The economic victims of violence: local exports during the Mexican drug war*

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Abstract

This paper documents how violence resulting from the Mexican Drug War hindered local export growth. Focusing on exports allows us to abstract from demand factors and measure effects on the local capacity to supply foreign markets. We compare exports of the same product to the same country, but facing differential exposure to violence after a close electoral outcome. Firms exogenously exposed to the Drug War experienced lower export growth. Violence eroded the local capacity to attract capital investment, disproportionately hampering large exporters and capital-intensive activities.

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1 Introduction

The path for developing countries to converge towards higher living standards is linked to their capacity to industrialize and compete in international markets. Recent papers have shown that exports can help close the productivity gap between low and high-income countries.[1] Researchers and policymakers highlight several factors that could explain the lack of competitiveness of developing economies. Among others, crime and violence are perceived as important obstacles.[2] Accordingly, governments spend a significant amount of resources in policies aimed at attenuating violence and crime.[3] In this paper, we show how an anti-crime policy that relies on the use of force can have the reverse effect on violence, further eroding local productive capacities.

We study this question in the context of the Mexican Drug War, which started during the Calderón Administration in late 2006. One party (PAN: *Partido de Acción Nacional*) led the war on drugs, and the rollout of war efforts was influenced by the party affiliation of local authorities. As showed by Dell (2015), a close election of a PAN mayor led to a disproportionate increase in violence during the Mexican Drug War. A first exploration of the effects of a discontinuous exposure to a PAN mayor using aggregate data finds negative effects on production growth concentrating in the manufacturing sector. This is consistent with findings from the Mexican Enterprise Survey - performed right before and at the peak of the drug war - showing growing concerns over crime as a productive constraint for manufacturing firms in northern Mexico, the region most affected by the ensuing violence.

Because violence can affect both demand and supply, our paper introduces new elements to the regression discontinuity (RD) setting in order to correctly identify the

[1] Exporting activity affects aggregate (Melitz, 2003; Bernard et al., 2003) and firm (Atkin et al., 2017; Garcia-Marin and Voigtländer, 2019) productivity and technology adoption (Bustos, 2011; Aw et al., 2011). The importance of trading in international markets is also reflected in large amounts invested in policies that promote market access (Lederman et al., 2010).

[2] Violence is highly regarded as an obstacle for development. See, for instance, the Sustainable Development Goal 16 from the United Nations Development Program. Hall and Jones (1999), among others, document cross-country correlations of measures of predation and income.

[3] In 2015, the federal government in Mexico spent 1% of the GDP in domestic security and the justice system. According to the OECD, in the same year governmental expenditure in public order and safety was 2.14% in Colombia and 2.09% in Chile.

effect of violence on the local capacity to supply foreign markets. Chiefly, we expand focusing on export growth from areas discontinuously exposed to a PAN mayor as an exogenous source of violence, comparing exports of the same product to the same destination, but originating from municipalities affected by different levels of violence. This minimizes the chance that resulting estimates are driven by foreign or national demand factors, and not strictly by effects on the local capacity to supply foreign markets.[4] While exploiting exogenous variation in violence to assess aggregate effects on local outcomes by itself would not identify supply-side effects from demand-side effects, our empirical strategy helps us do so. This empirical strategy also addresses concerns that our effects are driven by the "great trade collapse", which occurred at the same time, as our identification compares exports of the same product to the same destination. It is very unlikely that foreign demand within an example country, China, for the same product, semiconductors, changes deferentially for municipalities in which PAN experienced a close win.

Using firm-level data on exports, we document that export growth decreases by a yearly average of 22% as a consequence of a PAN mayor elected in a close election. This effect is persistent and continues into the next mayoral term: the 6-year growth rate in exports decreases a yearly average of 13%.[5] We only observe robust effects on export growth at the intensive margin.[6] Because the firm-level data include only exports of firms operating in single municipalities within a state, we also use public municipality-level data on exporters to confirm our results.[7] We find that export growth in munici-

[4]Correctly identifying the supply-side from the demand-side effects of a given shock is highly relevant for the design of policies aimed at attenuating its economic impacts. For example, during the recent COVID-19 crisis, a large debate ensued about the nature of the shock, that is, whether it was a demand or a supply shock, and the corresponding attenuating policies that are recommended in each case.

[5]The fact that shocks to exports can have long-lasting consequences has been explored in the literature that studies the effects of bank shocks. See Xu (2020).

