

A Work Project, presented as part of the requirements for the Award of a Master's degree in
Economics from the Nova School of Business and Economics.

Behind bars and borders
The effect of drug lord extraditions on local violence levels

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Countries suffering from drug-trafficking violence often use decapitation strategies to weaken drug-trafficking organizations (DTOs). Using the relatively new synthetic difference-in-differences estimator, I find that one key tool for decapitation strategies, extraditions, result in increased violence levels in municipalities where the corresponding cartel has already been active. My results are robust to several specifications, transformations and time shifts. The positive and significant average treatment effect on the treated (ATT) highlights the importance of the allocation of public security goods, especially after the extradition of top-level drug criminals.

Keywords

Policy Evaluation, Applied Microeconomics, Development Economics, Violence

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1. Introduction

On May 4th, 2022 the infamous head of the Colombian Clan del Golfo, Dairo Antonio Úsuga “Otoniel” was extradited to the US. This is only one of the more recent cases of the Colombian government’s decapitation strategy against narcotraffickers: to kill or arrest and optionally extradite the organizations’ leaders to weaken the whole organization. However, following the extradition of Otoniel, a major wave of violence hit the country, supposedly as retaliation by his organization, filling even European newspaper headlines (see for example Daniels (2022)). Similar reports followed the extradition of Joaquín "El Chapo" Guzmán from Mexico to the US in 2017, claiming that the extradition is directly related to the rising violence levels (Najar 2017). Extradition to the US poses one of the highest threats to drug cartel kingpins. In the US, they face a trial which almost inevitably leads to a long-term arrest and they cannot use any local power as they would when facing their trial and arrest in their country of origin.

Interestingly, little research has been conducted on the effect of extraditions on local violence levels. Some theoretical models are established and some qualitative research has been conducted, but to the best of my knowledge, there is not a single approach trying to quantify the effects of extraditions. Hence, this thesis aims to explain and quantify violence patterns following the extraditions of top-level criminals in the context of drug cartels in Mexico and Colombia. My prime example is the extradition of El Chapo from Mexico to the US. The reason for choosing this particular extradition is that (1) this case is undoubtedly famous enough for newspaper coverage of potential effects, which will be discussed in the data section and (2) the extradition itself came as a surprise. Even though there were already rumours about it, it was unclear until the extradition day, when exactly the extradition would happen. This prevents the most serious anticipation effects. My hypothesis is that as a result of drug lord extraditions, municipalities where this particular cartel is active face higher violence levels. To get a better

understanding of the external validity, I conduct a similar analysis of an earlier case in Mexico as well as the extradition of 13 paramilitary leaders in Colombia. The institutional context of the two countries is substantially different, providing additional variation which helps to understand the violence patterns even better.

The paper structure is the following: Section 2 reviews the existing literature and provides an overview on the country-specific situations in Mexico and Colombia. Section 3 discusses the data and its sources. Section 4 covers the method and Section 5 presents the estimation results. Finally, Section 6 concludes.

2. Literature Review

2.1. Theory of drug economics

A necessary start to the analysis is to assess why states are willing to extradite criminals to other states and why other states are willing to take them. Becker (1976) pointed out that the three major motives for crime punishment are deterrence, compensation and vengeance. He notes that in many cases an optimal public response in the form of crime punishment would be a monetary fine since it reimburses the victim for its loss. When it comes to drug-trafficking crime, monetary fines are undoubtedly inappropriate measures as this particular kind of crime yields high revenues and the state can at best roughly estimate how much money an organization has already earned through a certain practice. Hence, fines are not suitable to deter criminals from their illicit activities. Another option is imprisonment, which is often done in the case of drug criminality. A major drawback of imprisonment is that the general public has to pay for the punishment of the criminals since guards and food cost money (Becker 1976). Furthermore, the deterrence aspect of imprisonment in the home country might be neglectable. Local kingpins likely have high power, not only inside but also outside their organizations. This can be seen in the glorification of narcoculture in popular media or through tourism (Neaf 2018). Hence, it is fair to assume that drug kingpins will not be entirely cut from their

organization and from operating business by imprisonment. Even high-security prisons will offer the chance for high-profile criminals to circumvent usual security measures, as proved by two prison escapes by El Chapo.

A policy which combines strong deterrence, low social costs of imprisonment and cutting the kingpin away from their organization can be extradition. Theoretically, this could minimize the social loss from law enforcement particularly because another state covers the cost.

Assuming governments act rationally, the state to which the kingpin was extradited must expect the benefit of bringing a foreign criminal to trial and prison to exceed the costs. Potential benefits for the US are numerous. First and foremost, illicit drugs cause costs for the economy. For the year 2010, the economic costs alone are estimated at around \$215 billion. Furthermore, drug abuse in the US affects numerous aspects of everyday life such as healthcare, crime, productivity and the environment, to name just a few examples (National Drug Intelligence Center 2010). A potential channel from extraditions to drug-related impact on society is that the extraditions weaken the organizations, which leads to a decrease in production and/or smuggling activity, eventually decreasing drug supply to the US.

Hence, extraditions are in theory suitable to minimise social loss from law enforcement in the extraditing countries as well as bringing benefits to the countries to which the kingpin was delivered. From the aspect of the drug trafficking organization (DTO), while imprisonment in the home country can be seen as a temporal and potentially incomplete cut between the kingpin and the DTO, extraditions are a permanent cut between the kingpin and the DTO. To better understand how this might affect local violence, it is necessary to dive deeper into development economics' literature on conflict.

2.2. Classic literature on conflict

Numerous studies tried to link economic conditions with violent outcomes. For example, Collier and Hoeffler (1998) find that increased per capita income decreases the duration of civil war as with higher initial income, people have more to lose in the conflict. This is an early mention of the opportunity-cost channel of conflict. Consistent with the opportunity cost channel, Collier, Hoeffler and Söderbom (2004) find lower per capita income to lengthen violent conflict. Additionally, Fearon (2004) argues conflicts with rebels enjoying financing sources such as the drug trade, tend to last longer. Hence, low income and high potential revenues such as drug revenues not only increase the probability of violent conflict occurring but also the length of the violence.

An important strand of the conflict literature focuses on the resource curse. This phenomenon leads to states ending up experiencing violence and/or impoverishment specifically because of their abundance of (natural) resources such as oil or minerals. In the Latin American context, I believe drugs offer similar characteristics since coca is (1) easily lootable (2) geographically constrained in where it can be grown (3) an attractive good from a criminal's point of view as it is relatively cheap to produce and offers high profit margins.

The mechanism of the resource curse is that dependence on natural resources shifts labor and capital away from other sectors. This leads to vulnerability to commodity price shocks, slower economic growth and increased poverty rates. Resource-dependent governments also tend to be more corrupt, weaker, and less able to resolve internal conflicts. This incentivizes grievances and violent insurrections financed by looting natural resources. Drugs as a financing source are argued to have become increasingly important after the end of the Cold War when foreign direct financing by governments hostile to the central state collapsed and rebels replaced this income source with the income generated by the illicit drug revenues (Ross 2003). This argument coincides with the shifting of rebel groups towards drug trafficking in Colombia.

An outstanding contribution to the conflict literature is the theoretical model of the impact of economic shocks on civil conflict by Dal Bó and Dal Bó (2011). Extending traditional general equilibrium models by imperfect property rights, their model shows that positive income shocks do not unanimously reduce conflict incidence, but that positive shocks to capital-intensive sectors increase violence by increasing the amount of lootable resources. This is consistent with the opportunity cost explanation, but not with the results of Miguel, Satyanath, and Sergenti (2004) who found that higher income decreases conflict in sub-Saharan Africa. Dal Bó and Dal Bó's (2011) theoretic finding was confirmed empirically by Dube and Vargas (2013) considering exogenous price variation in oil and coffee in Colombia. They found a rapacity effect in the case of an increase in the oil price, so that the value of appropriable resources increases and the classic opportunity cost effect when the price of coffee increases by making labor outside the rebellion sector more attractive and therefore cutting labor-supply to rebels. This pattern seems to hold for different sectors proxying labor- and capital-intensive sectors as well as for different geographic settings, as confirmed by Berman et al. (2017) who look at the whole African continent, finding similar results.

Closer related to my specific research question, there is some evidence that improved law enforcement leads to higher levels of violence in drug markets (Werb et al. 2011; Osorio 2015) as well as of criminal groups in general (Smith and Varese 2001). Werb et al. (2011) review the literature on the link between drug law enforcement and drug violence, finding drug prohibition tends to increase drug market violence. They also argue that drug law enforcement removes key players from the drug market, thereby creating an incentive for new actors to enter the market and fill the vacuum. Since this is associated with high revenues and illicit industries rely more on violence rather than contracts and courts to resolve issues, the link between law enforcement and violence becomes more obvious. Similarly, Osorio (2015) finds for Mexico

that law enforcement in general and violent law enforcement in particular intensifies violence between competing DTOs.

Extraditions are expected to eventually cap the connection between an arrested kingpin and the DTO. One can expect that top-tier criminals can use their local power within their state of origin to still manage, at least to some extent, the operations of the organization. This limits their set of possible actions, but it is expected that an arrest alone does not remove a person entirely from the rest of the group. Extraditions provide this additional step. Hence, I expect, in line with much of the presented literature, both inter- and intra-group violence to increase in the time following the extradition.

