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The Economic Consequences of the Mexican Drug War

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Abstract: Military crackdowns often disrupt economic development by exacerbating violence. This paper examines the case of the Mexican Drug War, employing synthetic control methods. To prove causality, I use variation on statewide military operations, as well as the rollout of the war. Findings indicate a decrease in GDP per capita equal to 0.5%, in states with military operations. Determinants by which the Mexican Drug War hampered economic development include a proportional reduction in consumption per capita, and a decline in productive investment of at least 0.3%, driven by a drop of 3.2% in commercial credit granted to businesses.

Keywords: war on drugs, Mexico, economic growth, organized crime, synthetic control methods

1 Introduction

Mexico's previous federal administration (December, 2006–December, 2012), headed by President Felipe Calderón, launched an unprecedented military strategy to capture as many drug-lords as possible. The military crackdown consisted in sending approximately 45,000 soldiers to the streets to cover local police duties (Shirk and Wallman 2015). According to the government, this hard-handed security strategy came about as a response to an expansion in drug-related violence: Between 2001 and 2006, prior to the launching of the Mexican Drug War, total yearly drug-related homicides increased 94%, from 1080 to 2100 (Chabat 2010). However, after the implementation of Calderón's military crackdown, drug-related homicides spiked to over 16,000, eight times more than in 2006. The existing literature suggests that the main causal link between Calderón's military strategy and the spike in drug-related violence was the fragmentation of drug-trafficking

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organizations, hereinafter DTOs (Calderón et al., 2015; Dell, 2015; Guerrero, 2011; Merino, 2011; Shirk and Wallman, 2015).

Whereas most of the literature on violence in Mexico has focused on the impact of the drug war on homicides, the economic consequences of the Mexican Drug War remain understudied. What is more, economists have long examined the welfare impact of different sorts of violence like civil wars (Collier 1999), crime (Enamorado, López-Calva, and Rodríguez-Castelán 2014; Soares 2006) and terrorism (Abadie and Gardeazabal 2003; Bassil 2013); yet, there is little knowledge about the fiscal ramifications of a strong-armed military crackdown. The purpose of this paper is to fill this policy-knowledge gap by testing empirically whether the Mexican Drug War had an effect on economic development at the state level, using gross domestic product (GDP) per capita as the outcome of interest.

The research design for this paper consists mostly of synthetic control methods, which is an ideal methodology for comparative case studies at an aggregate level, with few units in the universe (e.g. only 32 states in Mexico). This recently developed econometric technique compares a homogenous “control” state, produced by applying a two-step optimization procedure, with a “treated” state over time. I define a state as “treated” if, at any point during Calderón’s administration (2007–2012), the army had an executive mandate to be a primary security provider in that state. To prove causality more systematically, I use panel data on the rate of statewide military operations conducted by the Mexican Army, as well as the geographic rollout of the war. Due to the lack of data, the proposed methodology is not capable of separating falls in GDP per capita that come as a consequence of disruptions in the Mexican drug industry, as opposed to falls in output unrelated to drug-trafficking. Notwithstanding this limitation, the 2013 World Drug Report shows a significant increase of drug-trafficking and drug-cultivation in Mexico (UNODC 2013, chs. 1B and 1C); therefore, any effect is likely to be a consequence of falls in output unrelated to the drug industry.

Findings indicate a reduction of 0.5% in GDP per capita for treated states, over the period 2007–2012, as a direct effect of the Mexican Drug War. Determinants include a proportional reduction in consumption per capita for treated states. By the same token, the Mexican Drug War caused a decline in productive investment of at least 0.3%, driven by a drop of 3.2% in commercial credit (non-consumption and non-mortgage) per capita granted to the private sector.

The paper proceeds as follows. Section 2 describes the Mexican Drug War, briefly. Section 3 identifies a proxy for the Mexican Drug War, and proves orthogonality in the assignment of treatment. Section 4 lays the foundations for the empirical strategy. Section 5 presents the results and robustness checks. Section 6 analyzes the economic determinants by which the Mexican Drug War hampered GDP per capita. Finally, I conclude with immediate policy applications.

2 The Mexican Drug War

The true political reasons for launching a large-scale drug war remain unknown. Watt (2011), Anaya-Muñoz (2012), and Bardallo-Bandera (2012) claim that the Mexican Drug War was a political strategy to legitimize Calderón's presidency after a rather narrow electoral victory of only 0.56%. Other academics like Chabat (2010) and Valdés-Castellanos (2013) suggest it was a desperate action to attend existing security concerns. One fact, however, is that President Calderón encountered a more complicated national security threat than all of his predecessors.

Whereas most Mexican presidents were able to regulate drug-trafficking activities through a *"pax mafiosa,"* three structural factors damaged the Mexican tolerance policy (Chabat 2010). First, Mexico began cooperating with US government agencies to eliminate the supply of illicit drugs (Craig 1980).¹ Second, cocaine-trafficking routes shifted to Mexico after the US blocked the narrower Caribbean-trafficking corridor in the 1980s and early 1990s, which allowed Mexican DTOs to capture quasi-rents from Colombian drug cartels (Chabat 2010; Toro 1995). Third, Mexico's democratization process decentralized political power and made it impossible to negotiate effectively with DTOs, leaving severe power vacuums (Osorio 2012).² Consequently, drug-related violence increased across Mexico, although at very controllable levels.

By the time Calderón took power, he faced a security threat of medium dimensions. Soon after receiving complaints about the expansion in the activities of DTOs by the governors of Michoacán, Baja California, and Guerrero, Calderón opted for sending Mexico's military forces out to the streets. In particular, Calderón's military crackdown sought to capture kingpins from all DTOs. Since the federal government tried to pursue this goal in conjunction with the local governments, the military strategy became known as *"joint operations"* (JOs). However, because of politics, the federal government seldom coordinated with the local police, many times taking over local public safety duties, permanently. For the rest of this paper, I use the terms "Mexican Drug War," "military strategy," "military crackdown," and "JOs," interchangeably.

In total, 11 out of the 32 Mexican states had a statewide JO at some point during the period 2007–2012. These states are Baja California, Chihuahua, Coahuila,

¹ In September 1975, the Mexican Army, in coordination with the US Drug Enforcement Administration (DEA), implemented "Operation Condor," the first military strategy against the supply of illicit drugs. This partnership in law enforcement expanded after the assassination of DEA-agent Enrique Camarena in 1985, and the initial negotiations of the North American Free Trade Agreement in 1990 (Chabat 2010).

² Mexico locked its political system into democracy with the election of Vicente Fox, the first president not to belong to the hegemonic political party, PRI.

Durango, Guerrero, Michoacán, Morelos, Nuevo León, Sinaloa, Tamaulipas, and Veracruz. Overall, the military crackdown was very successful at accomplishing its target: Only 11 out of the 37 most wanted drug-lords were still at large by the end of Calderón's administration.

Nonetheless, the decapitation of DTOs caused a "hydra effect." According to Guerrero (2011), the number of DTOs went from six to 16 during Calderón's presidency. This hydra effect responded to the organizational structure of the Mexican DTOs. Contrary to the Colombian Cartels and the Italian Mafia, the Mexican DTOs had a cellular organization rather than a vertical structure. That is, the Mexican DTOs were very much like a horizontal merger of enterprises or franchises, working within the same line-of-business in different Mexican states. The leader of the DTO coordinated and held together all corresponding cells, while managing international contracts for the transportation and final processing of drugs such as cocaine from Colombia and ephedrine from China. For instance, the Sinaloa DTO, led by Joaquín "El Chapo" Guzmán, contained within its organization at least 12 different cells, operating in nine different states: "Gente Nueva" in Sinaloa, "Los Mexicles" in Chihuahua, "El Tigre" in Baja California, "Los Mata-Zetas" in Veracruz, among others. Once the leader of the DTO fell from the structure, turf wars erupted within and across DTOs to gain control of trafficking-routes and major cities ("*plazas*").

