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Essays on Crime, Unemployment and Health

By

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Abstract

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Doctor of Philosophy in Agricultural and Resource Economics

University of California, Berkley

Professor Jeremy Magruder, Chair

This dissertation is composed of three chapters and studies issues related to crime, unemployment and health. The first chapter looks at the effect of funding for public safety on drug related violence. The second chapter, which is joint work with Santiago Guerrero, examines the effect of unemployment on crime during the latest great recession. The third chapter examines the effect that the introduction of ultra-low sulfur diesel has had on infant mortality.

The first paper develops a simple framework to describe the effect of increases in fighting capacity on violence and uses a large program in Mexico to empirically estimate the effect of funding for public safety on violence, specifically drug related violence. Starting in 2008, Mexico implemented a large program designed for the strengthening of the municipal police, the assignment of which was based on an index. The main areas of allowed expenditures for these funds were: the purchase of fighting equipment, technology infrastructure and training of the police force. Instrumenting funding with the arbitrary initial eligibility cutoff, I find that the funds led to large increases in drug related violence. Evidence is consistent with the funds allowing the police to fight criminal organizations which weakened organizations and in turn led to turf wars. The effect is not higher for PAN municipalities, the party whose main platform during the study period was to fight organized crime. Also, there does not seem to be an increase in violence in politically stable municipalities as a result of the program, but there is a decrease in areas with low land productivity. Consistent with theory, I also find suggestive evidence of an inverted U-shaped relationship between baseline funding for public safety and the effect of the program.

The second paper estimates the effect of unemployment on crime in Mexico. This study uses the variation in unemployment across metropolitan areas in Mexico induced by the latest great recession. Areas that were highly dependent on the US economy experienced the largest increases in unemployment, thus we instrument unemployment with the initial manufacturing and tourism labor share interacted with US GDP and find that increases in unemployment have led to decreases in crime in Mexico. The results are consistent with the decrease in potential targets due to the increases in unemployment outweighing the positive effect coming from the decrease in the opportunity cost of engaging in criminal activities as unemployment increases.

The third paper estimates the effect of the introduction of ultra-low sulfur diesel on infant health in Mexico. In 2006 the Mexican government began the rollout of ultra-low sulfur diesel in metropolitan areas, starting with border municipalities. Using a difference in differences approach, I find that, despite its potential to improve health outcomes, there is no evidence that sulfur regulation had a substantial effect on infant mortality outcomes.

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While at Berkeley I was very fortunate to meet wonderful people that have provided insight and long lasting friendship. I would like to thank my co-author Santiago Guerrero, whose insights helped me broaden my understanding of economics and provided invaluable contribution and support. Also, I would like to thank Manuel Barron, Kyle Emerick, Lydia Ashton and many others for their friendship, support and feedback. Also, thank you to Carmen Karahalios and Diana Lazo.

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Chapter 1

More Police Funding, More Violence? Regression Discontinuity Evidence

ABSTRACT

A lack of capacity to fight crime has often been cited as one of the reasons for the high levels of crime in developing countries, however there is little evidence on the effect of increased funding for public safety on violence. The main agenda of Mexico's Calderon administration (2006-2012) from the PAN party was to combat organized crime. To this end, one of the largest programs implemented during his administration was the Subsidy for Municipal Public Safety (SUMSEMUM), where municipalities were assigned funds for public safety expenditures destined for the strengthening of the municipal police based on an index. Estimates from a regression discontinuity design show that an additional year of funding led to more than a 100% increase in drug related homicides relative to baseline. Empirical results are consistent with the funds allowing municipalities to fight criminal organizations, which led to the weakening of organizations and subsequently to turf wars. Also, consistent with theory, there is suggestive evidence of an inverted U-shaped relationship between baseline funding for public safety and the effect of the program. There is no evidence that the effect is larger for PAN governed municipalities. Lastly, political stability and land productivity are important determinants of the effect of the program.

1.1 Introduction

One of the largest and most ambitious programs of the Calderon administration (2006-2012) aimed at the improvement of public safety infrastructure and increasing the municipal government's capacity to fight crime in Mexico was the Subsidy for Municipal Public Safety (SUMSEMUN). Despite being one of the most important programs during the Calderon administration, with total expenditures of almost 2 billion dollars over a five year period and representing an increase of approximately 28% in funding for public safety expenditures for the average municipality in the first year of the program, little is known of what its effects on violence have been. Moreover, there is little

empirical evidence regarding the effect of funds for public safety expenditures on violence. One of the main goals of the Calderon administration was to increase the state capacity to fight criminal organizations via stronger and more equipped law enforcement agencies (Escalante, 2012). To this aim, it implemented the SUBSEMUN program starting in 2008, a program that has been kept in place by the new administration. The funds were given to municipalities based on a compound index; in order to assure that every state had at least two funded municipalities, in the first year the rules of the program were such that the two municipalities with the highest index in each state were eligible for funding and then the ones in the country with the highest index up to when the funds were exhausted. The funds were designed to strengthen the municipal police and had four main areas of allowed expenditures: the purchase of fighting equipment, such as guns and police cars, public safety infrastructure, such as technology infrastructure, as well as the overall improvement of the municipal police force via training and also police salary increases in later years. Although there were four areas of allowed expenditures, audit reports indicate that the majority of the funds were used for fighting equipment and public safety infrastructure. Taking advantage of the arbitrary initial eligibility threshold based on the index, this study uses a fuzzy regression discontinuity design to estimate the impact of funds for public safety on violence in Mexico and finds that increases in funding has led to large increases in violence.

An often cited reason for conflict has been a lack of state capacity to fight crime; however, this study shows that increasing state capacity, through an increase in funds for public safety, can lead to increases in violence where organized crime coexists, at least in the short and medium run. One of the explanations given for the high levels of conflict in low income countries is that financially weak governments are unable to finance a strong police to contain violence (Fearon and Laitin, 2003). However, it is not clear whether increasing the capacity of the state to fight crime, via a stronger and more equipped police force, would lead to a decrease in crime, especially in the case where large criminal organizations with large financial capacity operate. If the state and criminal organizations are thought as parties fighting for the control of territories, as is the case where drug trafficking organizations fight for trafficking routs and local markets, a contest model where fighting resources are an input into the contest success function predicts that increases in the government's fighting inputs, holding the criminal organizations' inputs fixed, increases the winning probability for the government and hence the probability that the government engages in fighting (Tullock, 1980; Hirshleifer, 1989; Garfinkel and Skapeardas, 2007). While increases in fighting resources increase the probability of winning for the government, whether this increased funding leads to actual decreases or increases in violence depends on the initial funding level of the police as well as the magnitude of the increase. For instance, small increases in funding to underfunded police forces might not induce the police to fight criminal organizations unless the increase is large enough to substantially increase their probability of winning to offset the costs of fighting; in such a case, and if the increase is not so large such that criminal organizations do not fight back and decrease their criminal activity, we would expect to see an increase in confrontations between the police and criminal organizations. On the other hand if the police force and criminal organizations are initially engaging in fighting, an increase in funding might lead to a decrease in violence if it makes the police strong enough such that the criminals no longer fight the police and decrease their criminal activity.

Moreover, the decision of the police to fight criminal organizations might lead to the amplification of violence since weakening of an organization can lead to both within organization fighting and fighting between organizations for the control of the organization or of the territory. Dell (2012) uses a regression discontinuity design and finds that after a PAN win, that is the party associated with Calderon, a municipality experiences an increase in violence, which is consistent with PAN crackdowns weakening the incumbent organization and hence causing the rival organizations to fight for the

territory while the incumbent is weak. While her study shows that political outcomes are important determinants of violence in Mexico, the question of the effect of funding for public safety in general remains to be answered. In contrast to her study, this study specifically assesses the impact of large increases in funding for public safety expenditures and, while it might be thought that PAN municipalities would be the ones to make the most use of the funds and thus the ones that experience the largest increases in violence due to the program compared to non-PAN municipalities, there is no evidence of this.

This study expands on the crime and conflict literature and is one of the few studies to look directly at the effect of funding for public safety on violence. Studies that look at the effect of resources on conflict find that negative income shocks, an increase in the value of appropriable resources and aid all lead to an increase in conflict (Miguel et al, 2004; Dube and Vargas, 2011; Crost et al, 2014). In our case, territory could be thought of as the resource for which both the government and criminal organizations are fighting for. In terms of state capacity, most of the studies are on the effect of police on crime and find a negative effect; that is, increases in policing lead to decreases in crime (Levitt, 1997; Di Tella and Schargrodsky, 2004; Klick and Tabarrok, 2005; Evans and Owens, 2007; Chalfin and McCrary, 2013). These studies take place in contexts where large criminal organizations do not exist, the effect might be very different in contexts where criminal organizations with large financial capacity exist; also the effect of increased policing could be different to the effect of giving funds for arms and fighting equipment to the existing police force. In the context where organized crime operates, the effect of increasing the state's capacity to fight crime is ambiguous. The effect of such a program depends on the strength of the police force relative to criminal organizations.

Using a fuzzy regression discontinuity design, this study finds that increasing funds for public safety has led to large increases in violence in Mexico. Funding led to large increases in homicides, particularly in homicides of individuals without high school and public safety workers. Approximately 67% of the increase in homicides can be directly attributed to drug related homicides. If we look at the effect on homicides classified as being drug related, the study finds that an additional year of funding led to an increase in drug related homicides of more than 100%. Consistent with funding allowing municipalities to fight criminal organizations and criminal organizations fighting back, this study finds that funding led to increases in both the extensive and intensive margin of homicides resulting from clashes and homicides resulting from attacks to the government. Moreover the program did not increase the probability of there being a drug related homicide resulting from an execution; thus it seems that the program, by both increasing the intensity and by leading to the offset of violence between criminal organizations and public safety personnel, intensified drug related violence between criminal organizations. Also, consistent with funding being the most important for municipalities with mid-levels of initial funding, this study finds suggestive evidence of an inverted U-shaped relationship between the initial level of funding for public safety and the effect of the program. Although it might be expected that PAN municipalities would be more likely to make use of the funds, and thus to experience a larger increase in violence, there is no evidence of this. Consistent with low land productivity areas not being as valuable and thus not as contestable by criminal organizations, I find that in those areas the program led to decreases in violence. The program does not seem to increase violence in municipalities that had a history of political stability. Finally, if we assume that the effect of the program is the same for all municipalities that received funding, approximately 10% of the total drug related homicides in funded municipalities during the 2008-2010 period can be attributed to the program. The following section presents a simple setup of the decision to fight, Section 3 presents the details of the SUSBEMUN program, Section 4 describes the data, Section 5 presents the empirical strategy, Section 6 presents the results, Section 7 presents the robustness checks and Section 8 concludes.

1.2 The Decision to Fight

In this section I present a very simple setup for the decision to fight. This setup is very similar to the prisoner's dilemma games used in the conflict literature (Axelrod, 1984; Axelrod and Keohane, 1986; Fearon, 1998; Oye, 1986). Assume a static setup where two players simultaneously decide whether to fight or not. Assume that costs are private information. A player who chooses not to fight while the other decides to fight suffers a loss of $-l_i$ for each $i = \{1,2\}$. Let the costs of fighting for player i while the other player also fights be denoted by C_i^1 and the costs of fighting for player i while the other does not fight be $C_i^2 < C_i^1$, that is the costs incurred if player 1 fights are larger if player 2 also decides to fight than if player 2 does not fight back. If one party decides to fight while the other party does not fight, the party that initiates fighting wins with certainty and incurs a cost C_i^2, while the other party incurs a loss $-l_i$. Let V_i be the value of winning for player i and if both parties simultaneously decide to fight let p be the probability of winning for player 1 and (1-p) for player 2. The contest success function is given by $p = f(I_1, I_2)$, where I_1 and I_2 are fighting inputs of party 1 and 2 respectively and is increasing in I_1 and decreasing in I_2. Furthermore, assume that if neither party fights there exist payoffs Q_1 and Q_2 for party 1 and party 2 respectively, such that $V_1 - C_1^2 > Q_1 > pV_1 - C_1^1$ and $V_2 - C_2^2 > Q_2 > (1-p)V_2 - C_2^1$. For instance, one such payoff could be $Q_1 = pV_1 - C_1^2$ and $Q_2 = (1-p)V_2 - C_2^2$. The payoff matrix is as shown in panel A of figure 1.1.

If $E[V_i^F] > V_i^{NF}$ for both individuals then fighting is a dominant strategy for both parties. The probability threshold at which each party fights, and thus where (F,F) is a pure strategy Nash equilibrium, is given by:

$pV_1\text{-}C_1^1 > -l_1 \rightarrow p > \frac{C_1^1 - l_1}{V_1}$ for player 1 (1)

$(1\text{-}p)V_2\text{-}C_2^1 > -l_2 \rightarrow p < \frac{V_2 + l_2 - C_2^1}{V_2}$ for player 2 (2)

The above two conditions give the reaction functions shown in panel B of figure 1.1. Given that the probability of winning is a function of inputs, exogenously increasing inputs for player 1 while holding inputs for player 2 constant increase the probability of winning for player 1 and decreases the probability of player 2. This very simple static setup gives the following predictions that I will test in the empirical section:

1. If we assume that violence increases when both parties simultaneously decide to fight, we see from panel B of figure 1.1 that if inputs for player 1 are exogenously increased while holding inputs for player 2 fixed, violence might increase or decrease depending on the initial level of inputs for player 1 and the magnitude of the increase. In such a setup the highest increase in violence occurs when the increase in inputs for player 1 takes player 1 from the first region, where it is not willing to fight, to the middle region where both player 1 and player 2 decide to fight. This is more likely to occur if player 1 has a mid-level of initial resources/funding.
2. The lower the value of winning for player i the less likely it is that party i decides to fight.
3. If the model is extended to an infinitely repeated game with a probability of ending, it is easy to show that if δ is the probability that the game ends at any given

period, the lower the probability that the game ends the more likely it is that the Pareto optimal (NF, NF) equilibrium is sustained.

In the setting of the program and its effects on violence, player 1 could be thought of as the police force that experiences an increase in fighting resources and player 2 as a criminal organization. A similar game is played by criminal organizations; the weakening of organizations resulting from clashes with the police decreases the fighting resources of an organization and thus increases the probability of winning for other organizations or by members of the same organization that has been fractured. While this is a very simple model, assuming a static framework could be appropriate in a setting such as the one in this study, where interactions are relatively short lived since municipal governments are elected every three years and there is no reelection. Moreover the assumption that there is an exogenous increase in fighting inputs might be appropriate if the police force would not have otherwise acquired this fighting equipment.

1.3 The Program

This article studies the impact of increased funding for public safety in Mexico on violence. Central to the agenda of the Calderon administration was to combat crime, particularly organized crime, and to strengthen law enforcement agencies. To this end it designed the Subsidy for Public Safety (SUBSEMUN by its Spanish acronym), administered by the Executive Secretariat of the National Public Security System (SESNSP), a program by which funds were awarded to municipalities for the strengthening of the municipal police via better fighting equipment and technology. It was implemented starting in 2008; over the 2008-2012 period almost 2 billion dollars were allocated to it. The funds represented an increase of approximately 28% in funding for public safety expenditures for the average municipality in the first year of the program. Given that municipalities that were initially eligible for funding were more likely to receive funding in later years, after three years they had almost 90% more funding relative to the 2007 baseline than municipalities that never received funding, making the SUBSEMUN an important component of funding for public safety for eligible municipalities.

A distinguishing feature of this program was that it had designated areas of allowed expenditures. Municipalities in Mexico rely mainly on funding from the federal government with over 80% of their funding coming from federal transfers; approximately 50% of these federal funds are for designated spending, such as health, education and public safety (SEGOB, 2011). The SUBSEMUN had four main areas of allowed expenditures and these were: expenditures in fighting equipment, such as arms, vests and police cars, expenditures in security infrastructure, which is defined mainly as expenditures in technology infrastructure, training of the police force and starting in 2010 the equalization of police salaries across participating municipalities. According to audit reports, in audited municipalities the majority of the funds were spent on fighting equipment and technology infrastructure; the audit reports show that in 2008 and 2009 audited municipalities spent on average 78% of the funds on fighting equipment and 19% on infrastructure, in 2010 the average was 57% and 20% respectively and 18% going to salary increases for the police. In 2008 there were 150 funded municipalities, the number increased to 206 in 2009 and 2010 as shown in table 1.1.

1.3.1 Program Eligibility

Eligibility for the program was based on an index. Broadly defined, this index was based on the number of registered suspects, homicides and population. In 2008 the index was calculated as the average of the mean registered suspects, which are individuals who are suspected of having committed a given crime but who have not yet been convicted, and average homicides over the 1997-2005 period times 2005 population divided by 100000. The figures that determined the compound index were provided to the SESNSP by the National Institute of Geography and Statistics (INEGI); figures on registered suspects were from the database of registered suspects that the INEGI compiles, homicide statistics that the INEGI compiles that in turn are provided to the INEGI by the Secretary of Public Health (SSA), and population from the 2005 Census from the INEGI.

Initially the rules were designed such that every state in the country had at least two funded municipalities. In 2008, eligibility was based on the following rule: the two municipalities with the highest index in a given state were eligible for funding, this was to assure that every state received funding, and then those that were not the top two in their state but had the highest compound index in the country up to when a total 150 municipalities were funded. The amount awarded was then calculated as the share that a municipality contributed to the sum of the indexes for all eligible municipalities times the total budget assigned for the program in a given year; the federal government contributed 70% of the amount and the municipality had the responsibility to contribute the other 30%. Figure 1.2 shows a map of the municipalities that received the SUBSEMUN program by year for the 2008-2010 period. Given that initially the rules of the program were such that every state had at least two municipalities that were eligible for funding, the map shows that funded municipalities are not particularly concentrated in any specific region of the country. In later years the rules changed such that, in the index, the period that was used for the average registered suspects and homicides included later years, also different weights were given to homicides and to the different types of crime of the registered suspects. Also the rules of the program changed in later periods. The details of the program rules for the 2008-2012 period are reported in the appendix.

The rules of the program thus led to an arbitrary threshold that determined eligibility. In 2008 every state had its own cutoff, in some states the cutoff was the index of the second highest municipality and in others it was of whichever municipality that was in the list of the ones with the highest index in the country. Throughout the years, the years included in the compound index as well as the assignment rule changed. However, despite the rules changing, those that were initially eligible were much more likely to receive funding. Thus the initial eligibility rules led to an arbitrary cutoff above which municipalities were discontinuously more likely to receive funding.

1.4 Data

Data at the year municipality level are gathered from several sources. Data on homicides related to organized crime come from the Mexican National Security Council. Given that one of the main goals of the Calderon administration was to combat organized crime and in an effort to keep transparency in the government's actions and a record of organized crime violence, it designated a committee from representatives of various agencies whose responsibility was to classify homicides and assess whether it was a homicide related to organized crime. The assessment was done based on the modality of

the homicide as well whether either of the parties involved were suspected to be members of criminal organizations. Data on homicides related to organized crime were then also disaggregated into three categories: executions, which is where a civilian kills another civilian where either or both of whom were part of a criminal organization and there was no intervention of the authorities, clashes, which are homicides that result from a direct confrontation between public safety personnel and members of criminal organizations or from confrontations between criminal organizations, and aggressions, which is where a criminal organization directly attacks any government institution or any member of the government. These data for drug related homicides are available at the year municipality level for the 2007-2010 period, after 2010 the data were no longer collected. Data on general homicides for the 2000-2012 period come from mortality records from the Ministry of Health; it includes the age, education and occupation of the victim as well as the modality of the homicide and other information. Data on eligible municipalities for the 2008-2011 period as well as the amount awarded every year comes from Executive Secretariat of the National Public Security System (SESNSP). Electoral data for the 2000-2010 period come from the Center for Research and Development (CIDAC) and from the state's electoral institutes. Baseline characteristics come from the INEGI and are based on the 2005 census; also from the INEGI are data on registered suspects and homicides for the 1997-2005 period as well as population from the 2005 census, which are used to construct the index. Data on planted and harvested area of maize comes from the INEGI as well as data on funding for public safety expenditures by year.

1.5 Empirical Strategy

In order to estimate the effect of increasing state capacity, via increases in funding for public safety expenditures, on violence, we use a fuzzy regression discontinuity design. We take advantage of the fact that, given the initial rules of the program, an arbitrary threshold determined eligibility for the program. Moreover, despite the rules changing, those that were initially eligible were more likely to receive funding in any of the following years. Initially two municipalities in each state with the highest index were guaranteed funding and then those municipalities with the highest index in the country up to when the funds were exhausted, which in 2008 resulted in 150 municipalities being funded. Thus each state had an arbitrary threshold below which municipalities were not eligible. Using data on homicides, registered suspects and 2005 population from the INEGI, as stated by the program rules we calculate the running variable as the distance between the municipality's compound index and the state's eligibility threshold. The municipality's compound index is given by:

$$Index_{2008} = \left[.5Avg\ Homicides_{1997-2005} + .5Avg\ Registered\ Suspects_{1997-2005}\right] * \frac{Pop_{2005}}{100000}$$

We thus instrument treatment with the initial eligibility threshold. The statistics on registered suspects and homicides are compiled and kept by the INEGI. Given that for 2008 the index was based on registered suspects and homicides from the 1997-2005 period and that the program was not announced until 2007 when the statistics had already been published, there was no room for manipulation by the municipal governments. Using the initial eligibility threshold also has the additional advantage of addressing any potential selection and endogeneity issues in later years.

