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## Did the Military Interventions in the Mexican Drug War Increase Violence?

Valeria ESPINOSA and Donald B. RUBIN

We analyze publicly available data to estimate the causal effects of military interventions on the homicide rates in certain problematic regions in Mexico. We use the Rubin causal model to compare the post-intervention homicide rate in each intervened region to the hypothetical homicide rate for that same year had the military intervention not taken place. Because the effect of a military intervention is not confined to the municipality subject to the intervention, a nonstandard definition of units is necessary to estimate the causal effect of the intervention under the standard no-interference assumption of stable-unit treatment value assumption (SUTVA). Donor pools are created for each missing potential outcome under no intervention, thereby allowing for the estimation of unit-level causal effects. A multiple imputation approach accounts for uncertainty about the missing potential outcomes.

**KEY WORDS:** Causal inference; Donor pools; Matching; Rubin causal model.

### 1. INTRODUCTION

The Mexican presidency of Felipe Calderón (2006–2012) was characterized by its war against organized crime, and raised many questions regarding security and violence. It is estimated that during this period, the war claimed 60,000 lives (CNN 2012). Mexico has 31 states and a Federal District, which are further partitioned into municipalities and delegations, respectively. Throughout this document, we will refer to all 32 geographical entities as states and their political subdivisions as municipalities.

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Color versions of one or more of the figures in the article can be found online at [www.tandfonline.com/r/tas](http://www.tandfonline.com/r/tas).

In 2011, two articles were published in a leading Mexican magazine, *Nexos*, on the effect of the military interventions on civilian homicides. Through visual comparisons, but no formal statistical analysis, Escalante (2011) explored the possibility that military interventions had increased the homicide rates in those states where the interventions took place; later, Merino (2011) reached a similar conclusion.

In both articles, the question of interest is whether the military interventions increased the homicide rate in states where interventions took place, beyond what the homicide rate would have been without the interventions. For example, consider the state of Chihuahua, where more than one military intervention occurred; both articles attempt to compare the post-intervention homicide rate in Chihuahua to the counterfactual homicide rate in Chihuahua during the same time period had the state not experienced any military intervention. In the Rubin causal model (RCM; Rubin 1974), these homicide rates correspond to two potential outcomes for each state, one under active treatment (at least one military intervention),  $HR_{\text{state}}(1)$ , and one under control (no military intervention),  $HR_{\text{state}}(0)$ . The fundamental problem of causal inference is that only one of these potential outcomes is observed and therefore, implicitly or explicitly, the other missing potential outcome must be imputed to draw causal inferences. We use the RCM and propensity scores to analyze this question for all “regions,” defined to be collections of municipalities within a state, exposed to interventions.

Propensity scores (Rosenbaum and Rubin 1983) are used to create subsets of treated and control units with similar covariate values, here, regions with similar characteristics. The propensity score is a function of the covariates with the property that units with the same value of the propensity score will have, in expectation, the same values on covariates used to define, or to estimate, the propensity score. When covariate values in a subset of treated and control units are similar, we say that there is *covariate balance* in that subset. Within such subsets, the observed outcomes of the control units can be used to impute the missing control outcomes for the treated units, and the observed outcomes of the treated units can be used to impute the missing treated outcomes of the control units. The main goal of propensity scores is to achieve covariate balance. For any particular dataset, this balance should be checked to verify that we have created an appropriately balanced design.

Merino (2011) presented an analysis using only one covariate: pre-intervention homicide rate, but no assessment of balance.

Moreover, it is not obvious how to define pre-intervention homicide rate for the control units because the military interventions took place at varying points in time, and Merino (2011) did not clarify how this issue was addressed. The use of only one covariate makes the plausibility of unconfoundedness (see Section 3.2)—a critical assumption for causal inference—dubious.

Both Escalante (2011) and Merino (2011) used the state as the unit of analysis and both base their conclusions on averages across states. Merino (2011) also mentioned using the municipality as the unit but did not report results. We believe that neither of these definitions of units is appropriate, because although military operations tend to be directed at municipalities rather than entire states, it is reasonable to assume that a military intervention focused on a particular municipality also has an effect on its neighboring municipalities. To capture the effect on the municipality that was directly intervened as well as its neighbors, we define the unit of analysis to be the municipality that received the intervention together with its immediate neighbors—we call this collection of municipalities *a region*. This definition of the unit of analysis addresses some aspects of possible *interference* between municipalities, which can complicate a causal analysis. To impute the missing non-intervention potential outcome for each treated region, we create collections of non-intervened municipalities, where each collection is similar in certain background characteristics to the treated region, as described in Section 3.3. In addition to reporting estimates of the average effect across the regions, we report estimates of the causal effect within each region, allowing the analyst to compare the estimated effectiveness of interventions among specific regions of interest.

In Section 2, we discuss the covariates, the data sources, and the role that subject-matter experts played in the design of our study. Section 3 focuses on design issues including the definition of estimands, key assumptions, and the assessment of covariate balance. Section 4 introduces the estimation procedure, and Section 5 outlines the results. Section 6 contains conclusions and future steps.

## 2. DATA

We used publicly available data from three well-known sources: (i) the National Institute of Statistics and Geography (INEGI), (ii) the Center of Research for Development (CIDAC), and (iii) the Presidency website (PWS). The data from the third source were available until the presidential transition in December of 2012.

### 2.1 Covariates

There are 2456 municipalities in Mexico (INEGI, nd). For each of these municipalities, we have covariates related to demographics, economics, location, education, health, politics, and roads. We also considered as relevant three state-level variables. All covariates are listed in Table 1, together with their sources; INEGI was the source of most of the covariates. Escalante (2011) and Merino (2011) also used INEGI's homicide data. Merino (2011) compared the results obtained with INEGI's data—based on death certificates—to those obtained with the National Sys-

Table 1. Covariates used in the matching procedure.

Type	Municipality level	Source	State level	Source
Demographics	Homicide rate 2006*	INEGI	Homicide rate 2006*	INEGI
	<b>Indicator of 2006 homicide rate above the mean**</b>	INEGI		
	Population 2005	INEGI	Population 2005	INEGI
	Criminal-rivalry-related death count* (Dec 2006, Jan, Feb, Mar, Apr 2007)	PWS		
Economics	Government spending 2006	INEGI	GDP 2006	INEGI
Location	Latitude	INEGI		
	Longitude	INEGI		
Education	Average years of schooling	INEGI		
	Proportion that can read and write (and proportion of unknown)	INEGI		
	Proportion that speaks an indigenous language (and proportion of unknown)	INEGI		
Health	Number of doctors per medical unit	INEGI		
Politics	<b>Missingness indicator for the above**</b>	INEGI		
	<b>Political party in power at the end of 2006***</b>	CIDAC		
Roads	Total road length	INEGI		
	<b>Missingness indicator of above**</b>	INEGI		

Covariates in bold were exactly matched. Covariates marked with \*\* are binary and the one marked with \*\*\* is categorical. Homicide counts marked with \* were transformed to homicide rates using the 2005 population information

tem for Public Security—based on preliminary investigations of public ministries. In a separate analysis, he also used the federal government database (PWS), which attempts to count the homicides associated with organized crime. We used the PWS only to obtain pre-intervention values as background covariates.