Existing empirical evidence suggests that the Mexican Drug War had a significant effect on drug-related homicides. Using a propensity score matching estimator, Merino (2011) concludes that JOs caused 12,046 (52.4%) additional drug-related homicides between 2007 and 2010. By applying a regression discontinuity design on electoral results, Dell (2015) also finds supportive evidence for this causal hypothesis, with a very similar magnitude in municipalities that have a strong presence of DTOs (53.0%).³ Last, Calderón et al. (2015) expand these findings by running a difference-in-difference regression with time-varying unobservable controls of intentional homicide rates on drug-lord and drug-lieutenant arrests. Their results show a treatment effect as high as 51.8% on drug-related homicides, in the first 6-months after the capture of a drug-lieutenant.

Notwithstanding the unintended consequences of the Mexican Drug War, there would still have been a significant increase in drug-related homicides because of two foreign exogenous confounding events. First, an increase of cocaine seizure rates in Colombia after 2006, which according to Castillo, Mejía, and Restrepo

³ Dell (2015) uses variation in electoral results for municipalities where PAN, Calderón's political party, barely won, and where more federal enforcement assistance occurred at the beginning of the Mexican Drug War. Dell's electoral variation was lost after the rollout of the Mexican Drug War.

(2014), explains around 17.1% of all drug-related homicides in Mexico because of additional revenue from cocaine-trafficking.⁴ Second, the 2004 expiration of the US Federal Assault Weapons Ban (AWB), which according to Chicoine (2011) and Dube, Dube, and García-Ponce (2013), made semi-automatic weapons more accessible to DTOs in Mexican states along the US border, except for Baja California (e.g. California, the northern-neighboring state of Baja California, had a state level AWB). These authors conclude that easier access to semi-automatic weapons had a marginal effect on intentional homicide rates within the range of 16.4%–21.0%, for the period 2004–2008. Yet, the existing literature emphasizes Calderón's military strategy as the main driver of the variation in drug-related violence.

3 Identification strategy

The primary objective of this paper is to estimate the effect of the Mexican Drug War on economic development at the state level. Therefore, first, I identify a continuous proxy for the Mexican Drug War to describe its economic impact. A proper continuous proxy gathers variation in timing and intensity of treatment. I utilize the rollout in the government's implementation of statewide JOs as the most direct measurement for the timing of treatment.

Table 1 contains a timeline for all statewide JOs implemented during Calderón's administration. Evidently, Calderón launched some of these JOs as early as just days after assuming office in December 2006, and as late as weeks shy from leaving office in December 2012. However, the rollout in the implementation of the JOs does not capture the intensity of the Mexican Drug War across treated states. To measure treatment intensity, I use data from Mexico's Freedom of Information Act System on the rate of statewide interception operations (per 100,000 inhabitants), conducted by the Mexican Army.⁵ These operations include cargo and fugitive interceptions in highways, ports, streets, and international bridges. Namely, this variable is suitable as a measure of army deployment across Mexican states. Table 1 shows basic statistics – mean and standard deviations – on these interception operations (per 100,000 inhabitants) for treated states, for the years 2007–2012.

⁴ More cocaine seizure in Colombia (less supply) led to higher revenues because of an inelastic demand for illicit drugs in the US; subsequently, DTOs fought against each other to gain control over those additional revenues (Castillo, Mejía, and Restrepo 2014).

⁵ Tracking file identification number in the Freedom of Information Act System is 0000700008513.

Table 1: Drug-related violence and timing and intensity of treatment (2007–2012).

State	Month/Year	Average (std. dev.) rate p/100,000 inh. 2007–2012 [†]	
		Army operations	Drug-related homicides
Michoacán [*]	January, 2007	0.10 (0.05)	11.67 (2.89)
Baja California	January, 2007	0.02 (0.01)	20.35 (4.71)
Guerrero	January, 2007	0.05 (0.05)	33.95 (19.75)
Nuevo León	January, 2008	0.06 (0.05)	12.64 (15.96)
Tamaulipas	January, 2008	0.08 (0.07)	13.19 (9.79)
Sinaloa	January, 2008	0.14 (0.10)	38.63 (20.41)
Chihuahua	April, 2008	0.12 (0.07)	78.94 (33.05)
Durango	May, 2008	0.12 (0.09)	28.72 (18.99)
Coahuila	September, 2011	0.07 (0.08)	11.74 (9.76)
Veracruz	October, 2011	0.06 (0.06)	3.87 (1.58)
Morelos	May, 2012	0.06 (0.07)	12.24 (16.79)
All average	2008	0.08	24.18

[†]Data for military operations comes from the Freedom of Information Act System.

Data for drug-related homicides comes from the Bureau of Health Statistics (SINAIS).

^{*}Michoacán's JO initiated on December 11th, 2006, being reinforced in January, 2007.

Although the Army conducted a few interception operations prior to the Mexican Drug War, the number of these operations increased 10-fold during Calderón's administration. Most important, the nature of these operations was radically different after the implementation of the Mexican Drug War. For instance, the Mexican Army began using unconstitutional check-points, sophisticated detection technology (e.g. industrial ion scanners), and a larger number of soldiers traveling in convoys. Given these fundamental differences in the nature of interception operations after the beginning of the conflict, the interaction between the rollout of JOs and the rate of interception operations constitutes my continuous proxy for the Mexican Drug War.

The last column in Table 1 presents basic statistics of drug-related homicides for all treated states. I approximate drug-related homicides using the mortality databases from Mexico's Bureau of Health Statistics (SINAIS). In particular, I track leading causes of death associated to drug-related violence (e.g. murder by hanging, murder by gunshot, and murder by mutilation). Compared to official records of drug-related homicides from Mexican Intelligence Agencies – only available from 2007 to 2010 –, SINAIS data seem to approximate rather accurately drug-related homicides.

According to the existing literature (Calderón et al., 2015; Dell, 2015; Guerrero, 2011; Merino, 2011), war-intensity variation, interacted with the

rollout of JOs, should explain differences in drug-related homicides. To verify the reliability of my continuous proxy, I run ordinary least squares (OLS) and fixed-effects regressions on drug-related homicide rates for treated states, for the period 2007–2012. For this exercise, I control for the two identified confounding factors: i) cocaine seizure rates in Colombia and ii) dummies for Mexican states along the US border, affected by the 2004 expiration of the US Federal AWB. To account for dynamic tendency and simultaneity, I also include the first-lag value of the dependent variable. For the fixed-effects model with the first-lag value of the dependent variable, I utilize Arellano and Bond’s generalized method of moments (ABGMM) procedure – which uses higher-lag values of the dependent variable as instruments, to solve for serial correlation – to obtain consistent point-estimators (see Angrist and Pischke 2009, ch. 5). Table 2 contains the results of these regressions, which indicates that, on average, the Mexican Drug War explains as much as 30.8% of the variation in drug-related homicide rates during the course of the conflict. What is more, both confounding factors move in the predicted direction. Although smaller in magnitude than for previous papers, these findings are consistent with the conclusions of the existing literature. Differences in point-estimation most likely come from endogeneity biases, a smaller sample (e.g. state vs.

