Online Appendix to "Cross-Border Spillover: U.S. Gun Laws and Violence in Mexico" (Not intended for publication)

Arindrajit Dube* Oeindrila Dube† Omar García-Ponce‡

March 27, 2013

This Online Appendix includes three sections. Section A provides background information on California's gun laws. Section B includes supplementary details on dataset construction. Section C presents additional results, containing a detailed discussion of Tables A.I (definition of ports), A.II (effects across demographic groups), A.III (robustness checks using the proximity specification), A.IV (robustness checks using the border segment specification), A.V (additional falsifications), A.VI (spatial confounds), A.VII (robustness to specific U.S. border states), and A.VIII (robustness to the definition of ports). We also include an in-depth discussion of Figure A.VI (effects by distance bands), but remaining appendix figures are referenced in the main paper.

*Assistant Professor, Department of Economics, University of Massachusetts Amherst. Thompson Hall. Amherst, MA 01003. Email: adube@econs.umass.edu.

†Assistant Professor, Department of Politics, New York University. 19 West 4th Street. New York, NY 10012-1119. Email: odube@nyu.edu.

‡PhD Candidate, Department of Politics, New York University. 19 West 4th Street. New York, NY 10012-1119. Email: garcia.ponce@nyu.edu.

A Background Information on California's Gun Laws

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.[1] 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. 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, AZ and NM. 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, and this control was retained in the post-2004 period. One piece of evidence indicating the relative ease of obtaining assault weapons in New Mexico, 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

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

ranked number 1 on this list, most recently with 81 points. Specifically with reference to assault weapons, California gets a 10 out of 10 in this category. In contrast, Arizona, New Mexico and Texas scored less than 10 points in total, earning zero each in the assault weapons category.[2] 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 Arizona, New Mexico and Texas (358) was less than half of the reverse flow (943).[3]

B Data Description

B.1 Gun Seizures

We analyze data on crime gun seizures from the Mexican military, the Secretariat of National Defense (SEDENA). These are defined as the number of guns seized in the campaign against drug-traffickers and in violation of Mexico's gun laws. This data presents a partial picture since the Office of the Mexican Attorney General (PGR) also seizes crime guns, but has not released the municipal level data. Since aggregate numbers by the Mexican presidency specify the total number of guns seized annually (Calderón, 2009), this allows us to establish that SEDENA accounts for approximately 30% of total gun seizures nation-wide during the sample period. State-level figures from PGR and SEDENA also show that SEDENA accounts for 23% of the seizures in Baja California (which is directly south of California), and 28% of the seizures in the other Mexican border states, indicating that both agencies operate across various parts of the border and the data are not systematically missing for any particular region, such as the area near California.

B.2 Port Classification and Proximity

Table A.I shows how we classify border crossings into 18 ports of entry. A border crossing is considered a separate port if it is at least 20 miles away from another major border crossing.

[2] http://www.bradycampaign.org/stategunlaws/, accessed on March 8, 2012.
[3] http://www.atf.gov/statistics/trace-data/

Otherwise, they are considered part of the same port, and named after the border crossing with higher annual average truck flows. Distance is calculated from the actual border crossing, rather than the center of the port city. We used straight-line distances for these calculations, but the port classification remains the same if driving distances are used instead. The 18 ports generated by this classification are: San Diego, Tecate and El Centro in CA; Yuma, Lukeville, Sasabe, Nogales, Naco and Douglas in AZ; Columbus in NM; and El Paso, Presidio, Del Rio, Eagle Pass, Laredo, Rio Grande City, McAllen and Brownsville in TX. In addition, we discern which border municipios have a major highway[4] (as shown in Figure III of the paper).

The primary measures of proximity used in the analysis are based on centroid-to-centroid distance between a given municipio and the nearest of the treatment ports. We use centroid distance because this is the best way of capturing average distance from a port to a municipio, which is of interest since our outcome variables are the number of killings throughout the municipality. However, for robustness, we also employ edge-based proximity, which measures the distance to the closest point along a municipio's boundary.

B.3 Effective Number of Political Parties

We use electoral data compiled by the Center of Research for Development (CIDAC) to construct four measures of the effective number of political parties.[5] These are computed using municipality-level party vote shares in mayoral elections. We focus on elections for mayors since they represent the highest-ranking executives at the municipal level, and thus, were commonly the office holders involved in negotiating arrangements with cartels. In addition, we are not able to use electoral data for higher level executives such as governors since we aim to utilize municipal variation in competitiveness.

Our primary measure is the canonical Laakso-Taagepera (LT) index, which is defined as $N_{LT} = \frac{1}{\sum_i s_i^2}$, where s_i is party i's vote share. N_{LT} takes the inverse of the Herfindahl-Hirschman index of concentration, and thus a higher value of the index implies a larger number of effective

[4]The GIS shapefile for Mexican highways in 2009 comes from http://www.mapcruzin.com/download-mexico-canada-us-transportaton-shapefile.htm.

[5]This dataset was compiled by CIDAC using primary information provided by the local electoral institutions.

parties, and greater competition.

For robustness, we also consider three other measures which each deal with some potential shortcomings of the LT index. Some researchers have argued that this index overstates the effective number of parties when there is one dominant party. For example, with the largest party receiving 2/3rd of votes, the lower bound of N_{LT} is 1.8, which is close the value for a 2-party system, and therefore may be problematic. There have been numerous attempts to address this problem, beginning with Molinar (1991), who defined an alternative index as $N_M = 1 + \frac{\left(\sum_i s_i^2\right) - s_1^2}{\left(\sum_i s_i^2\right)^2}$. However, the Molinar index sometimes doesn't satisfy one of the desirable properties of the original LT index, that increasing the number of smaller parties should register a greater degree of competition. The measure proposed by Dunleavy and Boucek (2003), defined as $N_{DB} = \left(\frac{1}{\sum_i s_i^2} + \frac{1}{s_1}\right) \times \frac{1}{2}$, has been found to perform better in dealing with both of these problems. Another alternative which also addresses the same issues is the Golosov index, defined as $N_G = \sum_i \frac{s_i}{s_i + s_1^2 + s_i^2}$, where s_1 is the largest party vote share.[6]

For a given year, all four indices are computed using data on the most recent mayoral election. However, to test for differential effects of FAWB policy changes based on the degree of electoral competition, we also define mean values of these indices in the respective pre-treatment sample periods. For the 2002-2006 period, this includes the 2004 elections since these took place prior to the expiration of the FAWB in September. Analogously, for the 1992-1996 period, this includes the 1994 elections, since these took place prior to the passage of the FAWB in September of that year.

B.4 Control Covariates

SEDENA data on the number of individuals detained by the Mexican military during drug war operations (scaled by population) provide us with an important measure of enforcement at the municipal level. On the U.S. side, the Federal Bureau of Investigation (FBI) Uniform Crime Reports provide information on the number of police officers stationed in each port (in per capita terms). This data is available at the city level for 11 ports, and we assign the county-

[6] See Golosov (2010) for a more in-depth discussion of the respective strengths and weaknesses of these indices.

level equivalent for Tecate, CA; Columbus, NM; Presidio and Rio Grande in TX; and Lukeville, Naco, and Sasabe in AZ.