A fuzzy regression discontinuity estimate will give the causal estimate of funding on violence. This assumes the continuity of unobservable factors across the discontinuity. In our framework, the second stage equation is given by:

$$y_{it} = \beta_0 + \beta_1 \widehat{Funded}_{it} + \boldsymbol{\beta_2} f_n(N_{i,2008}) + \beta_3 1(N_{i,2008} \geq 0) + \boldsymbol{\beta_4} 1(N_{i,2008} \geq 0) * f_n\left(N_{i,2008}\right) + \gamma_t + \alpha_s + \varepsilon_{it} \quad (3)$$

Where y_{it} is the outcome, such as homicides or drug related homicides, for municipality i in year t. $Funded_{it}$ is an indicator that equals one in years in which municipality i is funded. I will also look at the cumulative years of funding as my main variable of interest since the program was meant for the purchasing of equipment, which can be thought of as fighting capital, and the benefits of which we would expect to accumulate over time. Thus while the coefficient on $Funded_{it}$ is the average effect of being funded in year t and any previous years in which the municipality got funded, the coefficient on $Cum\ yrs\ funded_{it}$ would give the effect of an additional year of funding. $N_{i,2008}$ is the 2008 normalized index, that is the difference between the index for municipality i and the state's eligibility threshold, $N_{i,2008} = Index_{i,2008} - E_{s,2008}$, where E_s is the state's eligibility threshold in 2008. $f_n\left(N_{i,2008}\right)$ are polynomial functions of the normalized score; thus the estimates control for trends in the index on both sides of the initial eligibility cutoff. γ_t and α_s are time and state fixed effects respectively. The regressions also control for any potential unobserved preexisting differences in the outcome at the initial eligibility threshold. Thus, loosely speaking, the empirical strategy could be thought of as a "diff-in-diff+RD". The first stage equation is given by:

$$\widehat{Funded}_{it} = \delta_0 + \rho_1 1(N_{i,2008} \geq 0) * After_t + \rho_2 1(N_{i,2008} \geq 0) + \boldsymbol{\rho_3} 1(N_{i,2008} \geq 0) * f_n\left(N_{i,2008}\right) + \boldsymbol{\rho_4} f_n\left(N_{i,2008}\right) + \gamma_t + \alpha_s + \mu_{it} \quad (4)$$

Where $1(N_{i,2008} \geq 0)$ is an indicator that takes the value of 1 if municipality i is to the right of the 2008 eligibility threshold in 2008 and $After_t$ is equal to 1 in 2008, when the program started, or later. Specification (3) is estimated using 2sls with standard errors clustered at the municipality level to address serial correlation in violence. The main analysis is for the 2007-2010 period since this is the period for which drug related homicides data are available.

1.6 Results

1.6.1 Summary Statistics

Table 1.2 shows summary statistics for the sample of municipalities that are within 300 units of the initial eligibility threshold, which constitutes the RD sample for the main analysis. Data on homicides come from the Ministry of Health and, as the table shows, almost 50% of homicides in the 2007-2010 period can be directly attributed to organized

crime. It is important to note that it is likely that drug related homicides account for more than 50% of total homicides since drug related homicides are only homicides that can be classified as being related to organized crime violence; the majority of drug related homicides are from executions, which represent about 86% of the total. Furthermore, approximately 9% of drug related homicides are homicides resulting from clashes between public safety personnel and criminal organizations or between criminal organizations.

1.6.2 Eligibility and Funding

Figure 1.3 shows the share of funded municipalities across the initial 2008 municipality index relative to the state's cutoff. The dots represent the share of funded municipalities in a 30 unit bin. The solid line represents a linear fit estimated on both sides of the initial eligibility threshold and the dashed line represents the 95% confidence interval. The graph shows that the rules of the program were followed almost perfectly in 2008. Moreover, in any given year, the probability of being funded sharply increases at the 2008 eligibility cutoff. Figure 1.4 shows a sharp increase in the total years of funding for municipalities that were to the right of the state's eligibility cutoff in 2008. Table 1.3 reports this first stage relationship. Columns 1-3 show the results with different parameterizations of the function *f(.),* which includes a linear, quadratic and cubic polynomial of the 2008 index on both sides of the cutoff. The relationship is strong and robust across specifications. If a municipality was eligible for funding in 2008, the probability of being funded in any given year is 96%, which confirms that the rules of the program were followed almost perfectly in 2008 and that those that were initially eligible were more likely to receive funding in later years. Panel B presents the relationship for cumulative years funded; municipalities that were initially eligible received an average of 1.93 years of cumulative funding in any given year over the 2007-2010 period.

1.6.3 Impact of the SUBSEMUN Program on Homicides

Figure 1.5 shows the relationship between homicides and program eligibility in 2008. The figure shows that, after the start of the program in 2008, those municipalities that were just eligible in 2008 experienced an increase in homicides relative to those that were just below the cutoff. It is important to note that the relationship becomes larger with the number of years since program implementation. This is to be expected, given that the funds were destined for fighting equipment and technology infrastructure for public safety, the effects of which would accumulate over time; if a municipality received funding the first year as well as the second, at the second year it would have accumulated more fighting capital, both from what was invested in the first year and from the second net of any depreciation. Importantly, there does not seem to be a significant discontinuity before the program started in 2007, and this is corroborated by the balance tests in table 1.4.

Table 1.4 shows tests for the smoothness of observables across the eligibility threshold. The regressions include state fixed effects and standard errors are clustered at the state level to account for any spatial correlation. It also controls for a linear, quadratic and cubic polynomial functions of the normalized score on both sides of the 2008 eligibility threshold. A key identifying assumption in an RD design is that other variables do not change discontinuously at the threshold; specifically in our framework, an identifying assumption is that the changes in the outcome would not have changed discontinuously at the initial eligibility cutoff in the absence of the program. As can be seen from the table, there are no significant discontinuities in baseline covariates at the eligibility

threshold, and most importantly there are no significant discontinuities in the pre-program 2007 period of the main outcomes of interest, homicides and drug related homicides, at the initial eligibility threshold, which increases confidence in the identifying assumption, moreover later I check for preexisting trends in the outcome at the initial eligibility threshold and the point estimate is not significant and close to zero. Note that in the quadratic fit the coefficient on PAN is negative and marginally significant. Media reports frequently reported that municipal mayors complained that there was corruption in the SUBSEMUN program such that Calderon was disproportionally funding municipalities with a PAN mayor, however this does not seem to be the case since the rules were followed almost perfectly and if anything those to the right were less likely to have a PAN mayor. Dell (2012) shows that municipalities where a PAN mayor wins by a small margin experience an increase in drug related violence, thus if anything this would bias any positive effect towards zero. Furthermore, the estimates that follow also control for any potential preexisting differences in the outcome at the discontinuity.

Table 1.5 shows the regression results for the effect of funding on homicides for the estimation equation (3) in Section 5. Panel A shows the results of the effect of being funded on any given year on homicides and panel B shows the effect an extra year of funding. As expected the point estimate on funded is larger than the coefficient on cumulative years; the coefficient on funded is not only the effect of being funded this year but also the effect from being funded in previous years, as fighting capital is expected to have a cumulative effect. The coefficients are large and robust across specifications. One additional year of funding increased homicides by about 6. This is a 48% increase over the 2007 baseline level of homicides for those eligible in 2008. Figure 1.6 shows that following the implementation of the program in 2008 there starts to be a sharp increase in gun related homicides of public safety workers. This is corroborated by the results in table 1.6, which presents results for the effect of the program on different groups. The program led to increases in homicides of individuals without high school and gun related homicides of public safety workers. The increase in homicides of public safety workers is consistent with the program increasing the confrontations between police and criminals due to the increase in fighting equipment for the police. Also importantly, the program does not seem to have an effect on homicides related to domestic violence, which is reassuring since it wouldn't be expected that the program would increase domestic violence homicides; also it did not lead to a significant increase in homicides of public safety workers not classified as being gun related, which supports the argument that the program in fact led to an increase in clashes between the police and criminal organizations and that the effect is not due to unobserved determinants of homicides, particularly for public safety personnel.

1.6.4 Impact of the SUBSEMUN Program on Drug Related Violence

Figure 1.7 shows the graphs for drug related homicides by year. Again drug related homicides sharply increase at the cutoff after the program starts and this effect increases over time, moreover there is not a significant discontinuity in drug related homicides in 2007, the pre-program year.

Table 1.7 shows the results of funding on drug related violence. Panel A repeats the results of table 1.5 for comparison purposes. Panel B and C separate the homicides into those that can be directly attributed to organized crime and those that cannot. Unclassified homicides are defined as the difference between homicides from the Ministry of Health database and homicides that have been classified as drug related. About 67% of the increase in homicides can be directly attributed to organized crime. One additional

year of funding increase drug related homicides by 4, which represents an increase of more than 100% over the 2007 baseline level of those eligible in 2008. The effect on homicides that cannot be classified is smaller both in magnitude and in the relative increase over baseline; an additional year of funding increases unclassified homicides by about 23%. I define unclassified homicides as the difference between homicides reported by the Ministry of Health and those that are classified as drug related and which come from the Mexican National Security Council. There is not much that can be said about these homicides; an explanation could be that they were drug related homicides but were not classified as so. Another alternative explanation could be that when the police receives fighting equipment and fights organized crime, criminal organizations give arms to more of their members, thus increasing homicides in general due to a higher circulation of guns.

1.6.5 Mechanisms

Funding led to an increase in drug related homicides and if this increase is due to the police fighting members of organized crime, which would weaken organizations and lead to fighting within organization and between organizations for the control of the territory, we would expect to see an increase in clashes between public safety personnel and criminal organizations. The best measure for clashes between public safety personnel and criminal organizations in the Ministry of Health database is gun related homicides of public safety workers and the best measure available in the drug related homicides database is the measure on clashes; although this measure might also include clashes between criminal organizations, findings from the previous section and the following findings on the effect on drug related homicides by subgroups provide evidence that the program induced the police to fight criminal organizations.

Figure 1.8 shows a sharp increase in the number of homicides resulting from drug related clashes and again there does not seem to be differences at the initial eligibility threshold in 2007, the pre-program period. Again we see that the differential increases over time. Table 1.8 shows the accompanying regression results by type of drug related homicide, for both the intensive and extensive margin. An additional year of funding increases the number of homicides resulting from clashes by .37, which represents an increase of about 120% over baseline. Columns 4-6 show the effect of the program on the extensive margin of drug related homicides by type. Although the program did not increase the offset of drug related violence resulting from executions, it did induce the offset of homicides resulting from clashes and from attacks to the government. While the clashes measure includes not only homicides from clashes between public safety personnel and criminal organizations but might also include homicides from clashes between criminal organizations, the fact that the program led to the offset of clashes but not of drug related executions, and that it led to an increase in attacks to the government as well as increases in gun related homicides of public safety workers, all provide evidence that the program in fact led to increases in clashes between public safety personnel and criminal organizations.

1.6.6 Robustness to Choice of Bandwidth

As shown in figure 1.9 the above results are robust to the choice of bandwidth. The figure plots the regression coefficients for homicides, gun related homicides of public safety workers, drug related homicides and drug related homicides resulting from clashes

and the corresponding 95% confidence interval for different bandwidths. When the bandwidth around the initial eligibility cutoff is decreased by 10 unit intervals the results, both in magnitude and in significance, remain largely unchanged, which increases confidence that the results are not driven by the choice of bandwidth.[1]

1.6.7 To Whom do the Funds Matter the Most?

Section 2 showed that the municipalities where we would expect to see the largest increases in violence are those with mid-levels of initial funding. It is expected that it is those in the middle that this increase in funding increases their probability of winning such that the probability of winning is now high enough to induce them to fight criminal organizations but not so high that criminal organizations do not fight back, as would be expected in municipalities with high levels of funding for public safety, or so low that they do not fight criminal organizations since the probability of winning is so low that the expected value of winning is not enough to offset the costs, as would be expected in municipalities with low levels of initial funding.

Table 1.9 and Figure 1.10 show the effect of the program by baseline level of funding for public safety. The results provide suggestive evidence in support of the predictions above. In order to look at the effect by baseline funding, initial level of funding is divided into three areas: low, middle and high. Low areas are municipalities in the first quartile of baseline funding for public safety, middle areas are those in the second and third quartile and high level areas are those in the fourth quartile. Although the point estimates are noisy due to the small sample size such that we cannot reject that the coefficients are the same, the results show that those in the middle are the ones with the highest increase in drug related homicides, drug related homicides resulting from clashes and, using data from the Ministry of Health, in gun related homicides of public safety workers; the pattern is similar for the offset of clashes. Figure 1.10 plots the point estimates for each of the areas as well as the 95% confidence interval for each estimate. The graph shows that the effect of the program seems to be higher for those in middle of the distribution of initial funding than for those with low levels of initial funding and then is lower for those in the highest level of the distribution, thus this is suggestive evidence of the existence of an inverted U-shaped relationship between initial funding and the effect of the program.

1.6.8 Political Factors and Agricultural Productivity

The major political parties in Mexico are PAN, PRI and PRD. Federal and state elections are every six years and municipal elections are held every three years. The main agenda of President Felipe Calderon (2006-2012) of the PAN party was to fight criminal organizations. Dell (2012) finds that municipalities where a PAN mayor wins by a small margin experience an increase in drug related violence immediately after the election, which is consistent with PAN municipalities fighting criminal organizations which leads to the weakening of organizations and to turf wars. If PAN municipalities are more likely to fight organized crime, it might be expected that after receiving funds for public safety expenditures they will be the ones to make the most use of those funds and subsequently

[1] The results are robust to using the Calonico et al (2014) optimal bandwidth with triangular kernel weights.

to see the largest increases in violence, however the results show that this does not seem to be the case.

Panel A in table 1.10 shows the heterogeneity results for PAN municipalities. In order to avoid reverse causality in later years the study takes advantage of the timing of the elections; the sample period used for the estimation is limited to years before the first municipal election took place in the post program period. For example, for municipalities that had had an election in 2006 the sample is limited to 2007 and 2008 and for those that had had an election in 2007 the sample is limited to 2007, 2008 and 2009. The results show that if anything the increase in violence was lower for PAN than for non-PAN municipalities. This could be because PAN municipalities were already experiencing higher levels of confrontations between the police and criminal organizations, which is supported by the higher level of homicides in clashes for PAN municipalities in the pre period, and hence the increase in funding did not matter as much to them as to municipalities that were not experiencing as many confrontations. Also, party alignment between the municipal mayor and the state governor does not have a differential effect on the effect of the program (see appendix). This is reassuring since the state government distributes funds from the federal government and, although there shouldn't be any differences on how these funds are allocated given that they are usually assigned based on formulas, this provides support that funded municipalities did not experience changes of other funding due to the program. Moreover, SUBSEMUN funding for municipalities at the initial eligibility threshold did not lead to changes in other funding in the post period, if anything they experienced an increase in other funding.

Panel B of table 1.10 shows the heterogeneous effect of the program based on political stability. A debate in Mexico has been whether it was the end of the one party system in early 2000 that has caused the increase in violence; one of the main arguments given is that it was the end of the one party system that led to increases in violence since before there were implicit agreements between the government and criminal organizations, agreements that were no longer credible when the party in power changed from term to term (Astorga, 2001; Serrano, 2007). Panel B shows the differential effect for municipalities that had had the same party in power in the pre period (2000-2007). It shows that municipalities that had a history of political stability, defined as having had the same party in the pre period, did not experience increases in violence as a result of the program, which is consistent with an infinitely repeated prisoner's dilemma game where the lower the probability that the game ends at any given time the higher the probability of cooperation. Moreover it not likely that this effect comes from these municipalities being PRI municipalities since there is significant variation in the party in power in these stable municipalities.

Finally, panel C of table 1.10 shows the effect of the program for municipalities with low land productivity. Low land productivity is defined as areas below the median level of maize yield, defined as harvested area over planted area, in 2005. In 2008 Mexico was the number one producer of marijuana (U.N. World Drug Report 2008). Dube et al (2014) find that decreases in maize prices increased cultivation of marijuana and opium in areas suited for maize cultivation. Thus if organizations engage in the production of drugs it is reasonable that areas that have low maize suitability would be less valuable to criminal organizations, unless it was a major drug trafficking rout, in which case it would bias our results given that drug trafficking routs are expected to be more valuable; it is in these low land productivity areas where it would be more likely that strengthening the police would lead criminal organizations to decrease criminal activity and not fight back given the territory is not as valuable. Panel C shows results that would are consistent with this. Municipalities that had low suitability experienced a decrease in total drug related violence of approximately 50% over baseline as a result of the program.

1.6.9 Medium Run Effects

What are the effects of the program in the medium run? It is possible that increases in funding initially might lead to increases in violence, however, as the funds keep accumulating such that the police becomes stronger and more equipped every year, there could be an eventual decrease in violence. This does not seem to have happened at least in the medium run. Since drug related homicides are not available after 2010, the analysis is done for the 2007-2012 period using homicide data from the Ministry of Health and as seen in Figure 1.11 and Table 1.13 there is no evidence that the program has led to decreases in homicides at least in the medium run.

1.7 Other Robustness Tests

Given that whether a municipality was to the right of the initial eligibility cutoff was arbitrary, we should not expect to see any differences across the eligibility threshold, nonetheless we present several robustness checks. A problem with the identification would be that those just above the initial eligibility threshold were experiencing an increasing trend in homicides in the pre-program period leading us to incorrectly attribute the increases in homicides to the program. Table 1.11 shows that this is not the case. Since drug related homicides data are not available to years prior to 2007 we use data on homicides from mortality records from the Ministry of Health. The sample used is for the 2000-2007 period. As shown in the table, the coefficient on the discontinuity is not significant and close to zero. Table 1.12 shows that the results are robust to including region by year trends. This increases our confidence that the results are not driven by time trends specific to certain regions. Also, even though the municipalities in our sample are not the most violent in the country, average drug related homicides of those to the right of the initial eligibility threshold in our sample is 3.9 at baseline and 19 in the rest that were initially eligible but not in our RD sample, a concern would be that there was increased federal support, for example in the form of military support, especially in the border states; border states have seen the most dramatic increase in violence and a concern would be the results are driven by changes in violence in these states. The results do not seem to be driven by border states, results are robust to excluding border states; also the results are robust to the inclusion of municipality fixed effects (see appendix). Finally, we should not expect to see any differential shocks in those initially eligible and this is supported by the fact that when controlling for non-homicide deaths, as a proxy for economic shocks and shocks to the health system, the results remain unchanged (see appendix).

1.8 Conclusion

What are the effects of increasing the state's capacity to fight crime on violence? The literature on conflict often points to the lack of capacity from the state to fight crime as a reason for the high levels of conflict in developing countries. And, although there is evidence on the effect of police on crime, there is little evidence on the effects of funding for public safety, specifically funds designed for the purchasing of fighting equipment and

security infrastructure, on violence in a context where large criminal organizations operate.

This study shows that, in such a context, increasing security infrastructure can lead to large increases in violence. An additional year of funding from the SUBSEMUN program in Mexico, which gave funding to municipalities to strengthen its police force via the purchase of fighting equipment, technology and training, led to an average increase of approximately 100% in drug related homicides over baseline. If we look at the effect on the types of drug related violence, the program led to an increase in the extensive and intensive margin of homicides resulting from clashes and from attacks to the government and in the intensive margin of homicides resulting from executions; also, using mortality records we see an increase in gun related homicides of public safety workers. These results are consistent with funding allowing authorities to fight criminal organizations as they became equipped with better fighting technology, which led to the weakening organizations which in turn led to more violence due to fighting within and between organizations for the control of the organization or of the territory. Also, consistent with theory, there is suggestive evidence of the existence of an inverted U-shaped relationship between the initial level of funding for public safety and the effect of the program, which suggests that perhaps larger increases in police funding for governments with a strong police force could potentially lead to a decrease in violence. The study also shows that PAN governed municipalities did not experience higher increases in violence than non-PAN municipalities due to the program, that politically stable municipalities did not experience increases in violence and that municipalities with low land productivity experienced a decrease of approximately 50% over baseline as a result of increased funding, consistent with low productivity areas being low value areas such that increases in funding led criminals to decrease criminal activity instead of fighting for the territory. Looking at the medium run effects, this study finds increases in violence as a result of the program in the short or medium run. Finally, if we assume that this effect is the same across all funded municipalities, then approximately 10% of drug related homicides in these municipalities during the 2008-2010 period can be attributed to the SUBSEMUN program.

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Tables

Table 1.1: Municipalities Funded and Total Amount of Funding

Year	Total municipalities funded	Total amount of funding
2008	150	˜361M USD
2009	206	˜361M USD
2010	206	˜423M USD

Table 1.2: Summary Statistics

	Mean	St Dev
Total homicides/yr	4.22	10.85
Total drug related homicides/yr	2.07	8.62
Drug related homicides in clashes/yr	0.24	1.72
Drug related homicides from executions/yr	1.79	7.78
Drug related homicides from attacks to the gov't/yr	.04	.41
Population/yr	42349.36	41110.22
Amount of funding awarded USD (millions)	1.1	0.19
Pan mayor	0.25	0.44
Municipalities	1550	

Note: The sample is for the 2007-2010 period and restricted to municipalities within 300 units of the state's 2008 eligibility threshold for the SUBSEMUN program.