Due to security concerns, certain covariates were not publicly available; examples include smuggling routes, drug crop locations, and the specific cartel(s) present in each region. Graphical information about cartel location after 2006 can be found online in maps created by Stratfor (2012). Coscia and Rios (2012) made a serious attempt to track the cartels at the municipality level based on extracting information (i.e., web scraping) from Google News. We decided not to include these data because they are not publicly available. We believe that obtaining these variables in a reliable and time-relevant form would improve the quality of a study such as ours.

### 2.2 Treatment Indicator and Outcomes

The treatment indicator,  $W$ , identifies whether a municipality received a military intervention ( $W = 1$ ) or not ( $W = 0$ ). The

encoding of this variable was based on the military interventions listed in Escalante (2011), which was obtained as a result of a web search of press releases and “comunicados” of the Office of the Secretary of Defense (SEDENA) giving the exact location of the event.

We attempt to assess whether military interventions increased violence measured by homicide rates per 100,000 inhabitants. Hence, our main outcome variable is the homicide rate one year after the military intervention. The homicide rate per 100,000 inhabitants is a standard measure of violence. For example, in 2011 the homicide rate in Mexico was 22.8, and the corresponding rates for Brazil, Colombia, Honduras, and the United States were 23.4, 33.6, 91.4, and 4.7, respectively (United Nations Office on Drugs and Crime 2013). Nevertheless, this average measure can hide great annual variations within a country: the homicide rate in Juárez, México was 147.77, in Marceió, Brazil it was 135.26, in Cali, Colombia it was 77.90, in San Pedro Sula, Honduras it was 158.87, and in New Orleans, USA it was 57.88 (Wikipedia 2014)

## 2.3 Subject Matter Experts

Rubin (2008) emphasized the importance of subject matter knowledge in the outcome-free design of a causal study. We discussed the project with three experts in the field: Viridiana Ríos and Elisa de Anda, who worked on these topics as graduate students at Harvard University, and Miguel Basañez. For example, they suggested including the covariate *political party in office at the end of 2006*, for each municipality. However, they were not otherwise actively involved in the design process.

## 3. DESIGN

Randomized experiments are considered the gold standard for obtaining objective causal inferences. However, in this setting, a randomized experiment is not possible, and an observational study is the only way to assess the question of interest. In accordance with the RCM, this observational study can be viewed as a broken randomized experiment because the assignment of the treatment (military intervention) was not randomized but occurred following some political process. Because the Mexican government had limited resources, military interventions were not sent to every municipality facing drug cartel-related violence, but to those municipalities deemed of highest priority. Therefore, we expect differences in the distributions of background covariates for the intervened and non-intervened municipalities. Our matching method attempts to find subsets of these municipalities with similar values on the key covariates. Following Rubin (2007, 2008), we attempt to approximate an experiment as closely as possible by separating the design phase from the analysis phase, where the key distinction is that in the design phase all outcome data are absent. Therefore, in the design of this study we only used data from 2006 or earlier because all interventions occurred after 2006, and any post-2006 variables are possibly affected by an intervention or its absence.

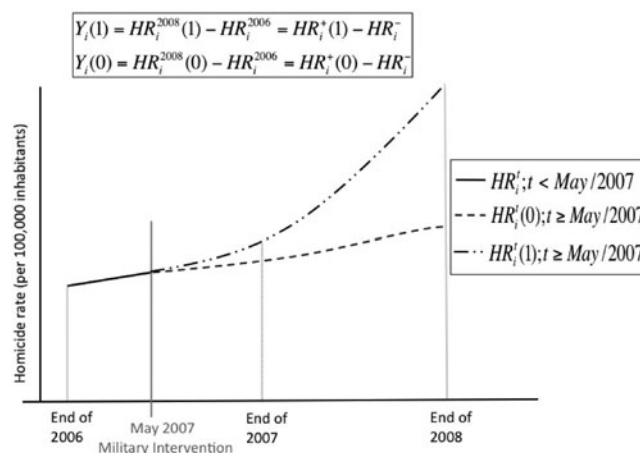


Figure 1. Sketch of the definition of the potential outcomes. For each region  $i$ ,  $HR_i$  denotes the homicide rate and  $Y_i$  denotes the post-intervention minus pre-intervention difference in homicide rate. The solid line corresponds to the pre-intervention homicide rates, and the upper and lower dashed lines correspond to the homicide rates with and without the military intervention, respectively.

## 3.1 Treated Units, Potential Outcomes, and Estimands

Following Escalante (2011), let a military intervention be a confrontation between army and organized crime that resulted in at least three civilian deaths (where civilian could refer to a member of a cartel); these deaths are not part of the outcomes, which count deaths that occurred the following year (see Figure 1). We define the treated units (i.e., the treated regions) as the groups of contiguous municipalities where at least one municipality received a direct military intervention between 2007 and 2010, and the time of treatment as the first time a municipality in the region received an intervention. Note that this definition of treatment is different from “sending military forces” to a municipality, which would be the ideal definition for this topic. However, a comprehensive list of such events is classified. Nonetheless, the use of the list given in Escalante (2011) is appealing because we use the same definition of intervened municipalities that the original *Nexos* article did, even though they focused on comparisons at the state level.

We consider 18 regions. We labeled each after one of its main municipalities, that is: Tijuana, Nogales, Madera, Juárez, Pánuco, Reynosa, Bustamante, Guadalupe, Villa de Cos, Teúl, Rincón de Romos, Sinaloa, Tepic, La Piedad, Celaya, Apatzingán, Coahuylana, and Acapulco. Numbering these from 1 to 18, let  $HR_i^t(W_i)$  denote the homicide rate for treated region  $i$  at the end of year  $t$  under treatment level  $W_i$ , where  $W_i = 0, 1$  ( $W_i = 1$  for treated regions), and let  $+$  generically denote “one year after” the military intervention. We are interested in comparing the observed homicide rate in region  $i$  one year after intervention,  $HR_i^+(1)$ , to the homicide rate in the same year had it not received the intervention,  $HR_i^+(0)$ . In other words, the estimands of interest are  $HR_i^+(1) - HR_i^+(0)$ , at the region level, and the average across all treated regions,  $\tau = \frac{\sum_{i=1}^N HR_i^+(1) - HR_i^+(0)}{N}$ , where  $N$  denotes the total number of treated regions. Because we did not observe any of the potential outcomes for these regions when not receiving a military



intervention,  $HR_i^+(0)$  is missing for every  $i$  and needs to be estimated, that is, imputed.