[6]The main implication of this finding is that violence only seems to increase marginal operating costs. If increases in violence manifest in the form of marginal costs, we should observe effects in the intensive margin. If the effect comes from an increase in fixed costs of servicing different markets, this would change exporting decisions at the extensive margin (Melitz, 2003). Moreover, since an importer-exporter trade relationship is costly to form and contracts are difficult to write and enforce, this result can also be explained by firms trying to sustain trading relationships with their clients (Macchiavello and Morjaria, 2015).

[7]The micro-data does not allow us to differentiate exports coming from a particular municipality when firms have multiple plants in different municipalities within the same state. Restricting to single plants guarantees the correct assignment of exports to the municipality of origin.

palities electing a PAN mayor in a close election experience an average yearly decrease of 15%.

One possible criticism to our identification strategy is that negative estimates may be driven by PAN mayors themselves, and not by the associated violence. This is improbable, as municipalities governed by PAN were likely to receive an economic benefit for several reasons. PAN is more market-friendly than its peers, and the federal administration at the time was likely to benefit municipalities governed by politically aligned PAN mayors (Azulai, 2017; de la Garza and Lopez-Videla, 2020). These potential biases lead to an underestimation of the negative economic effects of violence.[8]

Nevertheless, we investigate this conjecture by performing a series of placebo tests. As *ex-ante* violence is an important determinant of the deployment of law enforcement, we divide the country according to regions with more pre-existing violence. At the onset of the war most of the violence concentrated in northern Mexico. Accordingly, we split the sample into two parts: north and south. A PAN municipality in the south was less likely to be affected by the war, but still experienced the results of policies that are specific to this political party. We find that the effect is only present in the northern regions. In the southern regions a close PAN win is associated with an increase in export growth. Similarly, we use data collected by Coscia and Rios (2012) to define municipalities by *ex-ante* drug cartel activity.[9] Consistent with our results based on the geographical split, we find that municipalities with pre-existing cartel presence experience a significant decrease in export growth after a close PAN win. In the absence of pre-existing cartel activity, close PAN wins are correlated with an increase in export

[8] Another potential concern is that export activity might have not eroded as a consequence of violence, but displaced. Such displacement would threaten our identification strategy if exports moved from areas with a close PAN win to areas with a close PAN loss, yielding a violation of the SUTVA assumption. We believe this is highly unlikely, as close PAN loss locations are only a small subset of potential destinations, and given the spatial dispersion in the sample of municipalities with close elections (Online Appendix, Figure A.9). A separate concern regarding displacement is that our local estimates may not be representative of potential aggregate effects of violence on Mexican exports. In separate analyses excluded from this version of the paper, we found no effects of close PAN mayors on the chance that single-plant firms started operations outside of their original locations, suggesting that violence did not induce firms to disperse even when considering the broader Mexican territory as a potential displacement destination.

[9] The north/south and the cartel/non-cartel splits are not independent. Around two thirds of the municipalities with pre-existing cartel presence are located in the north sample.

growth. Finally, we run placebo tests using local elections that took place outside the drug war period. We find the effect of a PAN win is negative only for mayors whose terms take place during the peak of the war. While our preferred specifications control for market specificities, we find that our results are robust to aggregating firm data across markets. Moreover, municipality-level data on production and sales confirm aggregate effects in the tradable sector of the economy.

Another goal of this paper is to identify the channels through which violence affected local economic outcomes. By increasing predation on local firms, violence may push them to misallocate resources from production to protection activities, while eroding the capacity of local economies to attract capital investments. Moreover, violence may work as a disamenity, pushing workers away from the risk of being victimized and increasing local labor costs to prevent workers from moving to less violent areas. Finally, violence may affect the capacity of firms to transport and coordinate diverse sets of inputs and talents in their production processes.

To answer this question, we first identify the main economic victims of violence - the sectors that observed the largest negative effect on export growth as a consequence of the Mexican drug war. Effects concentrate in the export growth of electronics, vehicles and textiles. Moreover, violence eroded export growth for large exporters and exports of capital-intensive and complex products. We find stronger effects on products that do not rely on transportation services, and in products that depend on a concentrated input and occupation mix.

