Effect of the Mexican War on Drugs on Violent Crime and Economic Activity

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Abstract

We explore the economic consequences of organized crime, focusing on the Mexican War on Drugs as a natural experiment. Namely, we assess and document the effect on violence and on economic activity of kingpin removals—arrests or killings of high-ranking officers of criminal organizations. In a difference-in-differences approach that accounts for the staggered adoption design of the removals, we find they triggered violence in Mexico, increasing homicides (+55%), extortions (+43%), and kidnappings (+96%). The surge in violence affected economic outcomes both at the micro and macro level, decreasing firm survival (−7%) and aggregated firm production per capita (−17%).

Keywords: War on Drugs; Kingpin removals; Homicides, extortion, kidnapping; GDP per capita; Firm survival; Staggered Adoption Designs; Great Recession; Export manufacturing sector

JEL Codes: C23, D02, F16, I31, I38, L60

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1 Introduction

What are the economic consequences of organized crime? Criminal organizations rely on violence to enforce contracts or to expand their own businesses against their competitors (Beittel, 2013; Castillo et al., 2020). They also provide jobs and income, especially in marginalized communities, that combined with a romanticized culture they evoke (Grillo, 2012) alter career incentives and education choices of young men (Dammert, 2008; Damm and Dustmann, 2014; Dell et al., 2019; Melnikov et al., 2019). Their negative impact, however, goes beyond illegal markets. From the Sicilian mafia and the Chicago mob to those operating in modern times, criminal organizations participate in racketeering and extortion of legal businesses, threatening and deploying violence to coerce compliance.[1]

By exploiting a natural experiment in the context of the Mexican War on Drugs, we document the economic consequences of a shock that altered the way criminal organizations operated. We provide empirical evidence of the negative effects of kingpin removals—the arrests or killings of high-ranking officers of criminal organizations—on homicides, extortion, and kidnapping; and on economic outcomes both at the macro- and at the micro-economic level. Before the War on Drugs, Mexican criminal organizations sporadically relied on violence and mainly trafficked drugs. A few years in, they increased in number and in use of violence, and ventured away from drugs towards other profit-seeking activities. From being no more violent than cities in the U.S., cities in Mexico became extremely violent—some becoming as violent as war zones.[2] From being rare crimes affecting mainly well-off individuals, extortions and kidnappings became endemic, affecting businesses and the population at large. We find that kingpin removals increased homicides by 55 percent, extortions by 43 percent, and kidnappings by 96 percent. The surge in violent crime had broad economic consequences. Kingpin removals decreased GDP per capita by 17 percent and firm survival by 7 percent.

Our paper fills an important knowledge gap. While violence and organized crime are

[1] Anecdotal evidence and news reports about business extortion in modern times can be found, in addition to Mexico, for many countries along the supply chain of drugs. Widespread business extortion has been reported for Afghanistan, the main producer of opium seeds (New York Times; 2017, 2018); for the city of Karachi in Pakistan, through which much of Afghan opium is shipped (Financial Times 2012; Reuters 2013); for Honduras, which lies on the same route between Andean cocaine production and U.S. consumers as Mexico (Insight Crime, 2017); and for several countries in West Africa, through which South American cocaine is shipped to European markets (El Universal, 2017).

[2] In 2016 the mean homicide rate in U.S. cities was 8.9 per 100,000 (SD=9.2, N=213; Uniform Crime Reporting Program of the FBI. "Crime in the U.S.", 2016.). In 2005 the mean homicide rate in Mexican cities was about the same, 8.9 per 100,000 (SD=10.0, N=121; Homicides from administrative health records, INEGI). In 2010, in the midst of the war, the homicide rate in Mexican cities surged to 23.7 per 100,000 (SD=39.9).

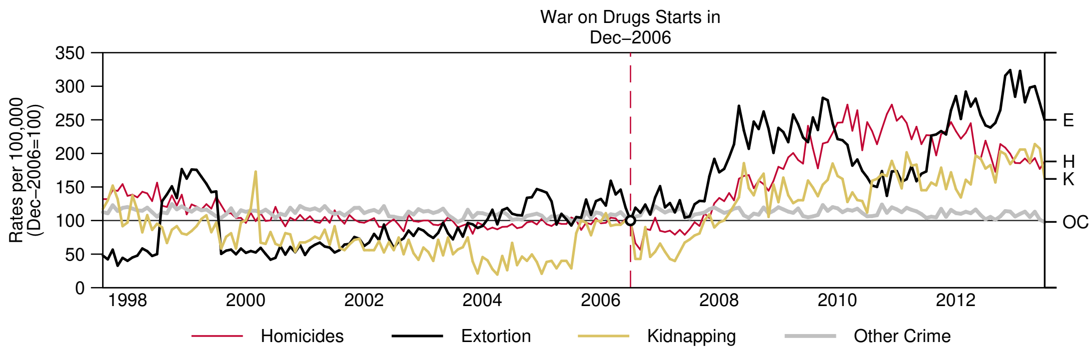
recognized as a security issue, and as a detriment to human development outcomes (for example, Currie et al., 2018; Camacho-Thompson and Vargas, 2018; Damm and Dustmann, 2014), little is known about the broader economic consequences of organized crime. These economic consequences could be economically significant, especially those of crimes that threaten property rights by expropriating the victim, such as extortion and kidnapping. However, empirical studies of these issues pose several important challenges. First, fearing retaliation, victims seldom report the crimes to authorities, silence often being a condition to escape harm. As a result, administrative data, even if nominally very comprehensive, tends to be inadequate for detecting any effects. Second, organized crime reaches far beyond its direct victims, but disentangling its economic consequences from other economic shocks is fraught with difficulties, in particular at the aggregate level (Abadie and Gardeazabal, 2003). At the local level, sufficiently granular information on economic outcomes is often unavailable. Third, even when sufficient data are available, reverse causality is often a credible threat for identifying the effects of crime on outcomes.

Mexico abounds with detailed information on crimes and on economic outcomes, allowing to overcome these measurement challenges. We rely on a uniquely rich combination of high-quality information sources, including administrative records from the police and the court system; several rounds of state-of-the-art crime victimization surveys partially going back before the start of the War on Drugs; data on the universe of formal employment based on social security records; and 25 years of economic census data covering all business establishments. From these data, we make our starting observation: during the War on Drugs kidnapping and extortion rates rose at a similar time, in a similar pattern, and by similar magnitudes as the homicide rate. Other crimes stayed almost constant (figure 1, panel a). At the same time, while violent crime increased, GDP growth slowed down noticeably after the start of the War on Drugs, and even before the Great Recession (figure 1, panel b).

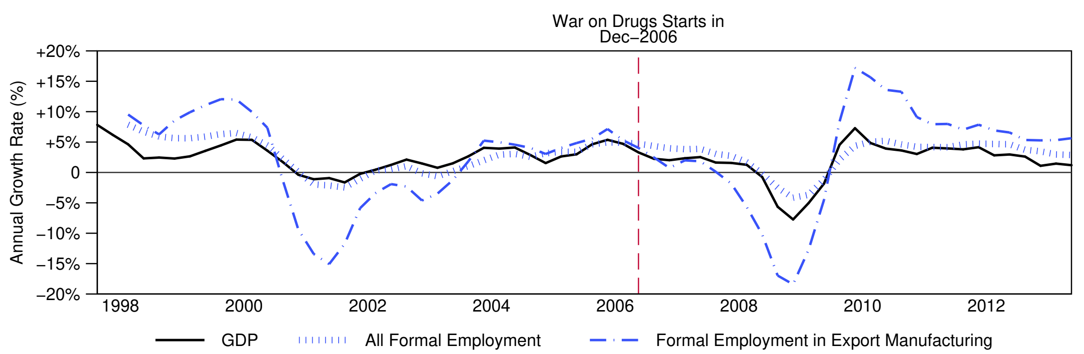
Our identification strategy exploits the way the War on Drugs was fought as a series of local events, providing us with a group of municipalities that were directly affected by it at different times, and a group of control municipalities that were not immediately affected beyond national trends. Specifically, we exploit the occurrence of kingpin removals—the successful arrests or killings of high-ranking officers of criminal organizations—as a shock to activities of organized crime in the affected location. Using a difference-in-differences research design, we estimate the effect kingpin removals had on the municipalities that experienced them. The first successful kingpin removal in a municipality, we argue, is a local shock that tipped the balance of power within and among drug trafficking organizations,

Figure 1: The Relation of the War on Drugs with Violent Crime and Economic Activity

A. Violent Crime



B. Economic Activity



Letters denote: Homicide, Extortion, Kidnapping, and Other Crime. Rates per 100,000 people according to population from censuses 1990 to 2020. Source: (1) Homicide rate: Administrative records of deaths by homicide, National Institute of Statistics and Geography (INEGI) (2) Crime rates: State level administrative records of reported crimes to the police, Executive Secretariat of the Public Security National System. (3) GDP: Quarterly GDP growth, seasonally adjusted series, National Institute of Statistics and Geography (INEGI). (4) Formal Employment: Own estimations using Mexico's census of formal employment (IMSS dataset).

changing the way they operate and causing a surge of violent and property crime. We focus on five outcomes at the municipality level that we construct from microdata—homicide, extortion, and kidnapping rates; GDP per capita; and firm survival—and a period of analysis corresponding to the six-year period preceding the War on Drugs and to the six-year period of federal administration under president Felipe Calderón, who started the Mexican War on Drugs in December of 2006. The end of his term in December of 2012 officially ended the war.

The identification strategy relies on the timing of a first removal being essentially random.[3] We document many anecdotes that suggest governments take months, more often years, to succeed with operations against a kingpin. Often luck granted success. Whereas the timing and exact location of removals are essentially random, the set of municipalities experiencing removals differs from the average municipality in Mexico. Organized criminals live and operate in populous places with infrastructure—border crossings to the U.S., ports, or toll highways—valuable for trade of both legal and illegal goods. We therefore choose a set of control municipalities with similar characteristics as municipalities with kingpin removals, notably population.

To account for potentially differential trends of municipalities with kingpin removals and control municipalities without them, we augment the standard two-way fixed effects model in two ways. First, we allow for a set of deterministic differential trends by baseline characteristics, including the strategic location of the municipality, the baseline capacity of its law enforcement and criminal justice system, and the baseline population. These trend controls should capture any factors that might have interacted with the underlying differences in event municipalities post 2006; for example, a general rise in violence in places exposed to Mexican drug trafficking owing to cocaine seizures in Colombia (Castillo et al., 2020).

The Great Recession, which occurred halfway during the War on Drugs, poses a threat to identification that deterministic linear trends alone might not sufficiently capture. The Great Recession spilled over from the United States, where it originated through a domestic housing-market and mortgage-lending burst, to Mexico via the economic linkages created by manufacturing in Mexico of final U.S. consumer goods. As figure 1 shows, export manufacturing employment started to decrease from the first quarter of 2008—almost contemporaneously with the start of the recession in the U.S. in December 2007. 'Maquiladora' factories in export-oriented industries are located close to ports, to the Mexico-U.S. border, or to key transportation hubs—the same infrastructure critical and valuable for trafficking drugs. The Great Recession affected specific municipalities given their local industrial structure, and could have led to violence because of the concurrent job destruction of young and low-educated males (Dell et al., 2019).[4] To account for this time-varying and potential confounding effect,

[3] We only focus on the first removal at a municipality because further removals might be endogenous to the first. Being a destabilizing shock, especially if accompanied by violence, the aftermath of a first removal attracts the attention of the government, potentially causing further removals.

[4] Atkin (2016) shows that trade reforms in the late 1980s and early 1990s inadvertently induced Mexican teenagers, males in particular, to drop out of school. Throughout the Great Recession, many young and low-educated males, who worked in the same locations that drug trafficking organizations value, found themselves suddenly unemployed. Dell et al. (2019) show that surges in unemployment of young males

we control directly for location-specific labor demand shocks using Bartik-type employment measures that project national employment trends on a specific location given its industrial structure.[5] Specifically, we control for a reduced-form Bartik instrument of employment in export-intensive manufacturing, in other manufacturing, and in other sectors, calculated from a monthly census of formal employment.

The first result concerns the effect of kingpin removals on homicides. Using monthly data of homicides from administrative health records, we find that kingpin removals increased the homicide rate by 9.2 per 100,000 people, equivalent to a 55 percent increase over municipalities without removals. Exploiting the monthly frequency and the universal coverage of the series, we validate our identification strategy and provide empirical evidence against reverse causality by recasting the data. Our natural experiment corresponds to a staggered adoption design, each municipality becoming permanently treated at different times.[6] When the time of treatment varies, two-way fixed effects equations will retrieve unbiased estimates of the average treatment effect on the treated only when treatment effects are homogeneous across time, which is hard to test within the design (Goodman-Bacon, 2021). Placebo tests, typically used to provide empirical evidence against reverse causality, are uninterpretable when time of treatment varies (Sun and Abraham, 2021).[7]

To validate our identification strategy, we recast the information into the well-known difference-in-differences design with multiple time periods and a single adoption date. We impute time of removal in control municipalities by taking random draws from the time distribution of the first removal at each location. This is valid under our identification assumption that removals are random events conditional on fixed effects and trend controls. Results show that homicides were not increasing in event municipalities before the first kingpin removal relative to control municipalities, allaying concerns of reverse causality. Given that most removal events took place well into the War on Drugs in 2008 and 2009, this should also allay concerns about any differential trends that might have remained after implementing the strategy we discuss above. Results also show that treatment effects are roughly homogeneous in the period we analyze, suggesting that a two-way fixed effects applied to the staggered

suitable as foot-soldiers for the cocaine trade raises the value of a location, prompting drug trafficking organizations to fight for its control, the fights leading to violence in Mexico.

[5]Strictly speaking, the national trends for each municipality are given by the trend in all other municipalities, leaving out the focus municipality.

[6]Refer also to Athey and Imbens (2018), Callaway and Sant'Anna (2021), de Chaisemartin and D'Haultfœuille (2020) for recent advances concerning staggered adoption designs.

[7]Note also that conventional placebo tests are under-powered, failing to reject the null hypothesis of no difference in trends (Roth, 2022).

adoption design should retrieve unbiased estimates. We obtain a point of 9.3 per 100,000 people using two-way fixed effects applied to the staggered adoption design, an estimate practically identical to our headline estimate of 9.2.

Kingpin removals led to surges in extortion and kidnapping. Unlike for homicides, where administrative vital registry data are available monthly and for a long time series, information on the crimes comes from a unique set of annual crime victimization surveys that offer the best possible measurement of these crimes.[8] For extortion, we additionally lack comparable data before the War on Drugs; for kidnapping, a single pre-2006 wave is available. This precludes using both municipality and time fixed effects. However, we can reproduce difference-in-difference estimates where is possible (for homicides and kidnapping) by replacing municipality fixed effects with a set of municipality control variables, obtaining an almost identical point estimate. A set of observable municipality characteristics in practice is as good as municipality fixed effects, suggesting that the observable controls contain all relevant municipality characteristics. This raises confidence that, in our specific setting, a cross-sectional regression can be a credible identification strategy for the extortion rate. We find that kingpin removals increased the kidnapping rate by 143 per 100,000 adults, equivalent to a 96 percent increase over municipalities without removals; and the extortion rate by 3,288 per 100,000 adults, equivalent to a 43 percent increase.

We then assess the effects of kingpin removals on economic outcomes. From the micro data of the economic census, we construct a series of GDP at the municipality level (a statistic which is not reported in official sources) by summing up the value added of all economic establishments in the municipality. This allows us to estimate the effect of kingpin removals on GDP per capita.[9] We find that kingpin removals decrease GDP per capita by 17 percent in affected municipalities. The sizable economic effect parallels the effect of wars or catastrophes.[10] When we include information for 2018, more than a decade after the start

[8] List survey experiments are the gold standard in the literature on measurement of crimes (Blair and Imai, 2012). But they are usually only done for small, non-representative groups. In Mexico we know of one list experiment conducted by Magaloni et al. (2011) comprising a single cross section in 2011 and eliciting information about extortion from around 1,800 individuals. We retrieve extortion rates similar to the ones they find.

[9] Our preferred specification for economic outcomes excludes the year 2008 because it overlaps with the Great Recession; which not only affected employment, it affected production directly. When we include 2008 in robustness checks, we find similar results for both GDP per capita and the firm survival rate.

[10] Of the 187 countries or territories with information for 1998 and 2013 in the World Development Indicators (World Bank), in only 17 of them GDP per capita at constant prices decreased. In Zimbabwe, a country marred by crises, the GDP per capita decreased by 20 percent, a decline similar to the one we find for municipalities that experienced captures.

of the War on Drugs, we find the same result. The economic effect not only is sizable, it persists.

The economic outcome at the microeconomic level is firm survival in the universe of businesses in Mexico. We use the method by Busso et al. (2018) to track individual firms across waves of the Economic Census. The method allows us to estimate the number of firms in a municipality that were also present in the preceding economic census. We find that kingpin removals reduce by 4 percentage points the proportion of firms surviving for at least five years, a reduction in the survival rate equivalent to 7 percent.

Our paper contributes to two main strands of literature. First, we contribute to the literature on violence in illegal markets. The existing literature focuses on homicides almost exclusively whereas we also consider surges in extortions and kidnapping to be a critical consequence of shocks occurring during the War of Drugs.[11] A few papers have attempted to link events that caused violence in Mexico to economic outcomes (Utar, 2022; for example) but often rely on indirect indicators of economic activity or in analysis of particular industries, or provide cursory and descriptive evidence on the role of property crimes.[12] Our paper is the first to analyze homicides, expropriating crimes, and broad economic outcomes within a unified framework.

Second, we contribute to the literature on the consequences of organized crime for economic activity and development. Several papers estimate the medium and long-run effects of the Italian mafia on economic outcomes (Acemoglu et al., 2019; Pinotti, 2015). Pinotti (2015) finds that the mafia in regions of southern Italy reduces GDP per capita by 16 percent in the medium term in affected regions; an estimate very similar to ours. Whereas the silent presence of organized crime, perhaps in complicity with corrupt officials, might be in a 'stationary bandits' equilibrium that seeks to maximize gains from formal or informal taxation (Sánchez de la Sierra, 2020), the unravelling of equilibria in illegal markets can be expected to unleash short-term rent exploitation and expropriation.[13] In that sense, our

[11] For example, it focuses on the effects on homicides of trade shocks (Dell et al., 2019; Dix-Carneiro et al., 2018), economic shocks (Dube et al., 2016), or suppression policies (Angrist and Kugler, 2008; Calderón et al., 2015; Castillo et al., 2020). Calderón et al. (2015) analyze the effect of kingpin removals on violence and find that homicides increase after them. Their identification strategy, however, lacks controls for time-varying confounders.

[12] Montoya (2016) studies the relation between homicides and firm-level outcomes. The paper provides detailed evidence of firm-level adjustments to violence in incumbent businesses, complementing the broader outcomes we analyze. But like other studies before (Gutiérrez-Romero, 2016; Contreras, 2014; for example), its focus on property crimes is cursory and descriptive.

[13] See also Magaloni et al. (2011) who study organized crime and extortion from a political science perspective,

paper is also related to a long strand of literature on the economic consequences of weak property rights. Contrary to influential early studies in that literature (Acemoglu et al., 2001; Johnson et al., 2002) our focus is on an encroachment on private property rights not by the state or by state officials, but by other private actors in a context where the state has lost its monopoly on violence. In that sense, our findings provide some evidence on the mechanisms by which the ability of the state to guarantee the rule of law translates into economic development.

The paper proceeds as follows. Section 2 provides the context and institutional background. Section 3 discusses the data sources and shows that kingpin removals concentrate in populous, strategic municipalities. Section 4 describes our sample and presents descriptive evidence. Section 5 details the identification strategy, explaining specific adjustments for each of the outcomes and describing how we transform the staggered adoption design to a difference-in-differences with multiple time periods and a single adoption date. Section 6 provides the main results and adjustments for multiple hypothesis testing. Section 7 details robustness checks, focusing on outliers, weighting, spillover effects, varying the the definition of the export-intensive manufacturing sector, and varying the period of analysis of economic outcomes. The section concludes by describing the results of additional robustness checks presented in the appendix. Section 8 concludes.