Let  $N_i$  denote the number of municipalities that form region  $i$ , and  $Pop_{ij}^+(W_i)$  and  $H_{ij}^+(W_i)$  the population size and number of homicides of the  $j$ th municipality in the  $i$ th region one year after receiving treatment level  $W_i$ . The potential outcomes can be expressed as

$$HR_i^+(1) = \frac{\sum_{j=1}^{N_i} H_{ij}^+(1)}{\sum_{j=1}^{N_i} Pop_{ij}^+(1)}, \text{ and } HR_i^+(0) = \frac{\sum_{j=1}^{N_i} H_{ij}^+(0)}{\sum_{j=1}^{N_i} Pop_{ij}^+(0)},$$

where  $Pop_i^+(W_i) = \sum_j^{N_i} Pop_{ij}^+(W_i)$  denotes the total population one year after region  $i$  receives treatment  $W_i$ .

Analogous to  $HR^+$ , let  $HR_i^-$  denote the homicide rate for region  $i$  one year before treatment is received, therefore being the same when  $W_i = 1$  or  $0$ . Let  $Y_i(1) = HR_i^+(1) - HR_i^-$  represent the difference in the homicide rate one year after the intervention and one year before the intervention. Similarly, let  $Y_i(0) = HR_i^+(0) - HR_i^-$  denote the difference in homicide rates for those same years without the intervention (see Figure 1 for a sketch of the definition of these modified potential outcomes). Note that the estimands defined using  $Y$  are identical to the estimands previously defined using  $HR$  because  $Y_i(1) - Y_i(0) = HR_i^+(1) - HR_i^+(0)$ . Thus,

$$\begin{aligned} \tau &= \frac{\sum_{i=1}^N HR_i^+(1) - HR_i^+(0)}{N} \\ &= \frac{\sum_{i=1}^N Y_i(1) - Y_i(0)}{N} = \bar{Y}(1) - \bar{Y}(0). \end{aligned}$$

### 3.2 Key Assumptions: SUTVA and Unconfoundedness

We make two key assumptions: *the stable-unit treatment value assumption*, SUTVA (Rubin 1980)—which is implicitly made in the notation introduced in the previous section—and unconfoundedness. SUTVA is fundamental in this formulation of the problem, and unconfoundedness is crucial for understanding the role of background covariates.

SUTVA comprises two parts: no hidden versions of treatments and no interference between units, that is, each unit's potential outcomes are functions of the unit label,  $i$ , and the treatment the unit received,  $W_i$ . *No hidden versions of treatments* means that there is an unambiguous definition of both treatments, thereby allowing each unit to be clearly identified as intervened or not. *No interference* means that no unit's treatment assignment has an effect on the potential outcomes of other units. It is reasonable to assume that the effect of a military intervention on a particular *municipality* would spillover to neighboring municipalities; hence, we grouped municipalities that received an intervention with their immediate neighbors to form treated regions for which “no interference” is more plausible and we proceed assuming that it holds. This definition may force treated regions to include more than one municipality that is directly intervened. A more ambitious approach could consider this as a case of a treatment with multiple levels. However, in this study, we aim to provide a simple analytical approach by providing a

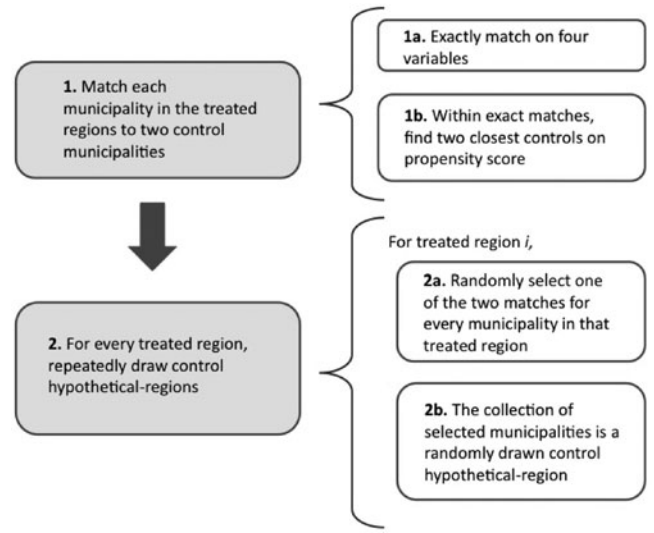


Figure 2. Flow chart of matching method.

definition of a binary treatment that is broad enough to classify all treated regions into the same treatment level.

To explain the unconfoundedness assumption, let  $\mathbf{X}$  denote the matrix of covariates, where the columns correspond to background covariates (e.g., those variables listed in Table 1 and relevant transformations), and row  $i$  ( $\mathbf{X}_i$ ) contains region  $i$ 's covariate values. Similarly, let  $\mathbf{Y}$  denote the  $N \times 2$  matrix where each row corresponds to the two potential outcomes for each region. Unconfoundedness means that, given  $\mathbf{X}$ , treatment assignment is functionally independent of  $\mathbf{Y}$ . If unconfoundedness holds, a formal causal interpretation of the results is straightforward. Section 3.5 assesses the credibility of the unconfoundedness assumption for the available set of covariates. As mentioned previously, there are unavailable covariates that might improve the plausibility of unconfoundedness in our study.

### 3.3 Matching Method

The goal of our matching method is to create collections of control municipalities that resemble the treated regions with respect to the background covariates in Table 1. We refer to each of these collections as a control hypothetical-region, because by definition each collection is composed of control municipalities that in general are not adjacent, and thus do not literally comprise a region. From the pool of 2208 control municipalities, we used a combination of exact and propensity score matching to find two control matches of comparable quality for each of the 248 municipalities forming the treated regions (Step 1 in Figure 2). For treated region  $i$ , a control hypothetical-region is formed by randomly selecting one of the two matches found for each municipality forming the  $i$ th treated region. A donor pool of control hypothetical-regions corresponding to region  $i$  is created by repeating this process multiple times (Step 2 in Figure 2).

