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Banco de México

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The Impact of the Capture of Leaders of Criminal Organizations on the Labor Market: Evidence from Mexico*

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Abstract: This paper estimates the impact of the capture of leaders of criminal organizations on the labor market in municipalities where these organizations operated between 2004 and 2006. The difference-in-difference analysis compares different employment outcomes in cartel locations and the rest, before and after the capture of cartel leaders. The results show that captures caused a decrease in nominal wages and paid employment in cartel municipalities. Using Economic Census Data, I find that captures also caused a fall in the number of establishments and had a negative impact on other establishment outcomes. This document focuses exclusively on the impact of the capture of leaders of criminal organizations on the labor market until 2011 without studying other possible consequences, and thus does not make an integral assessment of this policy.

Keywords: Organized Crime, Labor Markets, Firms.

JEL Classification: J20, J48, K42, O17

Resumen: Este documento estima el efecto de la captura de los líderes de las organizaciones criminales sobre el mercado laboral en municipios donde estas organizaciones operaban entre 2004 y 2006. El análisis de diferencias-en-diferencias compara diferentes variables del mercado laboral en mercados con y sin presencia de cárteles, antes y después de la captura de los líderes de dichas organizaciones. Los resultados muestran que las capturas causaron una caída en los salarios nominales y los niveles de empleo remunerado en municipalidades con presencia de cárteles. Con información de los Censos Económicos, se encontró que dichas capturas causaron una caída en el número de establecimientos y tuvieron un impacto negativo sobre otros indicadores a nivel de establecimiento. Este documento se enfoca exclusivamente en el impacto de la captura de los líderes de las organizaciones criminales en el mercado laboral hasta 2011, sin estudiar otras posibles consecuencias, y por ello no hace una valoración integral de esta acción de política pública.

Palabras Clave: Crimen Organizado, Mercados Laborales, Empresas.

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1. Introduction

Transnational criminal organizations are present in every country of the world. They earn an estimated of 1.5% of global GDP and their activities have an impact on global threats as diverse as terrorism and climate change (United Nations Environment Programme Annual Report 2012). For example, poppy production in Afghanistan funded the Taliban, and as much as 90% of logging in the Amazon basin, Central Africa and South East Asia is being carried out illegally by organized crime (United Nations Environment Programme Annual Report 2012). Therefore, fighting organized criminal organizations is one of the international community's greatest global challenges (United Nations Office on Drugs and Crime Annual Report 2019).

A commonly used policy against large criminal enterprises is the capture or killing of leadership, which is commonly called “decapitation” (Phillips 2015). Once the authorities capture or kill high-ranking members, the organization loses leadership, which can result in a bid for control among remaining members and the loss of chains of command within the organization (Calderon et al. 2015). Therefore, with a single capture or death, governments might have reasons to believe they can cripple large enterprises. This explains its use: the arrest of Al Capone, the capture and death of Pablo Escobar, or the 19 high-ranked mafia members who were captured in Italy in 2019.¹

However, it should be noted that the policy of capturing leaders of a large criminal organization might have negative outcomes. The resulting turf war can extend to bystanders, increasing fear. Or it can destroy businesses, as remaining factions use firm extortion to increase their ability to wage their war. Therefore, policymakers ideally should consider all possible costs and benefits (in both the short and long term): on the one hand, a direct and strong fight against these organizations could lead to significant increases in violence and adverse effects on firms and employment; on the other hand, allowing criminal syndicates to increase in size and strength without opposition could lead to a weakening of the rule of law, the obstruction of different institutions' work and damage the population's welfare. Thus, a

¹ CNN. 2019. 19 mafia suspects arrested in joint transatlantic raids. <https://www.cnn.com/2019/07/17/europe/mafia-arrests-fbi-italy-intl/index.html>

difficulty in the design of policies to combat organized crime is striking an optimal balance between costs and benefits, when often some of these costs are unanticipated.

This paper focuses exclusively on measuring the short-term spillover effects that the capture of leaders of criminal organizations could have on the labor market, without attempting to provide a comprehensive evaluation of this policy. This is the first study of the impact of the capture of leaders of criminal organizations on the labor market, and this is because a leaders' capture are rare events within a country, and once those happen, they tend to affect large areas simultaneously. I study the impact of the capture of leaders of criminal organizations in Mexico because: 1) during a two-year period there was a major leader ("kingpin") captured for each major drug trafficking organization (DTO or colloquially called cartel); 2) this change was caused by an unexpected policy, 3) there is spatial heterogeneity in cartel location; 4) and there is detailed data on employment, firm and household characteristics. This temporal heterogeneity in kingpin capture and geographic heterogeneity in cartel location facilitates the creation of a differences-in-difference strategy.

To complete this study, I construct a dataset at the municipality-quarter level, by merging data on labor outcomes (National Employment Survey, ENOE), municipality murder rates (Vital Statistics), population (National Population Council, CONAPO) and cartel municipality presence (Coscia and Rios 2017). The period of study is from 2005 to 2011, as multiple policies took place afterwards. For example, after 2011 the Oxycotin reformulation influenced violence in Mexico (Sobrino 2020). The dataset on cartel presence is constructed using a MOGO (Making Order using Google as an Oracle) framework which collects web content (for example, online newspapers and blogs) that links municipalities and reported cartels' areas of operation. I then use the timing of the first capture of high-ranking leaders for each cartel to estimate causal effects of a leaders' capture on wages and employment. Other datasets, such as the Economic Census, Social Security Administrative Records (IMSS), IPUMS Population Census sample and the CIDE (Center for Research and Teaching in Economics) drugs policy program database is also merged to study other relevant labor outcomes.

I find that cartel's leader capture reduced wages by 5.3% in municipalities with cartel presence, which is partially explained by a switch from paid to unpaid work of 1.2 percentage

points. The drop in wages and paid employment is gradual and becomes statistically significant a year after the capture. This timing coincides with the gradual increase in violence in the same localities (Lindo and Padilla-Romo 2018), but the effect is not confined to municipalities that see a large increase in violence.

This shift from paid to unpaid employment is partially explained by an increase in the proportion of workers doing domestic work and the informalization of the labor market. Moreover, using the short-term panel nature of ENOE,² I observe that after treatment, the formally employed in cartel areas transition more often to the informal market. Also, using IMSS administrative data I find that it also became harder to find a formal job: the number of entrants to the formal market decreased and it also became less likely for job changers to find new formal opportunities and when they do, it is not into higher paying firms. This evidence, in conjunction with a larger impact among workers with less education, implies that shocks to organized crime can have a large impact on inequality and intergenerational mobility.

I also explore this phenomenon from a firm perspective. Using data from the Economic Census I find that this drop in paid employment coincides with a decrease in the number of employers (establishments) in several industries. Moreover, it seems that firm composition was altered: firms who had less than 9 years in operation at the onset of the War on Drugs and informal firms³ were more likely to close. However, even those firms who survived saw a drop in revenue, production and number of workers.

The most closely related studies to my paper are Dell (2015) and Velasquez (2019), which also study the impact of the War on Drugs on labor outcomes. However, I take a focused look on this phenomenon and extend the previous work in several directions that can help us better understand the impact of shocks to criminal organizations on the labor market.

First, I focus explicitly on the impact of the capture of a cartel's leader.⁴ This difference is important because increased cartel activity can happen independently of extreme violence.

² ENOE can be used as a panel for only 5 consecutive quarters.

³ The Economic Census has no data on the formal status of a firm (this changed for the 2019 wave, which shows 62% of firms are informal). However, IMSS administrative data has information on the number of formal firms. My results show that there was formal-firm destruction, but its impact was much smaller.

⁴ Which, in addition to increase violence, can also change cartel behavior (cartel spending, protection racketing), military response, the increase in likelihood of future captures and the capture of lower ranked members.

This requires additional robustness checks that are possible by using the CIDE Program of Drug Policies (CIDEPPD) database, which contains information on all recorded violent events of the “War on Drugs” up to 2011.

Second, I extend this work by studying employers (firms) and the transition to informality. Due to data limitations, previous research had to study the impact of organized crime on firm outcomes for a particular type of firm like exporters (Gorrin et al. 2019) or to focus on firm survival (manufacture; Gutierrez-Romero and Oviedo 2014). In this paper I study the impact of a leaders’ capture on both, for several industries. This is possible because I use the confidential version of the Economic Census⁵ and, following the matching process of Busso et al. (2018) I build a new 15 year-long firm panel of all establishments in Mexico. The use of the IMSS administrative data, in addition to focusing on formal firms, allows me to follow individuals across time and study the informalization of the job market that followed.

Third, as my identification strategy is flexible and allows me to use both cross sectional and panel data,⁶ I am able to use ENOE, IMSS administrative data and the Economic Census.⁷ The use of these databases allows me to examine for further heterogeneous effects (for both firms and workers). For example, I can study firm survival (for each major industry, size, age and other firms’ characteristics), or to study the impact on workers by education or formal sector status. Moreover, the use of individual fixed effects on IMSS administrative data and the Economic Census allows me to study new outcomes such as firm migration, revenue of surviving firms, or the number of job changers and entrants to the formal job market (who might be of interest as they are more vulnerable to shocks; Gertler et al. 2016).

The rest of the paper is organized as follows. A background on Mexican cartels is in Section 2, while the data description is in Section 3. The identification strategy is presented in Section 4. Section 5 shows results, while Section 6 presents some heterogeneous effects and Section 7 shows the impact on employers. Section 8 offers multiple robustness checks. A discussion on mechanisms is presented in Section 9 and the paper concludes in Section 10.

⁵ Which allows me to study only establishments who are representative at the municipality level. For example, transportation firms report their outcomes at the State Level. By using the disaggregated and confidential version of the Economic Census, it is possible to create a dataset that is representative at the municipal level. This access at this level was given by INEGI Micro-data laboratory.

⁶ The results are not sensitive to the inclusion of the individual fixed effects for the periods and samples where it is possible to add them.

⁷ Which are significantly larger databases than the Mexican Family Life survey: I can study 821 municipalities, against 195 in the MxFLS third wave. Also, most of the datapoints on the third wave of the MxFLS refer to 2009 and 2010. In other words, results from the MxFLS do not significantly extend beyond this point.

2. Context and Intervention

With the presidential term of Felipe Calderon, who took office in 2006, new policies fighting organized crime were implemented in Mexico. In particular, the government favored an approach that sought the capture of high-ranking members of the major cartels.⁸ In this context, the first operation to combat cartel leadership took place on December of 2006. However, the first major capture of a cartel figurehead took place until August of 2007 (Rios 2013). From this date, it would take roughly a year (until December 29, 2008) for the kingpins of all five major cartels in Mexico to be captured. Table 1 shows the timeline of each cartel leaders' capture.⁹

The capture of key players caused internal struggles within these organizations (Rios 2013). As high ranked members of these cartels were eliminated, two problems emerged: a loss of leadership and a vacuum of power that resulted in violence. For example, in the case of the Sinaloa Cartel, after the capture of the lieutenant Alfredo Beltran Leyva, Alfredo's brothers Hector and Arturo blamed Joaquin Guzman Loera (El Chapo) for the capture (Rios 2013). As a result, after a decade of stable homicide rates, violence in Mexico increased noticeably (Figure 1). While alternative explanations have been proposed to explain the increase in violence, such as the expiration of the US Federal Assault Weapons Ban in 2004 (Chicoine 2016), the role of the deployment of federal troops (Merino 2011), and the US coca-seizures in Colombia (Castillo et al. 2020), several experts point to the relevance of cartel leaders' captures (Rios 2013).

It is also important to note that due to struggle for power within cartels, violence changed in type: Cartels augmented their reputation by increasing their cruelty and their crimes more frequently targeted civilians (Diaz-Cayeros et al. 2011). In addition, to finance their war, cartels diversified their sources of cash flows and turned to new crimes, such as firm extortion (Merino 2011).

⁸ The history of organized crime in Mexico and the relationship between these organizations and the Mexican government is the subject of several debates. See Grayson (2013) and Rios (2013) for a more in detail description of this relationship before the War on Drugs.

⁹ Notice that even though the probability of capture may be a function of intelligence and other police and military resources employed, the notes on each capture (see Appendix, Table A4) suggest that these captures were the result of random events where these individuals, who normally are guarded by groups with military weaponry, lowered their security or made a mistake.

3. Data

This paper brings together data from several sources, yielding a final data set at the municipality-quarter level, that covers the period from 2005 to 2011, which includes information on labor outcomes, murder rate, cartel presence, and kingpin time of capture.

To identify municipalities with cartel presence, I use the database created by Coscia and Rios (2017). This dataset has annual information from 1990 to 2010 on the municipality presence of each of the major cartels of Mexico. The authors constructed the dataset using a MOGO (Making Order using Google as an Oracle) framework for selecting the most reliable subset of web information to collect information on relationships between sets of entities (cartels and municipalities). It uses indexed web content (i.e., online newspapers and blogs) and various queries to identify cartels' areas of operation at the municipality level between 1990 and 2010.¹⁰ This approach is possible because of the culture of cartels in Mexico: they want to be seen in the media as they can signal their presence and avert confrontation with other cartels and send a message to the government (Rios 2013). As cartels responded to capture by expanding to new municipalities, I also define areas of operation using only data before the war on drugs began (2004-2006).¹¹ Figure 2 shows the distribution of cartels based on this definition: There are 2084 municipalities have no cartel presence, 208 have one, 89 have two, 55 have three, and only 18 have four. The main take-away from this figure is that cartel presence is widely distributed across the country.

These data are merged with the ENOE. ENOE is a nationally representative survey that contains information about different labor outcomes (wages, among others) for the Mexican population over the age of 14. Although the dataset is a rotating panel that follows individuals for five quarters, I collapse the data at the municipality-quarter level.^{12,13} Information is available from 2005 onwards, but I limit my main analysis to 2011 to avoid any effect from

¹⁰ The data by Coscia and Rios (2017) has two main limitations: 1) they capture the presence of a cartel with error and 2) they do not measure intensity or scope of the presence of the cartel in a municipality. The first limitation will bias my estimates towards zero, the second will be masking heterogeneity of cartel activities.

¹¹ Data are less reliable before 2004.

¹² This is also for practical reasons, as computational requirements for my analysis are lower with collapsed data.

¹³ All analysis from ENOE uses survey weights, as it is commonly used to estimate official statistics. This does not apply to the Economic Census and IMSS data, as they are not a survey and do not have weights. Please notice that ENOE is not representative at the municipality level. This is particularly important as there are some municipalities that are not sampled in all survey waves. However, I study those municipalities that I can see in all the survey waves. Moreover, I compared the data to that of Census data on the same municipalities.

the reformulation of Oxycotin (Sobrinho 2020). However, I also estimate some results with additional years using the Economic Census and IMSS data.

It is important to note that even though ENOE has the month in which the survey took place, I do a quarter-level analysis because the survey is representative at that level.¹⁴ Finally, some outcomes are log-transformed, while those traditionally expressed as a percentage are kept as a rate (i.e. the unemployment rate).

I also merge the murder rate.¹⁵ In order to construct it, I use the number of intentional homicides as reported by INEGI's Vital Statistics. The sum of murders is aggregated at the quarter-municipality level, and then it is expressed as a proportion of the CONAPO's municipality population times 100,000.

Finally, I merge cartel location with four additional datasets to test the impact of the capture of a cartel leader on some related outcomes or to perform robustness checks. The first one is IPUMS' 10% sample of the Population Census. This dataset is used to test whether captures changed population size and to replicate some results from ENOE.¹⁶ The second one is the CIDEPPD¹⁷ database, which contains information on all recorded violent events of the "War on Drugs".^{18,19} It is used to test other possible confounders (military operations with no captures). The third one is the Economic Census, which has data on all firms with a physical establishment in Mexico.²⁰ This is a cross-sectional dataset; however, it is possible to construct a panel of firms by using the methodology described by Busso et al. (2018). This process matches the same firm across different waves by using highly confidential

¹⁴ Although ENOE is nationally representative, it does not consider data on all municipalities. My set of municipalities tend to be more urban than the excluded municipalities.

¹⁵ The murder rate is used as the measure of crime in Mexico because it is the most accurate proxy for crime available. The reason is that given the nature of the crime, homicides are less likely to be systematically misreported. Moreover, the data on homicide has been vetted and confirmed through other data sources (Velasquez, 2019).

¹⁶ There are two differences between the Population Census and ENOE: 1) The Census has labor outcomes data every 10 years, and 2) instead of having data on a subset of municipalities, it has data on all municipalities.

¹⁷ Centro de Investigacion y Docencia Economica Program of Drug Policies.

¹⁸ This dataset has a unique history: at the beginning of the Presidency of Peña Nieto, the CIDE Drug Policy Program received, anonymously, a disc containing files that recorded all events that took place between Dec. 2006 and Nov. 2011. Due to the anonymity of the dataset, a multidisciplinary team of the Drug Policy Program in collaboration with CentroGeo, designed a validation system for the information based on open sources. This process is described in Atuesta et al. (2019).

