The Silent Taxation: Differentiated Effects of Drug-Trafficking Organizations Presence on Local Market Size in Mexico

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[1]This Capstone Project is submitted in partial fulfilment of the requirements for the MSc in Applied Social Data Science. All code and data necessary to replicate the findings are available at https://github.com/aabermejoq/DTO_Effects_Local_Markets.

Abstract

This study examines the short term economic effects of drug-trafficking organization (DTO) presence on local market size in Mexico, distinguishing between municipalities under monopoly control and those contested by multiple rival groups. DTO presence, intensity, persistence, and type of control are measured across varying treatment thresholds using the municipality–day dataset of Osorio and Beltrán (2020), which applies natural language processing to news and official reports. To address the scarcity of annual market size data at the municipal level, establishment counts are estimated with a supervised learning model trained on banking indicators, sectoral composition, and demographic characteristics. These estimates are combined with propensity score matching and weighted two-way fixed effects regressions to assess differential impacts. Results show that monopoly dominance has little or no significant effect on business growth in the year after entry, whereas contested control consistently reduces it—by up to five percentage points under certain definitions of sustained presence. These findings underscore the need to distinguish between monopolistic and competitive criminal governance when evaluating the economic consequences of organized crime.

Keywords: drug-trafficking organizations, organized crime, extortion, local markets, violence, Mexico, informal taxation, economic impact.

1 Motivation

December 11, 2006 marked a turning point for the political, social, and economic landscape of Mexico as former President Felipe Calderón launched the so-called "war on drugs." This security policy aimed to dismantle the Drug Trafficking Organizations (DTOs) that have had control over the drug trade for decades through the use of military force. However, despite its objectives, this strategy intensified violence by fragmenting cartel structures, triggering internal conflicts as factions fought for control over certain regions (Dell, 2015).

Furthermore, this destabilization led DTOs to diversify into extortion, kidnapping, oil theft, and other criminal activities, further complicating law enforcement efforts as smaller and more violent groups emerged (Rios, 2013). As a result, homicides linked to organized crime surged, raising the national homicide rate from 5.8 per 100,000 inhabitants in 2007 to over 25 in recent years. According to the Mexican Institute of Statistics (INEGI), more than 400,000 intentional homicides have been recorded between 2007 and 2024.[2] Likewise, the National Registry of Missing and Unlocated Persons (RNPED) reports close to 100,000 disappearances nationwide.[3]

In addition to the severe loss of life caused by rising homicide rates, the diversification and expansion of DTOs have imposed substantial economic costs. These arise not only from the erosion of human capital, but also from a series of direct and indirect effects, including reduced investment incentives, the diversion of public and private resources toward security, and disruptions to production and supply chains (IDB, 2010).

In particular, DTOs have a profound influence at the firm level. The expansion of their illicit activity portfolios has significantly increased the prevalence of systematic extortion payments—commonly referred to as "protection fees" or *derecho de piso*—through which these groups claim to "guarantee" business safety, often against threats they themselves

[2] INEGI. Defunciones por homicidio, 2007–2024. Available at: inegi.org.mx/temas/mortalidad

[3] Registro Nacional de Personas Desaparecidas y No Localizadas (RNPED), Secretaría de Gobernación. Available at: gob.mx/sesnsp

create. Such payments, sometimes fixed but more frequently scaled to the size of the firm or the strategic importance of the industry, have become a pervasive mechanism of control and revenue extraction.

In most cases, businesses have little choice but to comply: refusing to pay often entails the risk of severe retaliation, including physical attacks on facilities, harm to employees, or even targeted killings. The coercive nature of this arrangement leaves firms operating under constant threat, effectively transforming extortion into a predictable, quasi-institutionalized cost of doing business in affected areas. According to Mexico's National Victimization Survey of Businesses (ENVE) conducted by INEGI, extortion was the most frequently reported crime against businesses, with a prevalence rate of 1,562 per 10,000 economic units (INEGI, 2024).

Moreover, in certain regions, DTOs have expanded their activities well beyond extortion, exerting control over entire industries by setting prices, mandating the distribution of their own products, and blocking the entry of goods from other regions. Documented cases include avocado production and distribution in Michoacán, as well as the alcoholic beverages and food products sectors in the southern part of the State of Mexico.[4]

Nevertheless, the scope and significance of these organizations' influence are far from trivial. While their tactics are inherently coercive, DTOs often have an economic incentive to maintain business operations in order to secure a stable stream of extortion revenues. Furthermore, the nature of their interaction with local populations may vary according to the intensity of competition and violent confrontations with rival groups, occasionally leading them to cultivate goodwill in territories under their control (Magaloni et al., 2019). A notable example is the Sinaloa Cartel under Joaquín "El Chapo" Guzmán, which reportedly avoided excessively extractive practices and, on occasion, engaged in limited forms of economic assistance—such as the distribution of food—to foster local support.

[4] *El País*, "Control de negocios, amenazas y sobreprecios de hasta el 144%: así extorsiona la Familia Michoacana en el Estado de México" (Available only in Spanish), July 24, 2025. Available at: elpais.com

2 Research Question and Scope

The documented differences in extractive practices raise an important empirical question: *does the economic impact of DTOs entry vary depending on whether the territory is contested or under the exclusive control of a single group?* This study examines that question by comparing municipalities under exclusive control with those classified as "disputed" due to competition between rival organizations.

Distinguishing between these contexts is analytically relevant because the incentives and constraints faced by DTOs—and thus the scope and form of activities such as informal taxation—are likely to differ depending on the stability of territorial control.

Addressing this research question entails three principal challenges. First, measuring the outcome variable requires precise annual counts of economic units at the municipal level; however, the official INEGI directory is updated only during the economic censuses, limiting temporal granularity. Second, identifying the geographic presence of DTOs is methodologically complex due to the clandestine nature of their operations and the strategic incentives they face to conceal activities. Third, even with consistent measures of market size and DTO presence, establishing causal effects is inherently challenging, as municipalities with criminal organization may systematically differ from those without.

To address these challenges, the study follows a three-part empirical strategy. First, it estimates municipal-level counts of economic units by training a supervised learning model—based on point-of-sale terminal records, banking transaction data, and socio-demographic indicators—to interpolate the years between the 2014 and 2019 economic censuses.

Second, it maps DTOs presence and differentiates between contested and uncontested territories using the dataset of Osorio and Beltrán (2020), which applies Natural Language Processing to news reports and official sources to produce a detailed annual record of criminal organizations in Mexico from 2000 to 2018. The analysis tests alternative thresholds to capture sustained rather than incidental presence, and enabling the empirical assessment of the research question at multiple levels of DTO activity.

Third, it incorporates measures of institutional capacity, spatial spillovers from neighboring municipalities with DTO activity, and additional covariates into a propensity score matching framework, estimating the differential effects of DTO presence—contested versus uncontested—on local market size using two-way fixed effects models with standard errors clustered at the municipality level.

The results reveal a nuanced relationship between organized crime and local market size. For municipalities experiencing the entry of a single DTO, short-term effects on annual business growth are generally imprecise and rarely statistically significant. For certain mid-level treatment intensities, point estimates are negative, suggesting a modest contraction in local market activity, though the limited precision and temporal scope warrant caution in interpretation.

When focusing on municipalities under active territorial disputes——defined as the contemporaneous presence of multiple DTOs—negative effects become more frequent and, at certain thresholds, reach statistical significance. This pattern is consistent with the theoretical framework proposed by Magaloni et al. (2019), which posits that contested criminal environments amplify uncertainty, violence, and extortion pressures, thereby suppressing business growth. Nonetheless, variation in both the magnitude and significance of the estimates across thresholds, coupled with the potential for residual confounding, precludes definitive causal interpretation.

For treatment thresholds where the results are statistically significant, disputed DTO presence is associated with a drop in a municipality's annual business growth of roughly 2.5 to 5.1 percentage points, depending on how strictly sustained presence is defined.