DHS data also allows us to control for the number of undocumented immigrants apprehended in each border patrol sector. The border patrol sector is a DHS-defined geographic unit. Nine ports are uniquely assigned to one of these sectors. However, Douglas, Lukeville, Naco, Nogales, and Sasabe belong to the same sector (of Tucson). Likewise, Tecate, Columbus, and Eagle Pass are a part of the San Diego, El Paso, and Del Rio sectors, respectively. And, Rio Grande City, McAllen, and Brownsville are assigned to Rio Grande Valley's sector.

To account for the drug trade, we obtain SEDENA data on drugs seized by the military during drug-war operations in each municipio in Mexico, and U.S. county-level data on drugs seized by the El Paso Intelligence Center, a multi-agency center led by the U.S. Drug Enforcement Administration (DEA). 12 of the 18 ports are situated in different U.S. counties, and in these cases a unique county-level value of drug seizures is assigned to each port. However, San Diego and Tecate are both situated in San Diego County (CA); Lukeville and Sasabe belong to Pima County (AZ); and Naco and Douglas are part of Cochise County (AZ). Assigning drug values to the nearest port yields a municipio-level variable representing the value of drugs seized in the nearest port. For both types of seizures, we use international prices from the United Nations Office of Drugs and Crime to aggregate the value of the four major drugs traded across the two countries – marijuana, heroin, cocaine and methamphetamine.

Resa Nestares (2004) has also developed a municipal-level proxy measure of drug traffickers and offenders in Mexico. This is based on PGR reports of convictions of possession, sale and trafficking of drugs in the top 100 municipios involved in the drug trade, from 1998 to 2001—which precedes the period of our study. However, analysis of this measure demonstrates the extent to which classifying areas as cartel states on the basis of cartel leadership as in Chicoine (2011) yields a coarse grouping. For example, Baja California Sur, Nayarit and Durango are classified as non-cartel states prior to 2004 by the leadership base definition. But according to the Resa Nestares variable, these states rank 5th, 6th and 8th, respectively, of 32 states, in terms of the density of drug traffickers and offenders.

C Additional Results

C.1 Violence across Demographic Groups

In this subsection, we explore the heterogeneity of the estimated effect across demographic groups. If homicide increases are driven by members of crime syndicates targeting one other, the effects should be larger for deaths of young men from a lower socioeconomic stratum, as this is the demographic group most likely to be involved with drug cartels.[7] To explore this question, we disaggregate the counts of total homicides into sub-groups based on age, gender and educational attainment, which we use as a proxy for socioeconomic status.

We begin by re-generating municipio-level counts of killings for the observations that are not missing any one of these characteristics. Column (1) of Table A.II presents these effects, which are similar in magnitude to the baseline effects in Table II. The coefficients in columns (2)-(3) show that the treatment effects are substantially larger for the sub-group of individuals above the age of 18 who have not completed high school, relative to everyone else. Columns (4)-(5) shows that the same pattern holds for young men (between the ages of 18 and 30) who have not completed high school. Reassuringly for our interpretation, this is particularly true for gun-related homicides (Panel B). 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.

C.2 Robustness Checks

The results in this subsection test the robustness of our proximity-based specification (equation (2) in the main paper), to additional estimators, controls, and samples. Table A.III presents these results. Column (1) reproduces the baseline results from column (5) of Table II. Column (2) shows that the effects continue to be statistically significant with Negative Binomial estimation, although the coefficients are somewhat smaller in magnitude. As discussed in section

[7]Data from the Mexican presidency indicates that between 2007 and 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.

4 of the paper, the Poisson results are preferred as the consistency of the estimates do not rely on specific distributional assumptions (while cluster robust variance estimates deal with the potential overdispersion problem).

We next account for spatial linear trends in homicides by including an interaction of $proximityNCA$ with time in the specification. The maximum likelihood estimates do not converge with the inclusion of these trends along with municipio and year fixed effects. However, in column (3), we show that the coefficient is similar when we replace year effects with a post-2004 indicator, and in column (4) we include the linear trend control along with this indicator. The coefficients of interest are actually larger in magnitude in column (4) than in columns (3) or (1), and remain significant for both homicides and gun homicides, indicating that underlying trends do not confound the results.[8]

Since most guns are trafficked along major highways even once they reach Mexico, column (5) restricts the sample to those municipios that have at least one major highway. The coefficients are almost identical as the baseline, confirming that the results are not driven by some idiosyncratic feature of the few regions lacking highway access.

If a rise in homicides is correlated with factors that also promote other types of mortality, then our estimates may be biased upward if we do not control for these omitted factors or the ensuing increase in other deaths.[9] In column (6), we control for other non-homicide deaths, as well as non-gun related murders, and find that the results for homicides and gun-related homicides remain nearly identical.

In column (7), we address the alternative account that estimated violence increases reflect an increase in state enforcement efforts, such as government military operations, which are also potentially correlated with our treatment. We do so by controlling for the contemporaneous number of drug-related detentions per capita by the Mexican military. In addition, we account

[8]The estimates in column (4) are particularly large in comparison to those in column (3), and more similar in magnitude to those in column (1). The small size of the coefficients in (3) is likely to reflect the fact that time effects are very coarsely specified in this specification, relative to the specifications in both (1) and (4).

[9]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.

for differential enforcement levels across U.S. ports cities by controlling for the number of police officers per capita in the nearest port. Since enforcement controls are likely to respond positively to increased criminal activity induced by the policy change, including their contemporaneous values is a form of over-controlling, and represents a particularly tough hurdle. Even so, we find that the coefficients remain statistically significant at conventional levels and large in magnitude—especially for gun-related killings.

Finally, we explore the impact of the FAWB expiration on non-gun homicides. This expected effect is ambiguous. Added gun supply may have led to a substitution away from the use of other weapons, lowering these other types of homicides. On the other hand, it may have increased non-gun murders by expanding the drug war more generally, which has increasingly involved killings by other means such as beheadings and mutilations. Panel C of Table A.III shows the FAWB expiration did not affect non-gun homicides in any of the specifications, even while there is a strong impact on gun-related homicides (Panel B). This demonstrates that there was little substitution away from other types of murders and also provides additional validity to the causal channel proposed for our findings.

Next, we conduct the same robustness checks with the segment-based specification (equation (1) in the main paper). Panels A and B of Table A.IV show that the results are robust to all of these specifications. The coefficients fall with the inclusion of enforcement related variables (in column (7)), which, as discussed above, are potential over-controls. The effect for gun-related homicides becomes marginally insignificant in this specification (with a p-value of .11), but the effect for overall homicides remains statistically significant even with the inclusion of these controls. Panel C also confirms that there are no significant effects on non gun-related homicides with the segment-based specification.

C.3 Falsifications

Next, we present evidence on a few additional falsifications. If rising gun supply from the FAWB expiration led to an increase in violence associated with organized crime, then we should observe significant increases in homicides, but not suicides. Columns (1)-(3) of Table A.V show that the proximity interaction does not exert significant effects on suicides of any type, including those committed by guns. Column (4) also shows that there are no effects on accidents, which provides an additional check that *proximityNCA* $\times$ *post* is not spuriously correlated with other factors associated with rising deaths. These results support the idea that the rise in gun supply associated with this policy change led to a rise in murders, specifically, rather than other types of violent deaths.