¹⁹ There are some possible limitations of using this database: 1) The classification of an event might be biased by the unknown agency in charge of recollection. 2) The quality of the data varies according to the government agency in charge of the recollection of the event. 3) Data related to executions varies in quality and detail across geography and time.

²⁰ With certain limitations: It does not contain data on agriculture, cattle and foresting industries, foreign governments' organizations, political organizations or firms that share an address with the owner's household. It is also restricted to firms with a fixed and permanent location under the control of a single entity. The data is not representative level for industries who report data at the State level (like construction and transportation). For a detailed data of each industry, see the Census user's guide.

information.²¹ Finally, I use the Social Security Institute (IMSS) administrative data.²² This worker-level dataset allows me to track workers as long as they have a private sector formal job.

In Appendix A I show summary statistics (by cartel and not cartel regions) for ENOE (Table A1, A2), Economic Census (A3), IMSS administrative Data (A4) and Population Census (A5). Figure A1 shows which cartel municipalities could not be included in the analysis using ENOE. Overall, these tables show that Cartels tend to be in areas that are more urban and closer to the US Border. This explains why firms in cartel localities tend to be slightly larger. Overall, one needs to be cautious of these differences and they will be addressed in the following sections.

4. Methodology

A. Statistical Model

The empirical strategy can be generalized in the following regression framework:

$$(1) \ln(y_{mst}) = \alpha_{st} + \delta_m + \beta(\text{Cartel}_m \times \text{Post}_t) + \varepsilon_{mst}.$$

The unit of analysis is the municipality m , in state s , during the year-quarter t . I include municipality (δ_m) and state-by-year-by-quarter (α_{st} ; this is the interaction of the year-quarter fixed effects (t) with each state (s)) fixed effects to control for municipality characteristics that do not change over time, and to control for State-specific shocks over the sample period. The dependent variable is the natural logarithm of y_{mt} (which is any labor outcome, like the hourly wage). Except when the variable y_{mt} is a rate (like the unemployment rate), as in such case, the dependent variable is a rate (I do not apply any log-transformation). The treatment group are those municipalities where a cartel was present during 2004 and 2006 (where the dummy Cartel_m is equal to 1), and the treatment period are all the year-quarters following the capture of the leader of the cartel that has presence in such municipality (where the dummy Post_t is equal to 1). Therefore, β measures the impact of the changes in labor

²¹ See Appendix D for a detailed description of the matching process.

²² The data was accessed through the Econlab at Banco de Mexico. The EconLab collected and processed the data as part of its effort to promote evidence-based research and foster ties between Banco de Mexico's research staff and the academic community. Inquiries regarding the terms under which the data can be accessed should be directed to: econlab@banxico.org.mx.

outcomes in cartel municipalities for the year-quarters following the capture of the kingpin relevant to that municipality. Finally, ε_{mt} is the regression error.²³

The capture of a cartel leader is defined as the arrest of leaders or lieutenants who are at the highest levels of the cartel hierarchy. I classify as a capture of a leader when the press release by the Mexican institution in charge of the arrest indicated so. In municipalities with multiple cartel presence, I consider only the first capture in the municipality as my point of reference. While I could add the capture of lower-level members, these might be a function of the capture of higher ranked members. Also note that I focus on the first capture because it is likely that the probability of subsequent captures is endogenous. This means that my estimates will reflect the total effect of the first capture, which includes: changes in cartel behavior (cartel spending, protection racketing), increase in violence, military response, the increase in likelihood of future captures and the capture of lower ranked members.

I also use an alternative specification where I decompose my β_t coefficients into time-bins to show regression-based evidence for parallel trends. In other words:

$$(2) \quad \ln(y_{mst}) = \alpha_{st} + \delta_m + \beta_1(\text{Cartel } x \text{ 18 - 13 months before capture})_{mt} + \beta_2(\text{Cartel } x \text{ 12 - 7 months before})_{mt} + \beta_3(\text{Cartel } x \text{ 1 - 6 months after})_{mt} + \beta_4(\text{Cartel } x \text{ 7 - 12 months after})_{mt} + \beta_5(\text{Cartel } x \text{ 13 - 18 months after})_{mt} + \beta_6(\text{Cartel } x \text{ More than 18 months after})_{mt} + \varepsilon_{mst}.$$

The difference between equation (2) and (1) is that the dummy for cartel presence is interacted with different time bins. For example, I generate dummies equal to 1 for municipalities with cartel presence 7-12 months before the capture, and then another dummy for 6-1 months before, and so on. The omitted period is the 6 months before the capture, which means that the results are expressed as changes from the semester before the capture. For example, the coefficient β_3 shows the change in the labor outcomes in cartel

²³ Even though this regression has a high degree of fixed effects, this is an OLS-regression and requires the traditional assumptions of the OLS model. Among the required assumptions, I would like to focus on two of them: 1) Spherical errors are corrected through the appropriate clustering strategy and 2) random sampling/omitted variables. For the first one, several clustering strategies (Table B5) are used; and for the second one, the main concern is that cartel location might be endogenous. However, by varying the control group to include only municipalities that are similar through observables to cartel municipalities, this concern is diminished (See Robustness Checks).

municipalities, relative to non-cartel municipalities and cartel municipalities without capture, 6 months after the capture.^{24,25}

Finally, I estimate the clustered standard errors according to Abadie et al. (2017). They state that clustering should be design- or experiment-based. Under the first scenario, the problem is that there are groups in the population that were not sampled. As ENOE surveyed within each municipality, I should cluster at the municipality level. For brevity, I present my results using this strategy. The second clustering strategy implies that I should cluster by cartel because the source of variation was given at the cartel level. However, with only five cartels, there is a problem of too few clusters. The problem of few clusters is analogous to the problem of few observations: In both cases, small sample distributions for any estimator is not available but we know (from Monte Carlo evidence) that this will create substantially biased standard errors, that can make me over reject true null hypotheses (Cameron et al. 2015). This problem is present even if the number of observations per cluster is large and the model is consistent (Canay et al. 2019). As a response to this challenge, I can cluster on cartel-combinations, which leverages the fact that there are municipalities with multiple cartel presence. This procedure requires the use of wild-bootstrapped tests,²⁶ which under a scenario with a large number of observations per cluster, can perform well in settings with as little as five clusters (Canay et al. 2019).^{27,28} I include this analysis, among other alternatives, for a selected number of outcomes in Appendix B. That said, the analysis is not sensitive to the choosing of a particular clustering strategy.

B. Identifying Assumptions

To ensure credibility of the analysis, two assumptions must hold: Conditional Independence and Stable Unit Treatment Value Assumption (SUTVA).

²⁴ One might be tempted to do an IV-analysis, where the endogenous variable of murder rate could be exogenously shifted by the capture of a high-ranked member. The problem with this approach is that the exclusion restriction does not hold. There are several reasons why this might be the case: for example, as cartel members start to fight, cartel spending might be reduced and affect the demand for local goods.

²⁵ I do not use an individual fixed effect strategy because ENOE only follows an individual for 5 quarters and captures takes more than a year to show a statistically significant impact. This makes it impossible to test for parallel trends and will only show partial effects. However, I included this analysis in Appendix E, Table E1.

²⁶ The procedure generates a large number of bootstrap samples mimicking the original distribution. Then, each of them is used to compare the bootstrap test statistic, using the same test procedure as for the original sample (Roodman 2015).

²⁷ However, the probability of over-rejecting grows exponentially as the number of clusters or cluster size diminish.

²⁸ Due to the reliance in hard to test assumption of relatively homogeneity of covariates and balanced clusters, the general agreement is that there is no perfect way to deal with too few clusters. However, current best practices are to rely on wild-bootstrapped tests as they tend to function better than tests based on large sample theory even when these assumptions do not hold strongly (Canay et al. 2019).

The first key assumption is conditional independence: Given observable characteristics, treatment assignment should not be correlated with possible outcomes. The concern in this case is that cartel municipalities are systematically different from non-cartel municipalities. One thing I can do to minimize this concern, is to restrict my sample and include only cartel municipalities and rely on the plausibly exogenous timing of a capture to estimate causal effects. In other words, I can use as treatment group the cartel that gets treated first and the control group can be the cartel that gets treated last. Although this would create more closely comparable groups, my sample gets reduced and makes it impossible to study heterogeneous effects. However, the point estimates are not statistically different from the analysis that uses non-cartel municipalities as a control group (see Robustness Checks). Therefore, I still use my original research design.

Another test is to assess if there are parallel trends between cartel and non-cartel municipalities for the period before the capture. Figure 3 shows five outcomes of interest (the quarterly murder rate, hourly earnings,²⁹ weekly workers' hours, labor force participation, and number of paid employees) over time separately for municipalities with cartel presence and those without such presence. I mark with a vertical line the quarter of the first capture and show that before this line there is a parallel trend and a divergence afterwards for the murder rate, hourly wages, and paid employment. One aspect that I would like to draw attention to is that in all cases, both groups (cartel and not-cartel municipalities) tracked one another before treatment, which provides supporting evidence for using municipalities without cartel presence as a meaningful comparison group.

The second assumption is the Stable Unit Treatment Value Assumption (SUTVA). SUTVA will not hold as cartels conquered territories with no previous cartel presence as a result of cartel disruption, and violence increased in those newly conquered municipalities. This will mute the impact of a cartel leader capture on labor outcomes. A possible method to minimize the impact of spillovers is to eliminate from the sample those municipalities where cartels expanded and compare cartel municipalities against regions that never had a cartel (see Robustness Section).

²⁹ This is estimated as the sum of all wages over the sum of all worked hours in the municipality.

Another challenge regarding this key assumption is non-random migration as a response to increased violence. If this were the case, it would hinder the identification of the true impact of cartel decapitation. However, the general agreement in the literature is that there was little internal migration (Brown et al. 2018).³⁰ However, I test a migration response by running equation (1) on the total population size from the Population Census. Results are presented in Table 2.

My results show that there is no large migration response (column 1). However, it might be that there are some subgroups that are more likely to migrate as a response (for example, young, male and college educated workers). Once I restrict the sample, I still find no statistically significant effect on any subgroup.^{31,32}

Note that there are other mechanisms that could affect SUTVA (like prices or production chains) but there is not enough detailed data to test them.

5. Results

Table 3 presents the results of running equation (1) on the following labor outcomes: the proportion of the labor force in paid employment,³³ the proportion of the labor force in unpaid employment,³⁴ the unemployment rate,³⁵ and the proportion of the working age population not in the labor force (NILF).³⁶ I find that after a cartel leaders' capture, there is not only a decrease in the percentage of paid employment of 0.9 percentage points (column 1), but this effect is mirrored by an increase in the proportion of unpaid employment (column 2). There are no statistically significant effects on unemployment or in the proportion of workers not in the labor force (column 3 and 4). In other words, as individuals are leaving paid

³⁰ New evidence from Aldeco Leo et al. (2020) shows a small increase in internal migration caused by an increase in murders. As this effect is of approx. 78 thousand individuals and driven primarily by low-skilled individuals, the possible bias created is not large enough to alter my conclusions.

³¹ About US-migration: Rios (2014) found that crime-induced US-migration was driven by a small selection of municipalities and was able to identify those localities. See Robustness Section for this analysis.

³² Another possible test for migration is to consider firm migration. However, my analysis shows that the impact of firm migration was too small (Appendix, Table E8).

³³ Individuals who report both wages and working hours.

³⁴ Individuals who report zero wages, but still reporting working hours.

³⁵ Individuals who do not have a job but are looking for one.

³⁶ Individuals who do not have a job and are not looking for one.

employment, they transition to endeavors such as helping family members with their market activities or other activities that do not report a wage.³⁷

In Table 4 Panel A, I present the results of equation (1) for the following labor outcomes: log-working hours,³⁸ log-hourly wages,³⁹ log-monthly average wages⁴⁰ and log-monthly average wages for paid workers.⁴¹ These results show that cartel leaders' capture caused a decrease in wages independently of the chosen measure.^{42,43} In municipalities with cartel presence, after a cartel leaders' capture, there is a decline of hourly wages (5.3%, column 2), monthly average wages (4.2%, column 3) and monthly average wages if in paid employment (3.4%, column 4), while weekly working hours remain unaffected (column 1). Columns 2 and 3 are quite similar because in both cases, the denominators of the dependent variable (working hours and number of workers) remained unaffected. Column 4 is different, because the number of paid workers is also decreasing, creating a bias against negative results. However, the conclusion that decapitation decreased wages remains statistically significant.

These results indicate that households lose \$160-165 MXN monthly. This is similar to the amount spent by the average household on clothing, or half of the spending on education (INEGI).⁴⁴ In other words, this is an economically significant amount of lost income for the average household.

In Figure 4 I show the temporal decomposition of the impact of cartel leaders' capture on selected labor outcomes. The main takeaway from these figures is that it takes several months after cartel leaders' capture to see a response from the labor market. The outcomes that react the fastest are paid and unpaid work, which see a gradual change and becomes statistically significant at least a year after the capture. Wages also take more than a year to show a

³⁷ Notice that as this is a cross sectional analysis, not finding a significant effect on the unemployment rate does not mean there is no job destruction. It might be that certain jobs get destroyed, but workers had to move to new recently created jobs (which might be of lower quality).

³⁸ The logarithm of the sum of all working hours in the municipality.

³⁹ The sum of all earned monthly wages over the sum of all working hours. All wages are not converted into real wages.

⁴⁰ The sum of all earned monthly wages over the sum of all workers.

⁴¹ The sum of all earned monthly wages over the sum of all paid workers.

⁴² A standard concern about this type of analysis is that as individuals leave the labor market, they stop reporting earnings. However, as the effect of a leaders' captures is not of increased unemployment or changes in labor force participation, but of a shift from paid to unpaid employment, the concern is different. This will imply that the interpretation of my results is as follows: the leaders' capture of cartels caused a decrease of 5% in the average payment of the hour worked.

⁴³ On the other hand, individuals with higher earnings might be hesitant to report their true income, but my results are not sensitive to the elimination of the top earners of my sample (see heterogeneity analysis).

⁴⁴ Encuesta Nacional de Ingreso y Gasto de los Hogares (ENIGH), 2010.

decrease, which is in line with the evidence presented by Lindo and Padilla-Romo (2018) where it is noted that it takes more than a year after the capture for the murder rate to increase.

6. Heterogeneity

Some of the previous results are not surprising: Feng et al. (2018) showed that unemployment is largely a feature of developed economies. This is because in developing countries, they argue, only the most skilled workers search for wage jobs and most workers select into self-employment. This implies that in practice, few are unemployed. Having said that, it is not clear why leaders' capture decreases paid employment. Two explanations seem feasible: some might increase their home production, and/or they might transition to the informal sector.

Table 5 finds that a cartel leaders' capture increased the proportion of people in the working force spending time on home production (cleaning, cooking, etc.; column 3). This result is in line with Velasquez (2019): She argued that because of an increase in fear and lower opportunity costs, women were more likely to reduce their labor participation and increase their time on domestic chores.

The second possibility is that workers joined the informal market. In this case, I refer to informal work as those workers in firms that are not registered. In a sense, it is an indicator of low-quality jobs as they are not required to offer law-mandated benefits. I also report results for government and formal workers.⁴⁵ Results are presented in Table 6. My result suggests that decapitation causes an increase of 1% in the probability of being employed in unregistered firms in cartel municipalities. Using the panel nature of ENOE, I find evidence that shows that previously paid and formal workers were displaced to the informal market, while the unemployed did not join informality more often (Table E1).⁴⁶ Moreover, evidence using the Mexican Social Security Administrative data I find that a leaders capture decreased the number of entrants to the formal market, the wages of workers with experience and the probability of transitioning to better paying firms (Tables E2 and E3).

⁴⁵ Some subgroups are omitted, like subsistence agriculture, because sample sizes are too small.

⁴⁶ In the appendix there is a large discussion on how the panel differs from the cross-sectional analysis and the limitations of such analysis.

These results imply that a cartel leaders' capture negatively changes the probability of a high-quality job match. This is in line with previous research that finds that workers take mismatched jobs during recessions (Gertler et al. 2016).

Finally, as those more likely to become informal are usually those with lower levels of education, heterogeneity by skill level might be of interest. In Table 7, I show the decomposition of my results by education level: Hourly wages decrease the most among the lowest-skilled workers. This confirms that decapitation has implications on inequality: low-skilled workers, the ones that faced the largest drop in wages, are also the ones with the lowest initial wages.

7. Employers

As the labor market becomes more dependent on the informal sector and wages decrease, it is not clear what will happen to employers. For example, it might be that as revenue falls, firms must downsize or close. However, this could cause displaced workers to create new firms (who are smaller in size and informal in nature). In any case, as it takes time and resources to open new firms and transform them into stable job creators, firm destruction would mean a longer recovery period and a more insidious impact.^{47,48}

I explore these hypotheses by using data from the confidential version of the Mexican Economic Census. In Figure 5 I show the number of firms over time separately for municipalities with cartel presence and those without such presence. I mark with a vertical line the last Census wave before the War on Drugs and show that by 2009 there was a drop in the number of firms in cartel municipalities. More formally, I run the following regression:

$$(3) \ln(y_{mt}) = \alpha_t + \delta_m + \beta(\text{Cartel} \times 2009)_{mt} + \gamma(\text{Cartel} \times 2014)_{mt} + \varepsilon_{mt},$$

where y_{mt} represents the number of firms in municipality m during Census wave t . $(\text{Cartel} \times 2009)_{mt}$ is an indicator variable equal to one if the observation takes place during the 2009 wave in a municipality that had cartel presence (zero otherwise).

