Cross-border Spillover: U.S. Gun Laws and Violence in Mexico*

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Abstract

Do more guns cause more violence? We exploit a natural experiment induced by the 2004 expiration of the U.S. federal assault weapons ban to examine how the subsequent exogenous increase in gun supply affected violence in Mexico. The expiration relaxed the permissiveness of gun sales in border states such as Texas and Arizona, but not California, which retained a pre-existing state-level ban. Using data from mortality statistics and criminal prosecutions over 2002-2006, we show that homicides, gun-related homicides and gun crimes increased differentially in Mexican municipios located closer to Texas and Arizona ports of entry, versus California ports of entry. Our estimates suggest that the U.S. policy change caused at least 158 additional deaths each year in municipios near the border during the post-2004 period. Notably, gun seizures also increased differentially, and solely for the gun category that includes assault weapons. The results are robust to controls for drug trafficking, policing, unauthorized immigration, and economic conditions in U.S. border ports, as well as drug eradication, trends by income and education, and military and legal enforcement efforts in Mexican municipios. Our findings suggest that U.S. gun laws have exerted an unanticipated spillover on gun supply in Mexico, and this increase in gun supply has contributed to rising violence south of the border.

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1 Introduction

Do more guns cause more violence? This question remains the subject of contentious debate among policymakers and academics. On the one hand, more guns may enable criminals to commit more violent acts. On the other hand, guns may deter violence if criminals fear repercussion or self-defense from victims possessing these weapons. Previous analyses have sought to answer this question by assessing whether the permissiveness of gun laws in the U.S. affect criminality in the U.S. However, gun laws may arise endogenously in response to local crime rates, which poses a challenge to identifying the impact of gun supply stemming from legal changes within national boundaries.

This paper seeks to identify the effect of gun supply on violence in Mexico, by exploiting a major policy change governing the sale of assault weapons in a neighboring nation – the United States. Specifically, we focus on the 2004 expiration of the national Federal Assault Weapons Ban (FAWB) which lifted the prohibition on sales of military-style firearms in the U.S. We argue that this expiration also served as an exogenous shock to assault weapons supply in Mexico, as these guns are trafficked south of the border by Mexican drug cartels and crime syndicates. In fact, since 2004, the vast majority of guns seized in Mexico and submitted to the U.S. Bureau of Alcohol Tobacco Firearms and Explosives (BATF) were traced back to the United States (GAO Report, 2009).

The original 1994 FAWB included a 10-year sunset provision, mandating it would expire in September 2004 unless renewed by Congress. The ban did expire on this pre-determined date, which helps ensure that the timing of the policy change did not respond to overall violence levels in Mexico. In addition, when the law expired, several U.S. states retained their own bans on the sale of assault weapons. This group included California, which had (and continues to have) one of the most stringent gun control regimes in the country. Since other states with major ports of entry into Mexico did not have equivalent state-level laws, the FAWB expiration made it plausibly easier to obtain assault weapons in Mexican municipios bordering Arizona (AZ) and Texas (TX), as compared to municipios bordering California (CA). We use variation across Mexican municipios in distance to ports of entry bordering U.S. states selling assault weapons, as well as the timing of the 2004 policy change, to estimate the effect of the FAWB expiration on homicides and gun crimes in Mexico over 2002-2006.

We find that after 2004, there was a significant increase in homicides, and homicides tied specifically to guns, along with criminal charges for murder and gun crimes, in municipios closer to Arizona and Texas ports versus California ports. These increases occurred almost immediately after the ban's expiration, and appear to have persisted. Our estimates suggest that municipios at the AZ and TX ports experienced at least a 40% increase in homicides as compared to municipios 100 miles away following the change in law. The findings imply that

the FAWB expiration led to at least 158 additional annual deaths in the Mexican region within 100 miles of the border ports. Importantly, we also show that the policy change led to increased gun seizures by the Mexican military. Specifically, there was an increase in seizures within the gun category that includes assault weapons, but no corresponding increase for handguns. This is consistent with the account that the FAWB expiration led to increased violence through its effect on the supply of assault weapons.

The results are robust to controlling for a number of characteristics related to the drug trade, economic conditions, and enforcement on both sides of the border. This includes the value of drugs seized in the nearest U.S. port and in Mexican municipios, as well as drug crop eradication and drug-war related military detentions in Mexico. We also control for employment and average earnings in the nearest U.S. port, for trends by income and schooling levels across Mexican municipios, and for municipio-specific homicide trends. Additionally, we account for undocumented immigration and policing patterns in the nearest U.S. ports. The analysis specifically excludes the time period after 2006, when the government of Felipe Calderon sent the Mexican army into numerous states initiating a major escalation in the drug war. This allows us to more convincingly isolate the effect of gun law change relative to enforcement-related factors. We also show that the effects were larger in municipios with historically high drug trafficker density, and for demographic groups more likely be involved with cartels. This heterogeneity in the treatment effect suggests that greater gun use by the cartels played an important role behind the increase in homicides.

While gun trace statistics and anecdotal evidence have documented the current pattern of gun exports from the U.S. to Mexico, it is a separate question whether American gun laws contribute to an overall increase in firearms in Mexico, and whether this, in turn, leads to more violence on the Mexican side of the border. Our paper is the first to present evidence along these lines. We identify a possible cross-border spillover of U.S. gun laws on weapons supply in neighboring nations, which is an important, unintended consequence of U.S. legislation. Exploiting this cross-border externality to obtain exogenous variation represents a novel approach to identifying the effect of the gun supply on violence, and has broader applicability in other areas such as environmental and land use policies.

Our paper relates directly to the literature examining the effect of gun laws cause on crime in the United States, which has found mixed evidence or small effects depending on the type of law. Koper and Roth (2001) use variation in pre-existing state-level assault weapons bans to estimate the effect of the original FAWB enactment on homicide rates in U.S. states.[1] Their baseline difference-in-difference estimate suggests a 6.7% reduction in homicides, but the estimate is imprecise. However, comparing crime across states that already held a ban with those that

[1] The four states that had an assault weapons ban prior to 1994 included California, Connecticutt, Hawaii and New Jersey.

did not may either under or overestimate the true effect of FAWB. For example, if the control states (i.e., those that adopted the ban earlier) experienced decreasing violence trends or a fall in levels with a lag due to the policy adoption, then the measured effect will underestimate the true violence reduction stemming from the law. In contrast, if the state-level policies were adopted in places experiencing a differential growth in violence, then the measured effect will overestimate the true impact. In the third edition of his book, *More Guns, Less Crime,* John Lott argues that the while the introduction of the federal assault weapons ban was followed by some reduction in crime rates, accounting for trends in crime rates prior to the ban suggests the opposite account. A safe conclusion from the extant research is that the effects of FAWB expiration on U.S. crime rates are difficult to discern, and this may be related to endogeneity in policy adoption. This makes it especially important to assess the effect of the FAWB expiration on violence in Mexico, where it represents a plausibly exogenous policy shock.

Turning to federal laws that control the distribution or ownership of firearms, to date, the evidence has not indicated sizeable effects. Ludwig and Cook (2000) find no significant effects of the 1993 Brady Bill requirements concerning background checks for gun sales, on overall homicide or suicide rates. They posit that the null effect could arise from a spillover, if the Brady Bill reduced gun flows from treatment states into the secondary gun market, which in turn had homicide reducing effects in non-adopting control states. In our analysis, the first-order spillover of the U.S. policy shock on guns in Mexico is a virtue for the research design since it provides identifying variation in gun supply across Mexican municipios rather than contaminating our controls. Of course, there may be second-order spillovers within Mexico from an initially geographically specific supply shock in guns which suggests our estimates may also underestimate the full magnitude of the effect.

Crime-reducing effects of legislation allowing individuals to carry concealed weapons (CCW) were first put forward by Lott and Mustard (1997). While these results have been supported by Moody (2001), they have been challenged by Ayres and Donohue (1999, 2003), as well as Black and Nagin (1998), who found that the results were not robust to various assumptions and modeling choices. Two additional studies have cast empirical doubt on the deterrence account: Ludwig (1998) shows that there were no differential reductions in adult vs. juvenile victimization rates among CCW adopting states, although the laws require gun permit holders to be older than 18 or 21. Duggan (2003) shows that CCW laws did not increase gun ownership, and that counties with higher levels of gun ownership prior to the policy change did not experience larger decreases in crime. In addition, Donohue and Levitt (1998) provide a theoretical model countering the deterrence idea, in which more firearms lead to more violence by reducing the predictability of fights and increasing the number of violent confrontations.

Duggan et. al (forthcoming) examines the effect of gun shows, which allows vendors in some states to sell firearms without conducting background checks. These events do not appear

to increase homicides or suicides within three weeks, in or near the zipcode where shows take place. While their analysis focuses on localized effects, our paper presents a complementary approach by examining the effect of weapons when they are transported away from the location in which they are sold. We also analyze effects at the quarterly and annual level, since guns are durable goods and may promote mortality over a longer window. Knight (2011) also presents evidence of gun transportation, using a gravity model to show that crime guns are exported from states with weaker gun laws to states with stronger gun laws, and to a larger degree when states are geographically closer to each other. This evidence is consistent with our findings, as it highlights that distance matters for transporting guns, and that there is likely imperfect arbitrage across states. This type of within U.S. spillover also suggests that our results may be underestimated, to the extent that some assault weapons may flow to California in the wake of the FAWB expiration.

More generally, our analysis is related to the issue of arms trafficking from developed to developing countries, and its consequences for conflict. Evidence of such trafficking and its profitability is shown in DellaVigna and La Ferrara (2010), which finds that stock prices of weapons-making companies headquartered in countries with less transparency respond strongly to sudden changes in conflict in arms-embargoed countries. It is important to estimate the extent of trafficking-related cross-border spillovers, given the social and economic costs associated with resultant violence. In a recent analysis, Dell (2011) shows that the presence of drug trafficking routes in Mexico lowers female labor force participation rates and the wages of informal sector producers.

The remainder of the paper is structured as follows. In section 2, we provide background information on both U.S. gun laws and weapons trafficking to Mexico, as well as the escalation in the drug war. In Section 3, we describe our data and empirical strategy. Section 4 shows the main results as well as results from a number of additional tests and robustness checks. Section 5 concludes.

2 Background on Gun Laws and Weapons Trafficking

2.1 Assault Weapons Ban

On September 13, 1994, the United States Congress passed the "Violent Crime Control and Law Enforcement Act." Title XI, subtitle A of the act included a first time restriction on the manufacture, transfer and possession of semi-automatic weapons. The law focused on a group of firearms considered by ban advocates to be particularly dangerous, because they have features that facilitate the rapid firing of multiple shots, or make them useful in military and criminal

applications. In particular, it barred the manufacture of 19 specific semi-automatic firearms deemed to be "assault weapons" as well as any semi-automatic rifle, pistol, or shotgun that is capable of accepting a detachable magazine, and which has two or more of the following features: a telescoping or folding stock, a pistol grip, a flash suppressor, a grenade launcher, and a bayonet lug. Banned guns include the Avtomat Kalashnikov (AK) series, as well as the Colt AR-15 weapons series. The act was signed into law by then President Clinton for 10 years. However, as a consequence of a sunset provision, it was set to and did expire in September of 2004.

During this period, a handful of U.S. states had their own restrictions on assault weapons. This included California, which had an assault weapons ban in place prior to 1994. When the federal law sunsetted, California's ban remained in place. Therefore, while other states bordering Mexico experienced a change in the assault weapons control regime, the same was not true for California. This provides the basis for our research design comparing changes in crime in Mexican municipios bordering California versus other U.S. states.

In California, the control of assault weapons began with the passage of the Roberti-Roos Assault Weapons Control Act of 1989. The Act defined assault weapons in a manner similar to the federal ban. In particular, all weapons listed in section 12276 of California's Penal Code were (and continue to be) designated an assault weapon. Such firearms were designated controlled and as such could not be legally purchased, kept for sale, offered for sale, exposed for sale, given, lent, manufactured, distributed or imported as of 1991. Moreover, all pre-existing weapons were required to be registered as assault weapons with the Department of Justice. Banned weapons in California also include the AK and AR-15 weapons series.

California's weapons ban was subsequently strengthened between 1989 and 2002. The Roberti-Roos Act was challenged on constitutional grounds, but upheld by the State Supreme Court. The ruling found that effective August 16, 2000, firearm models that are variations of the AK or AR-15 with only minor differences from those two models are also considered assault weapons and are controlled. Weapons that were not registered before January 23, 2001 also had to be surrendered to law enforcement. In addition, CA Senate Bill 23, passed in 1999, and implemented in 2000 and 2002, broadened the reach of the ban. This bill introduced specific characteristics (such as flash suppressors, forward pistol grip, and the capacity to accept more than 10 rounds) that designate a gun an assault weapon.[2] Since 2002, CA's gun law regime has remained relatively uniform.

Our empirical strategy posits that the lifting of the FAWB made gun laws more permissive in TX and AZ. However, the ban would only represent a differential change in stringency compared to CA if CA's legislation was sufficiently strong to control assault weapons sales,

[2] Details about the California assault weapons ban can be found at: http://ag.ca.gov/firearms/forms/pdf/awguide.pdf

and this control was retained in the post-2004 period. One piece of evidence indicating the relative ease of obtaining assault weapons in Texas and Arizona versus California comes from the advocacy group The Brady Center to Prevent Gun Violence, which ranks states on the restrictiveness of their gun control laws on a 100 point scale. California has consistently ranked number 1 on this list, most recently with 79 points. Specifically with reference to assault weapons, California gets a 10 out of 10 in this category. In contrast, Texas and Arizona score less than 10 points in total, earning zero each in the assault weapons category.[3] Another piece of suggestive evidence comes from BATF Firearms Trace data from 2006, the earliest year available, which indicates that the flow of seized guns from California to Texas and Arizona (427) was less than half of the reverse flow (1055).[4]

A final indication that CA gun control laws were binding, and that the FAWB affected the gun control regime in TX and AZ comes from gun sales and production data. The primary data available on gun sales come from the National Instant Criminal Background Check System (NICS). As mandated by the 1993 Brady Act, sales of weapons by federally licensed firearm dealers are required to use the NICS for determining the legality of selling the weapon to a purchaser. A limitation of the data is that private sellers, including gun shows, are not reflected in these figures. With this caveat, Panel A of Figure I shows that there was approximately a 25% increase in combined gun sales in AZ and TX, as compared to California after 2004. While we cannot necessarily attribute all of this increase to the expiration of the ban, the figure shows that gun sales did rise differentially in states affected directly by the FAWB expiration, while gun sales remained essentially flat in CA. We also compare firearm productions in these states using data from the Annual Firearms Manufacturing and Exportation Reports (AFMER), which is prepared by BATF. Panel B of Figure I shows that the production of rifles in particular more than doubled after 2004 in AZ and TX, while remaining unchanged in CA.