We further explore the mechanisms through which violence altered economic outcomes. We find that the Mexican drug war decreased capital accumulation and the capital expenditure in new greenfield investments. We also find suggestive evidence that the war increased the number of providers of private security services. Contrary to the expectation from the disamenity channel, we find evidence that violence led to a reduction in wage growth, and we find no statistical effects on migration patterns. Taken together, these results suggest that the reduction in capital availability is the dominating channel connecting violence and export growth, with suggestive evidence

for the resource misallocation channel due to increasing protection costs.[10]

Our decision to focus on sales to foreign markets to better assess the supply-side effects of a violence shock diverges from previous studies on this topic and in this setting. Under our empirical strategy, we leverage an exogenous shock to local violence, but we are also able to abstract from foreign demand idiosyncrasies and composition-driven biases by comparing the change in exports of the same product to the same country of destination from municipalities differently affected by violence. While national demand affects both treated and control municipalities, local demand factors could only play a role if municipalities hosting given exporters represent a significant portion of their business sales. Focusing on exports of goods from - relatively large - exporting firms makes it unlikely that municipal demand would alter decisions at this margin, especially since the most affected sectors are electronics and vehicles. In a further attempt to control for local (neighboring) demand, we show that results are robust to including region fixed effects. We also show that non-tradable activities seem to be less affected.

Our paper relates to a growing literature that documents the negative economic consequences of violence. Research shows that violence can affect firm market values, housing prices, product prices, and corporate investment. Violence also impacts the labor decisions of firms through displacement and misallocation from production to security services.[11] Our paper contributes to this literature in several ways. First, it addresses identification challenges. A challenge to identify the economic effects of violence is the endogeneity problem. Crime is correlated with local time-varying non-observable economic variables that affect firms' prospects. There is also a reverse causality concern, as researchers have shown that crime reacts to local economic

[10]While lower capital attraction should lead to lower wage growth if the marginal productivity of labor decreases, the disamenity channel should lead to higher wage growth. The results suggest that the first effect dominates.

[11]See, for instance, Abadie and Gardeazabal (2003) for the effects on firm market values; Besley and Mueller (2012) and Frischtak and Mandel (2012) for housing prices; Pshisva and Suarez (2010) for corporate investment; Besley and Mueller (2018) for misallocation; and Adhikari (2013) and Clemens (2017) for labor force displacement. Rozo (2018) documents an increase in firm exit and a decrease in production through a decrease in output prices. Ksoll et al. (2016) show that electoral violence causes a decrease in exports due in part by an increase in worker absenteeism.

shocks.[12] Our study overcomes these issues by exploring exogenous variation in the local exposure to violence.

We provide evidence about both the types of firms that are affected and the mechanisms through which violence affects production. Currently in the literature there is little evidence of both the economic victims of violence and the mechanisms through which they are affected. Our paper aims at filling that gap.

Finally, while a broader segment of the literature has studied the economic consequences of conflicts or terrorist activity, fewer papers study the economic consequences of violence triggered by law-and-order government interventions. The distinction is relevant because, differently from civil conflicts, law enforcement activities are common in a broader set of countries. Similar to Mexico, territories are also divided among criminal groups. Importantly, the types of crimes that increase as a consequence of these operations is likely to be different from the types of crimes that increase after civil conflicts, as the organizations involved differ in their structure and objectives. A final distinction is that the effect of national policies for law-and-order are not contingent to conflict settings where political uncertainty might be an issue.[13] Therefore, informing about unintended consequences of these policies adds to the literature and can improve the decision-making of policy makers in a large set of countries.

We also relate to the literature that explores the effects of the Mexican Drug War.[14] Using different empirical strategies and samples of firms, some papers also study the impact on the economy. In an interesting contemporaneous paper, Utar (2018) shows that an increase in violence driven by the drug war generates a decrease in production to local markets, but not a decrease in exports.[15] Montoya (2016) finds that the drug

[12]For instance, Dell et al. (2019) and Dix-Carneiro et al. (2018) show that local economic conditions affect crime. The fact that criminal activity correlates with local time-varying non-observable economic variables threatens the internal validity of differences-in-differences analyses.

[13]Political uncertainty might add to the effects of violence if it has a different intensity on treatment and control groups, biasing estimates. See Julio and Yook (2012).

[14]There is a growing literature on the effects of the Mexican Drug war. Velásquez (2020) shows that the drug war affected labor market outcomes; Brown and Velásquez (2017) study the effect on human capital accumulation; Ajzenman et al. (2014) documents impacts on house prices; and Lindo and Padilla-Romo (2018) study the consequences of the kingpin targeting approach.