C.4 Choice of Distance Cutoff

In choosing our municipal sample, we utilize a 100-mile cutoff, and exclude more distant municipios. Our identification strategy relies on distinguishing between overall proximity to the border and proximity to a non-California port of entry. These two variables—*proximity border* and *proximity NCA*—become highly correlated as we move further away from the border. For example, the correlation between the two variables is .11 in the 25-50 mile distance band, .89 in the 50-75 miles distance band, and 1 in the 75-100 mile distance band. In particular, the values of *proximity border* and *proximity NCA* are identical for all municipios that are more than 65 miles away from the border. This is a reason to not include more distant municipios in our sample, as they do not provide identifying variation except through assumptions about functional form. However, there is nothing unique about the 100 mile mark, and the effect, if robust, should continue to hold in samples with other distance cutoffs.

To demonstrate the insensitivity of our findings to alternative cutoffs, we re-estimate equation (2) starting with a 25 mile sample, and then expand the sample by increments of 25 miles, until we reach 500 miles. We plot the coefficients and 95% confidence intervals associated with each sample in Figure A.VI. Here, the horizontal axis represents the distance cutoff, so a value of 200 denotes estimates in the sample of municipios within 200 miles of the U.S.-Mexico border. The results indicate that for all cutoffs between 50 and 500 miles, the point estimates are sizable and statistically significant, indicating that our results are not sensitive to the use of the 100-mile cutoff. The exception to this is the 25-mile band, which is such a small cutoff that the sample includes only 17 municipios, and fails to include even 23 of the 38 municipios at the border. For all cutoffs between 50 and 500 miles, the coefficients also fall in magnitude as we expand the distances. This reduction could reflect a tapering off in the marginal effects farther away from the border. However, it could also reflect greater collinearity between overall proximity and proximity to non-California ports in the samples with more distant municipios, which tends to reduce the estimated effects.

C.5 Controlling for Spatial Confounds

In this subsection, we consider and rule out potential spatial confounds to our estimated effects. We measure distance to the centroid since the centroid is the most appropriate marker for capturing the average distance from a port city to the municipio. However, under this approach, larger municipios will have greater measured distance, relative to when distance is measured to the edge of the municipio. This raises the potential concern that size differences may influence estimated effects, if for example, crime rates rose disproportionately in larger municipios in the post-2004 period. We address this issue in two ways. First, we control for the interaction of area (in square kilometers) with the post indicator. These results, shown in column (1) of Table A.VI, indicate that the coefficients are larger with the area control. In column (2), we take the alternate approach of measuring distance to the municipal edge. In column (3), we combine the two approaches, employing edge distance while controlling for *area* × *post*. All of these specifications use our original 100 mile sample, which avoids any differences in estimates arising due to sample changes. Overall, the robustness of the effects on total and gun-related homicides to these alternate approaches demonstrate that size and measurement issues do not confound our estimates.

Next, we consider potential spatial spillovers in violence, which may serve as an alternative mechanism through which gun supply affects homicides. For example, if violence tends to diffuse outward, then the initial entry of assaults weapons may cause an increase in violence at border municipios, but subsequent violence increases may reflect the diffusion of initial violence, rather than the diffusion of the guns themselves.[10] To examine this account, for each municipio i, we create measures of the average homicides and gun-related homicides in the set of municipios spatially contiguous to i, and refer to these variables as *neighbor homicides* and *neighbor gun-related homicides,* respectively. Since such violence propagation takes place over time, to reduce the possibility of correlated errors, column (4) of Table A.VI presents estimates with the one period lag of the neighbor variables.

In general, consistency of the estimates from this spatial model requires the assumption that error terms are not spatially correlated across periods, i.e., $E(e_{it}, e_{jt'}) = 0$ for all $i \neq j, t \neq t'$. To the extent that these disturbances are positively spatially correlated, this will tend to upwardly bias the estimates of spatial spillovers. This is because the presence of the correlated e_{it} terms induce a correlation between neighbor and own homicides even though there is no causal relationship between the two variables. However, as shown in column (4), the coefficients for *lag neighbor homicides* and *lag neighbor gun-related homicides* are close to zero in magnitude and statistically insignificant, indicating that there is little evidence of such spillovers. As an auxiliary check, we also include the contemporaneous as well as lagged variables in column (5), and find the results unchanged. Overall, there is little indication of either spatial spillovers or spatially correlated disturbances for killings in our sample.

C.6 Influence of Specific U.S. Border States

In this subsection, we address potential confounds stemming from the influence of particular U.S. border states. We first address the concern that other violence-promoting shocks to particular municipios near TX, AZ and NM may confound our estimated effects. 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 Sinaloa cartel's leader led to an increase in violence in Nuevo Laredo (on the Texas border) in 2004.[11] To assess the sensitivity of our findings to such

[10] We thank a referee for making this point.

[11] There are also specific violent events that took place on the Mexican side of the California border. For example, the Gulf and Tijuana cartels ended their year-long alliance in January 2005, and the head of the

shocks, Table A.VII reports the estimates when we drop all the municipios from the sample that are closest to a TX port (columns 1 and 2), to a AZ port (columns 3 and 4), and to a NM port (columns 5 and 6). We show the results both with and without our full set of controls.

We find that our key results continue to hold even when we drop all municipios whose nearest port is along specific border states. Dropping the AZ and NM do not affect any of the results. Dropping the TX segment is a particularly tough test, since this eliminates 60% of our sample. Even so, the effect on gun-related homicide continues to be quantitatively large and statistically significant, although the total homicide results are just shy of statistical significance at conventional levels (p value = 0.101). Overall, this sensitivity analysis indicates that localized events do not drive our finding that the expiration of the FAWB led to an increase in killings.

One other concern regarding the influence of a particular state is potential endogeneity in CA's decision to retain its state-level ban on assault weapons (and in TX, AZ and NM's decisions to remain without such bans). The most plausible account is one in which restrictive state-level gun laws are passed in response to rising nearby violence. Yet, this cannot explain our findings for two reasons. First, it is unclear why there would be a differential spurt in violence right around 2004 owing to the endogeneity in state maintenance of their pre-2004 policies. Second, we observe violence increases near the non-CA states which decided not to pass a state ban, which also runs contrary to the account.

C.7 Robustness to the Definition of Entry Ports

Our proximity measure is based on defining major ports of entry on the U.S.-Mexico border. The primary definition employed in this paper consolidates border crossings within 20 miles of each other into single ports, but does not impose any traffic-related restrictions.

In Table A.VIII we show that our findings are not driven by the choice of port definitions. We consider alternative distance cutoff rules for assigning border crossings to the same port, and also impose two truck traffic criteria, of at least 1000 or 5000 trucks per year. These

Gulf cartel dispatched their deadly enforcers, "Los Zetas," to seize smuggling routes in Baja California from the Tijuana cartel (STRATFOR, 2005). But such violence promoting events would bias the estimated effects downward.

are meaningful restrictions since substantial traffic flows imply that these locations are major transportation hubs. We find quantitatively similar and statistically significant effects of the expiration of the FAWB on violence in all nine cases, demonstrating the robustness of our results to these alternative port definitions.

References

[1] Chicoine, Luke. 2011. "Exporting the Second Amendment: U.S. Assault Weapons and the Homicide Rate in Mexico." Working Paper. University of Notre Dame.

[2] Dunleavy, Patrick and Françoise Boucek. 2003. "Constructing the Number of Parties." *Party Politics* 9: 291-315.