⁴⁷ To open a business, firms need on average 8.4 days (2017). However, to finish all the paperwork to have a fully operating business (including paying taxes, having energy and construction permits, among all other paperwork) firms need on average 1.6 years and 35.3 documents. Que tan difícil es abrir un negocio en Mexico. El Economista. See <https://www.eleconomista.com.mx/empresas/Que-tan-dificil-es-abrir-un-negocio-en-Mexico-20170620-0122.html>

⁴⁸ Also, one needs to consider how much time do families need to save, to have enough money to pay the sunk costs.

$(Cartel \times 2014)_{mt}$ is similar but for the 2014 wave. This is just equation (1) with a different time frequency, as the Census is a quinquennial dataset.

The results are shown in Table 8. First, I test the impact on the number of firms in the municipality and the number of firms per capita (columns 1 and 2), I also try a negative binomial model on the number of firms (column 3), add state-by-year fixed effects (columns 5 and 6) and pre-trends (columns 6 and 7). Overall, I find that cartel leaders' capture reduces the number of firms by 4-7% by 2014.⁴⁹ In other words, the impact of a cartel leaders' was not short-lived.

By industry, almost all economic sectors were affected (Table 9). In fact, I find that the number of retail establishments decreased 6% by 2014, which is similar to the impact on manufacture (7% by 2014). However, I found a null impact on the number of wholesale establishments and hotels. This could be explained by the fact that firm destruction was the strongest among young and smaller firms.⁵⁰ This is to be expected as larger and older firms fare better under most economic shocks because they have more capital (Fort et al., 2013). For example, they might have easier access to credit or are able to fire more workers without completely disrupting their production processes. In Table 10, I explore the industry heterogeneity of firm destruction among firms with more years in operation (those that were at least 9 years at the beginning of the War on Drugs). I find that firms with more years in operation, in all major industries, were likely to stay in operation after the shock.

The problem at this point is that, because of an attrition bias, it is not possible to study the impact on average firm outcomes. However, what can be done is to find subgroups of firms whose survival was not compromised by cartel disruption and study the impact of a cartel leaders' capture on those firms' outcomes. Notice that this analysis will not be ideal, as it will study the impact on larger and more profitable firms and will also include the effect of the destruction of competitors. However, this analysis will give us a lower benchmark of the true impact of the capture of a cartels' leader.⁵¹

⁴⁹ Please notice that this result includes both formal and informal firms with an establishment. For an analysis on formal firms, see Appendix E3.

⁵⁰ Please, notice that I included a column for "Single owner" firms. These are firms that are not part of or a branch of a larger firm, like a McDonalds. These firms include family businesses that do not respond to national planning campaigns. The purpose of this column is to eliminate the possibility that large firms are changing their business plans and opening in locations with lower violence levels.

⁵¹ The validity of this analysis is contingent on not finding statistically significant changes in a group of firms. If this were not the case, non-cartel regions would have a combination of firms that might have survived and might have not, in the presence of a cartel; and these

As my previous results (Table 10) show that firms that with at least 9 years in operation at the beginning of the War on Drugs were able to survive at the same rate in cartel and non-cartel municipalities, I can create a balanced panel of surviving firms. This panel was created using Busso et al. (2018) methodology (for a brief description, see Appendix D).^{52,53}

The first step is to measure firm migration. As the number of firms from my panel that moved as a response to violence is small (see Table E6), I excluded them from my analysis and created a balanced panel of firms that stayed in the same municipality for the duration of the four Census waves (1999, 2004, 2009 and 2014). I then estimated the impact of cartel leaders' capture on wages, workers, revenue, production costs and value of production,⁵⁴ using the following firm fixed effect regression:

$$(4) \quad \ln(y_{fmt}) = \alpha_t + \delta_f + \mu(Cartel \times 2004)_{mt} + \beta(Cartel \times 2009)_{mt} + \gamma(Cartel \times 2014)_{mt} + \varepsilon_{mt},$$

where y_{fmt} is the outcome of establishment f , in municipality m , during wave t . As I am including only firms that did not migrate, firm fixed effects also imply municipality fixed effects. $(Cartel \times 2009)$ is an indicator variable equal to one if the observation takes place during the 2009 wave in a municipality that had cartel presence before (2004-2006) the War on Drugs. $(Cartel \times 2014)_{mt}$ is similar, but for the 2014 wave.

The results are shown in Table 11. Panel A has the results for all firms, Panel B for retail, Panel C for manufacture, Panel D for restaurants and Panel E for hotels. In each Panel I show the results from equation 4 for five outcomes of interest: column 1 shows the impact on the logarithm of workers, column 2 on the logarithm of labor costs (sum of all wages paid), column 3 on the logarithm of total revenue, column 4 on the logarithm of costs (related to production) and column 5 on the logarithm of the value of production (as reported by the firm).

two groups would be compared to the highly productive cartel region firms that were not destroyed. This would affect my estimates in the following manner: as firms that would have been destroyed in the presence of cartels are more sensitive to shocks and have no observed counterfactual, then my reported negative coefficients are an underestimation of the true impact and I would have a downward bias.

⁵² 30% of firms alive in 1999 survived up to 2014. By that year, they represented 12% of all firms.

⁵³ This is also convenient for a particular reason: as old firms were not particularly impacted, I can use all the firms alive in 1999 and that survived the following waves. This allows me to create a large firm panel.

⁵⁴ These are the outcomes that have information for all Census waves.

Results from Panel A, which includes all firms, shows that cartel disruption has a negative impact in the number of workers and wages paid: by 2009, the number of workers was reduced by 1.3% and the sum of wages dropped by 4.5%. By 2014, the gap in the number of workers between cartel and non-cartel municipalities became non-statistically different from zero, but the drop in wages remained at a relatively similar level (-4%). I also found negative impacts on revenue (2.8% by 2014) and production (3.4% by 2014), with a negligible impact on production costs.

As approximately half of the firms within my panel are in retail, the results for this sector are similar (Panel B). Panel C shows the results for manufacture: the point estimates for most outcomes are negative, however, the standard errors are quite large, and my estimations are imprecise. Therefore, I do not have a statistically significant result for most outcomes (wages and production in 2009 are exceptions). The results for restaurants show a negative impact for wages, revenue, costs and production by 2014 similar to those of retail. Finally, I show the impact on hotels (Panel E). Hotels show a statistically significant reduction in a wide set of outcomes (wages, labor costs, revenue, costs and production) by 2014 and is the most damaged industry among all those I could study. A probable explanation of this drop of 13.5% in revenue is a fall from tourism and the fact that hotels cannot substitute consumption with locals at the same rate as a restaurant might do. Moreover, as total tourism for Mexico might have drop in favor of other countries, this result might be an underestimation of the effect of crime.⁵⁵ Overall, Table 11 shows that cartel disruption had a negative impact on many firm outcomes in several industries.

8. Robustness Checks

In this section I will present the results for several robustness checks. First, I start with those previously mentioned: spillovers, US-migration, and cartel unobservable differences. Spillovers might occur because cartels expanded to neighboring municipalities as a response to decapitation. This would create a bias against finding statistically significant results. In

⁵⁵ Another possibility is that tourists change plans and travel to safer destinations within Mexico. However, alternative specifications where other businesses in the same cartel localities are used as counterfactual, show that this effect is not driven by an increase in tourism in non-cartel areas.

Table 12, panel E, I eliminated municipalities that gained a cartel after a leader capture from my sample and estimated equation (1) on the logarithm of hourly wages, proportion of workers in unpaid work and the logarithm of the murder rate (columns 1, 2 and 3, respectively). I see a decrease in wages of 5.8%, which is larger than the coefficient resulting from using the full sample; however, it is not large enough to be statistically different.

US-migration might be an important mechanism. Therefore, I excluded the municipalities with the largest US crime-induced migration as recounted by Rios (2014),⁵⁶ and estimated equation (1). Results are shown in panel C and show that as the number of municipalities that drove migration is small, results do not change.

To reduce bias driven by differences in unobservable cartel characteristics I use as control the municipalities where La Familia-cartel is present (the last cartel to have its kingpin captured) and use as treatment the first cartel to have its kingpin captured (Golfo) and estimate equation (1). I show the results in Panel D. In this last example, the signs are as expected and the magnitude of the coefficients is similar, but as I lose a large number of observations, the result for paid employment is not significant (however, I do not lose statistical significance for other outcomes).

Next, I need to rule out the Great Recession. If areas most economically impacted by the Great Recession are the areas with cartel presence, it would be hard to disentangle each effect. However, it has been tested that the geographic heterogeneity of the Great Recession is different from that of the War on Drugs (Brown et al. 2018). To minimize this concern, I eliminate the municipalities that were hit the hardest by the Great Recession (US-bordering states and municipalities with above average levels of manufacture). Results without US-bordering states are presented in Panel A and Panel F uses the sample without municipalities with above-average level of employment in manufacture.

Then, I would like to see if the analysis is sensitive to selection on observables. I did the following: I regressed several municipality characteristics on cartel presence during the period before the start of the War on Drugs (Table A2), and I identified some characteristics that can predict cartel location before the War on Drugs: proximity to the US, urban areas

⁵⁶ These are the municipalities of Juarez, Culiacan, Tijuana, Chihuahua, Acapulco, Torreon, Guadalajara, Gomez Palacio, Mazatlan and Nogales.

and higher murder rates. Therefore, I included a sample that includes only urban municipalities⁵⁷ and a sample that excludes municipalities that were at the top 25% more violent municipalities at the beginning of my sample, and show the results in panel B and G, respectively. I found results that are not statistically different from those using the full sample. I also excluded those municipalities in the Mexico City Valley Area, as this is the largest metropolitan area in the country and has relatively small cartel presence. However, my results are not sensitive to this change (Panel H).

Finally, in Panel I, I test whether cartel decapitation impacted labor outcomes in municipalities where cartels worked with minimal violence. To do that, I restricted my sample to municipalities where violence never surpassed 2 murders per quarter (similar to pre-war levels). Results show that even in municipalities where cartels worked with minimal violence there was a significant impact on wages.

After the previous tests, I can show that no matter the sample selected, my results are robust. I also perform additional robustness checks that I provide in the appendix. First, to validate the results from ENOE, I replicate my results using data from the IPUMS 10% Sample of the Population Census (Table B1). I also estimate multiple hypothesis testing to control for type 1 error⁵⁸ (Table B2) and use alternative definitions to some outcomes of interest to test whether my results are sensitive to the definitions being used (Table B3). I eliminate each cartel individually from the analysis to rule out the possibility that a single cartel is driving my results (Table B4). Then, I test if there was a change in local characteristics (household demographics, for example) on cartel localities before the War on Drugs started using data from the Population Census (Table B6). I also included different clustering strategies (Table B5), replicate Table 12 using the Economic Census (Table B7) and a Goodman-Bacon Decomposition (Figure B1). Finally, I also include a detailed discussion on the impact of military and police operations on Appendix C. The results from these robustness checks show that my results are robust and not sensitive to a particular dataset, control group, outcome definition, clustering strategy, the exclusion of a particular cartel, and that the probability of type 1 error is of limited concern.

⁵⁷ Those with at least 20,000 individuals.

⁵⁸ Particularly, I estimate the p-values according to Benjamini and Hochberg (1995) and Benjamini et al. (2006).

9. Mechanisms

There are large data limitations that hinder the study of possible underlying mechanisms, and, at this moment, there is no study that offers a clear explanation on how violence affected the Mexican labor market. Velasquez (2019) suggested that because of an increase in fear of victimization and lower opportunity costs, women were more likely to reduce their labor market participation. However, this explanation is, at best, suggestive: it might be that women that are more likely to take actions to reduce their crime exposure are those living in regions where other mechanisms are taking place too. For example, it might be that women who are more likely to report feeling fear during the day are those that live in communities where firm extortion increased the most. Moreover, my analysis shows that this phenomenon did not affect each gender differently: both men and women of similar age groups respond similarly employment and wage-wise (Appendix E, Table E4).⁵⁹

To test whether workers were leaving paid employment because of fear, I did the following: I study heterogeneity by industry (Table E5). This is because by the nature of shootings, some industries are more exposed (like transportation). However, my results show that the industries that were the most affected (manufacture, health and construction) were not necessarily those most exposed. Also, I found no wage risk-premium in any industry. This does not mean that there is no fear mechanism, but that I did not find any statistically significant evidence of it.

A second possible mechanism is a drop in consumption for local goods and services: it might be that workers are still willing to go to work, but they will be hesitant to go out and buy goods as often as they did (dropping firms' revenue).⁶⁰ There is evidence that could be pointing towards this mechanism: the number of restaurants and retail establishments decreased, as the revenue of the surviving establishments. However, new evidence from Gorrin et al. (2019) find that exporting manufacturing firms (which do not rely as much on local markets) also saw their exports reduced. This means that if a drop in local consumption is a driving mechanism, it is not the only one at play.

⁵⁹ A possible cause of the difference between my results and Velasquez (2019) is the MxFLS smaller sample size.

⁶⁰ This might also include cartel spending on legal firms.

A third mechanism is an increase in firms' non-production costs, like robberies and extortion (which are not reported in the Census). However, this is not possible to test with available data: The only information on firm victimization started to be surveyed in 2012 (National Firm Victimization Survey, ENVE) and thus, I cannot have a pre-period. However, there is ample journalistic evidence in Mexico that suggest that extortion and robberies could be driving my results.⁶¹ In fact, from the little existing evidence we know that the most affected are smaller firms (as they cannot pay the bribes; ENVE), the fees are large (the owner of a small firm would need to save 7 months of their salaries to pay for the average cartel fee, ENVE) and the number of attacks against businesses has been rising since 2008.⁶²

Even with these limitations, I can rule out two possible mechanisms: 1) cartels and military recruitment and 2) money laundering. The possibility of cartels increasing recruitment as a main driver of my results is not likely as there are too few cartel members (est. 10,000 hitmen; Rios, 2013), which is too small to explain my results. Military recruitment is also unlikely to be driving my results because military men will not appear in my data.⁶³ About money laundering, the Intelligence Unit of the Ministry of Finance (in charge of identification of illegal business operations) states that the profile of businesses that cartels create to money launder are different from those that are present in the Economic Census, as they tend to not have a physical establishment nor any workers beyond the owner.⁶⁴

10. Conclusions

These results show that the capture of a cartel leader had costs beyond public spending in law enforcement and the increased death-toll from conflict. Capturing kingpins has a negative effect on wages and paid employment. Moreover, it decreases the average quality of jobs available by pushing workers into informality. As entrants and the less educated are the most impacted, this has implications for income inequality.

⁶¹ Empresarios de Juárez buscan deducir cuotas que entregan al narco. Animal Político. <https://www.animalpolitico.com/2011/05/empresarios-de-juarez-buscan-deducir-cuotas-que-entregan-al-narco/>

⁶² Pagas o Fuego. El Universal. <https://interactivo.eluniversal.com.mx/incendios-crimen/> Only data at the national level exists.

⁶³ And because military wages actually increased (Desercion militar cae 93.7% en 7 años por mejora salarial. El financiero. <https://www.elfinanciero.com.mx/nacional/desercion-militar-cae-93-en-anos-por-mejora-salarial>.

⁶⁴ Las empresas fantasmas de los carteles mexicanos para lavar el dinero. El Universal. <https://www.eluniversal.com.mx/nacion/las-empresas-fantasma-de-los-carteles-mexicanos-para-lavar-dinero>.

I also find that a cartel leaders' capture caused firm destruction across a wide series of industries, and caused a reduction in revenue, production, the number of workers and wages paid even among surviving firms. This stands out because surviving firms should be profiting from the destruction of some competitors and still had a worse economic performance.

Even though the results found in this paper show that captures of cartel leaders result in negative outcomes for local labor markets, these results are not necessarily representative of impacts long term (the period of study is only from 2005 to 2011). Further research is needed regarding this last point. Moreover, leaving organized crime unchecked could have adverse effects on its own: strong institutions and upholding the rule of law are desired outcomes by themselves. Thus, in designing policies to combat organized crime, the government faces a difficult trade-off: on the one hand, a direct and strong intervention against these organizations could lead to significant increases in violence and adverse effects on wages and employment; on the other hand, allowing criminal syndicates to increase in size and strength without opposition could lead to a weakening of the rule of law. As a result, the design of policies to deal with organized crime must strike an optimal balance that consider possible benefits and costs (in both the short and long term) towards the weakening of criminal organizations.