Methodologically, the study offers three contributions. First, it constructs a high-frequency proxy for municipal market size in settings where official statistics are scarce, combining financial data with demographic and geographic covariates. Second, it compiles municipality–year measures of DTO presence—such as years under treatment, the share of neighboring municipalities with DTO activity, and other relevant indicators—to better capture the historical trajectory of organized crime at the local level. Third, it incorporates these measures into a matched two-way fixed effects framework, which helps address observable confounding and time-invariant unobserved factors.

Overall, the analysis provides a careful, evidence-based assessment of the short term economic effects of DTO presence and territorial contestation. Although limited by data constraints, the results highlight the relevance of distinguishing between different forms of organized crime control when evaluating their economic implications.

The remainder of the document first provides a detailed review of the relevant literature, followed by a presentation of the empirical strategy, including data sources, matching procedure, and model specification. It then reports the main findings across treatment thresholds and concludes with a discussion of their implications.

3 Theoretical and Empirical Framework

3.1 The Impact of Crime on the Economy

A canonical starting point for analyzing the economic consequences of crime is Becker (1974), which conceptualizes criminal participation as an optimizing response to incentives under imperfect enforcement. This framework shows how illicit activity increases expected operating costs—via extortion, expropriation risk, and contractual instability—thereby influencing firms' entry, scale, and survival, and, in aggregate, distorting resource allocation and constraining productivity.

Building on this framework, Di Tella et al. (2010) apply the economics-of-crime perspective to Latin America, a region with some of the highest and most persistent crime rates worldwide but limited systematic evaluation of anti-crime policies. Their work examines how institutional weakness, poverty, and inequality interact with criminal incentives, and assesses interventions such as mandatory arrest laws and prison education programs. By combining regional evidence with insights from developed countries, they show that Becker's cost–benefit logic remains essential for understanding criminal behavior in contexts where illicit activity is pervasive and deeply intertwined with economic performance.

Beyond the microeconomic logic of individual decision-making, a large body of research in political economy and institutional quality emphasizes the central role of secure property rights and credible contract enforcement in sustaining investment and market exchange. When the rule of law is weakened and personal security cannot be guaranteed, the risk associated with long-term commitments rises sharply, prompting firms and investors to postpone, scale down, or abandon projects altogether. Such conditions not only deter new investment but also erode existing productive capacity, limit access to credit, and discourage innovation.

Over time, these dynamics contribute to slower capital accumulation, reduced productivity growth, and a reallocation of resources toward activities offering short-term returns or requiring minimal sunk costs (Haggard and Tiede, 2011).

Empirical research has quantified the macroeconomic costs of criminal activity. Focusing on Italy over the period 1979–2002, Detotto and Otranto (2010) conceptualize crime as an implicit "tax" on the entire economy—discouraging both domestic and foreign direct investment, reducing firms' competitiveness, and reallocating resources in ways that heighten uncertainty and inefficiency. In a different context, Cárdenas (2007) examines Colombia's economic slowdown in the 1980s and finds that declining productivity—rather than reductions in physical or human capital—was the main driver of reduced growth, directly linked to rising crime, particularly homicide rates fueled by expanding drug trafficking.

The bidirectional relationship between crime and economic performance is a recurring theme in the literature. While crime undermines output by increasing uncertainty, deterring investment, and distorting resource allocation, persistent underdevelopment and weak economic opportunities are likewise recognized as structural drivers of criminal activity. This simultaneity poses significant identification challenges, as causality may operate in both directions. To address these concerns, empirical studies frequently employ strategies such as the use of proxy and lagged variables, instrumental variables, and other advanced econometric techniques to mitigate endogeneity and more accurately capture the dynamic and reciprocal nature of the crime–economy nexus.

For instance, Archontakis and Constant (2021) analyze Namibia over the period 2000–2015 to examine the relationship between economic growth and crime rates. Their results show no evidence of a long-run equilibrium relationship; however, they identify a significant short-run effect in which increases in crime are associated with declines in economic growth. These findings underscore the complexity of the crime–economy nexus and highlight the need for econometric approaches capable of distinguishing between short-term dynamics and long-term structural relationships.

3.2 Organized Criminal Groups Dynamics

While general theories of crime and institutional quality help explain the broad economic consequences of insecurity, understanding the specific mechanisms through which organized criminal groups operate is crucial for this study. These organizations differ from other forms of criminal activity in their capacity to exert sustained control over territories, coordinate complex illicit and licit operations, and strategically interact with legal markets.

Their actions often go beyond episodic violence, encompassing systematic rent extraction, market manipulation, and even partial governance functions. A growing body of empirical work has examined these dynamics in diverse contexts, revealing how organized crime can reshape local economic structures, for example, examining the Italian case, Gurciullo et al. (2015) use network analysis to trace how the Sicilian Mafia infiltrates legitimate private-sector activities, securing influence over strategic industries and embedding itself in local economic networks. They show that this infiltration distorts competition, discourages private investment, and enables the sustained extraction of rents over time.

At a broader scale, World Bank (2017) document how the illicit drug trade organizations can affect state capacity by undermining institutional legitimacy, diverting public resources toward security, and weakening the enforcement of property rights. These dynamics are most severe where criminal organizations command substantial financial resources and coercive power, enabling them to operate alongside—or even in place of—formal state institutions.

Despite the evident risks these organizations pose to public safety and their negative impacts on security and economic performance, evidence from diverse contexts shows that their relationships with local communities are not always purely predatory. As Lessing (2020) notes, in areas of limited but not absent state presence, criminal governance can emerge in which local criminal groups act as de facto authorities—providing protection, enforcing informal rules, or even supplying goods and services to non-members.

3.3 Evidence on the Effect of DTOs on the Mexican Economy

Much of the existing literature on the effects of DTOs on the Mexican economy has focus on the violence generated by this organizations. For example, Robles et al. (2013) use municipal electricity consumption as a proxy for local economic activity and estimate the causal effect of violence through an instrumental variable strategy. The instrument exploits exogenous variation from historical cocaine seizures in Colombia interacted with the distance of Mexican municipalities to the United States border (Castillo et al., 2013), which predicts shifts in DTO activity and, consequently, local violence. Their results show that marginal increases in homicide rates reduce labor force participation, increase unemployment, and lead to substantial declines in both earned income and the share of business owners.

Likewise, Velásquez (2016) examines how the surge in drug-related violence has affected labor market outcomes in Mexico. Using a nationally representative longitudinal dataset and controlling for unobserved time-invariant heterogeneity, she finds that higher levels of local violence are associated with significant declines in earnings and productivity, particularly among self-employed men, while women tend to reduce their working hours or leave the labor force.

However, over the past two decades, Mexican DTOs have diversified their criminal portfolios—engaging in activities such as extortion and exerting control over entire markets—thereby extending their economic impact beyond drug trafficking and its associated violence. For instance, De Haro (2025) analyzes DTO expansion into Mexico's avocado sector, exploiting the U.S. fentanyl shock that reduced heroin demand. She finds that while poppy-growing municipalities saw declines in homicides and violent thefts, avocado-growing areas experienced increases in both—including killings of agricultural workers and truckload thefts—suggesting that DTOs offset drug revenue losses through intensified extortion in other lucrative markets.

Given the expansion of these organizations' portfolios of illicit activities, it is not sufficient to study only the effects of the violence they generate; it is also important to isolate the economic consequences of their territorial presence. In this regard, Juncal Mendoza (2024) analyzes municipal-level panel data to estimate the economic impact of DTO presence, finding that affected municipalities experience persistent declines in variables such as formal employment, average wages, and total wage mass.

By contrast, González (2015) reports no significant changes in local economic activity—proxied by satellite night lights—following the arrival of large DTOs between 2000 and 2010. Controlling for state-specific trends and political regimes, his findings suggest that DTO presence does not necessarily produce measurable shifts in aggregate economic output.

Although the existing studies vary in scope, methodology, and data sources, a common thread emerges when viewed through the lens of theoretical perspectives on DTO behavior. As mention before, economic strategies of organized crime are shaped by the degree of territorial competition they face. When rival groups contest control over the same area, uncertainty increases, violence escalates, and criminal organizations tend to adopt more predatory revenue-extraction methods, such as frequent and arbitrary extortion, kidnapping, or the forced monopolization of certain goods.