Table 2: Effect of the Mexican Drug War on drug-related homicide rates for treated states (2007–2012).

	(1) OLS	(2) OLS	(3) Fixed	(4) ABGMM
Roll×inter ops	77.424*** (21.267)	71.545*** (23.067)	102.969*** (37.343)	84.906** (37.109)
Colombia seizures	0.080** (0.035)			
Border×AWB		2.841 (3.983)		
Lag p/drug homicide	0.736*** (0.163)	0.756*** (0.160)		0.501*** (0.031)
State dummies	No	No	Yes	No
Year dummies	No	Yes	Yes	Yes
R-squared	0.710	0.736	0.753	
Number of states	11	11	11	11
Observations	66	66	66	66
Drug war ATT (drug hom. rate)	5.60	5.17	7.44	6.14

All regressions contain robust standard errors in parentheses. Connotations *, **, and *** mean significant at the 90 p., 95 p. and 99 p. level of confidence.

municipality level), and the exclusion of operations conducted by the Federal Police and the Mexican Navy.

Finally, for a valid identification strategy, I must verify orthogonality between the outcome of interest and the assignment of treatment. In other words, I must make sure that the JOs did not take place in the poorest Mexican states. Otherwise, my estimations would be biased because the treatment coefficient would pick up unobservable institutional factors that determine economic development, like corruption. Fortunately, Figure 1 depicts clear discrete evidence for the orthogonality between pre-treatment GDP per capita and the assignment of treatment. Seven out of the 11 treated states had GDP per capita values above the national median prior to the conflict.⁶ Only Guerrero, Michoacán, Morelos, and Veracruz had pre-treatment GDP per capita values below the national median. Similarly, before the beginning of the Mexican Drug War, six out of the 11 treated states had corrupt public institutions, based on the median bribery-index from the National Survey of Corruption and Good Governance.⁷ Not surprisingly, corrupt treated states, with the exception of Durango (marginally rich) and Nuevo León, were also “poor” treated states. The previous indicates that DTOs were located in states with easy access to trafficking routes rather than in poorer states with corrupt institutions, exclusively (Dell 2015).

Table A.1 in the Appendix section expands this evidence on treatment orthogonality, using continuous indicators for treatment, pre-treatment GDP per capita, and pre-treatment corruption levels. All models in Table A.1 correspond to OLS estimators. Clearly, neither pre-treatment GDP per capita nor pre-treatment corruption are good predictors for the assignment, timing, and intensity of treatment.

Figure 1: Discrete evidence for the orthogonality in the assignment of treatment.

⁶ I exclude Campeche and Tabasco from the sample because both economies are highly dependent on oil-drilling activity revenues, which belong to the federation (see below).

⁷ Corruption rates are based on bribery frequency to obtain local public services (e.g. pay traffic violations, property registration, etc.), as measured by the 2001, 2003, and 2005 National Survey of Corruption and Good Governance.

4 Empirical design

4.1 Synthetic control methods

A reliable continuous proxy for treatment, and orthogonality in its assignment, allow me to estimate the effect of the Mexican Drug War on GDP per capita for treated states. This being a comparative case study at an aggregate level, with few units in the universe, the empirical design consists of synthetic control methods (SCMs). In the context of Rubin's model for inferential causality, SCMs use the scientific solution to solve for the fundamental problem of causal inference (FPCI).⁸ Contrary to the statistical solution to the FPCI, the scientific solution depends on unit homogeneity between treated and control units, rather than on the independence assumption (Holland 1984).⁹

Hence, under the scientific solution to the FPCI, the economic impact of the Mexican Drug War ($G_{s,t}$) for state s at year t is simply the difference between the GDP per capita of state s with a JO ($Y_{s,t}^T$) and the outcome of the identical untreated state ($Y_{s,t}^C$), so long as there are no exogenous confounding factors driving the same causal mechanism (e.g. the expansion of drug-related violence):

$$G_{s,t} = Y_{s,t}^T - Y_{s,t}^C. \quad (1)$$

To accomplish unit homogeneity between treatment and control states, SCMs rely on economic theory and enough data variation from a pool of donor states, not exposed to treatment. Without loss of generality, assume that there are j donor states that can be observed for T years, and that there are T_0 years prior to treatment. Moreover, let $W = [w_1, \dots, w_j]$ be a vector of non-negative weights that sum to one, where the components of the vector represent the weights assigned to each of the donor states.¹⁰ Choosing a value of W creates a synthetic control for one particular treated state.

Abadie and Gardeazabal (2003) suggest a two-step optimization procedure to find the weights that accomplish unit homogeneity. In particular, their procedure consists in minimizing the following equation for each of the treated states:

$$(X_s - X_0 W)' V (X_s - X_0 W), \quad (2)$$

⁸ "It is impossible to observe the value[s] of [...] [treatment] and [control] on the *same* unit, and therefore, it is impossible to observe the effect" of whatever is being measure (Holland 1986).

⁹ Because of the low number of units in the universe, and the absence of randomization, is inappropriate to assume independence across units, over time.

¹⁰ Weights are constraint to positive values between zero and one (inclusive); hence, there is no extrapolation (Abadie and Gardeazabal 2003).

where X_s and X_0 are vectors of pre-war characteristics for treated unit s and all donor states, respectively; and V is a symmetric, positive semidefinite matrix that assigns a relative-importance factor to each of the outcome predictors. These authors solve equation (2) conditional on V , which in turn seeks to minimize the mean square prediction error (MSPE) during the pre-treatment (matching) period:

$$\text{MSPE}_s = \left(Y_{s,t} - \sum_{i=1}^j w_i^* Y_{s,t} \right)^2 \text{ for } t \in \{1, \dots, T_0\}. \quad (3)$$

In theory, Abadie and Gardeazabal (2003) show that if the synthetic control resembles closely a given treated state prior to treatment, then these same weights (W^*) can be used after period T_0 to estimate the treatment effect for that state:

$$\hat{G}_{s,t} = Y_{s,t} - \sum_{i=1}^j w_i^* Y_{s,t} \text{ for } t \in \{T_0+1, \dots, T\}. \quad (4)$$

Evidently, equation (4) resolves in an effect with an upward bias if the counterfactual of a treated unit is underestimated, and vice-versa (Abadie, Diamond, and Hainmueller 2010). In practice, it is hard to find a perfect weight vector such that the MSPE is exactly equal to zero. As matter of fact, the fitting could be poor, in which case Abadie, Diamond, and Hainmueller (2010) advise against using SCMs.

Finally, since there are at least two identified confounding factors that simultaneously provoked the expansion of drug-related violence, equation (4) estimates the effect of the total expansion in drug-related violence on GDP per capita. This is not a problem for the overall results because the confounding factors are exogenous.¹¹ Most important, the continuous proxy for the Mexican Drug War helps to uncover causality once a proper counterfactual becomes available.

4.2 Data and case implementation

For the current comparative case study, the outcome of interest (Y) is GDP per capita. Observed covariates (X) are population density, gross fixed assets, economic sectoral shares, and human capital. I average the aforementioned variables over the matching period: 1993–2003, except for population density, which only contains the value for 2003. Additionally, I augment these variables by including the level of GDP per capita in 2003. This economic growth model, borrowed from

¹¹ In the presence of negative spillovers from the two identified confounding factors on donor units, the effect of the Mexican Drug War on GDP per capita is a strict lower bound.