[3] Golosov, Grigorii. 2010. "The Effective Number of Parties: A New Approach." *Party Politics* 16: 171-92.

[4] Laakso Markku and Rein Taagepera. 1979. "The 'Effective' Number of Parties: A Measure with Application to West Europe" *Comparative Political Studies* 12: 3-27.

[5] Molinar, Juan. 1991. "Counting the Number of Parties: An Alternative Index." *American Political Science Review* 85: 1383-91.

[6] Resa Nestares, Carlos. 2004. "El mapa de las drogas en México." El Comercio de Drogas Ilegales en México-Notas de Investigación.

[7] STRATFOR Global Intelligence. 2005. "Mexico: the New Kingpin's Rise." February 22.

Table A.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	*Tecate*
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	*Lukeville*
AZ	Pima	Sasabe	954	37 miles to Nogales	*Sasabe*
AZ	Santa Cruz	Nogales	257,796	37 miles to Sasabe	*Nogales*
AZ	Cochise	Naco	4,271	24 miles to Douglas	*Naco*
AZ	Cochise	Douglas	27,000	24 miles to Naco	*Douglas*
NM	Luna	Columbus	4,737	59 miles to Santa Teresa	*Columbus*
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 is computed from the actual border crossing point versus the city center. A border crossing is considered a separate port if it is at least 20 miles away from another border crossing. If two border crossings are less than 20 miles apart, they are considered part of the same port, named after the border crossing with higher truck traffic. For instance, Andrade is less than 20 miles from Yuma and is considered to be part of the Yuma port. Following the same criteria, Santa Teresa, Roma, and Progreso are considered parts of the El Paso, Rio Grande, and McAllen ports, respectively.

Table A.II
The FAWB Expiration and Violence across Demographic Groups

	(1)	(2)	(3)	(4)	(5)
			Panel A: Homicides		
Proximity NCA x post	3.991***	4.935***	3.833**	7.720***	3.341**
	(1.478)	(1.713)	(1.870)	(2.331)	(1.522)
Observations	409	381	364	259	399
			Panel B: Gun-related Homicides		
Proximity NCA x post	6.477***	10.940***	3.799	13.894***	5.108**
	(2.168)	(2.657)	(2.360)	(3.187)	(2.279)
Observations	384	312	334	200	374
Proximity border x post control?	Y	Y	Y	Y	Y
Income, immigration and drug controls?	Y	Y	Y	Y	Y
Sample	All	Aged 18+ w/o HS	All but 18+ w/o HS	Males 18-30 w/o HS	All but males 18-30 w/o HS

Notes. See Table II.

Table A.III
The FAWB Expiration and Violence: Robustness Checks using the Proximity Specification

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Panel A: Homicides			
Proximity NCA x post	4.688***	2.512**	2.936*	6.220***	4.833***	4.219**	3.447*
	(1.810)	(1.171)	(1.562)	(2.308)	(1.870)	(1.782)	(1.866)
Proximity border x post	0.844	-0.190	0.090	-0.105	1.816	1.566	1.119
	(1.648)	(1.827)	(1.645)	(2.151)	(1.702)	(1.846)	(1.667)
Non-gun homicides	-	-	-	-	-	0.007***	-
						(0.002)	
Non-homicide deaths	-	-	-	-	-	0.000	-
						(0.000)	
Observations	409	409	409	409	350	409	409
				Panel B: Gun-related Homicides			
Proximity NCA x post	6.835***	3.476**	4.285**	7.590**	6.999***	6.964***	4.573*
	(2.399)	(1.513)	(2.124)	(3.022)	(2.543)	(2.332)	(2.473)
Proximity border x post	-0.135	-2.172	-1.592	-0.466	1.527	-0.818	0.304
	(2.872)	(2.630)	(2.849)	(3.044)	(3.088)	(3.053)	(2.704)
Non-gun homicides	-	-	-	-	-	-0.001	-
						(0.002)	
Non-homicide deaths	-	-	-	-	-	0.000	-
						(0.000)	
Observations	384	384	384	384	335	384	384
				Panel C: Non-gun Homicides			
Proximity NCA x post	-0.601	.566	-0.587	1.691	-0.687	-	-0.519
	(1.631)	(2.147)	(1.285)	(2.067)	(1.697)		(1.787)
Proximity x post	2.014	1.589	2.109	-0.531	1.664	-	1.952
	(2.308)	(2.844)	(2.354)	(2.925)	(2.513)		(2.275)
Observations	312	312	312	312	287	-	312
Year fixed effects?	Y	Y	-	-	Y	Y	Y
Post-2004 indicator?	-	-	Y	Y	-	-	-
Linear time trends?	-	-	-	Y	-	-	-
Income, immigration and drug controls?	Y	Y	Y	Y	Y	Y	Y
Current enforcement controls?	-	-	-	-	-	-	Y
Sample	100-mile	100-mile	100-mile	100-mile	100-mile & highway	100-mile	100-mile
Estimator	Poisson	Negative Binomial	Poisson	Poisson	Poisson	Poisson	Poisson

Notes. Columns (1) and (3)-(7) are based on Poisson regressions. Column (2) is based on negative binomial regressions. Variables not shown include municipio fixed effects. Municipio-cluster-robust standard errors are in parentheses. Income, immigration and drug controls are those in Table II. All specifications include year effects except columns (3)-(4), which include a post-2004 indicator. Current enforcement controls in Column (7) include municipal military drug-war detentions per capita, as well as police officers per capita in the nearest U.S. port. *** is significant at the 1% level; ** is significant at the 5% level; and * is significant at the 10% level.

Table A.IV
The FAWB Expiration and Violence: Robustness Checks using the Segment Specification

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Panel A: Homicides				
Segement NCA x post	0.532**	0.344**	0.361*	0.687**	0.551**	0.518**	0.401*
	(0.231)	(0.140)	(0.210)	(0.312)	(0.240)	(0.218)	(0.226)
Non-gun homicides	-	-	-	-	-	0.008***	-
						(0.002)	
Non-homicide deaths	-	-	-	-	-	0.000	-
						(0.000)	
Observations	180	180	180	180	162	180	180
			Panel B: Gun-related Homicides				
Segment NCA x post	0.760**	0.403**	0.533*	1.006***	0.805**	0.758**	0.555
	(0.326)	(0.192)	(0.284)	(0.388)	(0.337)	(0.306)	(0.349)
Non-gun homicides	-	-	-	-	-	0.003	-
						(0.003)	
Non-homicide deaths	-	-	-	-	-	0.000	-
						(0.000)	
Observations	177	177	177	177	162	177	177
			Panel C: Non-gun Homicides				
Segment NCA x post	-0.073	0.181	-0.097	-0.182	-0.122	-	-0.082
	(0.200)	(0.211)	(0.186)	(0.300)	(0.196)		(0.209)
Observations	156	156	156	156	143	-	156
Year fixed effects?	Y	Y	-	-	Y	Y	Y
Post-2004 indicator?	-	-	Y	Y	-	-	-
Linear time trends?	-	-	-	Y	-	-	-
Income, immigration and drug controls?	Y	Y	Y	Y	Y	Y	Y
Current enforcement controls?	-	-	-	-	-	-	Y
Sample	100-mile	100-mile	100-mile	100-mile	100-mile & highway	100-mile	100-mile
Estimator	Poisson	Negative Binomial	Poisson	Poisson	Poisson	Poisson	Poisson

Notes. Columns (1) and (3)-(7) are based on Poisson regressions. Column (2) is based on negative binomial regressions. Variables not shown include municipio fixed effects. Municipio-cluster-robust standard errors are in parentheses. Income, immigration and drug controls are those in Table II. All specifications include year effects except columns (3)-(4), which include a post-2004 indicator. Current enforcement controls in Column (7) include municipal military drug-war detentions per capita, as well as police officers per capita in the nearest U.S. port. *** is significant at the 1% level; ** is significant at the 5% level; and * is significant at the 10% level.