2 Context and Institutional Background

2.1 Mexican Drug Trafficking and Competition for Strategic Locations

Drug trafficking in Mexico emerged and flourished due to a large demand in the United States for illegal drugs. The estimate of the annual value in the U.S. of the market of illegal drugs, most of them arriving via Mexico, amounts to 108 billion dollars (Soloveichik, 2020), a large figure given that the value of all legal goods Mexico exports to the U.S. amounts to 327 billion.[14] Sharing a porous 2000-mile land border with U.S., Mexico has an ideal location in the narcotics supply chain. Until the 1980s the Mexican drug market comprised domestically grown marijuana and opium that were partially exported to the United States. Then, Mexican criminal organizations started shipping cocaine for Colombian producers, and soon the cocaine trade became a main line of business. More recently, they also started trafficking synthetic drugs from Asia.

using a small-sample list survey. Consistent with our results, they report higher extortion by organized crime in high-violence areas. Also broadly related is Alesina et al. (2019) who find that organized crime selectively uses targeted violence to influence elections and ultimately political outcomes.

[14] Both estimates correspond to 2017.

Drug trafficking in Mexico at its core is a wholesale and logistics business that depends on controlling few strategic locations. Synthetic drugs produced in China and cocaine grown in Colombia and in Peru enter Mexico via the Southern Pacific ports; as well as via Caribbean seaports such as Cancún. Imported and domestic drugs then move through the network of Mexican highways towards border crossings to the U.S. Contraband of illegal drugs relies on a limited capacity of customs authorities and security forces to monitor all ships, trucks, or personal vehicles carrying legitimate cargo or people. But few seaports or border crossings have a large enough cargo volume for significant amounts of illegal drugs to enter and leave Mexico undetected among legal traded goods.[15,16] Scarcity explains why drug trafficking organizations vie for the control of strategic ports and border crossings and of the highways connecting them.[17]

Before the War on Drugs, drug trafficking organizations (DTOs) operated within a relatively stable market-sharing arrangement.[18] Traditionally, Mexican DTOs have been organized as a coalition of local franchises that each control a specific strategic location, known as a 'plaza' (Grillo, 2012). Some organizations had a nationwide footprint and full control over the supply chain of drugs, whereas other organizations were based locally, especially around critical U.S. border crossings. Alliances and allegiances of local franchises to superregional and national strongmen were fluid, DTOs being both rivals and collaborators.[19] But changes in the political economy of Mexico, in particular increased political competition in the 1990s and 2000s, had disrupted market-sharing arrangements, rendering the equilibrium fragile.[20]

[15] For example; of the fifty official border crossing points, the ones located in the municipalities of Juárez, Tijuana, Reynosa, and Nuevo Laredo process most of the trucks moving goods between Mexico and the U.S. (Wainwright, 2017).

[16] Besides ports and border crossings, highways are valuable because drug traffickers want to minimize the cost of transporting drugs by using the most direct routes to the U.S. (Dell, 2015). Within the Mexican transportation network, federal highways with tolls encompass the most transited and valuable routes for trade of legal and illegal goods (Basu and Pearlman, 2017).

[17] It also explains why drug trafficking organizations base their operations in few locations within Mexico. Using online sources and a predictive algorithm, Coscia and Rios (2012) find that drug trafficking organizations are present in a few populous municipalities with border crossings, seaports, or major highways.

[18] At the outset of the war of drugs, seven organizations dominated the drug trade—Gulf; Sinaloa; Los Zetas; Beltrán Leyva; La Familia Michoacana; Tijuana adjoined with the Arellano Felix organization; and Juárez adjoined with the Carrillo Fuentes organization (Beittel, 2013).

[19] For example, several DTOs with a foothold around key border crossings allowed other organizations to operate in their territory for a fee (Guerrero-Gutiérrez, Eduarto, 2011; Beittel, 2013).

[20] Political scientists and historians assert that during much of Mexico's history, hegemonic rule by a political party (the PRI, ruling all levels of government from 1929-1989 and continuously holding the presidency until 2000) fostered a peaceful co-existence of drug trafficking organizations. Once the opposition, already controlling several states, won the presidency, an hegemonic rule could no longer guarantee impunity to trafficking organizations (Beittel, 2018), grant protection from rival organizations (Trejo and Ley, 2018), or

2.2 The War of Drugs as a Shock to the Equilibrium

The consensus narrative (e.g. Guerrero-Gutiérrez, Eduarto, 2011; Beittel, 2013; Calderón et al., 2015) ascribes the beginning of the War on Drugs to the first days of the incoming federal administration of President Felipe Calderón in December 2006. Narcotics policy or security issues played no role in the election campaign, which was fought on economic and social issues.(Boullosa and Wallace, 2015). During his second week in office, President Calderón sent a large garrison of army, navy and federal forces to siege drug cartel operatives encroaching on Michoacán, his home state. Whereas previous administrations had occasionally fought drug traffickers and made high-profile arrests, Calderón's military campaign soon became the largest in Mexico's history (Shirk and Wallman, 2015).

Calderón's preferred strategy consisted of taking out 'kingpins' of drug trafficking organizations through operations of federal forces, including the army and specialized units of federal police (Guerrero-Gutiérrez, Eduarto, 2011; Beittel, 2013; Calderón et al., 2015). Leaders targeted in such operations were captured or killed while resisting arrest, often violently. While kingpin removals frequently occur in the strategic locations where DTOs operate (as Section 3 shows), a lot of randomness is involved in if and when an operation is successful. For example, an operation, praised as a model example by the DEA of the U.S. and leading to the arrest on January 12, 2010 of Eduardo Teodoro, took five full months to complete, despite committed intelligence and military cooperation between both governments.[21] Another successful intelligence effort took almost a year, and the target barely escaped only to be captured later.[22] Some kingpin removals resulted by chance from routine operations such as traffic stops.[23] Randomness plays a large role in whether and when a kingpin removal happens.

The removal of kingpins broke the equilibrium in Mexican drug trafficking. Governments

settle property rights disputes over traffic routes and plazas (Castillo et al., 2020).

[21] The DEA issued a press release heaping praise on Mexican authorities (https://www.dea.gov/sites/default/files/divisions/hq/2010/pr011310p.html). The timeline is detailed in: Expansión staff "Los 'grandes capos' detenidos en la guerra contra el narcotráfico de Calderón." *Revista Expansión.* 6 Nov 2010 (link to article).

[22] After ten months of intelligence and cooperation, on December 11, 2009, Arturo Beltrán Leyva escaped a raid that left many of his bodyguards death. Shortly after the botched capture attempt, the U.S. discovered where he was hiding and apprised Mexican authorities who, on December 16, 2009, killed Beltrán Leyva. Dudley, Steven and Young, Rick. "Mexico The Takedown of 'The Boss of Bosses' '' *FRONTLINE Investigative Report, PBS.* 3 Feb 2011. (link to article).

[23] On February 10, 2002, a police officer pulled over a car for a routine check. The police officer failed to recognized Ramón Arellano Félix who got nervous and fatally shot the officer. The officer shot back and killed him (Boullosa and Wallace, 2015).

strive to take out leaders because their absence, at minimum, hampers the organization and, at best, unravels it (Shirk and Wallman, 2015). Such a strategy worked in Colombia, where taking out the leaders of the Medellín and Cali organizations unraveled the organizations. In their place, smaller and less violent organizations emerged (Beittel, 2018). But in Mexico the strategy failed. As early as 2007 the power vacuum caused by kingpin removals was reported to engender violence.[24] Several mechanisms can cause violence once a leader has been removed—for example, encroachment by other DTOs or bloody leadership succession conflicts within the affected DTO. Members taking over leadership often surpass their predecessors in using violence. Although the organization often survives removal of its leader, offshoots usually splinter from it.[25] Beyond the direct effects of removals themselves, an increased risk of removal of leaders weakens the relational contracts that sustain collaboration in illegal markets (Castillo, 2013). In a context of weaker relational contracts, more violent leaders, combined with more organizations willing to fight for the few strategic locations in the drug trade, exacerbate violence.

The removal of kingpins also was reported to have contributed to an expansion of DTOs into other lines of revenue-generating activities, including extortion and kidnapping (Jones, 2013; Beittel, 2013). Violence after removing a kingpin disrupted local drug revenue and increased the need for funds to finance weapons and fighters. As a result, local factions expanded into extortion and kidnapping as additional revenue-generating activities.[26,27] Kingpin removals exacerbated increases in extortion and kidnapping because the new generation of leaders surpassed their predecessors in use of violence and were more willing to expand to extortion and kidnapping (Jones, 2013). High baseline levels of violence also change the benefits and costs from inflicting suffering on civilians who are not themselves involved with the drug trade. The fear created in society by the surge in violence further bolsters the success rate of extortion and kidnapping attempts (Wainwright, 2017). Throughout the War on

[24] Cook (2007) in a report to the U.S. congress wrote that "arrests of key cartel leaders, particularly in the Tijuana and Gulf cartels, have led to increasing drug violence as cartels fight for control of the trafficking routes into the United States" (pp. 1).

[25] At the outset of the war, seven organizations dominated the drug trade. By its aftermath the seven organizations splintered to between 9 or 20 large criminal organizations operating closely with as many as 80 smaller ones (Beittel, 2018).

[26] For example, in 2008 the Sinaloa Cartel began its hostile takeover of Ciudad Juárez, location critical for the cocaine trade. The Sinaloa Cartel sieged the city and took out the drug distribution centers and safe-houses operated by the Juarez Cartel. Deprived of drug trafficking funds, the Juarez Cartel expanded to extortion and kidnapping. Dudley, Steven. "How Juárez's Police, Politicians Picked Winners of Gang War." *Insight Crime*, 13 Feb 2013. https://www.insightcrime.org/investigations/juarez-police-politicians-picked-winners-gang- war/

[27] Extortion, kidnapping, and other illegal business made so much money for the organizations first venturing into them that all organizations eventually expanded beyond drug trafficking (Boullosa and Wallace, 2015; Wainwright, 2017)

Drugs, kidnapping evolved from a crime victimizing a few rich people for large ransoms in operations that took months to a crime targeting poor or middle-class families for small ransoms in operations that took days or hours (Grillo, 2012; Boullosa and Wallace, 2015). From being relatively rare incidents in Mexico, extortion and kidnapping became endemic.[28, 29]

Extortion and kidnapping have devastating and direct effects on economic activity and on general welfare. A successful small or medium-sized business publicly associated with the owner and her family clearly signals the ability to pay when threatened. Family firms are often eponymous (Lemos and Scur, 2019) and therefore, easily linked to their owners. Entrepreneurs and professionals such as doctors or lawyers send similar public signals, increasing their risk of extortion (Jones, 2013). Extortion affects firms directly because drug traffickers tax business owners for protection from other traffickers and from themselves.[30] Kidnapping also affects the same type of population, either as a punishment for non-compliance with extortion demands, or independently due to a high perceived capacity to pay a ransom for the business owner or their family.[31] Besides their direct effect on their victims, extortion and kidnapping likely discourage business activity and wealth accumulation.

2.3 A Real-world Case Study

Before detailing data sources and laying out an empirical identification strategy, we narrate a real-world case of a Mexican town that usually escapes the narratives about the War on Drugs.[32] It illustrates the hypothesized link we propose between kingpin strategies and increases in homicides, extortions, and kidnappings, which in turn decrease economic activity and firm survival.[33]

Tepic, the capital of the small state of Nayarit, has about 300,000 inhabitants. It lies on an interstate highway that connects the U.S. border to main ports in the Pacific, around 200 kilometers south of Tepic, and that crosses Sinaloa, the birthplace of Mexican drug

[28] Neves, Yuri. "Cartel Extortion Getting Worse for Businesses in Mexico City." *Insight Crime* 20 May 2019 (link to article)

[29] Smith, Rory. "Hundreds of people in Mexico are kidnapped every year. And the problem's getting worse." *Vox* 11 May 2018 (link to article)

[30] When Ciudad Juárez was in the midst of a battle between the Sinaloa and the Juarez organizations, a small business owner paid the Juárez organization a weekly contribution roughly equal to the pay of one of his employees (Boullosa and Wallace, 2015).

[31] When Tijuana was amid a succession battle within the Arellano Felix organization, small business owners had to pay local gangs for refraining from kidnapping their families and affiliates (Jones, 2013).

[32] Tepic receives no mention neither on books by Grillo (2012), Wainwright (2017), or Boullosa and Wallace (2015) nor on reports by Beittel (2013; 2018) to the U.S. Congress.

[33] See Appendix A.1.1 for newspaper covers that describe the narrative.

trafficking and a major opium and marijuana producer. On April 12, 2010, Tepic went into lockdown. Hundreds of armed federal police operatives and local criminal foot-soldiers swarmed the streets after an early-morning raid that intended his capture instead left Santiago Lizárraga Ibarra dead.[34] Lizárraga was the local plaza chief for the Beltrán Leyva organization, engulfed in an internal succession struggle. After Lizárraga's death, a wave of killings ensued. Gruesome executions, daylight shootings, and frequent police and military raids were common. Banners urging the "car thieves from Mazatlán" to vacate Tepic lest they all be killed were hung on public spaces, a direct message from the local faction weakened by Lizárraga's death to the faction encouraged by his death to take over. That year the homicide rate surged to 92—it was 9 in 2005.

While Tepic registered 59 homicides in June 2010 alone, the police chief held a press conference on the latest concerning development: a spike in extortions, mostly by phone, of residents and business owners.[35] Fear pervading society prompted people to pay instead of ignoring or reporting the calls. This development was isolated to Tepic; no reports were received from other towns in Nayarit. After some time homicides waned, but extortion persisted. In June 2011, a popular town bookstore that had been run for over 50 years by the founder and his family suddenly closed. As local sources put it, we might never know whether the store closed because economic activity nationwide struggled; or because demand for books from traditional bookstores decreased; or because owing to extortion shaking the community the store's revenue had collapsed; or because the firm's founder, owner, and family patriarch had mysteriously disappeared.[36]

Are these anecdotes from Tepic isolated and random incidents, or do they show a pattern of kingpin removals and their effects on homicides, extortion, kidnapping, and economic activity? That is what our identification strategy seeks to answer. Before describing it, we first discuss the sources of our data.

[34] Staff La Jornada. "Muere en Tepic El Chaguín, mando del cártel de los hermanos Beltrán Leyva." *La Jornada* 13 Apr 2011. (link to article).

[35] Escobedo, Leonel "Bajo el Indice de Amenazas Telefónicas en la Ciudad de Tepic" *El Sol de Nayarit* 25 Jun 2010 (link to article).

[36] Mendoza, Juan José. "Cerró 'Publicaciones Azteca' '' *Economía desde Nayarit* 18 Sep 2011 (link to article).

3 Data

3.1 Kingpin Removals

Information on arrest or killings of drug trafficking organization leadership comes from Calderón et al. (2015). The dataset covers the period December 2006 to December 2010 and provides the date and municipality of arrest or killing of 18 national and 119 regional leaders. Kingpin removals took place in 73 municipalities and in Mexico City (which we drop from the main analysis). Our dependent variable turns one for a municipality when the first kingpin removal at the municipality happened, and zero otherwise.[37] Consistent with 2007 and 2008 being the less violent years, two thirds of all kingpin removals happened in 2009 or in 2010 (figure A.3 in the appendix). Against the common misconception that the war on drugs mostly affected the northern part of Mexico, kingpin removals took place all over the country. The map in the appendix (figure A.4) shows the spatial distribution of municipalities with kingpin removals.[38] All regions in Mexico experienced removals. Removals concentrate on municipalities with seaports, border crossings to the U.S., or tolled highways.[39] These types of infrastructure characterize locations that are strategic for shipping drugs imported from abroad or grown domestically to the U.S. Kingpin removals also concentrate in populous municipalities (see tables A.2 and A.3 in the appendix). Small municipalities seldom experienced a kingpin removal—only 2 municipalities below the median of municipality population, 1,200 inhabitants, experienced a removal—while all municipalities with 1 million people or more did.

[37] Calderón et al. (2015) consider the effect on violence of each of the kingpin removals at the municipality. We only consider the first event on each municipality because if each event leads to violence locally, then the event can ease further removals locally by further attracting government attention.

[38] Figure A.4 also presents an inset and a table. The inset shows the area encompassing the municipalities of Tepic and Puerto Vallarta. Puerto Vallarta is a large municipality, similar in size to Tepic, with a main port. A federal tolled highway connects it to another main port nearby and to Tepic, hence both municipalities are endowed with strategic infrastructure. But Tepic experienced a kingpin removal whereas Puerto Vallarta did not. In 2005 the homicide rate for both municipalities was similar, around 8 or 9 per 100,000, close to the national average. In 2010, it increased to 17 in Puerto Vallarta—an increase in line with the national trend—but it surged to 79 in Tepic.

[39] Half of the municipalities with kingpin removals have at least one of the three types of strategic infrastructure while only 14 percent of the municipalities without them do. In Mexico 1 percent of municipalities have border crossings with the U.S., 15 percent toll highways, and 2 percent seaports. For municipalities with removals the corresponding percentages are 11, 42, and 10.

3.2 Outcomes: Homicides, Extortion, and Kidnapping

Homicides

Information on homicides comes from vital records compiled by Mexico's National Statistical Institute (INEGI). We construct a monthly balanced panel of municipalities that covers from 1990 to 2018. Our dataset covers all death certificates with a recorded homicide where the municipality in which the death occurred is known.[40] Alternatively, some of the literature—e.g. Dell et al., 2019; Calderón et al., 2015; Castillo et al., 2020—focuses instead on a subset of the vital records that a government committee ascribed to the war on drugs. Besides some limitations, such as limited coverage (2006-2011) and not being publicly available, it is not clear whether all the homicides that are relevant to our paper would be classified as drug-related.[41]

Extortion and kidnapping

Lack of information on criminality and victimization probably explains the dearth of empirical evidence on extortion, kidnapping, and the economic effects of these expropriative property crimes. We utilize a variety of survey and administrative data sources, which complement each other in terms of breadth and depth of coverage.

The main source for extortion and kidnapping incidence is the *Encuesta Nacional de Victimización y Percepción sobre Seguridad Pública (ENVIPE)* survey series. The survey from INEGI is specifically designed to measure crime victimization in households and in persons 18 years or older. Although victimization is elicited through a direct question and not via a list experiment, the victimization rate from the ENVIPE falls well within the margin of error of list surveys in Mexico.[42] From 2010 to 2016, we estimate (using survey weights) the

[40] The most recent data vintage covers the period 1990-2018 and includes all death certificates registered in the country. Our data omits homicides where the location is unreported and lacks missing persons or clandestine burials. Around 3,300 death certificates (0.7 percent of the total) have no information on the municipality where the death occurred. The Mexican National Registry of Kidnapped or Disappeared Persons (*Registro Nacional De Personas Extraviadas o Desaparecidas; RNPED*) estimates that 14,000 individuals disappeared during 2006-2012. By 2020, most of them are still missing. In recent years, 2,000 clandestine burials with at least 3,000 bodies have been reported (Guillen et al., *The Intercept*, 13 Dec 2018; https://theintercept.com/2018/12/13/mexico-drug-war-mass-graves/). If anything, we expect homicides not included in our dataset to be higher in the most violent municipalities, which would lead us to under-estimate the effect of kingpin removals.

[41] For instance, while homicides that result from drug traffickers killing each other are likely included in the government classification, it is unlikely that a homicide to punish a civilian's non-compliance with extortion, or a homicide of a kidnapped business owner, would be classified as "war on drugs related."

[42] Magaloni et al. (2011) conducted a nationwide list-survey experiment (N=2500) in 2010. They report that 10

total of all extortions and kidnappings per year and per municipality and calculate the rate per adult population.[43]

We make explicit adjustment for three important features of this survey. First, the survey does not cover all municipalities in Mexico. Only 18 percent of all municipalities, accounting for 73 percent of Mexico's population, have at least one household interviewed in every year. We find a high correlation between municipality population with both the probability of being in the ENVIPE sample and the number of households interviewed.[44] In regressions using the ENVIPE survey, we restrict the sample to municipalities with at least 30 interviews in every year, assigning a value of zero to years in which interviewees report no extortions or no kidnappings. Second, the ENVIPE starts after the onset of the war on drugs. To obtain baseline information, we use the 2004 wave of the ENSI survey.[45] The information allows us to obtain baseline information on kidnapping (allowing us to perform difference-in-differences analysis) but not on extortion. Third, fear might have impelled persons to decline being interviewed. In the appendix, we show that non-response rates are indeed higher in states with higher proportion of population living in municipalities with kingpin removals (figure A.5). Our estimates could then be biased if persons declining being interviewed were more likely to had experienced extortion or kidnapping. But as we will show, the results are robust to dropping the states with consistently high non-response rates (Tamaulipas and Quintana Roo).