In Step 1a of the flow chart shown in Figure 2, we exactly matched on four variables: political party in office in 2006, the indicator for whether the 2006 homicide rate was above the national mean, the missingness indicator for the number of doctors per medical unit in 2005, and the missingness indicator

for total road length in the municipality (i.e., 1 if missing and 0 otherwise). The only covariates with missing data were doctors per medical unit in 2005 and total road length, and because we believe that such missingness gives us relevant information (e.g., these municipalities are harder to reach), we decided to match on the missing data pattern (D'Agostino and Rubin 2000). In Step 1b, for each municipality within a treated region, this exact matching defined a set of control municipalities from which the two closest on an estimated propensity score were selected. The propensity score was estimated using linear terms of the variables in Table 1 not used for exact matching, second-order interactions involving the 2006 homicide rate at the municipality level, the natural logarithm and square transformations of 2006 homicide rates at both the state and municipality levels, and transformations of covariates that are based on fractions (i.e., logit and proportion equal to zero). The final propensity score was chosen by assessing the quality of balance achieved. The matching was done without replacement and in decreasing order of estimated propensity score using the R package *MatchIt* (Ho et al. 2011).

### 3.4 Balance Assessment

To draw causal inferences from observational data, and to avoid relying on model-based extrapolation, which is implicit in common linear adjustment methods, it is critical for reliable answers to assess balance on key covariates before examining any outcomes (Rubin 2007, 2008; Cangul et al. 2009), just as is done in randomized experiments. This message is quite old (e.g., Cochran and Rubin 1973; Rubin 1973, 1979). When reasonable balance has been achieved, the design stage of the study ends and the analysis stage begins.

Balance was assessed at the same level that the matching was done: the municipality level. "Love plots" (Ahmed et al. 2006) and histograms are useful tools to evaluate balance. In this context, Love plots compare the differences in covariate means between the municipalities in treated regions and the control regions before matching (i.e., all the municipalities in Mexico that were not in any treated region) and after matching (i.e., the subset of municipalities that were matched to those municipalities forming treated regions). The comparisons for binary covariates consider the simple differences in proportions, whereas the comparison of each continuous covariate considers the difference in means standardized by the usual estimate of the pre-matching variance of that difference; the before matching value is the same as the usual *t*-statistic, whereas the post-matching numerator has changed, but the denominator is the same before and after matching. Smaller differences indicate better balance; in a Love plot, this is represented by the proximity of the symbols to the vertical line at zero, which denotes no difference in the average values of the covariates between municipalities in the treated regions and the set of controls.

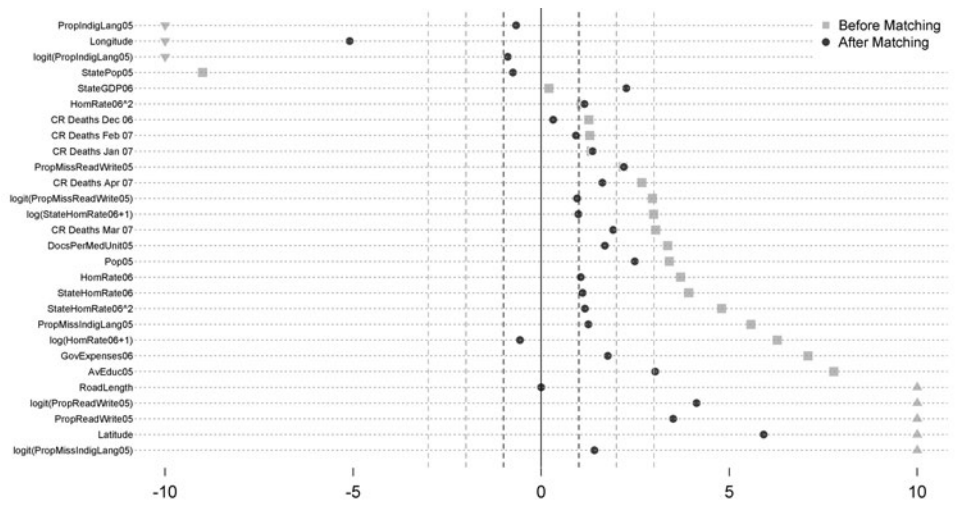
Figure 3 displays the Love plots for our study, where the pre-matching values are represented by orange squares and the post-matching values by purple dots. The covariates are displayed in ascending order, from top to bottom, of the observed difference before matching between municipalities in intervened and non-intervened regions. The vertical dashed lines correspond to 1,

2, and 3 (pre-matching) standard errors from zero. This plot shows that the overall balance is much better after matching. The standardized difference in means for variables involving pre-intervention homicide rates are considerably smaller after matching, except for the square of 2006 Homicide Rate, which is practically unchanged. The post-matching standardized difference in means is clearly larger than 3.0 for four covariates (i.e., latitude, longitude, and both the raw scale and logit transformation of the proportion of the 2005 population that can read and write). Ideally, we would want all of these differences to be very close to zero after matching. This was achieved for all binary covariates (see Figure 2(b)), but not for all continuous covariates, although the overall improvement in balance is clear. The difference in means for the State GDP in 2006 clearly increased after matching. In general, the balance on the monthly criminal-rivalry-related homicide rates before May 2007 (time of first intervention) did not improve as much as the balance for annual homicide rates; in particular, the January 2007 value is actually slightly increased by 0.04, which is almost imperceptible in Figure 3. Nevertheless, we chose to focus on balancing the 2006 homicide rate reported by INEGI because those are the official data, and the reliability of the criminal-rivalry-related death count is unclear. This focus is reflected by the inclusion of nonlinear terms involving 2006 homicide rate on the propensity score. The initial standardized differences of some covariates were outside the  $[-10, 10]$  range displayed in Figure 3(a) (i.e., latitude, longitude, and the two proportions related to speaking an indigenous language in 2005, both the raw scale and logit transformations). Therefore, instead of a pre-matching orange square, we displayed an orange upward triangle or orange downward triangle if they were greater than 10 or less than  $-10$ , respectively.