2.3. Country-specific context: Mexico

Typically, the literature focusses on the rise in violence in Mexico in the 2000s, following a period of a political de-facto monopoly by the Partido Revolucionario Institucional (PRI). During this era of little political competition, DTOs existed and shipped high amounts of drugs to the US. Despite this, drug-related violence was low in Mexico. Several papers argue that state sponsored protection rackets allowed DTOs to operate relatively unchallenged by the state and new competitors, as state officials protected certain DTOs, who in turn allocated a share of their profits towards state officials (Snyder and Durán Martínez 2009; Morris 2013; Trejo and Ley 2018). This contrasts the widely accepted view that violence and drug trafficking are inherently connected (Morris 2013). Violence remained low until political competition came into play. Snyder and Durán Matínez (2009) argue that competition among parties shortened the time horizon and increased uncertainty for state actors and DTOs, eventually leading to a collapse of the protection rackets. Without protection rackets, DTOs relied on violence to defend their market share against new competitors (Morris 2013). This is one explanation for the increased violence *between* cartels. Additionally, the kingpin removal can incentivise competing DTOs to increase their territory and attack the weakened DTO. Osorio

(2015) provides a theoretical model for this argumentation and claims that this kind of state intervention alters the military equilibrium between DTOs, creating an incentive for other DTOs to attack weakened competitors and therefore increasing inter-cartel violence. Moreover, the kingpin strategy can foster fragmentation as after the decapitation of an organization, smaller groups can easily split up and do business on their own. Beittel (2013) found that this led to higher violence levels in Mexico, most likely because the splitter group represents a new entrant to the market, fighting for market shares. This would be in line with Osorio's (2015) findings that the number of DTOs within a municipality is connected to higher levels of violence due to increased competition among DTOs. This is different in Colombia where most DTOs have political goals apart from profit maximization, which is not the case in Mexico.

However, inter-cartel violence is only one potential type of violence. The literature argues that the kingpin-strategy led to high levels of violence both *between* and *within* DTOs. The kingpin strategy aimed at arresting and killing top-tier DTO leaders, thereby decapitating the organizations. Theoretically, this could reduce violence by reducing the business competence of the organization (Shirk and Wallman 2015). Conversely, destabilizing the DTO is widely argued to increase rather than decrease the violence in areas, where the group operates (Werb et al. 2011; Beittel 2013; Osorio 2015; Shirk and Wallman 2015). This can be through succession wars or increased distrust within the group due to the increased likelihood of arrests (Shirk and Wallman 2015), i.e. intra-cartel violence.

A third potential channel is the one of profitability as pointed out by Shirk and Wallman (2015) and is related to the industrial organization of DTOs. They argue that Mexican DTOs increased their leverage along the drug supply chain from being mere couriers to distributors of the drugs, which went along with increased profitability over time. According to basic economic theory, this provides an incentive for new market entries and also increases the resistance of existing DTOs to new competitors. While legal enterprises can solve their disputes

through contracts or eventually courts¹, firms in the illicit sector tend to rely on violence. This channel can be linked to the opportunity costs channel in the classic conflict literature. Even though a rational agent will incorporate aspects like risk and aversion to violence, the outside option may be bad enough for the agent to join the DTO.

The three theoretical channels described above indicate that more political competition, the kingpin strategy and the increased profits of DTOs contributed to the rise in drug-related violence in Mexico after 2000. However, complementary explanations exist. For example, Dell (2015) emphasizes spillover effects in local violence levels. She utilizes a network model of trafficking routes to demonstrate that as law enforcement increases in one geography, DTOs shift to alternative routes, carrying violence to those geographies. More potential factors include institutional weaknesses (Morris 2013), geographic location of the municipality (Shirk and Wallman 2015; Dell 2015), partisan rivalry at different stages of the government (Trejo and Ley 2016) and socioeconomic variables like poverty rate or population size (Gutiérrez-Romero and Oviedo 2018).

Some of the presented studies include extraditions in their definition of law enforcement. However, to the best of my knowledge, no study has focussed exclusively on this effect.

2.4. Country-specific context: Colombia

The foundation of the first leftist guerrilla groups in Colombia was driven by political motives and land contention in the 1960s. The two biggest and most famous guerrillas are the Fuerzas Armadas Revolucionarias de Colombia (FARC) and the Ejército de Liberación Nacional (ELN). As a protection strategy, landowners hired private militias to defend their property, these groups are later known as paramilitaries. Later in the 1980s, the paramilitaries

¹ Of course, businesses following legal activities cannot prevent new market entries by contracts, but they typically have a strong preference to settle a conflict legally rather than violently

were hired or created by DTOs to defend their properties and guard their activities. It was also at this time that the guerrillas shifted towards the drug business to generate revenues to finance their insurgency (Grajales 2011; Krakowski 2015).

The third actor in the Colombian conflict is the state. A large body of research investigates the connection or at least tolerance of the state and the paramilitary groups (Avilés 2006; Krakowski 2015; Dube and Naidu 2015; Maher and Thomson 2018) as well as human right violations by the army, for example the false-positive scandal (Acemoglu et al. 2020).

The industrial organization of the Colombian drug market evolved from a de facto duopoly of the Medellín and Cali cartel towards a more fragmented market structure with numerous groups fighting for market shares (Werb et al. 2011). Examples of drug trafficking groups are not only traditional cartels, but also political groups such as left-wing guerrillas (FARC, ELN) and right-wing paramilitaries as (AUC, Clan del Golfo) who finance their fighting activities to some extent with illicit drug trafficking. This is a substantial difference from Mexico, where Cartels do not generally follow political goals.

Even though guerrillas and paramilitaries never had the primary goal of controlling the drug trade, they generated significant revenues by acting in this sector, thereby financing their operations (Angrist and Kugler 2008). Subsequently, the political insurrection by left-wing guerrillas and the counter-strategy by right-wing paramilitaries became more and more woven with the drug trade. For example, the FARC and ELN generated up to 84% and 66% respectively from drug trafficking and extortion (Suárez 2000). Similarly, paramilitaries generated up to 70% of their income through drug trade (Holmes et al. 2021). Consequently, the drug-related violence in Colombia can neither be seen as purely political, nor as purely related to organized crime. In effect, both play together and are indistinguishable from each other. Hence, it is neither necessary nor appropriate to divide violence between drug market-related or as part of the civil conflict.

Closely related to my research question is the study by Krakowski (2015) who analyses trends in paramilitary violence in western Colombia after the DDR process in 2003. He finds that paramilitaries contested by state officials are more violent, either to replace nonviolent resources or to react to the intensification of rapacious trends within their groups. Extraditing DTO-kingpins is a specific kind of state repression of violent groups and hence suitable to increase violence levels..

For further information on the Colombian context, refer to [Appendix 1](#).

3. Data

Since extraditions are not disclosed publicly, I only use top-level cases with sufficient media coverage. To avoid overlapping treatments, I use a small number of extraditions to increase the internal validity of my results. The three cases I focus on appeared as a surprise in timing to circumvent anticipation effects. The main case is the extradition of Joaquín "El Chapo" Guzmán in January 2017. The two other cases I provide estimates for are the extradition of Benjamín Arellano Félix in 2011 and the extradition of 13 paramilitary leaders from Colombia in 2008.

Crime and violence rates in both countries are high and the clearance rate is low (Shirk and Wallman 2015). As pointed out by Ríos and Shirk (2011), this makes the estimation of any policy on a concept like “drug-related violence” subjective. In both countries, government data exists but is not particularly reliable. Moreover, large violence cross-country datasets like ACLED or UCDP tend to underestimate violence, especially in the Colombian context as pointed out by Restrepo, Spagat and Vargas (2006). An alternative source in both countries are newspapers which typically provide statistics on violence, but not at the granular level required for this research design.

Hence, I make use of two modern datasets. For Mexico, I use the Organized Criminal Violence Event Data (OCVED) by Osorio and Beltrán (2020). Using machine learning and

natural language processing, the authors combine various news reports and governmental data from a total of 105 sources. Through the inclusion of local newspapers and governmental sources I believe that concerns regarding media coverage bias are minimized. I believe that the focus on top-tier criminals who are connected to big DTOs further increases the confidence of the data as I expect big DTOs to be better covered in media reports than smaller organizations. The dataset covers the years 2000-2018 and ten major DTOs. For descriptive statistics, consider [Table A 1](#) in [Appendix 2](#).