These conditions create an unstable environment for local businesses, eroding investment incentives and disrupting market activity. In contrast, when a single DTO consolidates control, the logic of profit maximization can sometimes lead it to moderate its predatory practices to avoid driving economic actors out of the market altogether.

In the Mexican context, Magaloni et al. (2019) provide systematic evidence for this distinction, showing that monopolistic DTOs may, under certain circumstances, offer forms of protection or even limited public goods provision—such as security against rival groups or basic material support—to strengthen their legitimacy among residents. This does not imply benevolence, but rather a strategic calculation to maintain a steady stream of rents from a functioning local economy. The study underscores how variations in market structure and territorial control can produce very different economic outcomes, from severe contraction under contested rule to partial economic stability under monopolistic governance. These findings suggest that any assessment of the economic effects of DTO presence must account not only for whether an organization is active in a given area, but also for the competitive context in which it operates.

In fact both Juncal Mendoza (2024) and González (2015) measure cartel presence rely on categorical indicators at the municipality–year level. A binary (present/absent) coding collapses meaningful variation—how intense, sustained the presence is—and cannot distinguish monopoly control from contested territories. This aggregation risks masking heterogeneous effects: sporadic mentions and durable territorial control are treated alike, and the economic consequences of single-group governance are conflated with those of violent competition.

Moreover, as González (2015) acknowledges, his results do not account for the fact that municipalities with DTO activity are likely to differ systematically from those without—differences that may include baseline market size, institutional capacity, geographic position within trafficking routes, and pre-treatment economic trends—all of which can be correlated with both cartel presence and economic performance. Without addressing these selection issues and potential spatial spillovers from neighboring municipalities, estimated coefficients largely capture partial correlations rather than causal effects.

The present study addresses these gaps by operationalizing and empirically testing the market-structure hypothesis formulated by Magaloni et al. (2019), which states that the economic consequences of DTO presence vary systematically depending on whether territorial control is monopolistic or contested.

To do so, i rely on the municipality–day dataset of Osorio and Beltrán (2020), which applies NLP to news and official reports to build a granular record of DTO activity in Mexico from 2000 to 2018. From this source, I construct annual measures that capture the intensity of presence (reports per 10,000 residents), its persistence (consecutive years under exposure), and the number of distinct groups active in a municipality.

These measures make it possible to classify each municipality–year as being under monopoly or contested control, and to apply multiple treatment thresholds that capture different levels of presence. This approach avoids the unrealistic assumption that a single report of DTO activity is equivalent to sustained territorial control, instead distinguishing between occasional mentions and persistent, high-intensity presence.

Combined with spatial exposure variables, the analysis uses a matched, two-way fixed effects design to estimate how both the intensity and the nature of territorial control influence local market growth, measured through changes in the number of establishments.

4 Methodology

4.1 Overview

This study estimates the causal effect of the entry and sustained presence of Drug Trafficking Organizations (DTOs) on municipal market size in Mexico. The primary estimand is the impact of crossing pre-specified intensity thresholds of DTO activity on the *year-over-year percentage change* in market size. A secondary aim is to assess whether this impact is stronger when territorial control is *contested*—i.e., when more than one DTO is active in the same municipality.

The empirical strategy proceeds in four steps. *First*, a municipality–year measure of market size construction using a supervised learning model trained on establishment-level microdata from INEGI's *Directorio Nacional de Unidades Económicas* (DENUE).

Second, I measure DTO activity as the annual rate of DTO-related reports per 10,000 residents and define treatment at multiple decile cutoffs of the pooled rate distribution, $p \in \{10, 20, \ldots, 90\}$. A municipality–year is treated ($D_{i,t}^{(p)} = 1$) if its rate meets or exceeds the pth cutoff; otherwise it is untreated. I also flag whether presence is disputed (more than one distinct DTO active).

Third, because treatment is not randomly assigned, I model the assignment mechanism for each decile using lagged, pre-treatment covariates—prior-year market size, fiscal autonomy, a socioeconomic marginalization index, the lagged mean neighbour report rate (among the ten nearest municipalities), and the lagged homicide rate—to obtain propensity scores, construct observation weights, and form a balanced matched sample.

Finally, for each level of treatment, I estimate weighted two-way fixed-effects regressions of the year-over-year percentage change in market size on lagged treatment status and controls, including municipality and year fixed effects and state GDP growth.

4.2 Local Market Size Measurement

To measure local market size—the total number of economic establishments within a municipality—I rely on the National Directory of Economic Units (*Directorio Nacional de Unidades Económicas*, DENUE) compiled by Mexico's statistical agency, the Instituto Nacional de Estadística y Geografía (INEGI).[5] DENUE is fully updated every five years, with benchmark releases in 2009, 2014, 2019, and 2024, which constrains the temporal frequency of the series.

To overcome this limitation and enhance temporal granularity, I develop a supervised learning approach (Random Forest) that estimates municipal establishment counts using banking-operations indicators and market-structure covariates. Specifically, I use the municipal series from *Información operativa de la banca comercial* published by Mexico's central bank (Banco de México)[6] as well as variables derived from DENUE: the historical average composition of establishments by size and each municipality's sectoral shares. The model yields annual establishment counts for 2012–2019.

A data-coverage limitation is that Banco de México reports these municipal banking series for only 284 municipalities (out of nearly 2,500 nationwide), which together account for roughly 70% of establishments. This constraint should be kept in mind when interpreting the results presented in the next section. Table 1 lists the variables included in the model.

The training data set uses those 284 municipalities observed over two years ($n = 568$), and an 80/20 split yields a test set of 112 observations. The model was tuned through 10-fold cross-validation with 500 trees and a grid on `mtry`; the selected model uses `mtry`=8 achieving $R^2 \approx 0.94$.

Out-of-sample performance is evaluated using bootstrap resampling. For each bootstrap draw, the model is refitted on a sample of the training data (with replacement), and performance is assessed on the fixed test set. Pointwise 95% prediction intervals are con-

[5] INEGI, DENUE portal: https://www.inegi.org.mx/app/mapa/denue/default.aspx.

[6] Banco de México, "Información operativa de la banca comercial por municipio": https://www.banxico.org.mx/SieInternet/consultarDirectorioInternetAction.do?sector=19&accion=consultarCuadro&idCuadro=CF660&locale=es.

structed from the 2.5th and 97.5th percentiles of the empirical predictive distribution for each municipality–year. Figure 1 compares the resulting bootstrapped predictions with the observed values on a log–log scale. Appendix A presents the frequency distribution of prediction errors, categorized by percentage error ranges.

Table 1: Predictor definitions, sources, imputations, and model importance

Variable	**Source**	**Imputation**	**Importance**
Population	CONAPO	Annual value	100.00
Credit card contracts	Banxico	Annual average of monthly series	79.88
Debit-card accounts	Banxico	Annual average of monthly series	79.31
Banking operations, total	Banxico	Annual average of monthly series	69.92
Bank branches	Banxico	Annual average of monthly series	66.27
Establishments with POS	Banxico	Annual average of monthly series	62.82
Share of micro firms	INEGI	Fixed by municipality	59.11
Share of medium firms	INEGI	Fixed by municipality	56.75
Share of large firms	INEGI	Fixed by municipality	56.37
Information sector share (media)	INEGI	Fixed by municipality	55.65
Bank staff	Banxico	Annual average of monthly series	54.19
Finance sector share	INEGI	Fixed by municipality	50.38
Manufacturing sector share	INEGI	Fixed by municipality	46.52
ATMs	Banxico	Annual average of monthly series	45.24
Share of small firms	INEGI	Fixed by municipality	42.09
Mobile-banking accounts	Banxico	Annual average of monthly series	41.38
POS terminals	Banxico	Annual average of monthly series	41.11
POS transactions	Banxico	Annual average of monthly series	39.39
Entertainment sector share	INEGI	Fixed by municipality	39.32
Transport sector share	INEGI	Fixed by municipality	39.17

Notes: Importance values are scaled so that the top predictor equals 100. Only the main predictors are shown; omitted variables include the remaining sector shares and state-level dummies.