Barro and Sala-i-Martin's (1995) work, is practically identical to the one used by Abadie and Gardeazabal (2003). One important aspect to notice is that the matching period for all treated states (1993–2003) stops four years before the beginning of the Mexican Drug War (2007) to avoid stiffer restrictions due to confounding shocks (the 2004 expiration of the US Federal AWB), as well as potential spillover effects that may damage estimations otherwise.

Disaggregate GDP data at the state level comes from Mexico's National Institute of Statistics and Geography (INEGI). This data is available from 1993 to 2012.¹² Records for gross fixed assets belong to the 1993, 1998, and 2003 economic censuses conducted by INEGI.¹³ Population estimations for all years also come from INEGI. Moreover, I calculate human capital by state, for individuals older than 15 years of age, based on records from the Ministry of Education (SEP) and from INEGI.¹⁴ Human capital data is available yearly from 1993 to 2012.

To build synthetic controls for all treated states, I use 19 of the 21 states (including the Federal District) from the donor pool, because two donor units (Campeche and Tabasco) have economies where over 65% of their GDP depends on oil-drilling activity revenues, which belong to the federation. Relying on donor's data variation and on the outcome predictors above, I construct synthetic controls for all of the 11 treated units. In addition, I run iteratively the two-step optimization procedure on all donor states to obtain synthetic controls for fitting assessment and causal inference uses.¹⁵

Serving as an example, Table 3 reports the pre-treatment values of the outcome predictors for Chihuahua, its synthetic control, and the average of all

12 There are two different GDP series: one that runs from 1993 to 2006, and another series that runs from 2003 to 2012. The latter series includes detailed regional measurements and new economic activities like agricultural services, oil and gas drilling, oil-related construction, land division services, new manufacturing divisions, as well as new tourism services, just to mention a few. This study uses the 2003-base series, and makes some adjustments to calculate the GDP for the earlier series.

13 I use gross fixed assets instead of gross total investment because the latter variable is not available for Mexico at the state level. Gross fixed assets include only those productive assets with a durability higher than one-year.

14 To build human capital variables by state, I apply Mas's (1995) methodology: $H_{r,t} = H_{r,t-1} + E_{r,t} + O_{r,t} + \delta_{r,t} H_{r,t-1}$, where $H_{r,t}$ is the level of schooling in state r , at time t ; $E_{r,t}$ is the inflow of the adult population with the same level of education; $O_{r,t}$ is the outflow of individuals moving to a higher level of schooling; and $\delta_{r,t}$ is the mortality rate at a certain level of education. INEGI provides the baseline levels and mortality rates by education category, while the Ministry of Education (SEP) reports schooling inflows and outflows.

15 I use the nested optimization method to build all synthetic controls, with the exception of Distrito Federal, Nuevo León, Quintana Roo, Tamaulipas, Tlaxcala, and Veracruz, which show no improvement, or no available convex combination, over the data-driven regression based method.

Table 3: Matching period characteristics for Chihuahua, synthetic Chihuahua, and donor units.

	Chihuahua	Synthetic Chihuahua	Mexico ¹
Real GDP per capita ^a	18604.90	18603.78	15556.32
Population density ^b	12.86	545.19	420.4
Gross fixed assets ^c	27.61	29.70	35.05
Sectoral shares (%) ^d			
Agriculture, forestry, and fishing	6.30	5.87	5.94
Energy, mining, and construction	10.18	10.18	10.98
Manufacturing industry	28.14	23.26	19.20
Marketable services	33.41	39.33	41.10
Nonmarketable services	21.97	21.36	22.78
Human capital (%) ^e			
Illiterates	5.18	8.67	10.81
Primary (0th–6th)	44.01	41.22	41.42
Secondary (7th–12th)	40.66	39.85	38.09
Higher education (+12th)	10.15	10.26	9.68

¹Excluding all treated states, Campeche, and Tabasco.

^aGDP per capita in 2003, (1993) Mexican Pesos.

^bPersons per square kilometer, 2003.

^cAverage gross fixed assets as percentage of the GDP for 1993, 1998, and 2003.

^dAverage percentage share of the GDP for the matching period 1993–2003.

^eMean percentage of working population (15+) for the matching period 1993–2003.

donors states. Indeed, synthetic controls outperform simple averages of donor states at accomplishing unit homogeneity. To save space, I do not report the matching period characteristics for all other treated states; however, an identical conclusion can be drawn from the rest of the treated states.¹⁶

4.3 Normalization and sample selection

For comparability purposes, I calculate the normalized mean square prediction error (NMSPE) and the normalized treatment effect ($\hat{NG}$) as percentages of the GDP per capita from equations (3) and (4), since units depart from different levels of GDP per capita:

¹⁶ Matching period characteristics for all other treated states are available from the author upon request.

$$\begin{aligned} \text{NMSPE}_s &= \left(100 - \frac{\sum_{i=1}^j w_i^* Y_{s,t}}{Y_{s,t}} \times 100 \right)^2 \text{ for } t \in \{1, \dots, T_0\}, \\ \hat{NG}_{s,t} &= \left(100 - \frac{\sum_{i=1}^j w_i^* Y_{s,t}}{Y_{s,t}} \times 100 \right) \text{ for } t \in \{T_0+1, \dots, T\}. \end{aligned} \quad (5)$$

According to the distribution of the NMSPE, the economic model above proves to be a good predictor of economic growth for many Mexican states. In particular, the yearly median value of the NMSPE for the full sample (treated and donor units) during the matching period is equal to 7.6%. Unfortunately, not all treated states accomplish a strong fit during the matching period as to obtain a reliable counterfactual.

Certain treated states obtain NMSPEs above the full sample median, such as Baja California (10.1%), Coahuila (8.1%), Morelos (7.7%), Nuevo León (15.9%), Tamaulipas (39.3%), and Veracruz (10.0%). In stark contrast, Chihuahua (3.2%), Durango (3.9%), Guerrero (0.2%), Michoacán (7.1%), and Sinaloa (2.0%) attain NMSPEs below the full sample median. Luckily for this paper, the latter set of treated states experienced JOs early during Calderón's administration, as well as the highest rates of military operations and drug-related violence (see Table 1).

For the sake of accuracy and brevity, I limit my discussion to Chihuahua, Durango, Guerrero, Michoacán, and Sinaloa. Provided that the proportion of pre-treatment poor treated states remains almost the same (e.g. two of five treated states), narrowing my analysis does not create a problem for causal inference, as orthogonality in the assignment of treatment continues to hold. Moreover, I emphasize the particular case of Chihuahua, because this state experienced homicide rates and army operations rates well above all other states.

5 Results

5.1 Effect of drug-related violence on GDP per capita

Figures 2 and A.1 depict the results for those treated states with an accurate synthetic control. The panels on the left (top) in Figures 2 and A.1 plot the trajectory of the GDP per capita for Chihuahua, Durango, Guerrero, Michoacán, Sinaloa, and their respective synthetic controls. Following equation (4), a simple visual comparison between treatment and synthetic control lines allows the impact assessment of drug-related violence – provoked by the Mexican Drug War and the identified

Figure 2: GDP per capita gap between Chihuahua and synthetic Chihuahua.

confounding factors – on GDP per capita, for each of the treated states. In normalized terms, Chihuahua presents an outcome gap equal to -13.2% , Durango an outcome difference of -6.7% , Guerrero an outcome division of -3.6% , Michoacán an outcome gap of -3.4% , and Sinaloa an outcome difference of -3.6% .