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Tables

TABLE 1: FIRST CAPTURE OF CARTEL LEADERS

Cartel	Name	Position	Date	Municipalities with Cartel	% of Population
Sinaloa-Beltran-Leyva	Alfredo Beltran Leyva	Leader	1/21/2008	83	0.19
Tijuana	Eduardo Arellano Felix	Leader	10/25/2008	31	0.1
Gulf	Juan Carlos de la Cruz Reyna	Lieutenant	8/29/2007	118	0.18
Juarez	Pedro Sanchez Arras	Lieutenant	5/13/2008	31	0.09
La Familia	Alberto Espinoza Barron	Lieutenant	12/29/2008	22	0.02

Sources: Information of first captures is based on a compendium of press releases of the Army (SEDENA), the Navy (SEMAR), and the Office of the Attorney General (PGR). The proportion of the population is estimated based on population counts from the National Population Council (CONAPO) and El Colegio de Mexico (COLMEX). This table was taken from Lindo and Padilla-Romo (2018).

TABLE 2: EFFECT OF CARTEL LEADERS' CAPTURE ON WORKING POPULATION SIZE

	ln(Total) (1)	ln(Men) (2)	ln(Women) (3)	ln(>30 years old) (4)	ln(<30 years old) (5)	ln(College) (6)
Cartel x Post	0.003 (0.010)	0.005 (0.01)	0.002 (0.011)	0.008 (0.011)	-0.000 (0.012)	-0.014 (0.038)
Municipalities	757	757	757	757	757	757

Sources: This table was estimated using the 10% IPUMS sample of the Mexican Population Census. Areas of Cartel operation are based on Coscia and Rios (2017) as described in the text.

Notes: All regressions include time and municipality fixed effects. Every regression is clustered at the municipality (standard errors in parenthesis). Regressions are estimated using the *reghdfe* Stata command (Correia 2015). This table was estimated using only the municipalities with the same ID as those included in ENOE.

TABLE 3: EFFECT OF CARTEL LEADERS' CAPTURE ON LABOR FORCE COMPOSITION

	Paid Employment (1)	Unpaid Employment (2)	Unemployment Rate (3)	Not in Labor Force (4)
Cartel x Post	-0.907* (0.49)	1.218** (0.48)	0.037 (0.13)	-0.135 (0.33)
Mean Outcome	75.88	15.6	4.24	41.12
Municipalities	821	821	821	821

Notes: All regressions include state-by-year-by-quarter and municipality fixed effects. Every regression has municipality (in parenthesis) clustered standard errors. Regressions are estimated using the *reghdfe* Stata command (Correia 2015) on municipality level collapsed data. Cartel refers to treated municipalities and Post refers to the period after each kingpin capture. The mean outcome refers to the value of the dependent variable in cartel regions during 2007, first quarter.

Definitions: Paid employment refers to the percentage of the labor force that reports a wage and working hours. Unpaid Employment refers to the percentage of the labor force that does not report any wages, while still reports working hours. Unemployment rate is the percentage of the labor force that does not have a job but is looking for one. NILF refers to the proportion of the population that is not in the labor force; that is, does not have a job and is not looking for one.

Sources: Labor outcomes are calculated using data from ENOE, while areas of Cartel areas of operation are based on Coscia and Rios (2017) as described in the text.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level

TABLE 4: EFFECT OF CARTEL LEADERS' CAPTURE ON WAGES AND WORKING HOURS

	ln(Av. Weekly Worked Hours) (1)	ln(Av. Hourly Wage) (2)	ln(Av. Monthly Wage) (3)	ln(Av. Monthly Wage if Paid) (4)
Cartel x Post	0.008 (0.024)	-0.053*** (0.014)	-0.042*** (0.015)	-0.034*** (0.012)
Mean Outcome	40.6 hours weekly	22.06 MXN	3,853 MXN	4,828.3 MXN
Municipalities	821	821	821	821

Notes: See Notes from Table 3. The mean outcome is the value of the dep. variable, with no log-transformation, in cartel regions during 2007, first quarter.

Definitions: Worked Hours refers at the average weekly working hours in the municipality. Average hourly wage refers to the sum of all nominal wages in the municipality, over the sum of all working hours. Average monthly wage refers to the sum of all wages, over the sum of all workers. Average Monthly wage if paid refers to the sum of all wages, over the sum of all individuals reporting working hours and being paid. All wages are nominal, not real wages.

Sources: Labor outcomes are calculated using data from ENOE, while areas of Cartel operation for each cartel are based on Coscia and Rios (2017) as described in the text.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE 5: EFFECT OF CARTEL LEADERS' CAPTURE ON NON-PAID ACTIVITIES

	Only in Workforce (1)	Also study (2)	Also Household Chores (3)	Other non-paid activities (4)
Cartel x Post	-1.17** (0.42)	-0.003 (0.11)	1.21*** (0.46)	-0.03 (0.14)
Mean Outcome	29.65	3.77	63.73	2.84
Municipalities	821	821	821	821

Notes: See notes from Table 3.

Definitions: "Only in workforce" refers at the percentage of the labor force that primarily uses all its working time for paid activities. "Also studies" refers to the percentage that uses a proportion of their time to study, while "also household chores" refers to those that also report household work (cleaning, cooking, taking care of children). Other non-paid activities refer to helping with community programs, taking care of extended community members, etc. The data does not let you see the amount of time spent on each activity.

Sources: Labor outcomes are calculated using data from ENOE, while areas of Cartel areas of operation are based on Coscia and Rios (2017) as described in the text.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level

TABLE 6: EFFECT OF CARTEL LEADERS' CAPTURE ON LABOR OUTCOMES, BY TYPE OF EMPLOYMENT

	% Workers (1)	ln(Av. Hourly Wages) (2)
Panel A: Formal Firms		
Cartel x Post	-0.001 (0.004)	-0.038 (0.069)
Mean	0.217	101.64
Panel B: Informal Firms		
Cartel x Post	0.010** (0.005)	-0.065*** (0.018)
Mean	0.294	92.45
Panel C: Domestic Workers		
Cartel x Post	-0.004 (0.005)	-0.026* (0.032)
Mean	0.29	79.56
Panel D: Public Sector		
Cartel x Post	-0.004 (0.002)	0.016 (0.092)
Mean	0.113	179.19
Municipalities	821	821

Notes: See Notes from Table 3.

Definitions: "% Workers" refers to the percentage of workers (paid and unpaid) that have a job in the category stated in each Panel. ln(Av. Hourly Wage) refers to the logarithm of the sum of all wages over the sum of all hours, for the category stated in each panel.

Sources: Labor outcomes are calculated using data from ENOE, while areas of Cartel areas of operation are based on Coscia and Rios (2017) as described in the text.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level

TABLE 7: EFFECT OF CARTEL LEADERS' CAPTURE ON LABOR OUTCOMES, BY EDUCATION LEVEL

	Labor Force Participation (1)	Paid Employment (2)	ln(Average Hourly Wages) (3)
Panel A: Less than High School			
Cartel x Post	3.38 (2.33)	-0.55 (0.583)	-0.041** (0.016)
Mean Outcome	67.65	78.99	70.79
Panel B: High School			
Cartel x Post	0.685 (0.558)	-0.56 (0.621)	0.0003 (0.041)
Mean Outcome	59.03	75.16	85.34
Panel C: College			
Cartel x Post	0.621 (1.06)	-0.43 (0.99)	0.063 (0.119)
Mean Outcome	68.99	73.59	172.59
Municipalities	821	821	821

Notes: See Notes from Table 3.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE 8: EFFECT OF CARTEL LEADERS' CAPTURE ON THE NUMBER OF FIRMS

	ln(Firm) (1)	ln(Firm/Pop) (2)	Firm (3)	ln(Firm) (4)	ln(Firm/Pop) (5)	ln(Firm) (6)	Firm (7)
Cartel x 2004						-0.01 (0.012)	-0.008 (0.02)
Cartel x 2009	-0.034** (0.013)	-0.037** (0.01)	-0.028** (0.016)	-0.035** (.016)	-0.07** (0.013)	-0.043** (0.02)	-0.048** (0.019)
Cartel x 2014	-0.075*** (0.020)	-0.07*** (0.016)	-0.067** (0.016)	-0.042* (0.02)	-.054** (0.017)	-0.049* (0.027)	-0.087*** (0.018)
OLS	X	X		X	X	X	
Neg Binomial			X				X
State-by-Year FE				X	X	X	
Municipalities	437	437	437	437	437	437	437

Notes: All regressions include time and municipality FE. Municipality standard errors in parenthesis. Municipalities with less than 40,000 individuals in 2004 are excluded.

Sources: Economic Census and Coscia and Rios (2017).

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE 9: EFFECT OF CARTEL LEADERS' CAPTURE ON THE NUMBER OF FIRMS, HETEROGENEITY BY TYPE OF FIRM

	ln(Retail) (1)	ln(Manuf.) (2)	ln(Wholesale) (3)	ln(Restaurants) (4)	ln(Hotels) (5)
Cartel x 2004	-0.007 (0.02)	0.011 (0.021)	0.069 (0.039)	-0.010 (0.02)	-0.009 (0.039)
Cartel x 2009	-0.053** (0.021)	-0.051* (0.025)	-0.047 (0.035)	-0.055** (0.026)	-0.004 (0.037)
Cartel x 2014	-0.067** (0.026)	-0.075** (0.031)	-0.021 (0.043)	-0.041* (0.024)	-0.026 (0.041)
	ln(Less than 5 years old) (6)	ln(Less than 5 workers) (7)	ln(5-10 workers) (8)	ln(Firm Creation) (9)	ln(Not Chain) (10)
Cartel x 2004	-0.01 (0.015)	-0.01 (0.03)	-0.01 (0.026)	0.02 (0.04)	- -
Cartel x 2009	-0.055** (0.024)	-0.027* (0.015)	-0.05 (0.035)	-0.06** (0.026)	-0.028** (0.014)
Cartel x 2014	-0.056** (0.031)	-0.032** (0.011)	-0.04 (0.040)	-0.067** (0.031)	-0.040* (0.021)
Municipalities	437	437	437	437	437

Notes: See notes in Table 8. All regressions include year and municipality FE. Firm Creation is defined as the number of firms of 1 years or less of age. There is no data for single owner firms (not chain) in 1999, so the base year is 2004 for this outcome. ***p<0.01, **p<0.05, *p<0.1

TABLE 10: SURVIVAL OF FIRMS THAT ARE AT LEAST 9 YEARS OLD AT THE BEGINNING OF THE WAR ON DRUGS

	ln(Firms) (1)	ln(Retail) (2)	ln(Manuf) (3)	ln(Wholesale) (4)	ln(Restaurants) (5)
Cartel x 2009	-0.006 (0.016)	0.007 (0.016)	-0.01 (0.033)	0.08* (0.041)	-0.010 (0.025)
Cartel x 2014	0.0021 (0.020)	-0.003 (0.018)	-0.01 (0.031)	0.08* (0.042)	0.011 (0.030)
Municipalities	437	437	437	437	437

Notes: See Table 8. The outcomes of interest should be interpreted as the natural logarithm of the number of firms.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE 11: EFFECT OF CARTEL LEADERS' CAPTURE ON FIRMS' OUTCOMES, PANEL FIRMS

Panel A: All Firms					
	ln(Workers) (1)	ln(Wages) (2)	ln(Rev) (3)	ln(Costs) (4)	ln(Prod) (5)
Cartel x 2004	0.002 (0.004)	0.028 (0.018)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.013)
Cartel x 2009	-0.013** (0.005)	-0.045** (0.021)	-0.04** (0.015)	-0.034** (0.016)	-0.042** (0.16)
Cartel x 2014	-0.007* (0.004)	-0.040** (0.018)	-0.028** (0.015)	-0.014 (0.014)	-0.034** (0.015)
Firms	429,537	429,537	429,537	429,537	429,537
Panel B: Retail					
Cartel x 2004	0.001 (0.004)	0.00 (0.001)	-0.01 (0.017)	-0.01 (0.02)	-0.018 (0.023)
Cartel x 2009	-0.014** (0.005)	-0.045** (0.019)	-0.04* (0.025)	-0.03 (0.023)	-0.063** (0.03)
Cartel x 2014	-0.004 (0.004)	-0.041** (0.016)	-0.037* (0.020)	-0.02 (0.019)	-0.04** (0.02)
Firms	203,991	203,991	203,991	203,991	203,991
Panel C: Manufacture					
Cartel x 2004	0.005 (0.009)	0.01 (0.03)	0.011 (0.026)	-0.02 (0.04)	-0.015 (0.026)
Cartel x 2009	-0.005 (0.011)	-0.059** (0.025)	-0.04 (0.03)	-0.03 (0.03)	-0.049** (0.02)
Cartel x 2014	-0.003 (0.011)	-0.015 (0.04)	-0.02 (0.03)	0.001 (0.029)	-0.028 (0.031)
Firms	35,954	35,954	35,954	35,954	35,954
Panel D: Restaurants					
Cartel x 2004	0.015 (0.009)	0.0009 (0.039)	-0.01 (0.02)	-0.007 (0.018)	-0.01 (0.02)
Cartel x 2009	-0.018 (0.012)	-0.08* (0.04)	-0.052** (0.021)	-0.05** (0.02)	-0.063** (0.02)
Cartel x 2014	-0.012 (0.011)	-0.03** (0.01)	-0.024** (0.011)	-0.02 (0.02)	-0.035** (0.013)
Firms	18,291	18,291	18,291	18,291	18,291
Panel E: Hotels					
Cartel x 2004	-0.0005 (0.044)	-0.009 (0.065)	0.001 (0.002)	0.002 (0.003)	0.001 (0.002)
Cartel x 2009	-0.03 (0.046)	-0.11 (0.078)	-0.05 (0.037)	-0.08** (0.04)	-0.04 (0.03)
Cartel x 2014	-0.032 (0.05)	-0.169* (0.101)	-0.135** (0.05)	-0.14** (0.048)	-0.12** (0.05)
Firms	4,383	4,383	4,383	4,383	4,383

Notes: See Table 8. All regressions include year and firm fixed effects. Regressions are estimated using the reghdfe Stata command (Correia, 2015) on firm level data. ***p<0.01, **p<0.05, *p<0.1

Definitions: Workers refer to the number of workers hired in the year before the Census Wave. Wages refer to all wages paid (nominal) during that year. Cost refers to production costs only. Production refers to the production value as reported by the firm.

Source: Economic Census and Coscia and Rios (2017).

TABLE 12: EFFECT OF CARTEL LEADERS' CAPTURE ON LABOR OUTCOMES, DIFFERENT SUBSAMPLES

	ln(Average Hourly Wage) (1)	Unpaid Work (2)	ln(Murder Rate) (3)		ln(Average Hourly Wage) (1)	Unpaid Work (2)	ln(Murder Rate) (3)
Panel A: Not US-bordering States				Panel E: No Municipalities conquered by cartels after the War on			
Cartel x Post	-0.057*** (0.017)	1.344** (0.55)	0.133*** (0.032)	Cartel x Post	-0.058*** (0.017)	1.232** (0.554)	0.148*** (0.032)
Mun.	714	714	714	Mun.	652	652	652
Panel B: No Rural				Panel F: No municipalities with high proportion of workers in			
Cartel x Post	-0.041*** (0.016)	1.012* (0.534)	0.071** (0.033)	Cartel x Post	-0.058*** (0.020)	1.12* (0.64)	0.099*** (0.04)
Mun.	606	606	606	Mun.	484	484	484
Panel C: No municipalities with high-US migration				Panel G: Restricted by baseline murder rate.			
Cartel x Post	-0.052*** (0.014)	1.170** (0.490)	0.095*** (0.030)	Cartel x Post	-0.053** (0.014)	1.17*** (0.51)	0.093** (0.032)
Mun.	811	811	811	Mun.	614	614	
Panel D: Only Golfo or Familia cartel				Panel H: Excluding Mexico Valley Area.			
Cartel x Post	-0.047** (0.019)	1.05 (0.887)	0.111** (0.048)	Cartel x Post	-0.060*** (0.015)	1.58*** (0.49)	0.106*** (0.033)
Mun.	139	139	139	Mun.	736	736	736
Panel J: More than 100,000 inhabitants				Panel I: Excluding Municipalities with high levels of violence			
Cartel x Post	-0.037** (0.011)	1.11*** (0.487)	0.091** (0.048)	Cartel x Post	-0.03*** (0.014)	0.80 (0.55)	
Mun.	151	151	151	Mun.	494	494	

Notes: See notes from Table 3. US-Bordering States are Chihuahua, Sonora, Baja California, Coahuila, Nuevo Leon and Tamaulipas. Urban are those municipalities with more than 20,000 inhabitants. Low Manufacture are those municipalities where the proportion of workers in manufacture is less than 16% during the first quarter of the 2005 ENOE. No large US migration: Juarez, Culiacan, Tijuana, Chihuahua, Acapulco, Torreon, Guadalajara, Gomez Palacio, Mazatlan and Nogales. The Mexico City Valley Area includes Mexico City, the State of Mexico and the municipality of Tizayuca. The sample that restricts by the murder rate at the baseline, excludes the municipalities that during the first quarter of 2005, were the top 25% most violent municipalities.