2.2 Gun Flows to Mexico

The combination of tough gun laws in Mexico, weak gun laws in the United States, and proximity makes it optimal for Mexican drug cartels and crime syndicates to source their firearms purchasing to the U.S.[5] The Mexican government has repeatedly asked for assistance from the United States in reducing assault weapons flows. In May 2010, Mexican President Felipe

[3] http://www.bradycampaign.org/stategunlaws/

[4] http://www.atf.gov/statistics/trace-data/

[5] Articles 9 and 10 from the Mexican Federal Law of Firearms allow possession and carrying of pistols of only calibers .380 (9mm) or less, and revolvers of calibers .38 special or less. High caliber guns, and those reserved for the exclusive use of the Army are forbidden from private ownership by Article 10. Permits for the transportation and use of firearms are issued for one year terms by the Ministry of National Defense, which operates the only legally authorized retail outlet for firearms in Mexico.

Calderon urged the U.S. Congress to reinstate a ban on assault weapons. He said, "I [...] fully understand the political sensitivity of this issue. But I will ask Congress to help us... and to understand how important it is for us that you enforce current laws to stem the supply of these weapons to criminals and consider reinstating the assault weapons ban (Los Angeles Times, May 20, 2010)." In February 2011, when the U.S. House of Representatives voted against proposed legislation requiring sellers in border states to report multiple purchases of military-style weapons, the Mexican ambassador tweeted "Unfortunate" (Washington Post, February 20, 2011). Frustration over the U.S. response has most recently led the Mexican government to explore suing American manufacturers and distributors of these weapons flowing into Mexico (CBS News, April 21, 2011).

The ideal data to track the pattern of assault weapons trafficking from various U.S. states to Mexico would come from the U.S. BATF's eTrace program. Since 2004, the Mexican government has sent information on about a quarter of its seized guns to eTrace. Unfortunately, in spite of a Freedom of Information Act request and subsequent appeals, BATF did not share this micro-data or any other information on assault weapons with us. The Mexican government also did not release its gun trace data. Below, we piece together publicly available data from the eTrace program to understand the spatial patterns and likely changes in gun trafficking over this period.

As of 2006, around 90% of the weapons confiscated in Mexico and submitted to BATF's eTrace program could be linked back to the United States (Figure II). The fraction traced to the U.S. rose between 2004 and 2006, suggesting increased sourcing from the U.S. While only a quarter of the weapons confiscated in Mexico are submitted for tracing, the evidence suggests that the vast majority of guns originate from the United States, and this likely grew since 2004.

Most of the guns from U.S. come from the border states. Between 2004 and 2008, 49% of guns traced to the U.S. originated from either Texas or Arizona. In contrast, 20% of guns were traced to California. If we normalize these flows by the states' population (31.5 million in Texas plus Arizona and 37.2 million in California in 2010), this suggests that the "export rate" of the other two states are nearly three times as large as that of California. While we do not have information about exports by state going back to the pre-2004 years, the combination of increased sales in Texas and Arizona after 2004, along with the post-2004 pattern of flows to Mexico suggests that overall, there was a sizeable increase in gun flows from Arizona and Texas to Mexico following 2004. This is consistent with the expiration of the FAWB increasing gun flows to Mexico.

2.3 Cartels and the Mexican Drug War

As the conflict in Mexico among various drug cartels, and between the cartels and the Mexican government has intensified, trafficking of automatic rifles, rocket propelled grenade launchers and high caliber machine guns have increased dramatically. Guns are typically not sent via tunnels in the desert or across the Rio Grande by boats, but through commercial ports of entry. In particular, "[f]firearms are generally trafficked along major U.S. highways and interstates and through border crossings into Mexico. The firearms are normally transported across the border by personal or commercial vehicle because, according to U.S. and Mexican government officials, the drug cartels have found these methods to have a high likelihood of success (p.22, GAO Report, 2009)." Richard Cortez, the mayor of the border city McAllen, Texas, commented on the lack of resources for enforcement within ports. He stated, "We have let the land ports of entry fall into disrepair . . . The criminal cartels are exploiting our weakness. According to the Department of Justice, 90 percent of the drugs smuggled into the U.S. enter through the land ports. The physical bulk cash that exits goes exclusively through the ports. There is no data on firearms, but anecdotally, the ports are where they too traverse the border."[6] This pattern of gun smuggling underscores our strategy of focusing on outcomes near major ports of entry, which we discuss in greater detail in the data section.

Within Mexico, there is some geographic specificity to where each of the cartels operate, and particular cartels are dominant in particular ports of entry into the U.S. Figure III shows the approximate areas of influence and headquarter locations of the Tijuana, Sinaloa, Juárez, and Gulf cartels over the 2002-2006 period. The Tijuana cartel is headquartered in Tijuana, directly south of San Diego, and operates only in the part of Mexico adjacent to California. Similarly, the Juárez cartel is headquartered in and dominates the port of Ciudad Juárez, which is directly South of El Paso, though its activities extend into parts of Mexico bordering both Arizona and Texas. The headquarter of the Gulf Cartel does not lie at the border, but it is dominant in the eastern part of Mexico, in the region bordering Texas. The Sinaloa Cartel held the broadest reach, since it was pursuing an aggressive campaign to contest territory over this period. However, it is dominant in the part of Mexico adjacent to Arizona. These patterns of control indicate that there would be high turf-based costs for smugglers from the Tijuana cartel, to operate along routes controlled by rival cartels near Texas and Arizona, and analogously, it would be costly for the Juárez and Gulf cartels to operate in the border region of California.

Recent evidence also suggests that Mexican drug trafficking organizations have increased their criminal operations into U.S. territory, and established associations with particular American street gangs. For instance, the National Drug Threat Assessment (2010) reports that Barrio Azteca, a major gang operating in Texas, is closely aligned with the Juárez cartel. Sim-

[6] Testimony of Richard Cortez, Mayor of McAllen, TX, before the House Committee on Homeland Security Subcommittee on Border and Maritime Security "Using Resources Effectively to Secure Our Border at Ports of Entry Stopping the Illicit Flow of Money, Guns and Drugs", April 5, 2011.

ilarly, the 18th street gang from California appears to be linked to the Tijuana cartel. Links between Mexican cartels and U.S. gangs reinforce cartel costs of operating outside of their dominant regions, by increasing smuggling costs on U.S. side of the border. This represents one type of turf-based cost to smuggling weapons and underscores why we exploit proximity to non-California ports, in particular, in assessing the impact of the FAWB expiration in the analysis.

Figure III also shows that the extent to which Mexican states are contested by cartels over this period is approximately the same across different parts of the border. For example, there are at least two large cartels present in each of the Mexican states, including those adjacent to California as well as Texas and Arizona. This uniformity of cartel structure is important in comparing violence based on proximity to non-California ports, since the presence of multiple cartels within a given area may be associated with higher levels of violence.

Finally, the relationship between drug cartels and gun trafficking also raises a potential challenge to attributing rising violence to changes in gun supply – violence may instead be related to intensification in drug trafficking patterns along different parts of the border. Although drug trafficking in Mexico has been a concern for many years, the fighting between cartels has risen dramatically over the past decade. There have been two major turning points in the Mexican drug war. First, in 2001, Joaquin "El Chapo" Guzman, the leader of the Sinaloa cartel, escaped from prison and attempted to take control of Mexico's drug trade. In particular, he attempted to take over important drug routes along the Texas and California borders. Violence increased throughout the country, but particularly in drug production areas, and in crossing points along the U.S.-Mexico border (Wall Street Journal, June 13, 2009). Second, in December of 2006, the president of Mexico, Felipe Calderon, initiated a new aggressive war on the cartels by using the military to fight organized crime. The military campaign was phased in differentially in various regions: it began with Michoacan and Baja California in December of 2006, and was extended to Chihuaha, Durango, Sinaloa, Nuevo Leon and Tamaulipas in 2007. In 2008, operations began in Jalisco and Guerrero. In 2009, there was a dramatic escalation in the drug war, and spiking violence in Juárez (bordering Texas) led President Calderon to send 10,000 additional soldiers to that city (Nexos, January 03, 2011). The year 2010 saw numerous arrests and deaths of cartel leaders, assassinations of politicians, and a reinforcement of the Mexican government's military operations. Given the importance and the staggered nature of the military operations, we exclude all years after 2006 from our primary sample. We also avoid comparing across the pre and post 2001 period by limiting attention to 2002 and 2006. This constitutes a relatively homogenous period in the context of the Mexican drug war.

3 Data

3.1 Data Sources and Construction of Key Variables

To examine how the 2004 expiration of the FAWB affected violence in Mexico, we analyze the effects on a number of different crime and violence-related dependent variables. We utilize mortality statistics data from the Mexican statistical agency, the Instituto Nacional de Estadística y Geografía (INEGI), which list the location and causes of death for the universe of deaths registered officially in Mexico. These individual level records are aggregated at the municipio level to generate annual and quarterly counts of killings in the 2000-2008 period. The cause of homicide is disaggregated further, allowing us to generate counts of homicides tied specifically to guns. While the gun-related homicides variable is a more direct measure of violence arising from gun law changes, it is also more likely to be mis-measured relative to total homicides and under-estimate actual gun killings, since the specific cause is unknown for 15% of murders in the sample. As such, we view total homicides as the preferred measure of violence and the key dependent variable in the analysis. We are also able to generate counts of non-gun homicides as well as non-homicide deaths, which we employ as control variables in some specifications. Finally, the mortality statistics record some basic demographic information about the deceased, including age, education, and gender. For the 88% of observations that are not missing these data, we generate homicide and gun-related homicide counts of sub-groups, such as individuals 18 and older without a high school degree, and young males between 18 and 30 without a high school degree.

To assess whether U.S. gun laws affect violence in Mexico through their effect on weapons supply, we employ data on crime gun seizures from the Mexican military, specifically the Secretariat of National Defense (SEDENA). Seizures are defined as the number of guns seized in the campaign against drug-traffickers and in violation of Mexico's gun laws. These data present only a partial picture of gun seizures, since the Office of the Mexican Attorney General (PGR, by its Spanish acronym), also seizes crime guns, but has not made this municipal level data available. Aggregate numbers indicate that over the 2002-2006 period, SEDENA accounted for approximately 30% of total seizures. The measures are disaggregated by weapons type, allowing us to analyze handguns separately from rifles, the category of guns that includes assault weapons. We generate annual level counts of handguns and rifles seized. Because this data is available at the daily level, we also create annual counts of guns seized in events where more than one gun was seized in a given municipio in a given day. We analyze multiple gun seizures separately since these are more likely to indicate incidents associated with organized crime.

We also analyze data on criminal convictions from INEGI, which records information on individuals prosecuted for particular crime categories, for the universe of criminal prosecutions

in Mexico. This data is also disaggregated by municipio and type of charge, allowing us to construct municipio-level annual counts of homicide charges and gun-related charges over the 2000-2006 period. We are not able to isolate the number of prosecutions for gun-murders in particular. However, the gun-related prosecutions include non-murder charges such as possession, illegal transport and trafficking of certain firearms. We view this as an advantage since it is possible that the expiration of the FAWB and subsequent increases in weapons in Mexico may affect various types of gun-related crimes, not just murder. We also create counts of other criminal charges, which are employed as controls in the analysis.

Since we aim to assess if the 2004 policy change has had larger effects in Mexican areas located closer to Texas and Arizona, relative to those closer to California, we construct several different measures of a municipio's proximity to various parts of the border. We create an indicator variable of whether the municipio lies along the U.S. Mexico border, which is further disaggregated into whether a municipio is spatially contiguous with the California segment of the border (which we call the CA segment), or the Texas and Arizona segments (which we call the non-CA segment). A second variable measures a municipio's proximity to major port cities that straddle the border, which are hubs through which goods - legal and illegal - are transported. As discussed in the previous section, most guns transported from the U.S. into Mexico arrive via across major highways that run through these port cities.

Table I shows how we classify border crossings into 13 major ports based on truck traffic and distance between crossings. In particular, a border crossing is considered a separate major port if it has an annual truck flow of at least 5000 per year during 2002-2006 (our sample period), and if it is at least 30 miles away from another major border crossing. If two border crossings are fewer than 30 miles apart, we consider them to be part of the same port, named after the border crossing with higher truck traffic. Data on truck flows come from the Bureau of Transportation Statistics (BTS) which reports the average number of trucks that go through U.S.-Mexico border crossings annually. Straightline distances were calculated based on distance from the actual border crossing, rather than the center of the port city, though the classification remains the same if we use driving distances. The criteria yield 13 ports: two in CA (El Centro and San Diego); three in AZ (Yuma, Nogales and Douglas); and eight in TX (El Paso, Presidio, Eagle Pass, Del Rio, Laredo, Rio Grande, McAllen and Brownsville). Table I also shows that there are two border crossings in the state of New Mexico but neither qualify as a major port: Columbus doesn't meet the 5,000 truck flow criteria; and Santa Teresa is considered to be part of the El Paso port given they are only 11 miles apart.

Figure IV shows the location of these ports, along with the highways on the Mexican side of the border. The green-shaded areas demarcate the set of municipios at the U.S. border which also have a highway going through them.[7] Locations which appear to have a highway

[7]The GIS shapefile for Mexican highways in 2009 comes from http://www.mapcruzin.com/download-mexico-

crossing but are not listed as major ports fail to meet either the 5,000 truck flow or 30-mile criteria. For example, the crossing between Yuma and Nogales is Sasabe, which has a truck flow less than 1,000 per year and is actually in a national park. The crossing between Nogales and Douglas is Naco, which lies within 24 miles from Douglas, and is thus considered to be a part of the Douglas port. To create a measure of a municipio's proximity to non-CA ports, we take the centroid-to-centroid distance between a given municipio and the nearest port in either Texas or Arizona, and subtract this distance (in thousands of miles) from 1. We also generate an equivalent proximity to border variable, based on distance to the nearest of any of the 13 ports.

Since the drug trade and associated crime levels may also be correlated with proximity to non-CA ports, we obtain data from SEDENA on drugs seized by the military during drug-war operations in each municipio. On the U.S. side, county-level measures of drugs seized are based on data from the U.S. Drug Enforcement Agency, the U.S. Customs and Border Protection and the Department of Homeland Security (DHS). For both types of seizures, we use international prices from the United Nations Office of Drug Control to aggregate the value of the four major drugs traded across the two countries – marijuana, heroin, cocaine and methamphetamines. Since each U.S. port is situated in a different U.S. county, a unique county-level drug value of seizure is assigned to each port, yielding a municipio-level variable representing the value of drugs seized in the nearest port. In addition, data from the Mexican authorities tells us the hectares of marijuana and heroin poppies eradicated within each Mexican municipio. Given the fraction of municipios with zero eradication or drug seizure, we take the log of one plus the hectares of drug crops eradicated, and the log of one plus the values of drugs seized in Mexican municipios, and use these log transformations as controls in the analysis.