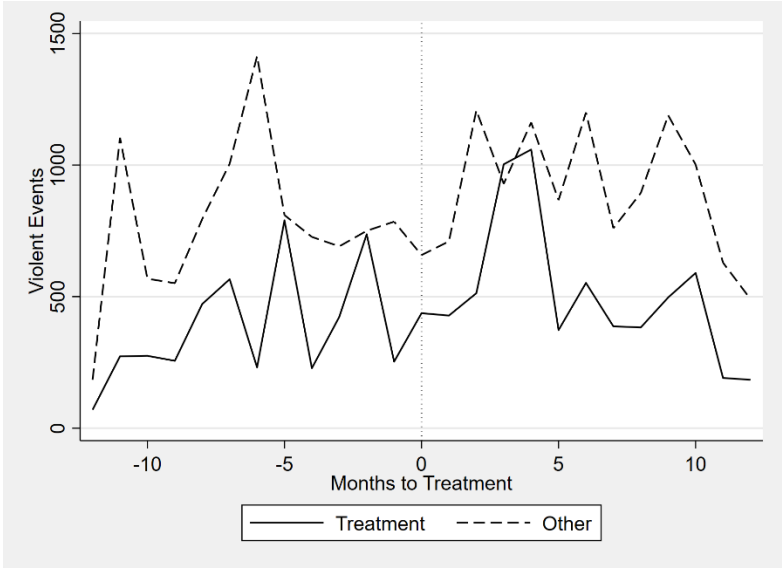
This dataset offers event-based information with the exact coordinates, similar to ACLED or UCDP. In [Figure A 1](#) in [Appendix 2](#), I provide a comparison of OCVED and UCDP cases to give the reader an idea about the difference. I use UCDP rather than ACLED due to Eck's (2012) finding that UCDP tends to be more appropriate in subnational settings. The main drawback of OCVED is that it only provides information about a violent event, without further specifying the type of violence. The newspaper articles which form the basis of the dataset of Osorio and Beltran (2020) cover mentions of clashes between DTOs, drug confiscations and the capture of top-level targets. Hence, drug violence in the dataset is a fairly broad concept. Furthermore, the authors find that DTOs are present in a large part of the country, however, this does not mean that drug violence is evenly distributed. For the spatial patterns of violence, consider [Figure A 3– A 7](#) in [Appendix 2](#).

For Colombia, I use the Violent Presence of Armed Actors in Colombia (ViPAA) a similar dataset provided by Osorio et al. (2019). The data collection is akin to the Mexican dataset. The usage of similar datasets increases the comparability of the results. This dataset covers years from 1976 to 2019 and 99 distinct groups, consisting of paramilitaries, guerrillas, state actors and smaller criminal groups.

Preliminary evidence suggests that the extradition of El Chapo indeed leads to a modest increase in violent events where the cartel is active, as indicated by [Figure 1](#). I do not rely on

external information on the cartel’s territory, since the raw data includes information on the actor of each event. Hence, I can assign a dummy variable on the presence of the Sinaloa cartel to each municipality in Mexico, based on whether there has been a violent event conducted by the cartel in the year before the extradition [Figure A 2 in Appendix 2](#) plots the treatment regions geographically.

Figure 1: Violence levels in Mexico in the study period



Note: The Figure plots the accumulated monthly number of violent events for two groups: municipalities where the Sinaloa cartel was active the year before the extradition (Treatment) and all other municipalities (Other).

Unfortunately, the data includes information on who was the actor, but not who was the victim of an event. Through the assignment of municipalities to the Sinaloa Cartel’s territory, I can still include the potential additional violence committed by competing cartels, when they attack the Sinaloa cartel, as described by Osorio (2015). Moreover, it allows to include violent events where the actor is unidentified. If I only used the number of events per group, those events would have to be dropped. Hence, I hope to capture all, even incompletely coded events of violence, however at the cost of precision of the exact mechanism of the violence.

Following the literature reviewed and the preliminary data, I hypothesize that the extradition of drug lords leads to an increase in violence in the months following the event in

the cartel territory. Moreover, I expect violence to expand spatially, meaning that I expect a rise in the probability of a municipality experiencing violence for treated municipalities.

4. Method

An intuitive policy evaluation method to assess the impact of a certain treatment D is a difference-in-differences (DID) setting. To account for heterogeneous treatment effects over time, one can include a dummy variable for each year in the study period to track the causal effect over time. My empirical specification is the following:²

$$\operatorname{arcsinh}(Y_{it}) = \alpha_i + \delta_t + \sum_{\tau=1}^T \beta_{\tau} \times \mathbb{1}(D_i = 1) \times \mathbb{1}(t = \tau) + \varepsilon_{it} \quad (1)$$

where $\operatorname{arcsinh}(Y_{it})$ is the number of violent events in observation i at time t , transformed by the inverse hyperbolic sine transformation; α_i and δ_t denote individual and time fixed effects, respectively; $\mathbb{1}(D_i = 1)$ is an indicator variable whether observation i belongs to the territory of the kingpin's cartel or not and $\mathbb{1}(t = \tau)$ is an indicator variable for each time period. Hence, the interaction of the indicator variables provides a dummy whether observation i is treated or not in period t . Finally, ε_{it} is an error term, clustered on the municipality level. The main assumption of DID-style event studies is that treatment and control observations follow parallel trends over time before the treatment occurs and would follow this trend after treatment, in the absence of treatment. In this context, treatment and control municipalities have to face the same trend in violence before the treatment and we should believe this trend would also hold after treatment if treated municipalities were not treated. Phrased differently, in DID settings, the underlying identifying assumption is that level differences between treated and non-treated observations are time-invariant in the absence of treatment, but trends over time are common to both groups (Angrist and Pischke 2009).

² As indicated in Table 1, I also use models where I exchange $\operatorname{arcsinh} Y_{it}$ for $\ln(Y_{it} + 1)$ as dependent variable.

This often leads to an arbitrary choice of control observations. Moreover, in my examples, violence patterns in treatment areas behave differently than in intuitive control areas. Hence, the parallel trends assumption is violated and a DID-style event study would not deliver credible causal estimates.

A potential remedy is the synthetic control (SC) method, initially proposed by Abadie and Gardeazabal (2003), which has been argued to be “the most important innovation in the policy evaluation literature in the last 15 years” (Athey and Imbens 2017). Contrary to traditional DID estimation, it is a data-driven procedure for the choice of the control group. The main idea behind the SC is to assign weights to all potential control observations in the donor pool. To avoid extrapolation, it is standard to restrict weights to be nonnegative and sum to 1. The algorithm proposed by the authors finds the weights, such that a weighted average of the observations in the donor pool most closely resembles the treated observation before treatment not only in trends but also in levels. This is usually done by minimizing the preintervention mean squared prediction error (MSPE) (Abadie, Diamond, and Hainmueller 2010).³

However, the SC method is usually employed with aggregate data where a single unit (or a small number of units) is treated. My data structure allows to use disaggregate data on the municipality level, which results in a large number of treated observations. To make use of this structure, I use an estimator combining SC and DID, proposed by Arkhangelsky et al. (2021). Contrasting pure SC, their synthetic difference-in-differences (SDID) estimator allows level differences between the treated and the synthetic control group. Intuitively, the observations in the donor pool are weighted not to resemble the treated units as in SC, but to resemble the treated units’ trend. The resulting synthetic control group therefore provides a credible control group for a DID analysis, if one believes the synthetic control to be an adequate counterfactual

³ For an application of the SC method to organised crime, consider Pinotti (2015), who estimates the impact of mafia emergence on economic development in two regions in southern Italy in the 1970s.

in case the treated observation was not treated. Therefore, the weights should indicate structural parameters, which do not change in absence of treatment (Pinotti 2015).

Formally, similar to the pure SC method, the SDID estimator seeks to minimize the MSPE in the pre-treatment period. The most important difference is the inclusion of unit fixed-effects α_i and a weight for time periods $\widehat{\lambda}_t^{sdid}$ allowing more similar time periods to receive higher weights:

$$(\hat{\tau}^{sdid}, \hat{\mu}, \hat{\alpha}, \hat{\beta}) = \underset{\tau, \mu, \alpha, \beta}{argmin} \left\{ \sum_{i=1}^N \sum_{t=1}^T (Y_{it} - \mu - \alpha_i - \beta_t - W_{it\tau})^2 \widehat{\omega}_i^{sdid} \widehat{\lambda}_t^{sdid} \right\} \quad (2)$$

Hence, SDID is a flexible combination of SC and DID estimation. Furthermore, Arkhangelsky et al. (2021) show that the estimator is consistent and asymptotically normal, allowing the use of conventional confidence intervals⁴.

To allow the pre-treatment period to be long enough for the SDID algorithm to properly construct a synthetic control group, I set the timeframe before the treatment to one year. Following the study conducted by Dell (2015) the post-treatment period is also set to one year to account for potential short- and medium-term effects. I refrain from a longer study period to avoid the unintentional inclusion of structural breaks in the control observations, which might bias the estimates considerably (Abadie 2021). Since I aggregate observations at the monthly level to avoid overfitting, this gives me $T_{pre} = 12$ pre-treatment and $T_{post} = 12$ post-treatment periods. Further below, I show that the coefficients are robust to small changes in the treatment periods. To further smoothen the pattern of violence, Osorio (2015) uses the natural logarithm of the count of violent events. Since my observations include zeros, I shift the events by 1 to accommodate zero values. However, as this practice is criticized, I provide estimates where the

⁴ In my preferred specification, I estimate the standard errors applying a bootstrap procedure with 5000 replications.

dependent variable is transformed applying the inverse hyperbolic sine transform⁵ (Johnson 1949; Burbidge, Magee, and Robb 1988; Woolley 2011; Bellemare 2018). Since this method has also gained criticism recently (Mullahy and Norton 2022; Chen and Roth 2023), I provide estimates for both approaches. The fact that they are not sensitive to the choice of the transform increases confidence about the true treatment effect. Notably, since the point estimates after the transformation depend on the units of the dependent variable, they are not interpretable as percentage approximations (Chen and Roth 2023). Therefore, I stick to an interpretation of log points.