Overall, the magnitude of the GDP per capita gap is directly proportional to the expansion of drug-related violence, presented numerically in Table 1. To illustrate this causal relationship, the right-hand side (bottom) panels in Figures 2 and A.1 plot drug-related homicide rates from SINAIIS (shaded areas) and from the Mexican Intelligence Agencies (gray dashed-dotted lines), along with the GDP per capita gap (solid line), obtained from the left-hand side (top) panels. As mentioned above, drug-related homicide rates from the Mexican Intelligence Agencies are only available from 2007 to 2010, and move rather closely to drug-related homicide rates from SINAIIS.

For most treated states, these graphs show that the line for the GDP per capita gap descends very slowly between 2004 and 2006, after the identified confounding factors begin to expand drug-related violence. However, the GDP per capita gap only widens dramatically after the implementation of the JOs, when drug-related homicide rates spike. Furthermore, the GDP per capita gap stabilizes after drug-related homicide rates fall. This is readily observable for Chihuahua, where drug-related violence increases and falls drastically. To a lesser extent, this situation also occurs in Durango and Sinaloa, albeit the fall in drug-related violence for these states is more gradual.

Conversely, Guerrero and Michoacán, the poorest states among the treated units, continue to show increasing signs of drug-related violence beyond Calderón's administration. This behavior points to deeper governance issues (e.g. creation of paramilitary groups, political instability, etc.). Consequently, the slope of the GDP per capita gap for Guerrero and Michoacán seems to keep getting steeper. In line with the findings for the Basque and Italian conflict cases by Abadie and Gardeazabal (2003) and Pinotti (2015), respectively, the main effect of drug-related violence on GDP per capita in Mexico occurs with a one-year lag.

5.1.1 Robustness tests

To determine the sensitivity of the findings above, I conduct four robustness tests. First, I expand the matching period up to the year prior to the rollout of the JOs, for each of the treated states. This first robustness check provides additional information for the construction of synthetic controls at the expense of spurious causal and inferential conclusions, due to the presence of confounding factors and spillover effects.

Second, I drop the donor unit with the highest weight from the donor pool for each of the treated states, separately. This falsification test studies whether state-specific effects are only driven by one single donor unit. Nevertheless, when conducting this robustness check, the value of the MSPE in equation (3)

necessarily increases because the optimal set of weights (W^*) becomes unavailable by construction. For instance, Sinaloa’s MSPE increases by half after dropping the principal donor unit from the optimal vector of weights (W^*). Table A.2 in the Appendix section presents the synthetic weights for treated states with an accurate synthetic control.

Third, I add drug-related homicide rates to the set of outcome predictors (X). This robustness check incorporates potentially endogenous structural trends of drug-related homicide rates. Similarly, I include an indicator of the average density-distance to the nearest US border bridge in the set of matching covariates (X). This test seeks to control for the presence of DTOs, which tend to locate closer to the US border, along the drug-trafficking routes.

Table 4 shows the results for all four robustness tests. Overall, the original model is robust to different checks. Statistically-speaking, all t-tests fail to reject the null hypothesis of a zero difference in the average of the treatment effects across models. The previous gives confidence to the original economic model, as well as to the results obtained thus far.

5.2 Inference: placebo studies

As in most comparative case studies, the small number of treated states in the universe and the absence of randomization do not allow the application of large sample inferential techniques. These limitations are common when using the sci-

Table 4: Robustness tests: GDP per capita gap (%) between treated and synthetic control units.

State	Original ^a	Matching period ^b	Maximum unit ^c	Pre-Homicide ^d	Geography ^e
Chihuahua	-13.20	-12.01	-11.93	-11.94	-14.89
Durango	-6.73	-6.91	-6.43	-7.67	-10.99
Guerrero	-3.59	-4.63	-4.57	-3.10	-3.09
Michoacán	-3.37	-3.12	-3.22	-2.70	-5.81
Sinaloa	-3.63	-2.23	1.65	-2.79	-3.63
Average	-6.10	-5.78	-4.90	-5.64	-7.68
T-test (p) [*]	-	0.510	0.327	0.282	0.140

^{*}Ho: difference in means between models is indistinguishable from zero.
^aOriginal model (see results above).
^bSets the matching period from 1993 up to year prior to the rollout of the JO.
^cDrops the donor unit with the highest weight from the donor pool.
^dAdds per capita homicide rates to the original set of matching covariates (X).
^eIncludes the density-distance to the nearest US border bridge in the outcome predictors (X).

entific solution to the FPCI, as opposed to the statistical solution. Therefore, to statistically validate my findings, I apply a couple of “placebo” studies, based on the results of running iteratively the two-step matching procedure on all donor units (previously performed above to obtain the median value of the NMSPE for the full sample).

The set of synthetic controls for donor units allows the construction of placebo effects by taking the outcome difference between untreated states and their respective synthetic controls. As previously suggested by Abadie, Diamond, and Hainmueller (2010), the distribution of placebo effects can be used for the statistical assessment of the treatment effects: If treated states are outliers in the placebo distribution, then the treatment effects are statistically significant (Abadie, Diamond, and Hainmueller 2010).

Figure 3 presents the results for the first placebo test, which consists in comparing the treatment distribution against the placebo distribution, at one point in time during treatment. For this inferential exercise, I drop all donor (and treated) states that attain a matching period NMSPE above the full sample median (7.6%), because these units do not provide reliable information. All gray, solid lines represent placebo effects; while dark, dashed lines depict treatment effects. I emphasize the GDP per capita gap for Chihuahua using a solid line. The shaded area in Figure 3 indicates the treatment period.

Seemingly, all treatment effects in this panel show an odd, negative behavior within the shaded area, in comparison to the placebo effects. By the end of the treatment period in 2012, five of the nine lowest GDP per capita gap lines

Figure 3: GDP per capita gap for donor and treated states with an accurate synthetic control (MSPE < Median).

are treated states. However, the binomial probability of this combination, under equal likelihood of outcomes, is only $\frac{9!}{5! \times 4! \times 2^9} = 0.246$, which does not let me infer causality on all of the treated states *together*, at conventional levels of confidence.

The second iterative placebo test builds p-values from the distribution of post/pre-treatment MSPE ratios to evaluate *individually* the significance of the treatment effects. I obtain the post-treatment MSPE from the squared values in equation (4). This placebo study includes those treated units with an accurate counterfactual, as well as all donor states.

Figure 4 presents the distribution of the post/pre-treatment MSPE ratios. The top panel plots those treated exposed to JOs in 2007, whereas the bottom panel shows states that became treated in 2008. For consistency, the placebo treatment period corresponds to the respective base year of the JOs in each of the panels.

Clearly, both distributions are skewed to the left, with the vast majority of donor states having ratios below 5. Conversely, Chihuahua, Durango, and Guerrero are all outliers in the distribution of ratios. Specifically, Chihuahua and Guerrero have the highest ratios at 19 and 43, respectively. The previous means that, under randomization, the probability of obtaining the highest ratio for either Chihuahua or Guerrero is $\frac{1}{20} = 0.05$. Consequently, the treatment effects for both of these states meet the conventional 5% level of confidence. The same logic applies to Durango, although this state presents an effect that is only statistically significant at the 10% level of confidence: $\frac{2}{20} = 0.10$. Finally, I cannot claim that findings for Michoacán and Sinaloa are statistically significant because neither of these treated states are outliers in the distribution of the post/pre-treatment MSPE ratios.