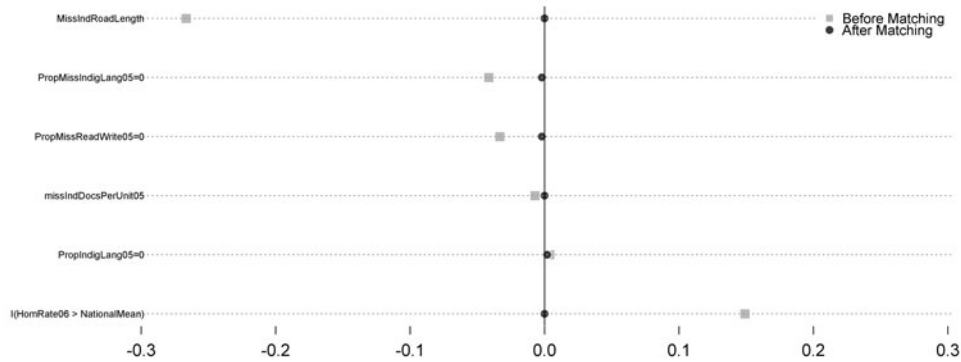
Figure 4(a) shows the propensity score distribution by region, where the after matching control region distributions (white histograms with black borders) consist of twice as many municipalities as the intervened regions (gray histograms). In some cases, the number of municipalities within a treated region is very small (e.g., four in Tijuana region). The balance for the municipalities in the Tijuana, Apatzingán, and Nogales regions is questionable because of the lack of overlap for larger propensity scores. Similarly, Figure 4(b) shows the distribution of the 2006 homicide rates. We included this plot because this lagged version of the outcome of interest was considered the most important covariate used in our matching procedure. However, the 2006 homicide rates in Apatzingán and Teúl show heavier tails than their control counterparts. Surprisingly, all four municipalities in the Bustamante region had no homicides in 2006, thereby explaining the distinctive histogram for this region. Despite the imperfect balance for treated and control regions revealed by Figures 3 and 4, we decided to maintain the full collection of treated regions. However, we also provide an estimate of the average treatment effect including only treated regions for which good balance was achieved by the matching procedure.

### 3.5 Assessing Unconfoundedness

Intuitively, unconfoundedness requires that we have a sufficiently rich set of pretreatment variables such that adjusting



(a) Continuous Covariates. Because these quantities are standardized, the dashed vertical lines correspond to one, two and three standard errors (using the pre-matching estimates of the variance) from zero. The “CR deaths” terms refer to criminal-rivalry-related deaths.



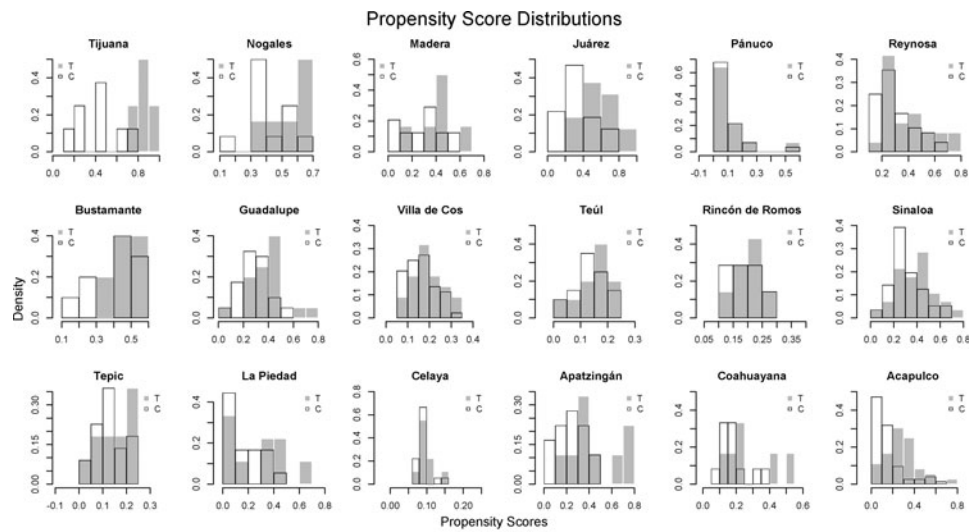
(b) Binary Covariates (indicators of missingness and 2006 homicide rate above the mean were exactly matched).

Figure 3. Love plots comparing the pre-matching and post-matching differences in means for background covariates: (a) Continuous covariates. These quantities are standardized: the dashed vertical lines correspond to one, two, and three standard errors (using the pre-matching estimates of the variance) from zero. The “CR deaths” terms refer to criminal-rivalry-related deaths. (b) Binary covariates (indicators of missingness and 2006 homicide rate above the mean were exactly matched).

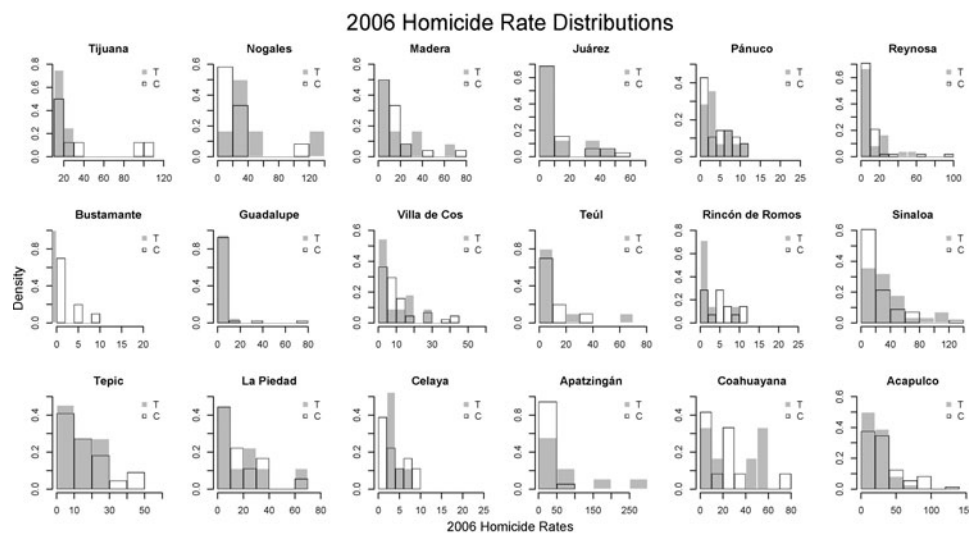
for these variables removes all systematic biases from comparisons between treated and control units. This critical assumption is not directly testable. However, as discussed in Crump et al. (2008) and Imbens and Rubin (2015), one can explore supplementary analyses to assess its credibility.

We focus on estimating a “pseudo” causal estimand with an a priori known value. We assess the treatment effect on homicide rate one year before intervention, which is such a pseudo-outcome. Given that the interventions occurred at different points in time, we used the 2006 homicide rate in the matching process because it was a proper covariate for all units. However, excluding Apatzingán, Pánuco, and Sinaloa for which the intervention occurred in 2007, the homicide rate *one year before intervention* (which can vary from region to region because it depends on the year the first intervention occurred for each treated region) can be considered a pseudo-outcome for which the intervention effect must be zero. The exclusion of these regions is relevant because for them the homicide rate *one year before intervention* is the 2006 homicide rate that was used in

our propensity score estimation. Hence, because the 2006 homicide rate played an important role in our matching, for these three regions a comparison of this variable is a balance check rather than a pseudo-outcome. This pseudo-outcome test corresponds to assessing the null hypothesis that there are no systematic differences in the pseudo-outcome between treatment groups, after adjusting on the original set of covariates in Table 1. Figure 5 compares the distributions of the homicide rates one year before intervention for the municipalities in treated regions and their matches. In general, the overlap of these distributions looks good. In fact, it looks better than that of the 2006 homicide rates shown in Figure 4(b). However, there are still some treated regions with heavier covariate tails than their control matches; a two-sample  $t$ -test of the average regional homicide rates leads to a two-sided  $p$ -value of 0.28, supporting the idea of no systematic pretreatment differences. Because this assessment involves no outcome data, it is a design-stage method and can also be thought of as an important part of the balance assessment.