Given my data structure, I create a balanced panel where the unit of observation is the municipality. However required for the SDID, it is noteworthy that the balanced panel structure rules out the possibility of compositional changes, which is a common caveat in traditional DID settings (Clarke and Schythe 2020). I define the outcome variable of interest to be the number of violent events in municipality i at time t . Descriptive statistics can be found in [Table A 1](#).

5. Results

5.1. Violence Intensity

The main result can be found in [Table 1](#). With a post-treatment period of 12 months, depicted in column (1), I find that the extradition of El Chapo leads to a 0.102 point rise in the logarithm of the number of violent events in treated municipalities. Restricting the post-treatment period to 6 months, the estimate increases to 0.16 log points. The results are significant at the 5%-level. The estimates increase slightly but are in a similar range when using the inverse hyperbolic sine of the count of violent events as the dependent variable. To demonstrate differences, I also include the estimate of a pure SC method in columns (5) and (6) of the table. Using a pure SC, the estimated effects are slightly below the point estimates using

⁵ The inverse hyperbolic sine transform is as follows: $\text{arcsinh}(y) = \ln(y + \sqrt{y^2 + 1})$

the SDID estimator. Overall, the estimates are not sensitive to the choice of the transform of the outcome variable. However, notably, the effects become insignificant when the SC estimator is used.

Table 1: Estimated Treatment Effects on violence intensity

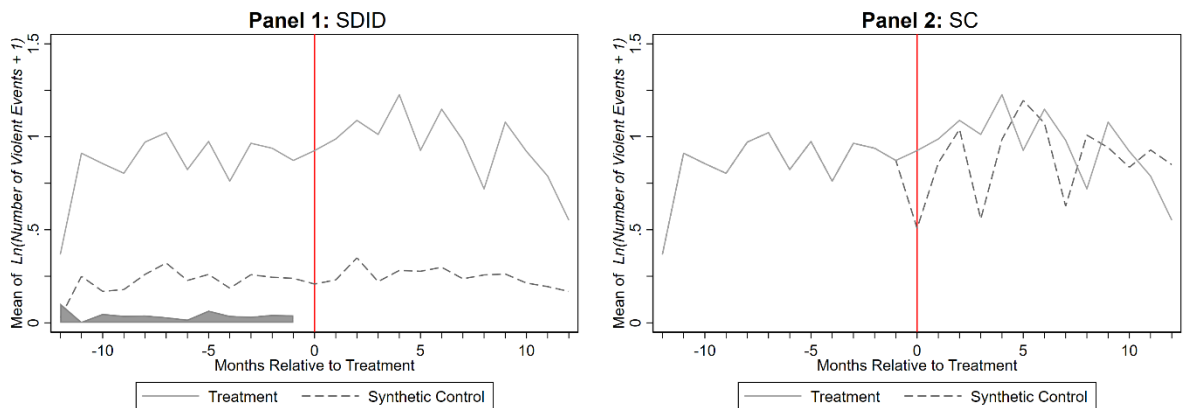
Dep. Variable	Ln($Y_{it} + 1$)		arcsinh(Y_{it})		Ln($Y_{it} + 1$)	
	(1)	(2)	(3)	(4)	(5)	(6)
Estimator	SDID	SDID	SDID	SDID	SC	SC
ATT	0.102** (0.0459)	0.160*** (0.0518)	0.119** (0.0558)	0.189*** (0.0626)	0.0727 (0.0924)	0.157 (0.100)
# post-treatment periods	12	6	12	6	12	6
Observations	24525	18639	24525	18639	24525	18639

Note: This Table shows the estimates from Equation (1) in different specifications. Columns (1), (3) and (5) are for a post-treatment period of 12 months while columns (2), (4) and (6) restrict the post-treatment period to 6 months. Columns (1) and (2) have the natural logarithm of the number of violent events as dependent variable, columns (3) and (4) have the inverse hyperbolic sine of the number of events as dependent variable. Columns (1) – (4) are SDID estimates while (5) and (6) are SC estimates. Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 2 visualizes two specifications. In the left panel, I plot the time series of the treatment municipalities, together with the synthetic control using the SDID method as indicated in column (1). In the right panel, I provide the results for the pure SC method from column (5).

Figure 2: Treatment and Synthetic Control Time Series of Violence



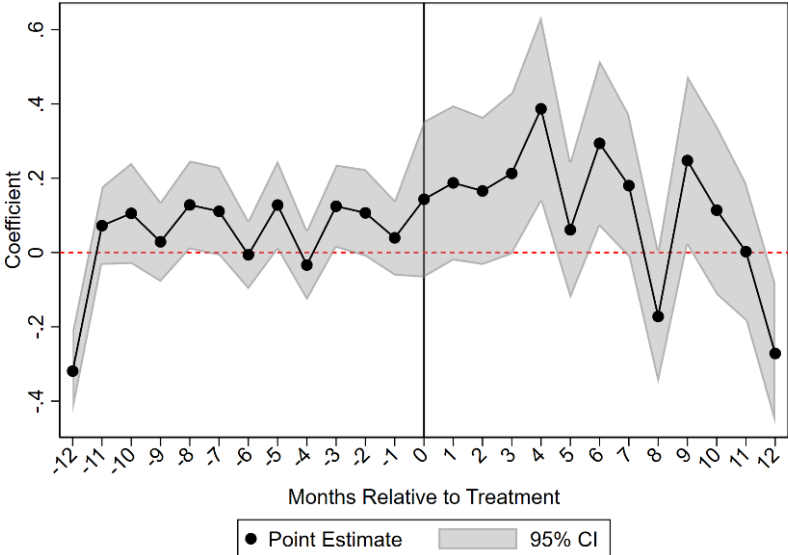
Note: The figure plots the number of violent events. In the left panel, it plots the treatment and control municipalities using the SDID method equivalent to column (1) in Table 1. In the right panel, a SC method is used to construct the synthetic counterfactual, equivalent to column (5) in Table 1. The grey area in the pre-treatment period of Panel 1 indicates the time weights assigned by the algorithm.

As can be seen from the graphs, the algorithm performs well in terms of fitting the pre-treatment patterns. Furthermore, one can see a clear deviation once the assigned municipalities

are treated. The rise in the coefficient in columns (2), (4) and (6) of [Table 1](#) suggests that estimates are heterogenous over time. To account for this fact, and to follow the recommendation in Roth et al. (2023) [Figure 3](#) plots the evolution of the coefficient in an Event-study plot as proposed by Clarke et al. (2023). This also allows to visually convey the parallel trends assumption, which is key for a causal interpretation of the estimates as ATT.

As one would expect, the coefficients for the pre-treatment period are close to 0. Even though some deviations are observable, the CIs seldomly deviate from the 0-line of no effect. Given the volatility of the outcome variable, I believe these small deviations in the pre-treatment period to be of little concern for the overall picture of the study. The pattern the plot reveals is one of two (significant) waves of violence following an extradition: one immediate for the first four months after the treatment and a second one roughly half a year after the treatment.

Figure 3: Event-Study plot of the treatment effect on violence intensity



Note: Contrary to regular Event Study plots, this plot considers pre-treatment aggregates as baseline periods (remember that SDID assigns weights to time periods as well). Therefore, there is not a single pre-treatment period normalized to 0, but the coefficients are relative to the whole weighted pre-treatment period.. For details, consider, Clarke et al. (2023).

The coefficients show an upward trend after the extradition, however it is insignificant until the 4th month after the event. I interpret the slight increases in the point estimates to capture the immediate reaction of DTOs towards the state, which typically fill the newspapers (Najar 2017; Daniels 2022). I believe succession wars within the cartels to be a part of this first wave of violence as well. A reason might be that, given the immediate weakening of a DTO, the remaining members have an incentive to fill the power vacuum. However, most likely, they will not immediately agree on one or multiple successors. Rather, I believe, the immediate reaction provokes a wave of intra-cartel violence where potential successors fight for power and influence, leading to an increase in violence where the Sinaloa cartel is active. However, noteworthy, these effects are insignificant meaning that they might be less severe than media coverage suggests. The medium-term effects roughly half a year after the extradition are in line with theoretical explanations by Osorio (2015). If the extradition and the weakening of the Sinaloa Cartel indeed came as a surprise, as media coverage suggests (Williams 2017), competing DTOs might need time to adjust their resources and operations to be able to attack the weakened cartel. This time is likely required to adjust the workforce and raise capital to violently compete for market shares and attractive trafficking routes.

To understand better the external validity of my results, I conduct a similar analysis for two other extraditions. Firstly, I look at one major extradition in Colombia. On May 13, 2008, as a surprise act, Colombia extradited 13 paramilitary leaders to the US. Among the extradited were high-profile criminals such as Salvatore Mancuso and Rodrigo Tovar Pupo, who were both involved in massacres against civilians, narcotrafficking and who together shared the command of the Northern Block of the AUC (*El Tiempo* 2006). Secondly, I look at the extradition of Benjamín Arellano Félix, operational head of the Tijuana Cartel, who was sent to the US in 2011. The results are reported in [Table A 2](#) and [Table A 3](#) in [Appendix 2](#). Interestingly, the Mexican extradition yields similar ATTs to the one of El Chapo, but the

Colombian one has no effect. This might be due to the different objectives and the industrial organization of DTOs in the two countries. As mentioned further above, Colombian cartels usually follow not only monetary but also political objectives. Hence, weakening a competitor might be less attractive compared to Mexico, where the reason for inter-cartel violence is mainly the fight for market shares.