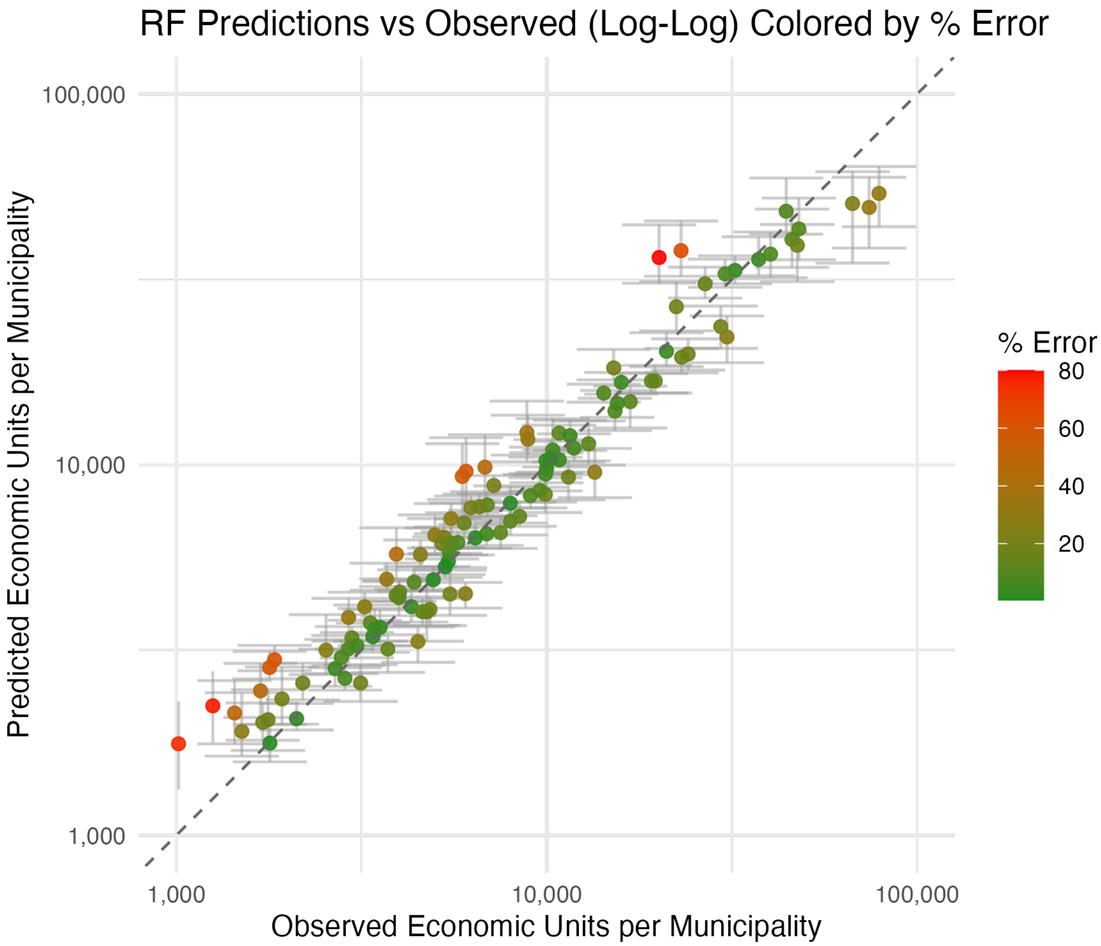

Figure 1: Random Forest predictions versus observed values on a log–log scale. Points are colored by absolute percentage error; vertical bars show 95% bootstrap percentile intervals.

4.3 DTO Presence and Treatment Definition

I measure the presence of drug trafficking organizations (DTOs) using the dataset compiled by Osorio and Beltrán (2020). This source applies machine–learning and natural–language–processing methods to transform unstructured text from news outlets and official communications into structured records at the municipality–day level. In practical terms, the data flag whether a municipality is mentioned in connection with DTO activity on a given date and, when possible, identify the specific organization(s) involved.

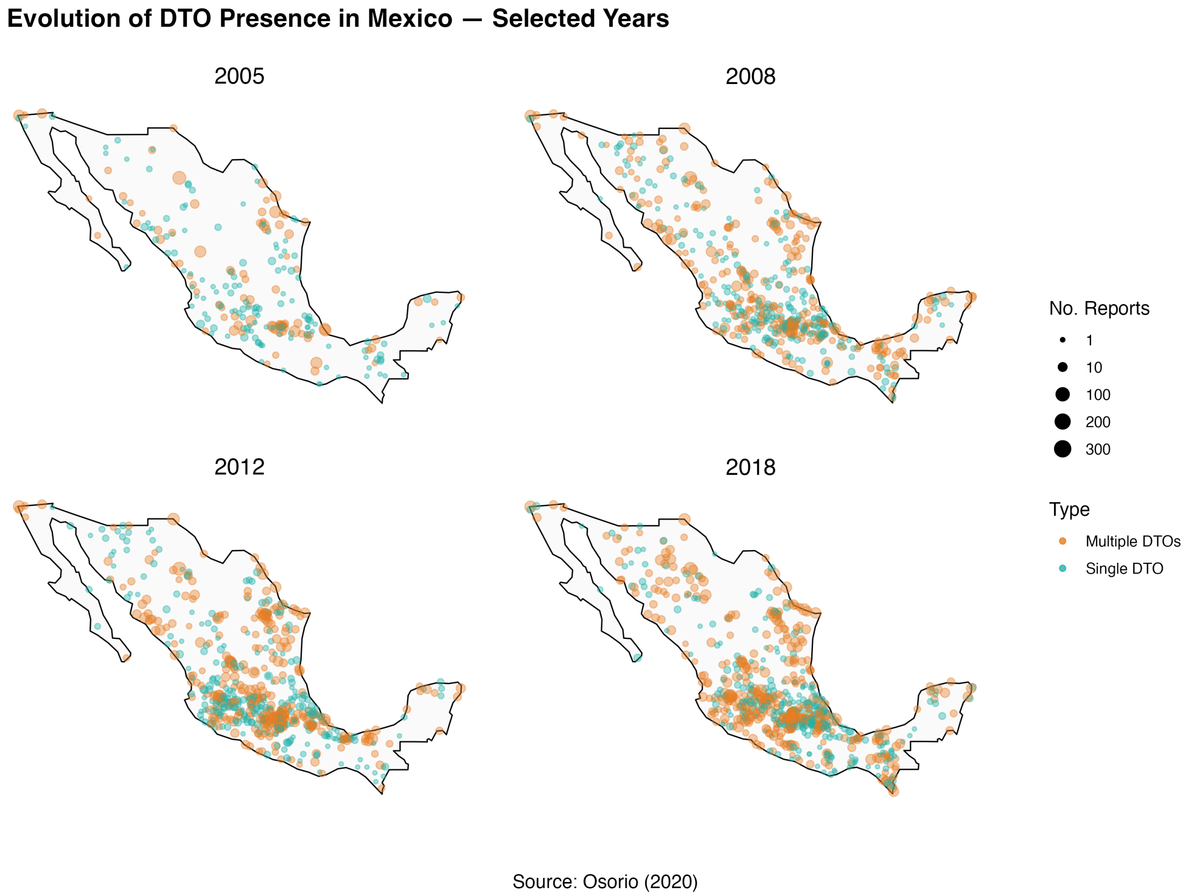

Figure 2: DTO presence in Mexico (2005, 2008, 2012, 2018). Size: reports per municipality–year; color: Single DTO (turquoise) vs. Multiple DTOs (orange). Source: Osorio and Beltrán (2020).

Figure 2 illustrates the evolution of DTO presence across selected years. In this context, "presence" refers to the detection of at least one report of a criminal organization in a municipality–year, as captured in the Osorio and Beltrán (2020) database. The maps reveal that, following the onset of the so–called "war on drugs" in 2006, criminal organizations expanded their geographic footprint and the proportion of municipalities under dispute increased substantially.

