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Homicides in Mexico and the expiration of the U.S. federal assault weapons ban: a difference-in-discontinuities approach

Luke E. Chicoine

Department of Economics, DePaul University and IZA, DePaul Center 6200 Chicago, IL 60604, USA. *email* <lchicoin@depaul.edu>

Abstract

The year following the expiration of the U.S. Assault Weapons Ban (AWB), the homicide rate in Mexico increased for the first time in a decade. A difference-in-discontinuities model and a unique dataset are used to compare discontinuities generated by close mayoral elections on either side of the AWB expiration. The model finds a statistically significant increase in the firearm homicide rate following the expiration of the AWB. This effect is larger closer to the U.S.–Mexico border, is isolated to the timing of the expiration, and there is no evidence of a concurrent increase in non-firearm homicides or other violent crime.

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JEL classifications: F52, I18, K42
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1. Introduction

After declining each of the 10 years the U.S. Federal Assault Weapons Ban (AWB) was in place, the homicide rate in Mexico increased over 250% in the 6 years following the ban's expiration. Originally passed on 13 September 1994, as part of a larger anti-crime bill, the law had a 10-year sunset provision that was not extended by Congress. As a result, the law expired on 13 September 2004. The AWB prohibited the domestic sale and production of a large class of semi-automatic firearms and limited the size of a magazine to 10 rounds. There were also a number of guns banned by name, including the AR-15 and AK-47. In Mexico, homicides committed with or without a firearm declined at similar rates prior to the AWB expiration. During the 6 years following the expiration of the AWB, the growth of homicides committed with firearms was more than four times larger than the growth of non-firearm homicides.

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This article examines whether the expiration of the AWB in the United States, and the increased availability of high powered firearms that accompanied the ban, contributed to the 2005 reversal of the homicide trend in Mexico. Using data from 2000 to 2006, estimates yield a 35% increase in the firearm homicide rate following the expiration of the AWB. This rules out the estimates being driven by President Calderon's 2007 deployment of federal troops, or 2007 changes in illicit narcotics prices. However, the estimates remain both quantitatively and qualitatively similar when the sample is expanded to include this period. The same model finds no evidence of a concurrent change in the non-firearm homicide rate, but does find an increase in the overall homicide rate in Mexico. Furthermore, there is no change in the underlying crime rate of six other major crimes and no evidence of changes in the illicit narcotics market that coincide with the expiration of the AWB. The increase in the firearm homicide rate is shown to be isolated to the timing of the expiration of the AWB, and the effect is found to be greater closer to U.S.–Mexico border crossings. These results rule out a secular increase in homicides and general criminal behavior, events that occurred at other points in time and changes in the state of the narcotics market. The expiration of the AWB is isolated as the only plausible explanation of the increase in homicides in Mexico in 2005 that fits these criteria, only impacts homicides committed with a firearm, and has a larger impact closer to the United States.

These conclusions are generated by a difference-in-discontinuity model (DiRD), separately estimating a regression discontinuity (RD) model on either side of the expiration of the AWB. An RD framework, motivated by Dell (2015), is used to compare municipalities in which the incumbent mayoral party wins a close election, to those where the incumbent is defeated. In municipalities where the incumbent party is defeated the resulting dissolution of the status quo, and forthcoming policy shock, provides an environment in which friction between criminal entities and law enforcement likely increases; when the incumbent party remains in power the status quo is maintained. Close elections provide a setting in which municipalities in Mexico are randomly sorted across the election threshold. The RD model is estimated for the post-AWB period, where the status quo effect is combined with the availability of high powered firearms from the United States, and a separate RD model is estimated for the years the ban was in place. To measure the impact of the expiration of the AWB following an incumbent defeat, these RD models are then differenced in a DiRD framework. Differencing the two estimates removes the consistent status quo effect of an incumbent losing, and the resulting estimate is the effect of having the newly available firearms in the post-AWB period, relative to their unavailability in the pre-expiration period. Available evidence also demonstrates that there is no change in the status quo effect within either the pre-expiration or the post-expiration period. Only when the comparison crosses the expiration of the AWB is there evidence of an increase in the firearm homicide rate.

A consistent complication in identifying the causes of the escalating homicide rate in Mexico that occurred over the past decade are the local spikes in violence due to conflict between drug trafficking organizations, and more recently, with the federal government. An important innovation of this model is that municipalities from every part of Mexico are sorted to either side of the election discontinuity, in both the pre- and post-expiration states of the AWB. This means that any specific conflict, and local spike in violence, is unlikely to be driving these estimates. A municipality with a close mayoral election is equally likely to be on either side of the status quo election threshold, and there is no statistically significant difference in violence leading up to an election that would bias this sorting. This is an important contribution to the literature that rules out specific localized conflict as an explanation of the model's estimates.

Finally, this work informs the policy debate on whether access to high-powered firearms, such as those banned under the AWB, can lead to a higher homicide rate. The evidence presented in this paper finds that, at least in certain settings, access to these firearms does lead to an increased rate of homicide. These findings also highlight the potential international ramifications of domestic policy in today's globalized world. Although the effect is found to be larger closer to the border, it is important to highlight that the effects found in this paper are not isolated to regions of Mexico that border the United States. This escalates the importance of a comprehensive evaluation of policy effects that can extend well beyond a nation's borders.

The remainder of the article is organized as follows. Background on homicide trends in Mexico, the prevalence of firearm trafficking across the U.S.–Mexico border and the current literature are summarized in Section 2. In Section 3, the underlying identification strategy and estimating model are described, and election and municipal data used in the paper are outlined in Section 4. In Section 5, the relevant tests demonstrate that the density of the observations and predetermined outcomes at the election discontinuity are consistent across the expiration of the AWB, and the key results of the paper are discussed in detail. Section 6 concludes.

2. Background and Literature Review

Time series data suggest that the expiration of the AWB is related to the homicide rate in Mexico. The national homicide rate, shown as fatalities per 10,000 people, from 1990 to 2010 is plotted in Figure 1.[1] Homicide rate data are available for the 5 years prior to the implementation of the AWB, from 1990 to 1994. During this period, the homicide rate in Mexico increased nearly 23% to 1.69 homicides per 10,000 people. In the USA, legal ownership of every firearm possessed prior to the law being enacted was extended into the post-implementation period; therefore, there was no immediate change in the stock of available firearms between 1994 and 1995. However, over each of the next 10 years, when the AWB was in place, the homicide rate in Mexico declined, possibly due to a deteriorating stock of high-powered firearms and large capacity magazines banned under the AWB. This trend reversed in the first year after the ban's expiration, 2005. Over the next 6 years the homicide rate increased more than 250%. The initial deployment of federal troops by President Calderon, aimed at directly confronting drug trafficking organizations in Mexico, coincides with the 2007 decline in the homicide rate.[2]

1 The range of the figure is cut at 1.75 to better focus on the relevant time period. The full plot can be seen in Online Appendix Figure A.1.

2 Prior to the election of President Calderon, Mexican officials had already publicly expressed concern regarding the expiration of the AWB. In January of 2006, an official with Mexico's Federal Preventive Police stated that, 'Assault rifles such as the AR-15 and the AK-47 are by far the most popular weapons imported into Mexico by the drug cartels' (Tobar, 2006). Within the first month of the Calderon administration, Mexico's Deputy Attorney General had said that, 'Re-imposing the U.S. Assault Weapons Ban would go a long way towards stemming the violence along the border. These weapons come from your country, we know that for a fact.' He also directly tied the post-AWB firearms to the increase in violence stating, 'there is a direct relationship between the flow of these weapons and the explosion of violence' (Hawley and Solache, 2007). Finally, in a 2010 speech to the USA Congress, President Calderon directly stated his belief that the expiration of the AWB has contributed to the violence in Mexico, and he directly asked congress to reinstate the ban (Charles, 2010; Sheridan, 2010).

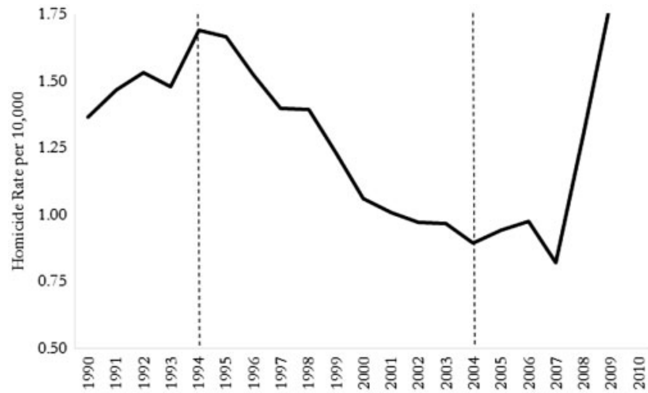

Figure 1. Homicide rate in Mexico.

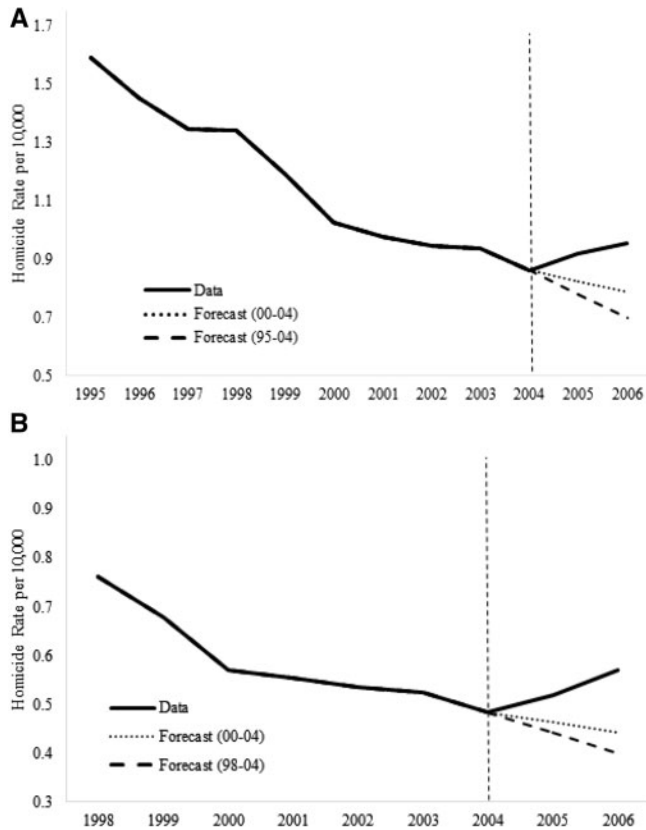

Figure 2. Homicide Rates and Counterfactual Trends. (a) Overall homicide rate, (b) Firearm homicide rate.