Data on the number of drug traffickers residing in a municipio between 1998-2001 from Resa Nestares (2004) gives us a measure of historical drug cartel presence. This data was generated on the basis of official PGR reports that disclose the usual place of residence of people convicted of drug related offenses, including possession, sale and drug trafficking. However, it was only collected for the top 100 municipios with the largest amounts drug trafficking. The number of traffickers is scaled by population 100,000, which yields a measure of drug trafficker density. We define municipios where this figure exceeds 200 as the high drug trafficker density sample, and assess whether our treatment effect is larger in these municipios with greater cartel presence.

SEDENA data on individuals detained by the Mexican military in the course of drug war operations provide us with an important indication of enforcement at the municipal level. These detentions (per capita) represent how actively authorities apprehend drug war criminals. INEGI data on the number of public attorneys stationed in each municipio give us another measure of enforcement. Attorneys are employed by branches of the PGR at the federal level,

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and branches of equivalent public agencies at the local level. We view this an indicator of legal enforcement since more attorneys (per capita) represent greater prosecuting power and greater government resources devoted to the criminal justice system. On the U.S. side, the Federal Bureau of Investigation (FBI) Uniform Crime Reports provide us with measures of police officers stationed in each port (in per capita terms). This data is available at the city level for all ports except Presidio, TX and Rio Grande, TX, for which we instead used the county-level aggregate.

Since unauthorized immigration also varies with proximity to ports, and Mexican crime syndicates including drug cartels are increasingly involved with trafficking migrants across the border, we control for the extent of unauthorized immigration in the nearest port. DHS data gives the number of illegal immigrants apprehended in each border patrol sector, a geographic unit that is defined by the DHS itself. Six ports are uniquely assigned to one of these border patrol sectors. However, Nogales and Douglas belong to the same sector (of Tucson). Likewise, Eagle Pass is a part of Del Rio's sector; and Rio Grande City, McAllen, and Brownsville are assigned to Rio Grande Valley's sector. We use the log of unauthorized immigration in the port as the control variable.

Finally, since economic conditions in Mexican municipios and proximate U.S. ports may also affect criminal activity, we obtain data on a number of economic variables from both sides of the border. Data from the 2000 Mexican Census compiled by INEGI give cross-sectional measures of log income per capita and the school enrollment ratio, defined as the fraction of the population attending school between the ages of 6 and 24, for the year 2000. INEGI data also gives us population and total municipal expenditure. For the U.S., the Quarterly Census of Employment and Wages from the Bureau of Labor Statistics provides county-level measures of average earnings and employment, which we combine with population from the U.S. Census Bureau to generate the employment-to-population ratio in the nearest port.

In terms of the sample period, we focus our analysis on 2002 to 2006, when the dynamics of the Mexican drug war remains relatively constant, and preceding the major military operations and cartel destabilization that started in 2006 (see Section 2). Since gun law changes in the U.S. are likely to affect violence differentially in regions close to the border, we also define two distance based samples. The border sample includes 37 municipios that lie along the U.S.-Mexico border, of which 34 fall along the non-CA segment and 3 along the CA segment. The 100-mile sample includes of municipios whose geographic centroid lie within 100 miles of the nearest of the 13 major border ports. There are 79 municipios in this sample, shaded in green in Panel A of Figure V.

3.2 Descriptive Statistics of Key Variables

Table II presents the descriptive statistics of our key variables, for municipios within the 100-mile sample (denoted as "Proximity to border port $<$ 100 miles") and for the municipios beyond (denoted as "Proximity border port $>$ 100 miles"). We show the key dependent variables in per capita terms since our estimation strategy essentially scales the outcome variables by population. The average per capita homicide rate is 0.17 in the 100-mile sample, and lower at 0.11 in the further sample, which indicates that violence levels are higher in areas closer to the border. The mean gun-related homicides in the 100-mile sample is 0.12, indicating that 70% of total homicides on average are gun-related. 85% of the closer municipios have a highway, compared to 55% of the municipios further away. Income per capita is higher but school enrollment ratio is relatively smaller in the region closer to U.S. ports. In addition, the fraction of municipios with high drug trafficker density is also larger in the 100 mile sample (18 percent as compared to less than 1 percent).

4 Empirical Strategy

Our empirical strategy exploits the natural experiment induced by the 2004 expiration of the U.S. Assault weapons ban and proximity to the U.S.-Mexico border. The 2004 legislative change on the U.S. side of the border represents an exogenous shock that increased the availability of weapons transported into Mexico. However, the ban did not affect all U.S. states equally, since it relaxed the permissiveness of gun sales in the border states of Texas and Arizona, but not in California, which retained a previous state-level ban. We take advantage of this exogenous variation to examine whether measures of violence increased differentially in Mexican municipios closer to Texas and Arizona, relative to municipios closer to CA ports, after 2004.

We expect proximity to matter for the transport of weapons due to three types of costs. First, time and material transport costs increase with distance for the shipment all goods, including legal goods. Second, transport costs for illegal goods additionally vary by distance based on increased risk of detection by law enforcement authorities: every additional mile on the highway increases the probability that police may detect that a vehicle contains smuggled goods. Third, (as discussed in section 2), Mexican cartels based out of regions south of California face high turf-costs for operating along routes and ports near Texas and Arizona, which are controlled by rival cartels. These turf-costs exist on the Mexican and U.S. side of the border given growing links between Mexican drug cartels and U.S. gangs.

We focus our analysis on Mexican municipios near the border, which are most likely to be affected by an influx of weapons from the U.S. In Figure VI, we plot over time the sum of total homicides and gun-related homicides in the border municipios that lie along the CA segment

of the border, versus the segment bordering TX and AZ. This figure captures the essence of our empirical strategy. Panel A shows that total homicides along the CA segment essentially stayed constant over the 2002 to 2006 period (Panel A), but increased sharply in the TX and AZ segment after 2004. In particular, in the CA segment, homicides increased by 45 over 2002-2004, but actually fell by 31 from 2004-2006. In contrast, in the non-CA segment, sum homicides fell by 32 in the pre period, but increased by 173 over 2004 to 2006. A simple difference-in-differences of the mean across the pre- and post-2004 period, and two types of segments suggests a differential increase of 160 homicides in Mexican municipios closer to TX and AZ after the gun law change. Panel B shows that the same pattern holds for homicides specifically tied to guns, which increased by 182 (or 35%) over 2004-2006 in the non-CA segment. The analogous difference-in-differences suggests a differential increase of 123 gun-related homicides in the post-2004 period. However, these raw means are meant to be suggestive, as they do not account for municipal characteristics and other potential changes that may be correlated with the expiration of the FAWB and violence near the border.

Our empirical strategy builds on this simple comparison in a number of ways. We estimate a difference-in-differences type specification which employs municipio fixed effects to control for any time invariant characteristics that may be correlated with both proximity to TX and AZ and violence, as well as year effects, which control for common year-to-year changes in killings. Panel A of Figure VII shows the distribution of homicides in our sample. It indicates that counts of homicides are bunched around a few integers: 45% of observations have no homicide, while 80% have 5 or fewer. This bunching makes OLS an unattractive option in smaller samples, and makes count regressions a more appropriate alternative. Panel B of Figure VII shows that homicides normalized by population 10,000 also shows left-censoring, and comparing this distribution against the normal density shows why OLS may be inappropriate. To account for the limited nature of the dependent variable, we instead employ a conditional fixed effects Poisson model. Population is used as an exposure variable in all specifications, to account for differential population levels in determining the extent of violence. Finally, we use cluster-robust standard errors as recommended by Cameron and Trivedi (2009) to control for possible violations of the assumption that the mean and the variance are equal.[8]

The log of the expected counts is specified as follows:

$$\ln E(y_{jt}|Z_{jt}) = \alpha_j + \beta_t + (SegmentNCA_j \times Post_t)\lambda + \mathbf{X}_{jt}\phi + \ln(population_{jt}) \qquad (1)$$

[8]The Negative Binomial has an advantage over Poisson estimation in that it allows for over-dispersion in the data, whereas the Poisson model does not, and assumes that the conditional mean equals the variance. However, this weakness can be overcome in the Poisson model by estimating robust standard errors, as recommended by Cameron and Trivedi (2009). In addition, the consistency of the coefficients in negative binomial estimation is more sensitive to the distributional assumption of the error term. For this reason, we opt to use the Poisson model with cluster-robust standard errors.

where y_{jt} are homicide counts in municipio j and year t, α_j are municipio fixed effects, β_t are year fixed effects, and Z_{jt} represents the full set of explanatory variables, i.e., $Z_{jt} = [\alpha_j, \beta_t, SegmentNCA_j \times Post_t, \mathbf{X}_{jt}, \ln(population_{jt})]$. $SegmentNCA_j$ equals 1 if the municipio lies along the non-CA segment of the U.S. Mexico border, namely along Texas and Arizona. $Post_t$ is a dummy variable which equals 1 for each year after the policy change in 2004. λ is the coefficient of interest and measures the log point increase in expected homicide counts differentially in municipios along the non-CA segment.

Since a municipio's exposure to the gun law change should vary according to its proximity to major ports in CA vs. TX and AZ, we also utilize a specification that exploits our continuous measure of proximity. The log of the expected counts is defined as:

$$\ln E(y_{jt}|Z_{jt}) = \alpha_j + \beta_t + (ProximityNCA_j \times Post_t)\theta + \mathbf{X}_{jt}\delta + \ln(population_{jt}) \qquad (2)$$

where y_{jt} are various counts of violence, including homicides and criminal prosecutions in municipio j and year t. $ProximityNCA_j$ is the proximity of municipio j to the nearest port in non-California border states, Texas and Arizona. $Post_t$ is a dummy variable which equals for each year after the policy change in 2004. The coefficient θ captures the extent to which violence rises differentially in municipios located closer to Texas and Arizona ports, relative to those located closer to California ports, in the post-2004 period. In the Poisson model, the coefficient should be interpreted as indicating that a one unit change in x leads to a θ log point change in expected y in equation (2). $\mathbf{X}_{jt}$ is a vector of control variables which varies across specifications. If general drug war violence increased differentially in areas closer to the border in the post-2004 period, to the extent that proximity to non-CA ports is correlated with distance, this would generate potential upward bias on θ. To account for this, we control for $Proximityborder_j \times Post_t$ which is a municipio's proximity to any port on the U.S. Mexico border, interacted with the post-2004 indicator.

Panel A of Figure V shows the source of variation employed in estimating equation (2), the proximity to ports in Texas and Arizona, controlling for the overall distance to the border ports. It is worth noting that this differs from the simple proximity to these two states, since there are some municipios located close to the Texas and Arizona parts of the border, that do not have a port nearby. This distinction is important since the account we put forward relies on gun flows across the border, which take place via major highways along port cities. Our empirical strategy effectively asks whether the darker green municipios saw larger increases in violence in the post-2004 period. Panel B of Figure V provides some initial visual evidence for an affirmative answer. Darker red municipios are those that saw a greater rise in homicide per capita after the expiration of the FAWB. The figure shows that municipios along the Arizona and Texas borders did witness a larger rise in violence following the expiration of the ban.

5 Results

5.1 Baseline Effects on Homicides

In this section, we build on the suggestive evidence shown in Figures V and VI, and assess the effect of the assault weapons ban expiration on violence by estimating equations (1) and (2) using a Poisson model. All specifications include municipality and year fixed effects with robust standard errors clustered at the municipality level. Panel A of Table III presents these results for total homicides. Column (1) presents estimates of equation (1). The coefficient implies that Mexican municipalities which lay along the non-California segment of the U.S. Mexico border experienced an additional 0.34 log point (or 40%) increase in homicides after 2004 as compared to the California segment. The average annual homicides in this segment during the post-treatment period (i.e., 2005-2006) was 655. The 40% estimate implies that the counterfactual number of deaths that would have prevailed in the absence of the 2004 FAWB expiration is 468. Subtracting 468 from 655 indicates that the policy change resulted in an additional 187 deaths per year in the border segment near Texas and Arizona. This is very similar to the simple estimate of 160 differential deaths calculated on the basis of Panel A in Figure VI.

Panel B of Table III presents the results for gun-related homicides. The coefficient of 0.40 in column (1) implies a 49% increase in the gun-related murders. Given the average deaths of 420 in the post-treatment period, the counterfactual gun-related murders would have been 282, indicating that there were an additional 138 gun-related deaths in the non-California border segment due to the policy change. Again, this is quite similar to the simple calculation of 123 additional deaths based on Panel B of Figure VI.

Columns (2)-(3) present estimates for equation (2) but continue to restrict the sample to the border municipios. Columns (4)-(5) expand the sample to the region that lies within 100 miles of ports on the U.S.-Mexico border. The coefficients are slightly smaller when we control for overall distance to border ports, but this is our preferred specification since it accounts for other factors which may have increased violence near the border regions and are potentially correlated with our treatment.

When we focus on the continuous proximity (i.e., distance) measures exploited in this estimate, the relevant coefficient for homicides (Panel A, column (5)) is 3.4. Given our proximity scale, this coefficient suggests that going a 100 miles towards the U.S.-Mexico border is associated with a 0.34 log point (or 40%) increase in homicides. The mean distance to border in the 100 mile sample is 41 miles (a value of 0.41). Multiplying 0.40 by 0.41 suggests that homicides increased by 0.164 or about 16% on average in the sample municipios due to the 2004 expiration of the assault weapons ban. For this distance band, the actual average number

of homicides in the post-2004 period was 1121. Since the implied counterfactual deaths is 963, the estimates indicate that the policy change resulted in an additional 158 deaths per year in the set of municipios within 100 miles of the border. For gun-related homicides, the relevant coefficient is 3.82 (Panel B, column (5)). This implies a 19% rise in gun deaths in the average municipio in the 100 mile sample, associated with an additional 115 gun-related murders due the 2004 U.S. policy change.

Figure VIII shows the effects of the change in law by year. Instead of interacting *ProximityNCA* with *Post*, we interact it with year dummies, using 2004 as the omitted category. The controls include overall proximity to the border interacted with each year. For homicides overall, we see a clear sharp rise between 2004 and 2005 and the effect mostly persists through 2006. The results for homicides specifically tied to guns is noisier, but the pattern of a large increase between 2004 and 2005, and persistence through 2006 is reproduced here as well.