Reports, however, are not equivalent to sustained territorial control. They are an imperfect proxy subject to several forms of measurement error. First, media coverage is uneven across space and time: urban centers, tourist destinations, or municipalities with prior notoriety may receive disproportionate attention relative to rural or peripheral areas with comparable underlying conditions. Second, variation in policing intensity and reporting practices can inflate or suppress mentions independently of actual DTO presence. Third, high–profile events (e.g., a sensational arrest) can temporarily spike mentions without indicating durable control. Because these forces do not produce a natural break in the distribution of reports, there is no single, defensible cutoff that cleanly separates isolated incidents from systematic presence.

To address this uncertainty, I implement a multi–threshold strategy that maps different operational definitions of "sustained presence" onto the same underlying data. For each municipality i and year t, let $C_{i,t}$ be the total number of DTO–related reports and $P_{i,t}$ the population in that year. I define the annual rate per 10,000 residents as

$$R_{i,t} = \frac{10{,}000\, C_{i,t}}{P_{i,t}}.$$

I then compute *decile* cutoffs (10th, 20th, ..., 90th percentiles) from the pooled distribution of all $R_{i,t}$ in the sample; denote the pth cutoff by $r^{(p)}$. For each decile p, I construct, for every municipality–year:

1. **Binary treatment indicator.** The municipality is coded as treated if its report rate meets or exceeds the decile threshold:

$$D_{i,t}^{(p)} = \begin{cases} 1, & \text{if } R_{i,t} \geq r^{(p)}, \\ 0, & \text{otherwise.} \end{cases}$$

2. **DTO multiplicity.** The number of distinct DTOs reported as active in that year.

3. **Cumulative exposure.** The total number of years since 2000 (the first year covered by Osorio and Beltrán (2020)) in which the municipality met the pth cutoff.

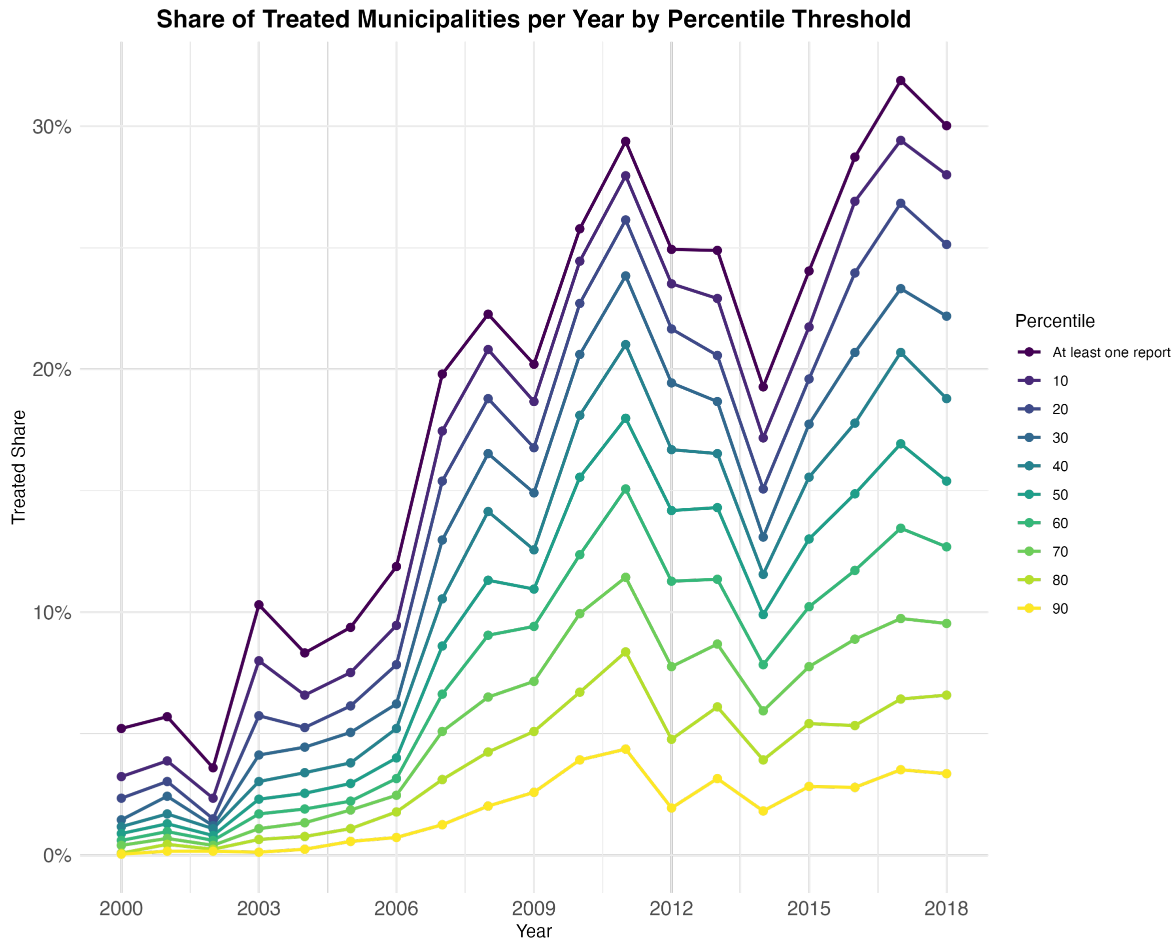

Figure 3: Share of treated municipalities (with DTO presence) per year by percentile threshold. Deciles correspond to stricter cutoffs based on reports of DTOs per 10,000 inhabitants.

Figure 3 shows that all thresholds indicate two distinct waves of DTO expansion: the first in the mid–2000s, and a second beginning in 2014 that drove treated municipalities to their peak in 2018—reaching nearly 30% under the most permissive definition.[7]

Finally, because DTO dynamics can have spillover effects across municipal borders, I incorporate spatial context with a single neighbor measure: the mean report rate among nearby municipalities. For each municipality, I identify its ten nearest neighbors using great–circle (Haversine) distance between centroids and, for each year, compute the unweighted average of their report rates. This mean neighbor report rate summarizes nearby activity

[7]Annex A presents the "ever treated" graph, showing the cumulative share of municipalities treated at least once, which peaks at nearly 60% of all municipalities in the country.

and is included as a spatial control to help separate local conditions from broader geographic spillovers.

4.4 Propensity Score Matching

For each decile-based treatment definition $D_{i,t}^{(p)}$ (where $p \in \{10, 20, \ldots, 90\}$ indexes the decile cutoff of the report-rate distribution), I run a separate propensity score matching (PSM) analysis so that results do not hinge on a single operational definition of treatment.

Outcomes and treatment. Let $Y_{i,t}$ denote market size for municipality i in year t. Define the outcome as the *percentage change*:

$$g_{i,t} \;=\; \frac{Y_{i,t} - Y_{i,t-1}}{Y_{i,t-1}} \times 100,$$

so that $g_{i,t}$ is the year-over-year change in percent. The treatment $D_{i,t}^{(p)}$ equals 1 if the municipality's DTO report rate per 10,000 residents in year t is at or above the pth-decile cutoff (computed from the pooled distribution), and 0 otherwise.

Assignment model. Because treatment is not randomly assigned, I model the assignment mechanism using pre-treatment covariates measured in the prior year.

$$X_{i,t-1} = \Big(Y_{i,t-1},\ \mathrm{FA}_{i,t-1},\ \mathrm{MI}_{i,t-1},\ \overline{R}_{\mathcal{N}(i),t-1},\ \mathrm{HR}_{i,t-1}\Big)$$

I then define the propensity score for decile p as:

$$e_{i,t}^{(p)} = \Pr\Big(D_{i,t}^{(p)} = 1 \mid X_{i,t-1}\Big) \tag{1}$$

Where:

Prior-year market size ($Y_{i,t-1}$). Baseline market size captures persistence and mean reversion in economic activity. Conditioning on $Y_{i,t-1}$ helps compare municipalities with similar initial market scales so that subsequent differences in growth are less likely to reflect pre-existing size differences.