The main analysis of this paper focuses on the 2004 expiration of the AWB, and to ensure that the post-deployment conflict is not responsible for any of the study's conclusions, this analysis focuses on the period ending in 2006. Due to data limitations—firearm homicide data are first available in 1998—it is not possible to examine the 1994

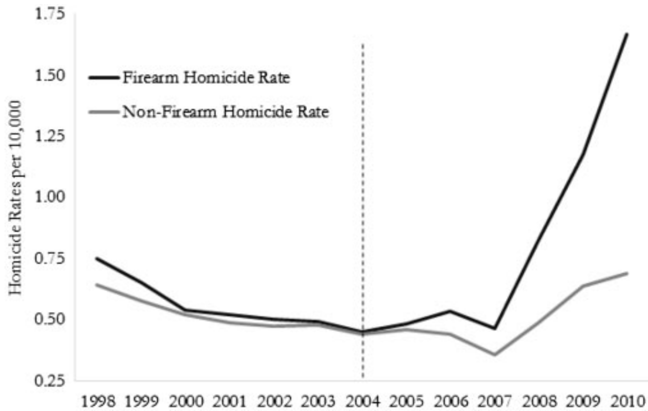

Figure 3. Firearm versus non-firearm homicide rate in Mexico.
Note: Firearm homicide data are available from SINAIS beginning in 1998.

implementation of the law. A simple counterfactual analysis of the trend can be used to illustrate the impact of the AWB expiration. The homicide rate from 1995 to 2006 is plotted in Figure 2a, the solid line. The dashed line is constructed using data from 1994 to 2004 to estimate an OLS slope, and that slope is used to predict the counterfactual levels for 2005 and 2006, assuming a continuation of the pre-expiration trend. The same is done to construct the dotted line, using only the more recent 2000–2004 data. The difference between the 2006 data and the forecasted level is between 19.6% (2000 forecast) and 29.8% (1995 forecast) of the 2004 homicide rate. The same exercise is repeated with data for homicides committed using firearms, shown in Figure 2b. The difference between the firearm homicide rate data and the forecasted level in 2006 is between 29.5% (2000 forecast) and 39.5% (1998 forecast) of the 2004 firearm homicide rate. Even though the post-2007 spike in the violence is massive, this descriptive analysis highlights the significant change in the homicide rate following the expiration of the AWB. The estimates from this simple descriptive analysis, for both the overall homicide rate and the firearm homicide rate, are similar to the estimates found by the DiRD model later in the paper.

If the reversal of trends is due to the expiration of the AWB, the change in the trend at the expiration of the AWB should be most prevalent for firearm homicides. The time series of the firearm homicide and non-firearm homicide rates are compared in Figure 3. The two trends are similar prior to 2004, if anything the decline of the firearm homicide rate is slightly steeper. However, immediately following the expiration of the AWB, the two rates diverge. In the first two post-AWB years, the firearm homicide rate increased 18%, while the non-firearm homicide rate declined less than 1%.[3] As the level of conflict increased over the next few years, this divergence continued to grow. This descriptive analysis is by no means definitive, but does provide evidence of a potential

3 The increased lethality of the post-AWB firearms could have led this divergence. Evidence shown in Online Appendix Table A.1 documents that the rate of firearm homicides increased relative to the level of illegal arms possession and non-firearm homicides following the expiration of the AWB. Additionally, like the overall time trend, the number of municipal months with a high level of firearm homicides, 4, 5 or 10, saw an annual increase of between 15% and 25% in 2005. The largest annual increase in each category since 1998, the initial year of the dataset (Online Appendix Table A.2).

relationship between the expiration of the AWB and both the homicide rate and the firearm homicide rate in Mexico that warrants further investigation.

For the relationship between the AWB and the homicide rate in Mexico to exist, firearms banned by the AWB must be moving from the U.S. to Mexico in the post-AWB period. Although data on this topic are not widely available, both the statistics and related literature suggest a connection. Importantly, gun laws in Mexico are extremely restrictive. Firearms can only be sold through a single retailer in Mexico City, and only a single, permitted, small caliber handgun can be kept at home (Kopel, 2013). The most common type of firearms seized by Mexican authorities and traced back to the United States are the AR-15 and AK-47 models (Goodman and Marizco, 2010), which were banned during the AWB. In 2008, 25% of all firearms traced backed to the United States were variants of the AR-15 and AK-47 (Chu and Krouse, 2010). The trafficking of the AK-47 from the United States is particularly important, it is one of the most widely available firearms in the world, and is used by non-state groups in every region of the world.[4] The fact that the AK-47 is being so heavily trafficked from the U.S. suggests that the United States is providing the lowest cost sources for this rifle.[5] Although comparable pre-expiration data do not exist, between 2005 and 2014, the U.S. and Mexico have confirmed that tens of thousands of illegal firearms were trafficked from the United States and into Mexico.[6] This includes nearly 50% of all firearms recovered between 2006 and 2012; when Mexican authorities confiscated a total of 154,943 firearms, 99,691 were submitted for tracing and 68,161 were traced back to the United States (Scroeder, 2013). These firearms were seized from criminals, actually submitted for tracing, and then had enough recoverable information remaining to be successfully traced back the U.S., likely setting a lower bound for the number and fraction of firearms trafficked from the United States. In fact, examining variation in licensed firearm dealers in the United States, relative to the size of their domestic local market, McDougal et al. (2015) estimated that the firearms successfully traced back to the U.S. from Mexico represent less than one-fifth of all firearms bought with intent to traffic to Mexico.

Recent literature provides the framework in which such a movement of firearms would be expected. In an international setting, DellaVigna and La Ferrara (2010) find that arms embargoes tend to increase stock prices of firearm manufactures in low accountability settings. Knight (2013) finds that crime guns in the United States flow from low regulation states to high regulation states, and that criminal firearm possession is higher in states exposed to weaker firearm laws in nearby states. In the context of the U.S.-Mexico border, a movement of firearms from the low regulation states (i.e. Texas and Arizona) to the high regulation area of Mexico is consistent with the findings in this literature. Previous work has also found a positive relationship between gun prevalence and homicides (Cook, 1983; Duggan, 2001; Miller et al., 2002; Cook and Ludwig, 2006),

4 Small Arms Survey's Weapon Identification Sheet for the Kalashnikov AK-47.

5 An AK-47 purchased in the USA can be sold in Mexico at 300–400% of the original price, with an additional markup for post-AWB models (Goodman and Marizco, 2010). Analysis of pre-expiration firearm catalogs (from River Arms Inc., Olympic Arms Inc. and Arma Lite Inc.) found that pre-expiration rifles had to be combined with additional parts that required assembly to meet 'pre-ban' specifications, and that pre-expiration consumer prices were roughly equivalent to 'law enforcement' models and post-AWB consumer prices. Therefore, the AWB added an additional cost of both time and money to build a pre-expiration model equivalent to what was available in the post-AWB period.

6 The exact number of firearms recovered in Mexico and traced back to the USA during this period is 90,654. These data are from the Government Accountability Office (2009), and the Firearm Tracing System released by the Bureau of Alcohol, Tobacco, Firearms and Explosives' (ATF) Office of Strategic Intelligence and Information.

and a survey of the literature by Hepburn and Hemenway (2004) concludes that there is a consistent positive relationship between firearm prevalence and homicides.[7] However, instead of focusing on the relationship between homicides and firearm prevalence, this paper examines the relationship between homicides and the type of firearms available. The most closely related work is Dube et al. (2013) who also investigated the impact of the expiration of the AWB in Mexico. They examined the effect in areas close to the border by exploiting geographic variation in state level bans within the United States, and found that municipalities in Mexico closer to Arizona and Texas border crossings had higher rates of homicide, relative to those near California border crossings. These findings are consistent with the conclusions from this paper. However, the current study comes to this conclusion without relying on geographic variation for identification, lessening the likelihood of localized spikes in violence generated by inter-cartel conflict driving the result. Furthermore, this paper finds evidence that the effect was more widespread than previously documented, and that it exists even without the inclusion of municipalities closest to the border.

3. Identification

3.1. Identification strategy

The identification model in this paper considers crime to be a function of two inputs: the existence of a status quo local government and the relative strength between criminal groups and law enforcement. Increased access to high-powered firearms (e.g. the AK-47 and AR-15) in the post-AWB period could grow the relative strength of criminal entities in Mexico, in comparison to the pre-expiration period. If these firearms are in fact more deadly, in times of disharmony the effect should be most dramatic on the directly affected crime: homicides committed with a firearm. The ideal experiment would be to randomly sort areas into two groups. A treatment group in which local policy is shocked, possibly leading to increased conflict, bringing relative strength into play, and a control group where a consistent status quo is maintained. A comparison of the ‘treatment’ and ‘control’ groups in the post-expiration period would yield an estimated effect of dissolving the status quo and in a state with high-relative power of the criminal entities (i.e. after introducing more powerful firearms). Repeating this experiment in the pre-expiration environment yields an estimated effect of dissolving of the status quo in a low-relative power state. Under the assumption that the effect of dissolving the status quo is consistent across time, differencing the two effects then isolates the impact of the increased relative power, in this case, due to the newly available firearms.

Using local election data from Mexico, it is possible to construct the experiment described above. In close elections, Lee (2008) shows that as long as some component of the vote cannot be precisely controlled, observations, in this case municipalities, are randomly distributed across the election threshold. This random distribution across the precise election threshold sorts municipalities into the two groups described above.

7 In contrast to this literature, other work finds increased access to firearms through gun shows, which generally have less regulated sales, does not increase firearm-related deaths (Duggan et al., 2011). Lott and Mustard (1997) find that less restrictive gun carrying laws decrease crime; however, other work on this topic has not found the same result (Ludwig, 1998; Ayres and Donohue, 2003).

Municipalities in which nothing changes, where the incumbent mayor's party is reelected, and municipalities in which the status quo is dissolved, where the incumbent party is defeated. Previous work exploits this type of variation using local mayoral elections in Mexico. Dell (2015) used an RD model to link efforts to combat illicit trafficking to political policies that the Partido Accion Nacional (PAN) pursued during President Felipe Calderon's term in office, and finds that these policing efforts led to an increase in homicides.[8] Unlike Dell (2015), the proposed estimating model does not rely on any specific party policies, but instead focuses on the success or failure of the incumbent political party in local mayoral elections. The defeat of the incumbent part, and the dissolution of the status quo, creates an environment in which conflict is more likely to occur. This could be due to explicit policing policies of the new mayor, as in Dell (2015), or more benign changes such as the severing of any implicit or explicit understanding between the outgoing administration and local criminal groups.[9] Conflict does not necessarily lead to an increase in violence; if law enforcement is sufficiently strong relative to the local criminal entities, increased interactions between law enforcement and criminals could lead to a reduction in crime. As described below, the effect of an incumbent defeat on firearm homicides is tested using an RD model in both the post-expiration (status quo + post-AWB relative strength) and pre-expiration (status quo + pre-expiration relative strength) periods. These estimates are then differenced in a DiRD model to isolate the impact of the increased non-government strength following the expiration of the AWB. A necessary assumption of this model is that impact of the change in the status quo, an incumbent defeat, is consistent across time. Evidence that this assumption holds in the data is shown later in the paper by estimating the DiRD model within both the pre- and post-expiration periods.

To ensure that estimates are not driven by changes in Mexican federal government policy the main analysis focuses on evaluating this identification strategy for the 2000 to 2006 period. Results are also shown to be consistent for the extended time period from 1999 to 2010; however, the 2000 to 2006 sample focuses on the period of a single administration, that of President Vicente Fox.

3.2. Estimating model

The effect of an incumbent loss, and dissolution of the status quo, is estimated using an RD model. This model is estimated separately for both the pre-expiration and post-expiration periods. The crime rates in the newly elected mayor's first full year in office, denoted at time-period t, are matched to the previous year's election results, denoted $(t-1)$. The running variable for the RD model is the vote margin ($v_{ms(t-1)}$), which is calculated as the vote share of the highest ranked challenger minus the incumbent party's vote share. The vote margin is positive if the challenger wins the

8 President Calderon was in office from 1 December 2006 to 30 November 2012. Estimates in Online Appendix Table A.3 demonstrate that the link between PAN elections and homicides does not exist prior to the election of President Calderon.