Table 1.3: SUBSEMUN 2008 Eligibility and Participation

	(1)	(2)	(3)
Panel A: Funded			
$1(N_{i,2008} \geq 0) * After$	0.96***	0.96***	0.96***
	(0.01)	(0.01)	(0.01)
Mean of 2008 non eligibles	0.02	0.02	0.02
Panel B: Cumulative years funded			
$1(N_{i,2008} \geq 0) * After$	1.93***	1.93***	1.93***
	(0.02)	(0.02)	(0.02)
Mean of 2008 non eligibles	0.03	0.03	0.03
Initial eligibility index polynomial controls	Linear	Quadratic	Cubic
Year FE	Y	Y	Y
State FE	Y	Y	Y
Observations	6200	6200	6200
Municipalities	1550	1550	1550

Note: The running variable is the distance between the 2008 municipality's index and the state eligibility threshold in 2008. Linear, quadratic and cubic regressions control for flexible trends of the running variable on each side of the eligibility threshold. Includes population controls. Sample is for the 2007-2010 period and limited to municipalities within 300 units of the state's 2008 initial eligibility threshold. Standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1

Table 1.4: Balance Tests

Dependent variable	Linear	Quadratic	Cubic
% with access to water (2005)	-0.01	-0.03	0.01
	(0.02)	(0.03)	(0.04)
% with access to sewage (2005)	-0.04	-0.03	-0.11
	(0.04)	(0.05)	(0.08)
% with access to electricity (2005)	-0.01	-.03*	-0.04
	(0.01)	(0.02)	(0.03)
% with access to water, sewage and elec (2005)	-0.02	-0.02	-0.03
	(0.01)	(0.02)	(0.02)
Income per capita (2005)	-569.2	924.8	-784.1
	(1148.00)	(1229.00)	(1687.00)
Development index(2005)	-0.02	-0.01	-0.03
	(0.02)	(0.02)	(0.03)
% 18 above with high school (2005)	-5.47	-1.5	-2.97
	(3.97)	(3.87)	(5.29)
% with Seguro Popular	6.66	5.39	2.74
	(3.98)	(5.42)	(7.61)
% with other health insurance	-6.68	-2.05	-1.26
	(4.73)	(4.88)	(8.48)
% with non dirt floor	-5.06	-5.54	-8.4
	(3.72)	(4.30)	(6.65)
PAN Mayor (2007)	-0.14	-0.22*	-0.17
	(0.10)	(0.12)	(0.17)
Homicides (2006-2007)	-1.96	-2.86	-1.95
	(1.56)	(1.88)	(2.48)
Drug related homicides (2007)	-0.18	-0.28	1.78
	(0.97)	(1.01)	(1.08)
Municipalities	1550	1550	1550

Note: The coefficient is for the 2008 eligibility indicator. The running variable is the distance between the 2008 municipality's index and the state eligibility threshold in 2008. Linear, quadratic and cubic regressions control for flexible trends of the running variable on each side of the initial eligibility threshold. Includes state fixed effects. Sample is limited to municipalities within 300 units of the state's 2008 eligibility threshold. Standard errors are in parentheses and clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table 1.5: Funding and Homicides

	(1)	(2)	(3)
Panel A: Funding on homicides			
Funded	12.1***	12.1***	12.0***
	(3.13)	(3.13)	(3.13)
2007 mean of 2008 eligibles	12.59	12.59	12.59
Panel B: Cumulative years funded on homicides			
Cumulative years funded	6.02***	6.01***	6.01***
	(1.56)	(1.56)	(1.56)
2007 mean of 2008 eligibles	12.59	12.59	12.59
Initial eligibility index polynomial controls	Linear	Quadratic	Cubic
Year FE	Y	Y	Y
State FE	Y	Y	Y
Observations	6200	6200	6200
Municipalities	1550	1550	1550

Note: Funded is instrumented with the 2008 eligibility indicator interacted with an indicator for 2008 onwards. The running variable is the distance between the 2008 municipality's index and the state eligibility threshold in 2008. Linear, quadratic and cubic regressions control for flexible trends of the running variable on each side of the eligibility threshold. Includes population controls. Sample is for the 2007-2010 period and limited to municipalities within 300 units of the state's 2008 eligibility threshold. Standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1

Table 1.6: Funding and Homicides by Groups

	(1)	(2)	(3)
Panel A: All homicides			
Cumulative years funded	5.29***	5.29***	5.28***
	(1.42)	(1.42)	(1.42)
2007 mean of 2008 eligibles	11.5	11.5	11.5
Panel B: Homicides of non-public safety workers aged 18+ w/o HS			
Cumulative years funded	3.48***	3.48***	3.47***
	(0.97)	(0.97)	(0.97)
2007 mean of 2008 eligibles	7.75	7.75	7.75
Panel C: Gun related homicides of public safety workers			
Cumulative years funded	0.28**	0.28**	0.28**
	(0.12)	(0.12)	(0.12)
2007 mean of 2008 eligibles	0.57	0.57	0.57
Panel D: Non-gun related homicides of public safety workers			
Cumulative years funded	0.052	0.052	0.052
	(0.04)	(0.04)	(0.04)
2007 mean of 2008 eligibles	0.25	0.25	0.25
Panel E: Domestic violence homicides			
Cumulative years funded	0.031	0.03	0.03
	(0.03)	(0.03)	(0.03)
2007 mean of 2008 eligibles	0.3	0.3	0.3
Polynomial	Linear	Quadratic	Cubic
Year FE	Y	Y	Y
State FE	Y	Y	Y
Observations	6200	6200	6200
Municipalities	1550	1550	1550

Note: Cumulative years funded is instrumented with the 2008 eligibility indicator interacted with an indicator for 2008 onwards. The running variable is the distance between the 2008 municipality's index and the state eligibility threshold in 2008. Linear, quadratic and cubic regressions control for flexible trends of the running variable on each side of the eligibility threshold. Includes population controls. Sample includes observations that are not missing age and education. Sample is for the 2007-2010 period and limited to municipalities within 300 units of the state's 2008 eligibility threshold. Standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1

Table 1.7: Cumulative Years of Funding and Drug Related Violence

	(1)	(2)	(3)
Panel A: Homicides			
Cumulative years funded	6.02***	6.01***	6.01***
	(1.56)	(1.56)	(1.56)
2007 mean of 2008 eligibles	12.59	12.59	12.59
Panel B: Drug Related homicides			
Cumulative years funded	4.01***	4.01***	4.00***
	(1.29)	(1.29)	(1.29)
2007 mean of 2008 eligibles	3.93	3.93	3.93
Panel C: Unclassified homicides			
Cumulative years funded	2.01***	2.00***	2.00***
	(0.49)	(0.49)	(0.49)
2007 mean of 2008 eligibles	8.66	8.66	8.66
Initial eligibility index polynomial controls	Linear	Quadratic	Cubic
Year FE	Y	Y	Y
State FE	Y	Y	Y
Observations	6200	6200	6200
Municipalities	1550	1550	1550

Note: Cumulative years funded is the total number of years funded from 2008 onwards and is instrumented with the 2008 eligibility indicator interacted with an indicator for 2008 onwards. The running variable is the distance between the 2008 municipality's index and the state eligibility threshold in 2008. Linear, quadratic and cubic regressions control for flexible trends of the running variable on each side of the eligibility threshold. Includes population controls. Sample is for the 2007-2010 period and limited to municipalities within 300 units of the state's 2008 eligibility threshold. Standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1

Table 1.8: Effects on the Intensive and Extensive Margin of Drug Related Homicides by Type

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Total drug related homicides						
	Homicides			Homicide probability		
Cumulative years funded	4.01***	4.01***	4.00***	0.015	0.015	0.015
	(1.29)	(1.29)	(1.29)	(0.03)	(0.03)	(0.03)
2007 mean of 2008 PAN eligible	3.93	3.93	3.93	0.69	0.69	0.69
Panel B: Drug related homicides from executions						
	Homicides			Homicide probability		
Cumulative years funded	3.51***	3.51***	3.50***	0.016	0.016	0.016
	(1.24)	(1.24)	(1.24)	(0.03)	(0.03)	(0.03)
2007 mean of 2008 stab eligible	3.59	3.59	3.59	0.66	0.66	0.66
Panel C: Drug related homicides in clashes						
	Homicides			Homicide probability		
Cumulative years funded	0.37***	0.37***	0.37***	0.10***	0.10***	0.10***
	(0.11)	(0.11)	(0.11)	(0.03)	(0.03)	(0.03)
2007 mean of 2008 low eligible	0.31	0.31	0.31	0.1	0.1	0.1
Panel D: Drug related homicides from attacks to the government						
	Homicides			Homicide probability		
Cumulative years funded	0.13**	0.13**	0.13**	0.03*	0.03*	0.03*
	(0.06)	(0.06)	(0.06)	(0.02)	(0.02)	(0.02)
2007 mean of 2008 low eligible	0.03	0.03	0.03	0.03	0.03	0.03
Initial eligibility index polynomial controls	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic
Year FE	Y	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y	Y
Observations	6200	6200	6200	6200	6200	6200
Municipalities	1550	1550	1550	1550	1550	1550

Note: Cumulative years funded is the total number of years funded from 2008 onwards and is instrumented with the 2008 eligibility indicator interacted with an indicator for 2008 onwards. The running variable is the distance between the 2008 municipality's index and the state eligibility threshold in 2008. Linear, quadratic and cubic regressions control for flexible trends of the running variable on each side of the eligibility threshold. Includes population controls. Sample is for the 2007-2010 period and limited to municipalities within 300 units of the state's 2008 eligibility threshold. Standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1

Table 1.9: Initial Funding for Public Safety and the Effect of the Program

	(1)	(2)	(3)
Panel A: Total drug related homicides			
Cumulative years*low	1.53***	1.52**	1.53**
	(0.59)	(0.60)	(0.61)
Cumulative years*med	5.12**	5.12**	5.10**
	(2.25)	(2.25)	(2.25)
Cumulative years*high	4.09*	4.10*	4.10*
	(2.35)	(2.36)	(2.36)
2007 mean of 2008 low eligibles	2.29	2.29	2.29
2007 mean of 2008 med eligibles	4.23	4.23	4.23
2007 mean of 2008 high eligibles	4.80	4.80	4.80
Panel B: Drug related homicides in clashes			
Cumulative years*low	0.22	0.22	0.22
	(0.14)	(0.14)	(0.14)
Cumulative years*med	0.57***	0.57***	0.56***
	(0.20)	(0.20)	(0.20)
Cumulative years*high	0.13	0.13	0.13
	(0.12)	(0.12)	(0.12)
2007 mean of 2008 low eligibles	0.00	0.00	0.00
2007 mean of 2008 med eligibles	0.53	0.53	0.53
2007 mean of 2008 high eligibles	0.20	0.20	0.20
Panel C: Gun related homicides of public safety workers			
Cumulative years*low	0.17	0.17	0.17
	(0.11)	(0.11)	(0.12)
Cumulative years*med	0.41**	0.41**	0.41**
	(0.16)	(0.16)	(0.16)
Cumulative years*high	0.14	0.14	0.14
	(0.29)	(0.29)	(0.29)
2007 mean of 2008 low eligibles	0.24	0.24	0.24
2007 mean of 2008 med eligibles	0.50	0.50	0.50
2007 mean of 2008 high eligibles	1.00	1.00	1.00
Initial eligibility index polynomial controls	Linear	Quadratic	Cubic
Year FE	Y	Y	Y
State FE	Y	Y	Y
Observations	6200	6200	6200
Municipalities	1550	1550	1550

Note: Cumulative years funded is the total number of years funded from 2008 onwards and is instrumented with the 2008 eligibility indicator interacted with an indicator for 2008 onwards. The running variable is the distance between the 2008 municipality index and the state eligibility threshold in 2008. Linear, quadratic and cubic regressions control for flexible trends

of the running variable on each side of the eligibility threshold. Includes population controls. Low, is an indicator equal to one for municipalities in the first quartile of 2007 baseline funding for public safety, medium is an indicator for those in the second and third quartile and high is an indicator equal to one for those in the fourth quartile. Drug related homicides in panel A and B are from the drug related homicides database and homicides of public safety workers in panel C are from the Ministry of Health database. Sample is for the 2007-2010 period and limited to municipalities within 300 units of the state's 2008 eligibility threshold. Standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1

Table 1.10: Drug Related Homicides Heterogeneity Results

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Mayor's party affiliation and the effect of the program						
	Total drug related homicides			Drug related hom in clashes		
Cumulative years funded	6.09**	6.15**	6.13**	0.61***	0.63***	0.63***
	(2.51)	(2.51)	(2.51)	(0.19)	(0.19)	(0.19)
Cumulative years*PAN	-4.62	-4.86*	-4.82*	-0.91**	-0.97**	-0.97**
	(2.84)	(2.84)	(2.83)	(0.40)	(0.41)	(0.41)
2007 mean of 2008 PAN eligible	3.88	3.88	3.88	0.94	0.94	0.94
2007 mean of 2008 non PAN eligible	3.94	3.94	3.94	0.11	0.11	0.11
Panel B: Political Stability and the effect of the program						
	Total drug related homicides			Drug related hom in clashes		
Cumulative years funded	4.62***	4.62***	4.62***	0.45***	0.45***	0.45***
	(1.49)	(1.49)	(1.49)	(0.12)	(0.12)	(0.12)
Cumulative years*stab	-4.71***	-4.72***	-4.72***	-0.89***	-0.89***	-0.89***
	(1.58)	(1.58)	(1.58)	(0.24)	(0.24)	(0.24)
2007 mean of 2008 stab eligible	1.63	1.63	1.63	0.25	0.25	0.25
2007 mean of 2008 non stab eligible	4.22	4.22	4.22	0.32	0.32	0.32
Panel C: Land productivity and the effect of the program						
	Total drug related homicides			Drug related hom in clashes		
Cumulative years funded	4.70***	4.70***	4.70***	0.42***	0.42***	0.42***
	(1.48)	(1.48)	(1.48)	(0.12)	(0.12)	(0.12)
Cumulative years*low	-5.62***	-5.68***	-5.68***	-0.52***	-0.53***	-0.53***
	(1.53)	(1.54)	(1.54)	(0.13)	(0.13)	(0.13)
2007 mean of 2008 low eligibles	1.88	1.88	1.88	0	0	0
2007 mean of 2008 high eligibles	4.19	4.19	4.19	0.35	0.35	0.35
Initial eligibility index polynomial controls	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic
Year FE	Y	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y	Y
Observations	6200	6200	6200	6200	6200	6200

Note: Cumulative years funded is the total number of years funded from 2008 onwards and is instrumented with the 2008 eligibility indicator interacted with an indicator for 2008 onwards. The running variable is the distance between the 2008 municipality's index and the state eligibility threshold in 2008. Linear, quadratic and cubic regressions control for flexible trends of the running variable on each side of the eligibility threshold. Includes population controls. In panel A the sample is limited to the period before the first post-program election took place. In panel B political stability is defined as those municipalities who had the same party in power in the pre-program (2000-2007) period. In panel C low is a proxy for land productivity and is an indicator equal to one for municipalities that had a below medium maize yield in 2005. Sample is for the 2007-2010 period and limited to municipalities within 300 units of the state's 2008 eligibility threshold. Standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1

Table 1.11: Trends in Homicides for 2008 Eligible Municipalities

	(1)	(2)	(3)
Year to year change in homicides (2001-2007)			
Eligible 2008*Trend	0.03	0.03	0.03
	(0.14)	(0.14)	(0.14)
Polynomial	Linear	Quadratic	Cubic
Year FE	Y	Y	Y
State FE	Y	Y	Y
Observations	10850	10850	10850
Municipalities	1550	1550	1550

Note: Eligible 2008 is an indicator for being to the right of the eligibility threshold in 2008. The running variable is the distance between the 2008 municipality's index and the state eligibility threshold in 2008. Linear, quadratic and cubic regressions control for flexible trends of the running variable on each side of the eligibility threshold. Controls for period to period changes in population. Standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1

Table 1.12: Robustness to Region Time Trends

	(1)	(2)	(3)
Panel A: Total homicides			
Cumulative years funded	5.75***	5.74***	5.74***
	(1.52)	(1.52)	(1.52)
2007 mean of 2008 eligibles	12.59	12.59	12.59
Panel B: Total drug related homicides			
Cumulative years funded	3.65***	3.65***	3.64***
	(1.27)	(1.27)	(1.27)
2007 mean of 2008 eligibles	3.93	3.93	3.93
Panel C: Unclassified homicides			
Cumulative years funded	2.10***	2.10***	2.09***
	(0.47)	(0.47)	(0.47)
2007 mean of 2008 eligibles	8.66	8.66	8.66
Initial eligibility index polynomial controls	Linear	Quadratic	Cubic
Year FE	Y	Y	Y
State FE	Y	Y	Y
Region time trends	Y	Y	Y
Observations	6200	6200	6200
Municipalities	1550	1550	1550

Note: Cumulative years funded is the total number of years funded from 2008 onwards and is instrumented with the 2008 eligibility indicator interacted with an indicator for 2008 onwards. The running variable is the distance between the 2008 municipality's index and the state eligibility threshold in 2008. Linear, quadratic and cubic regressions control for flexible trends of the running variable on each side of the eligibility threshold. Includes population controls. Sample is for the 2007-2010 period and limited to municipalities within 300 units of the state's 2008 eligibility threshold. Standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1

Table 1.13: Medium Run Effects on Homicides

	(1)	(2)	(3)
Panel A: Cumulative years funded (2007-2012)			
$1(N_{i,2008} \geq 0) * After$	2.79***	2.79***	2.79***
	(0.04)	(0.04)	(0.04)
Mean of 2008 non eligibles	0.07	0.07	0.07
Panel B: Homicides (2007-2012)			
Cumulative years funded	5.27***	5.55***	6.10***
	(1.63)	(1.97)	(2.35)
2007 mean of 2008 eligibles	12.59	12.59	12.59
Initial eligibility index polynomial controls	Linear	Quadratic	Cubic
Year FE	Y	Y	Y
Municipal FE	Y	Y	Y
Observations	9300	9300	9300
Municipalities	1550	1550	1550

Note: Cumulative years funded is the total number of years funded from 2008 onwards and is instrumented with the 2008 eligibility indicator interacted with an indicator for 2008 onwards. The running variable is the distance between the 2008 municipality's index and the state eligibility threshold in 2008. Linear, quadratic and cubic regressions control for flexible trends of the running variable on each side of the eligibility threshold. Includes population controls. Sample is for the 2007-2012 period and limited to municipalities within 300 units of the state's 2008 eligibility threshold. Standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1

Figures

Figure 1.1: The Decision to Fight

Panel A: Payoff matrix

		Player 2	
		F	NF
Player 1	F	$(pV_1 - C_1^1, (1-p)V_2 - C_2^1)$	$(V_1 - C_1^2, -l_2)$
	NF	$(-l_1, V_2 - C_2^2)$	(Q_1, Q_2)

Panel B: Reaction functions

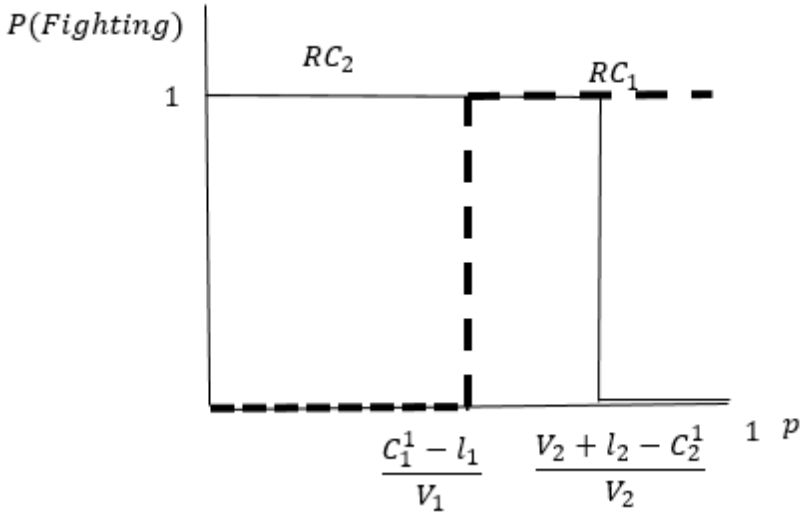
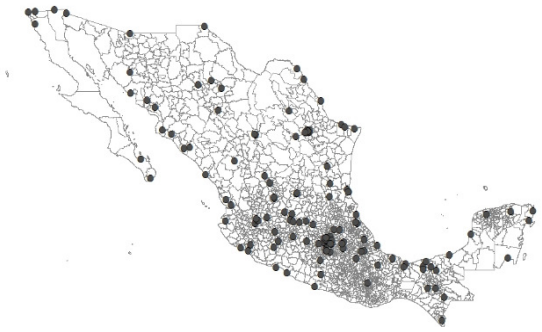
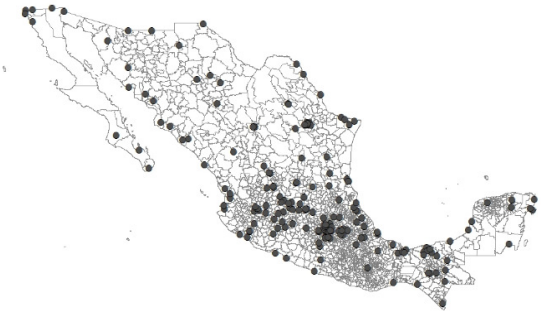


Figure 1.2: Map of SUBSEMUN Municipalities by Year

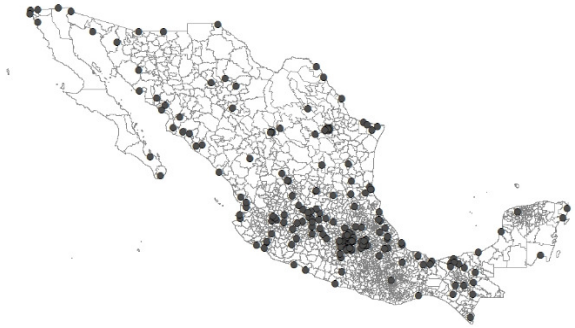
2008



2009

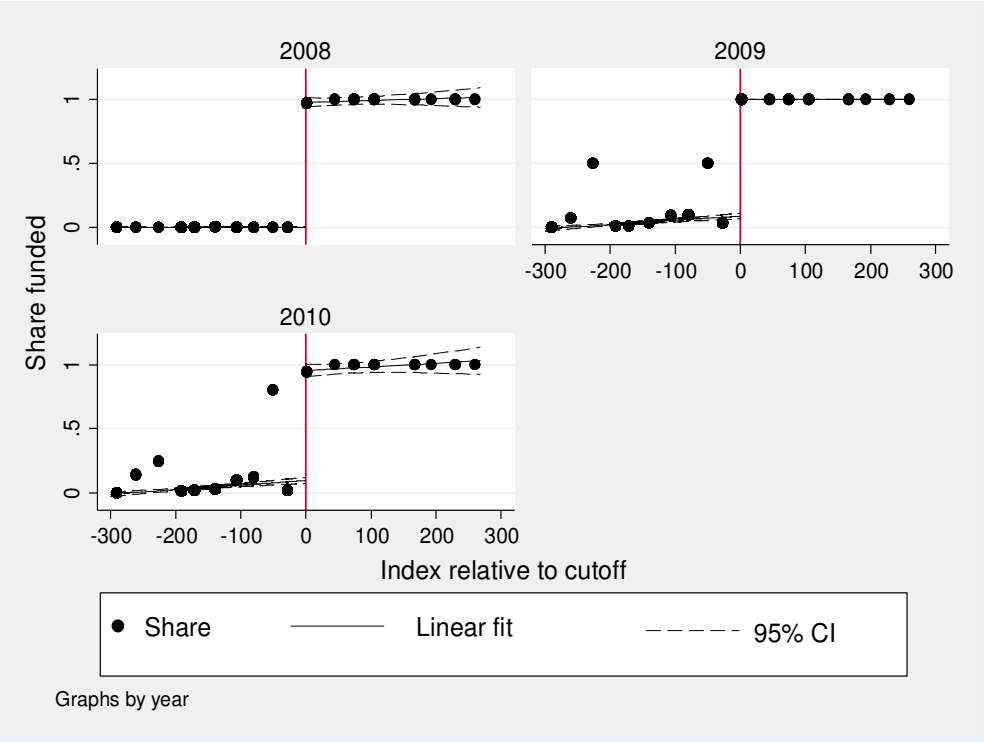


2010



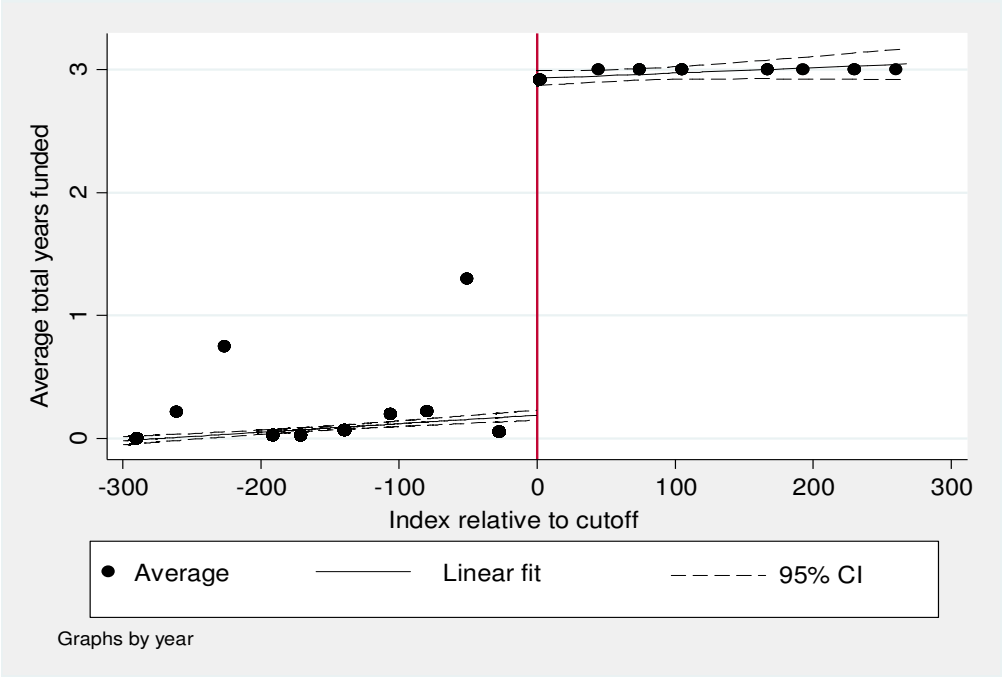
Note: Dots represent municipalities that participated in the SUBSEMUN program by year.