5.2. Violence Incidence

In a complementary analysis, I do not focus on violence intensity, but on violence incidence. Therefore, I assign each municipality a binary variable $violence_{it}$ which is equal to one if the number of violent events y in municipality i in month t is larger than zero.

$$violence_{it} = \begin{cases} 1 & \text{if } y_{it} > 0 \\ 0 & \text{otherwise} \end{cases} \quad (3)$$

The unconditional probability of experiencing a violent event in a given cell-month combination for a study period of 6 months prior to treatment and 10 months after treatment is about 21%. As can be seen in [Table 2](#), conditional on treatment status the probability becomes 49.3% and 14.6% pre-treatment and 53.8% and 17.2% post-treatment for treatment and control municipalities, respectively. I am interested in whether the extradition causes a spatial expansion of violence. In other words, I am trying to estimate whether the extradition leads to more municipalities being affected by violence.

Table 2: Conditional Probabilities of a municipality experiencing a violent event

Municipality	Treated	Control
$Pr(violence_{it} = 1 Post = 0)$	0.493	0.146
$Pr(violence_{it} = 1 Post = 1)$	0.538	0.172
N	140	841

Note: N refers to the number of municipalities classified as treatment and control observations.

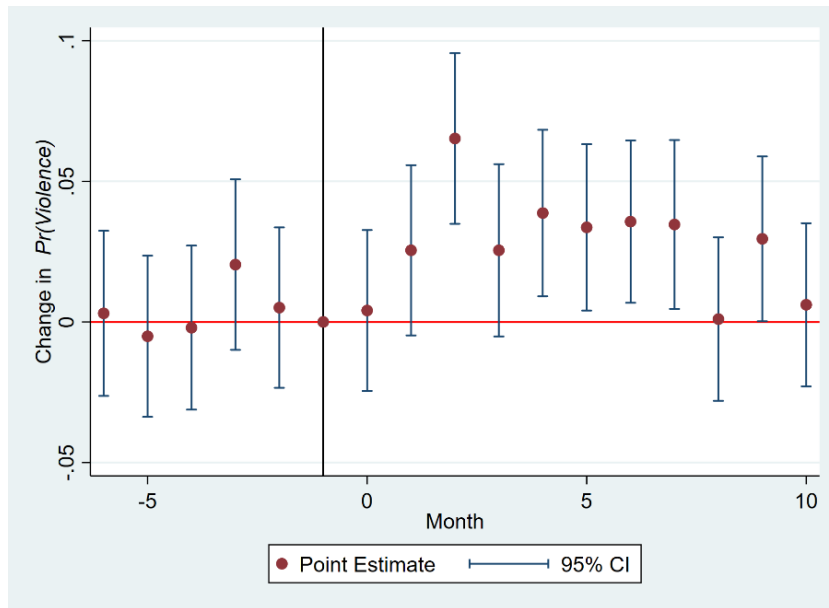
For the ease of interpretation and since the values for $violence_{it}$ are not close to 0 and 1, I stick to a LPM of the following form (Clarke and Schythe 2020):

$$violence_{it} = \alpha + \sum_{j=2}^J \beta_j (Lead\ j)_{it} + \sum_{k=1}^K \gamma_k (Lag\ k)_{it} + \mu_i + \lambda_t + \varepsilon_{it} \quad (4)$$

with an intercept α , $J = 6$ pre-treatment periods, $K = 10$ post-treatment periods and individual and time fixed-effects μ_i and λ_t , respectively. Since no population statistics were available on the municipality-month level, I could not include time-variant control variables. Finally, ε_{it} is an idiosyncratic error term. The standard errors are clustered at the municipality level.

The results can be found in [Figure 4](#). For completeness, [Table A 4](#) in [Appendix 2](#) shows the exact coefficients. Here, I focus on the classic event study plot.

Figure 4: Event-Study plot of the ATT on violence incidence



It can be seen that again, there is some variation before the intervention. However, the pre-event point estimates are always sufficiently close and never statistically different from 0. Furthermore, conflict incidence tells a similar story to conflict intensity with some remarkable differences. Similar to conflict intensity, I cannot find an immediate significant effect on violence incidence. This suggests that the increasing levels in violence do mostly occur in areas,

which are usually facing violent events. However, the point estimates rise for the first two months after the event, which is again in line with the theoretical model by Osorio (2015) that competing cartels have an incentive to fight with the weakened cartel for market shares and profitable routes. Again, the results suggest that the spark in violence incidence lasts for roughly half a year after the extradition, however in contrast to intensity, incidence remains significantly higher for up to seven months after the event.

5.3. Robustness

As discussed above, the SDID estimator is still relatively new, so there is not yet a standard practice for robustness checks. Nonetheless, in [Table 1](#), I already show the magnitude of the effects changes only slightly using different specifications and restricting the timeframe of the study period. Additionally, following Du and Shepotylo (2022), I alter the policy timing and the donor pool, excluding municipalities without the presence of major DTOs. Finally, I conduct a placebo estimation where I assign a placebo treatment 32 months before the real treatment. The results of these robustness checks can be seen in [Table A 5-A 7](#).

With a shifted policy timing of three months in both directions, the results remain significant and the point estimates only change by approximately one standard error in each direction. Hence, the ATT is robust to changes in the exact timing of the policy. Furthermore, when I restrict the donor pool only to municipalities where one of the nine big cartels in the data is available, the results remain positive and significant at the 10%-level. The point estimates change only slightly. Finally, the placebo estimation behaves as expected, meaning that a placebo treatment 32 months prior in time results in insignificant coefficients close to 0.

5.4. Limitations and policy implications

Even though the datasets provided by Osorio et al. (2019) and Osorio and Beltran (2020) are of high quality, I cannot rule out media coverage bias. Additionally, there is the possibility that events are double-coded since numerous data sources are summarized in the datasets. I

believe over-reporting to be of little concern since the authors describe extensively how they avoid double-coding. Rather, media coverage bias could lead to underreporting of violent events in remote areas. Therefore, I interpret my estimates as lower bounds of the true effect. Furthermore, I am not able to distinguish between intra- and intercartel violence. This requires more detailed data for further research to better explore the causal channels of violence at play.

I cannot entirely rule out structural breaks in the data since I use a large number of municipalities over 13 months, I cannot keep track of whether any of these municipalities face a similar treatment which would make their presence as control observations in the donor pool less convincing. However, since I am not using a small number of municipalities but a weighted average of all of them, I believe this to be a less severe concern than in traditional DID estimations. Furthermore, the fact that the findings are robust to a limited donor pool increases the confidence in the results.

Another issue might be changing territories within my study period. As described further above, my definition of the cartel area is municipalities where the cartel has been active before the treatment period. I implicitly assume this doesn't change over two years. Since the data includes a large number of events but numerous missing values when it comes to actor, this is somehow difficult to prove from the data itself. I believe that the most violence takes place in areas which are somehow attractive to the cartels, meaning that they are, for example, remote or located along an attractive route to the US. Most likely, this does not change over 2 years. However, it should be noted that shifts in cartel territory are a potential caveat to my analysis. This also applies to potential spatial spillover effects from treated to untreated municipalities.

Finally, my study emphasizes internal validity. Even though I looked at two other high-profile extraditions, and the effects are similar in the Mexican, but not in the Colombian case, the results are not generalizable to all extraditions or all extraditions of high-profile criminals.

Nonetheless, I believe my results to be relevant for policy choices in countries suffering from drug criminality. My analysis shows that for kingpin extraditions, immediate violence effects are less severe than newspapers indicate. Also, spatial contagion only takes place roughly 3 months after the event. This is valuable knowledge in relation to the use of security means. However, as mentioned above, for a better understanding of the exact mechanisms, further research is required.

6. Conclusion

This paper aimed to investigate whether the extradition of top-level drug lords cause an increase in local violence levels. I showed these events can lead to an increased number of violent events in the respective cartel's territory. Using the relatively new synthetic difference in differences estimator, I show that the extradition of El Chapo from the Sinaloa Cartel leads to an increase of 0.1-0.2 log points in violence in municipalities where the cartel is active. Furthermore, I use a LMP to show that the probability of violence happening increases only 3 months after the extradition significantly, which yields the interpretation that extraditions fuel violence levels with a time lag. In my understanding, this lag is due to competitors having to adjust their resources to be able to violently combat for new territory, market shares and attractive trafficking routes.

My results have policy implications for countries suffering from drug-trafficking and cartel violence when it comes to the allocation of security forces shortly after the extradition. However, it appears that market structure, goals of the DTOs and country-specific factors play an important role.

Finally, further research is necessary to gain insights on the exact relevant causal mechanisms as well as to accumulate results in other settings to gain confidence in the external validity of my results.