9 It is likely that low rates of violent crime, such as murder, are in the best interest of both the government and local organized crime. Data from municipalities in Mexico show there is a negative relationship between a party's 'years in power' in a municipality and firearm homicide rates. This could be due to either explicit communication networks, or an implicit understanding of acceptable behavior from both sides. The removal of the incumbent party would severe either of these relationships. Details of the analysis are included in Online Appendix Section A.3.

election and negative if the incumbent party is reelected.[10] The RD variable of interest is $D_{mst} = 1[v_{ms(t-1)} > 0]$, where D_{mst} is equal to one when the incumbent is defeated, and zero otherwise, yielding a sharp discontinuity. The following equation describes the RD model that is estimated separately for the pre- and post-expiration periods.

$$\begin{aligned}\ln(y_{mst}) = \alpha + D_{mst}\theta + \sum_{p=1}^{P} v_{ms(t-1)}^{p} D_{mst}\gamma_p \\ + \sum_{p=1}^{P} v_{ms(t-1)}^{p}\delta_p + X_{mst}\pi + \ln(y_{ms(t-1)})\rho + \tau_{st} + \varepsilon_{mst}.\end{aligned} \quad (1)$$

The dependent variable is the log of the crime rate in municipality m, in state s and year t. The coefficient of interest is θ, which measures the effect of a close incumbent loss on crime rates. However, as described in the previous subsection this estimate contains two components: (i) a time-invariant destruction of the status quo, and (ii) the relative strength of criminal groups. The second factor, relative strength, is possibly affected by the expiration of the AWB due to increased access to more powerful firearms. The polynomial of the running variable, the vote margin ($v_{ms(t-1)}$), is allowed to vary on either side of the discontinuity. X_{mst} is a vector of municipal level control variables for crime rates, demographics, and additional election and party information.[11] These elections range over a number of years; therefore, a set of state-specific year fixed effects (τ_{st}) are included. When these state-year fixed effects are incorporated into the model, only state-year cells with at least five observations are used in the estimation.[12] Although it is not possible to include municipal fixed effects, each municipality holds mayoral elections every three years and not every election is a 'close' election, a lagged dependent variable, $\ln(y_{ms(t-1)})$, is included to take into account the municipal level environment in which the outcome of interest is determined.

To separate the effect of an incumbent loss estimated in the above equation from a change in crime levels coinciding with the expiration of the AWB, the RD model

10 Mexico is a multiparty democracy in which individual candidates cannot run for reelection; therefore, reelection following a coalition administration is defined using the following conditions. If the incumbent mayoral administration is a coalition, the vote share is assigned to the incumbent party using the following rules. If the PRI, PAN and/or PRD parties are part of the coalition, the incumbent share is defined as the largest vote share of these 'tier 1' parties. If no 'tier 1' parties are part of the incumbent coalition, the largest vote share of the 'tier 2' parties is assigned (PVEM, PANAL, PT, Convergencia). If none of these parties are part of the incumbent coalition, the largest vote share is assigned, or the incumbent share is equal to zero if no incumbent party is running for reelection. In 98.84% of elections, this is equivalent to assigning the vote share to the largest vote share of any incumbent party, regardless of tier. In the 1% of cases that a smaller member of the incumbent coalition outperforms a larger member, it is unclear how to define incumbent performance, these cases are not included in the baseline analysis. Estimates defining incumbent vote share as simply the largest incumbent party vote share regardless of tier can be seen in panel (d) of Online Appendix Table A.4.

11 Election variables include the total vote share of the winning party, an indicator of whether a coalition of parties won the election, an indicator if the election occurred in the last half of the year, two sets of indicator variables for each of the nine most common parties in this sample (one for whether the party won the election and the other for whether the party was an incumbent) and a set of indicators for each year the incumbent party had been in power at the time of the election. Municipal characteristics include the portion of population that is male age 20–49 years, the logged values of the population density, GDP per capita, municipal government income per capita, infant mortality rate, a cubic measure of distance to the border, vehicular mortality rate, narcotics crime rate, theft rate, illegal weapons possession rate, rape rate, assault and battery rate and the property damage rate.

12 Estimates including all available municipalities using year fixed effects can be found in Online Appendix Table A.4.

described in Equation (1) is combined with a difference-in-difference model to construct a DiRD model.[13] This model compares the change at the discontinuity in the post-expiration ($P_t = 1[t \geq 2005]$) period to the same change during the period when the law was in place.

$$\begin{aligned}\ln(y_{mst}) = \alpha + P_t D_{mst}\beta + D_{mst}\theta + \sum_{p=1}^{P} v^{p}_{ms(t-1)} D_{mst}\gamma_p + \sum_{p=1}^{P} v^{p}_{ms(t-1)}\delta_p \\ + P_t\left[\sum_{p=1}^{P} v^{p}_{ms(t-1)} D_{mst}\mu_p + \sum_{p=1}^{P} v^{p}_{ms(t-1)}\phi_p\right] + X_{mst}\pi + \ln(y_{ms(t-1)})\rho + \tau_{st} + \varepsilon_{mst}.\end{aligned} \tag{2}$$

The model closely builds on Equation (1). The dependent variable is again the log of the crime rate in municipality m, in state s and year t. The set of control variables and trends remain the same, with the addition of allowing the vote margin polynomial to also vary across time periods. The percent change in the crime rate following the expiration of the AWB, relative to the pre-expiration period, is captured by β, the coefficient on the interaction between the post-expiration indicator (P_t) and the defeat of an incumbent indicator (D_{mst}). The DiRD model differences out the effect of the loss of the status quo, which occurs in both the pre- and post-expiration periods, yielding the change in the effect that occurs at the time of the AWB's expiration. Observations are weighted by a triangular kernel; the kernel is interacted with the municipal population for all logged crime rates.[14] Optimal bandwidths are calculated using a cross-validation (CV) procedure (Ludwig and Miller, 2007; Imbens and Lemieux, 2008) that is adjusted to accommodate the DiRD model.

4. Data

4.1. Election data

This paper utilizes a unique dataset of every mayoral election in Mexico between 1995 and 2010. Election data for a number of years, especially for the earlier years of the sample, are compiled by the *Instituto de Mercadotecnia y Opinion*, and *Elecciones en Mexico* has assembled information for the most recent years. Data available from electoral institutes of each state are also used to cross-reference the other sources and to fill in missing information.[15] Local mayoral elections generally occur every 3 years. This pattern can be seen in the first two columns of panel (a) in Table 1. The number of mayoral elections held the previous year is shown in the first column, and the fraction of the total number of elections in this time period is shown in the second column. The years above the line at the center of the table are pre-expiration, and the years below are post-AWB. The rotation of municipal elections every 3 years is visible in these first two columns. The year in which elections are held is determined at the state level, the smallest fraction of the country holds elections prior to 2000, 2003, 2006 and 2009. A

13 Grembi et al. (2016), Lalive (2008) and Leonardi and Pica (2013) also utilize similar DiRD strategies.

14 Estimates using alternative weighting methods, including uniform weights are shown in Online Appendix Table A.4. The table also contains a set of estimates that includes an interaction between the control variables (X_{mst})and the post-AWB indicator (P_t).

15 Information from the *Instituto de Mercadotecnia y Opinion* can be found at http://imocorp.com.mx and at http://www.eleccionesenmexico.org.mx for *Elecciones en Mexico*.

Table 1. Mayoral elections in the previous year

(a) Mayoral elections in the previous year

	Number of elections Count	% of total	Close elections (<7%) Count	% of total	Percent of elections That are close: by year
1999	796	9.82	197	7.07	24.75
2000	232	2.86	72	2.59	31.03
2001	771	9.51	245	8.80	31.78
2002	846	10.43	303	10.88	35.82
2003	233	2.87	77	2.76	33.05
2004	575	7.09	224	8.04	38.96
2005	1018	12.55	396	14.22	38.90
2006	230	2.84	81	2.91	35.22
2007	577	7.12	207	7.43	35.88
2008	1033	12.74	335	12.03	32.43
2009	190	2.34	65	2.33	34.21
2010	599	7.39	190	6.82	31.72
Total	7100		2392		

(b) Number of incumbent losses

Losses (of 4)	Municipalities Count	Percent
0	252	13.52
1	395	21.19
2	640	34.33
3	447	23.98
4	130	6.97

Notes: Shown in panel (a) are the total number of mayoral elections (left), number of close mayoral elections (center) and percent of elections each year that are close (right). Displayed in panel (b) are the number of incumbent losses (out of 4) in each municipality.

significantly larger number of municipalities hold elections in the other two cycles. The range from 1999 to 2010 includes four full electoral rotations, two on either side of the AWB expiration. The next two columns show a similar distribution for close elections, defined as elections in which an incumbent party either wins or loses by less than seven percentage points, the CV optimal bandwidth. This subsample yields a similar pattern to the previous two columns. The fraction of municipalities from each year that fall below the 7% cutoff is shown in the last column of panel (a). The range is relatively consistent over the entire period. The years with the largest fraction of close elections are balanced on either side of the AWB's expiration, the last year pre-expiration, and the first year post-expiration.

Sorting across the election discontinuity is determined by the success or failure of the party of the incumbent mayor. The number of times an incumbent party lost a mayoral election over the four election cycles, from 1999 to 2010, is shown in panel (b) of Table 1 for each of the 1864 municipalities. The municipalities are distributed across all possible outcomes, from having no incumbent losses (252 municipalities) to always voting challengers into office (130 municipalities). The incumbent party is not reelected in nearly half of the elections, 49.79% of the time. The frequency with which incumbents lose elections ensures that a sufficient number of incumbent losses will be included in the sample. The distribution of the municipalities guarantees that incumbent losses are spread across most of the municipalities in the sample; over 85% of municipalities have at least one incumbent loss during this period.

Additionally, the municipalities in which incumbents are defeated are distributed throughout the country. Over the period of the study there were a number of confrontations between cartels that led to local spikes in violence; a study focusing on geographical variation may be susceptible to bias generated by these events. Allowing municipalities to be sorted across the election threshold yields a sample in which all regions of the country are represented on either side of the election cutoff. In the two election cycles prior to the AWB expiration (1999–2004) each of the 31 states in the sample include both municipalities in which the incumbent won a close election, and those in which the incumbent lost. In the two election cycles following the expiration of the AWB (2005–2010), 29 of the 31 states include both types of municipalities, the two states that do not meet these criteria (Federal District and Baja California Sur) have a total of three close elections between them in the post-expiration period. In every part of the country incumbents both won and lost close elections, both prior to, and following the expiration of the AWB. The distribution of these elections are shown in Figure 4, for both the pre-expiration (Figure 4a) and post-expiration periods (Figure 4b).[16]

4.2. Municipal data

The main analysis of this paper consists of annual observations for every municipality outside of the state of Oaxaca, from 2000 to 2006. A total of 1864 municipalities.[17] Population and mortality (homicide and vehicle-caused deaths) data are from Mexico's National System of Health Information (SINAIS—*Sistema Nacional de Informacion en Salud*). Data for both firearm homicides and non-firearm homicides are also obtained from SINAIS, and are available beginning in 1998. Non-firearm homicides are defined as the difference between the total number of homicides and those committed with the use of a firearm. Crime data and municipal data for all other control variables are from Mexico's National Institute of Statistics and Geography (INEGI—*Instituto Nacional de Estadistica y Geografia*). To minimize any potential corruption in the judicial process,

16 Municipalities where incumbents won reelection by less than 7% twice during the specified time period are filled in black, and dark gray municipalities denote a single close incumbent win. The checkered pattern denotes municipalities in which there were two close incumbent elections, one won by the incumbent and the other lost. The municipalities shaded with a medium gray are those in which the incumbent lost a single close election, and municipalities filled in with light gray saw two close incumbent defeats. The gray lines represent state borders, and blank areas saw no close elections during the time period.