Figure 1.3: Initial 2008 Eligibility and Funding by Year



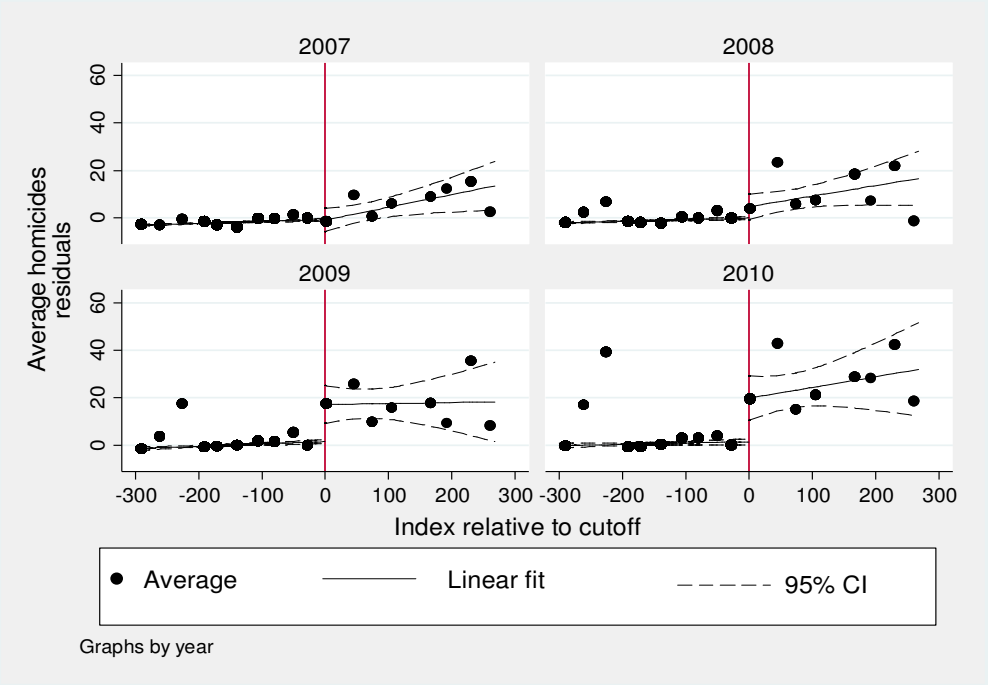
Note: Each dot represents the share of funded municipalities in a 30 unit bin of the 2008 normalized index, which is the distance between the 2008 index of the municipality and the 2008 state eligibility threshold. The solid lines represent a linear fit, separately estimated on each side of the eligibility threshold, and the dashed lines represent the 95% confidence interval.

Figure 1.4: Initial 2008 Eligibility and Total Years of Funding (2008-2010)



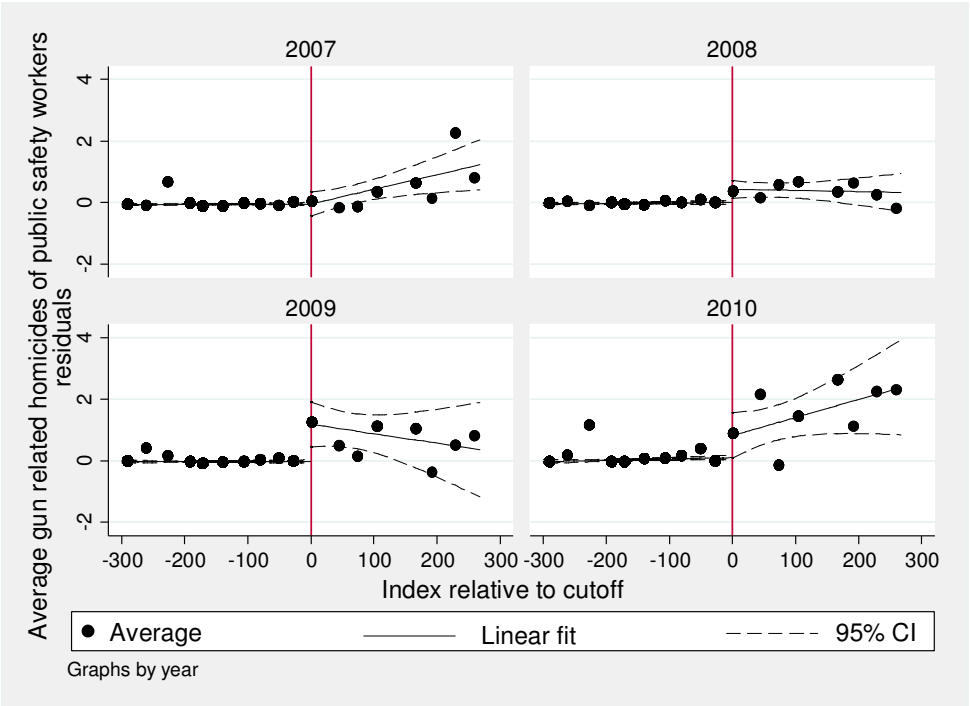
Note: Each dot represents the average total years of funding for the 2008-2010 period in a 30 unit bin of the 2008 normalized index, which is the distance between the 2008 index of the municipality and the 2008 state eligibility threshold. The solid lines represent a linear fit, separately estimated on each side of the eligibility threshold, and the dashed lines represent the 95% confidence interval.

Figure 1.5: Initial 2008 Eligibility and Average Total Homicides



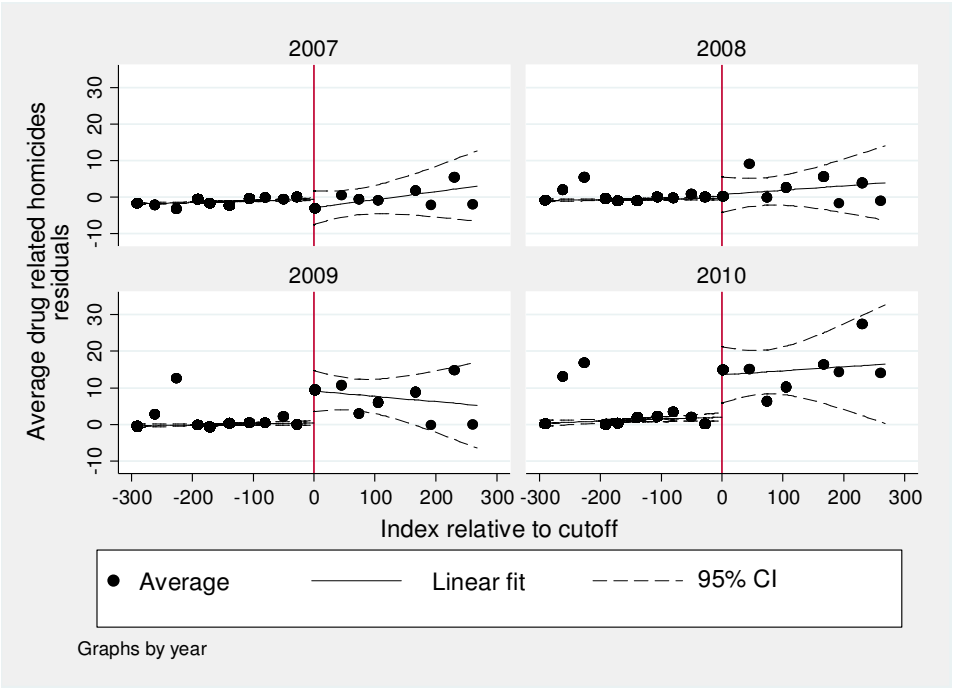
Note: Each dot represents average homicides, after controlling for state fixed effects, in a 30 unit bin of the 2008 normalized index, which is the distance between the 2008 index of the municipality and the 2008 state eligibility threshold. The solid lines represent a linear fit, separately estimated on each side of the eligibility threshold, and the dashed lines represent the 95% confidence interval.

Figure 1.6: Initial 2008 Eligibility and Average Gun Related Homicides of Public Safety Workers



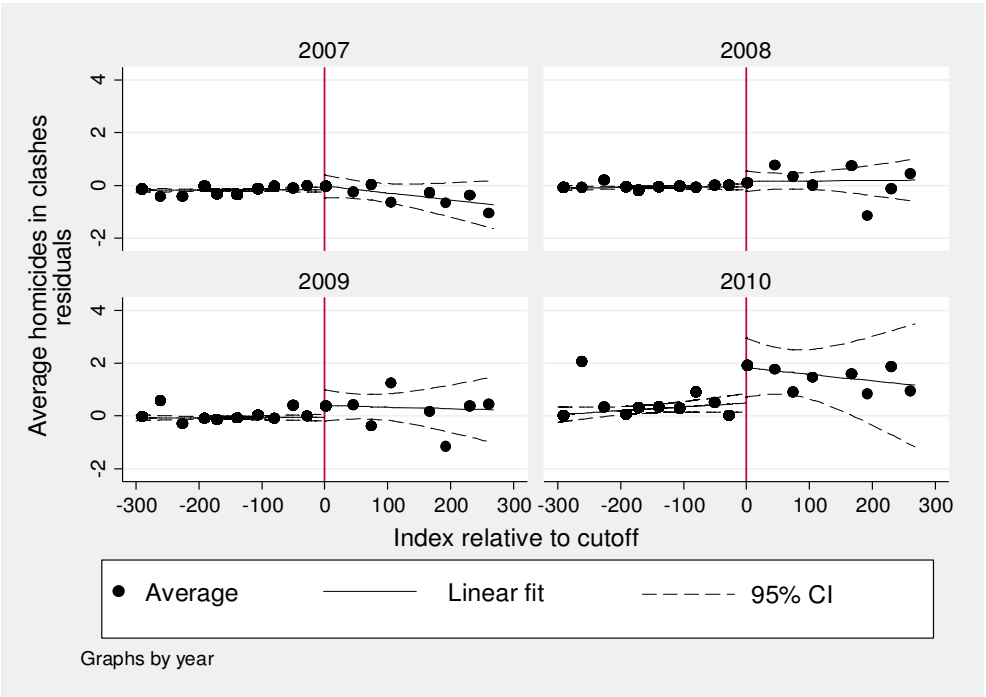
Note: Each dot represents average homicides, after controlling for state fixed effects, in a 30 unit bin of the 2008 normalized index, which is the distance between the 2008 index of the municipality and the 2008 state eligibility threshold. The solid lines represent a linear fit, separately estimated on each side of the eligibility threshold, and the dashed lines represent the 95% confidence interval.

Figure 1.7: Initial 2008 Eligibility and Average Total Drug Related Homicides



Note: Each dot represents average drug related homicides, after controlling for state fixed effects, in a 30 unit bin of the 2008 normalized index, which is the distance between the 2008 index of the municipality and the 2008 state eligibility threshold. The solid lines represent a linear fit, separately estimated on each side of the eligibility threshold, and the dashed lines represent the 95% confidence interval.

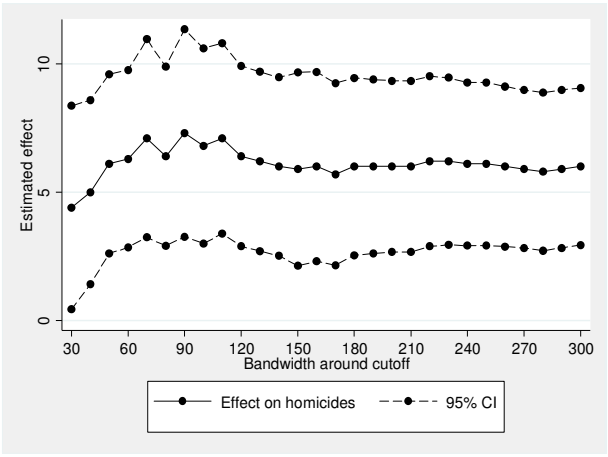
Figure 1.8: Initial 2008 Eligibility and Average Drug Related Homicides Resulting from Clashes



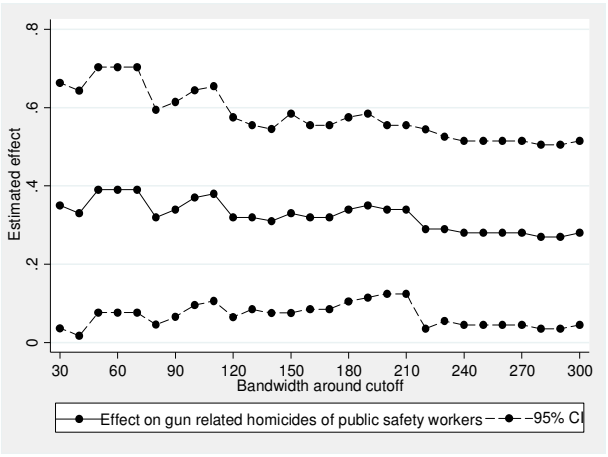
Note: Each dot represents average drug related homicides resulting from clashes, after controlling for state fixed effects, in a 30 unit bin of the 2008 normalized index, which is the distance between the 2008 index of the municipality and the 2008 state eligibility threshold. The solid lines represent a linear fit, separately estimated on each side of the eligibility threshold, and the dashed lines represent the 95% confidence interval.

Figure 1.9: Comparison of Bandwidths on the Effect of SUBSEMUN on Violence Outcomes

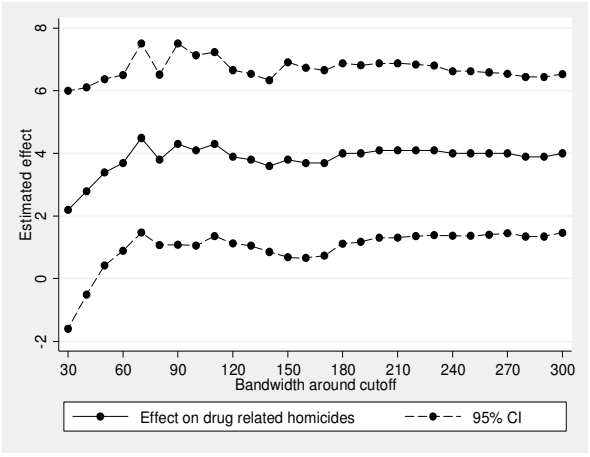
Panel A: Effect on Homicides



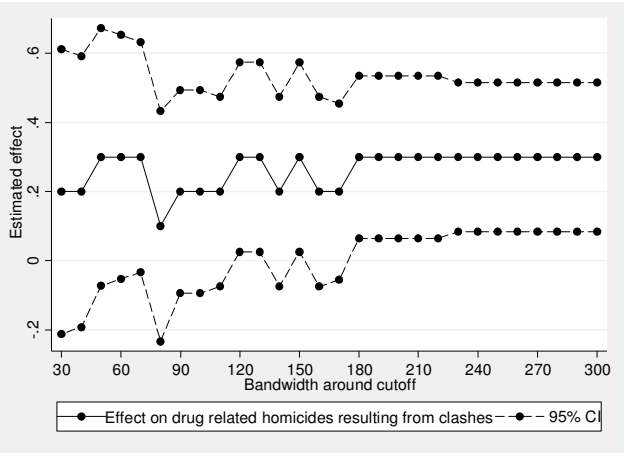
Panel B: Effect on gun related homicides of public safety workers



Panel C: Effect on drug related homicides



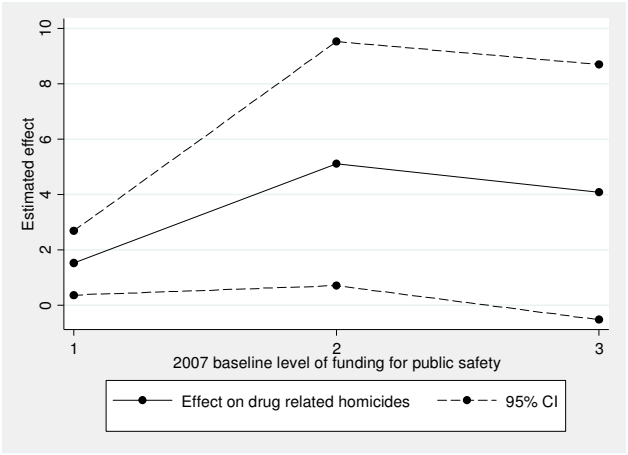
Panel D: Effect on drug related homicides resulting from clashes



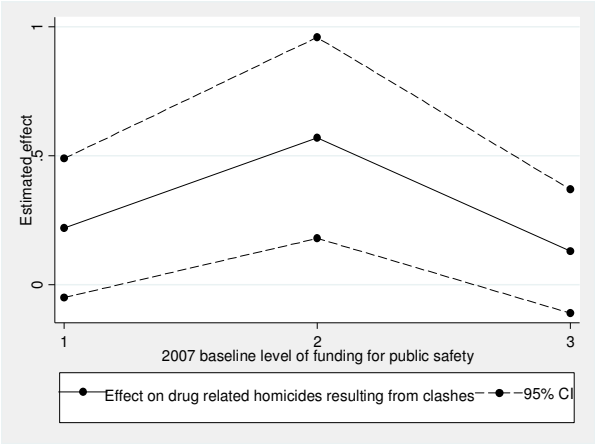
Note: The graphs show the RD estimation of the effect of receiving an extra year of SUBSEMUN funding on homicides, gun related homicides of public safety workers, drug related homicides, and drug related homicides resulting from clashes using different bandwidths around the normalized 2008 index and 95% confidence intervals constructed using clustered standard errors. The regressions control for linear trends in the normalized index on both sides of the 2008 eligibility threshold as well as population, state fixed effects and any potential pre period differences in the outcome at the 2008 eligibility threshold.

Figure 1.10: Effect of the SUBSEMUN Program on Violence Outcomes by Baseline Level of Funding for Public Safety

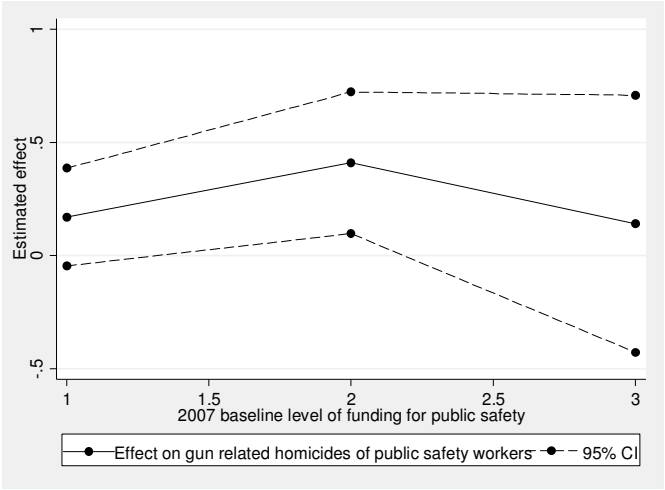
Panel A: Effect on drug related homicides



Panel B: Effect on drug related homicides resulting from clashes



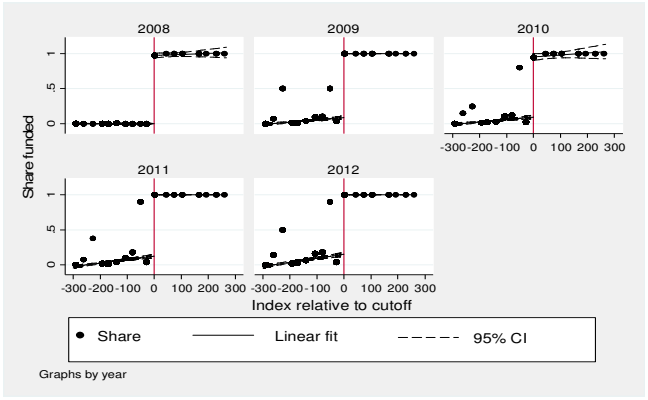
Panel C: Effect on gun related homicides of public safety workers



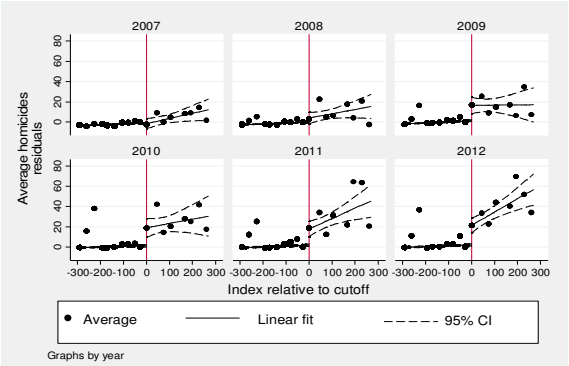
Note: For panel A and B each point represents the effect of the SUBSEMUN program on drug related homicides and drug related homicides resulting from clashes by the level of 2007 pre-program funding for public safety expenditures (and 95% confidence intervals from clustered standard errors). For panel C each point represents the effect of the SUBSEMUN program on gun related homicides of public safety workers from the Ministry of Health database by the level of 2007 pre-program funding for public safety expenditures (and 95% confidence intervals from clustered standard errors). Level 1 of funding is for municipalities in the first quartile of initial funding for public safety, level 2 if for municipalities in the second and third quartile and level 3 is for municipalities in the fourth quartile.