5.3 Causation: effect of the Mexican Drug War on gdp per capita

Thus far, I have established that a spike in drug-related violence, driven partly by the Mexican Drug War, had an effect on the economy in those states where the federal government implemented JOs. Using placebo studies, I have also proven that, once treated, said effect is statistically significant for the majority of the treated states that display an accurate synthetic control. Moreover, in Section 2, I have acknowledged that the spike in drug-related violence was simultaneously provoked by two foreign confounding factors: the 2004 expiration of the US

Figure 4: Distribution of post/pre treatment MSPE ratios by rollout of JOs (2007 and 2008).

Federal AWB, and a significant increase of cocaine seizure rates in Colombia after 2006. Therefore, the exogenous effect of drug-related violence on GDP per capita is not all a consequence of the Mexican Drug War.

To determine the direct causal effect of the Mexican Drug War on economic development, I run OLS on the variation of the normalized GDP per capita gap for the treated sample. Namely, the central model to evaluate the average treatment effect on the treated (ATT) is the following:

$$\hat{NG}_{s,t} = \alpha + \theta \hat{NG}_{s,t-1} + \beta JO_{s,t-1} + \gamma Z_{s,t-1} + \varepsilon_{s,t}, \quad (6)$$

where $\hat{NG}_{s,t}$ is the normalized GDP per capita gap, in percentage terms; $\hat{NG}_{s,t-1}$ is the lagged dependent variable, which controls for tendency; $JO_{s,t-1}$ is the lag value of my continuous proxy for the Mexican Drug War, the interaction term between the rollout of the JOs and the rate of interception operations; $Z_{s,t-1}$ is a vector with the lag values of the two aforementioned confounding factors – the interaction between the 2004 expiration of the US Federal AWB and a dummy for AWB-bordering states (Chicoine 2011; Dube, Dube, and García-Ponce 2013), and cocaine seizure rates in Colombia (Castillo, Mejía, and Restrepo 2014); and $\varepsilon_{s,t}$ are all other unobservables that influence the outcome. The coefficient of interest in equation (6) is β .

Alternatively, I include state fixed effects (λ_s) in equation (6) to control for possible systemic biases in the (normalized) GDP per capita gap, generated by the SCMs:

$$\hat{NG}_{s,t} = \lambda_s + \theta \hat{NG}_{s,t-1} + \beta JO_{s,t-1} + \gamma Z_{s,t-1} + \varepsilon_{s,t}. \quad (7)$$

However, the conditions for consistently estimating equation (7) are more complicated than OLS because, once state dummies are introduced, the error term ($\varepsilon_{s,t}$) becomes necessarily correlated with the lagged dependent variable ($\hat{NG}_{s,t-1}$). Following Angrist and Pischke (2009, ch. 5), I apply Arellano and Bond's generalized method of moments procedure (ABGMM) to solve for serial correlation.

Finally, I run an additional two-stage least squares (2SLS) model in which the indicator for the Mexican Drug War (JO_{t-1}), along with the two identified confounding variables (Z_{t-1}), enter equation (6) indirectly through exogenous drug-related violence:

$$\begin{aligned} \hat{NG}_{s,t} &= \alpha + \theta \hat{NG}_{s,t-1} + \delta phomicides_{gap,s,t-1} + \varepsilon_{s,t} \\ phomicides_{gap,s,t-1} &= \alpha^F + \theta^F \hat{NG}_{s,t-1} + \pi JO_{s,t-1} + \gamma Z_{s,t-1} + \nu_{s,t-1}, \end{aligned} \quad (8)$$

where $phomicides_{gap}$ is the gap in drug-related homicide rates between treated and synthetic control units. In this specification, the parameter of interest is $\pi \times \delta$.

Having established the mechanics of the minimization procedure in equations (2) and (3), I limit my sample observations from the end of the matching period onwards (2003–2012). I run equations (6), (7), and (8) for all treated states with a reliable synthetic control (Chihuahua, Durango, Guerrero, Michoacán, and Sinaloa), as well as for only those treated states that report a statistically significant GDP per capita gap (Chihuahua, Durango, and Guerrero). All together, my sample contains, at the most, 50 observations. Hence, equation (6) is more likely to provide the true ATT because OLS is consistent and unbiased for small

samples, whereas the Arellano and Bond's generalized method of moments procedure (ABGMM) and 2SLS estimators are only consistent in small-sample asymptotics (Angrist and Pischke 2009, ch. 4).

Table 5 presents the main results for the effect of the Mexican Drug War on GDP per capita gap, in percentage units. Columns 1–3 show the estimations for all treated units that have an accurate synthetic control, whereas columns 4–6 reduce the sample to treated states with a statistically significant GDP per capita gap. The last row in Table 5 presents the ATT of the Mexican Drug War on GDP per capita gap, in percentage terms. For most specifications, the coefficients for the Mexican Drug War and the confounding factors are statistically significant and move in the correct direction. What is more, there is little variation across estimators, implying no need for state dummies.

Given the properties of OLS and the number of states represented in the sample, my preferred specification is column 1. This specification explains around 69.1% of the outcome variation, and indicates a statistically significant ATT equal to -0.7% for Chihuahua, Durango, Guerrero, Michoacán, and Sinaloa, over the period 2003–2012. The 95% confidence interval of the ATT, under robust standard errors, is in the range of -1.4% and 0.4% . If there are zero spillovers, and a perfect linear relationship between the Mexican Drug War and GDP per capita, then an extrapolation of the ATT on all treated states amounts to a loss in GDP per capita equal to 0.5% , over the period 2003–2012. Given the share of treated states in Mexico's economy (over one-third), this is a considerable effect for a single policy, which partially explains the poor performance of Mexico's economy during Calderón's administration.

6 Economic channels

All of these results remain silent about the economic channels by which the Mexican Drug War hampered economic development. Recent literature on the matter suggests a significant effect of the Mexican Drug War on the labor market. Specifically, the conflict provoked a fall of 1.5% in female labor participation rates, and a wage reduction for male workers in the informal sector equal to 2.3% (Dell 2015). By the same token, BenYishay and Pearlman (2013) find a decrease in hours-worked equal to one unit per week as a consequence of the Mexican Drug War.

However, there is not any further empirical evidence on other possibly affected variables. In what follows, I measure the effect of the Mexican Drug War on two unexplored determinants for economic development: consumption and productive investment. Both of these variables are components of the GDP

Table 5: Average effect of the Mexican Drug War on GDP per capita gap (%) between treated states and synthetic controls (2003–2012).

	(1) OLS	(2) ABGMM	(3) 2SLS	(4) OLS	(5) ABGMM	(6) 2SLS
Lag p/drug homicide gap			−0.097*** (0.019)			−0.102*** (0.028)
Lag roll×intercep ops	−10.366* (5.494)	−11.604*** (3.912)		−18.078*** (6.402)	−18.949** (7.891)	
Lag Colombia seizures	−0.016 (0.010)	−0.006 (0.005)		−0.007 (0.013)	−0.000 (0.004)	
Lag border×AWB	−1.999** (0.864)	−2.105*** (0.585)		−1.730** (0.785)	−1.726*** (0.438)	
Lag p/GDP Gap	0.605*** (0.115)	0.572*** (0.159)	0.251** (0.125)	0.659*** (0.123)	0.645*** (0.059)	0.282 (0.199)
FIRST-STAGE						
Lag roll×intercep ops			125.944*** (34.312)			186.172*** (70.983)
Lag Colombia seizures			0.003 (0.062)			−0.023 (0.100)
Lag border×AWB			25.35** (9.733)			20.257** (9.103)
Lag p/GDP Gap			−3.573*** (1.114)			−3.671*** (1.296)
Restricted sample	No	No	No	Yes	Yes	Yes
R-squared	0.691		0.710	0.828		0.795
Number of states	5	5	5	3	3	3
Observations	50	50	50	30	30	30
F-Stat excluded var			10.9			6.0
Drug war ATT (p.)	−0.70	−0.77	−0.82	−1.11	−1.16	−1.16

All regressions contain robust standard errors in parentheses.
Connotations *, **, and *** mean significant at the 90 p., 95 p. and 99 p. level of confidence.

accounting equation, and contribute to economic growth by means of further production (Barro and Sala-i-Martin 1995).