17 Municipal government structure and mayoral elections in the state of Oaxaca differ from other states in Mexico, and a number of control variables are missing for these municipalities. Furthermore, there are over twice as many municipalities in Oaxaca than any other state in Mexico. This leads to Oaxaca having the smallest average population per municipality with only about a third of the population of the next smallest state. Due to these traits, municipalities from Oaxaca are not included in the analysis.

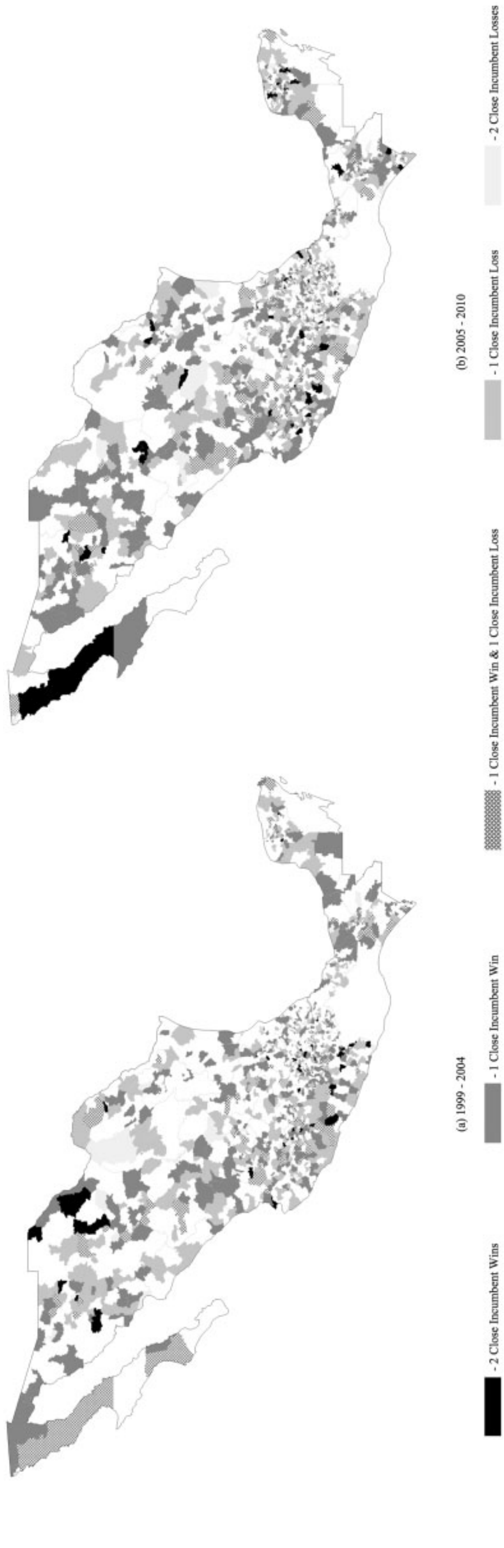

Figure 4. Close municipal elections: 1999–2010.

crime data from the beginning of the sample to 2008 are for the crimes of suspected criminals. After 2008, the definition used by INEGI changed to crimes of the accused criminals. These definitions do not require a conviction, only a documented crime, lessening the possibility of corruption skewing the data.[18]

Means of key variables, separated by incumbent election status and time-period, are included in Table 2 for the years 2000 to 2006. The variables shown are time-invariant, election outcomes, or once lagged to allow for validity tests of the DiRD model in the following section. All rates represent occurrences per 10,000 people, and log calculations take the natural log of the rate plus one to maintain a consistent measure of zero when there are no occurrences.[19] Variables included in the table, beginning at the top, are three separate homicide rates, the logged firearm homicide rate (the main outcome of interest), the logged (total) homicide rate and the logged non-firearm homicide rate. Logged crime rates included are theft, narcotics crime, illegal arms possession, assault and battery, property damage and rape. These crime rates are also used as control variables, and to demonstrate that the post-AWB increase is isolated to firearm homicides. The municipal characteristics shown are also included in the main model as control variables. Distance is calculated as the driving distance in kilometers to the nearest border crossing, and area is recorded as square kilometers. The selection of variables includes those that take into account municipal level well-being and investment, such as the infant mortality rate, per capita GDP and municipal income (taxes) collected.[20] The vehicular mortality rate is one of the few causes of death that has an age profile similar to homicides, and mostly young men are involved in violent crime; therefore, the fraction of the population that is male between 20 and 49 is included. Election statistics include the years an incumbent party has been in power, fraction of municipalities won by PAN and the vote share of the candidates from the three largest parties, PAN, PRI and PRD.

5. Results

5.1. Validity tests

Hahn et al. (2001) outline key assumptions that must be met to ensure proper identification of an RD model. Grembi et al. (2016) extend the necessary assumptions to the DiRD framework. In a cross-sectional RD model, the key assumption is that

18 During the pre-2008 period, the illegal arms possession variable is constructed as the sum of illegal arms possession and violations of federal firearms law. The two variables are recorded separately only before 2008; combining the two variables during this period creates a consistent variable that can be used when the time period of the sample is expanded. Estimates of the paper's main analysis are not sensitive to the substitution of violations of the federal firearms law in the 2000 to 2006 period. See Online Appendix Table A.5.

19 The paper's main estimates remain qualitatively similar, and results for firearm homicides statistically significant, with alternative rate calculations (i.e. 5000, 50,000 and 100,000). The base estimates use a logged crime rate per 10,000 to ensure that the model does not overweight smaller municipalities with one homicide. Estimates with alternative rate calculations and municipal population cutoffs can be found in Online Appendix Table A.4.

20 Distances are calculated using Google Maps. The furthest municipality is Isla Mujeres, which is 2306 km from the border. Municipal level GDP data are available in 5-year intervals, and state level data are available annually. Annual GDP estimates for municipalities were imputed using trends in the share of state GDP between the 5-year intervals. Only expenditure data, not municipal income data, are available for the municipalities in the Federal District.

Table 2. Means for pre- and post-AWB expiration time-periods, by election outcome of the incumbent party

Bandwith = 0.07	Pre-expiration means (2000–2004)				Post-expiration means (2005–2006)				Post-Exp. minus pre-exp.	
	Incumbent victory	Challenger victory	Difference	T-stat	Incumbent victory	Challenger victory	Difference	T-stat	Difference-in-difference	T-stat
ln(FirearmHom.Rate)$_{ms(t-1)}$	0.332	0.320	−0.012	[0.38]	0.292	0.212	−0.080	[1.90]*	−0.068	[−1.25]
ln(HomicideRate)$_{ms(t-1)}$	0.533	0.508	−0.026	[0.70]	0.441	0.373	−0.067	[1.36]	−0.042	[−0.66]
ln(Non − Fire.Hom.Rate)$_{ms(t-1)}$	0.270	0.251	−0.019	[0.80]	0.197	0.200	0.002	[−0.08]	0.022	[0.53]
ln(TheftRate)$_{ms(t-1)}$	0.900	0.922	0.022	[−0.40]	0.728	0.604	−0.124	[1.73]*	−0.146	[−1.60]
ln(NarcoticsCrimeRate)$_{ms(t-1)}$	0.402	0.362	−0.041	[1.02]	0.273	0.267	−0.005	[0.11]	0.035	[0.55]
ln(IllegalArmsRate)$_{ms(t-1)}$	0.620	0.653	0.032	[−0.74]	0.622	0.524	−0.098	[1.67]*	−0.130	[−1.76]*
ln(AssaultBatt.Rate)$_{ms(t-1)}$	1.037	1.007	−0.030	[0.59]	0.905	0.877	−0.028	[0.40]	0.002	[0.02]
ln(Prop.DamageRate)$_{ms(t-1)}$	0.498	0.530	0.032	[−0.86]	0.480	0.433	−0.047	[0.85]	−0.079	[−1.21]
ln(RapeRate)$_{ms(t-1)}$	0.252	0.269	0.017	[−0.70]	0.227	0.205	−0.022	[0.71]	−0.039	[−0.97]
Distance$_{ms}$	981	969	−11.71	[0.41]	1104	1102	−2.520	[0.06]	9.192	[0.18]
Area$_{ms}$	922	1069	146	[−0.82]	1115	778	−338	[1.27]	−484	[−1.54]
ln(GDPPerCapita)$_{ms(t-1)}$	8.952	8.988	0.036	[−1.06]	8.764	8.790	0.026	[−0.57]	−0.010	[−0.17]
ln(InfantMort.Rate)$_{ms(t-1)}$	1.341	1.316	−0.025	[0.53]	1.229	1.232	0.004	[−0.06]	0.029	[0.36]
ln(TaxRev.PerCapita)$_{ms(t-1)}$	5.765	5.768	0.003	[−0.06]	6.003	5.961	−0.042	[1.06]	−0.045	[−0.68]
ln(VehicularMort.Rate)$_{ms(t-1)}$	0.505	0.556	0.050	[−1.32]	0.495	0.484	−0.010	[0.19]	−0.061	[−0.92]
ln(Pop.Density)$_{ms(t-1)}$	4.003	4.088	0.085	[−0.80]	3.943	4.023	0.080	[−0.63]	−0.004	[−0.03]
PercentofPop.Male : 20 − 49$_{ms(t-1)}$	0.187	0.187	0.000	[−0.10]	0.185	0.185	0.000	[−0.16]	0.000	[0.08]
YearsInPower$_{ms(t-1)}$	4.288	4.446	0.158	[−1.55]	5.945	5.668	−0.277	[1.15]	−0.435	[−1.95]*
PAN; Win$_{ms(t-1)}$	0.172	0.362	0.190	[−6.63]***	0.174	0.405	0.231	[−5.66]***	0.041	[0.83]
Winning; Vote; Share$_{ms(t-1)}$	0.421	0.415	−0.006	[1.27]	0.418	0.406	−0.012	[1.60]	−0.006	[−0.63]
PAN; Vote; Share$_{ms(t-1)}$	0.281	0.302	0.021	[−1.92]*	0.278	0.293	0.015	[−0.95]	−0.006	[−0.34]
PRI; Vote; Share$_{ms(t-1)}$	0.406	0.389	−0.017	[3.17]***	0.399	0.376	−0.023	[2.69]***	−0.005	[−0.54]
PRD; Vote; Share$_{ms(t-1)}$	0.244	0.221	−0.022	[1.87]*	0.252	0.260	0.008	[−0.51]	0.030	[1.52]
N		921				477			1398	

Notes: ***$p<0.01$, **$p<0.05$, *$p<0.1$. Data include all municipality observations, outside of Oaxaca, with an election in which the incumbent party margin is within seven percentage points. All rates are per 10,000 people, and logged variables are calculated as the natural log of the rate plus one to include observations with zero occurrences. Per capita GDP and municipal income are recorded in 1000s of constant 1993 Mexican pesos. Population, homicide and mortality data are from SINAIS; all other data are from INEGI.

time-invariant and predetermined characteristics are equivalent within a sufficiently small range of the running variable, $E\,[x_m|v_m = v_m + e] \approx [x_m|v_m = v_m - e]$, most importantly this assumption must hold in the range of values that brackets the discontinuity threshold. Expanding this to the DiRD model only requires that any difference that may exist be consistent in both the pre- (0) and post-treatment (1) periods, $\{E\,[x_{m1} \mid v_{m1} = v_{m1} + e] - E\,[x_{m1}|v_{m1} = v_{m1} - e]\} \approx \{E\,[x_{m0} \mid v_{m0} = v_{m0} + e] - E\,[x_{m0}|v_{m0} = v_{m0} - e]\}$. If the RD assumptions are satisfied within each time period, it is easy to see that the DiRD criteria will also be met. While it remains unrealistic to definitively test the assumption that the characteristics of observations on either side of the cutoff are identical, it is possible to demonstrate that any differences in the observable characteristics of municipalities near the cutoff are consistent over time, if they exist at all. These differences are first examined using the sample means presented in Table 2. The DiRD model is used to test the same set of variables, and examine whether the variation in these characteristics at the election threshold remained consistent across the expiration of the AWB. Finally, the McCrary (2008) density test can be expanded to accommodate the DiRD framework to test if there is a difference in the pre- and post-expiration densities around the cutoff.