Figure 1.11: Short Run or Medium Run Effect?

Panel A: 2008 Initial Eligibility and Funding



Panel B: Initial Eligibility and Average Homicides



Note: Each dot represents the share of funded municipalities in panel A and average homicides after controlling for state fixed effects in Panel B, in a 30 unit bin of the 2008 normalized index, which is the distance between the 2008 index of the municipality and the 2008 state eligibility threshold. The solid lines represent a linear fit, separately estimated on each side of the eligibility threshold, and the dashed lines represent the 95% confidence interval.

Appendix

The Program

Table 1.14: Eligibility Rules by Year

Year	Eligibility Formula	Assignment Rule	Muns Funded	Total Amt
2008	$Index_{2008} = [.5Avg\ Homicides_{1997-2005} + .5Avg\ Reg\ Suspects_{1997-2005}] * \frac{Pop_{2005}}{100000}$	-top 2 with highest Index in each state -those with the highest index in the country	150	~361M USD
2009	$Index_{2009} = [.5Avg\ Homicides_{1997-2006} + .5Avg\ Reg\ Suspects_{1997-2006}] * \frac{Pop_{2005}}{100000}$	-top 3 with highest index in each state -border and tourist destinations -those with the highest index in the country	206	~361M USD
2010	$Index_{2010} = [.5Avg\ Homicides_{1997-2007} + .5Avg\ Reg\ Suspects_{1997-2007}] * \frac{Pop_{2005}}{100000}$	-those with more than 1000 avg suspects and homicides per 100k and that account for 90% of crime in the country -border and tourist destinations with more than 80 avg suspects per 100k pop -those with more than 20 avg suspects per 100k	206	~423M USD
2011	$Index_{2011} = \begin{bmatrix} .35Avg\ Homicides_{1997-2009} + \\ .3Avg\ Kidnappings\ Reg\ Sus_{2005-2009} \\ .35Avg\ Other\ Reg\ Suspects_{2005-2009} \end{bmatrix} * \frac{Pop_{2010}}{100000}$	-top 3 with highest Index in each state -those with index¿450 and that account for 90% of crime in country -border and tourist destinations	221	~423M USD
2012	$Index_{i,2012} = .5\frac{Pop_{i,2010}}{\sum_i Pop_{i,2010}} * 100 + .4[Crime\ Index_{i,2011} / \sum_i Crime\ Index_{i,2011}] + .1[Characteristics\ Index_i / \sum_i Characteristics\ Index_i]$ Crime index is based on homicides and registered suspects for all crimes. Characteristics index is based on whether a municipality is a border or tourism destination and on proximity to high crime municipalities	-top 3 with highest crime index in each state -those municipalities that were benefitted in the second and third year of the program and adhered to the rules -those with the highest index in the country and pop higher than the avg pop in the country	239	~426M USD

Heterogeneity Results

Table 1.15: Municipal and State Gov't Party Alignment and the Effect of the Program

	(1)	(2)	(3)
Dependent Variable: Drug Related Homicides			
Cumulative years funded	2.63***	2.70***	2.77***
	(0.86)	(0.86)	(0.87)
Cumulative years*state gov't same party	1.97	3.07	4.25
	(2.28)	(3.06)	(3.73)
2007 mean of 2008 eligibles	3.93	3.93	3.93
Initial eligibility index polynomial controls	Linear	Quadratic	Cubic
Year FE	Y	Y	Y
State FE	Y	Y	Y
Observations	6200	6200	6200
Municipalities	1550	1550	1550

Note: Cumulative years funded is the total number of years funded from 2008 onwards and is instrumented with the 2008 eligibility indicator interacted an indicator for 2008 onwards. The running variable is the distance between the 2008 municipality's index and the state eligibility threshold in 2008. Linear, quadratic and cubic regressions control for flexible trends of the running variable on each side of the eligibility threshold. Includes population controls. Sample is for the 2007-2010 period and limited to municipalities within 300 units of the state's 2008 eligibility threshold. Standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1

Table 1.16: Distance to US Border and the Effect of the Program

	(1)	(2)	(3)
Dependent Variable: Drug Related Homicides			
Cumulative years funded	3.52***	3.52***	3.52***
	(1.06)	(1.06)	(1.06)
Cumulative years*close to US border	1.13	1.14	1.1
	(2.90)	(2.90)	(2.88)
2007 mean of 2008 eligibles	3.93	3.93	3.93
Initial eligibility index polynomial controls	Linear	Quadratic	Cubic
Year FE	Y	Y	Y
State FE	Y	Y	Y
Observations	6200	6200	6200
Municipalities	1550	1550	1550

Note: Cumulative years funded is the total number of years funded from 2008 onwards and is instrumented with the 2008 eligibility indicator interacted with an indicator for 2008 onwards. The running variable is the distance between the 2008 municipality's index and the state eligibility threshold in 2008. Linear, quadratic and cubic regressions control for flexible trends of the running variable on each side of the eligibility threshold. Close to US border is an indicator equal to 1 for municipalities in the fourth quartile of distance to the US border. Includes population controls. Sample is for the 2007-2010 period and limited to municipalities within 300 units of the state's 2008 eligibility threshold. Standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1

Robustness Tests

Table 1.17: Robustness to Municipality FE, Controlling for Non-Homicide Deaths and Excluding Border States

	(1)	(2)	(3)
Dependent Variable: Drug Related Homicides			
Cumulative years funded	3.36***	3.90***	4.48***
	(1.10)	(1.30)	(1.61)
2007 mean of 2008 eligibles	3.93	3.93	4.17
Initial eligibility index polynomial controls	Linear	Linear	Linear
Year FE	Y	Y	Y
State FE	-	Y	Y
Municipality FE	Y	-	-
Control for non-homicide deaths	-	Y	-
Excludes border states	-	-	Y
Observations	6200	6200	6200
Municipalities	1550	1550	1550

Note: Cumulative years funded is the total number of years funded from 2008 onwards and is instrumented with the 2008 eligibility indicator interacted with an indicator for 2008 onwards. The running variable is the distance between the 2008 municipality's index and the state eligibility threshold in 2008. Linear, quadratic and cubic regressions control for flexible linear trends of the running variable on each side of the eligibility threshold. Includes population controls. Sample is for the 2007-2010 period and limited to municipalities within 300 units of the state's 2008 eligibility threshold. Standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1

Chapter 2

Criminal Behavior When There are No Jobs: Evidence from the Latest Great Recession

with Santiago Guerrero

ABSTRACT

The overall effect of unemployment on crime is theoretically ambiguous. Higher levels of unemployment have two opposing effects on crime. On the one hand more unemployment decreases the opportunity cost of committing a crime, which leads to an increase in crime. On the other hand, committing a crime in places with high unemployment becomes harder as income decreases and people stay more in their homes and are thus less exposed to robbery and other types of crime, which leads to a reduction in crime. Ultimately, the question of the impact of unemployment on crime is an empirical question. Previous studies have found mixed results. One major limitation of previous studies is that of reverse causality, as well as issues of omitted variables. In order to circumvent these issues, this study uses the variation in unemployment induced by the late 2000s recession in Mexico. The recession led to sharp differential increases in unemployment across metropolitan areas. We postulate these differential increases in unemployment are, for the most part, exogenous. Using an instrumental variables approach, there exists evidence that, in the case of Mexico, increases in unemployment lead to decreases in theft and fraud. There is no evidence that unemployment had an effect on other types of crime, such as property damage, sexual assaults and kidnappings.

1 Introduction

Theoretical models do not give a definite prediction on the effect of unemployment on crime. The direction of the effect depends on whether the decrease in the opportunity cost of crime effect, associated with higher unemployment rates, dominates the decrease in the supply of potential targets effect; which of these effects is stronger in turn depends on the probability of apprehension. Using an opportunity cost model, Becker's 1967 seminal paper predicts that an increase in unemployment should lead to an increase in crime. Given the conflicting results of empirical studies, Cantor and Land (1985) develop a structural model in which they combine the opportunity cost model with a criminal opportunity model; they argue that the effect is ambiguous since the opportunity cost effect increases crime as unemployment increases and at the same time the increase in unemployment leads to a decrease in income and a decrease in potential targets, as individuals remain in their neighborhoods and are able to protect their property, which leads to a decrease in crime. Lee (2009) formalizes the predictions of Cantor and Land (1985) and further incorporates apprehension rates into the model predicting that the

effect of unemployment on crime depends on the probability of being caught. That is, when the probability of being apprehended is low, the decrease in income and the decrease in the supply of potential targets dominate the opportunity cost effect, leading to a decrease in crime rates as unemployment increases; when the probability of being caught committing a crime is high, the opportunity cost effect dominates, with increases in unemployment increasing crime rates.

Most empirical studies have been conducted in the context of developed countries and have produced mixed results depending on the econometric specification and on the type of crime being considered. In the United States, studies using panel data have found that unemployment leads to an increase in property crime but the results are not consistent for other types of crime, with some studies finding a positive effect and others statistically insignificant effects (Lee, 1993; Levitt, 1996, 1997). Raphael and Winter-Ebmer (2000) use oil shocks and military base closures in the US as an instrument for crime and again find a positive effect on property crime but no clear relationship for violent crimes. One major limitation of this study is that they do not control for government expenditures and other studies have shown that oil shocks is positively related to government expenditures (Petanlar and Sadeghi 2012), in which case the exclusion restriction would be violated. Using a random effects and fixed effects model and data for New Zealand, Papps and Winkelmann (1998) find that when time and region fixed effects are included, only dishonesty, sexual offences and administrative offenses remain positive and statistically significant, but not other offenses.

Most of previous studies suffer from omitted variables biases, as well as from reverse causality. Common to these studies is the omission of the role of law enforcement on crime rates. In contrast to previous studies, we overcome those limitations by exploiting the sharp differential increase in unemployment caused by the late 2000s recession in Mexico across metropolitan areas. This differential increase provides a plausibly exogenous change in unemployment in a fixed effects framework. We will analyze the effect of unemployment on the different types of crime between 2005 and 2010. We will present results from two-stage least squares (2SLS) estimates using initial employment in manufacturing and tourism as a percentage of total employment interacted with lagged US GDP levels as instruments for current unemployment. Given the use of instrumental variables, we circumvent the issues of omitted variables persistent in most studies as well as issues of reverse causality. Preliminary results show that, in the case of Mexico, increases in unemployment lead to decreases in theft and fraud; however estimates on property damage, sexual assaults and kidnappings are not statistically significant. The paper is organized as follows. Section 2 provides a description of unemployment and crime in Mexico, section 3 describes the data sources, section 4 describes the identification strategy, section 5 presents the results and finally section 6 concludes.

2 Changes in unemployment and crime

In this study we will use the sharp differential changes in unemployment during the second quarter of 2008 to the fourth quarter of 2009, when there is a sharp increase in unemployment followed by a sharp decrease by the end of 2009. Figure 2.1 plots national unemployment rates and crime rates for the different types of crime. Between the second quarter of 2008 and the third quarter of 2009 we see a striking increase in unemployment followed by a sharp decrease; on the other hand, crime appears to decrease as unemployment increases, particularly for theft. Figures 2.2 and 2.3 show the trends in unemployment rates and the different types of crime from 2005 to 2010 by changes in unemployment. The metropolitan areas are divided into low, medium and high change areas based on quartile changes in unemployment from the second quarter of 2008 to the

third quarter of 2009, when the recession officially ended. Those in the first quartile are classified as low change in unemployment areas and those in the fourth are classified as high change areas, the rest are classified as middle change areas. The quartile of metropolitan areas with the highest increase in unemployment had an over 100% increase in unemployment between the second quarter of 2008 and the third of 2009, those in the middle had an approximately 55% increase in unemployment and those in the lowest quartile had a 30% increase in unemployment. As shown in Figure 2.2, previous to the recession, all three groups follow similar trends in unemployment.

Figure 2.3 shows that the areas with the highest change in unemployment in the period 2008q2-2009q3 had the largest decrease in every type of crime with the exception of kidnappings. Given that all areas experienced changes in unemployment, they all seem to have been affected in their crime rates by these changes in unemployment. Also, the three groups had similar trends in crime before the onset of the recession. Based on this evidence, there seems to be a strong link between unemployment and crime, thus it appears that in the case of Mexico the decrease in resources effect associated with increasing unemployment dominates the opportunity cost effect. That increasing unemployment leads to a decrease of potential targets is supported by the fact that according to the 2011 National Survey of Earnings and Expenditures (ENIGH by its Spanish acronym) Main Results Report done by the INEGI, from 2008 to 2010 average quarterly earnings decreased by 12.3%, with the top 10% of earners experiencing a decrease of 17.8% in average quarterly earnings and the bottom 10% experiencing a 7.6% decrease. Moreover, according to the 2009 Time Use Survey done by the INEGI, employed individuals spend twice as much time doing activities outside of the house than unemployed individuals and this difference is statistically significant.

3 Data Sources

We use a comprehensive dataset that allows us to control for various factors. Quarterly city unemployment figures come from the National Employment Survey (ENOE by its Spanish acronym) done by the National Institute of Statistics and Geography (INEGI by its Spanish acronym). Data on registered suspects come from the INEGI. A registered suspect is defined as an individual who has been accused of a crime but who has not yet been convicted. These data includes the type of crime for which the suspect is being charged, age of the suspect, the month of occurrence of the crime and the municipality of occurrence of the crime. We limit our analysis to the most common types of crime: theft, property damage, fraud, sexual assault, and kidnapping. The registered suspects for these types of crime account for 72% of the total number of registered suspects in Mexico during our study period. The percentage of employment in manufacturing and tourism comes from the ENOE tabulation reports done by the INEGI. Quarterly level data on US GDP come from the US Bureau of Economic Analysis. Population data come from the 2005 and 2010 INEGI Population Census; a linear projection of population will be used to estimate population at the municipality level between census years. Given that unemployment data is only available at the city level, all of our variables will be aggregated at the city and quarterly level.

Table 2.1 displays the summary statistics for the 2005q1-2010q4 period. As can be seen from the table, the most common types of crime are theft, property damage and fraud. Moreover, there is significant variation in the unemployment rate across the metropolitan areas in our sample as well as variation in the share in manufacturing and tourism labor share.

4 Estimation Strategy

Using the sharp differential changes in unemployment during the recession across metropolitan areas and given that areas with a large labor of manufacturing and tourism saw the largest increases in unemployment due to their dependence on the use economy, we will estimate a two stage least squares (2SLS). In order to estimate the effects of unemployment on crime first we will first use an OLS estimator. The estimation equation is:

$$\text{Crime}_{it} = \alpha + \beta\text{Unemployment}_{it} + \omega X_{it} + \rho_i + \varphi_t + \varepsilon_{it} \qquad (1)$$

Crime_{it} is the quarterly crime rate, defined as the number of registered suspects per 100,000 residents for city i in quarter t. $\text{Unemployment}_{it}$is the quarterly unemployment rate for city i. In some specifications X_{it} controls for shocks in government expenditures, such as expenditures on services, which includes public safety expenditures, wages for government workers, subsidies and investments at the city level. Also included are time fixed effects $\emptyset_t$ that control for unobserved confounders affecting all cities in the same period as well as city fixed effects that control for non-time varying unobserved city characteristics that affect unemployment and crime. We estimate equation (1) for the pooled data as well as for each crime category: theft, kidnappings, sexual assault, property damage and fraud. The identification assumption is that unobserved factors are not correlated with unemployment, conditional on the covariates; that is that $E[\text{unemployment}_{it} * \varepsilon_{it}|X_{it}, \rho_i, \varphi_t]=0$. Despite the short run nature of the analysis and that that the economic shock was mostly unexpected, our results might still be biased due to omitted variables, reverse causality and measurement error. Reverse causality could bias our estimates if, for instance, increases in crime cause firms to leave, which would increase unemployment, or if increases in crime increase unemployment due to former convicted criminals having difficulty finding jobs. In order to address these issues we will instrument changes in unemployment with initial manufacturing and tourism labor share interacted with lagged quarterly US GDP.

The second stage equation is:

$$\text{Crime}_{it} = \alpha + \beta\text{Unemployment}_{it} + \omega X_{it} + \rho_i + \varphi_t + \varepsilon_{it} \qquad (2)$$

The first stage equation is given by:

$$\text{Unemployemnt}_{it} = \beta \frac{L^{M}_{i2005q1} + L^{T}_{i2005q1}}{L^{total}_{i2005q1}} * \text{US GDP}_{t-1} + \omega X_{it} + \varphi_t + \rho_i + \epsilon_{it} \qquad (3)$$

Where unemployment, Unemployment_{it}, is instrumented with employment in manufacturing and tourism in the first quarter of 2005, when our sample period starts, in city i, as a percentage of total employment in the first quarter of 2005 in city i, interacted with lagged quarterly US GDP. We will also estimate the effect using only the manufacturing labor share interacted with lagged US GDP as an instrument for unemployment to check that the results are not driven only by manufacturing or only by tourism areas. The identification assumes that the shocks in unemployment are predetermined, our instrument cannot be correlated with omitted variables; that is that $E[\varepsilon_{it}|X_{i1}, \dots, X_{iT}]=0$. Given the size of the US economy and the nature of the latest great

recession in the US, we would expect changes in unemployment in Mexico coming from changes in US GDP to be mostly exogenous, that is it is unlikely that Mexico's unemployment rate could significantly influence US GDP during the latest 2008 recession.

5 Results

Table 2.2 and 2.4 show OLS and IV results for the effect of unemployment on total crime, where all five crime categories, theft, fraud, property damage, kidnappings and sexual assaults, are aggregated.

As we control for government expenditures, which, among other categories, includes expenditures for security personnel and other security related expenditures, the point estimate becomes smaller, suggesting that there are time varying unobservables that affect unemployment and crime. It could be, for instance, that as unemployment increases local governments have less resources and thus spend less on policing which in turn increases crime, which would bias our estimates upwards. In table 2.2, none of the fixed effects estimates are statistically significant. However, due to reverse causality and omitted variables, OLS estimates are likely to be biased, moreover measurement error would further bias our estimates towards zero. Also note that column (1) does not include time fixed effects and the results are highly significant; it does not seem that we have enough cross sectional variation as would be needed to identify an effect. Table 2.3 shows the first stage results. It shows the effect of lagged US GDP interacted with the initial manufacturing and tourism labor share on unemployment. As expected, due to the high volume of trade between the US and Mexico, according to the INEGI in 2004 exports from Mexico to the US accounted for more than 87% of Mexico's total exports, increases in US GDP lead to highly significant decreases in unemployment in Mexico; when the US economy expands it leads to an increase in demand for Mexican manufacturing imports, Mexico being one of the most important trading partner for the US, as well as tourism to Mexico which in turn decreases unemployment in metropolitan areas in Mexico that have a high share of their labor force employed in manufacturing and tourism. Moreover, not only is the effect highly significant but the first stage is strong with an F statistic of approximately 20. Table 2.5 shows the 2SLS results and the OLS results for comparison purposes. The IV result in column (3) and (4) are highly significant, suggesting that a 1% increase in unemployment leads to a decrease in the overall crime rate of more than 7. This estimate implies an almost 30% decrease in the overall crime rate over the average for a 1% increase in unemployment. However, given that the mean change in unemployment is 2.6 during the recession, this estimate implies that the recession is responsible for more than 100% of the decrease in total crime; thus the estimate seems implausibly large and the magnitude of the estimate should be taken with caution.

Tables 2.5 to 2.9 show the results for the crime rate of each type of offense. Columns (1) and (2) show the OLS results without and with government expenditures controls and columns (3) and (4) show the IV results without and with government expenditures controls. Again the OLS estimates are not significant and close to zero. Once we instrument for unemployment we see that increases in unemployment lead to large decreases in theft and fraud. The tables also show that once we control for government expenditures increases in unemployment lead to even larger decreases in crime, which is consistent with increases in unemployment leading to a decrease in expenditures on public safety and social programs which in turn increases crime. The effect on property damage, sexual assault and kidnappings is never statistically significant. Property damage and sexual assault crimes are typically not motivated by income, thus

it appears that decreases in income, more than the decrease in targets by individuals engaging less in activities outside the home, is what is driving the results on theft and fraud. The effect on kidnappings is never statistically significant and close to zero.

Table 2.10 and 2.11 show the first stage results when instrumenting with the baseline manufacturing labor share interacted with US GDP and the second stage results, respectively. A concern would be that are results are driven by tourism areas which are experiencing shocks in crime and unemployment but that these shocks are unrelated to changes in criminal behavior as unemployment increases. We again see that increases in US GDP lead to disproportionate and significant decreases in unemployment and manufacturing areas and the first stage is strong with an F statistic of 18. The second stage results are similar to the result when manufacturing and tourism and US GDP are used as instruments. Thus, it does not seem that our results are driven by only tourism or only manufacturing areas, which increases our confidence that increases in unemployment lead to changes in criminal behavior and that our results are not driven by unobserved shocks in any particular area.