We complement the survey information with administrative records on crimes from the police and the courts. In contrast with survey information, the records cover all municipalities and all reported or prosecuted cases of extortion and kidnapping. Police records are compiled by the Executive Secretariat of the National Public Security System (SESNSP), a government

percent (SE=4 percent) of the adult population suffered extortion. The national victimization rate estimated from the ENVIPE in the same year is 7.6 percent of the adult population.

[43]For each year we verified that the sum of our municipality estimates corresponds to the total number of the extortions and kidnappings reported by INEGI. We report extortion and kidnapping rates in terms of adult population instead of total population. Our preferred series of municipality population lacks estimates for the population 18 years or older. We use estimates for the population 15 years or older instead.

[44]The mean of the population of municipalities in the sample is 12 times the mean of municipalities outside the sample (168,189 vs 14,098). The correlation of municipality population and number of interviews is 0.73.

[45]Information about crime from the ENSI (*Encuesta Nacional Sobre Inseguridad*) exists in principle for 2004, 2008 and 2009. However, there is a controversy in Mexico around the information for 2008 and 2009, years that postdate the onset of the war on drugs (see https://inegi.org.mx/contenidos/programas/ensi/2010/doc/comunica1.pdf). The high level of violence, and possibly also the inexperience of the NGO conducting the fieldwork, let to very high non-response rates and to counter-intuitive patterns in the data in 2008 and 2009. The survey was then discontinued and replaced by the ENVIPE, with an improved methodology and under direct responsibility of the highly experienced National Statistics Institute. For this reason, we only use information from the 2004 wave, several years before the onset of the War on Drugs.

agency that oversees and coordinates across different levels of government public policy on safety matters. Data at the municipality level is available only since 2011. Court records at the municipality level are compiled by INEGI for the period 1997-2012. They comprise a subset of the police records: crimes being investigated and having named suspects. However, police records, and even more so court proceedings, vastly understate experienced crime victimization. For example, from the ENVIPE survey, we estimate 5.5 million extortions and 164,000 kidnappings in 2010. The same year, 757 extortions and 2,994 kidnappings appeared in court proceedings.[46] Work by National Statistics Institute suggests that extortion and kidnapping have the largest rate of under-reporting in administrative data among all categories of crime (INEGI, 2018).

Table 1 provides yearly extortion, kidnapping, and homicide rates. It reveals that the crimes surged during the War on Drugs and highlights that administrative records vastly understate the crimes.

3.3 Outcomes: GDP per capita and Firm Survival

GDP per capita

Information on GDP per capita at the municipality level is not reported in official Mexican statistics, but it can be calculated from the micro-data of the Economic Census. The quinquennial census of INEGI captures detailed information on non-agricultural production from all economic establishments in Mexico. Our measure of municipality level GDP is the sum of the gross value added of all establishments in the municipality.[47] Five waves allow us to calculate total gross value added for two points in time before the war on drugs (1998 and 2003), and for three points from its outset to beyond (2008, 2013, and 2018).[48] We restrict the sample for this outcome to municipalities with strictly positive gross value added in all five years (95 percent of all municipalities and 97 percent of the Mexican population). We omit from the preferred specification information for 2008 because it coincides with the large

[46] Police records only show 6,116 extortions and 1,223 kidnappings. Police records separately report kidnappings under federal and local jurisdiction. We only count reported kidnappings under local jurisdiction because municipality level information of kidnappings under the federal jurisdiction is not available for the period of analysis, explaining why in our datasets the number of reported kidnappings is lower than the number of prosecuted kidnappings.

[47] Variable created by the National Statistics Institute. It equals the value of production and services minus inputs and services used by the business. We express the measure in millions of Mexican pesos per person in constant 2018 prices.

[48] For descriptive statistics and for robustness checks, we add information from the second economic census corresponding to 1993. Information from the first economic census corresponding to 1988 is not publicly available.

Table 1: Crime Statistics: Extortion, Kidnapping, and Homicides
Rates per 100,000

Year	*Prosecuted*		*Reported*				*Experienced*		Homicides
	Extortion	Kidnapping	Extortion		Kidnapping*		Extortion	Kidnapping	
			St.	Mun.	St.	Mun.			
2000	0.9	3.1	1.8		0.9				10.8
2001	0.9	3.0	2.0		0.8				10.3
2002	0.9	3.0	2.4		0.6				10.0
2003	1.1	3.1	2.8		0.6				9.8
2004	1.1	3.0	3.4		0.5			60.9	9.1
2005	0.9	2.9	4.2		0.4				9.7
2006	0.9	2.6	4.3		1.0				9.9
2007	0.8	2.9	4.2		0.6				8.3
2008	1.1	3.3	6.4		1.2				13.1
2009	0.9	3.5	8.1		1.5				17.9
2010	0.9	3.8	7.7		1.5		6874.9	204.9	23.2
2011	0.9	4.4	5.7	5.3	1.8	1.5	5390.4	163.4	24.0
2012	0.8	3.7	8.8	8.2	1.7	1.5	7260.2	127.6	22.4
2013			9.8	9.3	2.0	1.8	9284.6	157.2	19.3
2014			6.8	6.7	1.6	1.6	9339.6	120.6	16.5
2015			5.9	5.8	1.2	1.2	8161.1	71.3	16.9
2016			6.0	5.9	1.3	1.3	8490.9	78.4	20.3

Rates use population from quinquennial censuses 2000 to 2020. Extortion and kidnapping rates use population 15 years of age or older and homicide rates use all population. Reported crime: The series starts in 1997 at the national and state levels and in 2011 at the municipality level. Rates are higher at the state level because some crimes report the state of occurrence but not the municipality. *Partial count: kidnappings prosecuted by federal jurisdiction were not reported in databases at the municipality or state levels before 2014. For this reason the prosecuted rate for kidnapping is higher than the reported to the police rate. The reported series in the table exclude federal jurisdiction kidnappings available from 2014 onwards.

drop in Mexico's GDP caused by the Great Recession (see figure 1). To account for the potentially confounding factor of the Great Recession in other years (which was transmitted into Mexico via manufacturing and trade), as a robustness exercise we estimate for each municipality the gross value added of manufactures and exclude it from total gross value added.

Firm survival

We also use information of the Economic Census to estimate firm survival. A firm in the Economic Census survived if it was also present in the previous wave. Firm survival can be estimated for the years 1998, 2003, 2008, and 2013.[49] We restrict the sample for this outcome to the sample of municipalities we use for GDP per capita. But the sample is slightly smaller because we were not given information from a few municipalities to preserve confidentiality of business responses.[50] To create the municipality firm survival rate, we divide the number of firms surviving by the number of firms in the municipality in 2003. Using the number of firms from a year preceding the War on Drugs precludes any effect of the war on the denominator of the firm survival rate.

Early waves of the Economic Census lack a code that identifies each firm across waves. For early waves, we use the algorithm by Busso et al. (2018) to identify firms across waves. For more recent waves, we use a unique and stable firm identifier assigned by INEGI.[51] The rate of successful matches by the algorithm increases over time, which likely reflects changes in match quality overtime besides potential changes in true survival. This will only introduce a bias in our difference-in-differences design if the trend in matching quality differs across municipalities with and without kingpin removals. To assess the possibility of such a bias, we exploit that matching results are available for different stages of the algorithm, corresponding to different degrees of confidence in the match. As a robustness check, we compare point estimates using all stages of the algorithm with estimates using only the first stages, which Busso et al. (2018) find produce better matches.[52] Point estimates using different stages will be similar if matching error does not induce bias.

[49] To ensure that businesses' responses are confidential, a panel of firms is not available to researchers. Instead researchers work with firm-level data in a computer laboratory in Mexico city through requests curated by the staff. The 2018 Economic Census was not available for calculations at the time we visited the INEGI micro data laboratory.

[50] We were not given information from municipalities with fewer than 30 businesses or with fewer than 3 businesses surviving. Few municipalities in the regression sample met these conditions.

[51] The National Statistics Institute started using a unique business identifier in the round corresponding to the year 2008. Previous rounds lack a unique identifier.

[52] Their algorithm uses nine stages, each matching firms not matched in previous stages. The first six stages provide 95 percent of the matches of all nine stages. Busso et al. (2018) find that matches in the first six stages correspond in higher proportion to the unique identifier from INEGI relative to the remaining three.

3.4 Other Variables

Municipality population

To express outcomes in rates per person, we carefully choose the denominator. When using population as a denominator to variables in rates per person, one needs to account for the possibility that violence, extortion, and kidnapping could have impelled persons to flee their home.[53] We use municipality population from quinquennial censuses 1990-2020 and complement them with model-based population projections for 2010-2030 by Mexico's National Population Council (CONAPO). Our preferred population denominator combines the actual population until 2005 with CONAPO's projections thereafter, linearly interpolating between census years. The results, however, are robust to using alternative ways of expressing variables in rates per person, namely: (1) using the 2005 population for all periods in our sample and (2) using the actual population from quinquennial censuses 1990-2020.

Formal employment

We use a census of formal employment to construct time-varying controls for labor demand shocks in export-intensive manufacturing, other manufacturing, and non-manufacturing. By law, employers must register contracts with the Mexican Social Security Institute (IMSS) so that the government can provide employees with health services and pensions; this database contains the universe of formal employment and provides monthly information at the municipality level from 1997 to date.

First we define export-intensive manufacturing similar to Atkin (2016). Using information from the National Statistics Institute (INEGI), we consider all employment in seven of the twenty-one sub-sectors that compose the manufacturing sector as export-intensive manufacturing employment because in the seven sub-sectors the percent of total production produced by export manufacturing firms is higher than 34, the mean for the manufacturing

[53] Narratives about the war on drugs argue many persons fled their homes impelled by violence. Boullosa and Wallace (2015) report that tens of thousands left Cd. Juarez between 2007 and 2011 and that one hundred thousand dwellings were abandoned. However, quantitative evidence—e.g. Basu and Pearlman (2017)—suggest that increases in homicides did not result in higher out-migration from the municipality of residence.

sector.[54, 55, 56] Then we obtain for each municipality a monthly series of predicted employment levels in export-intensive manufacturing, other manufacturing, and non-manufacturing for 1997 to 2018 by multiplying a demand shocks index (by construction equal to one in 2005) to the level of employment in the municipality in the corresponding month in 2005. We create the labor demand shocks index by interacting per industry (equivalent to 4-digit NAICS) initial municipality employment shares in 2005 with national industry employment growth rates following Bartik (1991).[57] As a robustness exercise, we compare the main results with results using two alternative definitions of export-intensive manufacturing employment. Table A.1 in the appendix details the sub-sectors that compose export manufacturing employment in the preferred definition and in the two alternative definitions.

[54] INEGI. (2018). *Valor agregado de exportación de la manufactura global.* Aguascalientes, México. Instituto Nacional de Estadística y Geografía.

[55] INEGI considers that a firm is an export manufacturing if it fulfills at least one of three conditions: (1) the firm exports all of its production and imports more than two-thirds of its inputs, (2) the firm is owned or controlled by foreign firms, (3) the firm exports most of its production and fulfills none of the previous conditions, but it is part of a global value chain.

[56] We use the average from 2003 to 2006, the first year with information and the year preceding the War on Drugs, to calculate the percent of total production produced by export manufacturing firms in each sub-sector. Refer to table A.1 in the appendix.

[57] We use the following equation:

$$\widehat{growth}^{s}_{mt} = \sum_{j \in s} \left(\frac{L^s_{mj,2005}}{L^s_{m,2005}} \right) \times \left(\frac{\sum\limits_{\substack{1<x<n \\ x \neq m}} L^s_{xj,t} - \sum\limits_{\substack{1<x<n \\ x \neq m}} L^s_{xj,2005}}{\sum\limits_{\substack{1<x<n \\ x \neq m}} L^s_{xj,2005}} \right)$$

The first component is the share of employment in industry j of total municipality employment in the sector (L^s_m) corresponding to a given month in 2005. The dataset from IMSS encompasses 276 industries According to how we define export-intensive manufacturing employment, 47 compose the export-intensive manufacturing sector, 99 other manufacturing, and 130 non-manufacturing. The second is the national industry j growth rate excluding municipality m for a given month in 2005 relative to the corresponding month in years 1997 to 2018. For each of the three sectors we obtain a monthly series of predicted employment levels ($\hat{L}^s_m$) for 1997 to 2018 by multiplying the growth rate ($\widehat{growth}^{s}_{mt}$) by the level of employment in the municipality in the corresponding month in 2005 ($L^s_{m,2005}$).

4 Descriptive Evidence

This section provides descriptive time-series evidence on trends in outcomes with data both before and after the onset of the War on Drugs; namely, homicides, GDP per capita, and firm survival. We compare trends for municipalities that experienced kingpin removals with trends for a chosen group of municipalities. The comparison provides a graphical depiction of the effects of removals and informs a difference-in-differences identification strategy detailed in the next section.

Sample

First we need to select the municipalities that make up the control group. As detailed in the preceding section, kingpin removals happened not only in populous municipalities with infrastructure strategic for shipping drugs to the U.S., they also happened in every municipality with a population of 1 million or more. Levels of outcomes, in particular of GDP per capita, of municipalities with kingpin removals should differ from levels in any control group created from the remaining municipalities. Empirical methods that force equal levels of outcomes before the shock of interest (e.g. matching or synthetic controls) can be prone to reversion to the mean bias (Daw and Hatfield, 2018).[58] Therefore, instead of creating a matched control group, we trim the sample by discarding all municipalities below the median population, which corresponds to about 12,000 inhabitants in 2005. The strong correlation of population with kingpin removals allows us to discard many potentially bad counterfactuals and only two municipalities with kingpin removals (see tables A.2 and A.3 in the appendix). As a robustness exercise, we present estimations using different percentiles to trim the sample.

The trimmed sample comprises 1,220 observations. Of them, 71 municipalities and Mexico City experienced removals. The main analysis excludes Mexico City, which is added back to the sample as a robustness check. The remaining municipalities divide into two groups: (i) adjacent to municipalities with removals (232) and (ii) not adjacent (916). Because they are likely affected by potential spatial spillovers, we remove from the main analysis municipalities adjacent to municipalities with removals. The control group then comprises 916 municipalities.[59] We limit the period of analysis to the presidential periods of Calderón

[58] Matching forces equality of means, but different populations likely have different means of outcomes before the shock of interest. After the shock, unconstrained means revert to the value of their corresponding population, leading to biased estimates.

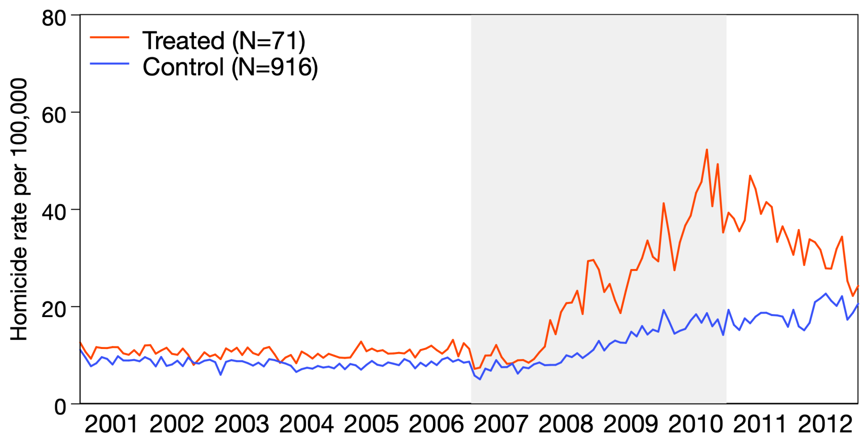
[59] Osorio (2015) argues that drug-related violence in Mexico spreads spatially to nearby areas. The map in figure A.4 in the appendix identifies the municipalities discarded.

(2006-2012) and of his predecessor (2000-2006).[60] As a robustness exercise, we expand the period of analysis.

Homicide rate

Figure 2 presents homicide rate trends in the sample and in the period of analysis. The shaded area demarcates the period of kingpin removals. Before the onset of the War on Drugs, trends in both group are stable. Slopes are neither positive nor negative; they revolve around their respective constant mean. In the presidency of Calderón, trends diverge. Municipalities with kingpin removals experienced substantial increases; those without them, a shallower and smoother increase.

Figure 2: Homicide Rate Trends



The shaded area shaded area demarcates the period of kingpin removals (December 2006 to December 2010). Weighted using municipality population in 2005.

Table 2 takes a closer look at trends before onset of the War on Drugs, at the level of individual municipalities instead of broad groups of them. We analyze trends for each municipality in our sample by running simple regressions of the number of monthly homicides in the municipality against a linear time trend. The first two columns count the number of regressions with statistically significant increasing or decreasing trends (p-value$<$0.05). Counts of either increasing or decreasing trends are similar between municipalities with and

[60] We start the period of analysis in 2001 because in that year the opposition won the presidency after over 70 years of hegemonic political rule by the PRI. Trends of outcomes before such a seismic change might not be informative. We end it in 2012 because in that year the PRI won back the presidency, an event that could have affected trends of outcomes.

without kingpin removals. The next two columns adjust p-values to control for false discovery rate.[61] Now fewer trends are significant, but again are no different in municipalities that will experience a kingpin removal. In municipalities with or without removals, before the War on Drugs homicides were neither increasing nor decreasing.

Table 2: Homicide Rate Trends
Municipality Level regressions
$h_t = \alpha + \beta t + \epsilon_t,\ t \in (1, 72)$

	p-value<0.05		q-value<0.05	
	$\hat{\beta} < 0$	$\hat{\beta} > 0$	$\hat{\beta} < 0$	$\hat{\beta} > 0$
With kingpin removals (N=71)	7	10	3	3
Without kingpin removals (N=916)	34	33	2	0

Benjamini et al. (2006) sharpened q-values as implemented by Anderson (2008). Adjustment for false discovery rate takes place within each group of municipalities. The sample of each regression consists of the number of homicides at each month in the presidential period preceding Calderón's presidency.

Economic activity outcomes

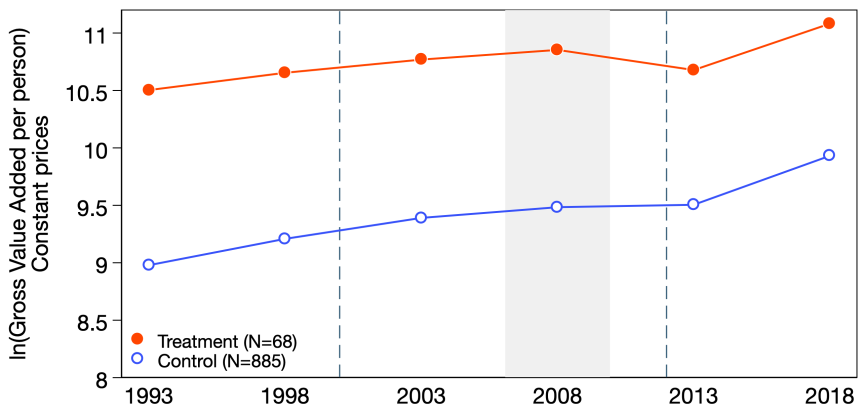
Figure 3 presents trends for GDP per capita and for firm survival. Dashed horizontal lines demarcate the period of analysis and the shaded area demarcates the period of kingpin removals. Before kingpin removals, trends for GDP per capita are parallel between groups, but the one with removals is substantially richer. In 2008, the first year with information shortly after the outset of the war on drugs, trends remained unchanged, but by 2013 they diverged. Between 2008 and 2013, GDP per capita in municipalities without removals increased by 12 percent. In municipalities with them, it decreased by 9 percent.

Before kingpin removals, trends of firm survival between groups also are parallel. In contrast with trends for GDP per capita, trends are parallel but without difference in levels. In 2003 the proportion of firms also present in the previous economic was similar between groups (47-48 percent). By 2008, the trends diverged. Fewer firms survived in 2008 in municipalities with kingpin removals (48 vs 53 percent). For both groups the proportion increased by 2013, but the gap between groups also increased (68 vs 77 percent).