Table A.V

Falsifications: The Effect of the FAWB Expiration on Suicides and Accidents

	(1)	(2)	(3)	(4)
	Suicides	*Gun Suicides*	*Non-gun Suicides*	*Accidents*
Proximity NCA x post	0.788	1.021	-0.096	0.426
	(1.331)	(4.967)	(5.058)	(0.554)
Observations	431	312	297	511
Proximity border x post control?	Y	Y	Y	Y
Income, immigration and drug controls?	Y	Y	Y	Y
Sample	100-mile	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. Income, immigration and drug controls are those used in columns (2) and (5) of Table II. *** is significant at the 1% level; ** is significant at the 5% level; and * is significant at the 10% level.

Table A.VI
The FAWB Expiration and Violence: Robustness to Spatial Confounds

	(1)	(2)	(3)	(4)	(5)
			Panel A: Homicides		
Proximity NCA x post	5.247***	3.284**	5.254***	4.743***	4.799**
	(1.848)	(1.461)	(1.772)	(1.913)	(1.920)
Lag neighbor homicides	-	-	-	0.002	0.002
				(0.005)	(0.005)
Neighbor homicides	-	-	-	-	0.002
					(0.005)
Observations	409	409	409	409	409
			Panel B: Gun-related Homicides		
Proximity NCA x post	8.261***	4.820**	7.877***	7.408***	7.441***
	(2.495)	(1.894)	(2.273)	(2.548)	(2.458)
Lag neighbor gun-related homicides	-	-	-	0.014	0.017
				(0.013)	(0.016)
Neighbor gun-related homicides	-	-	-	-	0.015
					(0.011)
Observations	384	384	384	384	384
Proximity border x post control?	Y	Y	Y	Y	Y
Income, immigration and drug controls?	Y	Y	Y	Y	Y
Area control?	Y	-	Y	-	-
Sample	100-mile	100-mile	100-mile	100-mile	100-mile
Distance measure	centroid	edge	edge	centroid	centroid

Notes. Variables not shown include municipio and year fixed effects. Robust standard errors clustered at the municipio level are shown in parentheses. Income, immigration and drug controls are those used in columns (2) and (5) of Table II. *** is significant at the 1% level; ** is significant at the 5% level; and * is significant at the 10% level.

Table A.VII

The FAWB Expiration and Violence: Robustness to Specific U.S. Border States

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Homicides					
Proximity NCA x post	2.067	1.547	4.408**	5.727**	4.145**	4.998***
	(1.261)	(2.135)	(2.215)	(2.311)	(1.920)	(1.701)
Observations	155	155	295	284	410	399
	Panel B: Gun-related Homicides					
Proximity NCA x post	3.906*	4.330*	4.784*	7.702**	4.673*	7.266***
	(2.157)	(2.261)	(2.807)	(3.264)	(2.409)	(2.247)
Observations	145	145	280	269	385	374
Proximity border x post control?	Y	Y	Y	Y	Y	Y
Income, immigration and drug controls?	-	Y	-	Y	-	Y
Sample	100-mile with nearest port in CA vs AZ, NM.	100-mile with nearest port in CA vs AZ, NM.	100-mile with nearest port in CA vs TX, NM.	100-mile with nearest port in CA vs TX, NM.	100-mile with nearest port in CA vs AZ, TX.	100-mile with nearest port in CA vs AZ, TX.

Notes. Variables not shown include municipio and year fixed effects. Robust standard errors clustered at the municipio level are shown in parentheses. Income, immigration and drug controls are those used in columns (2) and (5) of Table II. Columns (1) and (2) drop all municipios whose nearest port is along the border with TX; Columns (3) and (4) drop all municipios whose nearest port is along the border with AZ; Columns (5) and (6) drop all municipios whose nearest port is along the border with NM. *** is significant at the 1% level; ** is significant at the 5% level; and * is significant at the 10% level.

Table A.VIII

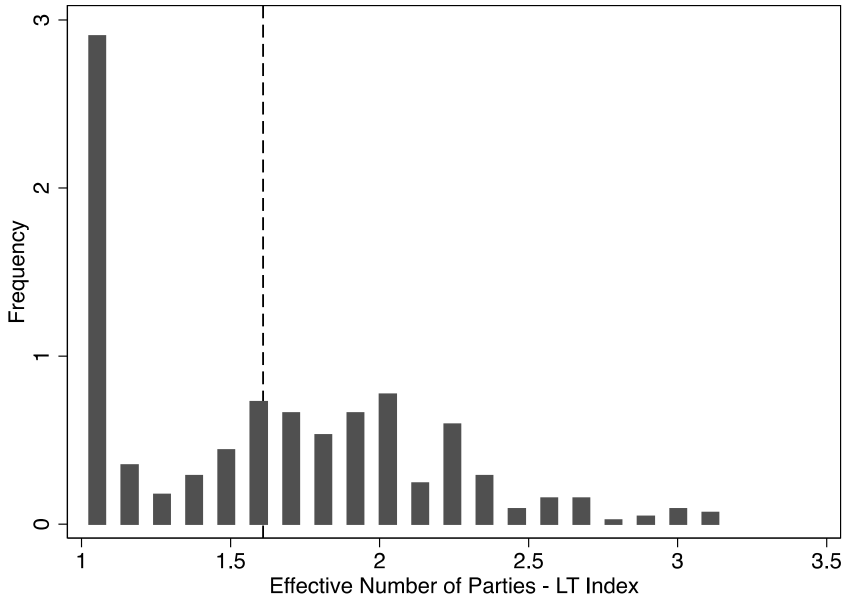
The FAWB Expiration and Violence: Robustness to Different Definitions of Entry Ports

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Panel A: Homicides				
Proximity NCA x post	3.776***	4.688***	4.390**	4.102***	4.782***	4.323**	4.395***	5.226***	4.611***
	(1.261)	(1.810)	(1.795)	(1.263)	(1.795)	(1.811)	(1.273)	(1.569)	(1.633)
Observations	454	409	409	439	394	394	434	389	389
					Panel B: Gun-related Homicides				
Proximity NCA x post	3.926**	6.835***	6.639***	3.855**	6.747***	6.313***	4.133**	7.085***	6.664***
	(1.778)	(2.399)	(2.415)	(1.783)	(2.378)	(2.436)	(1.925)	(2.100)	(2.239)
Observations	419	384	384	404	369	369	399	364	364
Proximity border x post control?	Y	Y	Y	Y	Y	Y	Y	Y	Y
Income, immigration and drug controls?	Y	Y	Y	Y	Y	Y	Y	Y	Y
Truck traffic criteria?	None	None	None	1000	1000	1000	5000	5000	5000
Distance to other crossing criteria?	None	20 miles	30 miles	None	20 miles	30 miles	None	20 miles	30 miles
Number of ports	22	18	16	20	16	14	17	14	13
Sample	100-mile	100-mile	100-mile	100-mile	100-mile	100-mile	100-mile	100-mile	100-mile