Fiscal autonomy ($\boldsymbol{FA}_{i,t-1}$). Fiscal autonomy proxies local state capacity. Higher autonomy may affect both the likelihood of DTO presence (through weaker institutions) and market performance (through public investment and service provision). The variable is drawn from INEGI's Public Finance series, which reports annual municipal- and state-level public finance aggregates for Mexico (INEGI, 2025).

Marginalization index ($\mathrm{MI}_{i,t-1}$). The marginalization index—produced by Mexico's National Population Council (CONAPO)—summarizes socioeconomic disadvantage across dimensions such as income, educational attainment, housing quality, and access to basic services. These structural conditions can both shape exposure to DTO activity and independently influence market outcomes. Because CONAPO releases the index only at census and intercensal rounds (roughly every five years), I construct an annual series by linearly interpolating municipality-level values for non-observation years. (Consejo Nacional de Población (CONAPO), 2025)

Neighbor report rate ($\overline{R}^{\text{neigh}}_{i,t-1}$). The mean DTO report rate among the ten geographically nearest municipalities (identified by great-circle/Haversine distance) captures spatial dependence in treatment. Municipalities adjacent to areas with high reported activity are more likely to be treated next period, and nearby activity can also affect local markets.

Homicide rate ($\mathrm{HR}_{i,t-1}$). I compute the homicide rate per 10,000 residents from Mexico's official crime statistics published by the Executive Secretariat of the National Public Security System (SESNSP)(Secretariado Ejecutivo del Sistema Nacional de Seguridad Pública (SESNSP), 2025). Monthly homicide counts are aggregated to the municipality–year level and divided by the population to obtain rates per 10,000 residents; the series are then lagged one year. As a direct indicator of recent lethal violence, $\mathrm{HR}_{i,t-1}$ helps absorb contemporaneous security conditions that are correlated with DTO dynamics and local economic performance.

This specification yields a propensity score $e^{(p)}_{i,t}$ for each municipality–year. I use these scores to assign weights that balance the distribution of the observable covariates between treated and control units for each decile definition of treatment.

4.5 Model Specification

With the propensity-score weights from the matching step, I estimate the causal effect using a weighted two-way fixed-effects regression with clustered-standard errors at the municipality level. The outcome is the year-over-year *percentage change* in market size, denoted by: $g_{i,t} \equiv \frac{Y_{i,t}-Y_{i,t-1}}{Y_{i,t-1}} \times 100$.

$$g_{it} = \beta_p D^{(p)}_{i,t-1} + \gamma \, \mathrm{DISPUTED}_{i,t-1} + \delta \, \Delta\mathrm{GDP}_{it} + \alpha_i + \lambda_t + \varepsilon_{it} \tag{2}$$

where:

- β_p is the Average Treatment Effect for percentile p,
- $\mathrm{DISPUTED}_{it}$ captures contested municipalities (more than 1 active DTOs),
- GDP_{it} is the annual state GDP growth rate,
- α_i and λ_t are municipality and year fixed effects, respectively,
- ε_{it} is the idiosyncratic error term.

The regression implies three relevant counterfactual mean outcomes:

$$\begin{aligned}
\text{Untreated (no presence):} \quad & g^{(0)}_{i,t} = \alpha_i + \lambda_t + \delta \, \Delta\mathrm{GDP}_{i,t} + \varepsilon_{i,t}, \\
\text{Treated, non-disputed:} \quad & g^{(1)}_{i,t} = g^{(0)}_{i,t} + \beta_p, \\
\text{Treated, disputed:} \quad & g^{(2)}_{i,t} = g^{(0)}_{i,t} + \beta_p + \gamma.
\end{aligned}$$

5 Results

5.1 Matching Results

The analysis uses a full matching approach, which links treated and control municipalities into flexible sets that can vary in size, provided each set contains at least one treated and one control unit. This design maximizes the use of available data by allowing many-to-one and one-to-many matches, rather than forcing a fixed matching ratio.

To ensure valid comparisons between treated and control municipalities, the analysis restricts the sample to municipalities that had never received treatment—according to the specified threshold—by 2012, the first year for which complete data are available. This restriction substantially reduces the sample size, from 2,272 municipalities to 1,374 under the most stringent threshold (where treatment eligibility is hardest to meet) and to 210 under the most permissive threshold. Nonetheless, it is essential for obtaining unbiased estimates of the treatment effect. Without this restriction, the covariates used in the matching procedure could be influenced by prior treatment exposure, even for municipalities classified as untreated in the current period but exposed to treatment in earlier years.

In this context, Table 2 provides a detailed summary of the composition of the matched sample across treatment percentiles. For the 90th percentile, no treated municipality remained in the matched sample, and it is therefore excluded from interpretation. For each percentile, the table reports the number of treated municipalities—distinguishing between those associated with exactly one DTO and those associated with more than one—alongside the number of matched controls. In addition to these raw counts, the table presents the Effective Sample Size (ESS), a metric that accounts for the effect of matching weights and serves as an indicator of the statistical efficiency retained after matching.

Table 2: Post-matching counts, Effective Sample Size (ESS), and ESS as percentage of matched sample, by Treatment Percentile

Percentile	Matched Treated (1 DTO)	Matched Treated (>1 DTO)	Matched Controls	Total Matched	ESS (Overall)	ESS (% of Matched)
10	9	10	191	210	41.2	19.6
20	7	23	264	294	119.8	40.7
30	16	35	375	426	219.9	51.6
40	6	39	489	534	135.4	25.3
50	2	47	659	708	292.6	41.3
60	4	35	843	882	239.8	27.2
70	1	30	1121	1152	268.1	23.3
80	0	11	1363	1374	54.9	4.0

Regarding the results of the matching process, Table 3 reports the standardized mean differences (SMD) for each covariate before and after matching, across treatment percentiles from 10 to 80. The SMD is computed as the difference in means between treated and control groups, divided by a pooled standard deviation, providing a scale-free measure of imbalance that is comparable across covariates with different units. Positive values indicate that treated units have, on average, higher covariate values than controls, while negative values indicate the opposite.

Table 3: Standardized mean differences (SMD) before and after matching by percentile and covariate

Percentile	Y (t-1)		$\overline{R}^{\text{neigh}}_{i,t-1}$		HR (t-1)		FA (t-1)		MI (t-1)	
Percentile	**Pre**	**Post**	**Pre**	**Post**	**Pre**	**Post**	**Pre**	**Post**	**Pre**	**Post**
10	-0.75	0.03	-3.31	-0.35	-1.30	-0.15	0.29	-0.10	-0.05	-0.00
20	-0.11	-0.01	-1.09	-0.12	-1.02	-0.03	0.15	0.06	0.30	-0.01
30	0.06	0.05	-0.62	-0.00	-0.74	-0.03	-0.01	0.05	0.41	0.03
40	0.29	-0.00	-0.47	-0.06	-0.68	-0.04	0.01	-0.07	0.75	0.03
50	0.26	-0.01	0.04	-0.16	-0.28	0.01	-0.11	-0.03	0.72	0.01
60	0.33	0.12	0.12	-0.07	-0.45	0.06	0.08	0.04	1.13	0.03
70	-0.22	0.11	-0.71	-0.03	0.21	0.13	0.08	-0.01	0.16	0.01
80	-0.63	-0.26	-0.15	-0.15	0.55	-0.74	-0.30	-0.02	-0.30	-0.10

Note: Entries are standardized mean differences (SMD). Columns under each covariate report Pre- and Post-matching SMD. Values with |SMD| > 0.1 are highlighted in red.