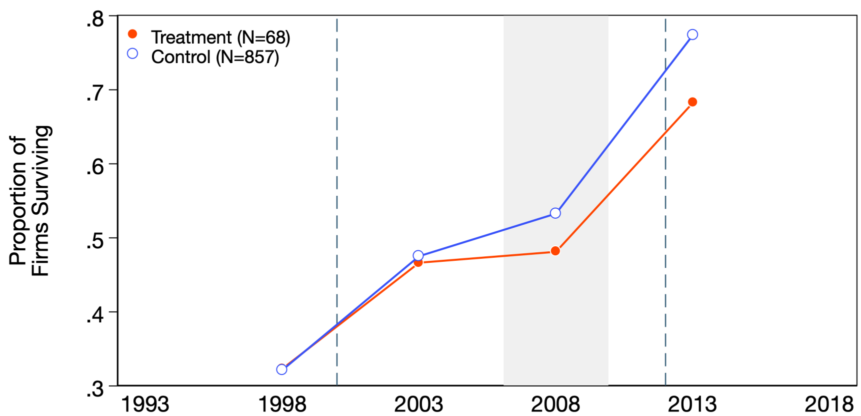
[61]We use Benjamini et al. (2006) sharpened q-values as implemented by Anderson (2008).

Figure 3: Economic Outcome Trends
GDP per capita and Firm Survival

A. GDP per capita



B. Firm Survival



The shaded area shaded area demarcates the period of kingpin removals (December 2006 to December 2010). Dashed lines demarcate the period of analysis (2001-2012). Weighted using municipality population in 2005.

5 Empirical Strategy

Our starting point is a standard two-way fixed effects equation with two additional sets of controls to account for additional exposure to shocks:

$$y_{mt} = \beta D_{mt} + \sum_{s} \gamma_s \tilde{L}^s_{mt} + \sum_{j} \gamma_j (X^j_m \times t) + u_m + w_t + \varepsilon_{mt} \quad (1)$$

where y_{mt} is the outcome in municipality m at a time t.[62] The sample of municipalities corresponds to the one detailed in the previous section (71 treated and 916 control municipalities). The dummy variable D_{mt} equals 1 only for municipalities with kingpin removals and only from the time of the first removal onwards, which varies from municipality to municipality.[63] Municipality fixed effects u_m capture any time-invariant differences between municipalities, including between municipalities with kingpin removals and without them. Common time fixed effects are denoted w_t. Standard errors are clustered at the municipality level. The parameter of interest, β, identifies the average treatment effect of kingpin removals under the assumption of homogeneity in treatment effects across units and time, and under the crucial assumption that trends in the absence of treatment are equal between treatment and control units, conditional on controls.

The first set of time-varying controls accounts for the differential impact the Great Recession—triggered by the US financial crisis and diffused to Mexico through strong trade links with the United States—could have in municipalities with kingpin removals. Shocks to employment not only have a direct effect on economic outcomes, they possibly have effects on crime, given earlier evidence by Dell et al. (2019) on the effects on homicides in Mexico of increased competition from Chinese exports.[64] The vector $\tilde{L}^j_{mt}$ consists of Bartik-type formal

[62] We express GDP per capita in logarithms and the remaining four outcomes in levels. Expressed in this way, outcomes show parallel trends (see figures provided as descriptive evidence). Many municipalities in the trimmed sample experienced no homicides, or extortions, or kidnappings. Non-normality of outcomes is less of a concern for inference given a large sample size. As a robustness check, we provide results from negative binomial regressions.

[63] For outcomes measured monthly the time of kingpin removal equals the month of removal whereas for those measured yearly it equals the year if the removal happened in June or earlier. For example, if the first removal happened in September 2008, the dummy variable equals 0 in 2008 and 1 in 2009. If it happened in April 2008, the dummy variable equals 1 in 2008.

[64] The appendix provides descriptive evidence of the relation of shocks to export-intensive manufacturing employment with homicides. Figure A.8 presents trends of homicides according to levels of employment in export-intensive employment. It shows parallel trends across groups and before the Great Recession. When a common shock has a differential impact across industries, using Bartik-type measures are the same as using local industry shares as instruments, and shares should be orthogonal to outcomes for the instrument to be valid (Goldsmith-Pinkham et al., 2020), which the trends in the figure suggest. Municipalities with kingpin removals and with high levels of export-intensive manufacturing employment overlap (51 of

employment in each municipality m at a time t expressed as proportion of the working-age population (15-64) and differentiated in export-intensive manufacturing, other manufacturing, and non-manufacturing. We differentiate Bartik-type formal employment because trade shocks affect not all manufacture employment but the employment exposed to trade shocks.

The second set of controls allows for deterministic (linear) time trends across key time-invariant characteristics that set municipalities with removals apart.[65] High population and strategic location distinguish the municipalities and correlate with how important a location is for trading legal and illegal goods. The vector X_m^j includes three linear trends. The first two correspond to a dummy variable for municipalities with a population in 2005 above the median of the sample and another for municipalities with a port, or a tolled highway, or a border crossing to the U.S.[66] The two linear trends aim at capturing the effect of shocks on the supply of cocaine in Colombia, which could have affected crime in locations in Mexico strategic for trading cocaine (Castillo et al., 2020). A third linear trend corresponds to a dummy variable for municipalities with a prosecuted crime rate above the median of the sample. The rate considers all crime prosecuted in a municipality and proxies the level of criminal activity before the War on Drugs.[67] The third linear trend control allays concerns of municipalities with high levels of criminal activity having a different trend of outcomes that confounds the effect of kingpin removals.

Estimation of equation 1 is possible whenever there is information on the outcome variable prior to kingpin removals, so that D_{mt} and u_m are identified separately. Of all the outcome variables, only extortion lacks municipality level information prior to kingpin removals. For extortions, we estimate the cross-sectional specification:

$$y_{mt} = \beta D_{mt} + \sum_s \gamma_s \tilde{L}^s_{mt} + \sum_j \gamma_j X^j_m + w_t + stfe_{st} + \varepsilon_{mt} \tag{2}$$

the 71 municipalities with kingpin removals in our sample also have high export-intensive manufacturing employment) and homicides after the Great Recession increase more in municipalities with high export-intensive manufacturing employment relative to the two other groups. Failing to account for shocks to employment will lead to upwardly biased estimates of the effect of kingpin removals.

[65] According to the descriptive evidence, municipalities with removals are intrinsically different, consequently having different levels of outcomes. When levels of outcomes differ, any difference-in-differences analysis should anticipate what the difference implies for trends (Kahn-Lang and Lang, 2020). In our setting, it implies additional exposure to shocks that could confound the effect of kingpin removals.

[66] The median corresponds to the specific sample tested. For example, the median population differs when using alternative population thresholds to trim the sample.

[67] The measure also proxies how effective is the government at prosecuting local crime. Of the three datasets for crime indicators, the dataset for prosecuted crime is the only one starting before the War on Drugs. The rate is per 100,000 people in the municipality. For each municipality, we estimate the mean of the monthly prosecuted crime rate from 2003 to 2006.

where state fixed effects replace municipality fixed effects, and differential trends in equation 1 are replaced by time-invariant, observable municipality-level control variables X_m^j.[68] Naturally, lacking time-invariant characteristics correlated with D_{mt} and with outcomes, the specification 2 could retrieve biased estimates of β. For the crimes where we can estimate both the two-way fixed effects and the OLS model, we obtain practically the same estimate of β. A set of observable municipality characteristics can proxy municipality fixed effects, raising confidence that OLS in our setting can be a credible identification strategy.

The high frequency of data for homicides allows us to provide empirical evidence against reverse causality (homicides leading to kingpin removals) and to validate our two-way fixed effects identification strategy. Our setting corresponds to a staggered adoption design (Athey and Imbens, 2018) because each municipality becomes permanently treated at different times. As we noted, when the date of treatment varies, a two-way fixed effects specification will retrieve unbiased estimates of the treatment effect on the treated only if treatment effects are homogeneous across time (Goodman-Bacon, 2021).

Using the high frequency of data for homicides, we recast the staggered adoption design into a difference-in-differences framework with multiple time periods and a single adoption date. Recasting the information requires two steps. First, we recast calendar time to time relative to kingpin removals. For municipalities with kingpin removals, we set as 0 the month of removal, and set the month before to be -1, the month after to be +1, and so on. Second, we impute time of removal for municipalities without them. We randomly assign the municipalities a removal date from the distribution of time of first removals (solid bars in figure A.3 in the appendix). Our imputation approach is valid under our maintained difference-in-differences assumption that the date of a specific kingpin removal is random, conditional on controls. We repeat the imputation process 1,000 times.[69]

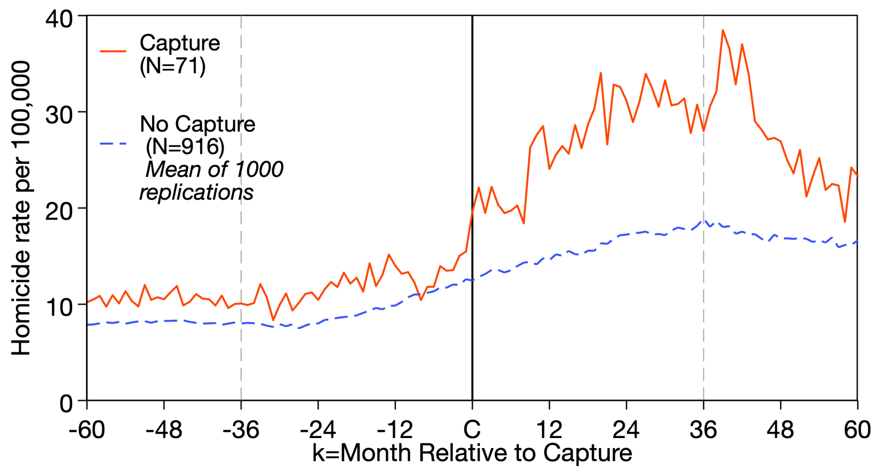
Figure 4 presents trends in homicides for the recast information, a single adoption date

[68] The vector includes six pre-determined variables. It accounts for high population and strategic location by including four dummy variables: the municipality has a population of million or more (all of them experienced kingpin removals), has a port, has a tolled-highway, and has a border crossing to the U.S. To account for high levels of criminal activity, it includes the mean prosecuted crime rate between 2004 and 2006. Finally, it also includes the mean prosecuted extortion rate (2004-06). If the variable correlates with the experienced yearly extortion rate, the mean-squared error of the regression should decrease.

[69] We obtain an equal number of synthetic datasets. Each contains information from the sample of municipalities with kingpin removals (the same information repeats across all datasets) and from the sample of municipalities without kingpin removals using one of the random draws from the distribution of time of first removals. We create the datasets using all observations, and not only those in the trimmed sample, to avoid repeating random draws each time we test the specifications in different samples.

and multiple time periods. The trend for the control group corresponds to the mean of the 1000 replications. Trends are parallel before kingpin removals, allaying concerns of reverse causality. After removals, trends diverge, suggesting large effects of removals on homicides. But because it accounts for no potential confounders, the effect likely is overstated.

Figure 4: Homicide Rate Trends
Time Relative to Kingpin Removal



We then estimate the regression model

$$h_{mk} = \beta D_{mk} + \sum_s \gamma_s \tilde{L}^s_{mk} + \sum_j \gamma_j (X^j_m \times k) + u_m + w_k + \varepsilon_{mk} \tag{3}$$

where k is time relative to the month of kingpin removal (set to zero in the month of removal, and with $k \in (-36, +36)$, h_{mk} is the homicide rate in municipality m at time k, D_{mk} is a dummy set to one in municipalities with removals when $k \leq 0$, and always to zero in non-removal municipalities. The remaining controls are the same as before. We run the regression model on each of our synthetic datasets. Point estimates presented in regression tables equal the mean of point estimates across regressions. The standard errors account for the additional uncertainty introduced by the imputation of removal dates for municipalities without them.[70] The coefficient of interest is β, the average effect of kingpin removals on homicides in the 36 months following a removal.

[70]We use Rubin's combining rules (Rubin, 1987). The standard error is the sum of within and between variance, $\overline{Var}(\hat{\beta}) = WI + [1 + (1/m)] \times BE$. Within variance equals $WI = (1/m) \sum_{D=1}^{m} \hat{Var}(\hat{\beta_D})$, where $\hat{Var}(\hat{\beta_D})$ corresponds to the variance of each parameter estimated. Between variance equals $BE = [1/(m - 1)] \sum_{D=1}^{m} (\hat{\beta}_D - \overline{\beta})^2$.

6 Results

6.1 Homicides

Difference-in-differences

Table 3 presents results of the effect of kingpin removals on homicides estimated using the recast information, a single adoption date and multiple time periods. Column 1 in the table only controls for two-way fixed effects. In this specification the estimate is 13 additional homicides per 100,000 people in the three years following a kingpin removal. Column 2 adds Bartik-type employment measures; columns 3 to 5 sequentially add the three differentials trends. Of the three differential trends, the only one statistically different from zero is the one for prosecuted crime, suggesting homicides were increasing before kingpin removals in municipalities with a higher pre-2006 crime rate. Column 5 is our preferred specification with the full trend control set. We find that kingpin removals increase the homicide rate by 9.2 per 100,000 people, or 55 percent relative to the mean of the municipalities without kingpin removals. The estimate in our preferred specification decreases by 3.8 points or 40 percent relative to the one in the model with fixed effects only. Patterns across the five models show that Bartik-type employment measures account for most (70 percent) of the decrease.

Of the three Bartik measures, only predicted employment in export manufacturing has a large and (marginally) significant negative coefficient, in line with our expectations. The effect of shocks to employment works through the export-intensive manufacturing sector only.[71]

The difference-in-differences with a single adoption date and multiple time periods allows us to assess parallel trends before kingpin removals and homogeneous effects over time. We use specification 3, expand the period of analysis to five years instead of three, and use leads and lags to disaggregate the effect by month. Figure 5 presents the results. Consistent with patterns in figure 4, point estimates and confidence intervals before kingpin removals suggest parallel trends. After kingpin removals, they suggest roughly homogeneous effects within 3 years of the removal, a time that corresponds to Calderón's presidency.

[71] By itself, the point estimate of -8.2 in our main specification suggests a large effect. Dell et al. (2019) find that a one standard deviation decline in manufacturing employment owing to Chinese competition increases a drug-related homicide rate by 5.4 per 100,000 people. We find that a one standard deviation decline in export manufacturing employment increases the homicide rate by 25 per 100,000 people, an effect five times higher. Two reasons likely explain this difference. First, we separate export-intensive manufacturing from other manufacturing, for which we find a null effect, explaining an attenuation of the coefficient for all manufacturing. Second, we use all the variation in local employment from national trends, whereas Dell et al. (2019) only uses variation from Chinese export competition.

Table 3: Difference-in-Differences
Dependent variable: Homicide Rate
±36 Months From Kingpin Removals

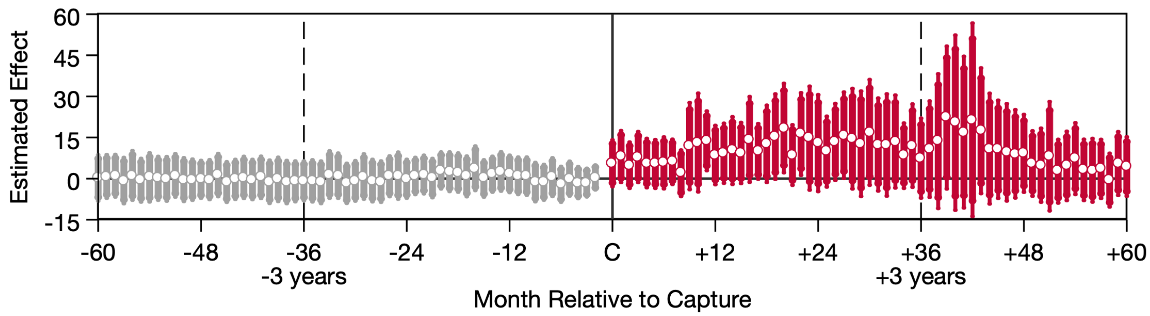
	(1)	(2)	(3)	(4)	(5)
$(Capture_{mk} = 1)$	13.01**	10.25***	9.84***	9.45***	9.21***
	(5.22)	(3.68)	(3.69)	(3.63)	(3.47)
Emp. Export Manufacturing$_{mk}$		-8.41*	-8.41*	-8.23*	-8.16*
		(4.96)	(4.95)	(4.94)	(4.89)
Emp. Other Manufacturing$_{mk}$		-0.46	-0.40	-0.28	-0.25
		(0.77)	(0.77)	(0.76)	(0.76)
Emp. Other Sectors$_{mk}$		0.54	0.52	0.40	0.50
		(1.06)	(1.06)	(1.06)	(1.04)
(Population > median)=1 $\times k$			0.06	0.01	0.00
			(0.04)	(0.04)	(0.04)
(Pros. Crime Rate > median)=1 $\times k$				0.13***	0.12**
				(0.05)	(0.05)
Strategic Location=1 $\times k$					0.04
					(0.07)
Observations	72,042	72,042	72,042	72,042	72,042
Municipalities (Clusters)	987	987	987	987	987
Municipality FE	Yes	Yes	Yes	Yes	Yes
Time to capture (k) FE	Yes	Yes	Yes	Yes	Yes
Dependent variable mean	16.73	16.73	16.73	16.73	16.73
$(\hat{\beta}/Dvm) * 100$	77.7	61.3	58.8	56.5	55

* $p < 0.10$, **$p < 0.05$, ***$p < 0.01$
Results from 1000 regressions summarized using Rubin's rules. Robust standard errors in parentheses. Regressions use municipality population (2005) as analytic weights. Employment per sector: Bartik-type formal employment divided by the population 15-64 years of age. Population: Population in 2005 above the median of the sample of each regression. Pros. crime rate: Mean prosecuted crime rate (2004-06) above the median of the sample of each regression. Strategic location: Has port, or tolled highway, or border crossing to the U.S.

Staggered adoption design: Two-way fixed effects

Table 4 presents results from running the two-way fixed effects specification in equation 1, where the 'removal' dummy turns on at the date the municipality experienced the removal. Consistent with figure 5 suggesting roughly homogeneous effects, the effect we find is the same. Kingpin removals increase the homicide rate by 9.3 per 100,000 people, an increase of 57 percent relative to the mean of the remaining municipalities. Results and patterns of controls are like the ones we describe for table 3 but with a relevant difference: point estimates for export-intensive manufacturing employment, albeit similar, are smaller and

Figure 5: Difference-in-Differences
Dependent variable: Homicide Rate
±36 Months From Kingpin Removals



Thick lines represent the 90 percent confidence interval and thin lines the 95 percent interval. Dots represent point estimates. Results from 1000 leads-and-lags regressions summarized using Rubin's rules.

imprecise.

6.2 Extortion and Kidnapping

We now turn to examining whether kingpin removals also affected extortion and kidnapping. With these outcomes, we now move from monthly, universal data with a long time series to annual, survey data with a time series that mostly starts after the onset of the War on Drugs. First we present results from cross-sectional OLS regressions. Then we compare the results to available two-way fixed effects estimates. Similar results suggest that in our context, cross-sectional OLS results for the extortion rate are valid.

Ordinary least squares

Table 5 presents OLS results for extortion and kidnapping from running the regression specified in equation 2. The main specifications show that kingpin removals increase the extortion rate by 43 percent and the kidnapping rate by 84 percent.[72] The table compares three variations of the regression. Columns 1 to 3 present results for the extortion rate. Column 1 only includes state and year fixed effects; column 2 adds Bartik-type employment measures; and column 3, the main specification, adds time-invariant controls. Columns 4 to 6 follow the same pattern for kidnapping. Column 3 shows that kingpin removals increase the

[72]Lacking municipality fixed effects, OLS might not retrieve consistent estimates for Bartik-type employment measures because local industry shares are assumed exogenous not to levels but to changes in the error term (Goldsmith-Pinkham et al., 2020).