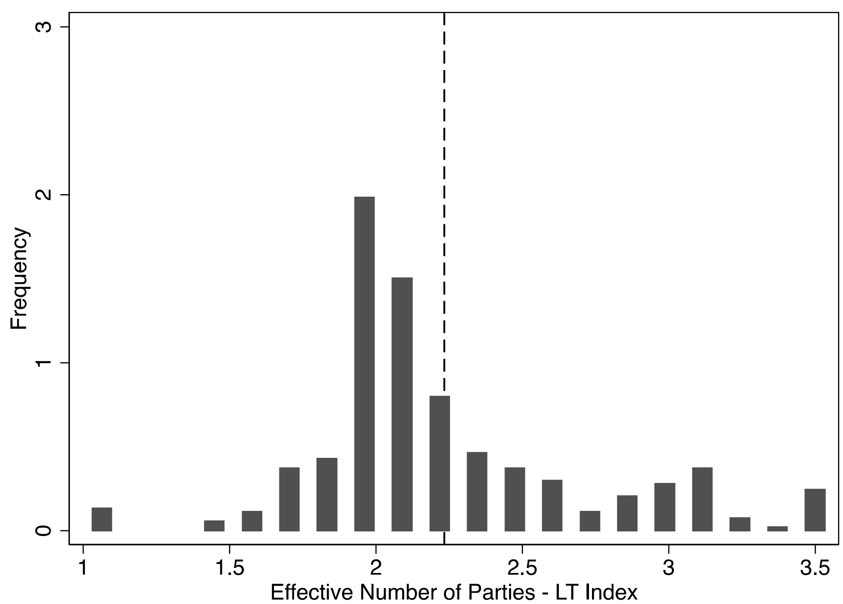
Notes. Variables not shown include municipio and year fixed effects. Robust standard errors clustered at the municipio level are shown in parentheses. Income, immigration and drug controls are those used in columns (2) and (5) of Table II. Columns (1)-(3) define ports of entry without any restrictions on truck flows; columns (4)-(6) require border crossings to have a truck flow of at least 1000 per year to be classified as ports of entry. Columns (7)-(9) require ports to have truck flows of at least 5000 per year. Columns (1), (4) and (7) do not place any restrictions on how far one bordering crossing is from another border crossing to be considered a separate port. Columns (2), (5) and (8) require two border crossings to be at least 20 miles apart to be classified as separate ports. Columns (3), (6) and (9) require border crossings to be at least 30 miles apart to be classified as separate ports. *** is significant at the 1% level; ** is significant at the 5% level; and * is significant at the 10% level.

Figure A.I
Political Competition Prior to the 1994 and 2004 Treatments

Panel A: *Prior to 1994 Treatment*



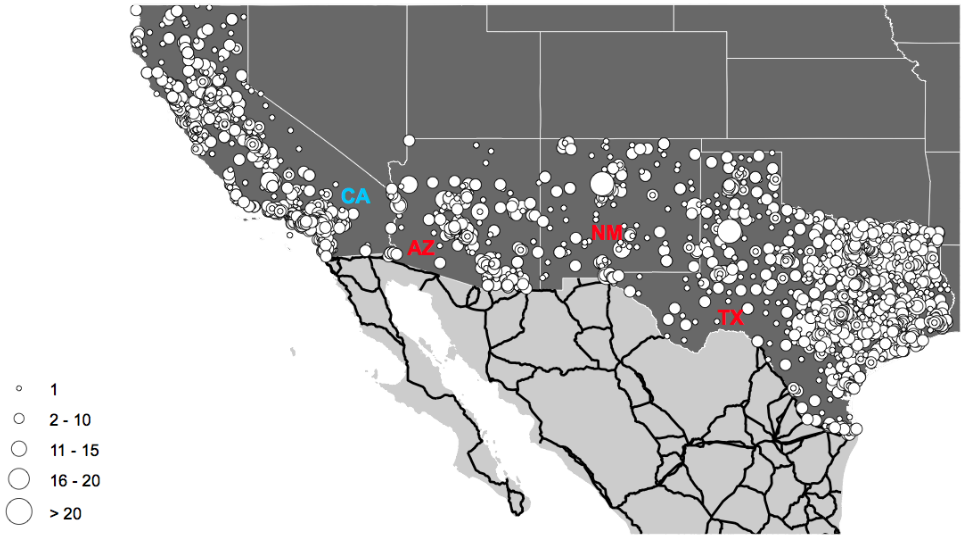
Panel B: *Prior to 2004 Treatment*



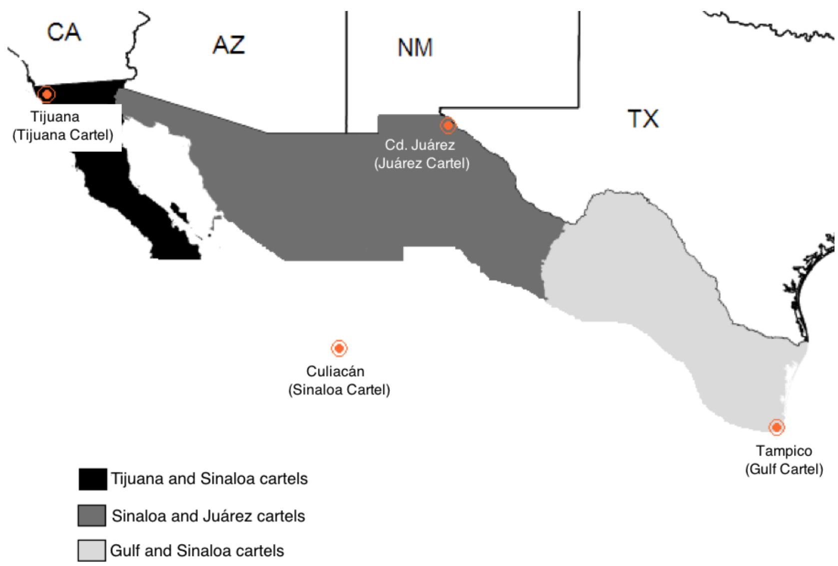
Notes. Panel A shows the distribution of the Laakso-Taagepera index of the effective number of political parties represented among mayoral elections, which resulted in the election of mayors within the 100-mile sample, during the sample period prior to the passage of the FAWB in September 1994. This includes mayors elected in the 1994 elections, which took place prior to September. Panel B shows the distribution of the same index among mayoral elections which resulted in the election of mayors within the 100-mile sample, during the sample period prior to the expiration of the FAWB in September 2004. This includes mayors elected in the 2004 elections, which also took place prior to September of that year. The dashed lines show the means of these indeces within these sub-samples.