Rising homicides may reflect increases in different types of murder, some of which may be connected to organized crime, and others which may be carried out by individuals in connection with personal matters. If the increase in homicides is driven by members of crime syndicates targeting one other, then the effects should be larger for deaths of young men from a lower socioeconomic strata, as this is the demographic group most likely to be involved with drug cartels and organized crime.[9] To explore this question, we disaggregate the counts of total homicides into sub-groups based on age, gender and educational attainment, which we use a proxy for socioeconomic status. Our approach is similar to Owens (2011) who also uses age-specific changes in homicide rates to detect organized criminal activity. Specifically, she finds an increase in homicide rate of 20-30 year olds following state-level criminalization of alcohol markets.

Since approximately 12 percent of the individual-level mortality statistics observations were missing data on one of these characteristics, and we aim to compare effects on sub-groups directly to effects on overall homicides, we begin by re-generating municipio-level counts of homicides and gun-related homicides for observations that are not missing any one of these characteristics. Column (1) of Table IV presents these results. The coefficients in columns (2)-(3) show that the treatment effects are much larger for the sub-group of individuals above the age of 18 who have not completed high school, relative to everyone else, or the complementary set. Columns (4)-(5) show that the ratio of the estimated effects for the sub-group versus its complementary set are even larger for young men (between the ages of 18 and 30) who have not completed high school. For example, for gun-related homicides, the coefficient for all non-missing killings in column (1) is 3.5. The coefficient for young men without high school in

[9] For example, data from the Mexican presidency indicates that over 2006 to 2010, men comprised over 92% of drug-war related killings, and the age decile which represented the largest fraction of deaths were those between the ages of 21 and 30.

column (5) is 8 and significant at the 1% level, while the coefficient for everyone else in column (6) is 2.4 and statistically insignificant. The larger effects for young men with relatively low educational attainment is consistent with the idea that the expansion of organized crime has made a larger contribution to the rise in killings.

5.2 Effects on Homicides away from the Border

To examine whether the treatment effect arises solely in the region close to the border, in Table V, we divide the sample into 100 mile distance bands and re-estimate equation (2) across these samples. Starting from the 300 mile band distance to border ports becomes highly collinear with distance to ports in TX and AZ, and thus the overall distance variable cannot be estimated separately and drops out of specifications (3)-(5). The pattern of results indicate that the 2004 policy change significantly increased homicides only in the nearest 100-mile band, since the coefficient falls in magnitude and becomes insignificant starting with the second band. It underscores why we focus on the 100-mile sample in the remainder of the analysis, estimating whether proximity even within this sample is associated with differential changes in killings. Additionally, the fact that we don't see significant negative effects away from the border suggests that this increase in violence near the border was a net increase and not displacement of violence from the interior.

5.3 Influence of Specific States

One threat to identification using geographic proximity to different U.S. states is the possibility that there were other violence promoting shocks to particular border municipios near TX and AZ. It is possible to find specific events that occurred in these areas around the time of the FAWB expiration. For instance, the killing of the brother of the head of the Sinaloa cartel led to an increase in violence in Nuevo Laredo (on the Texas border) in 2004. To assess the sensitivity of our findings to such shocks, Table VI reports the estimates when we drop all the municipios from the sample that are closest to an AZ port (column 2), and closest to a TX port (column 3). Reassuringly, using either treatment group we find sizeable and statistically significant effects on overall as well as gun related homicides, suggesting such localized events do not drive the results.

5.4 Robustness Checks

In Table VII, we test the robustness of our baseline results to alternative samples, controls for trends, and a battery of controls related to drug trade, law enforcement and economic conditions. Column (1) reproduces the baseline results from column (5) of Table III. We

begin by including linear time trends by municipio. The maximum likelihood estimates do not converge with the inclusion of the linear time trends in a specification with both municipio and year fixed effects. However, in column (2) we show that our estimated coefficient remains almost identical when we replace year effects with a post-2004 indicator, and in column (3) show the results after including linear trends along with the post-2004 indicator. The coefficient of interest is actually larger in magnitude with trend controls, and remains significant for both homicides and gun homicides.

Since most guns are trafficked along major highways even once they reach Mexico, column (4) restricts the sample to those municipios that have at least one major highway, which eliminates 12 municipios from our sample in the homicide specification. The coefficients remain roughly the same in magnitude as the baseline, which confirms that the results are not driven by some idiosyncratic feature of the few regions that lack highway access. If the additional homicides using firearms are being committed by drug cartels, we would expect to find stronger evidence in municipios with greater historical presence of drug traffickers. We consider the sample of high-drug trafficker density municipios (those with at least 200 estimated drug traffickers per 100,000 population in the 1998-2001 period). As shown in column (5), the effects are much larger for this set of municipios, consistent with the idea that cartels are an important factor in why gun law changes increase the homicide rate.[10]

If a rise in homicides is correlated with factors that also promote other types of mortality, then our estimates may be upward biased if we do not control for these omitted factors or the ensuing resultant increase in other deaths. For example, political destabilization, natural disasters or an economic downturn may result in greater non-murder deaths through a rise in poverty and erosion of basic services, while increasing violence and crime by reducing the opportunity cost of participating in illicit activities. In column (6), we control for other non-homicide deaths, as well as non-gun related murders, and find that the results for homicides are only slightly smaller, and gun related homicides are nearly identical. Both coefficients continue to be significant at conventional levels. We note that this might constitute over-controlling, since the supply of guns may empower crime syndicates broadly, facilitating murders with guns, or through other means such as beheadings and mutilations which have become a part of the drug-war landscape.

Next, we control for other port and municipio level characteristics that lead to violence, and are potentially correlated with the treatment. For example, if the extent of the drug trade increased differentially near Texas and Arizona in the post period independent of the increased availability of guns, then violence associated with drug-related changes may bias our estimated effects. To account for this, in column (7), we include the value of major drugs seized in the

[10] The finding of differentially larger effects for municipios with historically high drug trafficker density also obtains for other cutoffs of traffickers per capita.

nearest U.S. port of entry and the value of drugs seized in Mexican municipios by the military. We also control for municipal level eradication of marijuana and heroin poppy fields, which are likely to reflect both the extent of drug cultivation and Mexican government enforcement, either of which may lead to greater violence. Figure IX shows the municipal change in eradication between the pre- and post-2004 period. The spatial pattern indicates a larger fall in eradication in municipios proximate to CA, suggesting why it may be important to include these as controls.

We also include a measure of the (log) number of unauthorized immigrants apprehended in the nearest port. This variable may be correlated with violence since drug cartels are increasing involved in trafficking migrants, as well as kidnapping and extorting them as they attempt to cross the border. In addition, we control for economic characteristics. Controlling for the employment ratio and average earnings in the nearest U.S. port accounts for the possibility that worsening economic conditions in non-CA ports may have increased crime rates in these U.S. cities, leading to more gun-running or exerting spillovers on homicides in Mexico through cross-border links in crime syndicates. We control for economic conditions in Mexican municipios by including (log) per-capita municipal expenditures, and by interacting municipal income per capita and the school enrollment ratio in 2000 with the post-2004 indicator. Expenditures control for differential changes in the provision of basic services, in health, education and local security, while the interaction effects control for trends in poverty and school enrollment which may have direct effects on municipal crime. Column (6) shows that including this array of drug, migration and economic controls enlarges the magnitude of estimated coefficients relative to the baseline estimates (in column (1)). The column (7) coefficient estimates (4.503 and 6.489) imply 210 additional murders, and 195 additional gun related murders, respectively.

Finally, in column (8) we add several police, military and legal enforcement related measures to the previous set of controls. First, for each municipio, we control for the contemporaneous number of drug-related detentions per capita by the Mexican military. This helps capture the effect of government military operations, which are also potentially correlated with our treatment. Second, we include the number of public attorneys (per capita) posted by federal or local law enforcement agencies in each municipio. This accounts for the possibility that differential prosecution intensity may affect crime levels confounding our results. Third, we account for differential enforcement levels across U.S. ports cities by controlling for the number of police officers per capita in the nearest port.

It is important to note that these enforcement controls are likely to respond positively to increased criminal activity induced by the policy change. As such, including contemporaneous values of the enforcement variables is a form of over-controlling, and represents a particularly tough hurdle. Even so, we find that the coefficients remain large in column (8)–the coefficients suggest 196 additional murders and 149 additional gun related murders. Homicides continue to be significant at the 5% level, though gun related homicides is only significant at the 10%

level. Given the nature of the controls, however, we interpret the results as demonstrating a clear pattern of increased violence in areas near the non-California port cities following the expiration of the FAWB.

5.5 Types of Homicide

While the violence fueled by the FAWB expiration is expected to increase gun-related homicides, the expected effect on non-gun related homicides is ambiguous. If added gun supply led to a substitution away from the use of other weapons, the policy change may have decreased the incidence of non-gun homicides. On the other hand, if guns promoted the expansion of the drug war more generally, which has increasingly involved other types of murders such as beheadings, the policy shock may have increased non-gun murders. Nonetheless, these effects are likely to be smaller relative to increases in murders committed by guns. In Table VIII, we show the effects separately for overall homicides, gun related homicides, as well as non-gun homicides with the full set of controls (same as column (8) of Table VII). We find that the coefficient for non-gun homicides is smaller, and also statistically insignificant at the conventional levels.

5.6 Gun Seizures

In this section, we present supporting evidence on the proliferation of weapons in Mexico following the FAWB expiration in the U.S. The account we put forward suggests that the U.S. law change increased the supply of assault weapons in municipios closer to the Texas and Arizona ports. We use data on crime guns seized by the Mexican National Defense Secretariat (SEDENA) to test this hypothesis. A caveat to these results is that the SEDENA-siezed guns account for approximately 30% of gun seizures in our sample period. The data disaggregate guns seized by handguns and rifles, the category that includes assault weapons. If the FAWB expiration is causally related to violence through assault weapons sales, we should expect to see greater increases in the rifle category only.

We also create two additional outcomes, "multiple rifles" and "multiple handguns," which are counts of guns on days when multiple guns of that type were seized in a given municipality. Such multiple seizures are more likely to reflect gun possession by members of organized crime groups. We include the full vector of controls related to drugs, enforcement and economic conditions (i.e., the same as Table VII column (8)). The results in Table IX indicate that the policy shock increased the number of rifles seized in Mexico, but not the number of confiscated handguns. Strikingly, the effect is strongest for "multiple rifles" seizures, while the "multiple handguns" seizures has the opposite sign and is not significant.

Since the FAWB did not affect the permissiveness of gun sales governing handguns, the

findings on seizures of rifles–and in particular multiple rifles–is consistent with the idea that the law change increased the supply of the larger assault weapons south of the border. The fact that we observe significant effects based on proximity suggests there are in fact, turf or enforcement-based costs associated with transporting weapons and presents evidence against full arbitrage of gun price differentials within Mexico. In addition, the results on multiple rifle seizures, coupled with those from Table IV showing larger effects on homicides of young, uneducated men, as well as column (5) of Table VII showing greater effects among higher drug-trafficking municipios, suggests that killings associated with increased gun supply reflect greater activity by organized crime syndicates, which were best positioned to take advantage of looser U.S. gun regulations in trafficking weapons to Mexico.

5.7 Criminal Prosecutions

Next, we turn toward detecting the impact of the FAWB expiration on criminal prosecutions. In particular, we analyze counts of individuals charged for both homicides or gun-related crime charges. The gun charges encompass non-murder charges including possession, trafficking, and illegal transportation of firearms. However, if legal enforcement changed differentially in border municipios affected by the drug war in the post-2004 period, then this would bias the estimated effects. For example, if direct threats by cartels on judges and other officials resulted in a fall in criminal prosecutions in these areas, this omitted factor would attenuate the measured effect of the change in law on prosecutions. In fact, media accounts suggest that threats to law enforcement officials became widespread during this period (see New York Times, April 19, 2009). On the other hand, if the government decided to increase prosecutions generally as a result of increased violence, then omitting an enforcement-related control would upwardly bias the estimates. We control for potential changes in enforcement in multiple ways: first, we are able to control for the total number of prosecutions for all other non-gun and non-homicide related charges. Moreover, we employ the full set of economic, drug and enforcement controls used in column (8) of Table VII, which includes a time-varying measure of the number of public attorneys posted in each municipio.

Table X presents these results, and shows that both gun-charges and homicide-charges increase differentially in areas closer to TX and AZ ports after the 2004 FAWB expiration, in both the border and 100-mile samples. This evidence suggests that changes in U.S. gun laws have pervasive effects that are detectable not just in rising mortality from homicides, but also from the criminal justice system itself. The coefficient on the gun related homicides allows us to get a sense of how many the additional murders were prosecuted. For homicides, the regression coefficient of 8.98, along with the total post-period homicide prosecutions in our 100-mile sample (282) suggests an additional 105 homicide-related prosecutions due to the policy

change. The parallel specification (Table VII column 8) implied an additional 196 homicides from the expiration of the FAWB, suggesting that perhaps little over half of the additional murders were prosecuted. However, this estimate is only suggestive, since a single person could commit multiple murders and multiple defendants could be convicted for a single homicide.

5.8 Time-Shifted Placebos using Quarterly Data

The estimates thus far have focused on the annual level data, in part because many of our control variables, including those related to law enforcement, are only available only at the annual level. However, we can generate counts of homicides and gun-related homicides at the quarterly level. Using the data at the quarterly level poses two advantages. First, it allows us to be more precise in defining the timing of treatment and appropriate sample period. The expiration of the FAWB took effect in September 2004. We therefore define the annual post-treatment period as 2005 and 2006, but with the quarterly data we can re-define the last quarter of 2004 to be part of the post period. In addition, since Operation Michoacan began in December 2006, we can eliminate the last quarter of 2006 from our sample, to ensure that we are isolating the effect of gun law changes rather than the rise in drug war violence unleashed in the aftermath of these government military operations. Since the post-treatment sample period extends to the third quarter of 2006, we specify a symmetric pre-treatment period, which extends to the fourth quarter in 2002. Thus, the quarterly regressions are estimated over a 16-period window.

In addition, the more fine-grained quarterly data allows us to estimate the effect of placebo laws, which we generate by shifting the post treatment 5 periods backward and 5 periods forward, creating 10 placebo treatments. As an example, the placebo law resulting from the -3 quarter shift placebo redefines the post period so it starts in the first quarter of 2004 (3 periods before the expiration of the FAWB actually took effect) while extending the first sample period back to the first quarter of 2002, and analogously ending the sample period three quarters earlier. Again, we use Poisson regressions, and focus on our preferred specification which exploits the proximity to non-CA ports while controlling for overall proximity to the border.