Table 4: Staggered Adoption Design: Two-way Fixed Effects
Dependent variable: Homicide Rate
2004–2012

	(1)	(2)	(3)	(4)	(5)
$\text{Capture}_{mt} = 1$	12.79**	10.29***	9.95**	9.57**	9.25**
	(5.21)	(3.95)	(3.92)	(3.85)	(3.64)
Emp. Export $\text{Manufacturing}_{mt}$		-7.28	-7.28	-7.04	-6.93
		(4.93)	(4.92)	(4.91)	(4.86)
Emp. Other $\text{Manufacturing}_{mt}$		-0.19	-0.14	-0.05	0.01
		(0.49)	(0.48)	(0.48)	(0.48)
Emp. Other Sectors_{mt}		1.01	0.98	0.80	0.93
		(0.76)	(0.76)	(0.76)	(0.75)
(Population > sample median)=1 $\times t$			0.04	-0.01	-0.02
			(0.03)	(0.02)	(0.03)
(Pros. Crime Rate > sample median)=1 $\times t$				0.12***	0.11***
				(0.03)	(0.03)
Strategic Location=1 $\times t$					0.04
					(0.05)
Observations	106,524	106,524	106,524	106,524	106,524
Municipalities (Clusters)	987	987	987	987	987
Municipality FE	Yes	Yes	Yes	Yes	Yes
Time month-year (t) FE	Yes	Yes	Yes	Yes	Yes
Dependent variable mean	16.11	16.11	16.11	16.11	16.11
$(\hat{\beta}/Dvm) * 100$	64.9	63.9	61.8	59.4	57.4

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Robust standard errors in parentheses. Regressions use municipality population (2005) as analytic weights. Employment per sector: Bartik-type formal employment divided by the population 15-64 years of age. Population: Population in 2005 above the median of the sample of each regression. Pros. crime rate: Mean prosecuted crime rate (2004-06) above the median of the sample of each regression. Strategic location: Has port, or tolled highway, or border crossing to the U.S.

extortion rate by 3,288.7 per 100,000 adults, equivalent to an increase of 43 percent relative to control municipalities. Column 6 shows that they increase the kidnapping rate by 143.5 per 100,000 adults, equivalent to an increase of 84 percent.

Kidnapping: Two-way fixed effects

We obtain the same results for the kidnapping rate using either the OLS specification (equation 2) or and adapted version of our benchmark two-way fixed effects specification (equation 1).[73] We add to the sample kidnapping rates for 2004 estimated using the ENSI

[73] Applied to the kidnapping rate, the benchmark two-way fixed effects specification excludes differential trends because we have a single point in time before kingpin removals.

Table 5: Ordinary Least Squares
Dependent variables: Extortion and Kidnapping Rates
2010–2012

	Extortion			*Kidnapping*		
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Capture}_{my} = 1$	3350.0***	3211.2***	3288.7***	150.1**	154.4**	143.5**
	(1015.3)	(1061.7)	(1138.7)	(62.6)	(66.3)	(60.7)
Emp. Export $\text{Manufacturing}_{my}$		-458.7***	181.9		-1.5	7.3
		(113.4)	(223.9)		(8.1)	(12.8)
Emp. Other $\text{Manufacturing}_{my}$		-288.6*	-355.6**		4.2	1.5
		(163.7)	(150.0)		(9.1)	(10.7)
Emp. Other Sectors_{my}		68.2	47.4		-1.3	-2.2
		(59.7)	(78.5)		(2.4)	(4.1)
Pros. Ext. or Kid. (2004-06)			-322.5			-12.6
			(430.3)			(12.2)
Observations	438	438	438	438	438	438
Municipalities (Clusters)	146	146	146	146	146	146
Municipality FE						
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls						
- Prosecuted Crime (2004-06)			Yes			Yes
- Strategic Location dummies (3)			Yes			Yes
- Pop(2005)>1million=1			Yes			Yes
Dependent variable mean	7650.1	7650.1	7650.1	170.4	170.4	170.4
$(\hat{\beta}/Dvm) * 100$	43.8	42	43	88.1	90.6	84.2

* $p < 0.10$, **$p < 0.05$, ***$p < 0.01$
Robust standard errors in parentheses. Regressions use municipality population (2005) as analytic weights. Employment per sector: Bartik-type formal employment divided by the population 15-64 years of age. Population: Population in 2005 equal 1 million or above. Pros. crime rate: Mean prosecuted crime rate (2004-06) above the median of the sample of each regression. Strategic location dummies (3): Has port; has tolled highway; has border crossing to the U.S.

survey. Table 6 reports results from both specifications applied to the expanded sample. Columns 1 to 3 correspond to columns 4 to 6 in table 4, but use the expanded sample. Between tables, patterns in point estimates for removals are the same. The two-way fixed effects specification in column 4 in table 6 shows that kingpin removals increase the kidnapping rate by 143.6 per 100,000 adults, a point estimate statistically similar to OLS estimates in column 3 (150.1 per 100,000 adults) in the table and to those in column 6 in table 5 (143.5 per 100,000 adults).[74]

[74] Comparing columns 3 and 4 in table 6 suggests that OLS does not retrieve consistent estimates for the predicted employment measures, as expected (see Goldsmith-Pinkham et al., 2020). Two-way fixed effect results are too imprecise to inform about the relation of kidnapping with shocks to employment.

Table 6: OLS vs. Two-way Fixed Effects
Dependent variable: Kidnapping Rate
2004, 2010–2012

	OLS			*TWFE*
	(1)	(2)	(3)	(4)
$\text{Capture}_{my} = 1$	151.1***	154.0***	150.1***	143.6**
	(51.9)	(52.6)	(49.6)	(61.6)
Emp. Export $\text{Manufacturing}_{my}$		2.0	2.2	-7.4
		(6.2)	(11.1)	(61.5)
Emp. Other $\text{Manufacturing}_{my}$		0.0	-0.1	-12.3
		(6.3)	(7.4)	(24.0)
Emp. Other Sectors_{my}		-0.5	-3.1	-8.6
		(2.1)	(3.8)	(7.8)
Pros. Kidnapping. (2004-06)			-17.7	
			(11.1)	
Observations	584	584	584	584
Municipalities (Clusters)	146	146	146	146
Municipality FE				Yes
State FE	Yes	Yes	Yes	
Year FE	Yes	Yes	Yes	Yes
Controls				
- Pros. Crime (2004-06)			Yes	
- Strategic Location dummies (3)			Yes	
- Pop(2005)>1million=1			Yes	
Dependent variable mean	150.3	150.3	150.3	150.3
$(\hat{\beta}/Dvm) * 100$	100.5	102.4	99.8	95.5

* $p < 0.10$, **$p < 0.05$, ***$p < 0.01$
Robust standard errors in parentheses. Regressions use municipality population (2005) as analytic weights. Employment per sector: Bartik-type formal employment divided by the population 15-64 years of age. Population: Population in 2005 equal 1 million or above. Pros. crime rate: Mean prosecuted crime rate (2004-06) above the median of the sample of each regression. Strategic location dummies (3): Has port; has tolled highway; has border crossing to the U.S.

Table A.4 in the appendix provides results of repeating the exercise, but now for the homicide rate. To make it consistent with the other crime data, we transform the data from a monthly to a yearly frequency by taking means of monthly variables and by assigning the time of removal to a specific year, as described in the empirical strategy. Using the same controls, we obtain an OLS estimate of 9.3 per 100,000 people, again undistinguishable from difference-in-differences estimate of 9.2. Combined with the results for kidnapping, the results for the homicides suggest in our setting that municipality fixed effects can be proxied by a set of observable municipality characteristics. In the absence of extortion information preceding the War on Drugs, the OLS results we present for extortion can be credible.

6.3 GDP per capita and Firm Survival

Kingpin removals lower GDP per capita by 17 percent, a sizable effect considering how rarely GDP per capita decreases.[75] Table 7 presents results from running the benchmark two-way fixed effects specification (equation 1) on yearly data. Column 1 in the table excludes Bartik-type employment measures and differential trends; the point estimate is -0.24. Column 2 adds predicted employment measures whereas columns 3 to 5 add the three differentials trends sequentially. Column 5, the main specification, shows a point estimate of -0.19, equivalent to a reduction in GDP per capita of 17 percent. The bias in the naïve point estimate in column 1 mostly is explained by trends according to baseline characteristics, especially population and strategic location. The statistically significant point estimates suggest that GDP per capita in municipalities with a higher population and with a strategic location declined over time relative to other municipalities.

Kingpin removals lower firm survival by 4 percentage points, a decrease of 7 percent. Table 8 presents results from running the benchmark two-way fixed effects specification. Column 1 in the table excludes predicted employment measures and differential trends; the point estimate for kingpin removals is -0.09. Column 2 adds predicted employment measures whereas columns 3 to 5 add the three differentials trends sequentially. Column 5, the main specification, shows a point estimate for kingpin removals is -0.04, less than half of the one obtained in column 1 and equivalent to a decrease of 7 percent. Of the five outcomes, results for firm survival are the most susceptible to an upward bias. Bartik-type employment measures explain 60 percent and differential trends 40 percent. Estimates for differential trends suggest that firm survival was decreasing in municipalities with a higher prosecuted crime rate and with a higher population.

[75] The rule of thumb ($exp(\hat{\beta}) \approx 1 + \hat{\beta}$) used to interpret the coefficient $\hat{\beta}$ when the outcome is expressed in logarithms only applies to continuous regressors and better approximates the effect when the change in the regressor is small. With a dummy regressor, the effect of changing from 0 (no kingpin removal) to 1 equals $100 \times (exp(\hat{\beta}) - 1)$.

Table 7: Two-way Fixed Effects
Dependent variable: ln(Gross Value Added per capita)
1998, 2003, 2013

	(1)	(2)	(3)	(4)	(5)
$\text{Capture}_{my} = 1$	-0.24***	-0.24***	-0.21***	-0.21***	-0.19***
	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)
$\text{Emp. Export Manufacturing}_{my}$		0.03	0.03	0.03	0.03
		(0.02)	(0.02)	(0.02)	(0.02)
$\text{Emp. Other Manufacturing}_{my}$		0.02***	0.02***	0.02***	0.02***
		(0.01)	(0.01)	(0.01)	(0.01)
$\text{Emp. Other Sectors}_{my}$		-0.00	0.00	0.00	0.00
		(0.01)	(0.01)	(0.01)	(0.01)
(Population > sample median)=1 $\times y$			-0.07***	-0.06***	-0.05***
			(0.02)	(0.02)	(0.02)
(Pros. Crime Rate > sample median)=1 $\times y$				-0.03	-0.01
				(0.02)	(0.02)
Strategic Location=1 $\times y$					-0.03*
					(0.02)
Observations	2,859	2,859	2,859	2,859	2,859
Municipalities (Clusters)	953	953	953	953	953
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
$100 \times (exp(\hat{\beta}) - 1)$	-21.3	-21.4	-18.9	-18.6	-17.1

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

Robust standard errors in parentheses. Regressions use municipality population (2005) as analytic weights. Employment per sector: Bartik-type formal employment divided by the population 15-64 years of age. Population: Population in 2005 above the median of the sample of each regression. Pros. crime rate: Mean prosecuted crime rate (2004-06) above the median of the sample of each regression. Strategic location: Has port, or tolled highway, or border crossing to the U.S.

When the outcome is expressed in logarithms the effect of changing from 0 (no kingpin removal) to 1 equals $100 \times (exp(\hat{\beta}) - 1)$.

Table 8: Two-way Fixed Effects
Dependent variable: Proportion of Firms from Previous Census Surviving
1998, 2003, 2013

	(1)	(2)	(3)	(4)	(5)
$\text{Capture}_{my} = 1$	-0.09***	-0.06***	-0.04**	-0.04**	-0.04**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Emp. Export $\text{Manufacturing}_{my}$		-0.00	-0.00	-0.00	-0.00
		(0.00)	(0.00)	(0.00)	(0.00)
Emp. Other $\text{Manufacturing}_{my}$		-0.00	-0.00*	-0.00**	-0.00**
		(0.00)	(0.00)	(0.00)	(0.00)
Emp. Other Sectors_{my}		-0.01***	-0.01***	-0.01***	-0.01***
		(0.00)	(0.00)	(0.00)	(0.00)
(Population > sample median)=1 $\times y$			-0.04***	-0.03***	-0.02***
			(0.01)	(0.01)	(0.01)
(Pros. Crime Rate > sample median)=1 $\times y$				-0.04***	-0.04***
				(0.01)	(0.01)
Strategic Location=1 $\times y$					-0.00
					(0.01)
Observations	2,775	2,775	2,775	2,775	2,775
Municipalities (Clusters)	925	925	925	925	925
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Dependent variable mean	.51	.51	.51	.51	.51
$(\hat{\beta}/Dvm) * 100$	-17.2	-11.9	-8.4	-7.5	-7.1

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Robust standard errors in parentheses. Regressions use municipality population (2005) as analytic weights. Employment per sector: Bartik-type formal employment divided by the population 15-64 years of age. Population: Population in 2005 above the median of the sample of each regression. Pros. crime rate: Mean prosecuted crime rate (2004-06) above the median of the sample of each regression. Strategic location: Has port, or tolled highway, or border crossing to the U.S.

6.4 Adjustment for Multiple Hypothesis Testing

In table 9, we report adjustment for multiple hypothesis testing. Columns 1 and 2 present the point estimates and the p-values for each of the five outcomes. Trading off some type I errors for higher power, we first adjust for false discovery rate. Column 3 presents Benjamini et al. (2006) sharpened q-values.[76] We reject the null hypothesis of no effect on every outcome. Using the simple and conservative Šidák correction, we then adjust for family-wise error rate.[77] Column 4 presents the adjusted p-values. Once more we reject across the five outcomes the null hypothesis of no effect.

Table 9: Adjustment for Multiple Hypothesis Testing

	Estimate	p-value	FDR	FWER
Homicide rate (DiD)	9.21	0.0080	0.0110	0.0238
Extortion rate (OLS)	3288.71	0.0045	0.0100	0.0179
Kidnapping rate (TWFE)	143.51	0.0194	0.0140	0.0384
GDP per capita (TWFE)	-0.19	0.0008	0.0050	0.0040
Firm survival (TWFE)	-0.04	0.0454	0.0190	0.0454

FDR: False discovery rate, Sharpened q-values
FWER: Familywise error rate. Šidák correction

7 Robustness Checks

7.1 Main Robustness Checks

Weighting

All outcomes so far are weighted by population. Weighting is required for extortion and kidnapping, where outcomes are constructed from surveys which oversample both populous municipalities and households within populous municipalities. Weighting can correct the potentially endogenous sampling of the survey, leading to consistent estimates (Solon et al., 2015). For outcomes built from population data (such as homicides and economic outcomes) weighting should not matter if effects are homogeneous.[78] Figures in the appendix (figures

[76] We use the algorithm by Anderson (2008).

[77] Because we analyse a few number of outcomes, the penalty in the Šidák correction for assuming independence should be small. Outcomes should be correlated in the same direction within the family of violent crime outcomes and within the family of economic outcomes. We use the Šidák correction instead of methods that allow correlation within family of outcomes; for example, Westfall-Young or Romano and Wolf, because estimating them involves bootstrapping and estimating the difference-in-differences for homicide rates already involves a process similar to bootstrapping.

[78] If effects are heterogeneous across municipalities, weighted regressions estimate a different average effect of

A.6 and A.7) present unweighted trends for these outcomes, and suggest that weighting and unweighted point estimates should be similar.[79]

Results for homicides, GDP per capita, and firm survival are robust to weighting, but as expected weighting is required to identify the effect of extortion and kidnapping. Figure 6 compares point estimates and confidence intervals of the effect of kingpin removals from weighted and unweighted regressions. Results for homicides show similar point estimates but estimates from the weighted regression are more precise. For firm survival, point estimates and confidence intervals are practically identical. For GDP per capita, the point estimate from the unweighted regression—in which parallel trends are absent—is somewhat lower relative to its weighted counterpart, but still with overlapping confidence intervals. For extortion and kidnapping, weighted and unweighed regressions from household surveys contradict each other, with unweighted regressions showing no effect.

We interpret the results for extortion and kidnapping as being consistent with weighting correcting for endogenous sampling. Figure A.9 in the appendix compares results from weighted and unweighted regressions using administrative records on crimes from police records instead of victimization surveys. By covering all municipalities, police records have no endogenous sampling. But as a section 3 shows, they vastly understate experienced victimization. Weighted and unweighed estimates from police records for extortion agree with each other. For kidnapping, estimates also agree with each other, but weighted results are not statistically different from zero. Agreement of weighted and unweighted results from administrative records support our interpretation.

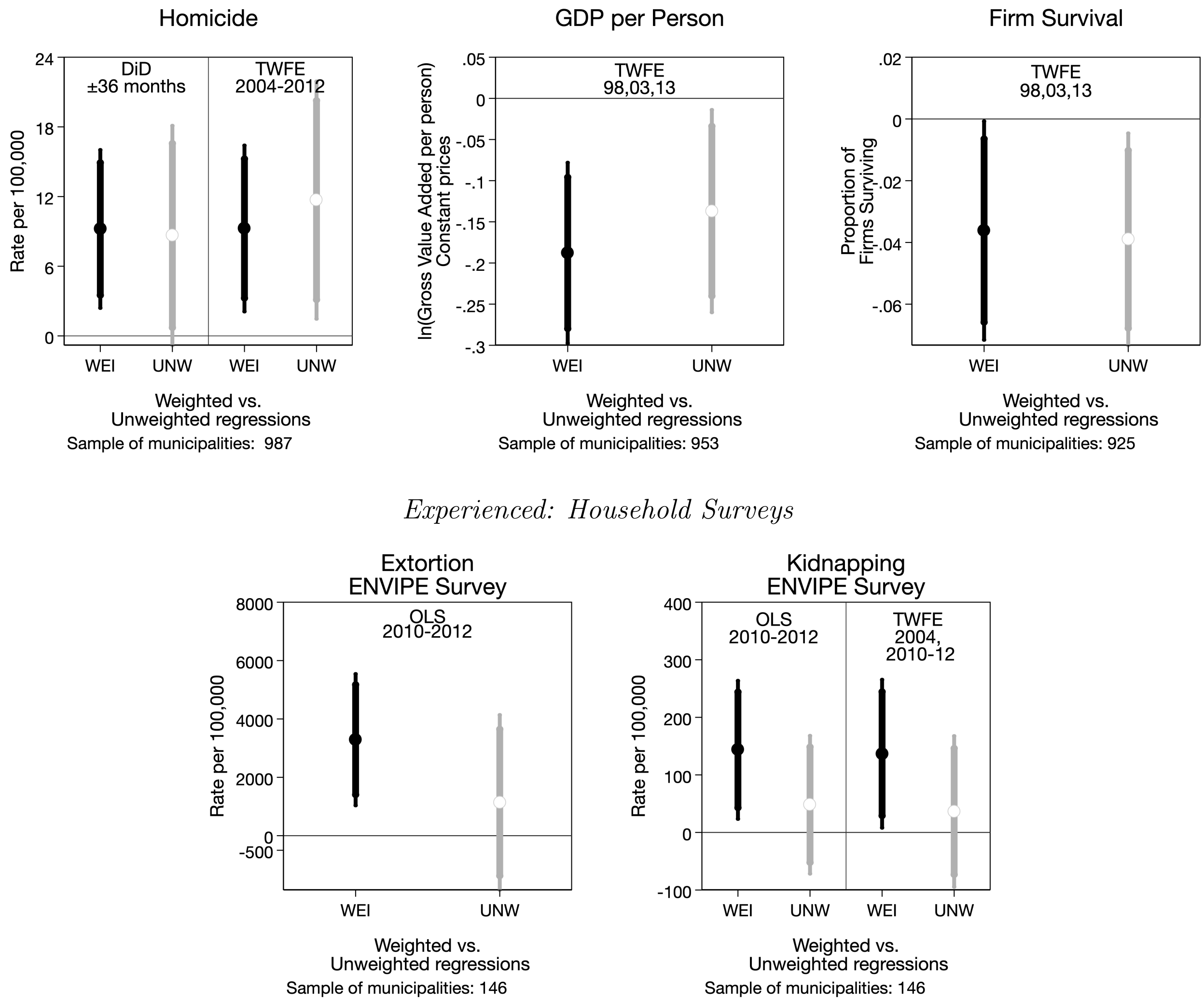
Outliers

Weighting, however, can give undue influence to outliers. Two observations with high population and with kingpin removals merit attention, Mexico City and Ciudad Juárez. Despite having been the scene of kingpin removals, Mexico City, the largest observation in terms of population, lacks relevance in drug traffic routes and as an export manufacturing

kingpin removals (the effect for the average individual) than unweighted regressions (the effect for the average municipality, which is a small place).

[79]The figures are the unweighted counterparts of figures 2 and 3 presented in section 4. Weighting should not matter for homicide rate (figure A.6) and for firm survival (figure A.7, panel b) because unweighted trends also suggest parallel trends, albeit with higher variance. In contrast with weighted trends, unweighted trends for GDP per capita (figure A.7, panel a) are not parallel before 2003—point estimates from weighted and unweighted regressions could differ.