6 Conclusion

Using the sharp variation in unemployment induced by the latest recession we find that, in the case of Mexico, there exists some evidence that increases in unemployment lead to decreases in theft and fraud. In order to address issues of reverse causality, omitted variables and measurement error in unemployment figures we instrument unemployment with the manufacturing and tourism labor share interacted with US GDP. The results are consistent with the decrease in potential targets as unemployment increase effect outweighing the decrease in the opportunity cost of committing a crime. This seems to be particularly relevant for crimes where individuals can earn an income by engaging in such types of crime, namely theft and fraud. The results are similar when instrumenting with the manufacturing and tourism labor share interacted US GDP and when instrumenting with the manufacturing labor share interacted with US GDP, thus the results do not seem to be driven by only tourism or only manufacturing areas.

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Tables

Table 2.1: Sample Summary Statistics

	Obs	Mean	St Dev	Min	Max
Theft per 100,000 residents	874	19.572	12.140	0.000	72.229
Quarter to quarter change in theft per 100,000 residents	874	-.463	5.147	-30.518	36.116
Total fraud per 100,000 residents	874	2.144	1.266	0.000	13.519
Quarter to quarter change in fraud per 100,000 residents	874	-.071	.873	-7.267	8.318
Total property damage per 100,000 residents	874	5.078	92.793	0.000	672.000
Quarter to quarter change in property damage per 100,000 residents	874	-.057	2.220	-25.916	28.262
Total sexual assault per 100,000 residents	874	1.843	40.106	0.000	296.000
Quarter to quarter change in sexual assault per 100,000 residents	874	-.031	.574	-2.435	3.772
Total kidnapping per 100,000 residents	874	1.112	10.461	0.000	71.000
Quarter to quarter change in kidnappings per 100,000 residents	874	.0008	.262	-1.190	1.222
Unemployment rate	874	4.901	1.605	1.774	11.080
Quarter to quarter change in unemployment	874	0.211	0.871	-3.012	2.820
2005q1 manufacturing and tourism labor share	874	0.233	0.063	0.127	0.397
2005q1 manufacturing labor share	874	0.158	0.071	0.045	0.333
2007q4 tourism labor share	874	0.075	0.027	0.047	0.205
Quarterly real US GDP (billions)	874	12916.290	223.756	12701.000	13311.000
Quarter to quarter change in real US GDP (billions)	874	-56.286	139.117	-303.000	126.000

The statistics are at the quarterly level and are for the 2005q1 to the 2010q4 period. According to the 2010 census total population was 112,336,538. Our sample accounts for 93,939,926 and excluded municipalities account for 18,396,612 of the total population in the country. That is, our municipalities account for 84% of the 2010 population in the country.

Table 2.2: OLS Estimates of the Effect of Unemployment on Crime

Dependent variable: total crime rate (per 100,000 residents)			
	(1)	(2)	(3)
Unemployment rate	-2.944***	0.104	0.065
	(0.864)	(1.143)	(1.143)
Metropolitan area FE	Y	Y	Y
Quarter FE	N	Y	Y
Government expenditures controls	N	N	Y
Dependent variable mean	24.741	24.741	24.741
Observations	874	874	874
Number of metropolitan areas	38	38	38

Values are coefficients from ordinary least squares regressions. Total crime rate is defined as the sum of suspects of theft, property damage, fraud, sexual assault and kidnapping per 100,000 residents. Standard errors are in parentheses and allow for heteroskedasticity and metropolitan area level correlation in the errors. Sample is for the first quarter of 2005 to the fourth quarter of 2010. *significant at 10% * *significant at 5% *** significant at 1%

Table 2.3: First Stage Results

Dependent variable: Unemployment rate		
	(1)	(2)
05q1 Manufacturing and tourism labor share*US GDP, t-1	-0.0005***	-0.0004***
	(0.0001)	(0.0001)
Metropolitan area FE	Y	Y
Quarter FE	Y	Y
Government expenditures controls	N	Y
F statistic	20.312	11.054
R-Squared	0.824	0.827
Observations	874	874
Number of metropolitan areas	38	38

Standard errors are in parentheses and allow for heteroskedasticity and metropolitan area level correlation in error term. Sample is for the first quarter of 2005 to the fourth quarter of 2010. *significant at 10% * *significant at 5% *** significant at 1%

Table 2.4: IV Estimates of the Effect of Unemployment on Crime

Dependent variable: total crime rate (per 100,000 residents)				
	(1)	(2)	(3)	(4)
	OLS	OLS	IV	IV
Unemployment rate	0.104	0.065	-7.563**	-9.525***
	(1.143)	(1.143)	(3.234)	(3.603)
Metropolitan area FE	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y
Government expenditures controls	N	Y	N	Y
Dependent variable mean	24.741	24.741	24.741	24.741
Observations	874	874	874	874
Number of metropolitan areas	38	38	38	38

Values are coefficients from two-stage least squares regressions. Total crime rate is defined as the sum of suspects of theft, property damage, fraud, sexual assault and kidnapping per 100,000 residents. Unemployment level is instrumented by initial 05q1 manufacturing and tourism labor share interacted with lagged quarterly US GDP. Sample is for the first quarter of 2005 to the fourth quarter of 2010. Standard errors are in parentheses and allow for heteroskedasticity and metropolitan area level correlation in the differences errors. *significant at 10% * *significant at 5% *** significant at 1%

Table 2.5: IV Estimates of the Effect of Unemployment on Theft

Dependent variable: theft rate (per 100,000 residents)				
	(1)	(2)	(3)	(4)
	OLS	OLS	IV	IV
Unemployment rate	0.323	0.282	-5.658**	-7.582**
	(0.959)	(0.960)	(2.507)	(3.097)
Metropolitan area FE	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y
Government expenditures controls	N	Y	N	Y
Dependent variable mean	19.572	19.572	19.572	19.572
Observations	874	874	874	874
Number of metropolitan areas	38	38	38	38

Values are coefficients from two-stage least squares regressions. Theft rate is defined as the number of registered suspects for theft per 100,000 residents. Unemployment level is instrumented by initial 05q1 manufacturing and tourism labor share interacted with lagged quarterly US GDP. Sample is for the first quarter of 2005 to the fourth quarter of 2010. Standard

errors are in parentheses and allow for heteroskedasticity and metropolitan area level correlation in the differences errors. *significant at 10% * *significant at 5% *** significant at 1%

Table 2.6: Estimates of the Effect of Unemployment on Fraud

Dependent variable: fraud rate (per 100,000 residents)				
	(1)	(2)	(3)	(4)
	OLS	OLS	IV	IV
Unemployment rate	-0.037	-0.038	-0.755***	-0.677***
	(0.068)	(0.070)	(0.282)	(0.254)
Metropolitan area FE	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y
Government expenditures controls	N	Y	N	Y
Dependent variable mean	2.144	2.144	2.144	2.144
Observations	874	874	874	874
Number of metropolitan areas	38	38	38	38

Values are coefficients from two-stage least squares regressions. The fraud rate is defined as the number of registered suspects for fraud per 100,000 residents. Unemployment level is instrumented by initial 05q1 manufacturing and tourism labor share interacted with lagged quarterly US GDP. Sample is for the first quarter of 2005 to the fourth quarter of 2010. Standard errors are in parentheses and allow for heteroskedasticity and metropolitan area level correlation in the differences errors. *significant at 10% * *significant at 5% *** significant at 1%

Table 2.7: Estimates of the Effect of Unemployment on Property Damage

Dependent variable: property damage rate (per 100,000 residents)				
	(1)	(2)	(3)	(4)
	OLS	OLS	IV	IV
Unemployment rate	-0.157	-0.161	-1.331	-1.427
	(0.243)	(0.242)	(1.005)	(1.004)
Metropolitan area FE	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y
Government expenditures controls	N	Y	N	Y
Dependent variable mean	5.070	5.070	5.070	5.070
Observations	874	874	874	874
Number of metropolitan areas	38	38	38	38

Values are coefficients from two-stage least squares regressions. The property damage rate is defined as the number of registered suspects for property damage per 100,000 residents. Unemployment level is instrumented by initial 05q1 manufacturing and tourism labor share interacted with lagged quarterly US GDP. Sample is for the first quarter of 2005 to the fourth quarter of 2010. Standard errors are in parentheses and allow for heteroskedasticity and metropolitan area level correlation in the differences errors. *significant at 10% * *significant at 5% *** significant at 1%

Table 2.8: Estimates of the Effect of Unemployment on Sexual Assaults

Dependent variable: sexual assault rate (per 100,000 residents)				
	(1)	(2)	(3)	(4)
	OLS	OLS	IV	IV
Unemployment rate	-0.0354	-0.031	0.203	0.197
	(0.056)	(0.050)	(0.126)	(0.167)
Metropolitan area FE	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y
Government expenditures controls	N	Y	N	Y
Dependent variable mean	1.843	1.843	1.843	1.843
Observations	874	874	874	874
Number of metropolitan areas	38	38	38	38

Values are coefficients from two-stage least squares regressions. The sexual assault rate is defined as the number of registered suspects for sexual assault per 100,000 residents. Unemployment level is instrumented by initial 05q1 manufacturing and tourism labor share interacted with lagged quarterly US GDP. Sample is for the first quarter of 2005 to the fourth quarter of 2010. Standard errors are in parentheses and allow for heteroskedasticity and metropolitan area level correlation in the differences errors. *significant at 10% * *significant at 5% *** significant at 1%

Table 2.9: Estimates of the Effect of Unemployment on Kidnappings

Dependent variable: sexual assault rate (per 100,000 residents)				
	(1)	(2)	(3)	(4)
	OLS	OLS	IV	IV
Unemployment rate	-0.0354	-0.031	0.203	0.197
	(0.056)	(0.050)	(0.126)	(0.167)
Metropolitan area FE	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y
Government expenditures controls	N	Y	N	Y
Dependent variable mean	1.843	1.843	1.843	1.843
Observations	874	874	874	874
Number of metropolitan areas	38	38	38	38

Values are coefficients from two-stage least squares regressions. The kidnapping rate is defined as the number of registered suspects for kidnapping per 100,000 residents. Unemployment level is instrumented by initial 05q1 manufacturing and tourism labor share interacted with lagged quarterly US GDP. Sample is for the first quarter of 2005 to the fourth quarter of 2010. Standard errors are in parentheses and allow for heteroskedasticity and metropolitan area level correlation in the differences errors. *significant at 10% * *significant at 5% *** significant at 1%

Table 2.10: First Stage Results Manufacturing Labor Share

Dependent variable: Unemployment rate	
05q4 Manufacturing labor share*US GDP, t-1	-0.0004***
	(0.0001)
Metropolitan area FE	Y
Quarter FE	Y
Government expenditures controls	N
F statistic	18.412
R-Squared	0.824
Observations	874
Number of metropolitan areas	38

Standard errors are in parentheses and allow for heteroskedasticity and metropolitan area level correlation in the error term.. Sample is for the first quarter of 2005 to the fourth quarter of 2010. *significant at 10% * *significant at 5% *** significant at 1%

Table 2.11: IV Estimates of Unemployment on Crime

	(1)	(2)	(3)	(4)	(5)
	IV Theft	IV Fraud	IV Property Damage	IV Sexual Assault	IV Kidnapping
Unemployment rate	-5.476**	-0.673**	-1.294	0.188	-0.010
	(2.385)	(0.301)	(0.984)	(0.131)	(0.050)
Metropolitan area FE	Y	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y	Y
Government expenditures controls	N	N	N	N	N
Dependent variable mean	19.572	2.144	5.07	1.843	1.112
Observations	874	874	874	874	874
Number of metropolitan areas	38	38	38	38	38

Values are coefficients from two-stage least squares regressions. The dependent variable in each column are defined as the number of registered suspects for theft, fraud, property damage, sexual assault and kidnappings per 100,000 residents. Unemployment level is instrumented by initial 05q1 manufacturing labor share interacted with lagged quarterly US GDP. Sample is for the first quarter of 2005 to the fourth quarter of 2010. Standard errors are in parentheses and allow for heteroskedasticity and metropolitan area level correlation in the differences errors. *significant at 10% * *significant at 5% *** significant at 1%

Figures

Figure 2.1: National Trends in Unemployment and Crime

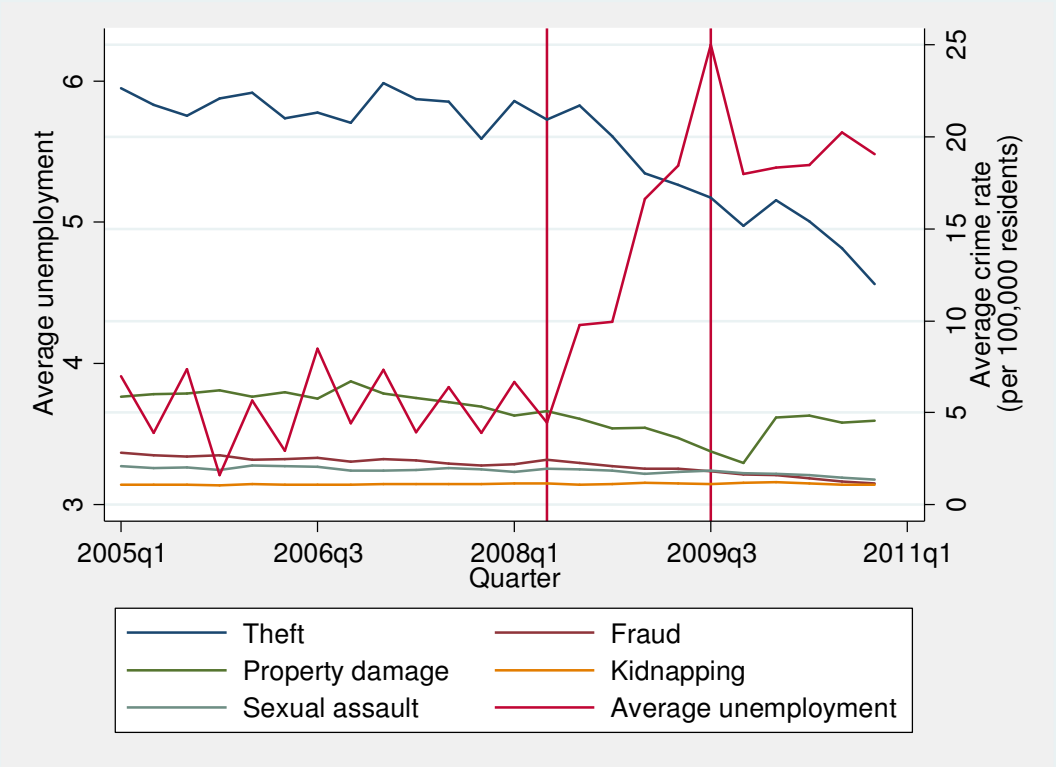


Figure 2.2: Trends in Average Unemployment by 2008q2-2009q3 Change in Unemployment

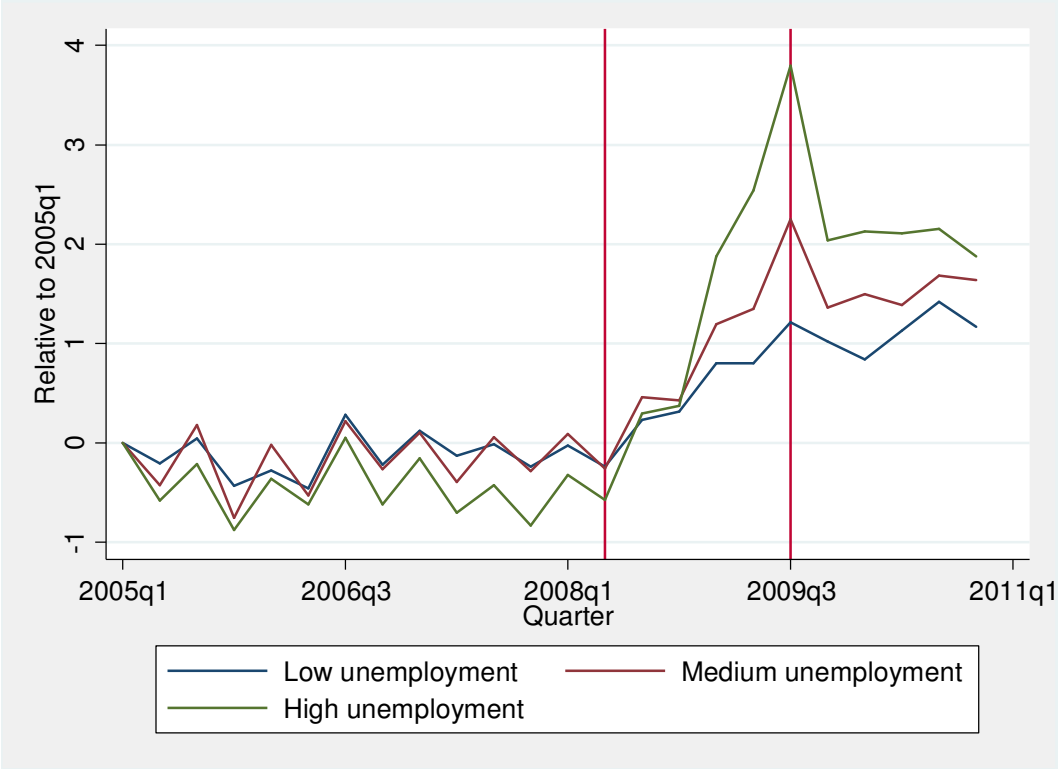
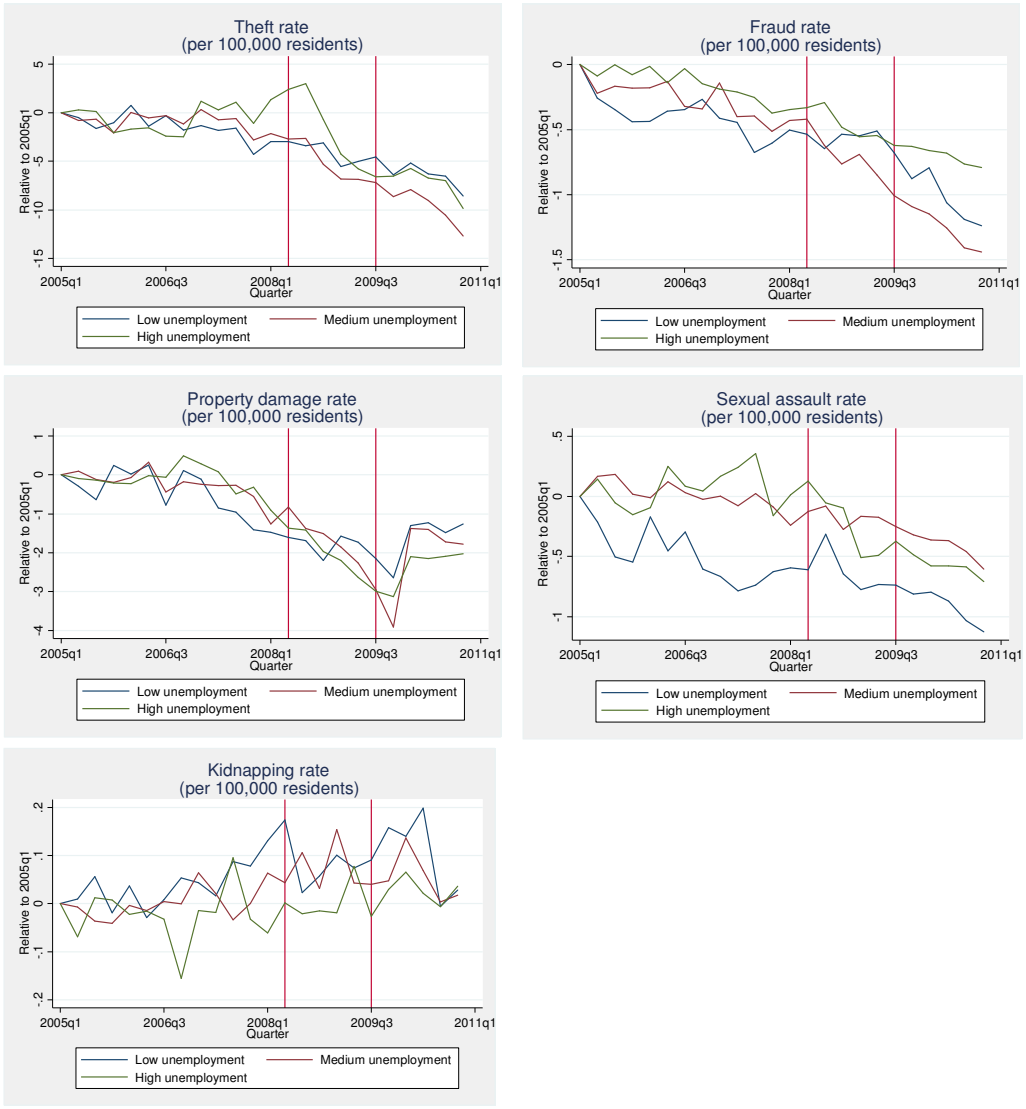


Figure 2.3: Trends in Crime (Relative to 2005q1) by 2008q2-2009q3 Change in Unemployment



Chapter 3

Does Sulfur Content in Diesel Affect Infant Mortality? Evidence from Mexico's Introduction of Ultra Low Sulfur Fuels

ABSTRACT

This study assesses the impact of sulfur content regulation of diesel on infant mortality in Mexico. Starting in 2006, ultra-low sulfur (ULS) gasoline and diesel began to be available in various Mexican metropolitan areas. Sulfur content regulation in fuels is important not only because it directly reduces sulfur dioxide emissions, but because, with the appropriate emissions control technology, it also affects the presence of other particulate matter in the air, which can have large impacts on infant health and mortality. I will use the introduction of ultra-low sulfur diesel in Mexican border cities, as a source of variation in air quality to investigate the impact of regulation of sulfur content in fuels on infant mortality. Determining the benefits of introducing cleaner gasoline is of particular importance in Mexico, where gasoline is subsidized by the government and improvements in the quality of gasoline are not fully internalized by the market. This will be one of the few studies in the context of Latin America to estimate the effects of sulfur content regulation on infant mortality. Despite its potential to improve health outcomes, I find that that sulfur regulation had no substantial effect on infant mortality outcomes.