6.1 Consumption

According to Valdés-Castellanos (2013), there is substantial evidence on the diversification in the criminal enterprises that several Mexican DTOs like Loz Zetas and La Familia performed during the Mexican Drug War, moving from drug-trafficking activities exclusively towards a combination of drug-trafficking, extortion, kidnapping, motor vehicle theft, and human-trafficking activities, as to maintain quasi-rents during turbulent times. The logic for a possible decline in consumption as the result of the Mexican Drug War is as follows: If households become victims of drug-related violence, either directly through organized crime or indirectly through fear, then they are likely to hedge their exposure against further violence. In particular, households may avoid consumption activities that placed them at a higher risk of organized-crime victimization, such as going out at night to have fun, or taking public transportation. These possible changes in consumption patterns erode the domestic market because economic resources, previously allocated to consumption, are no longer spent in the same manner.

To test for a decline in consumption, I use nine different cross-sectional waves of the Mexican Crime Victimization Survey, gathered by Mexico's Citizen Security Institute (ICESI) and INEGI. Most of these surveys contain a representative sample of Mexico.¹⁷ In addition, I collect aggregate records for local savings from Mexico's Federal Banking Regulator (CNBV) and the Central Bank of Mexico (BANXICO) to approximate aggregate consumption per capita, which is not available at the state level.¹⁸ This information, together with the previously obtained weights (W) from equations (2) and (3), allow me to build a panel database for treated states and their respective synthetic controls,¹⁹ containing the mean victimization cost,

17 Specifically, ENSI-1 (2001) has a non-representative sample of 35,001 observations, ENSI-2 (2002) a non-representative sample of 35,174 households, ENSI-3 (2004) a representative sample of 66,000 households, ENSI-5 (2007) a non-representative sample of 44,977 households, ENSI-6 (2008) a representative sample of 71,370 households, ENSI-7 (2009) a representative sample of 73,324 households, ENVIPE-1 (2010) a representative sample of 78,179 households, ENVIPE-2 (2011) a representative sample of 95,903 households, and ENVIPE-3 (2012) a representative sample of 95,810 households. All surveys contain sampling weights.

18 Aggregate savings per capita is inversely related to aggregate consumption per capita.

19 Optimal weights for synthetic controls (W) should remain valid for the comparison of household's aggregate consumption because the economic model in Section 4 incorporates demographic variables like human capital and population density.

fear for personal safety, average changes in consumption activities with a high-risk of victimization, and savings per capita (as a measure of aggregate consumption per capita).

Figures 5 and A.2 present graphically the mechanism for a potential drop in consumption, for treated states with a reliable synthetic control. The left (top) panels include the pooled mean victimization cost of extortion, kidnapping, and motor vehicle theft for treated and synthetic control units, in 1993 Mexican Pesos (dashed lines). Similarly, the right (bottom) panels plot fear for personal safety (shaped-lines) and changes in selected consumption activities like going out at night (darkest lines), both as percentages of the population. Clearly, mean victimization cost and fear for personal safety increases radically for all treated states, in relation to synthetic controls. Because households internalize directly and indirectly drug-related violence, consumption activities that put household at a higher risk (e.g. going out at night) decrease. Immediately visible is the strong negative relation between fear for personal safety and consumption.

Table 6 presents numeric evidence for the effect of the Mexican Drug War on mean victimization cost gap, fear for personal safety gap, and savings per capita gap between treated and synthetic control units. Specifically, I run equations (6) and (8) for the normalized values of the aforementioned variables, in percentage terms. All specifications restrict the sample to those treated states with a reliable synthetic control (Chihuahua, Durango, Guerrero, Michoacán, and Sinaloa), for the non-matching period (2003–2012).²⁰ For brevity, I exclude the first stage of the 2SLS estimator.

Findings indicate a statistically significant increase in mean victimization cost, although the spike in “wealth losses” is not proportional to the expansion in drug-related violence. Conversely, the percentage of the population feeling fearful for their personal safety in treated states increases two-fold, compared to synthetic controls. Therefore, during treatment, households internalize drug-related violence indirectly through fear, rather than directly through “wealth losses.” As a result of an increase in mean victimization cost and fear, the gap in savings per capita between treated states and synthetic controls increases by 4.4%, which means that aggregate consumption per capita declines simultaneously.

Extrapolating the results in Table 6 to all 11 treated states reduces the ATT on savings per capita to 2.9%. Considering that savings and consumption rates during the pre-treatment period (1993–2003) for all treated states are 12.6% and

²⁰ To control for tendency in mean victimization cost gap and fear for personal safety gap, I take the values from 2002 in lieu of 2003 because this latter year is not available in the Mexican Crime Victimization Survey.

Figure 5: Mechanism for the effect of the Mexican Drug War on “risky” consumption, for Chihuahua and synthetic Chihuahua.

68.3% of the GDP, respectively, the ATT of the Mexican Drug War on aggregate consumption per capita is equal to -0.5% . This effect is proportional to the effect of the Mexican Drug War on the GDP per capita.

Table 6: Average effect of the Mexican Drug War on victimization cost gap (%), fear for safety gap (%), and savings rates gap (%) between treated states and synthetic controls (2003–2012).

	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS
Lag p/drug homicide gap		2.253* (1.185)		9.030* (5.452)		0.219* (0.126)
Lag roll×intercep ops	261.756 (206.918)		446.357 (2076.954)		66.175** (30.079)	
Lag Colombia seizures	1.906* (1.082)		0.673 (4.187)		−0.048 (0.095)	
Lag border×AWB	47.179 (37.229)		633.421* (323.773)		−18.089** (8.134)	
Lag victim cost gap	0.046 (0.053)	0.076 (0.064)				
Lag fear-safe gap			0.363*** (0.088)	0.358*** (0.082)		
Lag p/savings gap					0.869*** (0.089)	1.075*** (0.074)
Dependent var	Cost	Cost	Fear-safe	Fear-safe	Savings	Savings
R-squared	0.352	0.074	0.463	0.415	0.887	0.846
Observations	30	30	30	30	45	45
F-Stat excluded var		20.8		23.2		8.3
Drug war ATT (p.)	65.04	30.32	29.75	118.61	4.41	2.92

All regressions contain robust standard errors in parentheses. Mean victimization cost and fear for personal safety are factored by inverse survey sampling weights. Connotations *, **, and *** mean significant at the 90 p., 95 p. and 99 p. level of confidence.

6.2 Productive investment

Another possibly affected determinant for economic development is productive investment. To proxy for this determinant, I use data for commercial credit granted to businesses from CNBV and BANXICO instead of gross domestic investment records, because the latter is not available yearly at the state level. Even if commercial credit granted to businesses is not a perfect proxy for gross domestic investments, the fraction of Mexico's capital market controlled by commercial financial intermediaries is crucial to the economy: According to the 2010 National Survey of Financial Competitiveness, conducted by CNBV and the Inter-American Development Bank, about 33% of all formal Mexican enterprises maintain banking loans at any point in time.²¹

²¹ As a caveat, the 2010 National Survey of Financial Competitiveness includes loans from development and foreign banks, which hold less than 16% of the capital market.