Figure A.II
Firearms Dealers and Cartel Presence along the U.S. Mexico Border

Panel A: *Licensed Firearms Dealers in the Border States*



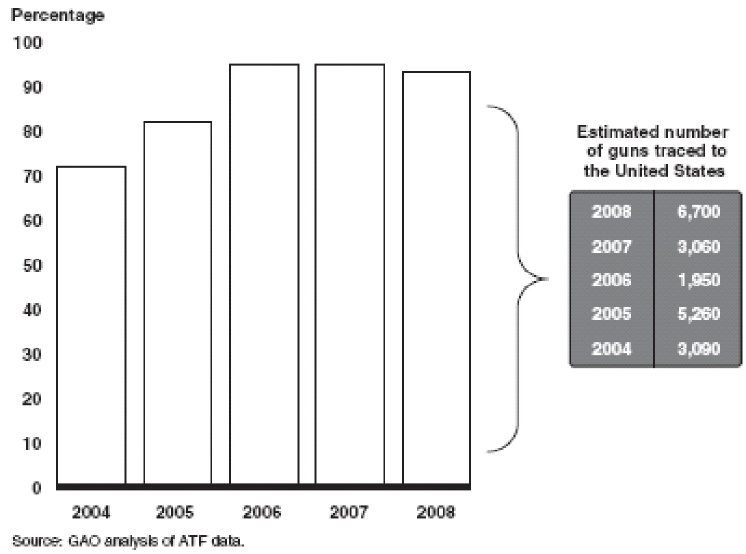
Panel B: *Cartel Presence along the U.S.-Mexico Border (2002-2006)*



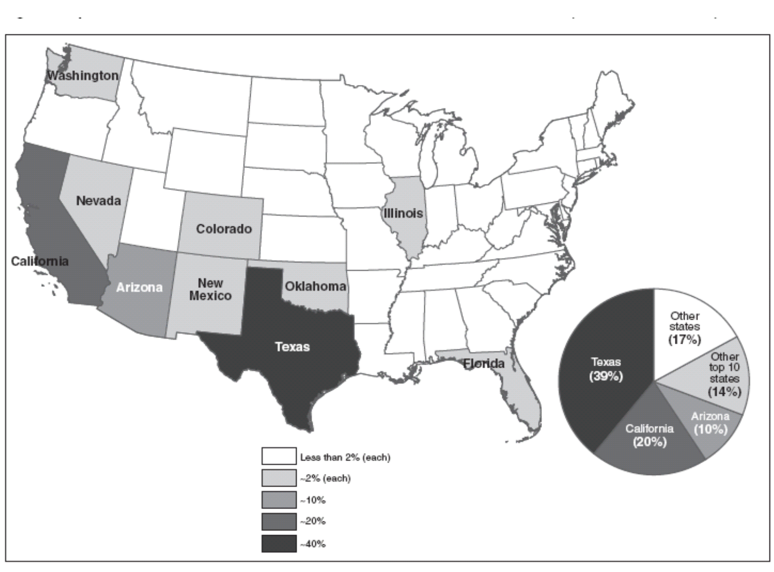
Notes. The map in Panel A uses data from the BATF to map graduated circles representing the number of licensed firearms dealers by ZIP code in California (CA), Arizona (AZ), New Mexico (NM) and Texas (TX) as of January 2010, the earliest date for which this information is publicly available. This data can be accessed from: http://www.atf.gov/about/foia/ffl-list.html/. The black lines represent highways in Mexico. Panel B shows the approximate geographic location of Mexican Cartels in border states over 2002-2006, based on information from Frías and Valdez (2002), Nájar (2005), CRS (2007), and STRATFOR Global Intelligence (2008). The shaded areas denote the areas in which various cartels operate. Circles with a dot inside represent the headquarter cities of each cartel, with the relevant cartel written in parentheses. The U.S. border states include California (CA), Arizona (AZ), New Mexico (NM) and Texas (TX).

Figure A.III
Mexican Crime Guns Traced to the United States

Panel A: *Overall Fraction Traced to U.S. Over Time*



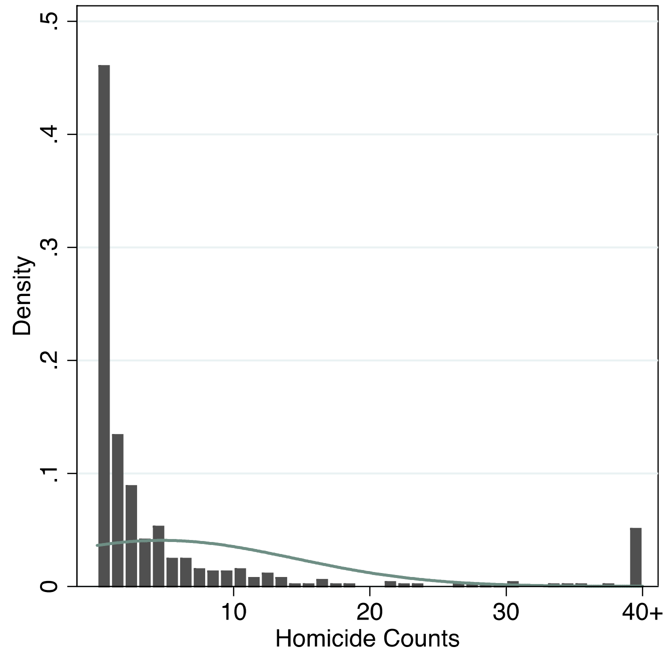
Panel B: *Source of Traced Guns – 2004-2008*



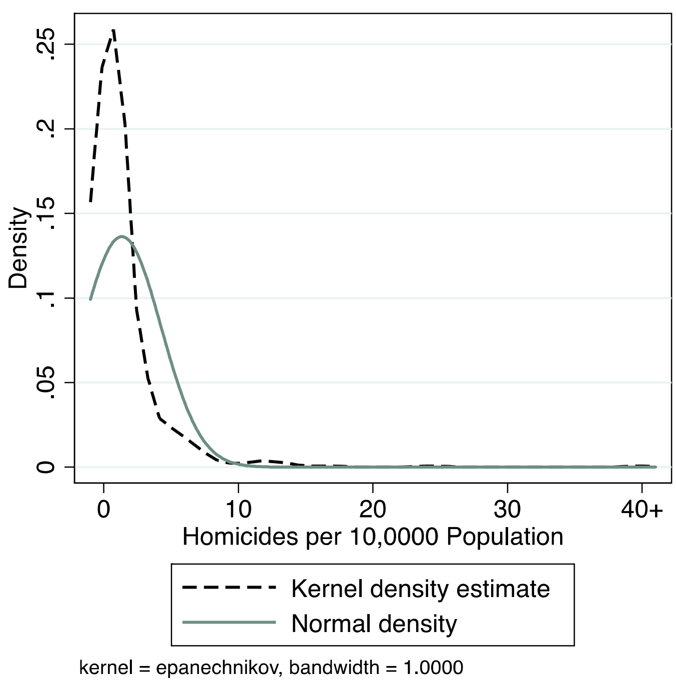
Notes. Both figures are from the GAO (2009) Report and based on BATF data. Mexican authorities send a quarter of seized firearms to BATF for tracing the location of the last legal transaction. The underlying data has not been made available to the public or researchers by BATF.