Figure X reports the coefficients from these placebo laws estimates, along with the 95% confidence intervals. First, it is worth noting that the 0 quarter baseline, which represents the actual treatment period, generates coefficients that are similar in magnitude to the equivalent specification in Panels A and B of Table III Column (5). For example, the coefficient for homicides is 2.78 and gun-related homicides is 2.65.[11] The placebo laws shifted backward by 1 quarter remain significant for homicides, which is not surprising given that most of our post and pre-treatment periods remain unaltered. However, the magnitude of the coefficient falls

[11] The estimated coefficients are not proportionately smaller given the smaller unit of time since the coefficients in Poisson regressions measure the impact of a unit change in x on y in log points.

even with this -1 placebo, and continues to fall monotonically for homicides and gun-homicides, become statistically insignificant by the -3 placebo. Going forward, the 1 quarter shift actually displays statistically significant coefficients that are larger in magnitude than in the 0 quarter baseline. This suggests that the effect occurred with a short delay, with the 2004 law change affecting violence even more after a 3-month lag. The magnitude of the coefficients continue to fall with additional quarter shifts. The quarterly regressions are less precisely estimated for homicides tied specifically to gun, although the effect is still significant at the 10% level for the actual law and at the 5% level for the 1 period shift, and the magnitude of the coefficients vary in the same pattern as the total homicides. Overall, given the tight timing with which the treatment affects homicides (precisely in the last quarter of 2004 when the law change occurred), and the smaller, insignificant effects of the placebo laws, the pattern in the quarterly results provide strong evidence that the 2004 FAWB expiration had immediate effects on violence in Mexico, and that these effects only grew in magnitude with a one-quarter lag. The lack of negative coefficients for the forward shifts also shows that the effect was persistent, which is consistent with evidence from Figures VI and VIII.

6 Conclusion

Our analysis has examined how the expiration of the U.S. federal assault weapons ban in 2004 affected violence in Mexico during the 2002-2006 period. Given widespread weapons trafficking across the U.S. Mexico border, the U.S. policy change represents an exogenous shock to the supply of weapons in Mexico. Since the policy weakened the gun control regime in Texas and Arizona more than in California, which retained a pre-existing state-level ban, we are able to exploit variation across Mexican municipios in distance to nearest port of entry into non-California border states, as well as the timing of the policy change, to identify this effect. We find that municipios closer to the Texas and Arizona ports (versus California ports) witnessed differential increases in total homicides, homicides tied specifically to guns, as well as criminal convictions for murders and gun-related offenses in the post-2004 period. These municipios also experienced larger increases in seizures of rifles (but not handguns), which provides evidence that the policy change increased violence through its effect on the supply of a particular type of weapon in Mexico.

Our baseline estimates suggest that municipios at the Texas and Arizona border ports saw total homicides rise by 40% compared to municipios 100 miles away, implying an additional 158 more homicides in the two years following the expiration of the FAWB. Importantly, the estimated effect is significant in municipios within 100 miles of the border, but small and insignificant for areas that are further away. This is consistent with the notion that a greater supply of assault weapons trafficked into Mexico from the U.S. transmits larger spillovers on

violence in areas proximate to the border. The results are robust to a number of controls for economic conditions, drug eradication, military and legal enforcement, municipio-level linear trends, as well as the extent of undocumented immigration, policing and drug trafficking in the nearest U.S. port. This indicates that the estimates are not driven by differential changes in the demand for drugs, enforcement efforts, or economic conditions in nearby regions. In addition, by using fine-grained quarterly data, we show that the change in U.S. gun law had immediate effects on killings in Mexico that persisted thereafter.

There are several reasons why it is surprising that gun law changes in the U.S. are found to have detectable, significant effects on violence in Mexico. First, it is possible that an increase in weapons exports from the U.S. to Mexico reduced weapons imports from other countries. Second, if a larger supply of assault weapons simply led to substitution toward murders using assault weapons, and away from murders using other types of weapons, then the law change may not necessarily produce a net increase in either gun-homicides or total homicides. Finally, more guns need not translate into more crime, as theoretically, access to firearms can have a deterrent effect. However, by showing that the 2004 change in U.S. gun law had differential effects on homicides in Mexican municipios with greater exposure to the policy shock, our results present evidence against these alternative scenarios. Ultimately, the analysis suggests that U.S. gun laws exert an unanticipated spillover on gun supply in Mexico, and this exogenous increase in gun supply has contributed to rising violence south of the border.

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Table I

Definition of Ports Based on Truck Traffic & Distance between Border Crossings

State	County	Border crossing	Mean truck traffic (2002-2006)	Distance to other nearest border crossing	Port
CA	San Diego	San Diego	726,866	20 miles to Tecate	*San Diego*
CA	San Diego	Tecate	65,943	20 miles to San Diego	*San Diego*
CA	Imperial	El Centro	295,452	44 miles to Yuma	*El Centro*
CA	Imperial	Andrade	2,207	17 miles to Yuma	*Yuma*
AZ	Yuma	Yuma	41,716	17 miles to Andrade	*Yuma*
AZ	Pima	Lukeville	921	80 miles to Sasabe	-
AZ	Pima	Sasabe	954	37 miles to Nogales	-
AZ	Santa Cruz	Nogales	257,796	37 miles to Sasabe	*Nogales*
AZ	Cochise	Naco	4,271	24 miles to Douglas	*Douglas*
AZ	Cochise	Douglas	27,000	24 miles to Naco	*Douglas*
NM	Luna	Columbus	4,737	59 miles to Santa Teresa	-
NM	Dona Ana	Santa Teresa	31,358	11 miles to El Paso	*El Paso*
TX	El Paso	El Paso	713,993	11 miles to Santa Teresa	*El Paso*
TX	Presidio	Presidio	6,365	197 miles to El Paso	*Presidio*
TX	Val Verde	Del Rio	66,254	52 miles to Eagle Pass	*Del Rio*
TX	Maverick	Eagle Pass	94,705	52 miles to Del Rio	*Eagle Pass*
TX	Webb	Laredo	1,432,466	89 miles to Rio Grande City	*Laredo*
TX	Starr	Roma	8,589	11 miles to Rio Grande City	*Rio Grande City*
TX	Starr	Rio Grande City	38,435	11 miles to Roma	*Rio Grande City*
TX	Hidalgo	McAllen	439,920	19 miles to Progreso	*McAllen*
TX	Hidalgo	Progreso	24,372	19 miles to McAllen	*McAllen*
TX	Cameron	Brownsville	236,461	50 miles to McAllen	*Brownsville*

Notes. Mean truck traffic is the annual average number of trucks that crossed the border during 2002-2006, based on data from the Bureau of Transportation Statistics (BTS). The distance to the other nearest border crossing was computed from the actual border crossing point, not the center of the city. A border crossing is considered a port if it has a mean truck flow above 5000, and if it is at least 30 miles away from another border crossing. If two border crossings are less than 30 miles apart, they are considered one single port, which is named after the border crossing with higher truck traffic. Three border crossings (Lukeville, Sasabe and Columbus) have mean truck flows less than 5000 (but are more than 30 miles from another crossing). These are not a part of the port classification and analysis, as indicated by a missing entry for the "Port" column. Andrade has a mean truck flow less than 5000, but is considered a part of Yuma port since it is less than 30 miles from Yuma. Naco is considered a part of the Douglas port by the same criteria.

Table II
Descriptive Statistics

Sample:	Proximity border < 100 miles			Proximity border > 100 miles		
	Obs.	**Mean**	**Std. dev.**	**Obs.**	**Mean**	**Std. dev.**
Panel-level Variables:						
Population	395	99392	246411	11770	40377	119645
Homicides per 1000 pop.	395	0.169	0.350	11770	0.109	0.259
Gun-related homicides per 1000 pop.	395	0.119	0.302	11770	0.065	0.209
Non-homicide deaths per 1000 pop.	395	4.016	1.730	11770	4.391	2.396
Non-gun homicides per 1000 pop.	395	0.039	0.091	11770	0.031	0.116
Rifles seized per 1000 pop.	395	0.091	0.353	11770	0.017	0.141
Multiple rifles seized per 1000 pop.	395	0.019	0.097	11770	0.003	0.036
Handguns seized per 1000 pop.	395	0.066	0.290	11770	0.016	0.121
Multiple handguns seized per 1000 pop.	395	0.009	0.074	11770	0.002	0.025
Homicide prosecutions per 1000 pop.	395	0.084	0.255	11770	0.041	0.155
Gun-related prosecutions per 1000 pop.	395	0.277	0.404	11770	0.106	0.203
Other prosecutions per 1000 pop.	395	3.423	3.421	11770	0.995	1.364
Log municipal expenditure per capita	389	-6.041	0.544	10388	-6.357	0.524
Public attorneys per 1000 pop.	271	0.062	0.091	11183	0.032	0.072
Drug war detainees per 1000 pop.	395	0.313	1.060	11770	0.033	0.268
Log drug value seized in municipio	395	7.434	8.091	11770	1.366	4.210
Log marijuana eradication	395	0.220	0.742	11770	0.213	0.891
Log poppy eradication	395	0.118	0.487	11770	0.143	0.755
Log drug value seized in nearest port	395	18.740	1.190	11770	18.070	0.550
Police officers in nearest port per 1000 pop.	395	1.718	0.911	11770	1.237	0.410
Log unauthorized immigrants in nearest port	395	11.681	0.876	11770	11.461	0.514
Log earnings per capita in nearest port	395	6.195	0.198	11770	6.117	0.103
Employment ratio in nearest port	395	0.285	0.060	11770	0.296	0.045
Cross-sectional variables:						
Segment NCA	100	0.310	0.465	2354	0.002	0.041
Proximity NCA (000s miles)	79	0.941	0.028	2323	0.502	0.141
Proximity border (000s miles)	79	0.943	0.026	2323	0.502	0.141
Highway	79	0.848	0.361	2354	0.551	0.498
High drug trafficker density	79	0.177	0.384	2354	0.006	0.080
Log municipal income per capita in 2000	78	9.021	0.297	2320	8.210	0.577
Municipal school enrollment in 2000 (percent)	79	57.201	4.956	2342	60.064	6.316

Notes. This table shows descriptive statistics of key variables for the main sample period, 2002-2006. Proximity border is a municipio's proximity to the nearest of all border ports. Proximity border<100 miles is the set of municipios that lie within 100 miles of the nearest border port, and this is referred to as the 100-mile sample. Proximity border>100 miles is the set of municipios beyond the 100-mile mark. Segment NCA is an indicator of whether the municipio lies on the non-CA segment of the U.S. Mexico border, adjacent to Texas (TX) and Arizona (AZ). Proximity NCA is the proximity of a municipio to the nearest port in TX and AZ. Highway is an indicator for whether a municipio has a highway. High drug trafficker density equals one if the fraction of drug traffickers per 100,000 inhabitants exceeds 200. Log municipal income per capita is the natural log of municipal GDP measured in US Dollars, in per capita terms in 2000. Municipal school enrollment is the fraction of the population aged 6-24 attending school in 2000. Log poppy and marijuana eradication are the natural log of hectares of each drug crop eradicated plus 1. Log drug value seized in each municipio is the natural log of the value of heroin, cocaine, marijuana and methamphetamines plus 1 seized in each municipio.

Table III

Expiration of the U.S. Assault Weapons Ban and Violence in Mexican Municipios

	(1)	(2)	(3)	(4)	(5)
			Panel A: Homicides		
Segment NCA x post	0.336*				
	(0.175)				
Proximity NCA x post		5.040**	3.410**	4.643*	3.399**
		(2.525)	(1.500)	(2.446)	(1.513)
Observations	185	185	185	345	345
			Panel B: Gun-related Homicides		
Segment NCA x post	0.403*				
	(0.231)				
Proximity NCA x post		5.800*	3.552*	5.362*	3.819**
		(3.277)	(1.822)	(3.151)	(1.843)
Observations	185	185	185	325	325
Proximity border x post control?			Y		Y
Sample	Border	Border	Border	100-mile	100-mile

Notes. All estimates are based on Poisson regressions using population exposure. Variables not shown include municipio and year fixed effects. Robust standard errors clustered at the municipio level are shown in parentheses. Segment NCA x post interacts an indicator of whether the municipio lies on the CA segment of the U.S. Mexico border with a post-2004 indicator. Proximity NCA x post interacts the proximity of a municipio to the nearest port in Texas and Arizona with a post-2004 indicator. Proximity border x post interacts the proximity to the nearest of all ports with a post-2004 indicator. The border sample includes the set of municipios that are located along the U.S. Mexcio border. The 100-mile sample includes the set of municipios that lie within 100 miles of a port on the U.S. Mexico border. *** is significant at the 1% level; ** is significant at the 5% level; and * is significant at the 10% level.

Table IV

Expiration of U.S. Assault Weapons Ban and Violence in Subgroups

	(1)	(2)	(3)	(4)	(5)
			Panel A: Homicides		
Proximity NCA x post	2.941**	4.183**	2.502**	5.700**	2.404*
	(1.428)	(1.863)	(1.238)	(2.531)	(1.257)
Observations	390	365	345	245	380
			Panel B: Gun-related Homicides		
Proximity NCA x post	3.468*	6.614**	1.777	8.054***	2.426
	(1.798)	(2.653)	(1.529)	(3.008)	(1.623)
Observations	365	295	315	190	355
Proximity border x post control?	Y	Y	Y	Y	Y
Sample	All	Aged 18+ w/o H.S.	All but 18+ w/o H.S.	Males 18-30 w/o H.S.	All but males 18-30 w/o H.S

Notes. All estimates are based on Poisson regressions using population exposure. Variables not shown include municipio and year fixed effects. Robust standard errors clustered at the municipio level are shown in parentheses. Proximity NCA x post interacts the proximity of a municipio to the nearest port in Texas and Arizona with a post-2004 indicator. Proximity border x post interacts the proximity to the nearest of all ports with a post-2004 indicator. The "All" sample includes all homicide or gun-related homicide observations not missing information about age, gender and education. As for the other samples, "18+" denotes those 18 years or older, 18-30 indicates those between the ages of 18 and 30, and "w/o H.S" indicates those without a High School degree. *** is significant at the 1% level; ** is significant at the 5% level; and * is significant at the 10% level.

Table V

Expiration of U.S. Assault Weapons Ban and Violence by Distance Bands

	(1)	(2)	(3)	(4)	(5)
			Panel A: Homicides		
Proximity NCA x post	3.399**	-10.731	1.560	1.768	0.914
	(1.513)	(8.251)	(1.864)	(1.424)	(1.275)
Observations	320	435	460	990	2,595
			Panel B: Gun-related Homicides		
Proximity NCA x post	3.819**	-7.205	3.536	2.174	1.522
	(1.843)	(13.074)	(2.518)	(1.745)	(1.800)
Observations	305	360	440	870	2,215
Proximity border x post control?	Y	Y	Y	Y	Y
Sample	0-100 miles	100-200 miles	200-300 miles	300-400 miles	400-500 miles

Notes. All estimates are based on Poisson regressions using population exposure. Variables not shown include municipio and year fixed effects. Robust standard errors clustered at the municipio level are shown in parentheses. Proximity NCA x post interacts the proximity of a municipio to the nearest port in Texas and Arizona with a post-2004 indicator. Proximity border x post interacts the proximity to the nearest of all ports with a post-2004 indicator. *** is significant at the 1% level; ** is significant at the 5% level; and * is significant at the 10% level.