Figure 6: Robustness to Weighting Scheme



Thick lines represent the 90 percent confidence interval and thin lines the 95 percent interval. Dots represent point estimates.

hub.[80] Mexico City attracted no attention from trafficking organizations and its homicide rate increased from 9 per 100,000 in 2006 population to 16 in October of 2010, an increase lower than the overall increase in the country in the same period. Ciudad Juárez, the sixth largest observation in terms of population, experienced fierce competition because it is a major border crossing point and the most important location for the cocaine trade. Relatedly, it is also the most important export manufacturing hub in Mexico. The homicide rate in Ciudad Juárez surged from 17 per 100,000 population in 2006 to 420 at its maximum in October 2010, a rate

[80]The population of Mexico city is five times the population of the second largest observation, Ecatepec de Morelos in Mexico state. Mexico City lacks relevance for drug trafficking, in particular for the traffic of cocaine. Smuggling cocaine from ports in the south-east and south-west to the U.S. requires no stops in Mexico City (see figure A.4 in the appendix).

close to one of warfare in non-state societies.[81] Our preferred estimations exclude Mexico City and keep Ciudad Juárez. Below we assess how robust results are to these and to other outliers.

Figure 7 compares point estimates and confidence intervals of the effect of the coefficient of interest for all five outcomes, for the baseline model and five alternative regressions. The first alternative sample includes Mexico City and the second instead excludes CiudadJuárez. The third excludes all municipalities with a population of 1 million or more, all of which experienced kingpin removals. The fourth keeps the baseline sample, but winsorizes all outcomes for the top 1 percent of observations, in each time period. For extortion and kidnapping, a fifth regression excludes municipalities from the states of Tamaulipas and Quintana Roo, both with a high non-response rates in the ENVIPE survey and a high proportion of population living in municipalities with kingpin removals.

Results are robust to outliers. Including Mexico City has no implications for the results. Excluding Ciudad Juárez only has implications for firm survival, the point estimate for kingpin removals decreases but remains statistically different from zero. Excluding all municipalities with a population of 1 million or more again only has implications for firm survival, its point estimate decreases by half and its confidence interval suggest no effect. We interpret the result as suggesting heterogeneous effects, with kingpin removals having a larger effect in large municipalities—and we note that Ciudad Juárez is among these large municipalities, besides being the municipality most affected by violence. Winsorizing leads to similar but more precise point estimates for all outcomes, as expected given the robustness to specific outliers we already showed. Finally, excluding Tamaulipas and Quintana Roo has no effects on extortion and kidnapping.

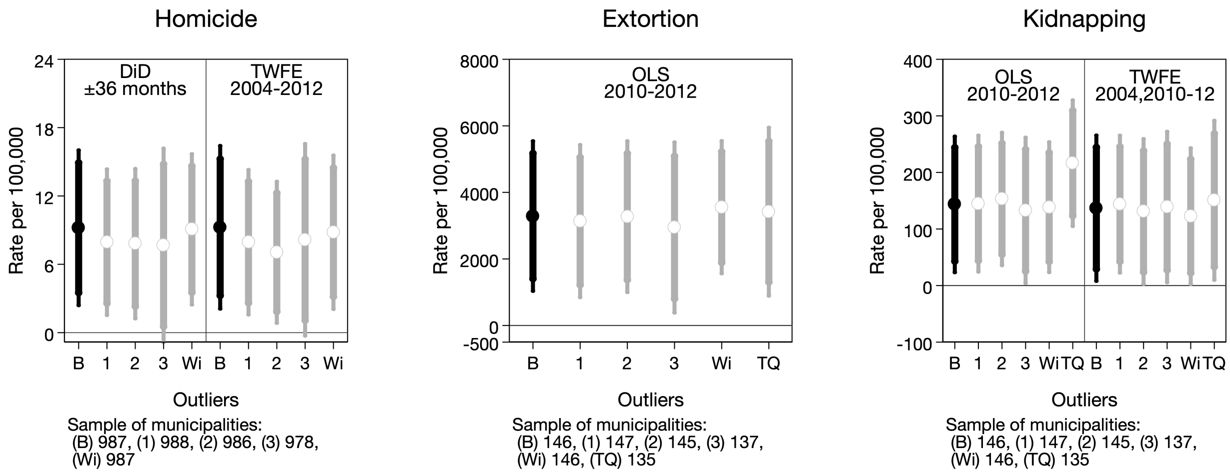
Alternative definitions of export-intensive manufacturing employment

Table 10 presents homicide rate results using alternative definitions of export-intensive manufacturing employment for constructing the Bartik controls for labor demand shocks. We use three different thresholds of the percent of total production produced by export manufacturing firms such that a sub-sector is classified as export-manufacturing intensive: the baseline case (34 percent, the mean across sectors); an expanded definition (50 percent); and a restricted definition (15 percent). Results show that increasing the threshold has a large effect in point estimates for Bartik-type employment measures and a small effect in point estimates for removals. Increasing the threshold from 15 to 50 percent, and in turn selecting

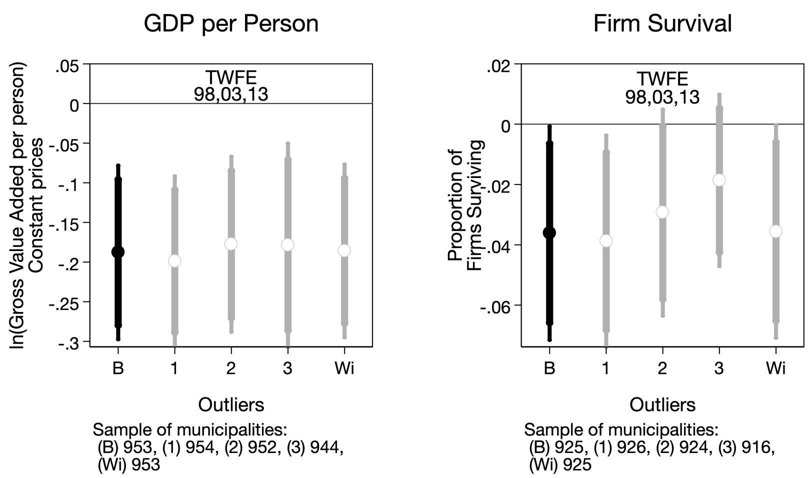
[81] Corresponding to 524 per 100,000 (Pinker, 2012).

Figure 7: Robustness to Outliers

A. Violent Crime



B. Economic Activity



Thick lines represent the 90 percent confidence interval and thin lines the 95 percent interval. Dots represent point estimates. (B): Benchmark regression, (1) Includes Mexico City, (2) Excludes Cd. Juárez, (3) Excludes municipalities with population 1 million or more, (W) Top 1% winsorized per time period, (U) Excludes states Tamaulipas and Quintana Roo for low response rates in the ENVIPE survey.

fewer sub-sectors to compose export-intensive manufacturing employment, has a small effect on the point estimate for removals, which decreases from 10.0 to 8.9, and large effect on the one for export-intensive manufacturing employment, which increases in magnitude from -5.7 to -10.0. Changing how we define the export-intensive manufacturing employment has no effect on the four remaining outcomes (see figure A.10 in the appendix).

Table 10: Difference-in-Differences
Dependent variable: Homicide Rate
±36 Months From Kingpin Removals
Robustness to Varying the Definition of the Export-intensive Manufacturing Sector

	Expanded		*Benchmark*		*Restricted*	
	(1)	(2)	(3)	(4)	(5)	(6)
$(Capture_m = 1) \times (Post_k = 1)$	11.02***	9.96***	10.25***	9.21***	9.82***	8.84***
	(3.95)	(3.70)	(3.68)	(3.47)	(3.57)	(3.39)
Emp. Export Manufacturing$_{mk}$	-5.85	-5.65	-8.41*	-8.16*	-10.26*	-10.00*
	(3.69)	(3.62)	(4.96)	(4.89)	(5.44)	(5.38)
Emp. Other Manufacturing$_{mk}$	-1.43	-1.14	-0.46	-0.25	-0.13	0.04
	(1.28)	(1.19)	(0.77)	(0.76)	(0.82)	(0.82)
Emp. Other Sectors$_{mk}$	0.52	0.48	0.54	0.50	0.41	0.35
	(1.09)	(1.08)	(1.06)	(1.04)	(0.99)	(0.97)
(Population > median)=1 $\times k$		-0.01		0.00		0.00
		(0.04)		(0.04)		(0.04)
(Pros. Crime Rate > median)=1 $\times k$		0.12***		0.12**		0.12**
		(0.05)		(0.05)		(0.05)
Strategic Location=1 $\times k$		0.05		0.04		0.04
		(0.08)		(0.07)		(0.07)
Observations	72,042	72,042	72,042	72,042	72,042	72,042
Municipalities (Clusters)	987	987	987	987	987	987
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Time to capture (k) FE	Yes	Yes	Yes	Yes	Yes	Yes
Dependent variable mean	16.73	16.73	16.73	16.73	16.73	16.73
$(\hat{\beta}/Dvm) * 100$	65.9	59.5	61.3	55	58.7	52.8

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

Results from 1000 regressions summarized using Rubin's rules. Robust standard errors in parentheses. Regressions use municipality population (2005) as analytic weights. Employment per sector: Bartik-type formal employment divided by the population 15-64 years of age. Population: Population in 2005 above the median of the sample of each regression. Pros. crime rate: Mean prosecuted crime rate (2004-06) above the median of the sample of each regression. Strategic location: Has port, or tolled highway, or border crossing to the U.S.

We consider all employment in a manufacturing sub-sector as export-intensive if the production of export manufacturing firms is higher than a given percent of total production. INEGI considers exported all production of export manufacturing firms.

Benchmark definition: The ratio of exports to production is higher than 34 percent, the overall ratio of the manufacture sector. Restricted definition: The ratio of exports to production is higher than 50 percent. Expanded definition: The ratio of exports to production is higher than 15 percent, the median of the twenty-one sub-sectors.

Excluding manufacturing production from GDP per capita

We use Bartik-type measures to channel the effect of the Great Recession in outcomes through labor demand shocks only. But the Great Recession affected manufacturing production directly.[82] As a robustness check, we estimate for each municipality the gross value added of manufactures and exclude it from total gross value added. If our identification strategy restricts the effect of the Great Recession through labor demand shocks only, point estimates should be similar when we exclude manufacture production.

Table 11 presents results excluding manufacturing production and contrasts them with the main results. The first pair of columns corresponds to the main specification, the second to excluding export manufacturing production, and the third to excluding all manufacturing production. The first column of each pair excludes employment measures and differential trends. While point estimates of kingpin removals from naïve two-way fixed effect regressions (first column of each pair) somewhat differ, point estimates of removals from our preferred specification are practically identical, suggesting that our identification strategy adequately channels the effect of the Great Recession in outcomes through labor demand shocks.

[82] In the second quarter of 2009, production in manufactures decreased by 14.5 percent. In the remaining sectors, the decrease was smaller: agriculture, -2.3 percent; mining, -4.4 percent; and services, -6.8 percent.

Table 11: Two-way Fixed Effects
Dependent variable: ln(Gross Value Added per capita)
1998, 2003, 2013
Robustness to Excluding Manufacturing Production

	Benchmark		*Exclude Gross Value Added of Manufactures*			
			Export		All Manuf.	
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Capture}_{my} = 1$	-0.24***	-0.19***	-0.26***	-0.20***	-0.28***	-0.19***
	(0.05)	(0.06)	(0.05)	(0.06)	(0.06)	(0.06)
Emp. Export $\text{Manunfacturing}_{my}$		0.03		0.01		0.01
		(0.02)		(0.02)		(0.02)
Emp. Other $\text{Manunfacturing}_{my}$		0.02***		-0.00		-0.01
		(0.01)		(0.01)		(0.01)
Emp. Other Sectors_{my}		0.00		-0.01		-0.01
		(0.01)		(0.01)		(0.01)
(Population $>$ s. median)=1 $\times y$		-0.05***		-0.03		-0.00
		(0.02)		(0.02)		(0.02)
(Crime Rate $>$ s. median)=1 $\times y$		-0.01		-0.04*		-0.07***
		(0.02)		(0.02)		(0.02)
Strategic Location=1 $\times y$		-0.03*		-0.02		-0.04*
		(0.02)		(0.02)		(0.02)
Observations	2,859	2,859	2,856	2,856	2,845	2,845
Municipalities (Clusters)	953	953	952	952	949	949
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
$100 \times (exp(\hat{\beta}) - 1)$	-21.3	-17.1	-23.1	-17.8	-24.1	-17.3

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

Robust standard errors in parentheses. Regressions use municipality population (2005) as analytic weights. Employment per sector: Bartik-type formal employment divided by the population 15-64 years of age. Population: Population in 2005 above the median of the sample of each regression. Pros. crime rate: Mean prosecuted crime rate (2004-06) above the median of the sample of each regression. Strategic location: Has port, or tolled highway, or border crossing to the U.S.

When the outcome is expressed in logarithms the effect of changing from 0 (no kingpin removal) to 1 equals $100 \times (exp(\hat{\beta}) - 1)$.

7.2 Other Robustness Checks

Results are robust to other robustness checks. The appendix provides figures we now discuss.

Varying the municipality population used to create rates or per person variables has no effects on results (figure A.11). Using population projections or actual population leads to the same point estimates while using population from 2005 across all time periods leads to a small increase in point estimates.[83] According to confidence intervals, the small increase is not statistically significant.[84]

The sample of municipalities excludes those below the median population, discarding only 2 observations with kingpin removals and 1219 observations without them. Figure A.12 presents results varying the threshold to the 25 percentile, the 75 percentile, and by not having a threshold. Results show no effect on point estimates for homicides, extortion, and kidnapping, and a small effect on point estimates for GDP per capita and firm survival.

We find evidence of spillover effects. Figure A.13 presents results from our preferred sample and compares them with regressions including municipalities without kingpin removals also neighboring municipalities with them. With the exception of homicides, point estimates for all outcomes decrease, suggesting that kingpin removals in a municipality impinge upon its neighbors.

Given the direct impact of the Great Recession on economic activity, our preferred specification excludes information for 2008. Figure A.14 compares results from regressions according to two groups, each with samples corresponding to three time periods. The first group consists of: (1) 1998, 2003, 2013; (2) 1993, 1998, 2003, and 2013; and (3) 1993, 1998, 2003, 2013, and 2018. The second group includes 2008 on each time period. The figure shows that including 2008, an atypical year owing to the Great Recession, lowers point estimates but they remain statistically different from zero. Expanding the sample, not possible for firm survival, has no effect in the results for GDP per capita. Results for GDP per capita are robust to allowing for more precisely estimated differential trends. Consistent with patterns

[83]The Mexican population increased 12 percent between 2005 and 2012, assuming the increase to be zero leads to the small increase.

[84]The preferred specification for firm survival uses as denominator across all time periods the number of firms in 2003 because removals both impinge upon the number of firms surviving and the total number of firms. The figure also presents results from regressions using the total number of firms each year as denominator. Consistent with kingpin removals also affecting the denominator by leading to a smaller number of total firms, point estimates decrease and are no longer statistically significant.

in figure 3, the effect in GDP per capita, at least until 2018, persists.

Finally, varying the minimum number of interviews per municipality that we require to build extortion and kidnapping rates from survey data from 30 to 15, to 45, or to 1 (Figure A.15) has no effect in point estimates for kidnapping and a minor effect on those for extortion, which increase when the minimum number of interviews is 45. Using the most precise matching algorithm has no effect in the results for firm survival (Figure A.5). Point estimate precision in negative binomial regressions and OLS for homicides, extortion, and kidnapping is similar (Table A.6), suggesting that the non-normal distribution of outcomes (figure A.16) has no implications for inference.

8 Concluding Remarks

We provide evidence on the reach of organized crime beyond illegal markets, and its associated economic cost. Exploiting the way the Mexican War on Drugs was fought as a series of plausibly random local events, we develop a difference-in-difference research design that compares outcomes in event municipalities—those that experienced a capture or killing of a high-ranking member of a criminal organization—to a set of control municipalities. We find that homicides, kidnapping, and extortion increased by 40-95 percent after a kingpin removal, compared to control municipalities. In turn, GDP per capita fell by 17 percent in event municipalities, a fall that persisted a decade after the initial events occurred. One potential mechanism behind the aggregate economic outcomes is firm survival: owing to a removal, firms are 7 percent more likely to dissolve.

The results suggest that organized criminal activity is a first-order economic issue, besides its security and welfare concerns that are important in themselves. Criminal organizations are deeply embedded in society not only in Mexico, but in many countries, especially those along the value chain of illegal drugs. While it would be very challenging to quantify the effect at the level of the entire economy, our estimates for affected regions suggest that the unravelling of organized crime can affect local economies to a degree that otherwise only war or pandemics do. Given that some of the national trend behind increasing violence in control municipalities might be a spillover from kingpin removals, we might underestimate the total effect of these events.

More tentatively, the results also draw attention to the importance of securing property rights vis-à-vis not only the state and its institutions but also the private actors who encroach on them. One important linkage between crime and economic outcomes seems to be economic coercion that is targeted towards the civilian population, including kidnappings for ransom, and extortion of individuals and businesses. These crimes are acts of expropriation executed with the threat and use of brutal violence. More research is needed to investigate more deeply how such acts alter economic incentives for investment and economic participation.

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A.1 Appendix

A.1.1 Tepic Case Study

Figure A.1: Local Newspapers Reported Surges in Violence and Extortion

A. *"Narcos cause terror in Tepic." May 27, 2010*

B. *"Extortion, crime on fashion in Tepic." June 24, 2010*

C. *Threating "Narco-message" from the local incumbent to a rival faction from the same organization. May 27, 2010*

Figure A.2: Local Bookstore Thanks the Public for Fifty Years of Patronage and Announces That It Will Close August 8, 2011

A.1.2 Kingpin Removals: When and Where They Happened

Figure A.3: When: Time of Removal

December, 2006 – December, 2010

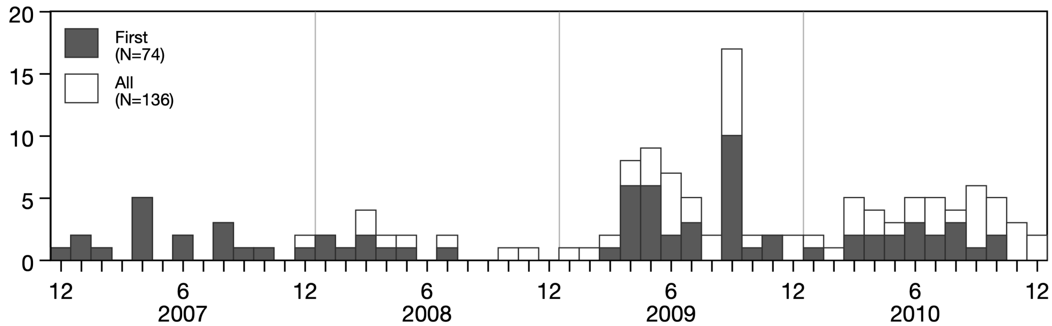
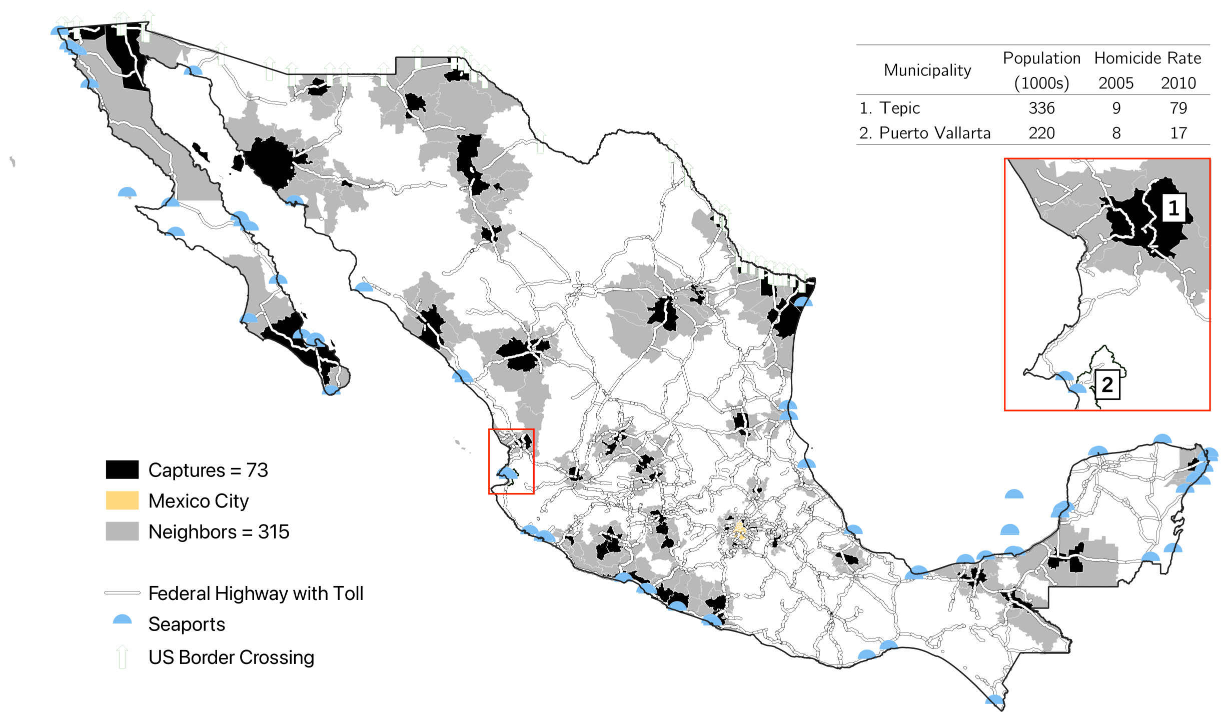