To initially test the balance of the sample across the discontinuity the difference in the means across the election threshold are calculated separately for the pre- and the post-expiration time periods, and are shown along with the t-statistic of the difference in Table 2. It is important to note that the characteristics being tested are either time-invariant (distance and area), or determined prior to the newly elected mayor's first full year in office. Only three non-election municipal characteristics across either time period have a statistically significant difference. They are the post-expiration lagged firearm homicide rate, the lagged theft rate and the lagged illegal arms rate. For each of these differences, the level of crime is lower for the municipalities where the incumbent lost; if this has any impact on the rate in the following year, it would bias the model away from finding a result. However, as described above this is not the key difference that needs to be explored. The key statistic of interest to the DiRD model is the difference of the differences, shown at the right-hand side of the table, again along with the t-statistic of the difference-in-difference calculation. Only one of the three variables that exhibited a difference in the post-AWB period is statistically different from the pre-expiration period. However, this difference suggests a relatively lower level of illegal arms possession following an incumbent defeat in the post-AWB period. Again, likely making it more difficult to find an impact of illegal arms during this period. The differences that occur more frequently during each time period are for the election outcomes. PAN are more likely to win close elections as the challenger, but consistently so across both time periods. Although the vote shares demonstrate some statistically significant differences within each time period, these differences are also consistent across time as seen in the difference-in-difference calculations.

These tests can be complemented by using the DiRD framework, described in Equation (2), to estimate the difference between the pre- and post-periods more precisely at the discontinuity threshold. When estimating the model using time-invariant characteristics as the outcome of interest, road distance to the nearest U.S. border crossing and area, the vector of control variables are not included. These estimates, obtained at the CV optimal bandwidth, can be seen in panel (a) of Table 3. The coefficient on distance is small and statistically insignificant, and after a single outlier is removed (Ocampo, Coahuila), there is no evidence of a statistically significant

Table 3. DiRD—estimates of time-invariant, election outcomes and lagged municipal characteristics and crime rates (2000–2006)

(a) Time-invariant	(1)	(c) Election outcomes	(2)	(d) Lagged alternative crimes	(3)	(e) Lagged municipal characteristics	(4)
$Distance_{ms}$	−12.35	PAN $Win_{ms(t\text{-}1)}$	0.016	ln(Theft $Rate)_{ms(t\text{-}1)}$	0.219	ln(GDP Per $Capita)_{ms(t\text{-}1)}$	0.053
	(53.95)		(0.189)		(0.208)		(0.094)
$Area_{ms}$	−1644**	PAN Vote $Share_{m(t\text{-}1)}$	−0.025	ln(Narcotics Crime $Rate)_{ms(t\text{-}1)}$	−0.127	ln(Infant Mort. $Rate)_{ms(t\text{-}1)}$	−0.020
	(678)		(0.053)		(0.167)		(0.194)
$Area_{ms}$ (*drop one municipality*)	−744	PRI Vote $Share_{ms(t\text{-}1)}$	0.004	ln(Illegal Arms $Rate)_{ms(t\text{-}1)}$	−0.078	ln(Tax Rev. Per $Capita)_{ms(t\text{-}1)}$	0.334*
	(563)		(0.032)		(0.196)		(0.188)
(b) Lagged homicides		PRD vote $Share_{ms(t\text{-}1)}$	0.053	ln(Assault and Battery $Rate)_{ms(t\text{-}1)}$	0.646***	ln(Vehicular Mortality $Rate)_{ms(t\text{-}1)}$	0.240
			(0.056)		(0.213)		(0.189)
ln(Firearm Homicide $Rate)_{ms(t\text{-}1)}$	−0.127	Winning Vote $Share_{ms(t\text{-}1)}$	0.024	ln(Property Damage $Rate)_{ms(t\text{-}1)}$	−0.151	Percent of Pop. Male: $20–49_{ms(t\text{-}1)}$	−0.006
	(0.144)		(0.028)		(0.164)		(0.005)
ln(Non-Firearm Homicide $Rate)_{ms(t\text{-}1)}$	0.147	Years In $Power_{ms(t\text{-}1)}$	−1.337	ln(Rape $Rate)_{ms(t\text{-}1)}$	−0.188	ln(Pop. $Density)_{ms(t\text{-}1)}$	0.888*
	(0.124)		(0.818)		(0.123)		(0.464)
ln(Homicide $Rate)_{ms(t\text{-}1)}$	0.032						
	(0.166)						
N	1360	N	1360	N	1360	N	1360

Notes: $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$. The dependent variable is the value of the specified municipal characteristic. The estimates shown are for the coefficient (β) on the interaction between the indicator variable for an incumbent defeat (D_{mst}) and the post-AWB indicator (P_t), from Equation (2). Each regression includes state-year fixed effects, a cubic RD polynomial allowed to vary on either side of the election threshold and AWB expiration. The top five estimates in panel (c) include an indicator for whether the election is held in the last half of the year, and the predetermined outcomes of years in power (set of indicators), incumbent party (set of indicators), area and distance (cubic). The estimate for years in power, determined in the previous election, only include the area and distance (cubic) controls. Excluding the dependent variable, all estimates in panels (b), (d) and (e) also include lagged values of the portion of the population that is male age 20–49 years, the logged population density, GDP per capita, municipal government income per capita, infant mortality rate, vehicular mortality rate, narcotics crime rate, theft rate, illegal weapons possession rate, rape rate, assault and battery rate and the property damage rate. The eligible sample includes all municipalities outside of Oaxaca. Each regression is weighted using a triangular kernel, calculated using the vote margin, which is interacted with population for panels (b), (d) and (e), and total votes cast for panel (c).

change in municipality area at the RD threshold. Estimates in panel (c) demonstrate that the election environment and outcomes are consistent over time, at the threshold. Similar to Table 2, there is no evidence that PAN is more likely to win close elections following the expiration of the AWB, and no evidence of a change in the vote share received on either side of the election threshold for any of the three major parties or in the winning party's vote share. Unlike the calculations in Table 2, the final estimate in panel (c) finds no statistically significant evidence of a change in the length of time that the incumbent party was in power. Importantly, this evidence reveals that the electoral environment remained consistent through the ban's expiration.

Estimates of once lagged, $(t-1)$, crime rates and municipal characteristics are shown in panel (d) and panel (e), respectively. The difference in the discontinuities for five of the six municipal crime rates are statistically insignificant. This includes the illegal weapons possession variable, which is statistically significant and negative in Table 2. The only crime variable that shows evidence of imbalance in the lagged period is assault and battery. However, this variable is small and statistically in significant in Table 2, and the post-AWB change in discontinuity will be shown to be small and statistically insignificant when using time t, year after the election, crime rates. This movement is counter to the evolution in firearm homicides, suggesting that the assault and battery rate is not a factor contributing to the change in firearm violence. Of the six municipal characteristics used as control variables in this study, only the logged tax rate and population density yield statistically significant evidence of possible imbalance across the AWB expiration. However, like the estimates for area in panel (a), removing a single outlier again yields estimates that are small and statistically insignificant.[21]

Like the estimates in Table 2, the estimates for all three homicides rates, in panel (b) of Table 3, are small and statistically insignificant. Most importantly, there is no evidence of a difference in the discontinuities of the lagged firearm homicide rate, the key variable of interest. These results establish that all three lagged homicide rates were consistent on either side of the AWB expiration, at the election threshold. Together, the full set of baseline estimates shown in Table 2 and Table 3 demonstrate that any change in homicide rate following the expiration of the AWB cannot be explained by permanent characteristics of the municipality, the election environment in which the incumbent was removed from office or observable characteristics of the municipality prior to the newly elected official taking charge.

Finally, the density of the municipalities around the election threshold is examined. The distribution of municipalities sorted by vote margin is shown in Figure 5, separately for (a) pre- and (b) post-AWB expiration periods. Positive margins indicate an incumbent defeat. In both time periods, the distribution of municipalities follows a similar pattern, both distributions have a steady increase through the threshold to the modal bin which is immediately to the right of the cutoff. With the distribution skewed away from incumbent party reelection it is unlikely that the group in power is manipulating the election; furthermore, the distributions yield comparable patterns, suggesting that the election dynamics are similar both before and after the expiration of

21 The coefficient estimate for the lagged ln(Tax Rate) reduces to 0.187 with a standard error of (0.189) when Poza Rica de Hidalgo, Veracruz is removed from the estimate. Without Guadalajara, Jalisco the estimate for the lagged ln(Population Density) reduces to 0.213 with a standard error of (0.445). The paper's main model is also re-estimated excluding these municipalities. The results for the firearm homicide rate remain statistically significant, and can be found in Appendix Table A.4.

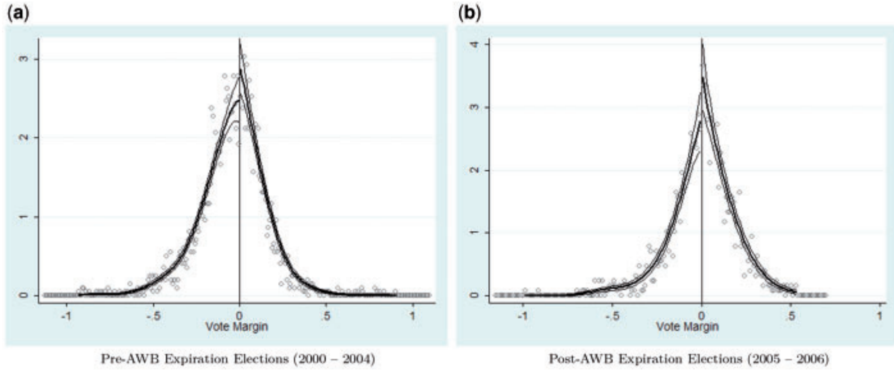

Figure 5. Frequency of municipalities by vote margin (>0 if incumbent defeated).

Table 4. DiRD—density test—estimates of the logged bin count

	ln(Bin Count)			
	BW = 0.10	BW = 0.15	BW = 0.20	BW = 0.25
	(1)	(2)	(3)	(4)
$P_t * D_{mst}$ (Bin Size = 0.01)	0.040	−0.022	−0.028	−0.111
	(0.255)	(0.236)	(0.241)	(0.260)
$P_t * D_{mst}$ (Bin Size = 0.005)	0.080	−0.007	0.017	−0.035
	(0.259)	(0.273)	(0.248)	(0.241)
$P_t * D_{mst}$ (Bin Size = 0.002)	0.076	0.010	−0.086	−0.062
	(0.281)	(0.227)	(0.208)	(0.187)

Notes: ***$p<0.01$, **$p<0.05$, *$p<0.1$. The dependent variable is the logged count of observations within the specified bin size. The estimates shown are for the coefficient on the interaction between the indicator variable for an incumbent defeat and the post-AWB indicator, from a density test adjusted to the DiRD framework.

the AWB. Whether there exists any difference in the density at the threshold between the two time periods can be more formally tested by comparing logged bin counts in a DiRD framework. The results across a number of bandwidths and bin sizes are shown in Table 4. Estimates across all bandwidth and bin size combinations yield small and statistically insignificant estimates.[22] This information can also be expressed by plotting the differences in the logged bin count (post–pre) across the election threshold. These plots can be seen in Figure 6 for the 20% bandwidth and all three bin sizes (0.01, 0.005 and 0.0025). These figures again show no evidence of a change in the distribution of municipalities around the election threshold.