(a) Propensity score histogram for treated regions and their matched controls.



(b) 2006 homicide rate histograms for treated regions and their matched controls.

Figure 4. Histograms comparing the distributions of (a) the propensity scores, and (b) the most important pretreatment covariate (2006 homicide rate) for municipalities in each treated region (gray) and its matched control (white with black borders). Two histograms are overlaid for each region.

#### 4. ESTIMATION

Using pretreatment covariates to predict outcomes can increase efficiency of estimation. However, in this case a careful modeling of the outcome should take into account the interference between neighboring municipalities in a treated region. Such modeling, however, is beyond the scope of this document, which is to use a *simple* analysis that is more statistically rigorous than the previous studies. Therefore, we only use one pretreatment covariate to transform the observed outcome into what is commonly referred to as a *gain score*. Such gain scores correspond to the  $Y_i$  outcomes defined in Section 3.1, which are the post-intervention minus pre-intervention differences in homicide rates. We estimate the missing potential outcome of treated region  $i$ ,  $Y_i(0)$ , using municipalities that were not intervened but are *similar* to those forming the treated regions before 2007, in particular in their 2006 homicide rates, but might not

be contiguous. We believe it is reasonable to assume that in the absence of an intervention in such *similar* municipalities, there are no spill-over effects from that municipality to its neighbors. Furthermore, we believe that constructing hypothetical regions similar to the treated regions is better achieved by the matching method that we used than by requiring geographical contiguity, because many key covariates relevant for the outcomes, like pre-intervention homicide rates, political composition, and measures of pre-intervention governmental presence (e.g., percentage of indigenous language speakers and road network length), are not well matched if a contiguity restriction is imposed. We believe that the importance of these variables dominates the remaining relationships between contiguous municipalities that are not controlled using the covariates considered. As stated previously, we denote the *observed* outcome of treated region  $i$  by  $Y_i(1)$ , and its counterfactual outcome under control by  $Y_i(0)$ .



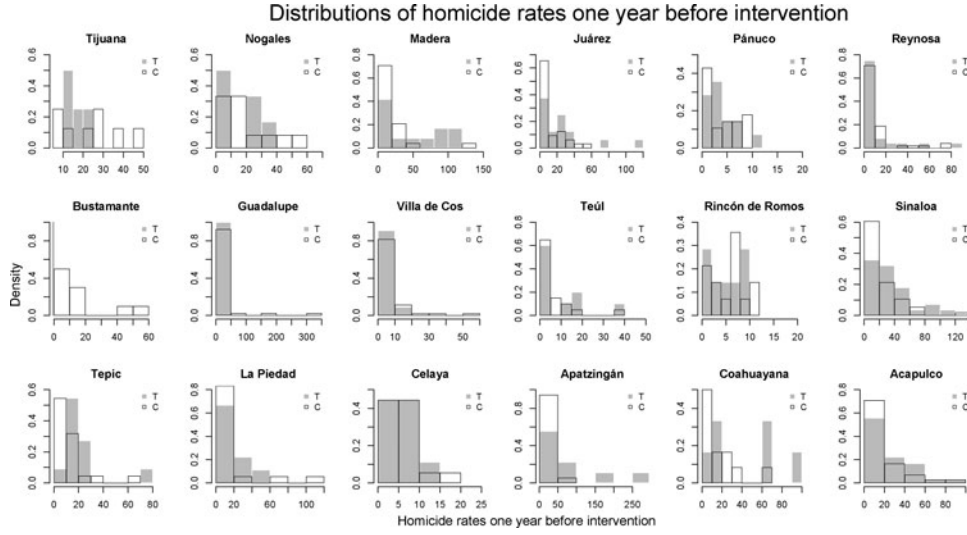


Figure 5. Histograms of homicide rate one year before intervention for treated regions and their control matches. Note that the year that corresponds to this covariate depends on the first intervention received in the treated region.

We estimate the intervention effects by *multiply imputing* (Rubin 1987, 2004) the missing potential outcome for each treated region. We create a donor pool of control hypothetical-regions to impute these outcomes. The number of municipalities forming each treated region,  $N_i$ , determines the size of the corresponding donor pool, which is  $2^{N_i}$ . The donor pool sizes range from  $2^4$ , for the Tijuana region, to  $2^{36}$ , for the Acapulco region. Instead of listing all control hypothetical-regions and then drawing from this pool at random, for every imputation of  $Y_i(0)$ , we generate a control hypothetical-region by randomly selecting a match for every municipality in treated region  $i$ , justified by the comparable quality between matched controls for each treated municipality. Let  $k_{ij} \sim 1 + \text{Bern}(1/2)$  denote the index of the control match selected for the  $j$ th municipality in treated region  $i$ . Analogous to the notation defined in Section 3.1, let  $H_{ijk_{ij}}^-$ ,  $P_{ijk_{ij}}^-$ ,  $H_{ijk_{ij}}^+$ , and  $P_{ijk_{ij}}^+$  denote the homicide count and the population of this match one year before (−) and after (+) the first intervention in the region. Thus, one imputation (draw) of  $Y_i(0)$  (the difference in homicide rates between the post-intervention and pre-intervention years had region  $i$  not received an intervention),  $Y_i^{\text{draw}}(0)$ , is obtained by

$$Y_i^{\text{draw}}(0) = \frac{\sum_{j=1}^{N_i} H_{ijk_{ij}}^+}{\sum_{j=1}^{N_i} P_{ijk_{ij}}^+} - \frac{\sum_{j=1}^{N_i} H_{ijk_{ij}}^-}{\sum_{j=1}^{N_i} P_{ijk_{ij}}^-},$$

with the associated treatment effect for region  $i$  being

$$\tau_i^{\text{draw}} = Y_i(1) - Y_i^{\text{draw}}(0).$$

Then, we obtain a draw of the estimand by averaging the draws for the  $N$  treated regions,

$$\tau^{\text{draw}} = \frac{\sum_i \tau_i^{\text{draw}}}{N}.$$

Repeating this imputation multiple times approximates the distribution of the estimands under our assumptions, and gives a measure of uncertainty for the point estimates of each region, as well as for the overall average. Formally, this estimation pro-

cedure can be viewed as a version of the Bayesian bootstrap (Rubin 1981) with point mass priors on the observed values for the control hypothetical-regions. Let  $\hat{\tau}$  and  $\hat{\tau}_i$  be the mean of the distributions of  $\tau^{\text{draw}}$  and  $\tau_i^{\text{draw}}$ , that is, our point estimates. The estimated 95% intervals shown in Figure 6 and Table 2 are given by the 0.025 and 0.975 quantiles of the distributions of these draws. Note that the RCM allows for the region-level effect assessment, although these estimates are obviously less precise than those of the average treatment effect on all treated regions.