3.1 Introduction

During the last decade developed countries have adopted low sulfur gasoline and diesel, with the US adopting in late 2006. However, most developing countries still have high levels of sulfur in gasoline and diesel. EPA studies have documented adverse effects from short term exposure to sulfur dioxide on hospital admissions, particularly for children and the elderly. US EPA studies have shown that the benefits of adoption of ULSD (ultra-low sulfur diesel), which include avoided premature deaths and hospital admissions from respiratory illnesses, when combined with other vehicle emission standards, are more than ten times higher than the costs; this benefit calculations are a lower bound, since effects on infant premature deaths are only included in supplemental calculations due to the fact that the EPA sees the effects on infant mortality as uncertain since there is only one study documenting the impacts of pollution on infant mortality, namely that of Woodruff et al. (1997) (EPA 2005). Regulation of sulfur content in fuels directly reduces sulfur dioxide emissions as well as other contaminants in the air, such as particulate matter and oxides of nitrogen, since high levels of sulfur in fuels impede the proper functioning of emissions control technologies in vehicles. Hence, in order for other policies regarding vehicle emissions control to be efficient, first the sulfur content in fuels has to be regulated. According to the EPA, in the US, the introduction of ULSD, coupled with other pollution control technologies, has resulted in 90% lower harmful emissions from heavy duty vehicles. In Mexico, sulfur oxides are the second largest emitted pollutants from

anthropogenic sources. The Secretariat of the Environment and Natural Resources of Mexico (SEMARNAT by its Spanish acronym) has estimated benefits of $11,373 millions of dollars compared to costs of $4,683 millions of dollars from the implementation of ULS gasoline and diesel in Mexico; this estimate includes avoided premature deaths based on the Wooruff et al (1997) study and it is estimated that in 2007, 11 infant premature deaths would be avoided due to the regulation and in 2008 12 would be avoided (SEMARNAT 2006). In 2006, treated municipalities accounted for 105 respiratory related deaths of infants one years of age and younger; assuming perfect compliance, this could potentially mean an approximate 10% reduction in premature deaths.

In recognition of the potential benefits that the introduction of low sulfur fuels has on health, in 2006 Mexico passed norm 086-SEMARNAT-SENER-SCFI-2005, which specified the content of sulfur in fuels. It established a maximum sulfur content in diesel of 15ppm, down from 500 ppm, and a maximum of 80 ppm for premium and magna gasoline, down from a previous maximum of 500 ppm. The average for premium gasoline was established at 30 ppm, down from 250 ppm, and for magna from 300 to 30 ppm. Starting in 2006, ULS gasoline and diesel began to be available in various metropolitan areas. In January 2007 all of the border cities had fully implemented ULSD. The initial goal was to have ultra low sulfur fuels available throughout the territory by 2009; however this date has now been moved to 2015. Please refer to table 3.1 for a list of municipalities which have ultra low sulfur fuels available as well as for the date of introduction. Although PEMEX plans to make significant investments in new technologies in order to have domestically produced ULS fuels, to date, the internal demand has been met by imports.

This study assesses the impact of sulfur content in diesel on infant mortality using Mexico's introduction of ULSD as a source of exogenous variation in sulfur dioxide emissions and other pollutants. Implementation of ULSD is unrelated to infant mortality in treated municipalities and it is likely due to the proximity of these municipalities with the US. I use a comprehensive data set consisting of 88 Mexican municipalities that constitute the major economic regions in the country, allowing me to control for various monthly economic indicators, monthly weather and monthly fuel sales. I use a difference in difference estimator, controlling for time varying observables in treated versus control municipalities. Sulfur content regulation is estimated to have no statistically significant effect on infant mortality.

The remainder of the paper is organized as follows: Section 2 provides background information. Section 3 describes the data used and section 4 presents the identification strategy. Section 5 shows the results, section 6 presents a discussion and section 7 concludes.

3.2 Background

3.2.1 Mexico's Petroleum Industry

PEMEX, the state owned oil company, is in charge of refining, storing, distributing and marketing gasoline and diesel. The retailers are PEMEX franchise service stations. There are three types of fuels sold: diesel, magna gasoline, with 87 octanes, and premium gasoline, with 92 octanes. The prices are set by the Ministry of Finance and revised on a monthly basis. In 2008, the price of premium gasoline was, on average, 13% higher than that of magna. In 2009 Diesel accounted for approximately 28% of the total national volume of sales, premium gasoline accounted for 5% and magna gasoline accounted for

67%. Figure 1 shows that national sales of magna gasoline have been steadily increasing, while those for premium have declined; sales of diesel have remained fairly constant over time.

3.2.2 Literature Review

Studies have found that high levels of air pollution have a negative effect on infant mortality. Studies have used cross sectional variation, time series variation and cohort analysis to determine the impact of air quality on infant health, but in all cases there are confounding factors and the results are sensitive to including various controls. The seminal paper on this subject is that of Chay et al due to its clean identification strategy. Taking advantage of the variation in pollution due to economic shocks, Chay et al. find that in the US a $1mg/m^3$ reduction in particulate matter decreases infant deaths by approximately 4-8 per 100,000 births (Chay et al. 1999). Currie et al (2004) find that, in California, reductions in CO and PM10 resulted in the avoidance of over 1,000 premature infant deaths in the 1990s.

However, to my knowledge, there have not been studies on the effects of sulfur content regulation on infant mortality. Assessing the effects of sulfur regulation on infant health outcomes in a Latin American context is important given the relatively high rates of birth and the high levels of sulfur content in fuels throughout Latin America. Furthermore, doing it in the context of Mexico has an additional advantage since the price of gasoline is fixed by the government at the regional level, which allows me to isolate the impacts of introducing ULS fuels from changes in demand induced by unobserved factors.

3.3 Data

Mortality records and birth registration records were obtained from the Ministry of Health. Mortality records include, among other information, locality of occurrence, municipality of occurrence, date of occurrence, age and cause. Birth registration records include information on municipality of residence of the mother, date of birth, birth characteristics, such as the order of birth, place where it occurred, whether it was a single, double or triple birth, who attended the mother at birth, as well as parental characteristics, such the parent's education, occupation, age and marital status, among others. As seen in table 3.3, in my study period, which is from January 2005 to December 2008, my sample municipalities account for 5,239 out of a national total of 13,430 for respiratory related deaths of infants one year old and younger; they account for 3,168,514 total births during this period out of a national total of 8,074,204. Hence they account for 39% of the total respiratory related deaths for infants one year old and younger in the country and for 39% of the total births in the country. For fatalities due to respiratory illnesses for children 5 years of age and younger, they account for 5,809 out of a national total of 14,628. As seen from these figures, the majority of the deaths due to respiratory causes occur within the first year of birth. For this reason and because total births can be used as a proxy for the population at risk and to avoid migration concerns, this study will focus on the impacts for children one year old and younger.

Economic variables come from the National Institute of Statistics and Geography (INEGI). These data include monthly economic indices for whole sales, retail sales, whole sale purchases, retail sale purchases, remunerations in the whole sales sector, remunerations in the retail sector, employed personnel in the whole sales sector and

employed personnel in the retail sector[2]. Since these indices are for the municipalities where the major 32 Mexican metropolitan areas are encountered, I concentrate my study on these municipalities[3]. These are used to control for economic shocks that could impact infant mortality. Other studies also use economic characteristics since economic conditions can have a direct impact on infant health, through higher quality healthcare, nutrition and due to parent's overall habits, such as smoking (Chay et al 1999). Also economic shocks can affect pollution levels; for example when occupied personnel decreases this could potentially lead to lower pollution through decreased traffic congestion; also negative economic shocks in the area could decrease manufacturing levels and thus reduce pollution from plants.

I also control for gasoline sales and diesel sales. The Ministry of Finance increased the price of Diesel to cover part of the costs of importing ultra low sulfur diesel; hence when not controlling for diesel sales I am estimating the quality and quantity effect of the introduction of ULSD; controlling for it isolates the quality effect. Including the sales of gasoline controls for shocks; for instance, it could be that post regulation there was an increase in the sales of gasoline undermining the effect of the improved quality of diesel. Monthly volume sales of diesel, magna gasoline and premium gasoline are merged at the municipality level.

Since weather conditions affect not only respiratory related deaths directly but also pollution levels (Auffhammer et al 2009), I control for various weather variables; other studies looking at the impact of pollution levels on infant mortality also view weather as a potential confounder. Weather data comes from the National Climatic Data Center's Cooperative Station Data (NOAA), which includes daily average temperature, maximum temperature, minimum temperature, wind speed, dew point and precipitation for more than 100 weather stations in Mexico. To obtain monthly weather I take the average of the average daily temperature, the maximum of the maximum temperature and the minimum of the minimum temperature, average wind speed, average dew point and total precipitation for the month. I match the closest weather station to each locality using the Vincenty distance. Using this algorithm, the average distance from the sample localities to the respective closest weather station is 12 miles, with a standard deviation of 8 miles. To obtain weather conditions at a given municipality, I take the average of these weather variables, except for rainfall where I take the sum, for all the localities in a given municipality[4].

A concern could be that municipalities that have a high percentage of months with zero outcomes are fundamentally different in an unobserved manner from those that have a higher percentage and hence are not appropriate controls. Since the data generating process for these municipalities could be different, I exclude municipalities that in any given year have zero outcomes. I also tried trimming the sample to those that have 60% of the months with zero outcomes, but results are similar to the non trimmed sample; hence these municipalities do not seem to be fundamentally different from the rest.

Table 3.2 displays the average characteristics of my sample municipalities compared to the rest of the country using data from the 2005 census. As expected, since these municipalities comprise the major metropolitan areas in the country, the level of education is higher, the proportion of the population who has access to healthcare is higher in the sample municipalities, the proportion of households with a female head is lower, and it is more likely that the house has running water drainage, electricity and a computer.

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[2] Figures for indices are CPI deflated. Base year 2003=100.

[3] The sample includes seven treated municipalities: Mexicali, Tecate, Tijuana, Juarez, Matamoros, Nuevo Laredo and Reynosa. All of which are border municipalities. There are 81 control municipalities: in 30 of which are the major cities in the 30 states and the rest of which are municipalities in Mexico City and the Federal District.

[4] In Mexico a municipality has various localities.

Table 3.3 provides summary information for the dependent variable, the economic variables and the parental and birth characteristics for the pretreatment period, 2005 and 2006, and the post treatment period, 2007 and 2008; most of these variables show changes from year to year, although some of them are small in magnitude.

Figure 2 plots the residuals of regressing pollution levels on month of the year dummies; these are plots of concentrations of PM10 and SO2 for Mexicali, Tijuana, both of which are treated cities, and Monterrey, a control city. SO2 concentrations appear to have fallen for Mexicali after the regulation, but at the same time PM10 concentrations increased. For Tijuana, they appear to be more or less similar. For Monterrey SO2 emissions also seem to have decreased, although not as much as in Mexicali, after January 2007 despite the fact that it was not treated; PM10 concentrations have been increasing as in Mexicali. Only a few cities have a monitoring station and from these figures it is not clear what the effects of the regulation would be on health outcomes. For instance, it could be that treatment cities experienced individual economic shocks in the post treatment period that increased vehicle circulation and manufacturing, increasing pollution levels despite the improved fuel quality.

3.4 Estimation Strategy

To estimate the impact of the regulation on infant mortality a difference in difference estimator is used. The basic estimation equation is:

$$\ln(y_{mt}) = \beta Treatment_{mt} + \boldsymbol{\theta W_{mt}} + \boldsymbol{\omega X_{mt}} + \boldsymbol{\gamma Fuel\_Sales_{mt}} + \boldsymbol{\alpha Z_{mt}} + \rho_m + \varphi_t + \varepsilon_{mt} \quad (1)$$

Where y_{mt} is the fatality rate, which is total fatalities due to respiratory illnesses of children one year old and younger divided by total births, in municipality m at time t, where t is either month or year. $Treatment_{mt}$ is an indicator equal to one in years in which municipality m is treated. Hence β is the coefficient of interest. $\boldsymbol{W_{mt}}$ is a vector of weather variables that controls for monthly weather shocks at the municipality level, $\boldsymbol{X_{mt}}$ is a vector that controls for economic shocks at the municipality level, $\boldsymbol{Fuel\_Sales_{mt}}$ is a vector of variables of fuel sales, which includes sales of diesel, magna gasoline and premium gasoline at the municipality level. $\mathbf{Z_{mt}}$ is a vector of variables that controls for birth and parental characteristics of municipality m at time t. ρ_m are municipality fixed effects, that control for time invariant unobserved municipality characteristics that cause municipalities to have different levels of total fatalities. φ_t controls for time varying unobservables common to all municipalities. The identification assumption is that unobserved factors are not correlated with the treatment, conditional on the covariates; that is that $E[Treat_{mt} * \varepsilon_{mt} | \boldsymbol{W_{mt}}, \boldsymbol{X_{mt}}, \boldsymbol{Fuel_{Sales_{mt}}}, \mathbf{Z_{mt}}, \rho_m, \varphi_t]=0$. To take into account that unobserved determinants could be correlated across observations in a given municipality, the standard errors used for inference are clustered at the municipality level in some estimation results; I also show the results when the data is collapsed into a pre and post treatment period.[5]

[5] Since when using a difference in difference estimator with a small number of control localities could result in inconsistent standard errors I follow Bertrand et al (2004) and collapsed the data to a before and after.

A concern for the identification assumption to hold is that the decision to treat was based on differential pre treatment trends of infant mortality on treated versus control municipalities. To test this I ran the following specification on the pre treatment data:

$$y_{mt} = \partial Treated\ Municipality + \emptyset Trend_{mt} + \beta Treated\ Municipality * Trend_{mt} + \rho_m + \varphi_t + \varepsilon_{mt} \quad (2)$$

Wherey_{mt} is either the month to month change in total respiratory related deaths or the month to month change in the fatality rate. *Trend* is the month number, which ranges from 1 to 24. As can be seen in Table 3.4, the coefficient on $Treated\ Municipality * Trend_{mt}$ is not significant, hence treated municipalities do not seem to be following a different trend than control municipalities. Figure 3 shows average monthly fatality rates before and after treatment for treated and control municipalities. There does not appear to be a pattern in control versus treated municipalities prior to treatment, which further confirms that the decision to treat was not based on differential trends in infant mortality. Since the US adopted ULSD in 2006, the decision to treat in Mexican border municipalities was probably due to the high volume of trade by land between Mexico and the United States and hence exogenous to local health outcomes.

3.5 Results

Table 3.5 shows the estimates of the impact of diesel sulfur content regulation infant mortality rates at the monthly level. The first column includes municipality and time fixed effects. The next columns successively add parent and birth characteristics, weather, economic activity, gasoline sales and volume sales controls. Once economic activity controls are added, the coefficient becomes significant, which seems as if the regulation had a positive effect on infant mortality, which is contrary to what would be expected. Economic shocks were negatively correlated with treatment and with infant mortality; this could be if for instance post treatment there was a negative economic shock which lowers income causing a decrease in medical care. After controlling for economic shocks, the point estimate does not change very much across specifications. These results are using the monthly data and using the total births in a given month as the population at risk, however this is a poor measure of the population at risk since it is not taking into account the births in the previous months; it is hard to believe that most of the respiratory related fatalities occur within less than the first month of birth. Due to this reason the following estimates are based on the collapsed data at the annual level.

Table 3.6, 3.7 and 3.8, present the results using the annual data. In table 3.6, parent and birth characteristics explain most of the variation in infant mortality. Once economic indices are added, fuel sales do not explain much of the variation; as expected economic activity indices are a good proxy for sales of gasoline and diesel. Although the point estimates are still positive, they are no longer significant as they were when the monthly data was used. The effect of the policy could depend on the time since its implementation. The longer the policy is in effect and if the policy improved air quality, its effects on health would be expected to increase over time; as time progresses the population has been exposed to better air quality for a longer period. Table 3.7 displays the results of regressions similar to those of table 3.6 but with the interaction of the regulation dummy with number of years since the date of the policy implementation, that is either one or two years. The point estimate on the treatment dummy is now negative and the coefficient on the interaction is not significant. Next, I investigate heterogeneous effects across the distinct treated municipalities. In table 3.8, the ATE is positive and

significant, again opposite of what would be expected. The treatment effects differ significantly across municipalities, with some having a positive and others a negative effect. However, even when standard errors are clustered, they could still be biased given the small number of control municipalities, as found by Bertrand et al (2002); following Bertrand et al (2002) I collapse the data into a before and an after.

Table 3.9 and 3.10 show the impact of the regulation on infant mortality using the pre and post period collapsed data set. The estimates in table 3.9 are positive and not significant. In table 3.10, the ATE is not significant and no longer do the treatment effects differ across municipalities. The results are not stable across techniques and specifications. Thus, the regulation does not seem to have significantly impacted infant mortality.

3.6 Discussion

One of the possible reasons of why the regulation does not appear to have been effective is that it had no impact on the channel through which it was supposed to affect infant mortality, namely air quality improvements. From figure 2 it is not clear what the effect of it was on SO2 and PM10 concentrations; since pollution data is only available for a few cities and a large portion of the observations are missing, it would be difficult to assess what the impacts have been on pollution levels. Also, even though mothers with children who are less than one year old may be less likely to migrate, there is still some concern of migration shocks, particularly in the border cities. Another potential concern is that the stable unit treatment assumption is not met, particularly for localities that share a border with municipalities that are not treated; I am aggregating at the municipality level and it is possible that some localities in a given municipality go to another locality that is not treated to purchase fuels, which would bias the coefficient upwards. Also I am not controlling for economic activity in the US part of the border and cannot control for fuel sales in the US side of the border; it could be that there were increased fuel sales in the US side for vehicles that cross the border or that there were increased pollution levels in the US side that affect the Mexican border cities.

3.7 Conclusion

This study assesses the impact of sulfur content regulation in diesel on infant mortality. Using death and birth certificates data along with weather and economic activity controls, I find that there is not a significant effect of the regulation on infant mortality outcomes. One of the main concerns is that the regulation had no impact on the channel through which the regulation is supposed to have impacted infant mortality, namely pollution levels. Also, the effects of the regulation will be more apparent in the long run; in order to gain the full benefits there has to be a change in the vehicle fleet, since in order for low sulfur content to significantly reduce vehicle emissions, the vehicles have to be equipped with the appropriate emissions control technology. In conclusion, there does not appear to be short run effects on infant mortality from the introduction of ultra-low sulfur diesel.

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Tables

Table 3.1: Date of Introduction of ULS Fuels by Municipality and Fuel Type

Municipality	Border City	Date of Introduction	Fuel Type
All cities		October 2006	Premium
Playas de Rosarito	Yes	January 2007	Diesel
Ensenada	Yes	January 2007	Diesel
Mexicali	Yes	January 2007	Diesel
Nogales	Yes	January 2007	Diesel
Ciudad Juarez	Yes	January 2007	Diesel
Nuevo Laredo	Yes	January 2007	Diesel
Reynosa	Yes	January 2007	Diesel
Magdalena	Yes	January 2007	Diesel
Sabinas	Yes	January 2007	Diesel
Veracruz	No	January 2008	Premium
Monterrey Santa Catarina	No	October 2008	Magna
Monterrey Santa Catarina	No	January 2010	Diesel
Guadalajara	No	March 2009	Magna
Guadalajara	No	August 2009	Diesel
Zapopan	No	March 2009	Magna
Zapopan	No	August 2009	Diesel
Mexico City (All Municipalities)	No	July 2009	Magna
Mexico City (All Municipalities)	No	February 2010	Diesel

Source: PEMEX Refinacion

Table 3.2: Characteristics of Municipalities in Sample and the Rest of the Country

	Sample Municipalities	Rest of the Country	Test of Equality t-statistic
Male Years of Education	6.337	4.192	-56.359***
Female Years of Education	6.255	4.697	-61.816***
Fraction of Population Less than 4 Years Old	0.109	.111	2.80***
Fraction of Population Female 15-49 Years Old	0.244	.236	-10.245***
Fraction of Population with Healthcare	0.412	.260	-40.035***
Percent of HH with Female Head	0.151	0.165	7.992***
Average residents per HH	4.126	4.438	22.635**
Fraction of HH with Running Water	0.492	.459	-5.988**
Fraction of HH with Drainage	0.591	.462	-26.688***
Fraction of HH with Electricity	0.846	0.774	-16.292***
Fraction HH with Computer	0.062	0.020	-45.743***
Observations (Localities)	6150	101080	

Source: INEGI. 2005 Censo de Poblacion y Vivienda

Table 3.3: Summary Statistics

	2005	2006	2007	2008
Total Births in the Country	2216818	2152781	2097930	1606675
Total Re Respiratory Related Fatalities 1 yr old and Younger in the Country	3610	3331	3351	3138
<u>All Municipalities in Sample</u>				
Respiratory Related Fatalities 5 yrs old and younger	1450	1418	1416	1369
Respiratory Related Fatalities 1 yr old and Younger	1344	1317	1313	1265
Total Births	856286	837020	822031	653177
Total Diesel Sales	20751327	21981511	22877192	2.3E+07
Total Magna Gasoline Sales	55846399	59088899	63158063	6.6E+07
Total Premium Gasoline Sales	55846399	59088899	63158063	6.6E+07
Mean Index of Occupied Whole Personnel	103	104	106	108
Mean Index of Occupied Retail Personnel	107	115	120	125
Mean Index of Whole Purchases	106	111	113	114
Mean Index of Retail Purchases	115	121	124	122
Mean Index of Whole Remunerations	101	99	104	110
Mean Index of Retail Remunerations	109	111	114	116
Fraction Single Birth	0.991	0.988	0.987	0.987
Fraction Double Birth	0.009	0.012	0.013	0.012
Fraction Triple Birth	0.001	0.001	0.001	0.001
Mean Birth Order	1.946	1.954	1.937	1.878
Fraction Not in Hospital	0.014	0.013	0.010	0.006
Mother's Age	25.839	25.879	25.861	25.964
Father's Age	28.882	28.940	28.936	28.980
Fraction Single	0.102	0.102	0.101	0.096
Fraction Divorced	0.001	0.001	0.002	0.002
Fraction Married	0.490	0.478	0.458	0.466
Fraction Free Union	0.405	0.417	0.437	0.435
Fraction of Mothers w/No Schooling	0.011	0.010	0.009	0.008
Fraction of Fathers w/No Schooling	0.010	0.009	0.009	0.008
Fraction of Mothers w/Primary Schooling	0.217	0.203	0.184	0.158
Fraction of Fathers w/Primary Schooling	0.194	0.181	0.169	0.148
Fraction of Mothers w/High School Education	0.644	0.651	0.664	0.673
Fraction of Fathers w/High School Education	0.644	0.651	0.659	0.665
Fraction of Mothers w/College Education	0.125	0.134	0.141	0.159
Fraction of Fathers w/College Education	0.150	0.156	0.161	0.177