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8. Appendix 1

A question addressed in the literature is whether the violence in Colombia in the study period should be regarded as civil conflict or as crime. The literature is ambiguous about this question. While Angrist and Kugler (2008) state that as most civilian victims in their study period (1990-2000) are men, this indicates the conflict-related nature of the violence. This view is shared by Maher and Thomson (2018) who argue that after the disarmament, demobilization and reintegration (DDR) procedure, the neo-paramilitaries operate in the same way as the “classic” paramilitary groups and consist, to some extent, of the same leading figures. Contrary to their view, Krakowski (2015) argues that at least paramilitary violence should be considered crime violence since there is no large difference in policy preferences between paramilitaries and the government, which, in his view, is the base for political conflict.

An example of linking coca and violence in Colombia is the study by Angrist and Kugler (2008). Using the downfall of the so-called Andean air-bridge which used to be the main smuggler route for cocaine into the USA from Bolivia and Peru as an exogenous shock to coca prices in Colombia, they find that areas with higher coca production face a larger increase in violent events after the price increase. They also state that guerrillas and paramilitary groups generate large profits from illicit activities related to coca, be it through taxes, kidnappings, extortions, protection rackets or direct control of production. This indicates that not only local income levels are important determinants of conflict through the opportunity cost channel, but also how this income is generated. This is coherent with the aforementioned findings of empirical studies on the effect of labour- and capital-intensive industries on conflict (Dube and Vargas 2013; Berman et al. 2017).

9. Appendix 2

Figure A 1: Comparison of annually reported events in OCVED and UCDP

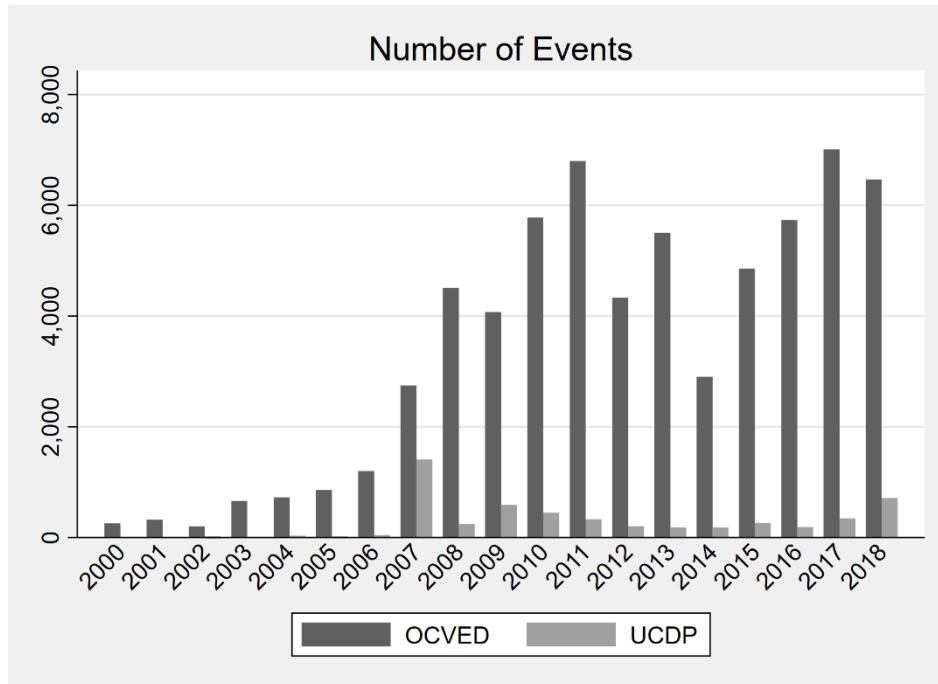


Figure A 2: Treatment and Donor Pool Municipalities

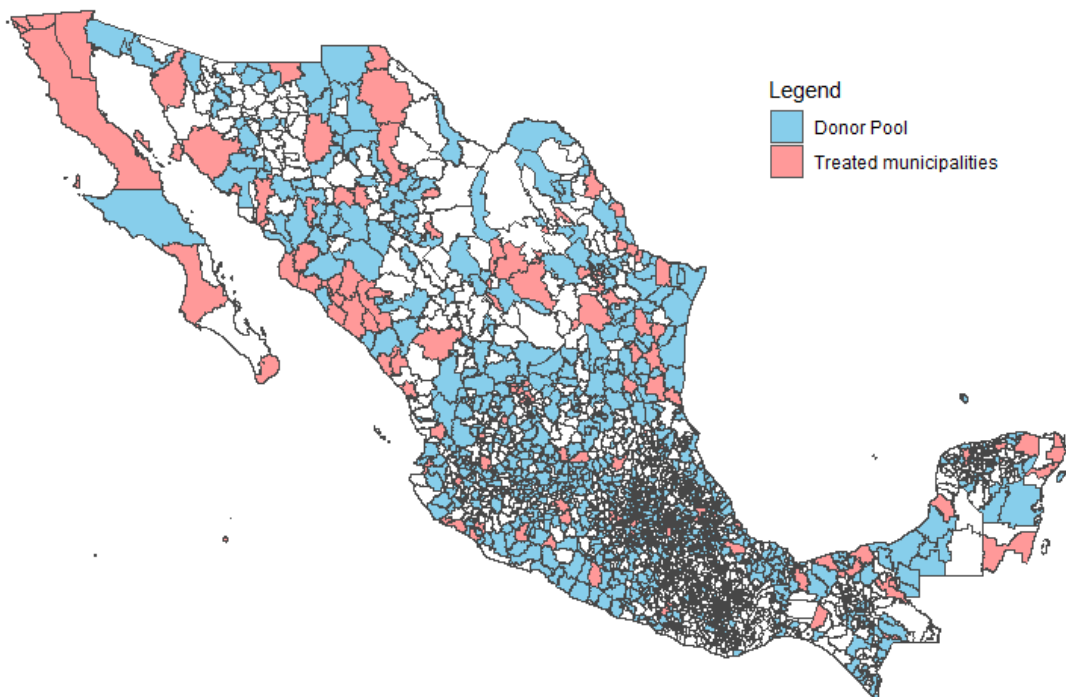


Figure A 3: Number of Events per Municipality prior to Treatment (2016)

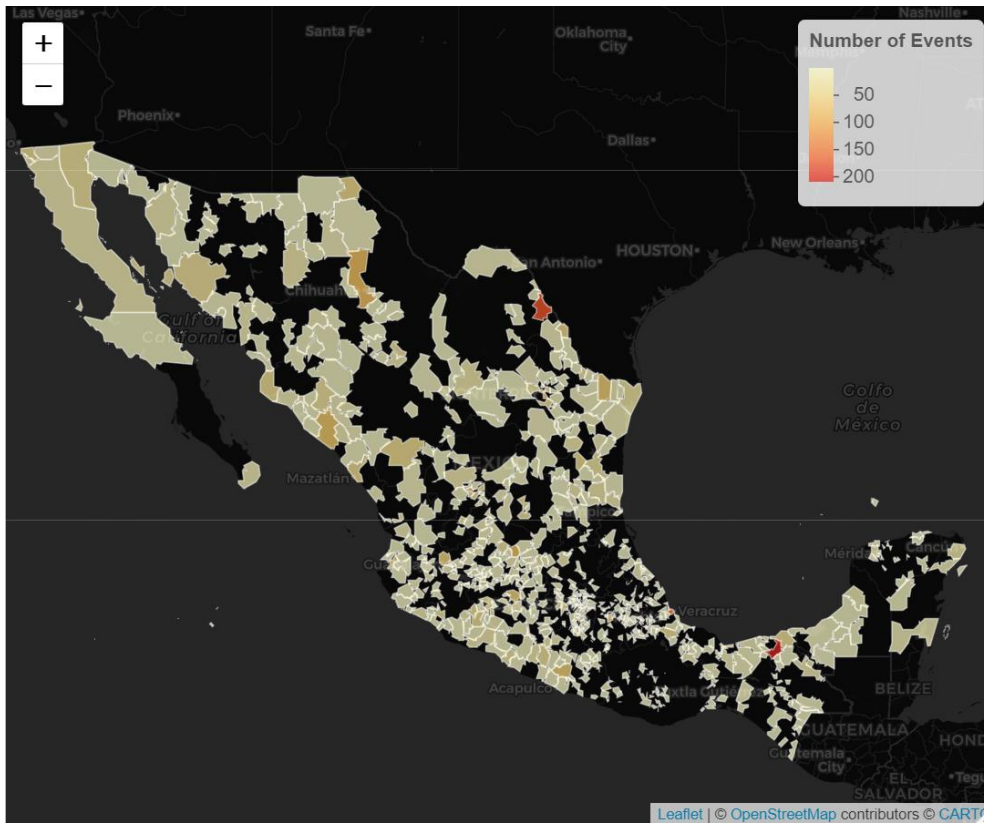


Figure A 4: Number of Events per Municipality after Treatment (2017)