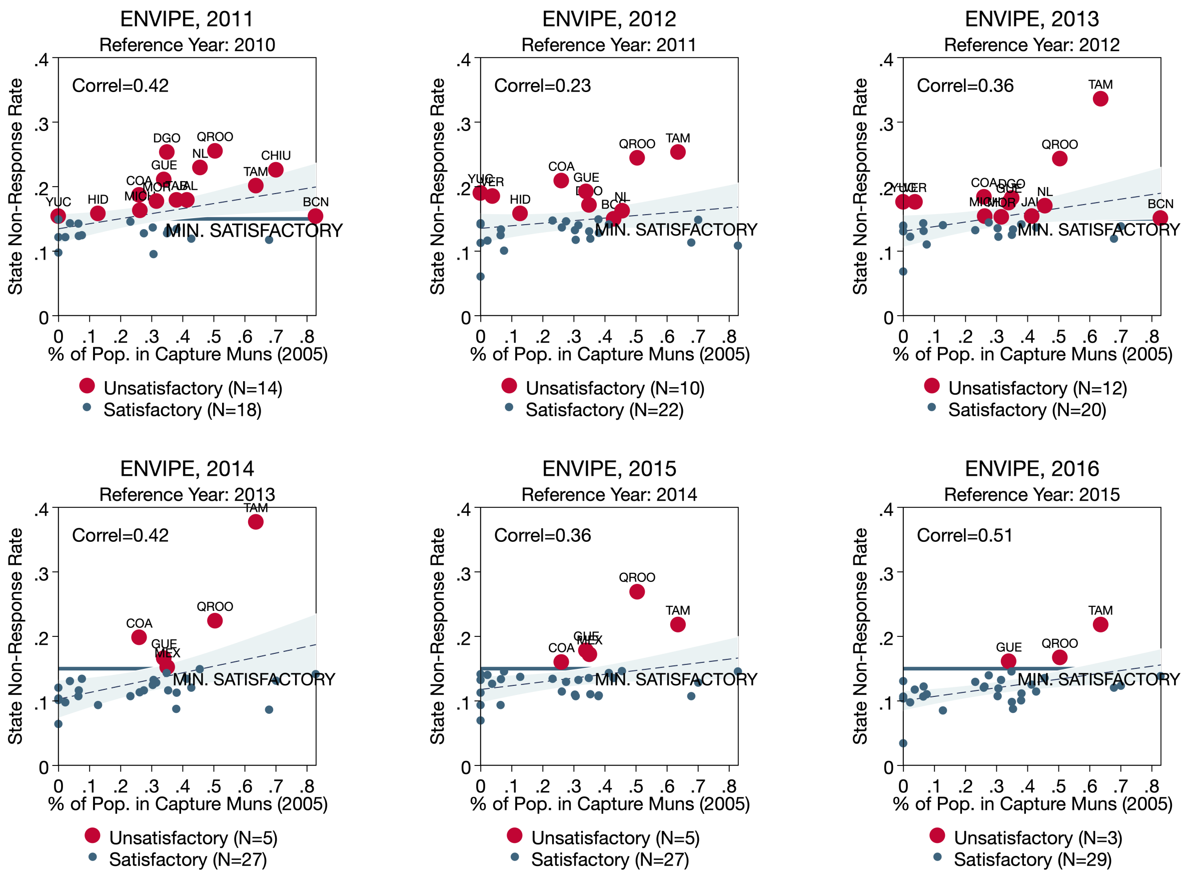
Figure A.4: Where: Location of Municipalities with Removals and of their Neighbors



A.1.3 Additional Tables and Figures

A.1.3.1 Descriptive

Figure A.5: Non-Response Rate and Percentage of Population in Municipalities with Removals
ENVIPE Survey. Rounds 2011-2016



The National Statistics Institute deemed a non-response rate of 15 percent to be the maximum threshold for a satisfactory result in a state.

Table A.1: Export Manufacturing Industries

	Production of export manufacturing firms / Total Production (%)					Export Manufacturing			Atkin (2016)
	2003	2004	2005	2006	2003-06	Bench.	Rest.	Exp.	
Manufacture sector	34.5	34.0	33.8	34.0	34.1				
Sub-sectors (n=21)									
311 - Food	3.9	3.5	3.4	3.5	3.6				
312 - Beverages and tobacco	6.4	5.1	4.9	5.6	5.5				
313 - Textiles inputs	23.7	21.7	22.1	20.3	21.9			Yes	
314 - Textiles except apparel	10.3	10.5	11.5	11.7	11.0				
315 - Apparel	36.8	34.9	32.7	29.7	33.5			Yes	Yes
316 - Leather	9.6	9.5	8.9	9.3	9.3				Yes
321 - Wood	10.1	11.4	12.1	13.0	11.7				
322 - Paper	9.2	9.1	10.8	10.9	10.0				
323 - Printing and related	5.7	6.5	6.7	7.0	6.5				
324 - Products from oil and coal	13.4	11.4	10.3	8.8	11.0				
325 - Chemical	9.5	9.8	9.8	10.8	10.0				
326 - Plastic	23.7	24.2	24.6	26.9	24.8			Yes	
327 - Items from non-metallic minerals	15.4	15.6	15.7	14.5	15.3			Yes	
331 - Basic metal industries	32.1	32.0	34.4	39.8	34.6	Yes		Yes	Yes
332 - Metallic products	37.5	34.8	33.1	33.1	34.6	Yes		Yes	Yes
333 - Machinery and equipment	39.0	42.0	42.0	43.7	41.7	Yes		Yes	Yes
334 - Electronics	79.3	83.6	85.3	81.1	82.3	Yes	Yes	Yes	Yes
335 - Electrical equipment	57.6	57.6	56.6	56.2	57.0	Yes	Yes	Yes	Yes
336 - Transportation equipment	52.7	52.8	52.5	52.0	52.5	Yes	Yes	Yes	Yes
337 - Furniture	9.2	9.7	9.8	9.2	9.4				Yes
339 - Other manufacturing	60.0	62.9	62.5	62.3	61.9	Yes	Yes	Yes	Yes
Median	15.4	15.6	15.7	14.5	15.3	7	4	11	10

Source: Own estimations using information from: *Valor agregado de exportación de la manufactura global.* https://www.inegi.org.mx/temas/pibval/

Bench.: Benchmark definition. The ratio of exports to production is higher than 34 percent, the overall ratio of the manufacture sector.

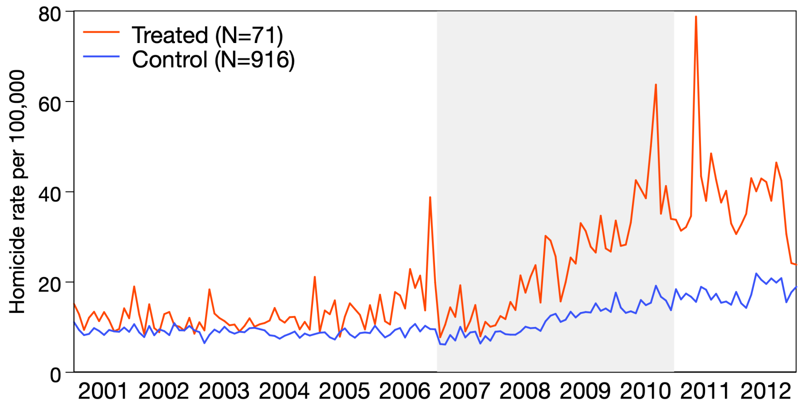
Rest.: Restricted definition. The ratio of exports to production is higher than 50 percent.

Exp.: Expanded definition: The ratio of exports to production is higher than 15 percent, the median of the twenty-one sub-sectors.

Atkin (2016) considers sub-sectors as export manufacturing if more than 50 percent of output was exported for at least half the years between 1985 and 2000. He uses a different information source for the value of exports and production.

Figure A.6: Homicide Rate Trends
Unweighted

A. Calendar Time



B.2 Time Relative to Kingpin Removal

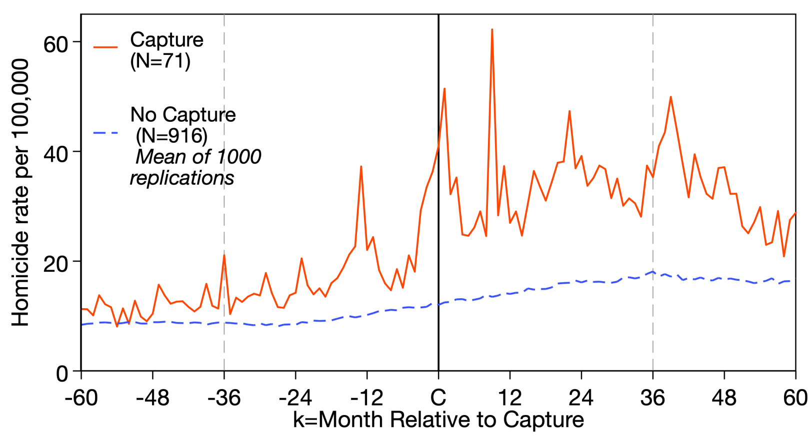
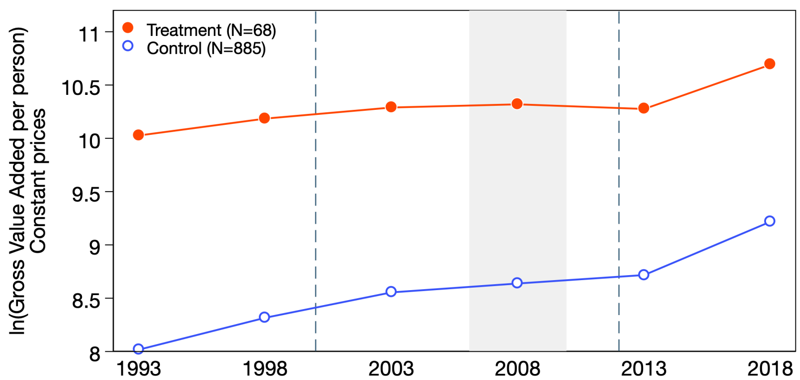
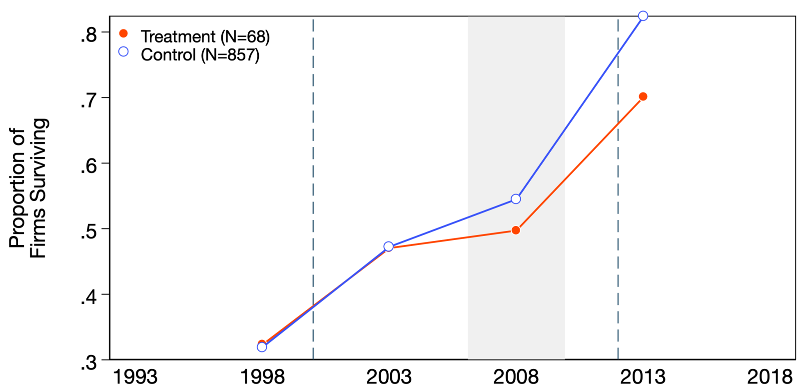


Figure A.7: Economic Outcome Trends
GDP per capita and Firm Survival
Unweighted

A. GDP per capita

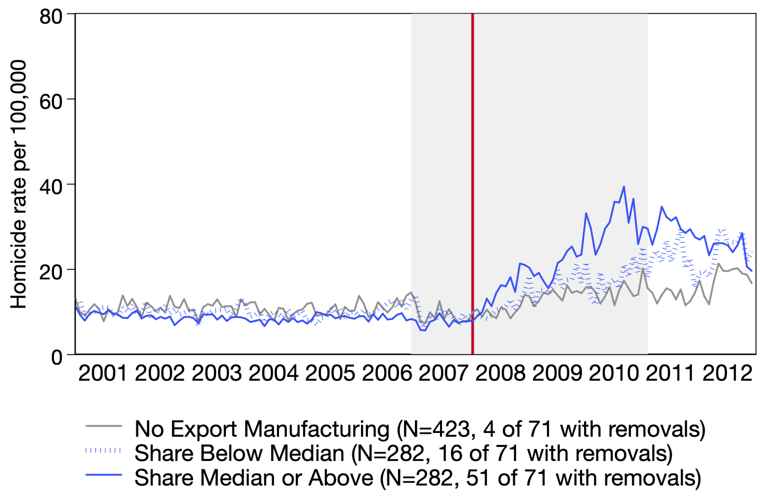


B. Firm survival



The shaded area shaded area demarcates the period of kingpin removals (December 2006 to December 2010). Dashed lines demarcate the period of analysis (2001-2012). Weighted using municipality population in 2005.

Figure A.8: Homicide Rate Trends According to Employment in Export-intensive Manufactures



The red line denotes the start of the Great Recession. The shaded area demarcates the period of kingpin removals. Municipalities are divided in three groups according to export-intensive manufacture employment: (1) no employment, (2) has employment and the share is below the median of the sample of the municipalities with employment, (3) the share is above the median.

A.1.3.2 Relation of Municipality Population with Kingpin Removals

Table A.2: Kingpin Removals According to Municipality Population

Population (2005)	All		Captures	
	#	%	#	%
<11,746	1219	50.0	2	2.7
11,746 - 57,000	917	37.6	15	20.3
57,000 - 1 million	293	12.0	47	63.5
1 million - 8 million	9	0.4	9	12.2
Mexico City (8.7m)	1	0.0	1	1.4
Total	2439	100.0	74	100.0

Table A.3: Linear Probability Model
Dependent variable: Removal=1 (N=74), Capture=0 (N=2,365)

	(1)	(2)	(3)	(4)	(5)	(6)
Population (2005)	0.868^{***}	0.796^{***}	0.318^{**}	1.117^{***}	1.021^{***}	0.888^{***}
	(0.116)	(0.063)	(0.147)	(0.232)	(0.157)	(0.071)
Population (2005) ^{2}				-0.450	-0.228^{**}	-0.089^{***}
				(0.421)	(0.105)	(0.008)
R^2	0.20	0.29	0.15	0.20	0.30	0.30
Municipalities	2,429	2,438	2,439	2,429	2,438	2,439
Sample						
- Population < 1 million	Yes			Yes		
- Population < 8 million		Yes			Yes	
- All Municipalities			Yes			Yes

Municipality population in millions.

A.1.3.3 OLS Model Can Retrieve the Difference-in-Differences Result for Homicides

Table A.4: Estimate for Kingpin Removals
is the Same in OLS and in Two-way Fixed Effects
Dependent variable: Homicide Rate
2004–2012

	OLS			*TWFE*
	(1)	(2)	(3)	(4)
$\text{Capture}_{my} = 1$	10.6^{**}	10.9^{**}	9.3^{**}	9.2^{***}
	(4.2)	(4.5)	(4.0)	(3.5)
Emp. Export $\text{Manufacturing}_{my}$		0.3	-0.7	-8.7
		(0.2)	(0.5)	(6.4)
Emp. Other $\text{Manufacturing}_{my}$		-0.2^{*}	-0.1	-0.2
		(0.1)	(0.1)	(0.6)
Emp. Other Sectors_{my}		-0.0	-0.1	1.5
		(0.1)	(0.1)	(1.1)
Observations	8,877	8,877	8,877	8,877
Municipalities (Clusters)	987	987	987	987
Municipality FE				Yes
State FE	Yes	Yes	Yes	
Year FE	Yes	Yes	Yes	Yes
Controls				
- Pros. Crime (2004-06)			Yes	
- Strategic Location dummies (3)			Yes	
- Pop(2005)>1million=1			Yes	
Dep. variable mean	16.1	16.1	16.1	16.1
$(\hat{\beta}/Dvm) * 100$	65.9	67.6	58.0	56.9

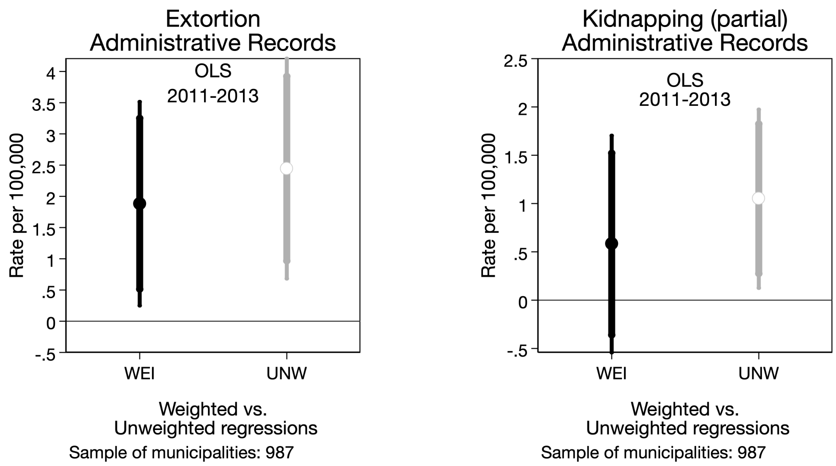
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Robust standard errors in parentheses. Regressions use municipality population (2005) as analytic weights. Employment per sector: Bartik-type formal employment divided by the population 15-64 years of age. Population: Population in 2005 equal 1 million or above. Pros. crime rate: Mean prosecuted crime rate (2004-06) above the median of the sample of each regression. Strategic location dummies (3): Has port; has tolled highway; has border crossing to the U.S.

A.1.4 Other Robustness Checks

A.1.4.1 Extortion and Kidnapping from Administrative Records

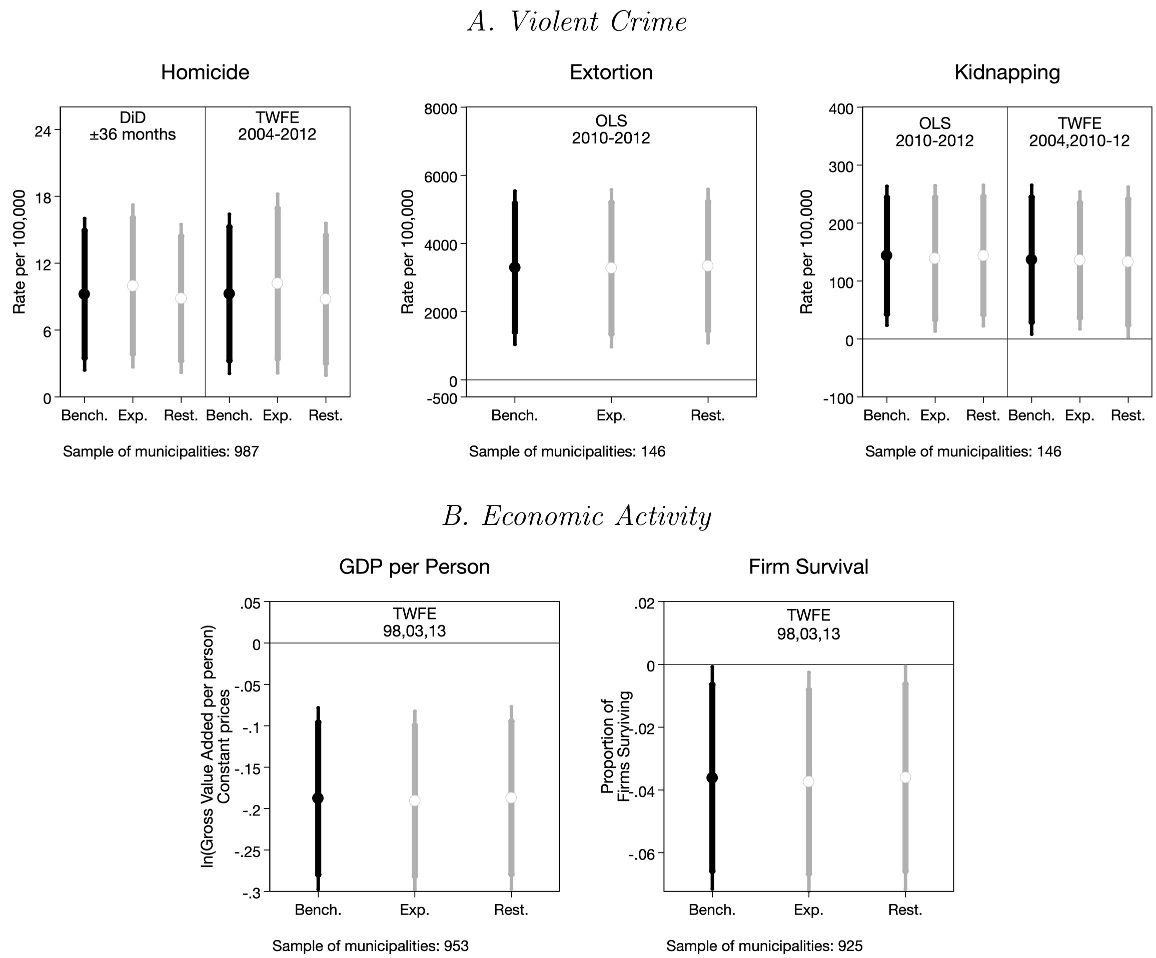
Figure A.9: Robustness to Weighting Scheme
Administrative Records of Extortion and Kidnappings Reported to the Police



Thick lines represent the 90 percent confidence interval and thin lines the 95 percent interval. Dots represent point estimates. Regressions using administrative records use the period 2011-2013 instead of 2010-2012 because they start in 2011.