Sources: Labor outcomes are calculated using data from ENOE, while areas of Cartel operation for each cartel are based on Coscia and Rios (2017) as described in the text. Panel I refers to municipalities where the quarterly murder rate never reach 3 murders per 100,000.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Figures

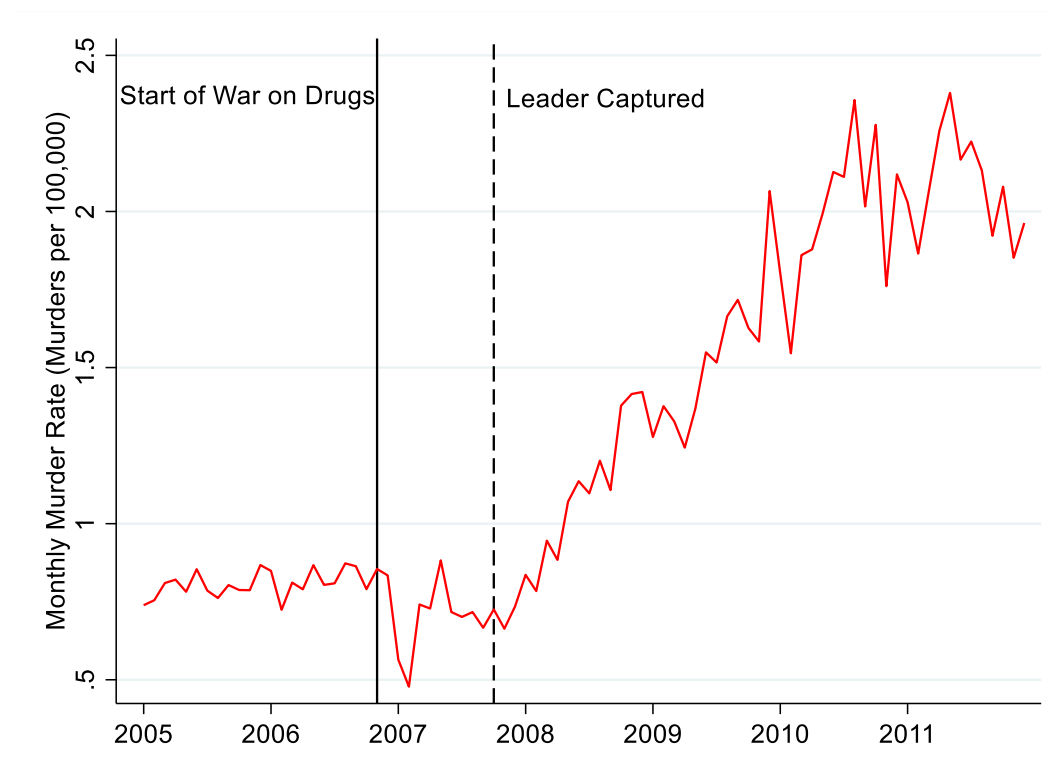


FIGURE 1
NATIONAL HOMICIDE RATE, HOMICIDES PER 100,000

Notes: Vertical lines are drawn to highlight the beginning of the War on Drugs and the first capture of a cartel during the War on Drugs. Homicide rates are calculated based on the universe of death certificates from the vital statistics of the National Institute of Statistics and Geography (INEGI) and population counts from the National Council of Population (CONAPO).

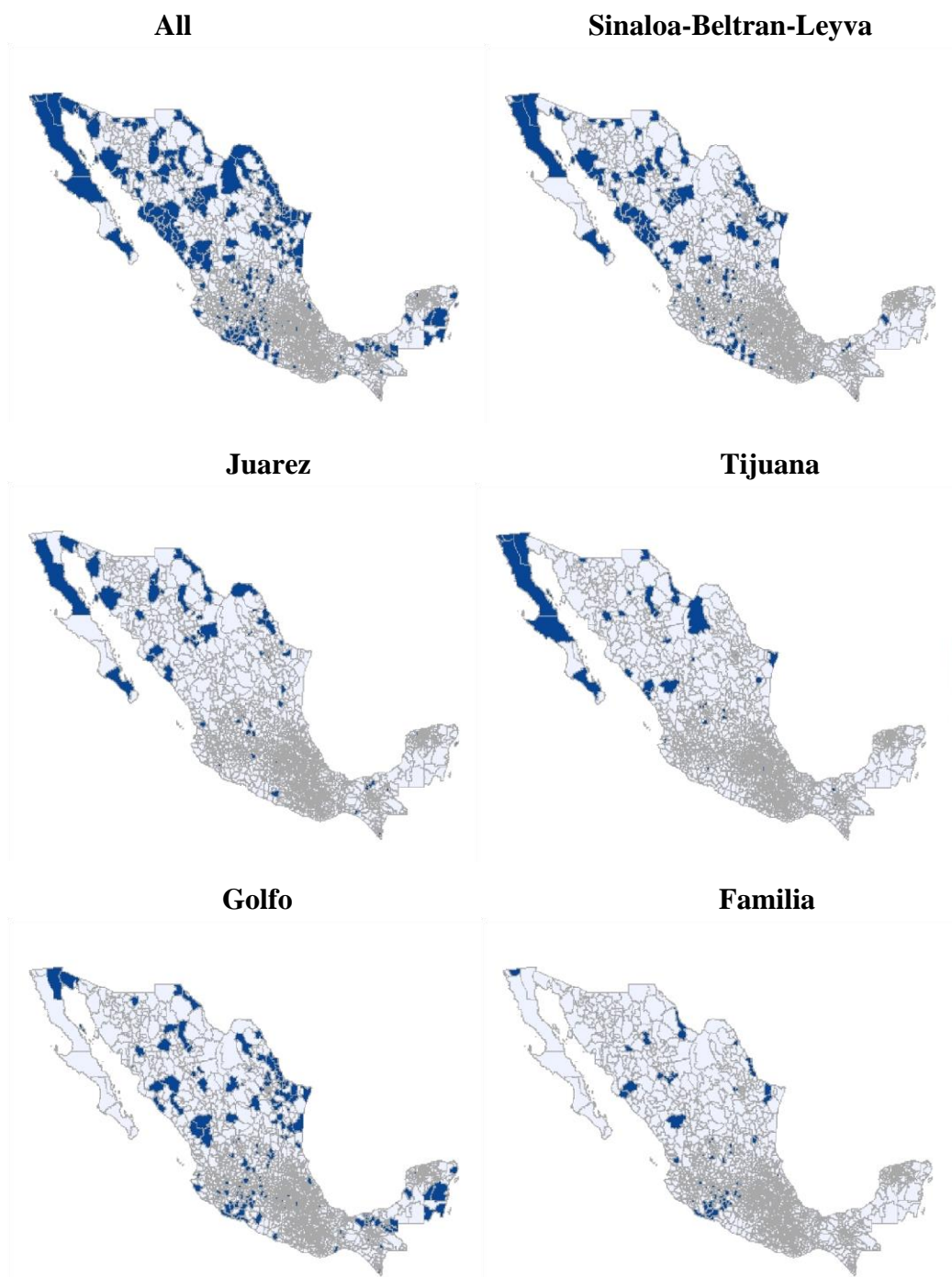


FIGURE 2
CARTEL PRESENCE BY MUNICIPALITY

Notes: Each panel shows the municipalities where a specific cartel had presence between 2004 and 2006.
Source: Coscia and Rios (2017).

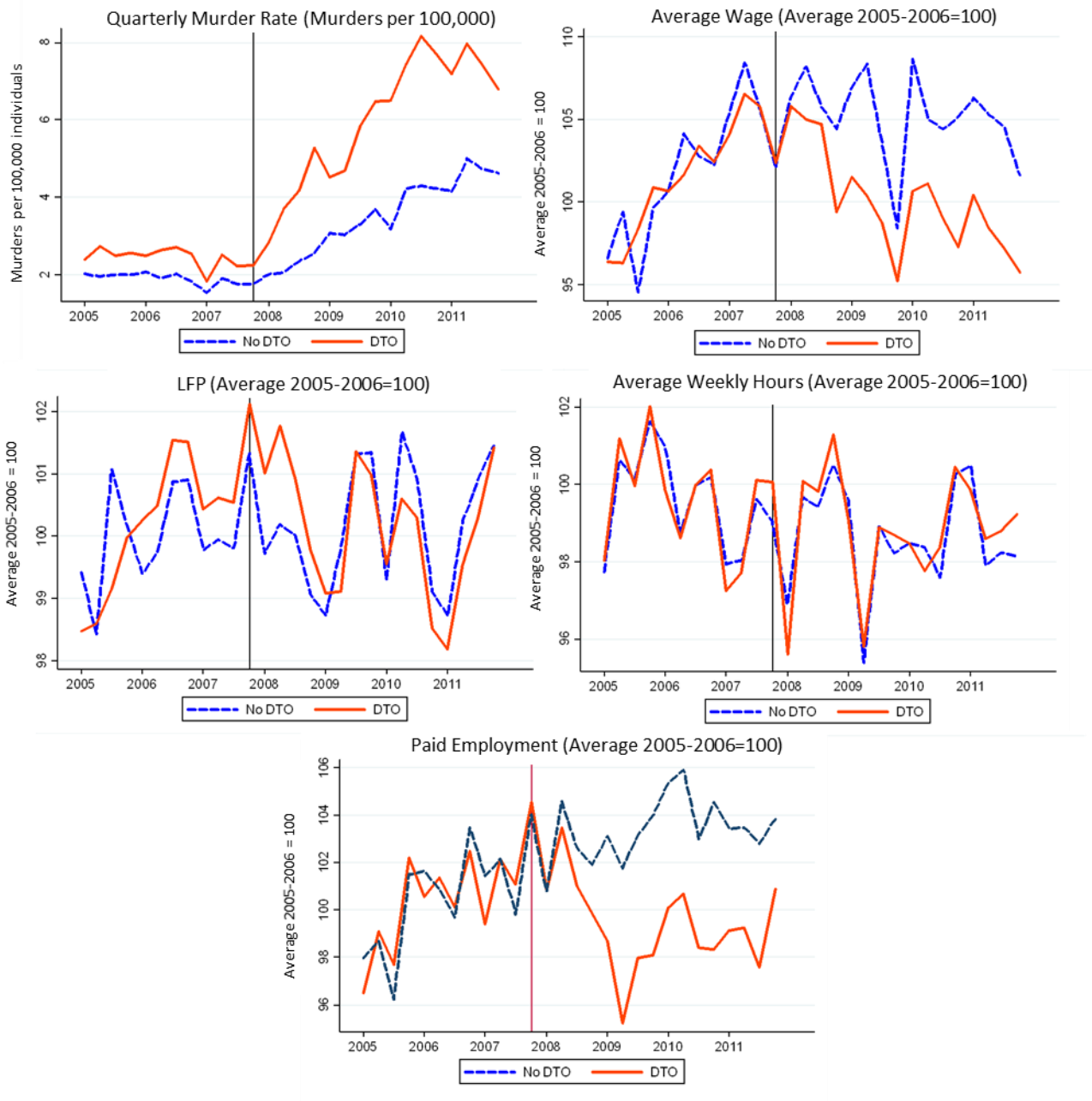


FIGURE 3

LABOR OUTCOMES IN MUNICIPALITIES WITH CARTEL PRESENCE AND MUNICIPALITIES WITHOUT CARTEL PRESENCE

Notes: The vertical lines are drawn to highlight the first capture of a cartel leader during the War on Drugs. These graphs were made using data from ENOE and Coscia and Rios (2017) and each time series was standardized equating their average values during the pre-treatment period (2005 to 2006) equal to 100 (except for the murder rate, which is expressed as the number of murders per 100,000 individuals).

Sources: Coscia and Rios (2017) and ENOE.

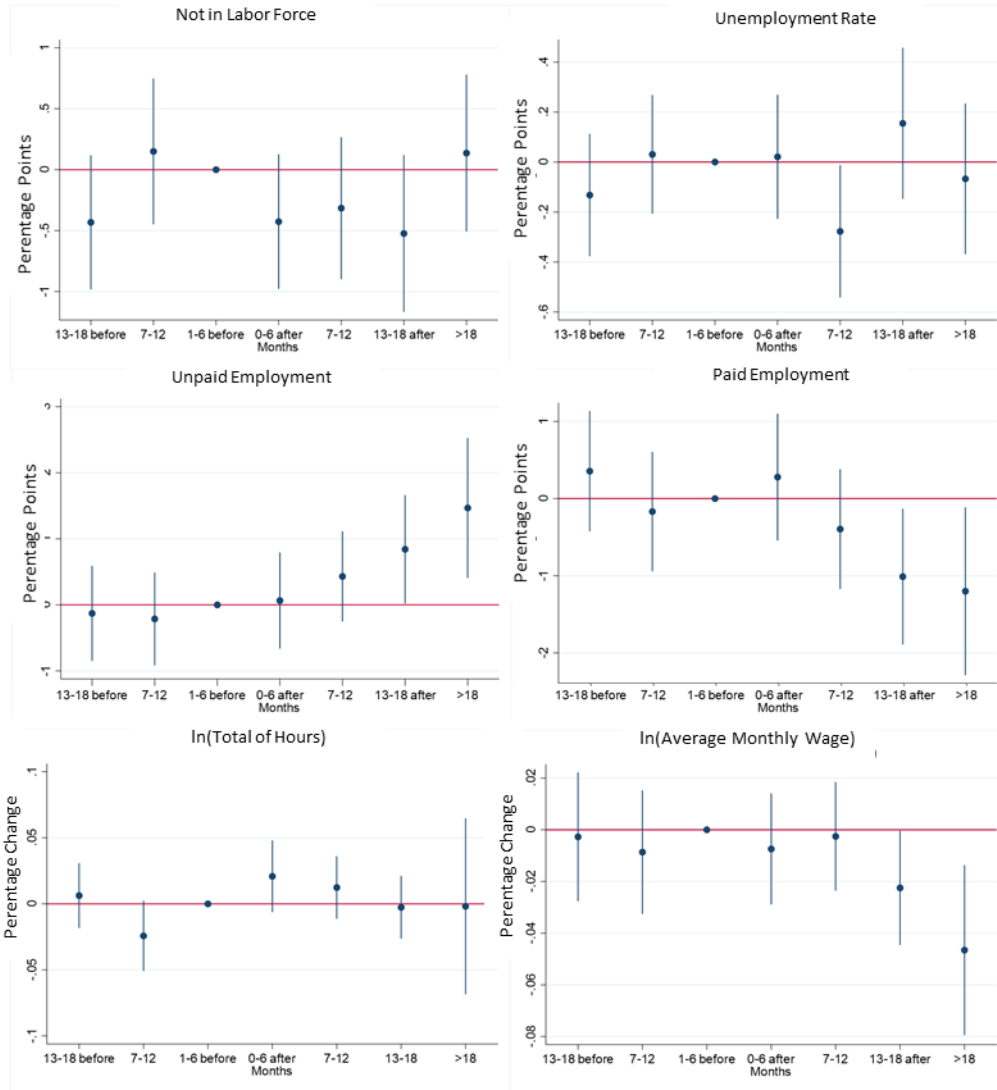


FIGURE 4

EFFECT OF CARTEL LEADERS' CAPTURE ON LABOR OUTCOMES, PARALLEL TREND TEST

Notes: All regressions also include State-by-Year-by-Quarter-Fixed Effects. The omitted period is the semester before the capture. Standard errors are clustered at the municipality level. Regressions were estimated using the *reghdfe* Stata command (Correia 2015) on municipality level collapsed data. Standard errors show a 95% confidence interval around the point estimates (shown as a blue dot).

Sources: Coscia and Rios (2017) and ENOE.

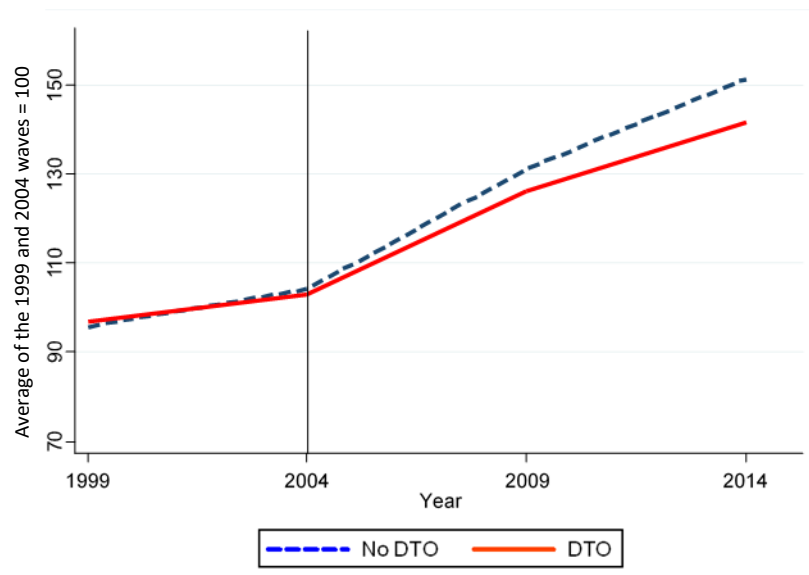


FIGURE 5

NUMBER OF FIRMS IN MUNICIPALITIES WITH AND WITHOUT CARTEL PRESENCE (AVERAGE OF 1999 AND 2004 WAVES=100)

Notes: The vertical line is drawn to highlight the last Census wave before the capture of a cartel leader during the war on drugs.