22 The combinations of options shown in Table 4 are selected to cover the optimized bin size (0.007 ~ 0.01) and bandwidth (0.16 ~ 0.19) for the McCrary (2008) density test for the pre- and post-RD samples. Estimates shown are from a model with a quadratic polynomial to best match point estimates from the separate pre- and post-McCrary density tests. Estimates using a cubic polynomial again yield 16 statistically insignificant estimates.

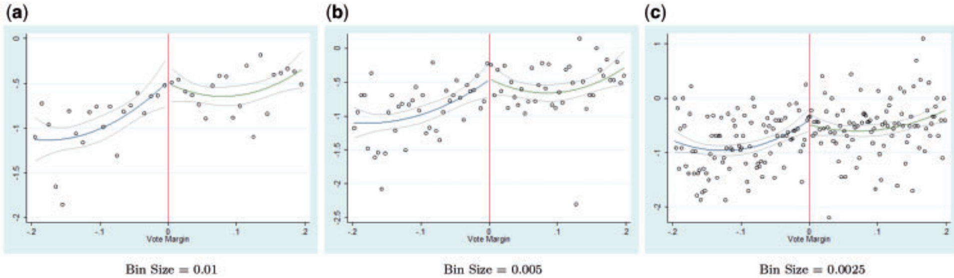

Figure 6. Difference in logged bin count (Post–Pre; Bandwidth = 0.20).

5.2. Firearm homicide rate and the expiration of the AWB

Before using Equation (2) to estimate the effect of the expiration of the AWB on the firearm homicide rate, the RD model, estimated separately for the pre- and post-expiration periods, can be used to illustrate how the logged firearm homicide rate changes across the election threshold. To do this, the residuals of the logged firearm homicide rate are extracted using the control variables and state-year fixed effects shown in Equation (1). The residuals are plotted in Figure 7 on either side of the vote threshold with a cubic spline and 95% confidence bands. The CV optimized bandwidth (0.07) is shown with 35 bins on either side of the election threshold, and observations are weighted by the interaction between a triangular kernel and municipal population. The incumbent vote margin is plotted on the horizontal axis. Positive values denote a victory by a challenger, and negative values an incumbent victory. Prior to the expiration of the AWB, Figure 7(a), there is a large decline at the election threshold. This decline is the graphical representation of the coefficient, θ, on the RD indicator variable, D_{mst}, in Equation (1). The same calculation is repeated for the post-expiration time period, shown in Figure 7(b); here the effect is less pronounced, but positive. A significant change from the pre-expiration period. The coefficient, β, on the DiRD estimator, P_tD_{mst}, from Equation (2), calculates the difference between the post-expiration change and the pre-expiration change. This can be demonstrated by taking the difference, post minus pre, of each bin shown in Figure 7(a) and (b). The result is shown in Figure 7(c) for the 2000–2006 period, and in Figure 7(d) for the extended 1999–2010 period. Each of these figures illustrates the sizable increase in the firearm homicide rate that followed the expiration of the AWB.

To quantify the magnitude of the effects seen in Figure 7, estimates using the natural log of the firearm homicide rate as the dependent variable are shown in Table 5. Each estimate is from a separate regression and number of observations in each regression is shown below the point estimate and standard error. Estimates in panel (a) use the CV optimal bandwidth, alternative bandwidths from 0.05 to 0.09 are shown in panel (b). RD estimates of θ, from Equation (1), are shown for the pre-expiration period in column (1). As seen in the previous figure, the estimates for the effect of an incumbent loss prior to the expiration of the ban is large and negative across all bandwidths. This suggests that without the availability of the restricted firearms, the relative strength of the newly elected mayors enables them to overcome any adverse impact generated by the deterioration of the status quo. The same exercise is repeated for the post-AWB period, the RD estimates are shown in column (2). The point estimates are positive, but

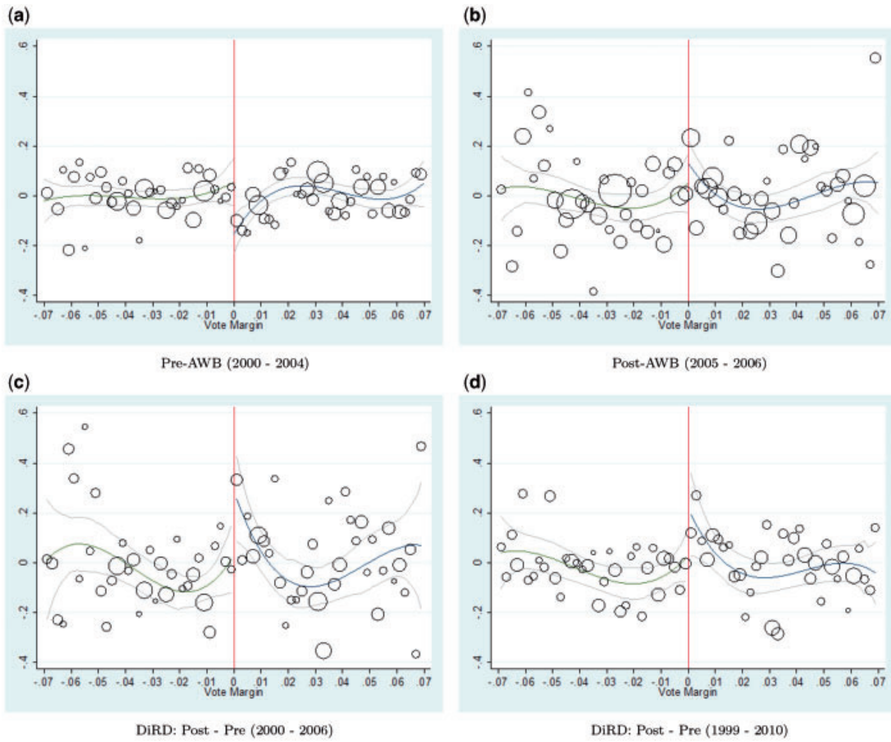

Figure 7. Residuals of ln(firearm homicide rate): Using Equation (3) controls (X_{mst},$\ln(y_{ms(t-1)})$, $\tau_{(s)t}$).

generally smaller in magnitude than the pre-expiration estimates. This matches the pattern seen in the residual plot. However, the correct comparison is not the value of the post-expiration estimate alone, but whether there was a change relative to the pre-expiration baseline.

Equation (2) is used to estimate the difference in the incumbency effect before and after the expiration of the AWB; the difference is captured by the equation's β coefficient. The DiRD estimates for Equation (2)'s β coefficient, with the logged firearm homicide rate as the dependent variable, are shown in column (3) of Table 5. The estimates for the change in the firearm homicide rate following the ban's expiration are positive and statistically significant for each of the five bandwidths. At the CV optimal bandwidth, the model estimates an increase in the firearm homicide rate of about 35% following the expiration of the AWB.[23] This evidence is consistent with the hypothesis

23 Alternative model specifications are shown in Online Appendix Table A.4. The table includes the baseline estimates for comparison, and 19 alternatives, each estimated using the state-year fixed effects used throughout the paper as well as models with only year fixed effects, and a post-AWB indicator variable. Estimates are shown for alternative weights, definition of incumbency, samples restricted to larger municipalities, samples dropping municipalities with the largest change in narcotics related activity, samples dropping municipalities with the highest levels of firearm homicide rates to rule out the effect of outliers, samples using crime rates per 100,000, 50,000 and 5000, alternative spline calculations and estimates dropping the outlying municipalities from the baseline estimates. Furthermore, to ensure that no single municipality is driving the firearm homicide estimates, the model was estimated dropping

Table 5. DiRD—effect of AWB expiration on ln(Firearm Homicide Rate) (2000–2006)

	Pre-expiration (2000–2004)	Post-expiration (2005–2006)	DiRD (2000–2006)
	(1)	(2)	(3)
		(a) CV optimal bandwidth	
ln(Firearm Homicide Rate)$_{mst}$	−0.262***	0.079	0.354***
BW = 0.07	(0.080)	(0.106)	(0.131)
	N=897	N=463	N=1360
		(b) Alternative bandwidths	
ln(Firearm Homicide Rate)$_{mst}$	−0.180*	0.085	0.276*
BW = 0.05	(0.105)	(0.114)	(0.157)
	N=638	N=344	N=982
ln(Firearm Homicide Rate)$_{mst}$	−0.197**	0.201*	0.434***
BW = 0.06	(0.090)	(0.108)	(0.142)
	N=760	N=399	N=1159
ln(Firearm Homicide Rate)$_{mst}$	−0.211***	0.003	0.273**
BW = 0.08	(0.072)	(0.096)	(0.119)
	N=1016	N=518	N=1534
ln(Firearm Homicide Rate)$_{mst}$	−0.229***	0.115	0.333***
BW = 0.09	(0.065)	(0.092)	(0.110)
	N=1130	N=583	N=1713

Notes: ***$p<0.01$, **$p<0.05$, *$p<0.1$. The dependent variable in all estimates is the natural log of the firearm homicide rate. The estimates shown in column (3) are for the coefficient (β) on the interaction between the indicator variable for an incumbent defeat (D_{mst}) and the post-AWB indicator (P_t), from Equation (2); the estimates shown in column (1) and column (2) are for the coefficient (θ) on the indicator for an incumbent defeat, from Equation (1). Regressions include state-year fixed effects, a lagged value of the dependent variable, a cubic RD polynomial allowed to vary on either side of the election threshold and AWB expiration, and the set of municipal controls described in footnote 11. The eligible sample includes all municipalities outside of Oaxaca. Each regression is weighted using an interaction between a triangular kernel, calculated using the vote margin and the municipal population.

that the expiration of the AWB contributed to the reversal of the firearm homicide trend in Mexico that began in 2005. Furthermore, the relationship described by the estimates in Table 5 must exist if the theory that the increased availability of high powered firearms led to an increase in the firearm homicide rate is correct. Due to the central importance of this result to the paper's hypothesis, the following sections investigate alternative explanations of the increase in the firearm homicide rate, a more detailed analysis of the timing of the increase, and whether the distance to the U.S. border impacts the magnitude of the increase in firearm homicides.

5.3. Examining alternative explanations

The evidence that the increase in the firearm homicide rate coincided with the expiration of the AWB is shown in the previous section. However, this relationship

one municipality at a time. Each point estimate remains statistically significant at the 95% confidence level, and ranges from 0.290 to 0.382. These results demonstrate the strength of the firearm homicide estimates, and rule out any outlying municipality driving the result.

alone is not enough to isolate the cause of the increase in firearm homicides to the expiration of the AWB. A central concern could be that the increase in firearm homicides identified in the previous section is driven by a larger secular trend affecting all homicide and crime rates. To investigate this, the DiRD model shown in Equation (2) is estimated using the non-firearm homicide rate and a number of other crime rates, as the dependent variable. The estimates of the DiRD coefficient, β, for these alternative crime rates are shown in panel (a) of Table 6. The estimate for the logged non-firearm homicide rate is shown in column (2), and for comparison the analogous estimate using the logged firearm homicide rate can be found in column (1). Estimates for the overall homicide rate, both firearm and non-firearm, are shown in column (3), and the other six columns include estimates using the alternative crime rates as the dependent variable.