At the most permissive thresholds, treated municipalities tend to be substantially smaller in pre-treatment market size, are located closer to other treated units, and, somewhat unexpectedly, display lower pre-treatment homicide rates than their control counterparts. They also show higher levels of financial autonomy, indicating greater institutional capacity despite their limited economic scale. One plausible interpretation of these patterns is that the municipalities that "survived" the initial wave of DTO expansion—thus remaining eligible to appear as newly treated in the sample—were precisely those with stronger institutional capacity and relatively secure environments, having initially resisted co-optation through reinforced security or governance measures. Ultimately, however, these same municipalities may have succumbed to DTO presence, suggesting that institutional resilience can delay but not always prevent infiltration when broader regional dynamics remain favorable to organized crime expansion.

As the treatment threshold becomes more restrictive, the differences between treated and control municipalities grow more pronounced. Notably, for thresholds between the 20th and 70th percentiles, treated municipalities exhibit substantially higher levels of marginalization than their control counterparts. This pattern suggests that the municipalities reaching the highest levels of DTO presence are disproportionately those facing severe socioeconomic disadvantages, where structural vulnerabilities may create favorable conditions for sustained organized crime activity.

A similar dynamic emerges for homicide rates. While treated municipalities start with lower baseline homicide rates at low thresholds, the difference with controls becomes substantially larger as the threshold increases. This widening gap is partly explained by contamination in the control group: at higher thresholds, many municipalities classified as controls still have some DTO presence, albeit below the threshold, and therefore already exhibit elevated levels of violence.

For pre-treatment market size, measured as the logarithm of the number of economic units, the standardized mean differences are largest at the most permissive (percentile 10) and most restrictive (percentile 80) thresholds, and comparatively smaller at intermediate levels. This pattern suggests that when the treatment criterion is very lax, treated municipalities tend to be substantially smaller than their controls because the group includes many economically modest localities that nonetheless meet the low bar for treatment. At the opposite extreme, a very strict threshold concentrates treatment in a small set of municipalities that are again markedly smaller in economic scale than the available controls. In contrast, at intermediate thresholds, the treated group includes a more economically diverse set of municipalities, which narrows the gap with controls.

After matching, balance improves markedly across all covariates. The substantial pre-matching disparities in $\overline{R}^{\text{neigh}}_{i,t-1}$ and HR_{t-1} are almost entirely eliminated, while FA_{t-1} and MI_{t-1} are brought well within the conventional $|\text{SMD}| < 0.1$ threshold in most cases. The 20th, 30th, and 50th percentiles stand out for achieving the most consistently balanced post-matching results, with nearly all covariates falling below the imbalance threshold. By contrast, the 80th percentile exhibits substantial residual imbalance across nearly all covariates, making it unsuitable for credible causal inference under the present specification.

5.2 Two-way fixed effects results

Taking into account the sample size at each cutoff, as well as the quality of covariate balance achieved in the preceding matching stage, this section reports the results from the econometric specification designed to estimate the causal effect of drug cartel entry into Mexican municipalities on the annual growth rate in the total number of businesses.

The model is estimated within a two–way fixed effects framework, incorporating current state–level GDP growth as a control variable to account for broader economic trends. Standard errors are clustered at the municipality level. Table 4 presents the estimated coefficients of interest at different treatment thresholds.

Across most percentile thresholds considered, the coefficient associated with the treatment condition (β_p)—defined as the presence of at least one DTO—does not attain conventional levels of statistical significance. The sole exception is observed at the 40th percentile, where the estimated coefficient is moderately negative and statistically significant at the

10 percent level (-2.083, s.e. 1.051), suggesting that cartel entry under this threshold is associated with an approximate two–percentage–point reduction in annual business growth. However, the lack of a consistent pattern across cutoffs constrains the ability to identify a systematic and robust treatment effect of cartel presence alone.

In contrast, the coefficient on the territorial dispute indicator (γ_p)—capturing the additional effect when more than one cartel is present—exhibits a more stable and negative association with business growth. At the 10th, 40th, 60th, and 70th percentiles, γ_p is negative, statistically significant, and in some cases large in magnitude (e.g., -4.768 at the 10th percentile and -3.056 at the 40th percentile), indicating that contested criminal environments exert an additional adverse influence on local economic performance.

The total effect on disputed municipalities is computed as

$$\theta_p = \beta_p + \gamma_p,$$

The standard error of θ_p is obtained using the delta method, which in this linear case reduces to the formula:

$$\mathrm{SE}(\hat{\theta}_p) = \sqrt{\mathrm{Var}(\hat{\beta}_p) + \mathrm{Var}(\hat{\gamma}_p) + 2\,\mathrm{Cov}(\hat{\beta}_p, \hat{\gamma}_p)}.$$

Here, $\mathrm{Var}(\hat{\beta}_p)$ and $\mathrm{Var}(\hat{\gamma}_p)$ are the estimated variances of the coefficients reported by the regression model, while $\mathrm{Cov}(\hat{\beta}_p, \hat{\gamma}_p)$ is the estimated covariance between them, also obtained from the model's variance–covariance matrix. This approach correctly accounts for the correlation between the two estimates, ensuring that the reported standard error for θ_p reflects both the individual estimation uncertainty and their joint dependence.

Table 4: Effects by percentile: β_p (Treated), γ_p (Disputed), and $\theta_p = \beta_p + \gamma_p$ with cluster-robust standard errors (municipality) and post-matching sample sizes.

Decile	$\boldsymbol{\beta_p}$ **(Treated)**	$\boldsymbol{\gamma_p}$ **(Disputed)**	$\boldsymbol{\theta_p}$ **(Total)**	$\mathbf{N}_{\text{treated}}$	$\mathbf{N}_{\text{control}}$	$\mathbf{N}_{\text{total}}$
10	0.521 (1.778)	-4.768^{**} (1.778)	-4.247 (3.243)	19	191	210
20	-0.810 (1.867)	-2.423 (1.924)	-3.233 (2.445)	30	264	294
30	-0.356 (1.146)	-3.354 (2.390)	-3.710^{*} (1.993)	51	375	426
40	-2.083^{*} (1.051)	-3.056^{**} (1.286)	-5.139^{***} (1.758)	45	489	534
50	-1.533 (1.083)	-0.978 (0.861)	-2.511^{*} (1.388)	49	659	708
60	0.157 (1.467)	-1.593^{**} (0.734)	-1.436 (1.752)	39	843	882
70	-0.341 (1.164)	-1.141^{*} (0.588)	-1.482 (1.301)	31	1121	1152

Robust SEs clustered at the municipality level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

For instance, at the 40th percentile, the estimates are $\hat{\beta}_{40} = -2.083$ (s.e. 1.051) and $\hat{\gamma}_{40} = -3.056$ (s.e. 1.286), with a positive covariance term. Applying the formula above yields:

$$\hat{\theta}_{40} = -5.139 \quad (\text{s.e.} = 1.758),$$

indicating that municipalities with both cartel presence and active territorial disputes exhibit, on average, a reduction of approximately 5.1 percentage points in their annual business growth rate relative to untreated municipalities, with this combined effect statistically significant at the 1% level. At multiple thresholds, particularly at the 30th, 40th, and 50th percentiles, θ_p is negative, statistically significant, and larger in absolute magnitude than either β_p or γ_p individually. Overall, results suggest that the combination of cartel presence with territorial dispute produces consistently negative and, in several cases, statistically robust impacts on local market dynamics. This supports the interpretation that competitive criminal environments generate an additional layer of instability and deterrence for economic activity, beyond the baseline effects of single–cartel control.

5.3 Result limitations

Overall, the restriction of limiting the sample to municipalities that had not received treatment prior to 2012 substantially reduces the statistical power to detect significant effects. This is reflected in the relatively large standard errors observed across almost all treatment thresholds.

Beyond sample size considerations, the results should be interpreted with particular caution for treatment deciles that exhibited covariate imbalances in the previous section. For instance, it is highly plausible that the coefficient on *Disputed territories* in the 10th percentile is directly influenced by spillover effects from municipalities treated in $t-1$. This concern is supported by the fact that the standardized mean difference of R_t^{neigh} remains at 0.35 after matching—well above the conventional 0.10 threshold—indicating a substantial residual imbalance that may bias the estimated effect.