Sources: Economic Census and Coscia and Rios (2017).

Appendix

Appendix A. Summary Statistics

In this appendix I added different tables that have summary statistics for the different datasets used in this paper. In particular: 1) Table A1 shows a summary statistics of the average individual in cartel (or no cartel) areas using data from ENOE, 2) Table A2 shows a correlation between cartel presence and several municipality characteristics, 3) Table A3 shows a summary statistics of the Economic Census data by cartel area, 4) Table A4 shows a summary statistics for IMSS administrative data, 6) Table A5 compares some descriptive statistics from ENOE, IPUMS and the Population Census. Finally, Figure A1 shows which municipalities with cartel presence could not be studied because they are not present in ENOE. In a sense, these tables and graph show which observable characteristics are similar and which ones are different among cartel municipalities and the rest. Identifying these key differences is a key element to create a robust identification strategy and develop the appropriate robustness tests.

Overall, I find that cartel municipalities tend to be in urban areas and are also closer to the border (Table A2). This can explain that firms in cartel areas tend to be slightly larger than firms located in non-cartel areas. These differences were taken into consideration in the Robustness tests used in Section 8.

TABLE A1: SUMMARY STATISTICS

Variable	Total	
	Cartel	No Cartel
From Total Population		
Proportion Men	0.47	0.47
Proportion Age<25	0.27	0.28
Proportion Age<45 and Age>25	0.43	0.42
Proportion Age>66	0.24	0.23
From those with a job		
Proportion Retail	0.18	0.17
Proportion No Establishment	0.21	0.23
Proportion High School	0.43	0.38
Monthly Income	3951.81	2798.52
Weekly Working Hours	41.3	40.68
Total Sample Size (individuals)	182,646	87,870
<u>Municipalities</u>	<u>210</u>	<u>611</u>

Sources: This summary statistics was created using individual level data of the first quarter of ENOE (2005 I). Cartel Location was identified using data from Coscia and Rios (2017)

TABLE A2: CORRELATION BETWEEN CARTEL PRESENCE AND MUNICIPALITIES CHARACTERISTICS

	2005-2006	2008-2009	2010-2011
% Manuf	-0.004 (0.003)	-0.003 (0.003)	0.000 (0.003)
Border	0.241*** (0.012)	0.241*** (0.012)	0.241*** (0.012)
ln(Population)	1.040*** (0.036)	1.040*** (0.036)	1.040*** (0.036)
Annual Murder Rate	3.558*** (0.353)	3.558*** (0.639)	3.558*** (1.123)
ln(Monthly Wage)	0.430*** (0.014)	0.430*** (0.013)	0.430*** (0.013)
Municipalities	821	821	821

Notes: This table shows the result of regressing the variable of interest (on the left) on a dummy equal to 1 if the municipality had any cartel (controlling for time dummies). This analysis is repeated for the 3 time periods (shown at the head of each column). Clustered standard errors are estimated at the municipality level and shown in parenthesis. See Notes from Table 3.

TABLE A3: FIRMS' SUMMARY STATISTICS

Panel A. Municipality Averages of All Urban Firms in 2004

	Cartel	No Cartel
	Mean	Mean
Number of workers	5.28	4.04
Wages (Thousand MXN)	310.54	256.17
Proportion Manufacture	0.11	0.12
Proportion Retail	0.47	0.51
Proportion Restaurants	0.89	0.92
Municipalities	153	284

Panel B. Panel of Urban Surviving Firms in 2004

	Cartel	No Cartel
	Mean	Mean
Number of workers	9.1	7.8
Wages (Thousand MXN)	569.16	430.8
Proportion Manufacture	0.13	0.11
Proportion Retail	0.70	0.70
Proportion Restaurants	0.07	0.07
Firms	289,258	140,279

Notes: Municipalities with Cartel presence are identified using data from Coscia and Rios (2017) as described in the text. Firm data comes from the Mexican Economic Census. I only included municipalities that in 2004 (according to National Population Counts) had more than 40,000 inhabitants.

TABLE A4: IMSS ADMINISTRATIVE RECORDS SUMMARY STATISTICS (AVERAGE FOR 2006)

	Cartel	No Cartel
	Mean	Mean
Average Daily Wage	190.96	185.4
Proportion Keepers	0.68	0.68
Proportion Entrants	0.13	0.13
Proportion Changers	0.19	0.19
Municipalities	143	265

Notes: Municipalities with DTO presence are identified using data from Coscia and Rios (2017) as described in the text. This table is a summary statistic of the random 3% sample of all municipalities with more than 40,000, who are present in every single period of the dataset. It excludes Mexico City as the dataset has not standard classification for regions within the city.

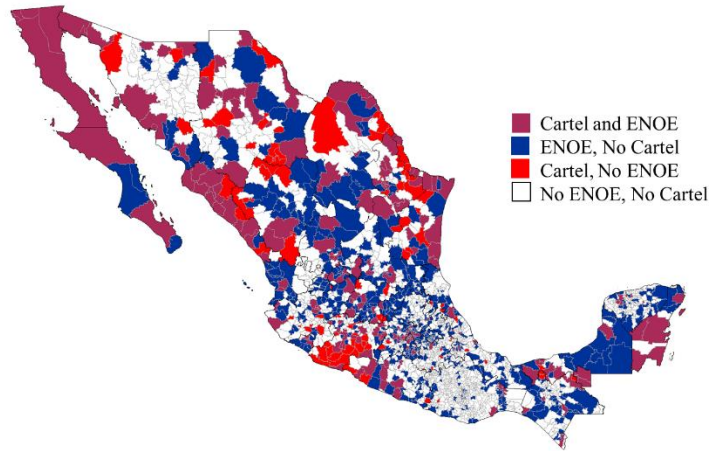


FIGURE A1
ENOE AND CARTEL MUNICIPALITIES
Sources: ENOE and Coscia and Rios (2017)

TABLE A5: COMPARISON OF ENOE, IPUMS AND POPULATION CENSUS

Variable	ENOE		IPUMS		Full Census	
	Cartel	No Cartel	Cartel	No Cartel	Cartel	No Cartel
Percentage Men	48.4	48.3	48.3	48.3	48.2	48.3
Percentage Age<25	28.8	28.7	28.7	28.8	28.8	28.8
Labor Force Participation	59.8	59.6	59.8	59.7	59.9	59.7
Percentage High School or More	41.9	40.9	41.8	41.7	41.8	41.6
Weekly Working Hours	41.3	40.8	41.2	40.9	41	40.8
Monthly Wage (Median)	4,608	4,998	4,748	4,992	4,740	5,011
Municipalities	210	611	210	611	210	611

Sources: ENOE (2010), Full Population Census (2010) and IPUMS Population Sample

Appendix B. Additional Robustness Checks

TABLE B1: CARTEL LEADERS' CAPTURE IMPACT ON LABOR OUTCOMES, USING OTHER DATASETS

	ln(hourly wage) (1)	ln(Hours) (2)	Unemployment Rate (3)	Labor Force Participation (4)
IPUMS: Sample Comparable to ENOE				
Cartel x Post	-0.052 (0.025)	0.003 (0.003)	0.003 (0.003)	-0.002 (0.004)
Municipalities	757	757	757	757
Full Census: ENOE Municipalities				
Cartel x Post	-0.056 (0.023)	0.007 (0.005)	0.028 (0.016)	-0.01 (0.012)
Municipalities	821	821	821	821
Full Census: All Municipalities				
Cartel x Post	-0.053 (0.022)	0.009 (0.005)	0.029 (0.017)	-0.09 (0.011)
Municipalities	2,469	2,469	2,469	2,469
ENOE (Up to 2010 IV)				
Cartel x Post	-0.053 (0.018)	0.009 (0.013)	0.029 (0.016)	-0.009 (0.13)
Municipalities	821	821	821	821

Notes: See Notes from Table 3.

Sources: IPUMS (10% Population Census Sample), ENOE, Full Census and Coscia and Rios (2017).

TABLE B2: MULTIPLE HYPOTHESIS TESTING

	Labor Force Participation (1)	Paid Work (2)	ln(Hours) (3)	ln(Average Wage) (4)	ln(Murder Rate) (5)
P-value Cartel-Combination	0.499	0.020	0.506	0.000	0.002
BH-FDR	0.506	0.034	0.506	0.001	0.005
BKY	0.254	0.036	0.254	0.006	0.011

Notes: The p-values in the first line are those of the cartel-combination clustering (Table 3 with State-by-year-by-quarter fixed effects). BH-FDR p-values were calculated as described by Benjamini and Hochberg (1995). BKY p-values were calculated according to Benjamini et al. (2006).

TABLE B3: CHANGING VARIABLE DEFINITION

	ln (Employed +Unemployed)	ln(Median Municipality Hourly Wage)	ln(Average Paid Hour)	Percentage of Workers being Paid	Quartic Root of Murder Rate
	(1)	(2)	(3)	(4)	(5)
Cartel x Post	-0.005 (0.024)	-0.052*** (0.017)	0.192 (0.013)	-1.283* (0.508)	0.0216** (0.008)
Municipalities	821	821	821	821	821

Notes: Every regression has municipality clustered standard errors (in parenthesis). Regressions are estimated using the *reghdfe* Stata command (Correia, 2015).

Definitions: “Employed + Unemployed” is just the raw level of the labor force size. Median municipality hourly wage is estimated as the median non-zero hourly wages of all workers in the municipality. Average Paid Hour is the average weekly hours for paid workers. Percentage of workers being paid is the percentage of all active workers that receive any wages. See Notes of Table 3. Stars are assigned based on cartel-combination clustering.

TABLE B4: ELIMINATING A SINGLE CARTEL FROM EQUATION (1)

	No Sinaloa (1)	No Tijuana (2)	No Golfo (3)	No Juarez (4)	No Familia (5)
Panel A. Outcome: Unpaid Employment					
Cartel x Post	1.243* (0.584)	1.055** (0.533)	0.933 (0.774)	1.0433* (0.557)	0.84* (0.51)
Panel B. Outcome: Log-Hourly Wage					
Cartel x Post	-0.051*** (0.017)	-0.046*** (0.016)	-0.053*** (0.018)	-0.046*** (0.016)	-0.055*** (0.016)
Municipalities	739	784	700	777	784

Notes: See notes from Table 4.

TABLE B5: EFFECT OF CARTEL LEADERS' CAPTURE ON LABOR OUTCOMES, P-VALUES UNDER DIFFERENT CLUSTERING STRATEGIES

	Labor Force Participation (1)	Unemployed (2)	ln(Hours) (3)	ln(Hourly Wage) (4)	ln(Murder Rate) (5)
Panel A: Full Sample					
Cartel x Post	0.0183	0.281	0.115	-0.47	0.233
P-value: State	0.55	0.152	0.536	0.000	0.001
P-value: Cartel and State	0.38	0.119	0.512	0.012	0.046
P-value: State and Quarter-Year	0.534	0.185	0.514	0.01	0.017
Municipalities	821	821	821	821	821
Panel B: Excluding Municipalities with multiple cartels					
Cartel x Post	0.732	0.183	-0.003	-0.053	0.214
P-value: Only 5 Cartels	0.127	0.74	0.947	0.09	0.11
Municipalities	640	640	640	640	640

Notes: All regressions include time and municipality level fixed effects. From bottom-up, p-value of different clustering strategies in Panel A: 1) Cartel, 2) Cartel and State, 3) State and Quarter-Year. Panel B is different as it excludes municipalities with multiple cartels and thus, has limited number of clusters. (it is clustered at the cartel level). The wildbootstrapping required for each regression was done with the STATA command boottest with 1,000 repetitions, as stated in Roodman (2015).

TABLE B6: EFFECT OF CARTEL LEADERS' CAPTURE ON MUNICIPALITIES PRE-TRENDS

	Secondary Finished (1)	No Water (2)	No Sewage (3)	# Rooms (4)
Cartel x 2005	0.0019 (0.002)	0.002 (0.005)	0.016* (0.010)	-0.012 (0.009)
	Years of Schooling (5)	# Children (6)	Literacy (7)	Indigenous Language (8)
Cartel x 2005	0.0142 (0.019)	0.015* (0.007)	-0.002 (0.002)	0.003 (0.002)
Municipalities	757	757	757	757

Notes: See notes on Table B1. In this particular case, I present the $\hat{\beta}$ estimator when the base year is 2000 and the post period is 2005. Municipality clustered standard errors are presented in parenthesis.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

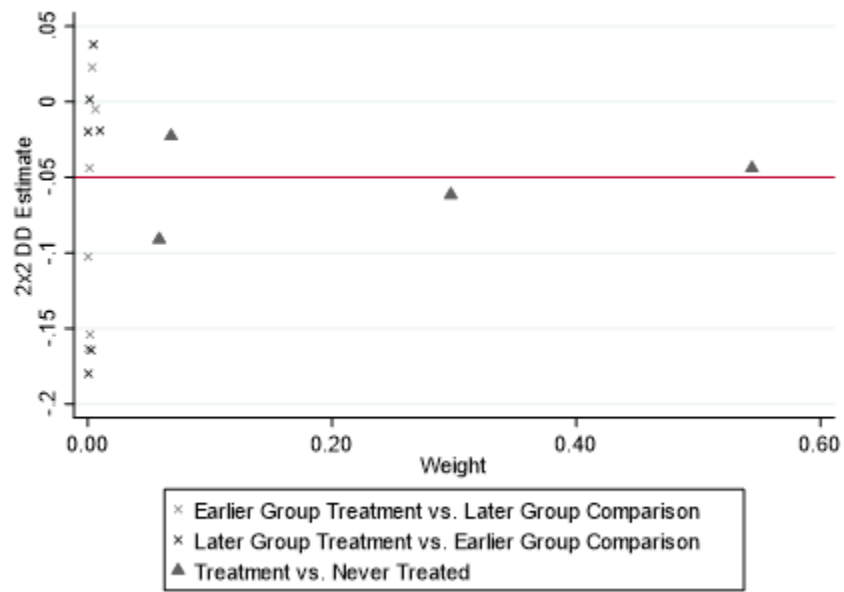


FIGURE B1

GOODMAN-BACON DECOMPOSITION OF THE IMPACT OF CARTEL LEADERS' CAPTURE ON WAGES

Notes: The scatterplot shows the Goodman-Bacon (2018) decomposition of the analysis on wages. The figure plots each of the 2x2 DD components of the Differences-in-Differences analysis. The triangle shows the impact when the treatment group is compared against the never treated, while the x's are the timing-only terms.

Sources: ENOE and Coscia and Rios (2017).

TABLE B7: EFFECT OF CARTEL LEADERS' CAPTURE ON DIFFERENT SUBSAMPLES OF THE ECONOMIC CENSUS

ln(Firms/Population)		ln(Firms/Population)	
Panel A: Not US-Bordering States		Panel E: No Municipalities conquered by cartels after the War on Drugs Started	
Cartel x 2009	-0.037*** (0.013)	Cartel x 2009	-0.043*** (0.020)
Cartel x 2014	-0.07 (0.20)	Cartel x 2014	-0.075*** (0.022)
Municipalities	402	Municipalities	400
Panel B: Only Golfo or Familia		Panel F: No Municipalities with high proportion of workers in manufacture	
Cartel x 2009	-0.051*** (0.016)	Cartel x 2009	-0.03*** (0.011)
Cartel x 2014		Cartel x 2014	-0.062*** (0.03)
Municipalities	139	Municipalities	397
Panel C: No Municipalities with high US-Migration		Panel G: No Municipality with long military operations	
Cartel x 2009	-0.037*** (0.012)	Cartel x 2009	-0.04*** (0.012)
Cartel x 2014	-0.069*** (0.021)	Cartel x 2014	-0.068*** (0.025)
Municipalities	427	Municipalities	389

Notes: See Notes from Table 3. Low Manufacturing are those municipalities where the proportion of workers in manufacture is less than 16% of their workforce. The States with military operations were Michoacan, Guerrero, Chiapas, Chihuahua, San Luis Potosi, Veracruz, Aguascalientes, Nuevo Leon, Tamaulipas and Durango. No Large US migration refers to Juarez, Culiacan, Tijuana, Chihuahua, Acapulco, Torreon, Guadalajara, Gomez Palacios, Mazatlan and Nogales.

Appendix C. The impact of government interventions

The captures studied in this paper coincide with the broader war initiated against the cartels. This war included raids that did not target leadership, and the movement of the army into major cities (Lindo and Padilla-Romo 2018). As some of these actions happened because of captures of cartel leaders (but not always), they could be a mechanism and a confounder. As those operations might have a similar impact as cartel leaders' captures, I need to disentangle both effects as best as possible. In this section I offer additional evidence that suggests that my results are not being driven exclusively by such operations.