Most important, 24% of total commercial credit granted to the businesses goes towards investment.

Having established the influence of bank lending on private investment, I examine a potential decline in commercial credit (non-consumption and non-mortgage) granted to the private sector as a consequence of the Mexican Drug War. Bonaccorsi di Patti (2009) proposes two reasons for a decline in commercial credit granted to businesses after a spike in drug-related violence: 1) The bank's inability to assess the quality of borrowers because of an uncertain propensity to victimization; and 2) a decrease in trust among parties in the domestic financial market.

The previous reasons do not apply to the local public sector because local governments can ultimately be bailed out by the federal government (e.g. lender of last resort). Subsequently, credit could move to the local public sector if the demand for it exists. Notwithstanding this possible substitution effect, productive investment would still decline because public investment, in contrast to private investment, has not been a productive input for GDP since 1982, when Mexico adopted several privatization and decentralization reforms (see Ramirez 2010).

Table 7 contains the result for the effect of the Mexican Drug War on commercial credit (non-consumption and non-mortgage) by sector. Using again the optimal weights (W^*) from above, I run equations (6) and (8) for the gap in commercial credit per capita granted to the private sector, the gap in commercial credit per capita granted to the public sector, and the gap in private-to-total credit ratio between treated and synthetic control units, in normalized terms. Just as in Tables 5 and 6, I limit my sample to those treated states with a reliable synthetic control, for the period 2003–2012.

The first two columns indicate a significant loss in commercial credit per capita granted to the private sector as high as 4.9%, as consequence of the Mexican Drug War. Expanding these results to all treated states reduces the ATT to –3.2%. According to records from BANXICO and the World Bank, commercial credit (non-consumption and non-mortgage) granted to businesses during the pre-treatment period accounts for 6.4% of GDP, whereas private gross domestic investment during the same period of time amounts to 18.5% of GDP.²² Provided that 24% of all commercial credit granted to business goes towards investment, the ATT on productive investment for all treated states is equal to –0.3%. This effect, however, does not account for any changes in privately owned-capital investment.

Finally, there is a positive effect of the Mexican Drug War on commercial credit per capita granted to the public sector, even though the ATT is not statistically different from zero. In fact, there is little or no change on the private-to-total credit ratio, suggesting a null credit substitution effect. All of these findings are

²² These percentages are roughly equal across states.

Table 7: Average effect of the Mexican Drug War on the gap in commercial credit per capita granted to the private sector (%), the gap in commercial credit per capita granted to the public sector (%), and the gap in private-to-total credit ratio (%) between treated states and synthetic controls (2003–2012).

	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS
Lag p/drug homicide gap		−0.351* (0.208)		0.659 (0.450)		−0.054 (0.116)
Lag roll×intercep ops	−37.775 (63.169)		83.970 (154.758)		20.186 (42.871)	
Lag pub credit gap	−0.004 (0.209)		−0.063 (0.422)		−0.167 (0.112)	
Lag border×AWB	−19.582 (14.806)		44.751 (33.077)		−3.539 (5.552)	
Lag priv credit gap	0.799*** (0.111)	0.808*** (0.096)				
Lag pub credit gap			0.888*** (0.134)	0.834*** (0.126)		
Lag priv ratio gap					0.698*** (0.073)	0.681*** (0.069)
Dependent var	Priv credit	Priv credit	Pub credit	Pub credit	Priv ratio	Priv ratio
R-squared	0.843	0.839	0.706	0.682	0.742	0.732
Observations	45	45	45	45	45	45
F-Stat excluded var		12.5		12.1		11.2
Drug war ATT (p.)	−2.52	−4.92	5.60	9.42	1.35	−0.75

All regressions contain robust standard errors in parentheses.

Connotations *, **, and *** mean significant at the 90 p., 95 p. and 99 p. level of confidence.

consistent with the evidence for a lower access to credit in Italy as a consequence of organized crime, found by Bonaccorsi di Patti (2009).

7 Conclusion

The main results in this paper suggest a significant effect of the Mexican Drug War on the GDP per capita for states with military operations equal to −0.5%, over the period 2007–2012. Economic determinants by which the Mexican Drug War hampered economic development include a proportional reduction in consumption, and a decline in productive investment equal to 0.3%. This latter determinant is driven by a drop of 3.2% on commercial credit (non-consumption and non-mortgage) per capita granted to the private sector as a consequence of the military conflict.

Evidently, there would still have been a decrease in the GDP per capita of Mexico (additional to the aforementioned effects) – even without the government intervention–, because of drug-violence caused by an increase of cocaine seizure rates in Colombia after 2006, as well as the 2004 expiration of the US Federal Assault Weapons Ban. Yet, this papers shows how the military strategy fueled drug-violence, provoking a larger negative effect on the GDP per capita of at least –0.5%, for treated states.

Current leaders in Central American countries should not consider hard-handed security strategies as good responses to contain organized crime, if the organizational structure of the criminal enterprises are horizontal like in Mexico. Instead, leaders should concentrate on promoting the rule-of-law, increasing trust in the criminal justice system, and punishing extrajudicial killings as alternatives for justice. Fortunately for Mexico, the criminal justice system reform, approved during Calderón administration in 2008, is finally taking effect. This reform is reducing the percentage of defendants put in pre-trial custody by 20%, dropping the average time to resolve criminal cases from 180 days to 34, and ameliorating prison overcrowding by 70% (The Economist 2016). This sort of institutions are part of the answer to containing organized crime in Mexico, rather than a drug war.

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Appendix

Table A.1: Continuous evidence for the orthogonality in the assignment of treatment.

	(1) OLS	(2) OLS	(3) OLS
Pre-treatment p/GDP	0.003 (0.009)		0.002 (0.009)
Pre-treatment corruption		–0.003 (0.017)	–0.002 (0.017)
R-squared	0.002	0.001	0.002
Number of states	30	30	30
Observations	30	30	30

All regressions contain robust standard errors in parentheses. Connotations *, **, and *** mean significant at the 90 p, 95 p and 99 p confidence level. Sample excludes Campeche and Tabasco.

Table A.2: Synthetic weights for treated states with an accurate synthetic control.

Donor pool [†]	Chihuahua	Durango	Guerrero	Michoacán	Sinaloa
Aguascalientes	0	0.187	0	0	0
Baja California Sur	0	0	0.016	0	0.260
Chiapas	0	0	0.166	0	0
Colima	0	0	0	0	0
Distrito Federal	0.077	0	0.011	0	0.025
Guanajuato	0.323	0.088	0	0.148	0
Hidalgo	0	0	0	0	0
Jalisco	0.219	0.162	0	0	0
Mexico	0	0	0	0	0
Nayarit	0	0	0.099	0	0.520
Oaxaca	0	0	0.690	0.266	0
Puebla	0	0	0	0	0
Queretaro	0.122	0.215	0	0	0
Quintana Roo	0	0	0.018	0	0
San Luis Potosi	0	0	0	0	0
Sonora	0.259	0	0	0	0.019
Tlaxcala	0	0	0	0	0
Yucatan	0	0	0	0.319	0
Zacatecas	0	0.348	0	0.267	0.176

[†]Sample excludes all treated states, Campeche and Tabasco.

Figure A.1: GDP per capita gap for treated states with an accurate synthetic control.

Figure A.2: Mechanism for the effect of the Mexican Drug War on "risky" consumption, for treated and synthetic control units.

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