Recall that the use of  $Y_i$  instead of  $HR_i$  only has an effect on the estimates and not on the estimand. Henceforth, all results are given in terms of the post-intervention homicide rates.

## 5. RESULTS ASSUMING UNCONFOUNDEDNESS

Our analysis suggests that the military interventions resulted in an increase in the average homicide rate. However, the estimated effects vary considerably across the treated regions. Results are displayed in Figures 6 and 7, and Table 2. Of the 18 treated regions, only Rincón de Romos and Apatzingán had a significant reduction in the homicide rate, that is, relative to what would have happened without the intervention. Nine regions have 95% intervals containing zero, including Reynosa and La Piedad. The Juárez region is a clear outlier, and its estimate of the intervention effect is almost three times as large as any other. Perhaps this is not surprising, because since the 1990s, Ciudad Juárez has been notorious for displays of violence, such as more than 1000 unsolved murders of young women between 1993 and 2003, and for being a major center of narcotics trafficking linked to the Juárez Cartel. Removing the Juárez region from the analysis yields a smaller, but still positive, estimate of the average treatment effect on the treated, and the interval still excludes zero. Specifically, the estimated average intervention effects with and without Juárez are 10.97 and 6.52, respectively.

Assessments for Tijuana, Nogales, Apatzingán, and, to a lesser degree, Juárez, Acapulco, and Madera, should be made

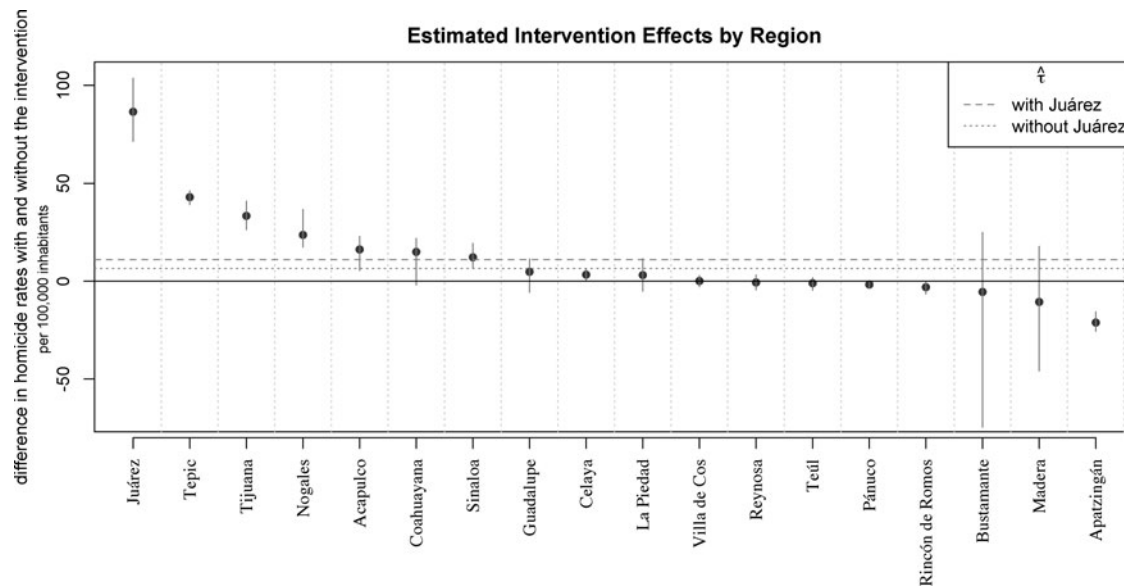


Figure 6. Estimated treatment effects per region shown in decreasing order. The dark horizontal line denotes zero, and the dashed and dotted lines denote the average intervention effect including and excluding Juárez, respectively.

with care given the limitations in the overlap shown in the propensity score and/or pre-intervention homicide rate distributions of municipalities in each of these treated regions and their matched controls. The estimate of the average intervention effect excluding these regions is still positive, 5.78, and its estimated 95% interval is  $(-0.29, 8.92)$ . The lower bound is only slightly below zero, therefore still suggesting that the interventions lead to an increase in homicide rates the year after.

The assessment of unconfoundedness described in Section 3.5 improves the credibility of this assumption. However, a limitation of our analysis is that cartel information was not used. If car-

tel information is needed for unconfoundedness to hold, a causal conclusion is not valid; then the results can only be interpreted from a conditional association perspective, where the relationship between homicide rates and military intervention is assessed by comparing intervened and non-intervened regions that are balanced on these conditioned background covariates. This is a way of effectively “controlling for background covariates,” which is commonly attempted by using only linear models.

Another limitation is that we do not have a comprehensive list of interventions. Such a list can only lead us to remove municipalities from the current control pool. Because the interventions

Table 2. Point estimates and 95% intervals for estimated effects

Region	Number of municipalities	Date of first intervention	Estimated effect one year after		
			$Y_i(1)$	$\hat{\tau}_i$	95% interval
Juárez	16	2008	117.96	86.53	(71.39, 103.65)
Tepic	11	2010	42.98	42.92	(39.03, 46.17)
Tijuana	4	2008	45.20	33.34	(26.33, 40.95)
Nogales	6	2008	40.15	23.63	(17.38, 36.65)
Acapulco	36	2008	26.57	16.20	(5.49, 22.89)
Coahuayana	6	2010	23.03	14.94	(-2.01, 21.91)
Sinaloa	28	2007	11.58	12.19	(6.83, 19.27)
Guadalupe	20	2009	13.11	4.77	(-5.75, 11.27)
Celaya	9	2009	5.24	3.33	(0.46, 6.36)
La Piedad	9	2010	9.94	3.13	(-5.06, 11.49)
Villa de Cos	22	2008	4.54	0.09	(-2.81, 2.92)
Reynosa	24	2008	4.86	-0.68	(-4.45, 3.14)
Teúl	10	2009	5.62	-1.05	(-4.67, 1.84)
Pánuco	14	2007	-0.02	-1.71	(-3.70, 0.09)
Rincón de Romos	7	2008	-0.82	-3.08	(-6.65, -0.15)
Bustamante	5	2010	14.42	-5.50	(-74.58, 24.90)
Madera	12	2010	3.09	-10.60	(-45.89, 17.86)
Apatzingán	9	2007	-31.25	-21.13	(-25.70, -15.68)
Average (all treated regions)	248	—	18.68	10.97	(6.24, 14.27)
Average (excluding Juárez)	232	—	12.84	6.52	(1.64, 9.81)
Average (including only the twelve well-balanced regions)	165	—	11.21	5.78	(-0.29, 8.9)