First, I will show that my results are robust to controlling for shocks common across all cartel municipalities. For example, it might be that cartels responded to President Calderon's War declaration. This is possible to test because the declaration of war happened during the last month of 2006, nine months before the first cartel leader was captured. To study this possibility, I created a dummy equal to 1 for all cartel municipalities after the declaration of war and included this variable as an additional control. Similarly, I repeated this exercise creating a dummy equal to 1 for all cartels for all periods after the Election of President Calderon. Results are presented in Table C1 and show that the general results hold after the inclusion of such dummies.

A second potential concern with my empirical strategy is that the impact of the capture is intertwined with the impact of State-wide military operations (listed in Table C2). To explore this possibility, Figure C1 graphs each of the eight major State operations (up to 2010). Each panel restricts attention to the state of the operation and plots separately the average wage for municipalities with and without cartel presence. Collectively, these panels indicate that the major operations of the drug war did not precipitate increases in homicides in municipalities with a DTO presence relative to those without a DTO presence.

A third option is that my estimates are reflecting the impact of smaller-scale fights between the military, police and cartels. In order to explore this possibility, I tracked all the reported confrontations between any armed forces (military and police) and criminal organizations in the country between 2006 and 2011 using the CIDE drugs policy program database.⁶⁵ The

⁶⁵ Available at: <http://www.politicadedrogas.org/PPD/index.php/observatorio/descargables.html>

data shows a problem: operations were widely used across the country. However, there were several municipalities where no operation took place until much later into the war. In other words, there are years in between the kingpin capture and the first operation (for some municipalities). Moreover, even in the municipalities where operations took place much earlier, there was time heterogeneity. This allows me to create an event study according to the following regression:

$$(5) \quad \ln(y_{mt}) = \alpha_{st} + \delta_m + \beta(\text{Cartel} \times \text{Capture})_{mt} + \gamma(\text{Cartel} \times \text{Operations})_{mt} + \varepsilon_{mt},$$

where β measures the cartel leaders' capture's impact on cartel areas' wages (relative to non-cartel areas) and γ measures the marginal impact of other operations. If other operations happened too close in time to kingpin captures in cartel municipalities, multicollinearity between captures and other operations would increase, making β to lose statistical significance.

Overall, there are many possibilities on how to measure "other operations" (number of events, events started by the criminal organizations, events started by the government, etc.), but my results are robust to a wide array of definitions (I keep only four for brevity). In Panel A I count all the incidents in the municipality-quarter where there was an event between a government agency and a criminal organization. Panel B counts only those that resulted in at least a death. Panel C includes those that resulted in at least an arrest, and Panel D those that resulted in any seizing (drugs, money, weaponry, etc.). In column 1 I report all operations, and in column 2 those operations between cartels and the military. Column 3 reports the events where other government organisms were involved. Column 4 eliminates the municipalities of capture (as violence increased too quickly in those localities, and operations took place soon afterwards).⁶⁶

My results are shown in Table C3. Overall, my results indicate that operations (whether they are fought by the military or not) have a negative impact on wages. However, the impact is so small that my original analysis on the impact of captures remains robust.

⁶⁶ There are many possibilities on how to define operations. For example, the database lets you know if the event was initiated by a government agency or not, among many other interesting details. In any case, varying the definition of "operation" to include those initiated by the government or to count the number of men involved in the fight, does not change my results. I only report the results using the operation count (by municipality and quarter) for brevity.

The problem with the previous regression is that some operations might be a response to previous operations. Therefore, a different approach may be required. To address these concerns, I create fake treatments (placebos). In other words, I study if the first operation against a particular cartel, which had similar operation-observables (number of killed, detainees, seizures, etc.) to an operation that resulted in a kingpin capture, had the same impact as the kingpin capture.⁶⁷ I vary the definition of fake treatment. In other words, I run the following regression:

$$(6) \ln(y_{mt}) = \alpha_{st} + \delta_m + \beta(\text{Cartel} \times \text{Capture})_{mt} + \gamma(\text{Cartel} \times \text{Fake Capture})_{mt} + \varepsilon_{mt}.$$

Equation (6) is similar in interpretation to (5). Although I vary the definition of fake capture, I keep one constant: a shock will affect only the cartel whose members were affected. For example, if my fake capture is the first operation that resulted in killed cartel members, it will only affect the municipalities where that cartel is present. I also include an additional regression where I only include (cartel and non-cartel) municipalities that did not have any operation until 2010 (restricting the dataset up to 2009). Results are shown in Table C4.

Column 1 studies the impact of the first operation that had more than 7 detainees, while column 2 studies the impact of the first operation that had more than 5 killed (both in the top 90% of all events). Then, in column 3 I study the impact of the first operation in the municipality. Column 4 studies the impact of the first outlier event (by measure of number of killed, detainees or seizures). Finally, column 5 includes only those municipalities that had no operations until 2010 (the regression includes data up to 2009). In all these cases, I see that these fake treatments did have a negative but small and not-statistically significant effect on wages. Moreover, municipalities that had not seen operations⁶⁸ still saw a decrease in wages by 2009.

⁶⁷ The logic behind this regression is the following: there were multiple operations that in paper were similar in observables to those that resulted in a capture; however, these operations did not end up having any impact even in the municipality where the event took place. For example, Pedro Sanchez Arras (Tijuana Cartel) at the moment of his capture lied and gave a fake name. Simultaneously, 6 other cartel members were captured (no deaths), and 4 vehicles and 8 guns were seized by the military. Given observables, this operation is similar to the event that took place in Vicente Guerrero, Durango on May of 2008 where 1 cartel members was detained and 8 were killed, and vehicles and guns were seized by the military. However, there was no similar response on the labor market or the murder rate from this event.

⁶⁸ These operations include even those between cartels and local police.

These results are not surprising given that the timing of these operations is not consistent with the timing of the drop in wages in cartel areas. For example, the first operation with an outlier number of detainees for the Golfo cartel happened two quarters before the capture of its kingpin. However, for a similar event to take place for the Sinaloa Cartel, one would need to wait until the third quarter of 2010 (1 year and 4 months after the drop in wages is statistically significant). Therefore, it does not seem reasonable that the first operation with a large number of detainees triggered a large labor market response.^{69,70}

Finally, I would like to see if each capture had a similar (independently of the resources used in each capture). In order to do that, I run the following regression:

$$(7) \ln(y_{mt}) = \alpha_{st} + \delta_m + \beta(\text{Cartel } A \times \text{Capture } A)_{mt} + \varepsilon_{mt}.$$

In other words, I estimate the impact of each capture independently (by eliminating other cartel areas from the regression).

Results are shown in Table C5. Overall, my results show that all captures had a negative effect. However, there is variation in the magnitude: The captures of the Sinaloa, Juarez and Golfo kingpins had a similar magnitude (negative 4-6%), while the impact of the capture of Tijuana's kingpin was larger and Familia cartel's kingpin was smaller. A possible explanation is that those two cartels have the smallest presence in my sample and thus, are prone to larger estimation errors.

⁶⁹ I can use a similar argument for all definitions of fake treatments used. For example, the first operation with an outlier number of deaths for the Familia Cartel was until the third quarter of 2011 (almost two years after the drop in wages).

⁷⁰ As there is the possibility that there is a mistake in identifying the cartel that fought during the operation, I also used a different definition based on location (a cartel is assumed to fight as long as there is an event in the cartel area of operation). However, my results are robust to such definition.

TABLE C1: EFFECT OF CARTEL LEADERS' CAPTURES ON LABOR OUTCOMES, PLACEBO
ADDITIONAL CONTROLS

	Labor Force Participation (1)	Unpaid or Unemployed (2)	ln(Hours) (3)	ln(Hourly Wage) (4)
Panel A: Regression has additional dummy for War on Drugs				
Cartel x (1-6 months after capture)	0.484* (0.263)	-0.278 (0.351)	0.016 (0.011)	-0.007 (0.011)
Cartel x (7-12 months after capture)	0.245 (0.281)	0.256 (0.32)	0.007 (0.01)	-0.004 (0.01)
Cartel x (13-18 months after capture)	0.346 (0.293)	0.746* (0.363)	-0.005 (0.011)	-0.008 (0.011)
Cartel x (more than 18 months after capture)	-0.064 (0.341)	1.225** (0.45)	-0.008 (0.024)	-0.039*** (0.014)
Panel B: Cartel x Post, Regression has additional dummy for Calderon Election				
Cartel x (1-6 months after capture)	0.0399 (0.205)	-0.284 (0.416)	0.017 (0.01)	-0.014 (0.011)
Cartel x (7-12 months after capture)	0.181 (0.267)	0.252 (0.307)	0.008 (0.009)	-0.009 (0.01)
Cartel x (13-18 months after capture)	0.291 (0.281)	0.742** (0.364)	-0.005 (0.01)	-0.013 (0.011)
Cartel x (more than 18 months after capture)	-0.177 (0.314)	1.217** (0.439)	-0.007 (0.025)	-0.048** (0.014)
Municipalities	821	821	821	821

Notes: This table includes additional controls. In Panel A I include an additional dummy equal to 1 after the beginning of the war on drugs for cartel municipalities. Panel B has a dummy equal to 1 after the election of President Calderon for cartel municipalities. See the rest of the Notes from Table 3.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE C2: LIST OF OPERATIONS

Name	State	Date
Michoacan	Michoacan	Dec., 2006
Sierra Madre-Chihuahua	Chihuahua	Jan, 2007
Chiapas	Chiapas	Jan, 2007
San Luis	San Luis Potosi	July, 2007
Aguascalientes	Aguascalientes	Sep., 2008
Laguna	Durango	June, 2008
Culiacan-Navolato-	Sinaloa	May, 2008
Guerrero Seguro	Guerrero	Oct., 2011
Guerrero	Guerrero	Jan, 2007
Veracruz	Veracruz	Mar, 2007
Veracruz Seguro	Veracruz	Oct., 2011
Morelos Seguro	Morelos	May, 2012
Nuevo Leon-Tamaulipas	Nuevo Leon, Tamaulipas	Jan, 2007
Noreste	Nuevo Leon, Tamaulipas	Oct, 2008
Monclova	Coahuila	Feb, 2013
Triangulo de la brecha	Estado de Mexico	Jan, 2011

Source: Lindo and Padilla-Romo (2018).

TABLE C3: EFFECT OF CARTEL LEADERS' CAPTURES ON LN(WAGES), CONTROLLING FOR MILITARY AND POLICE OPERATIONS

	All operations (1)	Military Operations (2)	Operations not carried by military (3)	No Municipalities of Capture (4)
Panel A: Treatment is Operations				
Cartel x Capture	-0.051*** (0.014)	-0.052*** (0.014)	-0.052*** (0.014)	-0.048*** (0.014)
Cartel x Operations	-0.006** (0.002)	-0.007 (0.004)	-0.009** (0.004)	-0.006** (0.002)
Municipalities	821	821	821	821
Panel B: Treatment is Number of Killed				
Cartel x Capture	-0.053*** (0.014)	-0.052*** (0.014)	-0.053*** (0.014)	-0.0498*** (0.014)
Cartel x Operations	-0.002 (0.001)	-0.002 (0.002)	-0.002 (0.002)	-0.002*** (0.001)
Municipalities	821	821	821	821
Panel C: Treatment is Number of				
Cartel x Capture	-0.052*** (0.014)	-0.052*** (0.014)	-0.052*** (0.014)	-0.049*** (0.014)
Cartel x Operations	-0.003*** (0.001)	-0.003 (0.002)	-0.003 (0.002)	-0.004*** (0.001)
Municipalities	821	821	821	821
Panel D: Treatment is Seizures				
Cartel x Capture	-0.052*** (0.014)	-0.052*** (0.014)	-0.052*** (0.014)	-0.048*** (0.014)
Cartel x Operations	-0.002*** (0.002)	-0.006* (0.003)	-0.004*** (0.001)	-0.005*** (0.002)
Municipalities	821	821	821	821

Notes: This table varies the definition of possible additional treatment. Panel A includes a count of all operations, while Panel B counts only operations that resulted in at least a death. Panel C includes operations that resulted in at least one detainee and Panel D all operations where a good was seized. See notes from Table 3.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE C4: PLACEBO TREATMENTS

	First outlier in detainees	First outlier in killed	First Operation in municipality	Outlier event	No Municipality with any operation before 2010
	(1)	(2)	(3)	(4)	(5)
Panel A: Fake Treatment affects whole					
Cartel x Capture	-0.054*** (0.013)	-0.049*** (0.012)	-0.042*** (0.012)	-0.052*** (0.012)	-0.061*** (0.022)
Cartel x Fake Capture	0.001 (0.013)	-0.008 (0.012)	-0.01 (0.010)	-0.01 (0.015)	
Municipalities	821	821	821	821	564

Notes: In this table, treatment is defined by cartel. In other words, I study the impact of the capture of each kingpin on wages. See notes from Table 3.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE C5: EFFECT OF EACH CARTEL LEADER CAPTURE ON LN(WAGES)

	Sinaloa- Beltran- Leyva	Tijuana	Juarez	Golfo	Familia
	(1)	(2)	(3)	(4)	(5)
Cartel x Capture	-0.036*** (0.017)	-0.09*** (0.022)	-0.052*** (0.017)	-0.04** (0.016)	-0.01* (0.005)
Municipalities in Treatment	112	37	44	122	38

Notes: In this table, treatment is defined by cartel. In other words, I study the impact of the capture of each kingpin on wages. See notes from Table 3.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

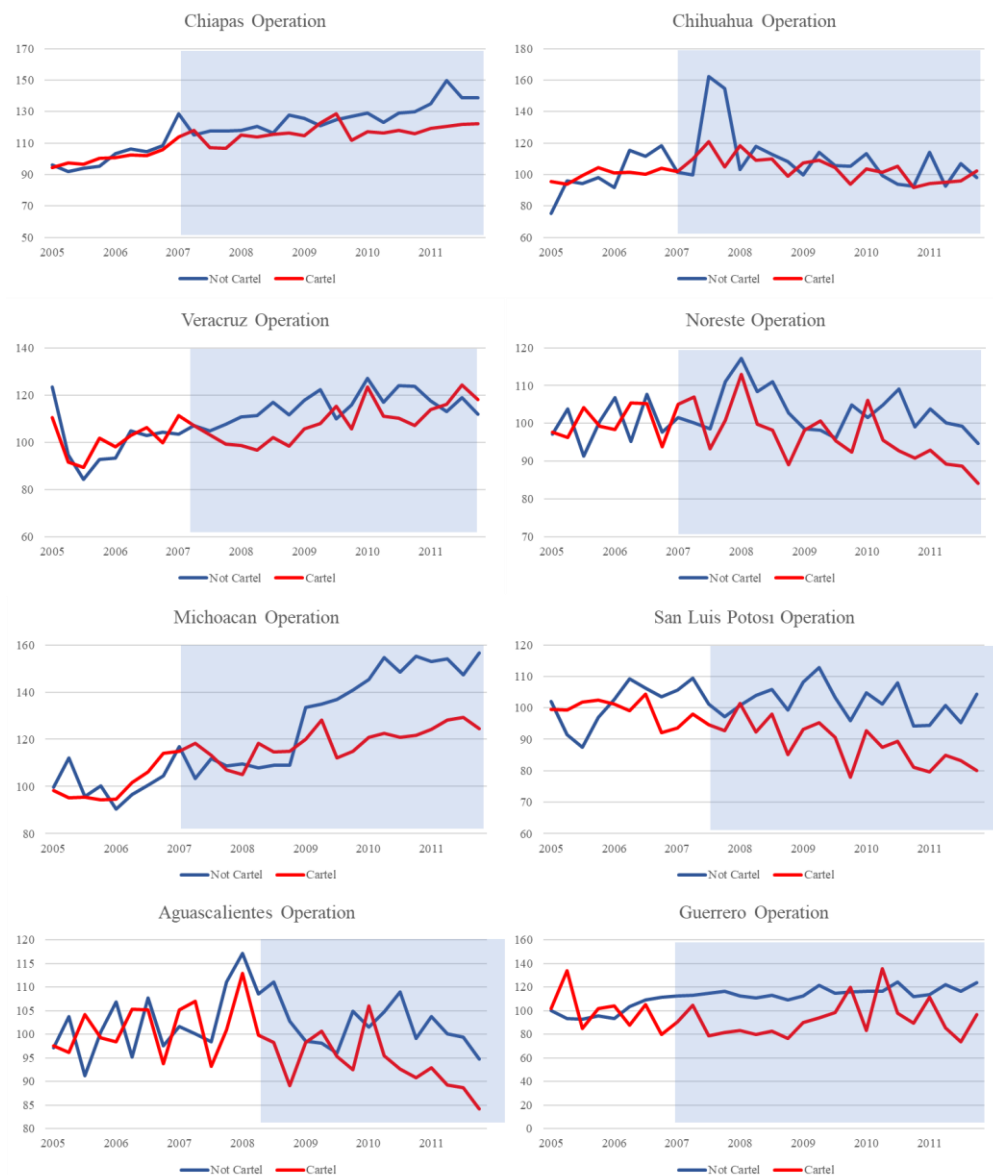


FIGURE C1

HOURLY WAGES (AVERAGE 2005-2006 = 100) AND STATE LEVEL OPERATIONS

Notes: These set of graphs compare the hourly wage (average 2005-2006=100) between cartel areas and non-cartel areas for each State where a State level operation took place. The shadowed area represents the dates through which the operation took place.