Table VI

Expiration of U.S. Assault Weapons Ban and Violence - Sensitivity to Treatment Groups

	(1)	(2)	(3)
		Panel A: Homicides	
Proximity NCA x post	3.399**	2.835***	3.353**
	(1.513)	(1.020)	(1.655)
Observations	395	85	330
		Panel B: Gun-related Homicides	
Proximity NCA x post	3.819**	5.683***	3.283*
	(1.843)	(1.156)	(1.994)
Observations	370	85	305
Proximity border x post control?	Y	Y	Y
Sample	All	Nearest port in CA/AZ	Nearest port in CA/TX

Notes. Variables not shown include municipio and year fixed effects. Robust standard errors clustered at the municipio level are shown in parentheses. Proximity NCA x post interacts the proximity of a municipio to the nearest port in Texas and/or Arizona with a post-2004 indicator. In Column (2) we drop all municipios whose nearest port is along the border with TX; in column (3) we drop all municipios whose nearest port is along the border with AZ. *** is significant at the 1% level; ** is significant at the 5% level; and * is significant at the 10% level.

Table VII
Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A: Homicides				
Proximity NCA x post	3.399**	3.392**	5.397***	3.281**	6.006***	2.236***	4.503***	4.206**
	(1.513)	(1.515)	(1.568)	(1.476)	(2.173)	(0.862)	(1.609)	(2.120)
Observations	395	395	395	219	70	395	384	258
				Panel B: Gun-related homicides				
Proximity NCA x post	3.819**	3.803**	5.069***	3.619**	7.269*	3.533***	6.489***	4.945*
	(1.843)	(1.847)	(1.902)	(1.782)	(3.786)	(1.248)	(2.226)	(2.725)
Observations	370	370	370	206	70	370	359	240
Proximity border x post?	Y	Y	Y	Y	Y	Y	Y	Y
Year fixed effects?	Y			Y	Y	Y	Y	Y
Post-2004 indicator?		Y	Y					
Linear time trends?			Y					
Non-hom. death and non-gun hom.?						Y		
Income, immigration and drugs?							Y	Y
Current enforcement?								Y
Sample	100-mile	100-mile	100-mile	100-mile & highway	100-mile & high drug trafficker density	100-mile	100-mile	100-mile

Notes. All estimates are based on Poisson regressions using population exposure. Variables not shown include municipio fixed effects. Robust standard errors clustered at the municipio level are shown in parentheses. Proximity NCA x post interacts the proximity of a municipio to the nearest port in Texas and Arizona with a post-2004 indicator. Proximity border x post interacts the proximity to the nearest of all ports with a post-2004 indicator. All specifications include year effects except columns (2)-(3) which include a post-2004 indicator. Column (3) also includes linear time trends at the municipio level. Column (4) restricts the sample to municipios that have a highway. Column (5) restricts the sample to municipios where the number of drug traffickers per 100,000 inhabitants exceeds 200. Column (6) controls for counts of non-homicide deaths and non-gun related homicides. Income, immigration and drug controls in Column (7) include: log municipal per capita income in 2000 and the schooling ratio in 2000 interacted with a post-2004 indicator; log municipal expenditures per capita; log value of municipal drug seizures plus 1; log hectares of marijuana and heroin poppies eradicated in each municipio plus 1; as well as the employment ratio, log average earnings, log unauthorized immigrants and log value of drugs seized in the nearest U.S. port of entry. Current enforcement controls in column (8) include attorneys per capita and military drug-war detentions per capita in Mexican municipios, as well as police officers per capita in the nearest U.S. port. The 100-mile sample includes the set of municipios that lie within 100 miles of a port on the U.S. Mexico border. The 100-mile & highway sample further restricts the sample to municipios with a major highway. *** is significant at the 1% level; ** is significant at the 5% level; and * is significant at the 10% level.

Table VIII
Expiration of U.S. Assault Weapons Ban and Type of Homicides

	(1)	(2)	(3)
	Homicides	*Gun Homicides*	*Non-Gun Homicides*
Proximity NCA x post	4.206**	4.945*	2.540
	(2.120)	(2.725)	(3.331)
Observations	258	240	208
Proximity border x post control?	Y	Y	Y
Income, immigration and drug controls?	Y	Y	Y
Current enforcement controls?	Y	Y	Y
Sample	100-mile	100-mile	100-mile

Notes. Variables not shown include municipio and year fixed effects. Robust standard errors clustered at the municipio level are shown in parentheses. Proximity NCA x post interacts the proximity of a municipio to the nearest port in Texas and Arizona with a post-2004 indicator. *** is significant at the 1% level; ** is significant at the 5% level; and * is significant at the 10% level.

Table IX
Expiration of the U.S. Federal Assault Weapons Ban and Gun Seizures in Mexico

	(1) *Rifles*	(2) *Multiple Rifles*	(3) *Handguns*	(4) *Multiple Handguns*
Proximity NCA x post	18.787***	35.398***	-1.350	-12.632
	(6.520)	(11.687)	(6.894)	(15.910)
Proximity border x post control?	Y	Y	Y	Y
Income, immigration and drug controls?	Y	Y	Y	Y
Current enforcement controls?	Y	Y	Y	Y
Observations	224	114	164	94
Sample	100-mile	100-mile	100-mile	100-mile

Notes. All estimates are based on Poisson regressions using population exposure. Variables not shown include municipio and year fixed effects. Robust standard errors clustered at the municipio level are shown in parentheses. Proximity NCA x post interacts the proximity of a municipio to the nearest port in Texas and Arizona with a post-2004 indicator. Proximity border x post interacts the proximity to the nearest of all ports with a post-2004 indicator. Income, immigration and drug controls include: log municipal per capita income in 2000 and the schooling ratio in 2000 interacted with a post-2004 indicator; log municipal expenditures per capita; log value of municipal drug seizures plus 1; and log hectares of marijuana and heroin poppies eradicated plus 1; as well as the employment ratio, log average earnings, log unauthorized immigrants and log value of drugs seized in the nearest U.S. port of entry. Current enforcement controls include attorneys per capita and military drug war detentions per capita in Mexican municipios, and police officers per capita in the nearest U.S. port. The 100-mile sample includes the set of municipios that lie within 100 miles of a port on the U.S. Mexico border. The 100-mile & highway sample further restricts the sample to municipios with a major highway. *** is significant at the 1% level; ** is significant at the 5% level; and * is significant at the 10% level.

Table X

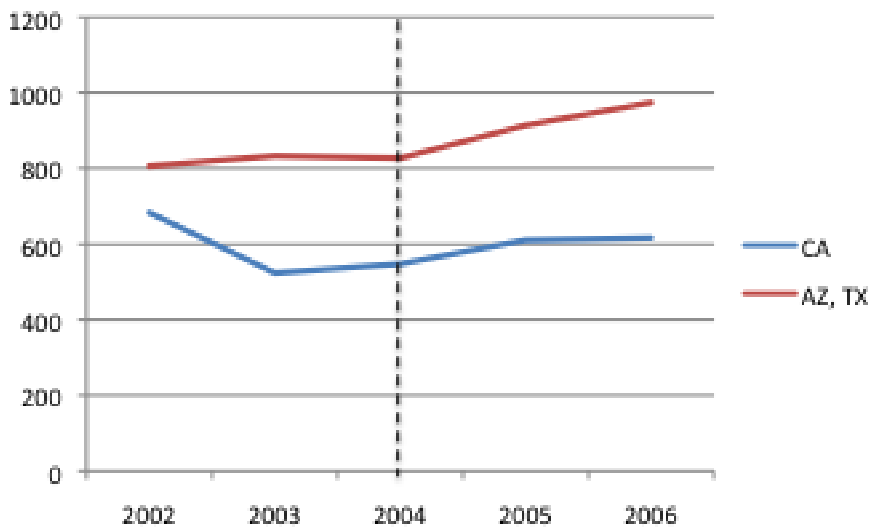
Expiration of U.S. Assault Weapons Ban and Criminal Prosecutions

	(1)	(2)	(3)	(4)
	Homicide		*Gun-related*	
Dependent Variable:	*Prosecutions*		*Prosecutions*	
Proximity NCA x post	9.603**	8.981**	8.597***	6.120***
	(3.993)	(3.679)	(3.228)	(1.710)
Observations	113	225	135	291
Proximity border x post control?	Y	Y	Y	Y
Income, immigration and drug controls?	Y	Y	Y	Y
Current enforcement controls?	Y	Y	Y	Y
Sample	Border	100-mile	Border	100-mile

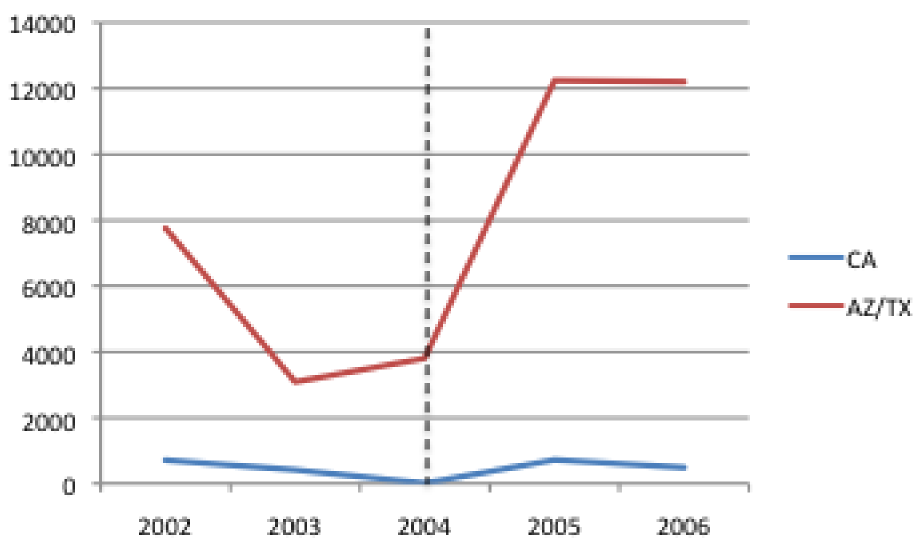
Notes. All estimates are based on Poisson regressions using population exposure. Variables not shown include municipio and year fixed effects; other non-gun and non-homicide prosecutions. Robust standard errors clustered at the municipio level are shown in parentheses. Proximity NCA x post interacts the proximity of a municipio to the nearest port in Texas and Arizona with a post-2004 indicator. Proximity border x post interacts the proximity to the nearest of all ports with a post-2004 indicator. Income, immigration and drug controls include: log municipal per capita income in 2000 and the schooling ratio in 2000 interacted with a post-2004 indicator; log municipal expenditures per capita; log value of municipal drug seizures plus 1; and log hectares of marijuana and heroin poppies eradicated plus 1; as well as the employment ratio, log average earnings, log unauthorized immigrants and log value of drugs seized in the nearest U.S. port of entry. Current enforcement controls include attorneys per capita and military drug war detentions per capita in Mexican municipios, and police officers per capita in the nearest U.S. port. The 100-mile sample includes the set of municipios that lie within 100 miles of a port on the U.S. Mexico border. The 100-mile & highway sample further restricts the sample to municipios with a major highway. *** is significant at the 1% level; ** is significant at the 5% level; and * is significant at the 10% level.

Figure I
Gun Sales and Production – California versus Other Border States

Panel A: *Estimated Annual Total Gun Sales*



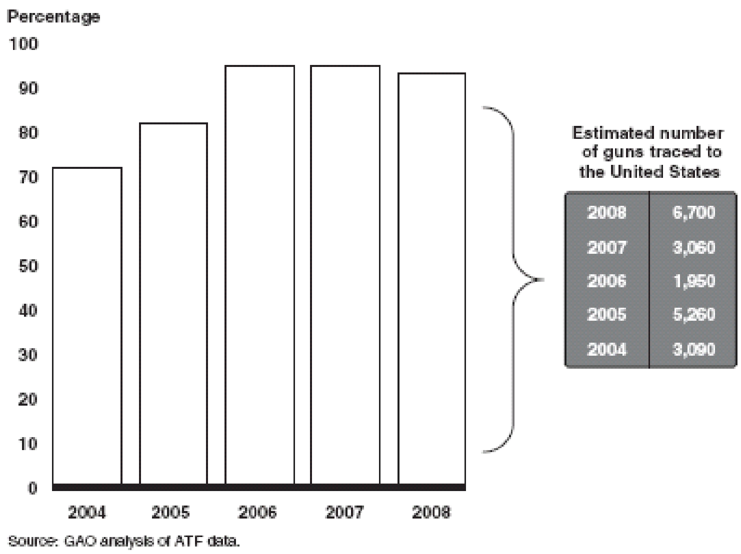
Panel B: *Annual Production of Rifles*



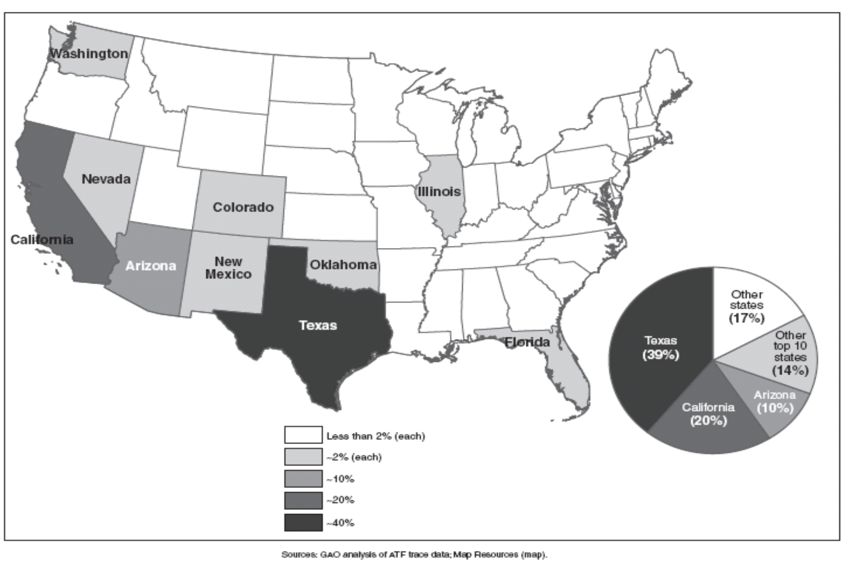
Notes. In Panel A, the total number of gun sales is approximated by the number of FBI NICS firearm background checks originating in the relevant state. The NICS data is available at: http://www.fbi.gov/about-us/cjis/nics/reports/state_totals_2011. In Panel B, data on the annual production of rifles comes from the Annual Firearms Manufacturing and Exportation Reports (AFMER). The dashed vertical line in 2004 represents the expiration of the federal assault weapons ban.