Figure A.IV
Distribution of Homicide Counts and Homicides per 10,000 Population

Panel A: *Homicide Counts*



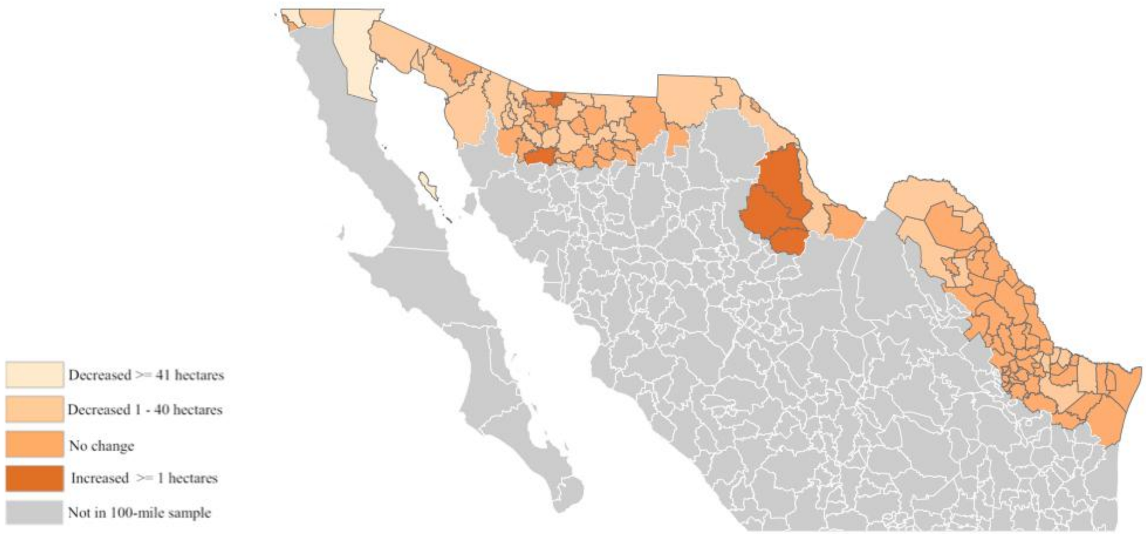
Panel B: *Homicides per 10,000 Population*



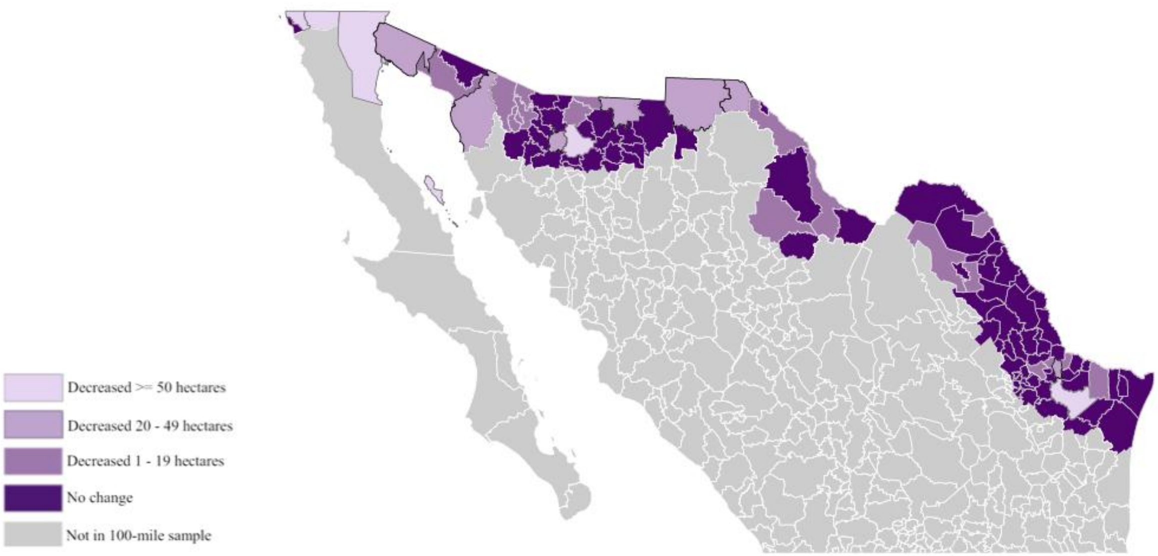
Notes. Panel A reports the histogram of homicide counts in the sample, along with a fitted Normal density, for the sample of municipios within 100 miles of a border port, over 2002-2006. Counts of 40 or more homicides are aggregated into the category "40". Panel B reports the kernel density estimate of homicides per 10,000 population in each municipio over the same time period, along with a fitted normal density estimate.

Figure A.V
Changes in Drug Eradication in Mexican Municipios

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

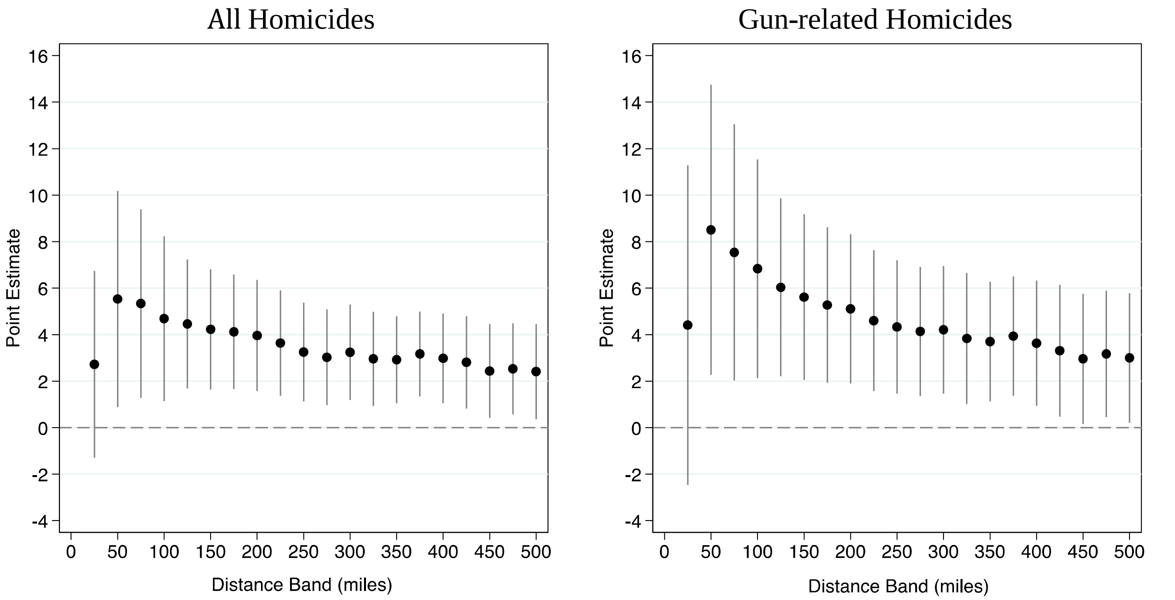


Panel B: *Change in Hectares of Poppy Eradicated – 2005-2006 versus 2002-2004*



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) for the set of municipios within 100 miles of an entry port, the 100-mile sample. Lighter colors indicate larger *decreases* in eradication over this period.

Figure A.VI
Effects on Violence by Distance Bands



Notes. Black dots plot the Poisson regression coefficients for *Proximity NCA* x *Post* regressed on the outcome (annual counts of homicides in the left panel, and counts of gun-related homicides in the right panel), restricting the sample to the set of municipios that lie within the designated distance bands. Controls include municipio and year fixed effects; *Proximity Border* x *post*; and the income, immigration and drug controls used in columns (2) and (5) of Table II. Population is used as exposure. Municipio-cluster-robust standard errors are used to calculate the 95% confidence intervals indicated by vertical bars.