Fraction of Mothers Blue Collar Workers	0.816	0.811	0.877	0.890
Fraction of Fathers Blue Collar Workers	0.839	0.845	0.869	0.869
Fraction of Mothers Business Sector	0.024	0.021	0.019	0.025
Fraction of Fathers Business Sector	0.022	0.016	0.021	0.037
Fraction of Mothers Not Remunerated	0.040	0.030	0.022	0.024
Fraction of Fathers Not Remunerated	0.010	0.008	0.008	0.009
Fraction of Mothers Self Employed	0.158	0.166	0.103	0.084
Fraction of Fathers Self Employed	0.138	0.137	0.108	0.092

Table 3.4: Pre Treatment Time Trends

	Month to Month Change in Total Fatalities	Month to Month Change in Mortality Rate
Treated Municipality*Trend	0.022	-1.667
	[0.026]	[2.031]
Trend	0.096	10.693
	[0.023]**	[4.969]*
Constant	-1.435	-197.921
	[0.389]**	[63.956]**
Time and Municipality FE	Yes	Yes
Observations	2024	2024
Number of mun˙id	88	88
R-squared	0.09	0.05

Values are coefficients from OLS regressions. Clustered standard errors at the municipality level are in brackets. Sample is restricted to the pre treatment period: from January 2005 to December 2006 *significant at 10% ** significant at 5% *** significant at 1%

Table 3.5: OLS Estimation Results

	Dependent Variable: ln(Monthly Infant Mortality Rate)					
	(1)	(2)	(3)	(4)	(6)	(7)
Treatment (0/1)	0.126	0.127	0.139	0.209	0.214	0.233
	[0.081]	[0.088]	[0.092]	[0.087]*	[0.087]*	[0.088]**
Constant	5.123	5.02	5.56	5.90	5.91	5.56
	[0.091]**	[4.819]	[4.955]	[4.998]	[4.986]	[5.047]
Observations	2140	2082	2078	2078	2078	2078
Number of Municipalities	88	88	88	88	88	88
R-squared	0.49	0.49	0.5	0.5	0.51	0.51
Time and Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Parent and Birth Characteristics	No	Yes	Yes	Yes	Yes	Yes
Weather	No	No	Yes	Yes	Yes	Yes
Economic Activity Indices	No	No	No	Yes	Yes	Yes
Gasoline Volume Sales	No	No	No	No	Yes	Yes
Diesel Sales	No	No	No	No	No	Yes

Notes: Values are coefficients from OLS regressions. Sample period is from January 2005 to December 2008. Clustered standard errors at the municipality level are in brackets. parent characteristics include percent of female newborns, percent of mothers who are divorced, married, and free union, age of the mother and the father, percent mother with no schooling, with primary schooling completed, with high school completed and college completed, children born alive, percent of mothers who are blue collar workers, percent of fathers who are blue collar workers, percent who are business owners, percent not remunerated and percent self employed. Characteristics of the birth include percent of births who occurred at hospital and percent not at hospital, order of birth, percent that were a single birth, a double birth or a triple birth. * significant at 10%; **significant at 5% *** significant at 1%

Table 3.6: Estimated Effects of Regulation on Infant Mortality Based on Annual Data

	Dependent Variable: ln(Annual Infant Mortality Rate)					
	(1)	(2)	(3)	(4)	(6)	(7)
Treatment (0/1)	0.208	0.225	0.224	0.212	0.213	0.222
	[0.128]	[0.156]	[0.161]	[0.155]	[0.156]	[0.157]
Constant	4.981	7.24	7.96	6.94	6.89	6.84
	[0.040]**	[5.010]	[5.648]	[6.452]	[6.474]	[6.472]
Observations	352	352	352	352	352	352
Number of Municipalities	88	88	88	88	88	88
R-squared	0.08	0.17	0.19	0.23	0.23	0.23
Time and Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Parent and Birth Characteristics	No	Yes	Yes	Yes	Yes	Yes
Weather	No	No	Yes	Yes	Yes	Yes
Economic Activity Indices	No	No	No	Yes	Yes	Yes
Gasoline Volume Sales	No	No	No	No	Yes	Yes
Diesel Sales	No	No	No	No	No	Yes

Notes: Values are coefficients from OLS regressions. Sample period is from January 2005 to December 2008. Clustered standard errors at the municipality level are in brackets. Monthly data collapsed at the annual level by municipality. Economic indices are the average for the year. Weather indices are the average for the year with the exception of rainfall which is the yearly total. Gasoline and diesel sales are the total for the year. Parent characteristics are the average for the year and they include percent of female newborns, percent of mothers who are divorced, married, and free union, age of the mother and the father, percent mother with no schooling, with primary schooling completed, with high school completed and college completed, children born alive, percent of mothers who are blue collar workers, percent of fathers who are blue collar workers, percent who are business owners, percent not remunerated and percent self employed. Characteristics of the birth include percent of births who occurred at hospital and percent not at hospital, order of birth, percent that were a single birth, a double birth or a triple birth. *significant at 10% ** significant at 5%; *** significant at 1%

Table 3.7: Estimated Effects Based on Annual Data: Length of Policy Exposure Effects

	Dependent Variable: ln(Annual Infant Mortality Rate)					
	(1)	(2)	(3)	(4)	(6)	(7)
Treatment (0/1)	-0.126	-0.114	-0.1	-0.173	-0.171	-0.167
	[0.322]	[0.292]	[0.325]	[0.307]	[0.308]	[0.310]
Duration	0.103	0.078	0.155	0.02	0.019	0.016
	[0.034]**	[0.056]	[0.082]	[0.095]	[0.096]	[0.097]
Treatment*Duration	0.222	0.238	0.221	0.276	0.276	0.28
	[0.208]	[0.206]	[0.210]	[0.196]	[0.196]	[0.198]
Constant	4.981	7.348	8.811	7.839	7.788	7.744
	[0.041]**	[5.032]	[5.679]	[6.440]	[6.460]	[6.451]
Observations	352	352	352	352	352	352
Number of Municipalities	88	88	88	88	88	88
R-squared	0.08	0.17	0.19	0.23	0.23	0.23
Time and Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Parent and Birth Characteristics	No	Yes	Yes	Yes	Yes	Yes
Weather	No	No	Yes	Yes	Yes	Yes
Economic Activity Indices	No	No	No	Yes	Yes	Yes
Gasoline Volume Sales	No	No	No	No	Yes	Yes
Diesel Sales	No	No	No	No	No	Yes

Notes: Values are coefficients from OLS regressions. Sample period is from January 2005 to December 2008. Clustered standard errors at the municipality level are in brackets. Monthly data collapsed at the annual level by municipality. Economic indices are the average for the year. Weather indices are the average for the year with the exception of rainfall which is the yearly total. Gasoline and diesel sales are the total for the year. Parent characteristics are the average for the year and they include percent of female newborns, percent of mothers who are divorced, married, and free union, age of the mother and the father, percent mother with no schooling, with primary schooling completed, with high school completed and college completed, children born alive, percent of mothers who are blue collar workers, percent of fathers who are blue collar workers, percent who are business owners, percent not remunerated and percent self-employed. Characteristics of the birth include percent of births who occurred at hospital and percent not at hospital, order of birth, percent that were a single birth, a double birth or a triple birth. * Significant at 10% ** significant at 5%; *** significant at 1%

Table 3.8: Estimated Effects Based on Annual Data Effects by Municipality

	Dependent Variable: ln(Annual Infant Mortality Rate)					
	(1)	(2)	(3)	(4)	(6)	(7)
Treatment (0/1)	0.641 [0.051]**	0.717 [0.109]**	0.651 [0.151]**	0.49 [0.244]*	0.491 [0.244]*	0.496 [0.247]*
Treatment*Tijuana	-0.226 [0.000]**	-0.24 [0.168]	-0.264 [0.194]	-0.158 [0.207]	-0.156 [0.208]	-0.145 [0.205]
Treatment*Tecate	-0.905 [0.000]**	-0.792 [0.112]**	-0.852 [0.151]**	-0.758 [0.193]**	-0.757 [0.193]**	-0.761 [0.195]**
Treatment*Mexicali	-0.44 [0.000]**	-0.498 [0.124]**	-0.21 [0.251]	0.007 [0.425]	0.012 [0.428]	0.002 [0.434]
Treatment*Juarez	-0.45 [0.000]**	-0.637 [0.211]**	-0.513 [0.223]*	-0.091 [0.337]	-0.089 [0.338]	-0.078 [0.337]
Treatment*NuevoLaredo	-0.822 [0.000]**	-1.058 [0.114]**	-0.989 [0.121]**	-0.825 [0.218]**	-0.825 [0.218]**	-0.812 [0.214]**
Treatment*Reynosa	-0.193 [0.000]**	-0.25 [0.089]**	-0.126 [0.106]	-0.175 [0.396]	-0.175 [0.397]	-0.179 [0.398]
Constant	4.981 [0.041]**	21.659 [7.361]**	8.307 [5.674]	7.521 [6.663]	7.461 [6.683]	7.38 [6.694]
Observations	352	352	352	352	352	352
Number of Municipalities	88	88	88	88	88	88
R-squared	0.09	0.18	0.2	0.24	0.24	0.24
Time and Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Parent and Birth Characteristics	No	Yes	Yes	Yes	Yes	Yes
Weather	No	No	Yes	Yes	Yes	Yes
Economic Activity Indices	No	No	No	Yes	Yes	Yes
Gasoline Volume Sales	No	No	No	No	Yes	Yes
Diesel Sales	No	No	No	No	No	Yes

Notes: _Values are coefficients from OLS regressions. Sample period is from January 2005 to December 2008. Clustered standard errors at the municipality level are in brackets. Monthly data collapsed at the annual level by municipality. Economic indices are the average for the year. Weather indices are the average for the year with the exception of rainfall which is the yearly total. Gasoline and diesel sales are the total for the year. Parent characteristics are the average for the year and they include percent of female newborns, percent of mothers who are divorced, married, and free union, age of the mother and the father, percent mother with no schooling, with primary schooling completed, with high school completed and college completed, children born alive, percent of mothers who are blue collar workers, percent of fathers who are blue collar workers, percent who are business owners, percent not remunerated and percent self employed. Characteristics of the birth include percent of births who occurred at hospital and percent not at hospital, order of birth, percent that were a single birth, a double birth or a triple birth. *significant at 10% ** significant at 5%; *** significant at 1%

Table 3.9: Estimated Effects Based on Before and After Collapsed Data

	Dependent Variable: ln(Infant Mortality Rate)					
	(1)	(2)	(3)	(4)	(6)	(7)
Treatment (0/1)	0.215	0.245	0.339	0.449	0.44	0.442
	[0.171]	[0.216]	[0.243]	[0.318]	[0.321]	[0.327]
Constant	5.002	-5.60	-7.78	-8.29	-8.46	-8.40
	[0.033]**	[14.137]	[15.797]	[17.063]	[17.188]	[17.434]
Observations	176	176	176	176	176	176
Number of Municipalities	88	88	88	88	88	88
R-squared	0.11	0.36	0.42	0.48	0.49	0.49
Time and Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Parent and Birth Characteristics	No	Yes	Yes	Yes	Yes	Yes
Weather	No	No	Yes	Yes	Yes	Yes
Economic Activity Indices	No	No	No	Yes	Yes	Yes
Gasoline Volume Sales	No	No	No	No	Yes	Yes
Diesel Sales	No	No	No	No	No	Yes

Notes: Values are coefficients from OLS regressions. Sample period is from January 2005 to December 2008. Clustered standard errors at the municipality level are in brackets. Monthly data collapsed at the treatment period level by municipality. Economic indices are the average for the two years pre and two years post regulation. Weather indices are the average for the two years pre and the two years post regulation with the exception of rainfall which is the pre and post regulation total. Gasoline and diesel sales are the total for the two years pre and the two post regulation. Parent characteristics are the average for the two years pre and post regulation and they include percent of female newborns, percent of mothers who are divorced, married, and free union, age of the mother and the father, percent mother with no schooling, with primary schooling completed, with high school completed and college completed, children born alive, percent of mothers who are blue collar workers, percent of fathers who are blue collar workers, percent who are business owners, percent not remunerated and percent self employed. Characteristics of the birth include percent of births who occurred at hospital and percent not at hospital, order of birth, percent that were a single birth, a double birth or a triple birth. *significant at 10% ** significant at 5%; *** significant at 1%

Table 3.10: Estimated Effects Based Before and After Collapsed Data by Municipality

	Dependent Variable: ln(Infant Mortality Rate)					
	(1)	(2)	(3)	(4)	(6)	(7)
Treatment (0/1)	0.664	0.852	0.936	1.036	1.041	1.041
	[0.441]	[0.446]	[0.489]	[0.687]	[0.694]	[0.702]
Treatment*Tijuana	-0.094	-0.208	-0.276	-0.404	-0.41	-0.406
	[0.620]	[0.666]	[0.705]	[0.787]	[0.795]	[0.808]
Treatment*Tecate	-1.009	-0.978	-1.168	-1.407	-1.41	-1.411
	[0.620]	[0.650]	[0.682]	[0.765]	[0.772]	[0.781]
Treatment*Mexicali	-0.458	-0.697	-0.204	-0.343	-0.38	-0.378
	[0.620]	[0.644]	[0.723]	[0.928]	[0.943]	[0.955]
Treatment*Juarez	-0.496	-0.879	-0.824	-0.682	-0.69	-0.685
	[0.620]	[0.661]	[0.677]	[0.832]	[0.840]	[0.859]
Treatment*Nuevo Laredo	-0.866	-1.228	-1.14	-1.065	-1.069	-1.065
	[0.620]	[0.615]	[0.633]	[0.742]	[0.749]	[0.761]
Treatment*Reynosa	-0.224	-0.302	-0.031	0.471	0.452	0.452
	[0.620]	[0.620]	[0.622]	[0.877]	[0.887]	[0.897]
Constant	5.002	11.989	-0.64	16.466	16.252	3.368
	[0.033]**	[9.382]	[15.934]	[15.363]	[15.523]	[18.184]
Observations	176	176	176	176	176	176
Number of Municipalities	88	88	88	88	88	88
R-squared	0.15	0.42	0.49	0.57	0.57	0.57
Time and Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Parent and Birth Characteristics	No	Yes	Yes	Yes	Yes	Yes
Weather	No	No	Yes	Yes	Yes	Yes
Economic Activity Indices	No	No	No	Yes	Yes	Yes
Gasoline Volume Sales	No	No	No	No	Yes	Yes
Diesel Sales	No	No	No	No	No	Yes

Notes: Values are coefficients from OLS regressions. Sample period is from January 2005 to December 2008. Clustered standard errors at the municipality level are in brackets. Monthly data collapsed at the treatment period level by municipality. Economic indices are the average for the two years pre and two years post regulation. Weather indices are the average for the two years pre and the two years post regulation with the exception of rainfall which is the pre and post regulation total. Gasoline and diesel sales are the total for the two years pre and the two post regulation. Parent characteristics are the average for the two years pre and post regulation and they include percent of female newborns, percent of mothers who are divorced, married, and free union, age of the mother and the father, percent mother with no schooling, with primary schooling completed, with high school completed and college completed, children born alive, percent of mothers who are blue collar workers, percent of fathers who are blue collar workers, percent who are business owners, percent not remunerated and percent self employed. Characteristics of the birth include percent of births who occurred at hospital and percent not at hospital, order of birth, percent that were a single birth, a double birth or a triple birth. *significant at 10% ** significant at 5%; *** significant at 1%

Figure 3.1: Monthly National Sales Volume by Fuel Type

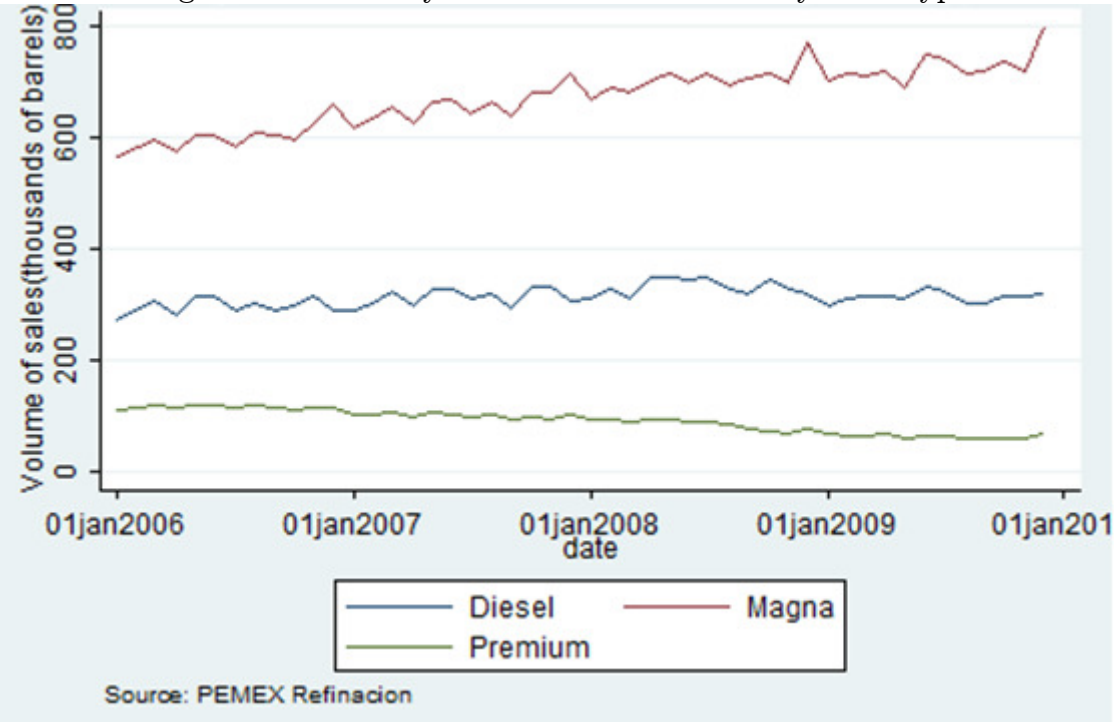
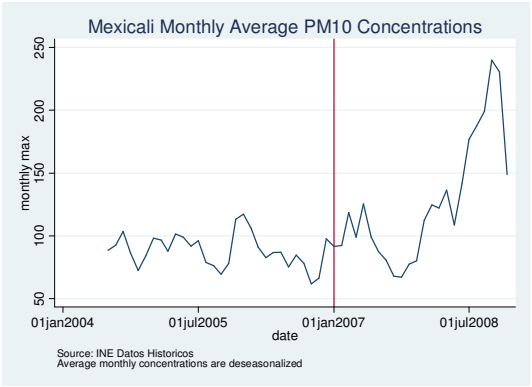
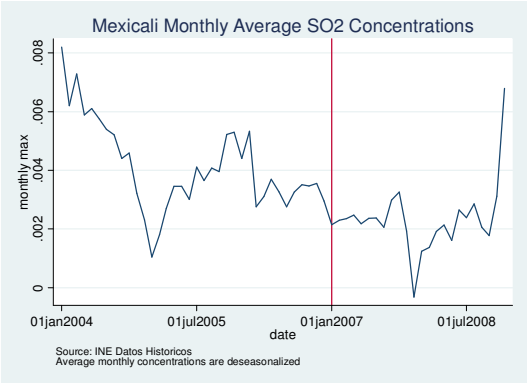
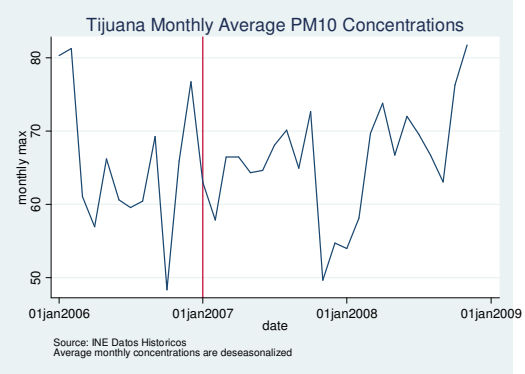
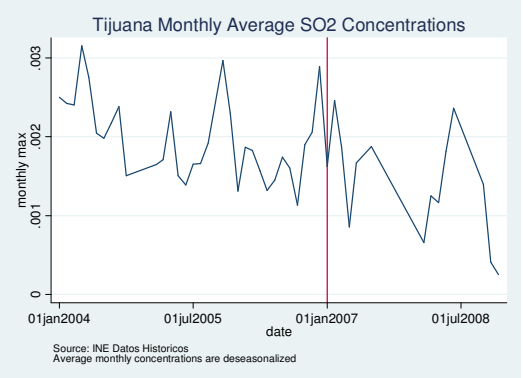


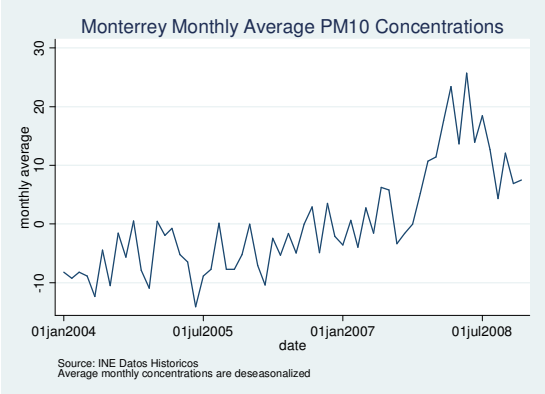
Figure 3.2: Plots Of Deseasonalized Mean Monthly PM10 and SO2 Concentrations by City











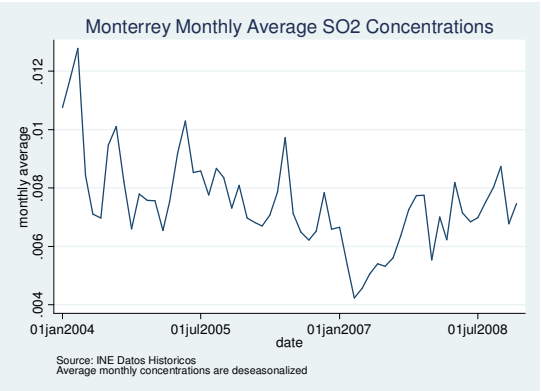


Figure 3.3: Monthly Fatality Rate Pre and Post Treatment