Figure II
Guns Seized in Mexico Traced to the United States

Panel A: *Overall Fraction Traced to US Over Time*

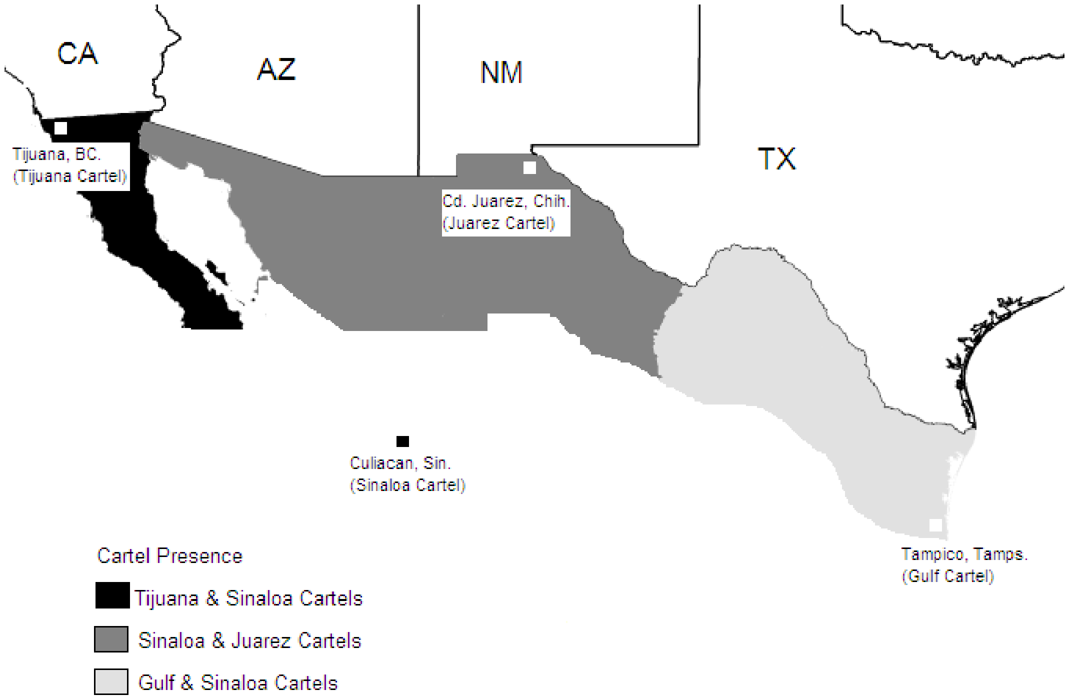


Panel B: *Source of Traced Guns – 2004-2008*



Notes. Both figures are from the GAO (2009) Report and based on ATF data. Mexican authorities send a sample of seized firearms to ATF for tracing the location of the last legal transaction. This data has not been made available to the public or researchers by the ATF.

Figure III
Cartel Presence along the Mexico-US Border (2002-2006)



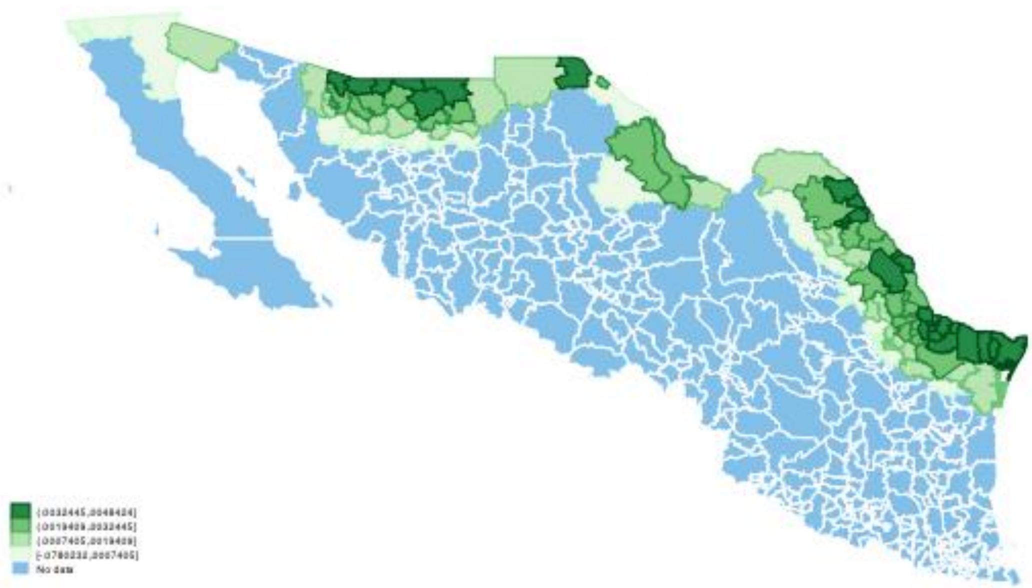
Notes. Notes. This map shows the approximate geographic location of Mexican Cartels in border states over 2002-2006, based on information from *La Jornada* (2002), *La Jornada* (2005, CRS (2007), and STRATFOR Global Intelligence (2008). The shaded areas denote the areas in which various cartels operate. The squares represent the headquarter cities of each cartel, with the relevant cartel is written in parentheses. The U.S. border states include California (CA), Arizona (AZ), New Mexico (NM) and Texas (TX).

Figure IV
Highways and Ports of Entry on the U.S. Mexico Border

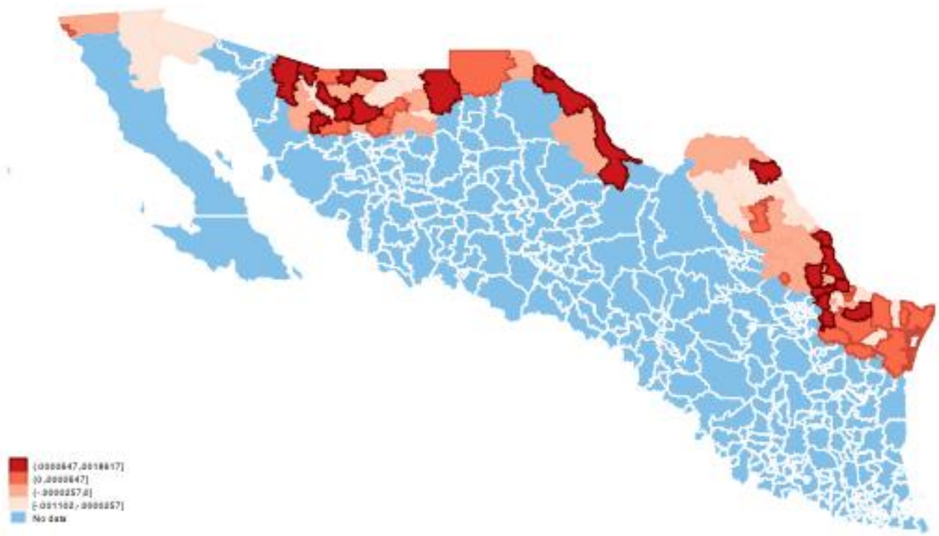
Notes. Panel C shows highways in Mexico. The black lines represent highways. Municipios in green constitute the sample of municipios on the border with highways. Port cities in California (San Diego, El Centro) are marked in red. Port cities in other states are marked in blue: Yuma (AZ), Nogales (AZ), Douglas (AZ), El Paso (TX), Presidio (TX), Eagle Pass (TX), Del Rio (TX), Laredo (TX), Rio Grande (TX), McAllen (TX), and Brownsville (TX).

Figure V
Proximity to Ports and Change in Violence in Sample Mexican Municipios

Panel A: *Continuous measure of Proximity of Municipios to non-CA Ports of Entry*



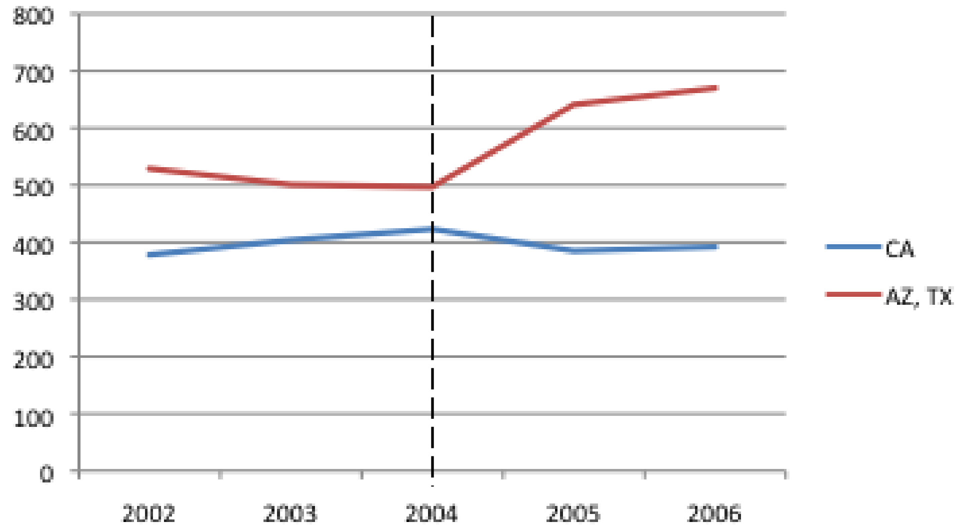
Panel B: *Change in Homicide per Capita – 2005-2006 versus 2002-2004*



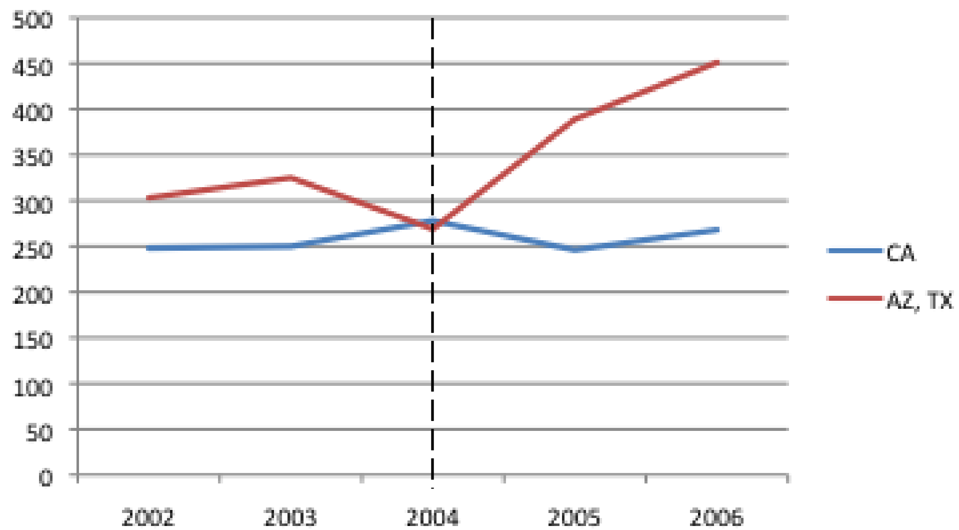
Notes. Panel A shows the "residual proximity" of each muncipio. This is constructed by first regressing "proximity" (i.e., 100 miles – distance) to the nearest Non-California port on proximity to the nearest port for the municipios in the within-100 miles sample, and then generating residuals from this regression, which represent the identifying variation in our research design. The magnitudes of these residuals are represented by the four shades of green so that darker shades signify proximity to a non-CA port, holding overall proximity constant. Panel B shows the change in the average homicide per capita in a municipio between the pre-treatment (2002-2004) and post-treatment (2005-2006) periods. Darker red signifies greater increase in homicides. All municipios outside of the 100 miles of the ports are shown in light blue in both panels.

Figure VI
Total number of Homicides in Municipalities Bordering California versus Other Border States

Panel A: *All Homicides*



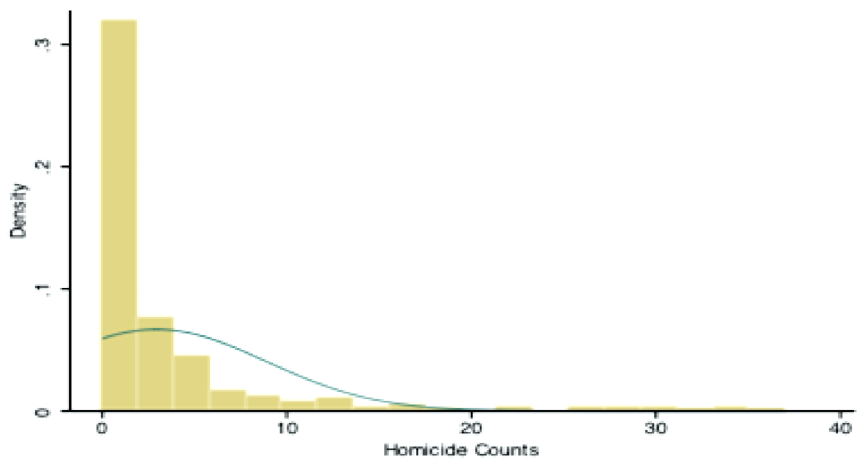
Panel B: *Gun-related homicides*



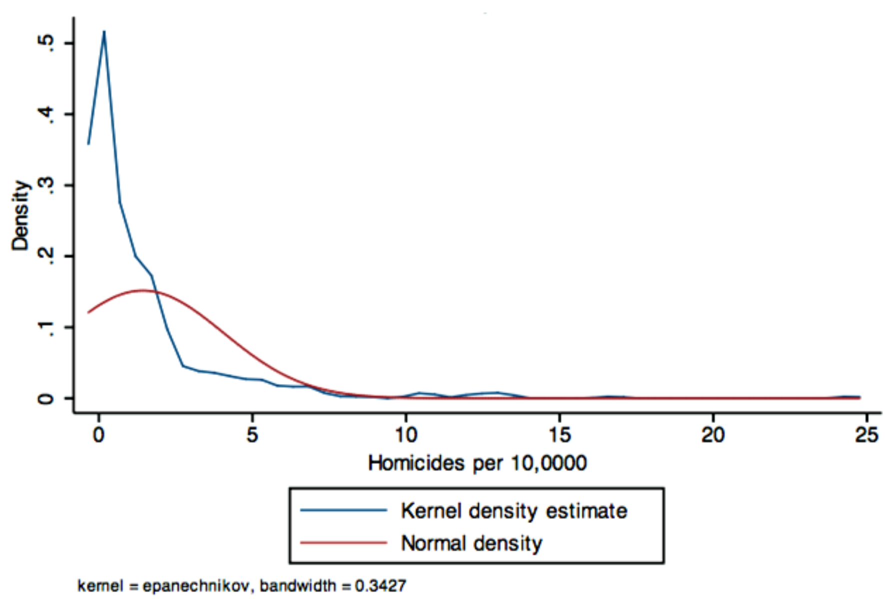
Notes. Panel A plots the total number of homicides for the "border segment" of municipios adjacent to California, versus those adjacent to Texas and Arizona. In panel B plots the equivalent numbers of homicides that were specifically tied to guns. Many homicides are not tied to any specific weapon in the data, so this represents a lower bound of the true gun related homicides. The expiration of the federal assault weapons ban occurred in 2004. The dashed line denotes 2004, the year in which the federal assault weapons ban expired.