Source: Coscia and Rios (2017) and ENOE.

Appendix D. Summary of Busso et al. (2018)

To complete this paper, I construct a firm panel using the methodology by Busso et al. (2018). With some exceptions, the Economic Census does not have a unique firm ID⁷¹ and such variable had to be created through the use of confidential data. The procedure works in the following manner:

- a) Names are standardized: Special characters (like punctuation and accents) are eliminated or substituted. The description of each establishment is also standardized.
- b) Indexing (pre-matching): Candidates for linkage are created. If two establishments have the same location, up to the number of the building and block, in two consecutive waves, and the name of the owner is the same in both waves, a match is created.
- c) Comparison: Text strings are compared.
- d) Matching Classification: Then, an identifier is assigned to a linked establishment. The linkages are tagged, to be excluded in further phases and a number is assigned denoting the phase in which it was linked.
- e) Validation: The linkage process is compared to that between the 2009 and 2014 waves, where a panel ID exists.

To cover steps these steps, a 10-phase algorithm is created. The rule is that if 2 out of 3 factors (legal entity, industry or location) are the same in 2 consecutive waves, a unit is considered the same across time.

The phases are:

1. NICNOP Code (Identification Number and Operative Number).
2. Location (State, Municipality, Locality, AGEb, Block) and Industry.

⁷¹ There is a unique ID that links certain years, like 2009 and 2014; or a unique ID that links only the very large firms across the years.

3. First, location up to building number is used. Then, establishments that have a similarity of at least 45% in Establishment Name and 75% in Legal Entity Name are linked.
4. First, a combination of Municipality, Industry and Legal Entity are indexed. Then establishments with similarity of at least 30% in Establishment Name are linked.
5. Establishments with same combination of AGEB, and Legal Entity are linked.
6. First, establishments with the same Locality, AGEB, Block and Industry are indexed. Then establishments with the same soundex in Establishment Name and Legal Entity are linked.
7. Establishments with the same combination of State, Municipality, Locality, AGEB, Block, Industry and Birth Year.
8. Establishments with the same combination of State, Municipality, Locality, AGEB, Block, Industry and Exterior Number.
9. First, establishments with the same combination of State, Municipality, Locality, and AGEB or Industry are indexed. Then establishments with similarity of at least 65% in Establishment Name and Legal Entity are linked.
10. Establishments with the same combination of Industry, Establishment Name and Legal Entity.

In addition to this process, I also included 2 conditions: In order to reduce the matching error,⁷² I only included firms that have a matching firm id variable for 2009 and 2014 (where such id exists). Secondly, I eliminated the 1999 wave, as it had several coding differences that might have exacerbated the matching error.

⁷² There is a 9% discrepancy from a perfect panel data, as there are observations that do not have enough information to be linked across time or false matches.

Appendix E. Additional Outcomes and Heterogeneity

Transitions

As previously stated, cartel leaders' capture increases the proportion of workers in informal jobs. However, it is not clear if it is that formal workers are losing their jobs or that there are no new formal jobs (or both things are happening at the same time). To explore these possibilities, I do the following: 1) I use the rotating nature of ENOE, and study transitions from the formal to informal market and 2) Use the Social Security Institute (IMSS) administrative data, to study the impact on formal workers and their wages.⁷³ Notice that the following analysis is not comparable to the previous cross-sectional study for the following reasons: 1) IMSS data is restricted to the formal market. However, this panel can be extended as long as the worker remains in the formal sector. 2) The analysis using ENOE can cover the informal market; however, it only follows individuals for 5 quarters (and captures takes at least a year to have a statistically significant impact, making it impossible to test parallel trends).⁷⁴

First, I start with the analysis that uses ENOE panel data and show the results in in Table E1. I study the impact of a leaders' capture on the following outcomes: 1) Probability of becoming an unpaid worker, given initial paid employment. 2) Probability of unemployment, given initial formal employment. 3) Probability of informal employment, given initial formal employment. 4) Probability of formal employment, given initial informal employment. 5) Ln(monthly wage), for workers that were paid for the 5 quarters.

My estimates show that 1 year after capture, paid workers in cartel areas were 1.2% more likely to becoming unpaid. However, I did not find a statistically significant impact on the probability of becoming unemployed, which is in line with the aggregate evidence. Column (3) shows that formal workers were 0.8% more likely to become informal one year after the

⁷³ I use a random sample of 3% of all the data due to computational limitations: as some regressions use individual and/or fixed effects, estimating a linear regression with 86 million observations (full sample) is computationally burdensome. However, I use a random sample that is a compromise between computational capabilities and representativeness.

⁷⁴ As it is a rotating panel, it means that not all individuals and not all municipalities are being followed at the same time. This means that the first quarter for certain individuals or localities might be the last for others.

capture. Finally, I study the impact on wages. My results show that one year after the capture, there is a 1.5% drop in wages.⁷⁵

The next step is to study the impact using IMSS's administrative records. Using this dataset, I can test if the capture of a leader had a differentiated effect on the number of entrants to the formal market and job changers. These classifications are particularly important because Gertler et al. (2016) showed that new workers (entrants and firm changers) might be particularly sensitive to shocks. Therefore, it might not be surprising that those most affected are job searchers. For this reason, I classify workers into 4 categories and study the impact of leaders' capture on each. The classification I chose is based on Gertler et al. (2016): a) Keepers are defined as those workers that have being with the same employer for at least 12 months. b) Entrants are those workers that show up for the first time in the formal market. c) Changers are those that had less than 12 months with their current employer (direct changers did not spend time outside of the formal market, while "Not direct changers" are those workers that stayed outside of the formal market for at least 1 month).^{76,77}

My results are shown in Table E2. Entrants (column 2) and changers (columns 3 and 4) were particularly sensitive to treatment, while keepers were not (there is a small negative effect, but it is not statistically significant). The effect on job changers and entrants took place immediately after the capture of the leader and lasted for 2.5 years. However, a likely scenario is that, as the War on Drugs progressed, new municipalities gained cartel presence and violence increased in those regions muting the effect.⁷⁸ Nevertheless, these results show that it became harder to obtain a new formal job.

Finally, IMSS data could help us understand this drop in reduced job opportunities. For example, one could see if there are less formal firms to apply to,⁷⁹ if "keepers" see their wages reduced, if changers are moving to low paying firms (within the same municipality),⁸⁰

⁷⁵ The aggregate results show that wages decrease 5% a year and a half after capture. Using aggregate data there is a similar drop in wages of 1-2%. However, it is not statistically significant at that point in time.

⁷⁶ The first year of this dataset I use is 2006. This is because I use the year of 2005 to identify workers with formal work experience.

⁷⁷ 2/3 of all workers in my dataset are "keepers", 12.5% entrants, 11.5% direct changers and 8.5% not direct changers.

⁷⁸ Another possibility is that the shock was larger and lasted longer for informal workers.

⁷⁹ The Economic Census has information on both formal and informal firms. Before the 2019 wave, it was not possible to distinguish between the two. Information from the 2019 Census wave states that 62.6% of all firms are informal.

⁸⁰ To categorize a lower paying firm, I simply use as indicator the median firm wage before the shock took place in the municipality.

and how long it takes for them to find a new formal job. All these indicators can tell us about a possible change in employee bargaining power and I explore these questions in Table E3.

My results show that there is a drop in the number of formal firms (of about 1-2%). This is smaller than the effect I found using the Economic Census, implying that formal firms (by being larger and thus, more resilient), were able to survive at higher rates. Moreover, we can see that the number of workers decreases before firm destruction increases. This signals the possibility that firms fired workers before closing, to save labor costs and survive.

In Column 2 I see that keepers also saw a drop in wages. However, this drop is quite small ($<1\%$), which confirms that informal workers were the most affected. Column 3 states the impact on the probability of changers to move to a firm whose average wage was at least as good as their previous employer,⁸¹ and my results show that after a leaders' capture it became harder to find a better paying job. As changing job is how most workers can get raises,⁸² this can help to explain the total drop in wages. Finally, in column 4 I did not find an effect on the amount of time required to obtain a job.

Overall, my results state that the capture of a cartel's leader indeed damaged the formal market and pushed people out of formality. However, the largest negative outcomes were for those working in the informal market.

⁸¹ The reference year is the year before the shock.

⁸² Forbes, 2014. Employees Who Stay In Companies Longer Than Two Years Get Paid 50% Less. <https://www.forbes.com/sites/cameronkeng/2014/06/22/employees-that-stay-in-companies-longer-than-2-years-get-paid-50-less/>

TABLE E1: EFFECT OF CARTEL LEADERS' CAPTURE ON INDIVIDUAL LEVEL OUTCOMES

	Prob of unpaid Employment, given initial paid employment	Prob of unemployment, given initial paid employment	Prob of informal employment, given initial formal employment	Prob of formal employment, given initial informal employment	ln(Monthly Wage)
	(1)	(2)	(3)	(4)	(5)
t+1	0.007 (0.005)	0.001 (0.001)	0.008 (0.003)	-0.003 (0.003)	-0.008 (0.008)
t+2	-0.008 (0.005)	-0.003 (0.003)	-0.005 (0.003)	-0.002 (0.003)	-0.004 (0.005)
t+3	0.006 (0.006)	0.007 (0.001)	0.009** (0.003)	-0.001 (0.003)	-0.005 (0.005)
t+4	0.012* (0.006)	0.001 (0.002)	0.008** (0.003)	0.0001 (0.003)	-0.015** (0.005)
Individuals	74,407	74,407	37,262	55,107	24,460

Notes: All regressions include individual, sociodemographic, municipality and time fixed effects. Municipality clustered standard errors in parenthesis.

Sources: Labor Outcomes are obtained from ENOE, while areas of Cartel areas are based on Coscia and Rios (2017).

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level

TABLE E2: EFFECT OF CARTEL LEADERS' CAPTURE ON THE NUMBER OF WORKERS REGISTERED AT IMSS

	ln(Keepers)	ln(Entrants)	ln(Direct Changers)	ln(Not Direct Changers)
	(1)	(2)	(3)	(4)
Cartel x 13-18 months before	0.005 (0.007)	-0.002 (0.02)	-0.004 (0.007)	-0.007 (0.022)
Cartel x 7-12 months before	0.011 (0.006)	-0.009 (0.018)	0.003 (0.007)	-0.028 (0.020)
Cartel x 1-6 months before	0.00 (0.0)	0.00 (0.0)	0.00 (0.0)	0.00 (0.0)
Cartel x 1-6 months after	-0.004 (0.004)	-0.025** (0.011)	-0.01** (0.004)	-0.026* (0.013)
Cartel x 7-12 months after	-0.005 (0.004)	-0.022** (0.011)	-0.004** (0.002)	-0.021* (0.012)
Cartel x 13-18 months after	-0.001 (0.004)	-0.040** (0.011)	-0.004* (0.002)	-0.003 (0.013)
Cartel x 19-24 months after	-0.005 (0.004)	-0.02*** (0.012)	-0.01** (0.004)	-0.012 (0.019)
Cartel x 25-30 months after	-0.003 (0.004)	-0.014 (0.011)	-0.01** (0.004)	0.010 (0.013)
Cartel x 31-36 months after	-0.004 (0.004)	0.01 (0.012)	-0.001 (0.004)	-0.003 (0.013)
Cartel x 37-42 months after	-0.006 (0.004)	0.016 (0.012)	-0.007 (0.004)	-0.012 (0.013)
Cartel x 43-48 months after	-0.002 (0.004)	0.017 (0.013)	0.008 (0.005)	-0.001 (0.014)
Municipalities	408	408	408	408

Notes: As IMSS data is reported by firm, the regressions are clustered at the firm level. All regressions include state-by-month-by-year and municipality fixed effects. Regressions were computed on a random monthly 3% sample for each period.

Sources: Labor Outcomes are obtained from IMSS administrative data, while areas of Cartel areas are based on Coscia and Rios (2017).

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level

TABLE E3: EFFECT OF CARTEL LEADERS' CAPTURE ON JOB TRANSITIONS

	ln(IMSS Firms)	ln(Wage Keepers)	P(Moved to Higher Paying firm)	ln(Months between formal jobs)
	(1)	(2)	(3)	(4)
Cartel x 13-18 months before	0.002 (0.008)	0.009 (0.005)	-0.002 (0.002)	-0.06 (0.035)
Cartel x 12-7 months before	0.004 (0.007)	-0.003 (0.004)	0.002 (0.002)	-0.14 (0.34)
Cartel x 6-1 month before	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
Cartel x 0-6 months after	-0.003 (0.005)	0.006 (0.004)	0.003 (0.029)	-0.32 (0.328)
Cartel x 7-12 months after	-0.005 (0.006)	0.001 (0.004)	-0.003 (0.028)	0.152 (0.386)
Cartel x 13-18 months after	-0.009 (0.006)	-0.011** (0.004)	-0.016 (0.02)	-0.112 (0.358)
Cartel x 19-24 months after	-0.009 (0.006)	-0.008** (0.003)	-0.04*** (0.02)	0.038 (0.329)
Cartel x 25-30 months after	-0.011** (0.005)	-0.005 (0.004)	-0.06*** (0.027)	0.268 (0.377)
Cartels x 31-36 months after	-0.025*** (0.005)	-0.001 (0.004)	-0.045 (0.029)	-0.417 (0.359)
Firms	277,239	277,239		
Individuals		648,180	201,551	201,551
Collapsed data	X			
Municipality FE	X	X	X	X
Firm FE		X	X	X
Individual FE		X	X	X

Notes: As IMSS data is reported by firms, the regressions are clustered at the firm level. All regressions include state-by-month-by-year and municipality fixed effects. Regressions were computed on a random 3% sample.

Sources: Labor Outcomes are obtained from IMSS administrative data, while areas of Cartel areas are based on Coscia and Rios (2017).

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level

Other outcomes

The rest of this section includes heterogeneity analysis that was omitted from the main text for the sake of brevity for which I did not have space in the main analysis. Table E4 decomposes the main analysis by gender and age, Table E5 decomposes ENOE outcomes by industry of employment and Table E6 shows the number of firms that moved between key Economic Census waves.

TABLE E4: EFFECT OF CARTEL LEADERS' CAPTURE ON WAGES BY AGE AND SEX

	All Ages	Aged 18-24	Aged 25-45	Aged 46-65	Older than 65 years old
Panel A: Impact on Men's					
Wages: DTO x Post	-0.047** (0.016)	-0.054*** (0.018)	-0.044** (0.018)	-0.083** (0.25)	-0.0618 (0.046)
Unpaid Employment: DTO x Post	1.62* (0.86)	1.62* (0.86)	0.518 (0.57)	1.36** (0.69)	(1.007)
Municipalities	821	821	821	821	821
Panel B: Impact on Women's					
Wages: DTO x Post	-0.068** (0.019)	-0.021* (0.024)	-0.081** (0.023)	-0.063* (0.033)	-0.047 (0.054)
Unpaid Employment: DTO x Post	1.61** (0.579)	0.61 (1.069)	1.61** (0.71)	2.202** (0.807)	0.29 (1.19)
Municipalities	821	821	821	821	821

Notes: See notes from Table 3. On the top of the column I describe which age-group the regression is using as a sample. In each cell I report the Beta coefficient from equation (1) on the mentioned age-group and the outcome at the left.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE E5: EFFECT OF CARTEL LEADERS' CAPTURE ON LABOR OUTCOMES (ENOE) BY INDUSTRY

	Government and Institutions (1)	Restaurants (2)	Construction (3)	Manufacture (4)	Health (5)
ln(Hourly Wage)	-0.009 (0.027)	-0.035 (0.025)	-0.030** (0.015)	-0.089*** (0.021)	-0.11*** (0.037)
Proportion of workers	-0.004** (0.002)	0.001 (0.002)	-0.003 (0.003)	-0.005 (0.004)	0.000 (0.002)
Municipalities	799	808	821	819	743
	Retail (6)	Wholesale (7)	Media (8)	Finance (9)	Transp. (10)
ln(Hourly Wage)	0.000 (0.022)	-0.042* (0.028)	-0.038 (0.048)	-0.023 (0.052)	-0.041 (0.027)
Proportion of workers	-0.000 (0.003)	-0.002 (0.001)	<0.001 (<0.001)	-0.0001 (0.001)	<0.001 (0.002)
Municipalities	821	751	478	477	783

Notes: See Notes from Table 3.

TABLE E6: NUMBER AND PERCENTAGE OF FIRMS THAT MOVED
(FROM PREVIOUS CENSUS WAVE)

	DTO		No DTO	
	Number of Firms That Recently Moved	Proportion of Total of Firms	Number of Firms That Recently Moved	Proportion of Total of Firms
2009	256	0.33	169	0.26
2014	48	0.06	50	0.08

Notes: This table was created using data from the Firm Census and Coscia and Rios (2017).