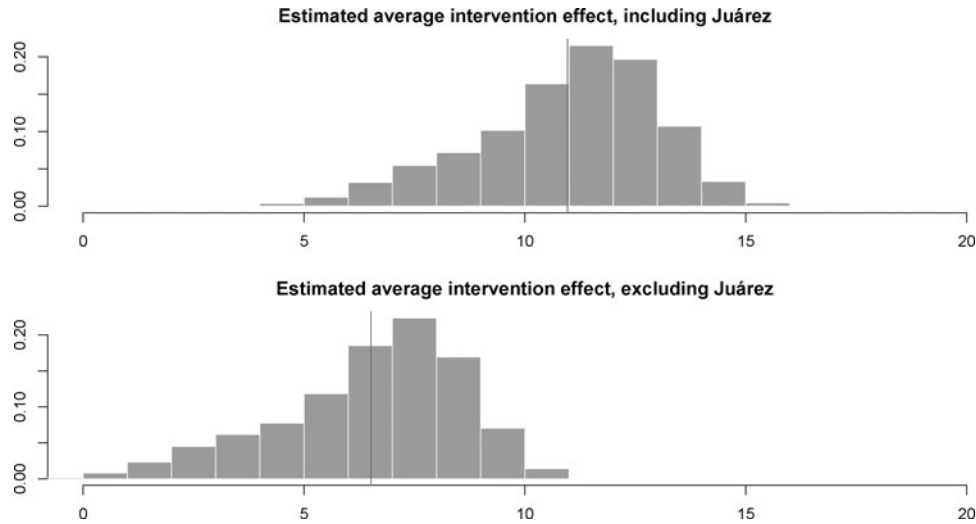


Figure 7. Distributions of the overall estimated average difference in post-intervention minus pre-intervention homicide rates with and without the intervention, including (top) and excluding (bottom) Juárez.

took place in municipalities that were thought to need them the most, we would expect lower pre-intervention homicide rates in the *true-controls*, leading to larger differences between control and treated pre-intervention homicide rates compared to those reported here. Thus, assuming pre-intervention homicide rates are predictive of the post-intervention homicide rates, using these *true-control* matches would most likely increase the estimated treatment effect, relative to the one reported here.

Perhaps confining the analysis to one year post-intervention is too limited to evaluate fully the effectiveness of these military interventions. The homicide rate in Ciudad Juárez, located in the region with the highest increase in homicide rate, dropped considerably in 2012 (La Redacción 2013), which suggests that following the homicide rate for several years after the intervention takes place could give a more complete picture of the effect of an intervention. It would not be surprising if, initially, a military intervention upset the local balance in power between cartels and local police—which is particularly relevant if these bodies were infiltrated by organized crime—thereby increasing the violence in the short term, but that military presence also decreased homicide rates in the long term. We do not report estimates beyond the first year because we only have homicide counts for all municipalities up to 2011, considerably reducing the number of regions that can be used to estimate the second year post-intervention effect and beyond.

In the spirit of Abadie, Diamond, and Hainmueller (2010) and Fisher randomization tests, an alternative way of assessing the significance of our average causal effect estimate is to conduct a series of studies that exclude all treated regions, by iteratively applying the matching method and the estimation procedure, described in Sections 3.3 and 4, respectively, to random subsets of 248 control municipalities, where 248 is the number of municipalities in the treated regions (see Table 2). We refer to these subsets as pseudo-treated municipalities and use the observed intervention dates and region-membership variables to define the pseudo-treated regions. However, when creating the pseudo-treated regions, it is not always possible to find two matches for each pseudo-treated municipality. If that occurs, we

proceed with the analysis and ignore the fact that some pseudo-treated municipalities had no matches, which could make the pseudo-treatment effect estimates more extreme. We repeated this procedure 1000 times for the difference in post-intervention minus pre-intervention homicide rates between pseudo-treated municipalities and their matches. Because this method involves only control municipalities, the treatment effect should be zero, which is consistent with the 95% interval obtained for the pseudo-treatment effect ( $-4.0, 4.5$ ). Furthermore, the estimated average intervention effects with and without Juárez, as well as the estimated effect including only the well balanced regions (i.e., 10.97, 6.52, and 5.78, respectively), are all larger than the 97.5% quantile of the distribution of the estimated pseudo-treatment effects, which supports the credibility of the estimation method used for the main analysis.

## 6. DISCUSSION

The design and analysis stages of this observational study posed interesting challenges. Here, we explored the simplest approach that we believed was at all credible. Although some modeling efforts could be helpful, it is unclear how to account for the interference in neighboring municipalities with this approach, as mentioned earlier in Section 4. Therefore, this type of study is probably better suited for a causal network analysis at the municipality level because of the interference between neighboring municipalities, and the possibility of defining multiple levels of treatment, but would require a more sophisticated model for the response. Further exploration of such an approach is left for future work.

In the no-interference approach we took here, the feasibility of SUTVA induced a modification to the definition of the units. Furthermore, the imputation procedure involved subtleties in the construction of control hypothetical-regions comparable to the treated ones. We proposed an analysis method based on permutation ideas that allowed for the multiple imputation of the homicide rate of the treated regions, had they not received a military intervention. We believe that our approach is more

statistically principled than those offered by Escalante (2011) and Merino (2011), by increasing the feasibility of the assumptions being made implicitly or explicitly, the explicit evaluation of covariate balance, an assessment of unconfoundedness, and the use of a supplementary Fisher randomization test using only control municipalities. Our method multiply imputes the missing potential outcomes for each treated region, resulting in approximate distributions for the estimated average treatment effect across the treated regions, as well as estimated treatment effects for each region.

There are unavailable covariates that affect the plausibility of the unconfoundedness assumption. However, our assessment of this assumption (shown in Section 3.5) supports its plausibility. Nevertheless, if unconfoundedness is not achieved, a principled conditional association interpretation of these results can still be made. For this interpretation, homicide rates are compared for intervened and non-intervened regions with similar values of background covariates.

The yearly homicide rates require population information to be calculated, and INEGI has population information every five years. Therefore, our calculation of the homicide rates involved linear interpolation of these population values (and the 2011 population data were extrapolated). Modeling the yearly population at the municipality level would improve our analysis. Also, our study could be refined by conducting a web search of SEDENA press releases and Google News ourselves, to obtain a more comprehensive list of interventions.

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