Figure VII
Distribution of Homicide Counts and Homicides per 10,000 Population

Panel A: *Homicide Counts*



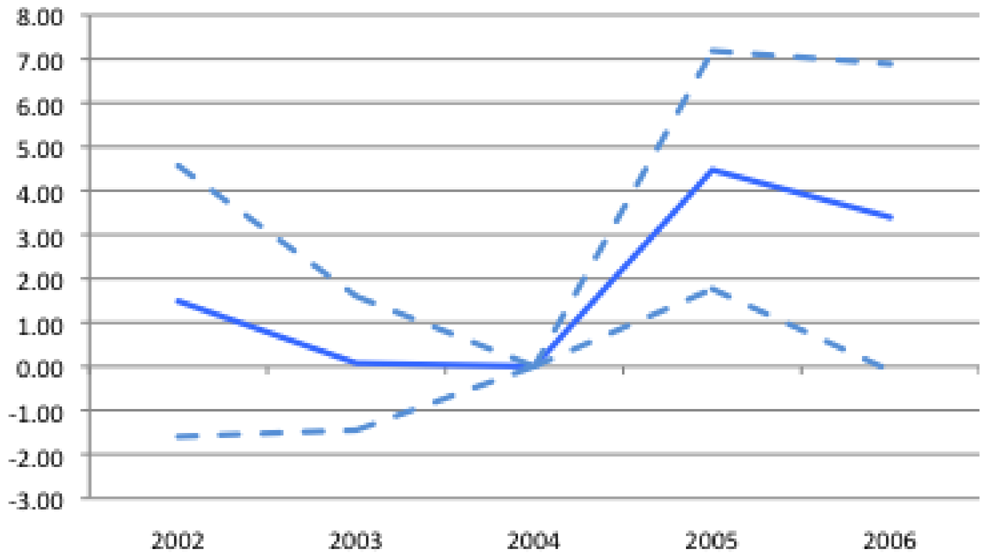
Panel B: *Homicides per 10,000 Population*



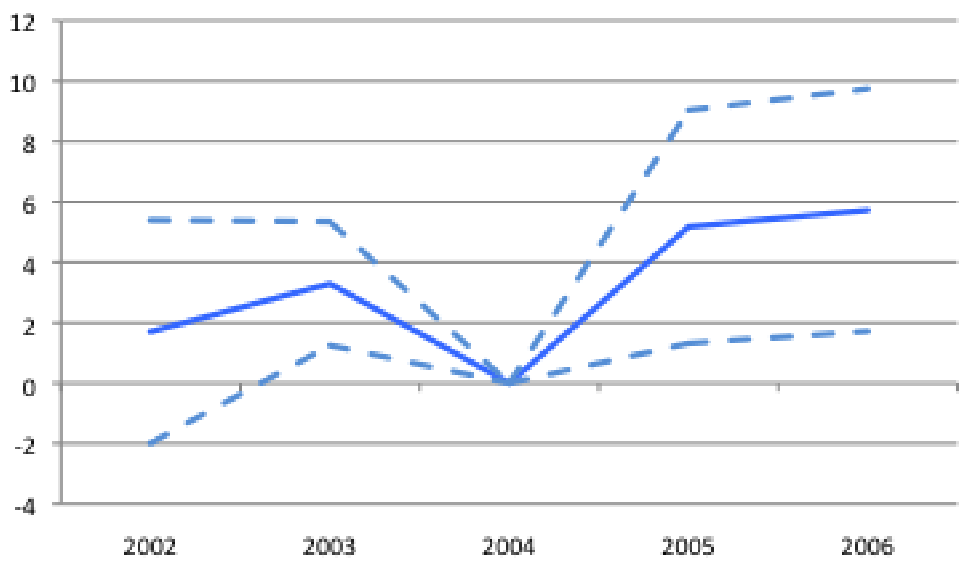
Notes. Panel A reports the histogram of homicide counts in the sample, along with a fitted Normal density, for our sample of municipios within 100 miles of a port of entry, over 2002-2006. Panel B reports the kernel density estimate of homicides per 10,000 population in each municipio over the same time period, along with a fitted normal density estimate.

Figure VIII
Effect on Crime by Year

Panel A: *All Homicides*



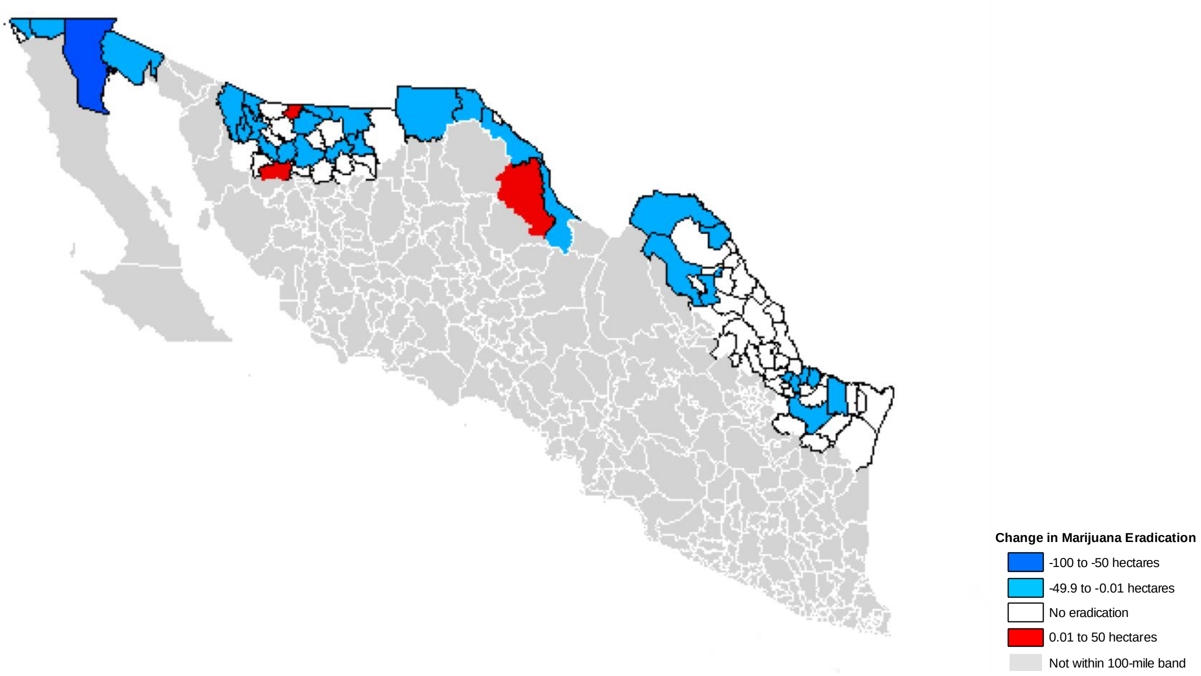
Panel B: *Gun-related Homicides*



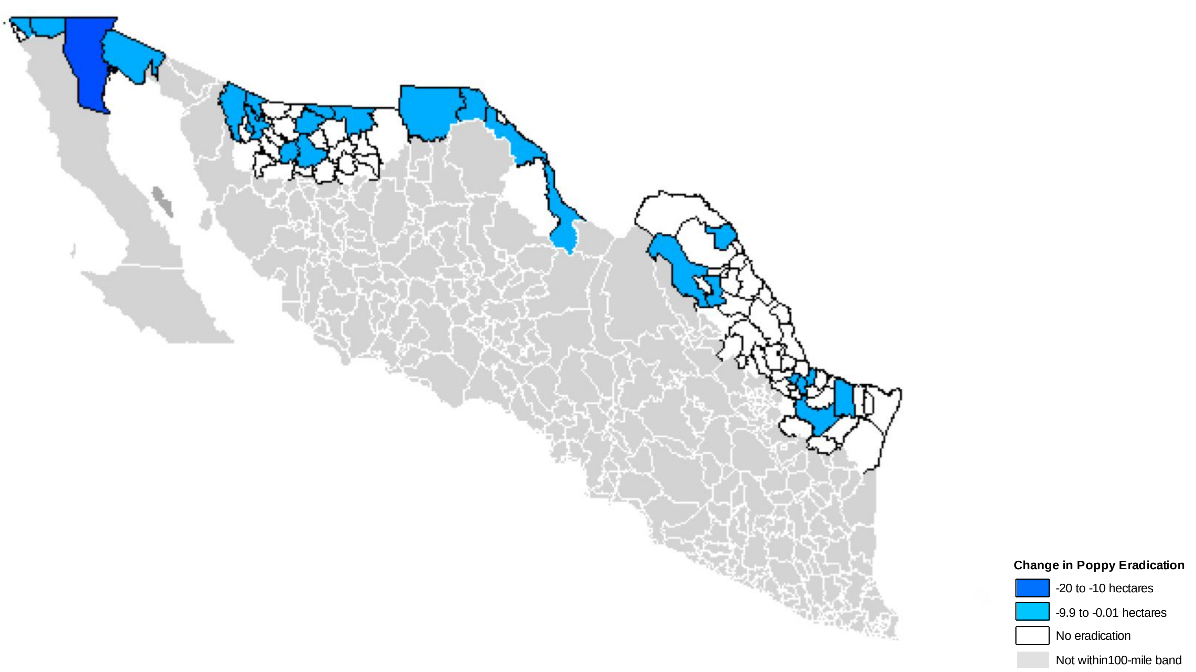
Notes. The solid blue line plots the Poisson regression coefficients for *Proximity NCA* interacted with each year regressed on the outcome (annual counts of homicides in Panel A, and counts of homicides specifically tied to Guns in Panel B). 2004 is the omitted category. Controls include municipio and quarter fixed effects and *Proximit Border* x *post*. Population is used as exposure. Municipio-cluster-robust standard errors are used to calculate the 95% confidence intervals indicated by dashed lines.

Figure IX
Changes in Drug Eradication in Mexican Municipios

<u>Panel A:</u> *Change in Hectares of Marijuana Eradicated – 2005-2006 versus 2002-2004*



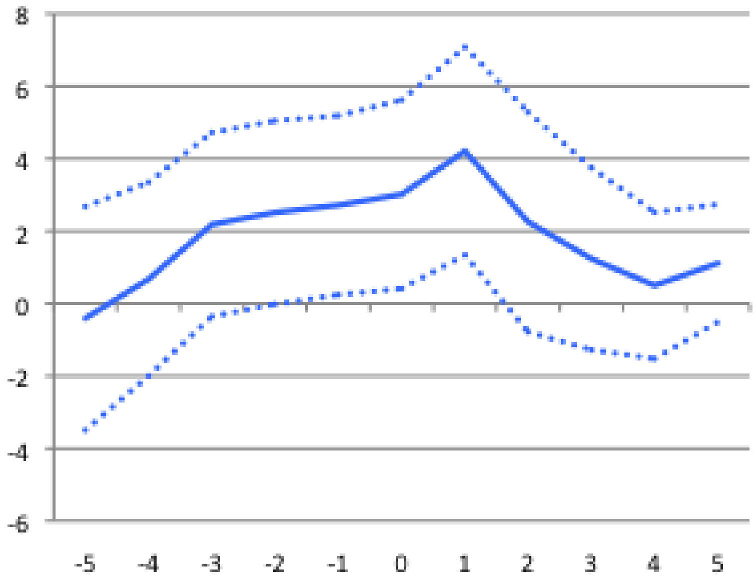
<u>Panel B:</u> *Change in Hectares of Poppy Eradicated – 2005-2006 versus 2002-2004*



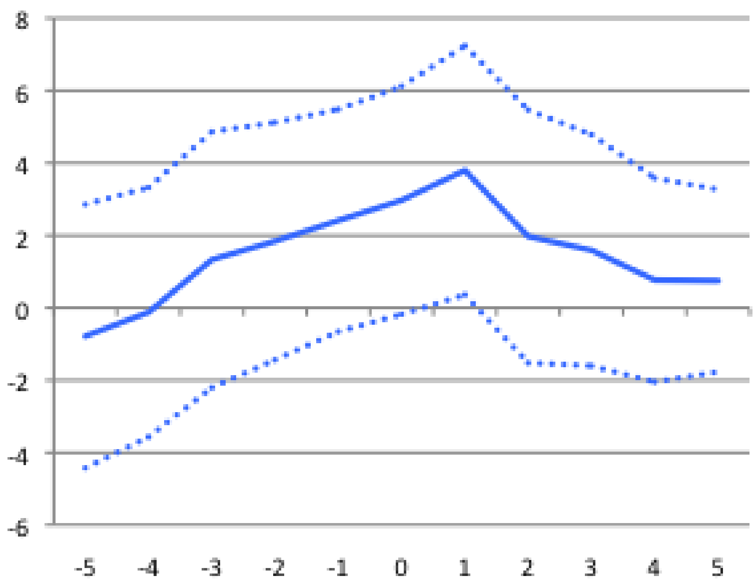
Notes. This figure shows the change in hectares of marijuana and poppy eradicated in each municipio between the pre-treatment period (2002-2004) and post-treatment period (2005-2006). White municipios experienced no change in average annual eradication between the pre and post periods. Red municipios experienced an increase in eradication. Blue municipios experienced a decrease in eradication. Darker colors indicate larger changes in eradication.

Figure X
Estimated Effects from Placebo Laws – Time Shifted by Quarter

<u>Panel A:</u> *All Homicides*



<u>Panel B:</u> *Gun-related Homicides*



Notes. The solid blue line plots the Poisson regression coefficients for *Proximity NCA* x *Post* regressed on the outcome (Quarterly counts of homicides in Panel A, and counts of homicides specifically tied to Guns in Panel B) using quarterly data. The quarter of placebo treatment is shifted by up to 5 quarters from Quarter 4 of 2004 forwards or backwards. The estimation sample is a symmetric 16 quarter window around the treatment. Controls include municipio and quarter fixed effects and *Proximity Border* x *Post*. Population is used as exposure. Municipio-cluster-robust standard errors are used to calculate the 95% confidence intervals indicated by dashed lines.