The most important takeaway from Table 6 is that there was no concurrent increase in the non-firearm homicide rate; this isolates the increase in homicides to only those committed with firearms. However, increased access to more deadly firearms could have simply substituted murders that would have happened in any state of the AWB from non-firearm homicides to homicides with a firearm. If this substitution occurred, the change in firearm homicides would be more a function of the individuals wielding the firearms, and not the actual firearms. However, there is no evidence of a decline in the non-firearm homicide rate. This removes the possibility that the increase in the firearm homicide rate was a simple substitution away from non-firearm homicides, and lends more evidence to the hypothesis that newly available assault weapons led to an increase in homicides that would not have otherwise occurred. This is directly seen in column (3), where the model estimates a large, roughly 30%, and statistically significant increase in the overall homicide rate.

Examining the crime rates from six of the most common non-homicide crimes yields insight into whether a secular trend in the general level of crime could be behind the surge in the post-AWB firearm homicide rate. Interestingly, estimates for illegal arms possession are small and statistically insignificant. This is consistent with the theory that the expiration of the AWB changed the type of firearms available and that these guns are more lethal; to impact the level of gun-violence the expiration need not affect the quantity of firearms available.[24] Estimates for the rate of theft, assault and battery and rape are also small and statistically insignificant. The estimate for property damage is larger, but also statistically insignificant. As previously discussed, assault and battery is the only crime that is not balanced in the lagged period. However, as seen in Table 6, there was no increase in this violent crime that contemporaneously changed with the firearm homicide rate.

The only non-firearm crime that consistently shows evidence of change following the expiration of the AWB is the level of narcotics crime. The importance of this result is that it contains no evidence of a rise in narcotics-related crime that could have caused the increase in firearm homicides following 2004. Possible explanations for this change could be a strategic shift away from municipalities where the incumbent party was defeated, much like the results found in Dell (2015); this type of movement of criminal

24 Evidence of this is shown in Online Appendix Table A.1, where it is documented that following the expiration of the AWB, the rate of firearm homicides increased relative to a given level of illegal arms possession.

Table 6. DiRD—effect of AWB expiration on alternative crime rates (2000–2006)

	ln(Firearm Homicide Rate)	ln(Non-Firearm Homicide Rate)	ln(Homicide Rate)	ln(Theft Rate)	ln(Narcotics Crime Rate)	ln(Illegal Arms Rate)	ln(Assault and Battery Rate)	ln(Property Damage Rate)	ln(Rape Rate)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(a) Alternative Crime Rates (2000–2006)									
$P_t{}^*D_{mst}$	0.354***	0.121	0.320**	−0.060	−0.708***	0.032	0.141	0.256	−0.110
N = 1360	(0.131)	(0.119)	(0.154)	(0.201)	(0.150)	(0.177)	(0.195)	(0.166)	(0.123)
(b) Extended sample (1999–2010)									
$P_t{}^*D_{mst}$	0.255**	0.015	0.190*	−0.040	−0.240**	0.097	0.146	0.233**	−0.023
N = 2307	(0.101)	(0.086)	(0.113)	(0.140)	(0.113)	(0.130)	(0.136)	(0.115)	(0.084)
(c) Extended sample: remove top decile of pre-expiration narcotics crime (1999–2010)									
$P_t{}^*D_{mst}$	0.240**	0.035	0.188	0.014	−0.220*	−0.116	0.057	0.190	−0.077
N = 2073	(0.104)	(0.092)	(0.120)	(0.154)	(0.114)	(0.137)	(0.150)	(0.124)	(0.091)

Notes: $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$. The dependent variable is the natural log of the stated crime. The estimates shown are for the coefficient (β) on the interaction between the indicator variable for an incumbent defeat (D_{mst}) and the post-AWB indicator (P_t), from Equation (2). Regressions include state-year fixed effects, a lagged value of the dependent variable, a cubic RD polynomial allowed to vary on either side of the election threshold and AWB expiration, and the set of municipal controls described in footnote 11, excluding the dependent variable. The eligible sample includes all municipalities outside of Oaxaca. Each regression is weighted using an interaction between a triangular kernel, calculated using the vote margin and the municipal population.

organizations would likely put a downward pressure on violent activity. Alternatively, this result could also be due to the strengthening of these groups, relative to the local government, following the increase in their access to high powered firearms. In this case, the estimate would be additional evidence of a decline in the government's relative strength following the expiration of the AWB. While the narcotics crime result is interesting, examining its cause is largely outside the scope of this study. Of central importance is that there is no evidence that narcotics crime increased at the same time as the firearm homicide rate. Additionally, evidence presented in later in this section shows that there was no change in the overall narcotics market that coincided with the expiration of the AWB, and that pre-expiration levels of narcotics crime are unrelated to the magnitude of the increase in the post-AWB firearm homicide rate. The estimates in Table 6 indicate that there was no general increase in crime rates, or non-firearm homicides, that coincided with the increase in the firearm homicide rate. Furthermore, the increase in the firearm homicide rate was not a substitution away from other types of homicides, but led to an increase in the overall rate of homicide.

Extending the model past 2006 is not entirely straightforward. The DiRD model used in this paper identifies the effect of pre- and post-expiration discontinuities using the change in the party of a municipality's mayor, irrespective of which party is entering or leaving office. However, during Calderon's administration, Dell (2015) found that Calderon's PAN party begins to act more aggressively against drug trafficking organizations.[25] Therefore, expanding the model into the period of the Calderon administration makes identification, and possibly interpretation, more difficult. Expanding the sample to include the years from 1999 to 2010 allows two cycles of mayoral elections to occur on either side of the AWB expiration. Estimates for this time period are shown in panel (b) of Table 6.

The pattern of the estimates for this extended sample mirrors the results from the original 2000 to 2006 sample. The estimate on the logged firearm homicide rate is large, positive and statistically significant. The estimate for the non-firearm homicide rate is close to zero and statistically insignificant; the estimate for the homicide rate is large, positive, and statistically significant at the 90% confidence level. Four of the other crime rates show no evidence of an effect, the narcotics crime rate is again negative and the effect on property damage is positive and statistically significant in this extended time period.

It is unclear exactly how much of these estimates are driven by the expiration of the AWB, and how much may be due to the aggressive tactics employed in the later years of the expanded sample. This aggressiveness was targeted at parts of the country in which cartel activity and violence increased to unacceptable levels; therefore, to attempt and correct for this, municipalities that were in the top decile of pre-expiration narcotics crime are removed from the sample and the model re-estimated. This strategy has the added benefit of only relying on pre-expiration information, ensuring that the expiration of the ban itself does not influence which municipalities remain in the sample. These estimates are shown in panel (c) of Table 6. A similar pattern emerges. There are consistently large positive estimates for the firearm homicide rate, and there remains no evidence of a secular increase in the general rate of crime.

25 President Calderon's administration begins in 2007 for the purposes of this paper, the exact inauguration date was 1 December 2006.

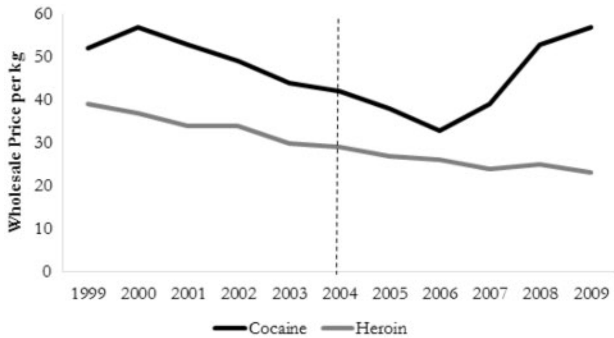

Figure 8. Wholesale price per kg (adjusted for purity and inflation, 2009 USD).

Table 7. Percent of self-reported drug use in the past year: United States (all individuals 12 and older)

	2002	2003	2004	2005	2006	2007	2008	2009
Cocaine	2.5	2.5	2.4	2.3	2.5	2.3	2.1	1.9
Heroin	0.2	0.1	0.2	0.2	0.2	0.1	0.2	0.2
Marijuana	11	10.6	10.6	10.4	10.3	10.1	10.3	11.3

Note: From the National Survey on Drug Use and Health, as reported in various rounds of the National Drug Intelligence Center's National Drug Threat Assessment.

After ruling out an underlying increase in the general rate of crime as a possible cause of the increase in the firearm homicide rate, an alternative explanation could be attributed to a change in the illicit narcotics market facing the drug trafficking organizations in Mexico that coincided with the expiration of the AWB. Two pieces of information are used to examine whether or not such a change occurred. First, price data from the United Nations Office on Drugs and Crime's World Drug Report are shown in Figure 8. The figure plots the retail price in the U.S., the central market for illicit narcotics trafficked from Mexico, for both heroin and cocaine. The third major drug trafficked into the United States is marijuana; however, it does not have a single market price because its production is extremely fragmented. Both prices steadily decline through the expiration of the AWB. It may be of interest to note that the increase in the cocaine price follows the Calderon offensive.[26] In addition to the stability in the price trend at the time of the AWB expiration, the National Survey on Drug Use and Health provides data on self-reported drug use which are displayed in Table 7. There is no evidence of a significant change in reported use for any of the three drugs that coincided with the expiration of the AWB. This information demonstrates that there was no change in either the price or use of narcotics at the time the AWB expired, and rules out changes in the illicit narcotics market as a possible cause for the increase in the firearm homicide rate.

26 Castillo et al. (2014) also find evidence that policy changes in Colombia may help explain the 2007 spike in cocaine prices, and the post-2007 increase in the homicide rate in Mexico.

In addition to ruling out alternative explanations of what caused the increase in the firearm homicide rate, it is also important to show that the estimated effect is unique to the timing of the AWB's expiration. This demonstrates the implicit assumption used in the identification strategy that the dissolution of the status quo should be consistent across periods where relative strength remains consistent. In practice, this means that comparisons of two RD estimates within a consistent state of the AWB should yield no difference in the status quo effect. The 3-year cycle of mayoral elections can be exploited to create a set of smaller samples and pseudo cutoffs.

The first comparison uses the true cutoff, the AWB expiration, to define pre- and post-treatment years. All municipalities included in these estimates had an incumbent margin victory (or defeat) of less than seven percentage points. The year immediately following the expiration of the AWB, 2005, has a large number of eligible municipalities with newly elected mayors. Municipalities within the same set of states that had newly elected officials in 2005, also had newly elected officials three years prior, in 2002.[27] The balance in the 2-year sample used here is essential to isolate the timing effect, and rule out changes in the sample composition as a cause of any difference between the discontinuities. Estimating the DiRD model with only these 2 years yields the point estimates seen in the column (3) of panel (b) in Table 8. The estimated increase in the firearm homicide rate is similar to the estimates seen using the full sample, and is again statistically significant. The sample can also be expanded, while preserving its balance, to include one additional year on either side of the AWB's expiration. Data from 2003 are added to the pre-expiration sample, and the set of observations 3 years later, from 2006, are added to the post-expiration sample. Although these years do not contain enough observations to be estimated on their own, they can complement the 2-year sample from column (3). Estimates from DiRD model after expanding the sample to include an additional year on either side of the cutoff are shown in column (4) of panel (b), and yield a similar estimate.