Similarly, at higher treatment thresholds, it is plausible that some municipalities classified as controls systematically experience the presence of DTOs, yet without exerting a statistically detectable impact on local market size. This misclassification would imply a potential selection bias, which could artificially inflate the estimated coefficients for treated units at these thresholds.

Finally, the interpretation of these results is inherently constrained by the temporal structure of the data. The estimated coefficients capture only the short–term effect of DTO entry—specifically, the change in the year immediately following their arrival—and do not reflect medium– or long–term economic consequences. As such, any persistence, attenuation, or amplification of these effects over time remains outside the scope of this analysis.

6 Conclusions

This study examines how the arrival and sustained presence of drug-trafficking organizations (DTOs) shape local market size in Mexico, with particular attention to whether these effects vary by type of territorial control. Following the framework proposed by Magaloni et al. (2019), a monopoly-controlled territory can give a single DTO incentives to allow local businesses to keep operating, ensuring a steady flow of extortion revenues. In contrast, when two or more groups compete for the same area, those incentives change—violence tends to escalate, uncertainty increases, and the conditions for running a business deteriorate sharply.

To address this, the analysis combined three main elements. First, an annual measurement construction of market size at the municipal level by training a supervised learning model on establishment-level census data, banking indicators, and socio-demographic variables. This step filled the gaps between economic censuses, which are too far apart to capture short-term changes.

Second, the analysis draws on the municipality–year dataset on DTO presence developed by Osorio and Beltrán (2020), which applies natural language processing to media and official reports. Building on this source, I introduced a multi-threshold framework to capture different intensities of sustained presence, and incorporated spatial spillover measures to account for DTO activity in neighbouring municipalities. These steps allowed me to distinguish between uncontested and contested control while testing the robustness of the results across varying definitions of treatment.

Third, treated and control municipalities were matched on key pre-treatment variables and estimated effects using a two-way fixed-effects model, with state GDP growth included to control for broader economic trends.

The results present a mixed picture. In municipalities where only one DTO was active, the short-term effect on annual business growth was small and generally not statistically significant. At only one threshold—the 40th percentile—did the effect become weakly significant. This pattern fits the idea that monopolistic DTOs might avoid excessive extraction in order to keep the local economy functioning enough to support their own revenue streams, at least in the first years of control.

In contested municipalities—those with more than one active DTO—the additional effect on business growth was consistently negative at several thresholds, often reaching statistical significance. In some cases, the combined effect of presence and dispute amounted to a drop of between 2.5 and 5 percentage points in annual business growth. This is consistent with accounts of contested criminal environments producing more violence, disruption, and predatory practices that push firms to close or relocate.

The study also has important limitations. Restricting the analysis to municipalities untreated before 2012 reduced the risk of bias from prior exposure, but it also cut the sample size, especially at lower thresholds. That, in turn, limited statistical power and widened confidence intervals. The DTO presence data, while rich and detailed, come from media and official reports and are therefore affected by coverage bias—urban, high-profile, or strategically important places tend to get more attention than remote rural areas. The multi-threshold strategy and spatial controls help, but they cannot fully remove this problem. And the outcome measure captures only the first year after DTO entry, so the results reflect short-term effects rather than medium- or long-term dynamics.

Even with these caveats, the findings have clear policy implications. The most severe economic damage is concentrated in contested territories. Reducing the intensity of territorial disputes—through targeted security operations, disrupting the capacity of rival groups to compete, or strengthening local institutions—could help protect local markets. At the same time, it is important to recognise that simply replacing contested control with monopolistic control does not solve the problem of criminal governance. A territory under a single DTO may experience less disruption, but it remains under coercive taxation.

There is also scope for extending this work. The market-size proxy developed here could be adapted to other countries with similar data limitations. Alternative indicators of local economic activity, like electricity consumption, satellite night-lights, or mobile payments, could be added to cross-check results. Disaggregating effects by sector, firm size, or informality could also shed light on which types of businesses are most vulnerable to each type of control.

In the end, this study shows that not all DTO presence is equal. The short-term economic effects are much worse when rival groups are fighting than when a single group controls the territory. That distinction matters—for understanding the economic impact of organised crime, for interpreting the mixed results in the existing literature, and for designing policies that aim to limit the economic damage while addressing the underlying security problem.

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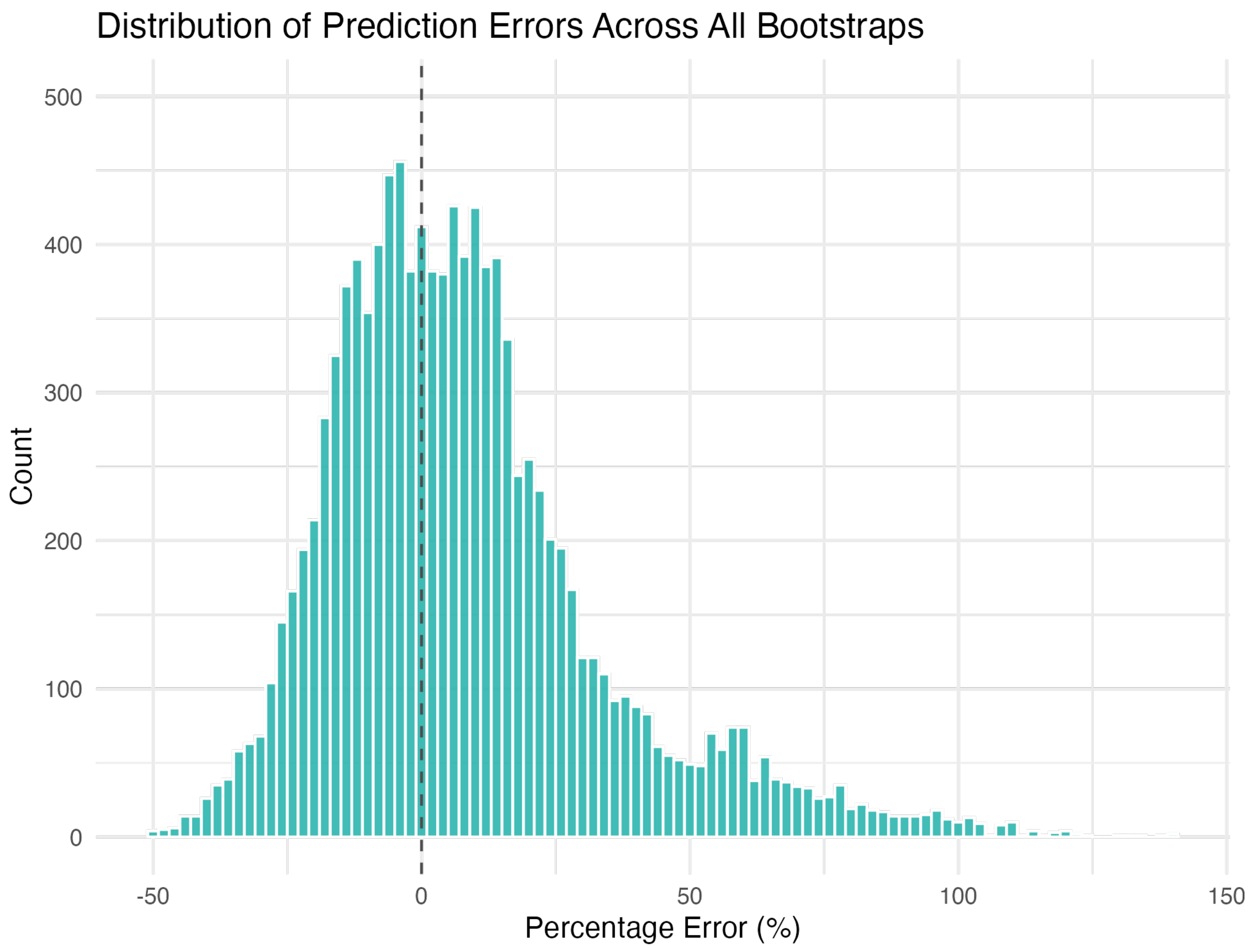
A Random Forest Bootstrap Errors Count



B Cumulative Share of "Ever Treated" Municipalities