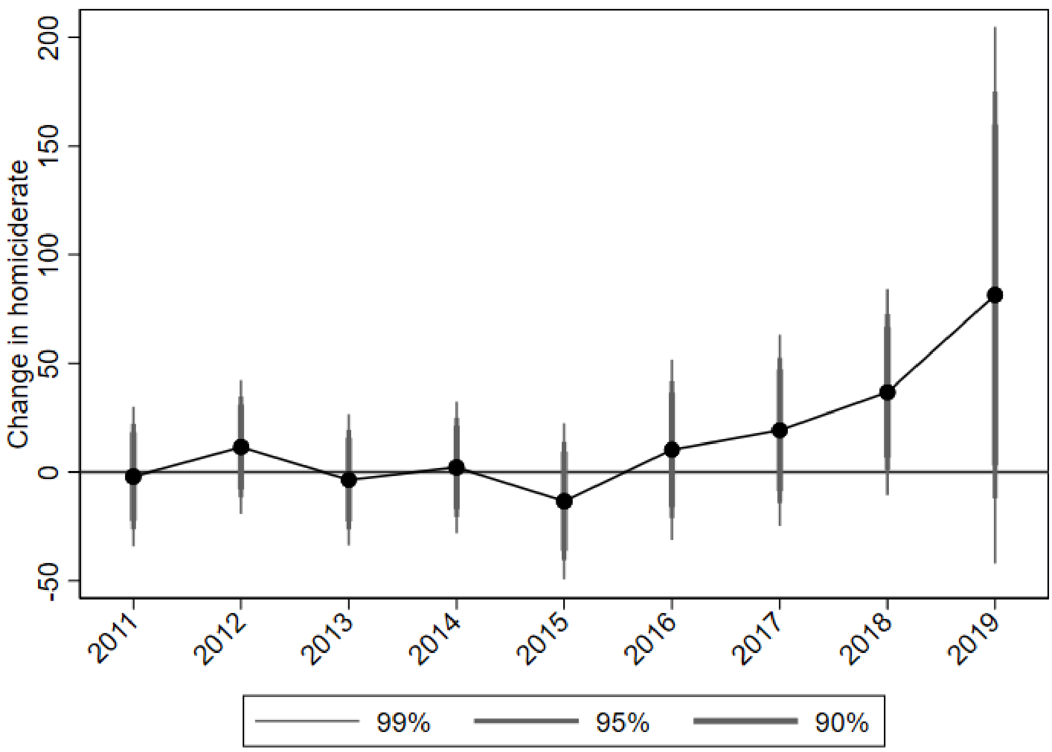


Figure E. 2 Alternative graphical representation for red and pink subsample