To construct samples around pseudo cutoffs, each election cycle can be moved 3 years in either direction. This again allows for the inclusion of a consistent set of municipal elections around two pseudo cutoffs, one in 2002, and the other in 2008. For the pre-expiration pseudo cutoff, data from the previous 3-year election cycle are used. The 'pre-treatment' sample is defined as data from 1999 (and 2000) municipalities with representatives elected in the previous year, and the 'post-treatment' sample includes data from 2002 (and 2003). The estimate for the 2-year sample is shown in column (1) of panel (a), and the estimate with 2 years on either side of the cutoff is shown in column (2) of panel (a). The estimates show no signs of the increase seen at the expiration of the AWB, both are smaller in magnitude and statistically insignificant.

A second pseudo cutoff can be constructed in the post-expiration period using the same technique. Moving the cutoff one election cycle after the expiration of the AWB yields a 'pre-treatment' sample of 2005 (and 2006), and a 'post-treatment' sample of 2008 (and 2009). As with earlier estimates using data from the Calderon period, the estimated effect is more difficult to interpret due to the importance of party affiliation. The post-expiration pseudo estimates for the full sample are shown in panel (c), and estimates for

27 The state of Veracruz, which had newly elected officials in 2001, held their following round of elections prior to 2005. Therefore, the 2001 Veracruz observations are combined with the 2002 observations when paired with the data from 2005 to balance the set of states in the sample.

Table 8. Pseudo estimates of effect away from expiration of AWB

	(a) Pseudo cutoff: pre-expiration		(b) AWB expiration	
Pre-cutoff years	1999	1999/2000	2002	2002/2003
Post-cutoff years	2002	2002/2003	2005	2005/2006
	(1)	(2)	(3)	(4)
ln(Firearm Homicide Rate)	−0.089	0.067	0.327*	0.317**
	(0.264)	(0.224)	(0.187)	(0.151)
	(c) Pseudo cutoff: post-expiration		(d) Pseudo cutoff: post-expiration (*Remove Top 10% of Pre-2005 Narcotics Crime Rates*)	
Pre-cutoff years	2005	2005/2006	2005	2005/2006
Post-cutoff years	2008	2008/2009	2008	2008/2009
ln(Firearm Homicide Rate)	−0.036	−0.145	−0.004	−0.096
	(0.155)	(0.156)	(0.161)	(0.166)
N	713	854	667	789

Notes: ***$p<0.01$, **$p<0.05$, *$p<0.1$. The dependent variable in all estimates is the natural log of the firearm homicide rate. The estimates shown are for the coefficient (β) on the interaction between the indicator variable for an incumbent defeat (D_{mst}) and the post-cutoff indicator (P_t), from Equation (2). The placement of the cutoff is defined by the pre- and post-years specified in each panel. Regressions include state-year fixed effects, a lagged value of the dependent variable, a cubic RD polynomial allowed to vary on either side of the election threshold and AWB expiration and the set of municipal controls described in footnote 11. The eligible sample includes all municipalities outside of Oaxaca. Each regression is weighted using an interaction between a triangular kernel calculated using the vote margin and the municipal population.

the restricted sample without municipalities in the top decile of pre-expiration narcotics crime are in panel (d). All four estimates are small and statistically insignificant.

Within both the pre- and post-expiration periods, comparisons of RD estimates yield no evidence of a statistically significant difference in the status quo effect. This supports the key assumption of a consistent status quo effect over time. The evidence from Table 8 demonstrates that only when the sample crosses the expiration of the AWB, when there is an increase in the availability of high-powered firearms from the USA, is there evidence of an increase in the firearm homicide rate.

5.4. Distance to U.S. border and the firearm homicide rate

In addition to the timing of the effect, it would be expected that the magnitude of the increase in the firearm homicide rate would be greater in municipalities closer to U.S.–Mexico border crossings. Although this is not a necessary condition for the effect of the expiration of the AWB to exist, the further a municipality is from the border, the greater the cost of acquiring an illegal firearm from the United States. To investigate whether this occurs, an additional distance term is added to the DiRD equation. The distance is again measured as the road distance from the municipality to the nearest border crossing into the U.S. For ease of interpretation, an inverse distance term is calculated and normalized to be between zero and one using the following equation:

$$\text{InverseDistance}(I_m) = 1 - \left(\frac{\text{Distance}}{2306}\right). \tag{3}$$

A municipality that shares a border crossing with the U.S. has a value equal to one, and the value of I_m for the furthest municipality is zero. To estimate whether the effect of the expiration of the AWB is different for municipalities closer to the border, I_m is added to the DiRD equation. The estimating equation with the inclusion of the inverse distance variable can be expressed in the following form:

$$\begin{aligned}\ln(y_{mst}) = \alpha &+ I_m P_t D_{mst}\beta + P_t D_{mst}\theta_1 + P_t I_m \theta_2 + I_m D_{mst}\theta_3 + I_m\theta_4 + D_{mst}\theta_5 \\ &+ \sum_{p=1}^{P} v_{mst}^{p} D_{mst}\gamma_p + \sum_{p=1}^{P} v_{mst}^{p}\delta_p + P_t\left[\sum_{p=1}^{P} v_{mst}^{p} D_{mst}\mu_p + \sum_{p=1}^{P} v_{mst}^{p}\phi_p\right] \\ &+ X_{mst}\pi + \ln(y_{ms(t-1)})\rho + \tau_t + \varepsilon_{mst}.\end{aligned} \tag{4}$$

The outcome of interest is again the logged firearm homicide rate in municipality m, in state s and year t. The coefficient of interest is the coefficient on the triple interaction, β. This coefficient measures the change in the DiRD effect when moving from the furthest municipality ($I_m = 0$) to the border ($I_m = 1$), if the increase in the firearm homicide rate is greater closer to the border, the coefficient will be positive ($\beta > 0$). The inverse distance is also interacted with the post-expiration indicator variable (P_t) to capture any nationwide changes in the distance effect, and the RD indicator (D_{mst}) to capture whether the distance matters in a different way if the incumbent is defeated. I_m is also included alone to capture any time and election invariant effect of the distance to the U.S.–Mexico border. The same set of trend and control variables are used, except for the previously included distance variables, and the model is estimated using year-specific fixed effects (τ_t). With the large number of items estimated in this model, the key focus of the output is not the exact point estimate of β, but whether the estimated value is positive, as expected.

Estimates from Equation (4) can be seen in Table 9. Coefficient estimates using data from the 2000 to 2006 period are shown in the first two columns, and for the expanded 1999 to 2010 sample in the last four columns, all using a 7% bandwidth. The sample used in the first two columns only includes 2 years of post-AWB elections, and therefore, do not include municipalities from all parts of the country. In this context, the extended sample has the benefit of balanced geographic distribution. Additionally, the relevant distance for illicit trafficking from the Yucatan Peninsula may not be road distance, for this part of the country movement over the Gulf of Mexico could be preferred. To take this into account, the states that make up the peninsula are removed from columns (1), (3) and (5).[28] The sample used to estimate the results shown in panel (a) of Table 9 include municipalities on the U.S.–Mexico border; all six estimates yield evidence that the effect of the AWB is larger the closer a municipality is to the border.[29] Although estimates in the first two columns, for the pre-Calderon period, are smaller and statistically insignificant, they remain positive, yielding suggestive evidence of a

28 These states are Campeche, Chiapas, Quintana Roo, Tabasco and Yucatan.

29 All two dozen estimates for alternative bandwidths also yield positive point estimates, evidence of a larger impact closer to the border. Estimates are shown in Online Appendix Table A.6.

Table 9. Inverse distance to USA–Mexico border and the effect of AWB expiration on ln(Firearm Homicide Rate)

(a) Full sample

	2000–2006		1999–2010			
					(Remove Top 10% of Pre-2005 Narcotics Crime Rates)	
	Remove Yucatan Peninsula	Full sample	Remove Yucatan Peninsula	Full sample	Remove Yucatan Peninsula	Full sample
	(1)	(2)	(3)	(4)	(5)	(6)
ln(Firearm Homicide Rate)$_{mst}$	0.226	0.093	0.429**	0.389***	0.610**	0.464***
	(0.263)	(0.155)	(0.189)	(0.124)	(0.239)	(0.137)
N	1200	1398	2053	2392	1826	2156

(b) Distance from U.S. border greater than 100 km

	2000–2006		1999–2010			
					(Remove Top 10% of Pre-2005 Narcotics Crime Rates)	
	Remove Yucatan Peninsula	Full sample	Remove Yucatan Peninsula	Full sample	Remove Yucatan Peninsula	Full sample
	(1)	(2)	(3)	(4)	(5)	(6)
ln(Firearm Homicide Rate)$_{mst}$	0.129	0.160	0.219	0.437***	0.616**	0.481***
	(0.340)	(0.178)	(0.219)	(0.135)	(0.246)	(0.139)
N	1164	1362	1989	2328	1802	2132

Notes: $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$. The dependent variable in all estimates is the natural log of the firearm homicide rate. The estimates shown are for the coefficient (β) on the triple interaction between the indicator variable for an incumbent defeat (D_{mst}), the post-AWB indicator (P_t) and the inverse distance measure (I_m) from Equation (4). All regressions include year fixed effects, the lagged value of the dependent variable, a cubic RD polynomial allowed to vary on either side of the election threshold and AWB expiration and the set of municipal controls described in footnote 11, except for the previously described distance control. The eligible sample includes all municipalities outside of Oaxaca. Each regression is weighted using an interaction between a triangular kernel calculated using the vote margin and the municipal population.

stronger effect closer to the border. This is reinforced by the four large and statistically significant estimates using the extended sample.

Removing municipalities on the Yucatan Peninsula from the sample also removes Mexico's southern border. For balance, estimates in panel (b) only include municipalities that are at least 100 km from the U.S. border. This is done to ensure that the distance effect is not solely driven by a large effect directly at the border. The estimates in panel (b) remain consistently positive, and almost identical when municipalities in the top decile of pre-expiration narcotics crime are removed. The effect of the AWB expiration is not limited to the U.S.–Mexico border region, the area analyzed in Dube et al. (2013), but impacted a much larger part of the country. In fact, estimating the basic DiRD estimate without municipalities within 100 km of a U.S.–Mexico border crossing yields a point-estimate of 0.363 that is statistically significant at the 99% confidence level, similar to the main estimate from Table 5. These estimates reinforce that the expiration of the AWB led to a large increase in the firearm homicide rate, even in areas away from the U.S.–Mexico border.

6. Conclusion

In municipalities with close incumbent defeats the expiration of the AWB coincided with a 35% increase in the firearm homicide rate in Mexico, and this increase was isolated to the timing of the AWB's expiration. There was no concurrent increase in either non-firearm homicides or the overall rate of crime, and the illicit narcotics market was stable through the expiration of the AWB. Furthermore, the magnitude of the increase in firearm homicides is positively associated with proximity to the U.S.–Mexico border. The expiration of the AWB is the lone explanation for the increase in the firearm homicide rate that fits each criteria, and possesses the magnitude to have such a widespread effect. Finally, there is no evidence of a substitution away from non-firearm homicides, thus the expiration of the AWB also led to an increase in the overall homicide rate.

This paper provides a number of important contributions to the literature. It introduces a dataset that can be used to identify any number of municipal level outcomes and evaluate policy in the world's 11th most populous country. The estimates from this paper show that the effect of the AWB expiration spread far beyond the border region, yielding evidence of an impact that is more widespread than previously documented. The paper also addresses the widely debated relationship between firearms and homicide prevalence, finding that access to high powered firearms, which were banned for a decade in the United States and are not legally available in Mexico, does lead to higher levels of homicides. Finally, much like DellaVigna and La Ferrara (2010), it highlights the fact that these type of rule of law issues are no longer domestic ones, but regulations made in one nation can significantly impact individuals who have no direct say in the decision making process.

Supplementary material

Supplementary data for this paper are available at *Journal of Economic Geography* online.

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