A.1.4.2 Robustness to Varying the Definition of the Export-intensive Manufacturing Sector

Figure A.10: Robustness to Varying the Definition of the Export-intensive Manufacturing Sector

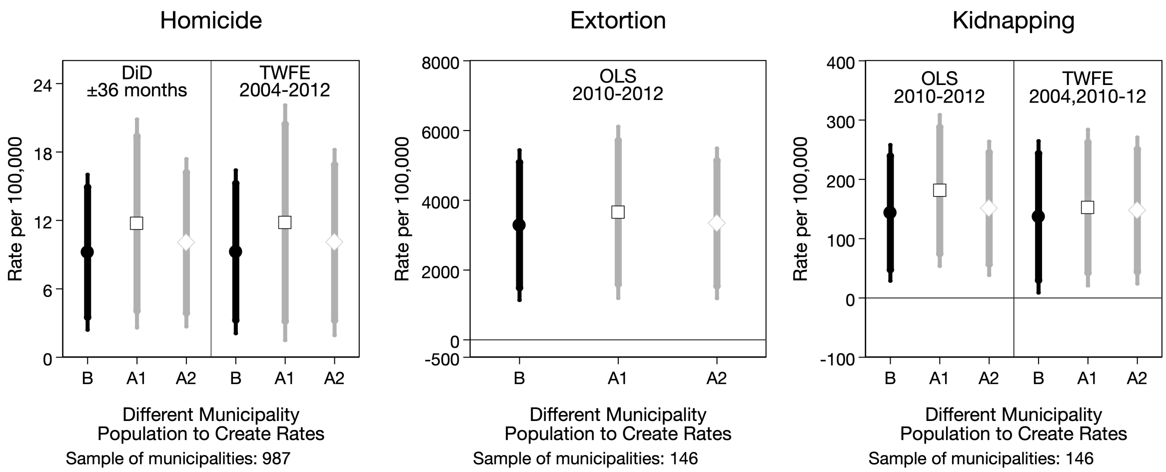


Thick lines represent the 90 percent confidence interval and thin lines the 95 percent interval. Dots represent point estimates. We consider all employment in a manufacturing sub-sector as export-intensive if the production of export manufacturing firms is higher than a given percent of total production. INEGI considers exported all production of export manufacturing firms. Benchmark definition: The ratio of exports to production is higher than 34 percent, the overall ratio of the manufacture sector. Restricted definition: The ratio of exports to production is higher than 50 percent. Expanded definition: The ratio of exports to production is higher than 15 percent, the median of the twenty-one sub-sectors.

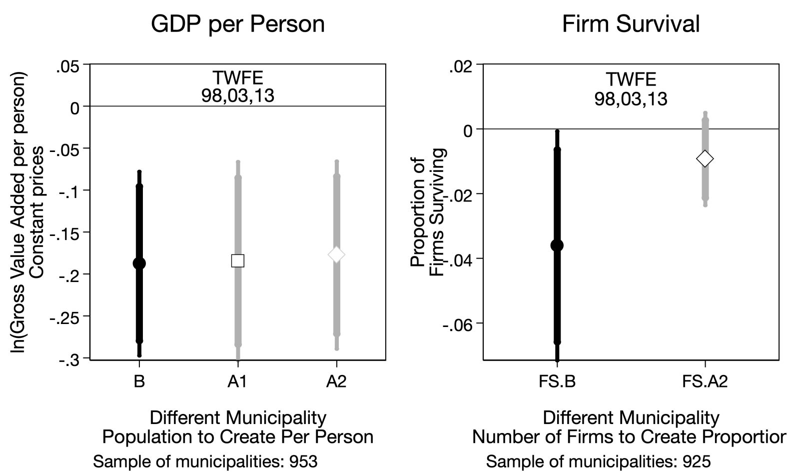
A.1.4.3 Population Used to Create Rates or per person Variables

Figure A.11: Variable Creation Uses Different Municipality Populations as Denominators

A. Violent Crime



B. Economic Activity

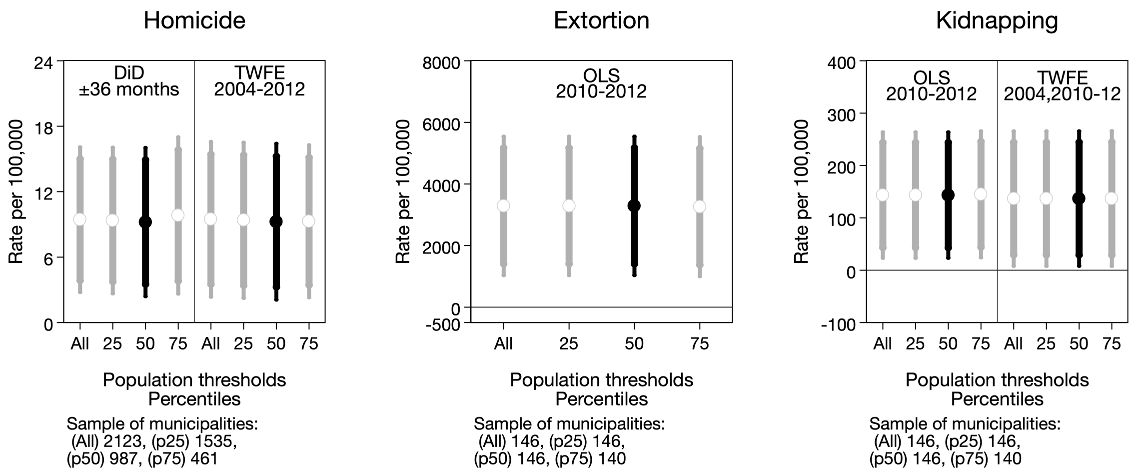


Thick lines represent the 90 percent confidence interval and thin lines the 95 percent interval. Dots represent point estimates. Municipality populations used as denominators to construct rates: B. Census population 1990, 1995, 2000, and 2005; population projections 2010-2020. A1. Census population 2005. A2. Census population 1990-2005; 2010-2020. Linear trends per municipality fill years with missing information (e.g. 1991-1994, 2006-2009). FS.B Number of firms in 2003 (e.g firms surviving in 2008 as proportion of firms in 2003, firms surviving in 2013 as proportion of firms in 2003). FS.A2. Number of firms in the previous census (e.g firms surviving in 2008 as proportion of firms in 2003, firms surviving in 2013 as proportion of firms in 2008).

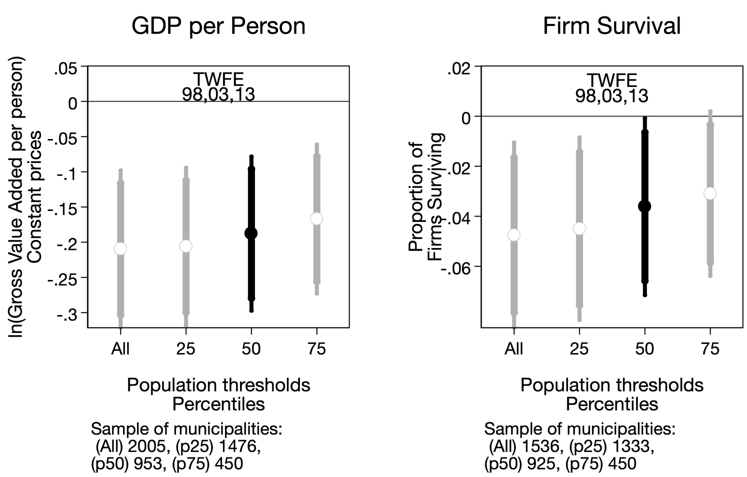
A.1.4.4 Sample of Municipalities According to Population

Figure A.12: Robustness to Varying Sample of Municipalities
Sample According to Population (Census 2005) Percentiles Thresholds

A. Violent Crime



B. Economic Activity

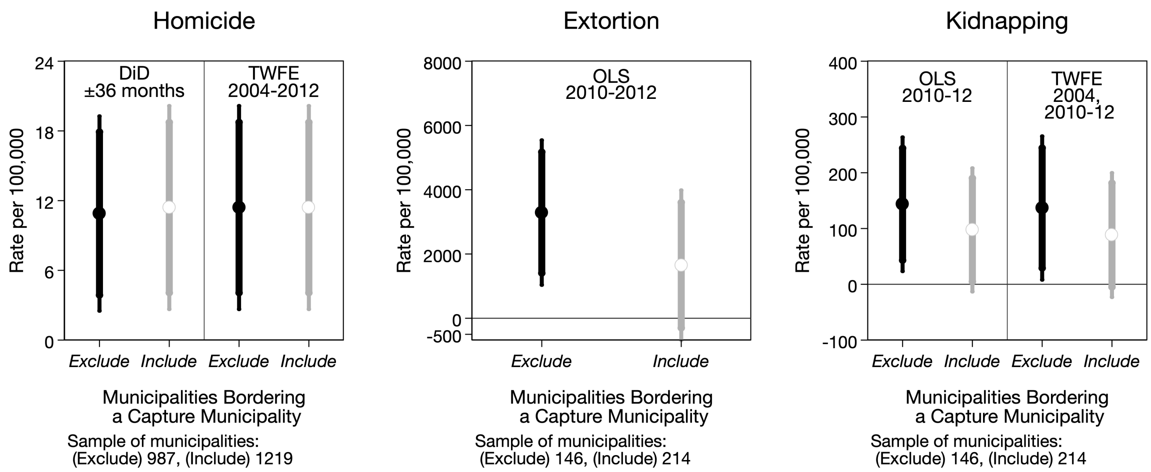


Thick lines represent the 90 percent confidence interval and thin lines the 95 percent interval. Dots represent point estimates. Population thresholds: All(≥ 1), 25 percentile ($\geq 3,936$), Median ($\geq 11,746$), 75 percentile ($\geq 28,760$).

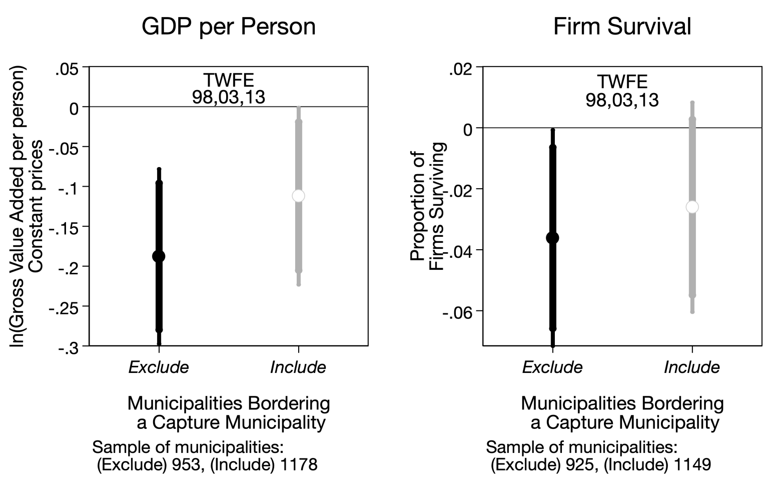
A.1.4.5 Spillovers

Figure A.13: Evidence of Spillover Effects
Sample Includes or Excludes Municipalities
Bordering Municipalities with Removals

A. Violent Crime



B. Economic Activity

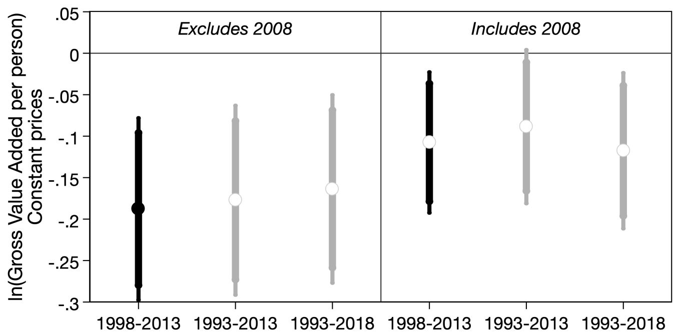


Thick lines represent the 90 percent confidence interval and thin lines the 95 percent interval. Dots represent point estimates.

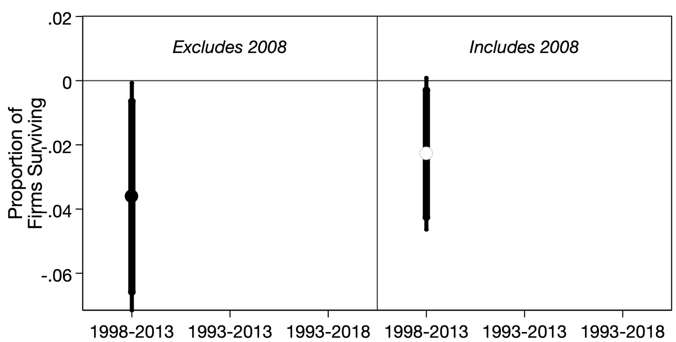
A.1.4.6 Including the Year Most Affected by the Great Recession in the Sample of Economic Outcomes

Figure A.14: Robustness to Including 2008

A. GDP per capita



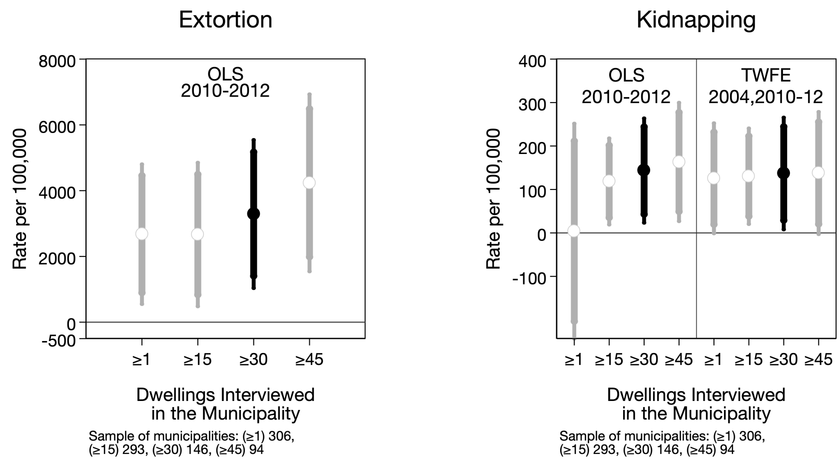
B. Firm Survival



Thick lines represent the 90 percent confidence interval and thin lines the 95 percent interval. Dots represent point estimates.

A.1.4.7 Robustness Specific to the ENVIPE Survey

Figure A.15: Robustness Check: ENVIPE Sampling
Sample Restricted According to Dwellings Interviewed in the Municipality



Thick lines represent the 90 percent confidence interval and thin lines the 95 percent interval. Dots represent point estimates.

A.1.4.8 Robustness Specific to Firm Survival: Alternative Matching Method

Table A.5: Alternative Definition of Firm Survival:
Matches restricted to stages 1 to 6 only
Difference-in-Differences
Dependent variable: Proportion of Firms from Previous Census Surviving

	(1)	(2)	(3)	(4)	(5)
$\text{Capture}_{my} = 1$	-0.09***	-0.06***	-0.04**	-0.04**	-0.04**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Emp. Export $\text{Manufacturing}_{my}$		-0.00	-0.00	-0.00	-0.00
		(0.00)	(0.00)	(0.00)	(0.00)
Emp. Other $\text{Manufacturing}_{my}$		-0.00	-0.00*	-0.00*	-0.00**
		(0.00)	(0.00)	(0.00)	(0.00)
Emp. Other Sectors_{my}		-0.01***	-0.01***	-0.01***	-0.01***
		(0.00)	(0.00)	(0.00)	(0.00)
(Population > sample median)=1 $\times y$			-0.04***	-0.03***	-0.03***
			(0.01)	(0.01)	(0.01)
(Pros. Crime Rate > sample median)=1 $\times y$				-0.03***	-0.03***
				(0.01)	(0.01)
Strategic Location=1 $\times y$					-0.00
					(0.00)
Observations	2,778	2,778	2,778	2,778	2,778
Municipalities (Clusters)	926	926	926	926	926
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Dependent variable mean	.47	.47	.47	.47	.47
$(\hat{\theta}/Dvm) * 100$	-18.4	-12.8	-9.1	-8.2	-7.7

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

Robust standard errors in parentheses. Regressions use municipality population (2005) as analytic weights.

Employment per sector: Bartik formal employment divided by the population 15-64 years of age. Population: Population from the census 2005 above the median of the regression sample. Pros. crime rate: Mean prosecuted crime rate (2004-06) above the median of the regression sample. Strategic location: Has port, or border crossing to the U.S., or a tolled highway.

A.1.4.9 Negative Binomial Regression: Homicides, Extortion, and Kidnapping

Figure A.16: Densities of Dependent Variables

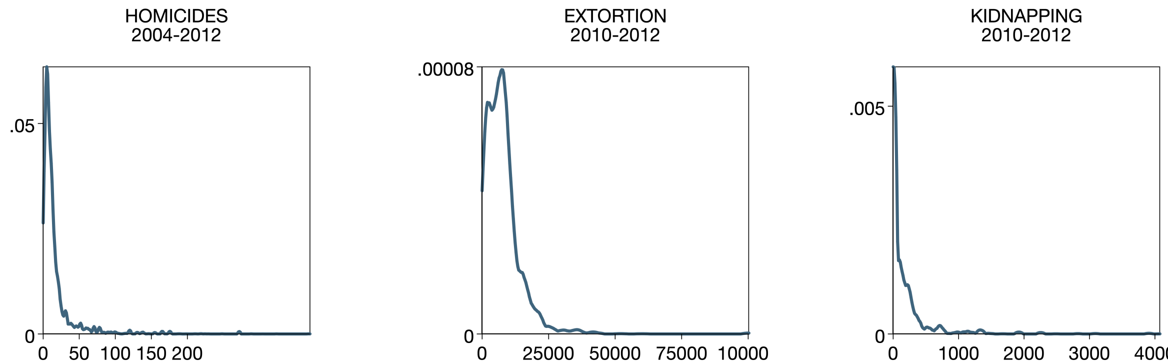


Table A.6: Negative Binomial Regressions vs. OLS
Weighted

	Negative Binomial Regression			*OLS*		
	(1) HOM	(2) EXT	(3) KID	(4) HOM	(5) EXT	(6) KID
$\text{Capture}_{my} = 1$	0.2^{**}	0.6^{***}	1.8^{***}	9.3^{**}	3288.7^{***}	150.1^{***}
	(0.1)	(0.2)	(0.6)	(4.0)	(1138.7)	(49.6)
Observations	8,877	438	584	8,877	438	584
Municipalities (Clusters)	987	146	146	987	146	146
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls						
- Predicted Employment_{my}	Yes	Yes	Yes	Yes	Yes	Yes
- Time invariant	Yes	Yes	Yes	Yes	Yes	Yes