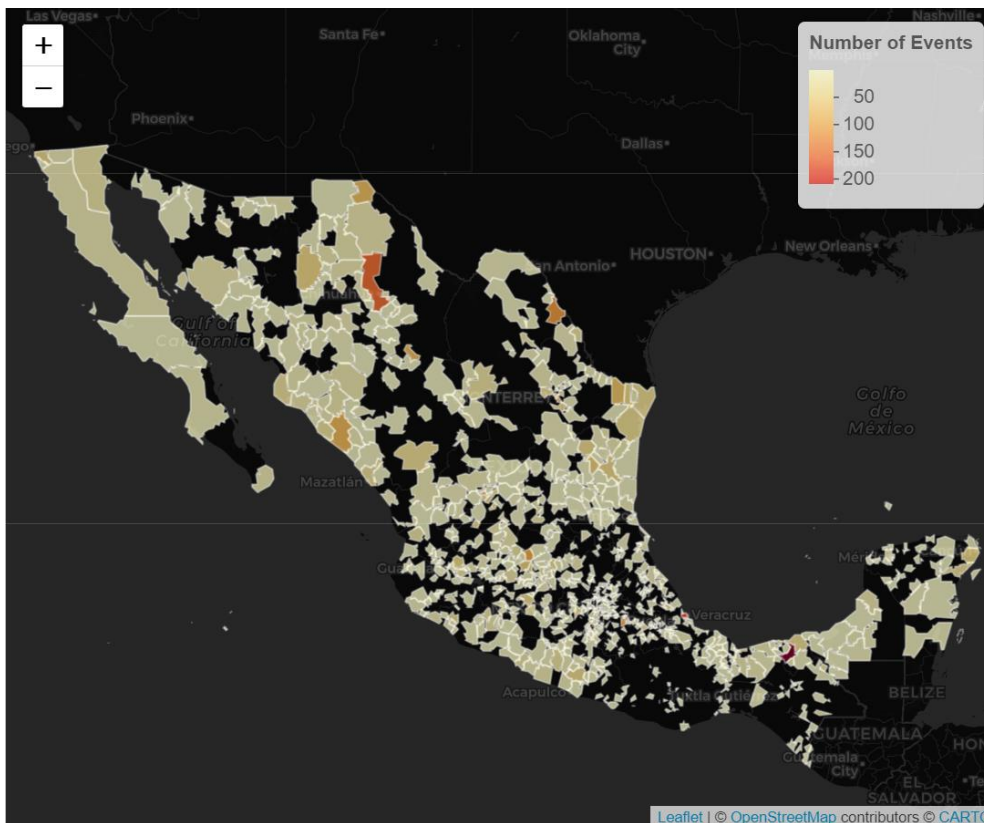


Figure A 5: Number of Violent Events in Treatment Municipalities prior to Treatment (2016)

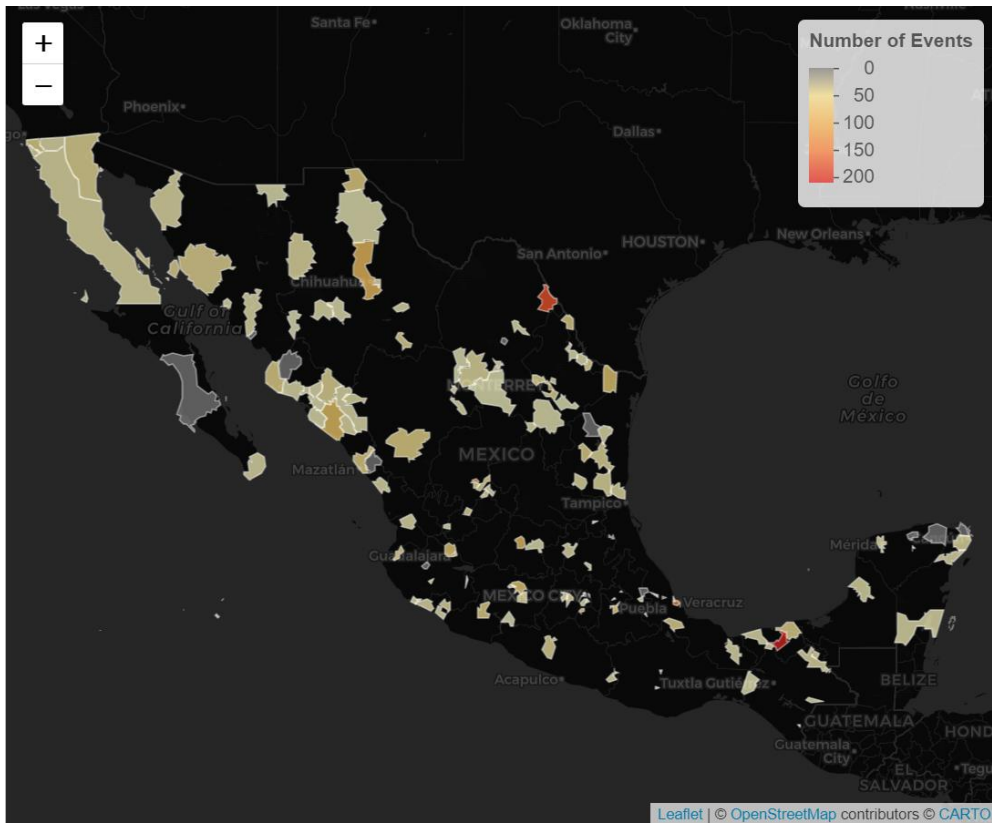


Figure A 6: Number of Violent Events in Treatment Municipalities after Treatment (2017)

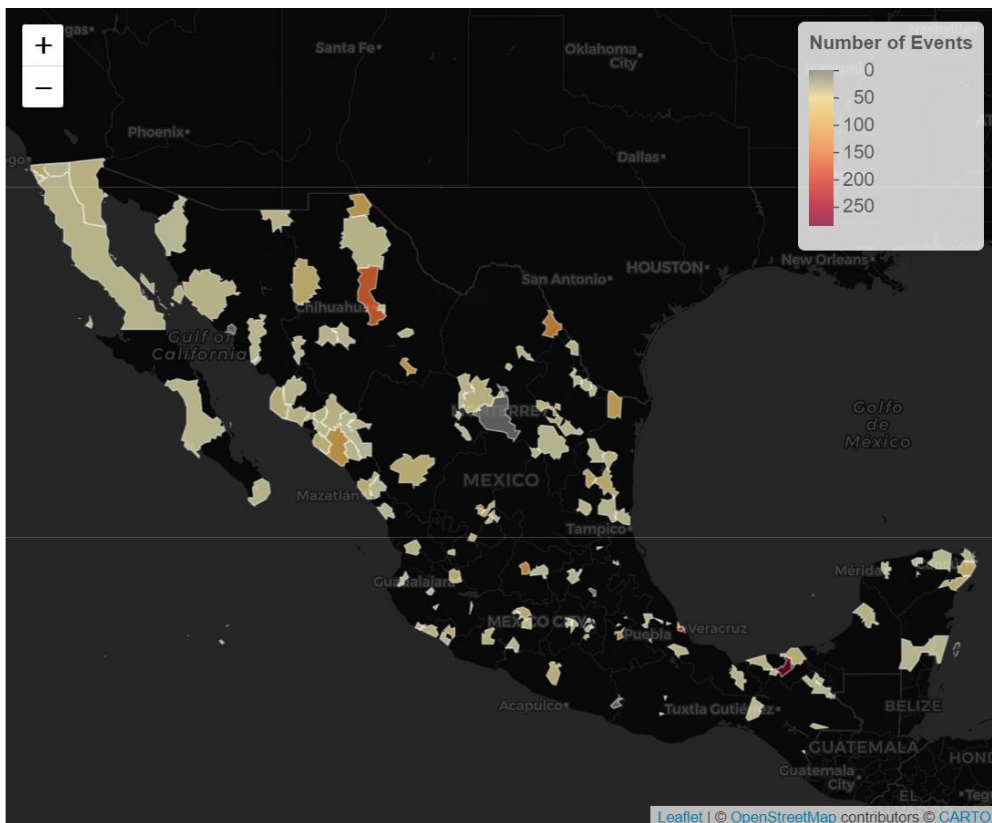


Figure A 7: Difference in the Number of Violent Events in Treatment Municipalities after vs. before Treatment

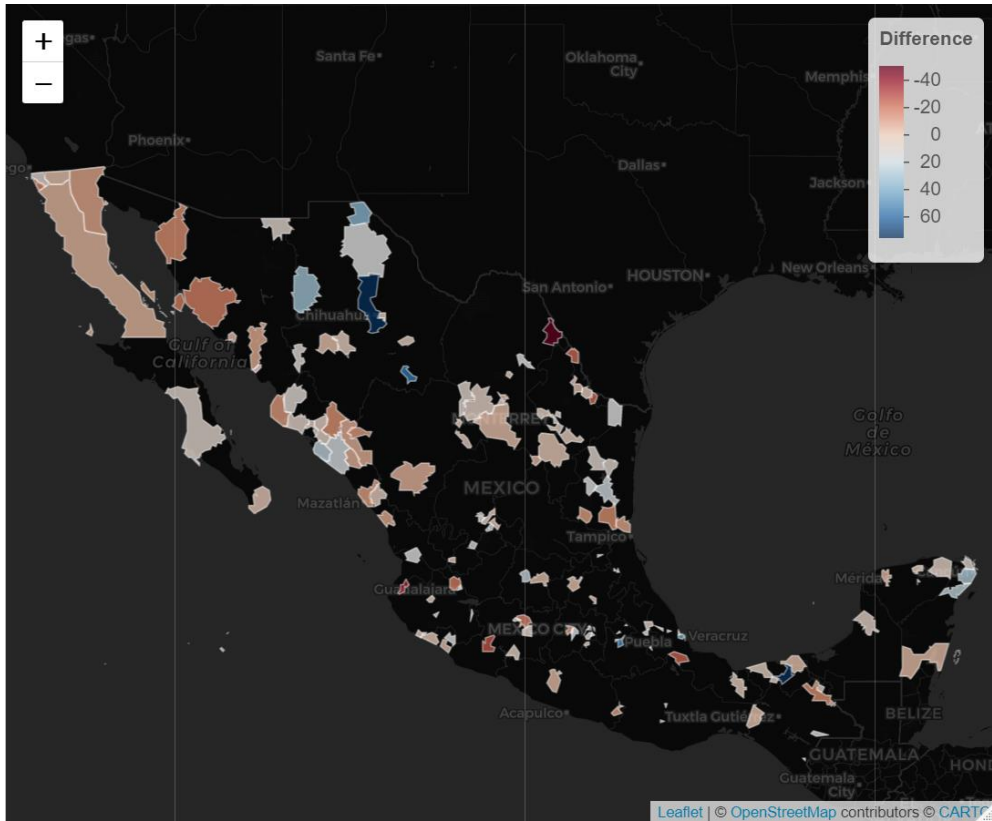
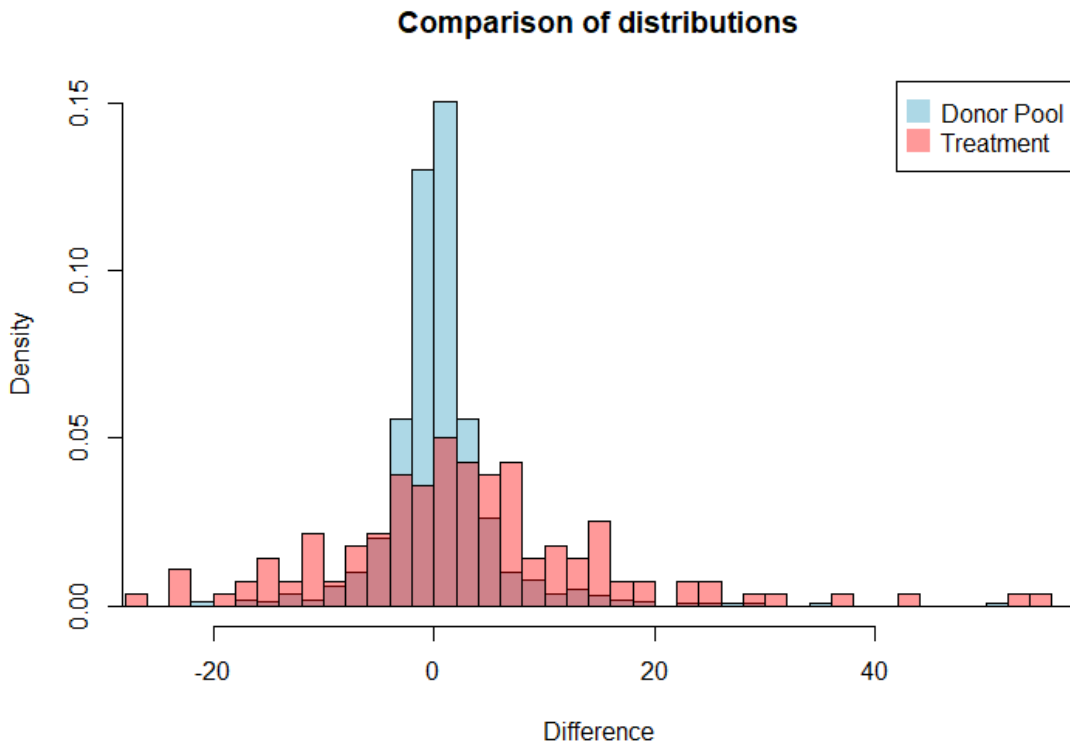


Figure A 8: Comparison of the Change in the Number of Violent Events per Municipality: Treatment vs. Donor Pool



Note: The overlaying histograms show the distribution of the change (after – before treatment) in the number of violent events for municipalities in the donor pool and the treatment group, respectively.

Table A 1: Descriptive Statistics

Group	Treatment		Control	
	(1)	(2)	(3)	(4)
Time to Treatment	Before	After	Before	After
Violence Intensity:				
Total Number of Events	8,053	10,681	5,905	7,615
Mean	4.793	5.869	0.585	0.697
Standard Deviation	13.691	17.469	3.165	3.096
Min.	0	0	0	0
Max.	245	335	80	75
Violence Incidence:				
Mean	0.493	0.527	0.146	0.168
Standard Deviation	0.500	0.499	0.353	0.373
Min.	0	0	0	0
Max.	1	1	1	1
Population Size:				
Mean	257,299.7		73,547.5	
Standard Deviation	382,370.1		156,143.6	
Min.	906		369	
Max.	1,922,523		1,835,486	
% Men under 30 years:				
Mean	16.357		16.492	
Standard Deviation	1.372		1.551	
Min.	12.571		11.163	
Max.	19.927		22.800	
% Working Population				
Mean	46.083		44.313	
Standard Deviation	6.677		6.583	
Min.	23.978		12.130	
Max.	60.485		62.477	
Number of Months	12	12	12	12
Number of Municipalities	140	140	841	841

Note: Population Size, the fraction of young men and the fraction of the working population are not used in the estimation strategy, since only yearly and not monthly numbers are available, hence they are time-invariant in my study-period. Nonetheless, they are supposed to give a better understanding of the process and potential differences between treated and untreated municipalities.

Table A 2: Treatment Effects for an alternative extradition - Benjamín Arellano Félix

Dep. Variable	Ln($Y_{it} + 1$)		arcsinh(Y_{it})		Ln($Y_{it} + 1$)	
	(1)	(2)	(3)	(4)	(6)	(7)
Estimator	SDID	SDID	SDID	SDID	SC	SC
ATT	0.168** (0.0741)	0.0845 (0.0815)	0.210** (0.0882)	0.114 (0.0969)	0.202* (0.115)	0.0954 (0.132)
# post-treatment periods	12	6	12	6	12	6
N	22325	16967	22325	16967	22325	16967

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A 3: Treatment Effects for an alternative extradition - Colombia's extradition of 13 paramilitary officers

Dep. Variable	Ln($Y_{it} + 1$)		arcsinh(Y_{it})		Ln($Y_{it} + 1$)	
	(1)	(2)	(3)	(4)	(6)	(7)
Estimator	SDID	SDID	SDID	SDID	SC	SC
ATT	0.0191 (0.0190)	0.0205 (0.0200)	0.0254 (0.0244)	0.0270 (0.0256)	0.210*** (0.0353)	0.186*** (0.0337)
# post-treatment periods	12	6	12	6	12	6
N	12225	9291	12225	9291	12225	9291

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A 4: Coefficients for the time periods in the LPM

Model	(1) LPM
Lead 6	0.00306 (0.0150)
Lead 5	-0.00510 (0.0146)
Lead 4	-0.00204 (0.0149)
Lead 3	0.0204 (0.0155)
Lead 2	0.00510 (0.0145)
Lag 0	0.00408 (0.0146)
Lag 1	0.0255* (0.0154)
Lag 2	0.0652*** (0.0155)
Lag 3	0.0255 (0.0156)
Lag 4	0.0387** (0.0151)
Lag 5	0.0336** (0.0151)
Lag 6	0.0357** (0.0147)
Lag 7	0.0347** (0.0153)
Lag 8	0.00102 (0.0148)
Lag 9	0.0296** (0.0149)
Lag 10	0.00612 (0.0148)
Constant	0.192*** (0.00994)
N	16677

Note: Coefficients from Equation (4) as depicted in Figure 4.

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Robustness Checks:

Shifted Treatment timing:

Table A 5: Robustness Check - Shifted treatment timing

Dependent Variable	(1)	(2)	(3)
	$\text{Ln}(Y_{it} + 1)$	$\text{Ln}(Y_{it} + 1)$	$\text{Ln}(Y_{it} + 1)$
ATT	0.102** (0.042)	0.139*** (0.043)	0.069* (0.042)
Period of Treatment Assignment	-1	-4	+4
N	24525	24525	24525

Note: The table shows ATTs estimated from Equation (1) by artificially shifted the begin of the treatment period 1 and four months before the real treatment (indicated by columns (1) and (2); as well as four month after the real treatment (column (3)). Standard Errors are derived using a bootstrapping procedure with 500 replications each.

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Restricted donor-pool:

Table A 6: Robustness Check - restricted donor pool

Dependent Variable	(1)	(2)
	$\text{Ln}(Y_{it} + 1)$	$\text{arcsinh}(Y_{it})$
ATT	0.108* (0.059)	0.123* (0.072)
# post-treatment periods	6	6
N	9329	9329

Note: The donor pool is restricted such that only municipalities where one of the nine big cartels is active remain in the data. Standard Errors are derived using a bootstrapping procedure with 500 replications each. Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Placebo estimation:

Table A 7: Robustness Check - Placebo Estimation

Dependent Variable	(1)	(2)
	$\text{Ln}(Y_{it} + 1)$	$\text{arcsinh}(Y_{it})$
ATT	0.027 (0.061)	0.037 (0.073)
# post-treatment periods	8	8
N	18375	18375

Note: Placebo estimation shifting the treatment period artificially 32 months back in time. Standard Errors are derived using a bootstrapping procedure with 500 replications each.

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$