[15]While controlling for time-invariant firm-level unobservables, Utar (2018) instruments for differences in local violence with the interaction between cartel baseline presence, the governor's choice to join the drug war, and the estimated price of cocaine. This different identification is the most likely

war affected firms of all sizes in the manufacturing sector and small firms in the non-tradable sector. Gutiérrez-Romero and Oviedo (2018) find a decrease in the value of production. However, the economic magnitudes are small and the significance is not robust. While these papers rely on differences-in-differences or instrumental variable methods, our study uses plausibly exogenous changes in violence to find negative consequences of the Drug War on local exports.[16] We also provide further evidence on the mechanism through which these effects may operate, and the activities in which the effect concentrates. Violence acts as an increase in marginal costs of production, and these costs seem to concentrate on larger exporters, and on exporters in capital dependent industries. Finally, our suggestive evidence of negative effects of the Drug War on capital accumulation and greenfield investments are consistent with the findings in Ashby and Ramos (2013), who document a negative association between crime and FDI at the state level in Mexico.

2 Empirical setting

2.1 The Mexican political landscape and the Drug War

For 71 years, the Institutional Revolutionary Party (*Partido Revolucionario Institucional, PRI*) was the single party ruling Mexico. Elections existed, but they were not competitive. In the 1990s, politicians from different parties started winning local elections, and, in 2000, Mexico elected its first non-PRI president since 1929. Some analysts suggest that, during PRI rule, there was a tacit agreement between the government and the drug traffickers allowing cartels to operate as long as they complied with some rules (O'Neil, 2009). For example, cartels could not cause major disruptions to civilian life. Importantly, violence was contained. When other parties gained power, this relation-

source the opposite effects on exports between our papers. Another difference is that Utar (2018) excludes export-processing firms or 'maquiladoras'. This is an unlikely source for the difference. Using census data we confirm a drop in revenues and production for the whole tradable sector and for the tradable sector excluding export-processing firms.

[16] Our choice comes at a cost: because we restrict the sample to a small number of municipalities with close elections, we are unable to use surveys that provide more detailed firm information, as the intersection between the databases is very small.

ship was shaken, as cartels had to negotiate with new officials from other parties. The election of Vicente Fox (PAN) as president in 2000 triggered some institutional changes, but it was only on 2 July 2006, when Felipe Calderón (PAN) was elected president, that changes intensified. Calderón governed from December 1st 2006 until November 30th 2012. Just after taking office, he declared the war on drugs, sending the army to several provinces. The policy had tragic consequences. The arrest or assassination kingpins triggered disputes for territorial power and an escalation in violence (Lindo and Padilla-Romo, 2018). Members from the same organization or from rival cartels can exploit the weakening of the leadership to try to gain the control of the organization. Once in charge, new leaders have to assert their authority, in many cases through the use of force. Cartels also retaliated against politicians, police officers, armed forces, and journalists.

Increases in violence also affected civilian life. During Calderón's administration, the number of homicides increased by 160%, from 10,452 in 2006 to 27,213 in 2011 (Figure 1). Total homicides were concentrated in the northern regions of the country, closer to the US border (Figure 2). These are the regions where the main cartels smuggle drugs into the US. In reaction to the crackdown, there is evidence that cartels began to diversify their activities into other crimes, such as extortion, human trafficking, oil theft, kidnapping, and robbery. Consistently, survey data shows how firms were negatively affected by the ensuing violence. For instance, according to the World Bank Enterprise Survey, between 2006 and 2009 the percentage of establishments paying for security increased from 41.5% to 59%, and the percentage of establishments experiencing losses as a result of theft, robbery, or vandalism doubled from 15% to 30%. Moreover, as we will show below, firms in northern Mexico -where the growth in violence was greatest- became more likely to express that violence was a key constraint to their operations, while Manufacturing firms in northern Mexico became most likely to identify violence as their most binding constraint.

The main strategy of the anti-drug policy was to use aggressive law enforcement

that targeted cartel leaders.[17] These operations were mainly organized at the federal level, but coordination with local authorities was important. All municipalities and states in Mexico control a police force. The municipality has the power to remove or appoint the municipal police chief. According to Article 115 of the Mexican Constitution, the municipal police has the responsibility to provide security and prevent crime. The important role of the mayor in the implementation of the Drug War can also be seen in practice. From 2006 until 2014, organized crime killed 63 former mayors or mayors in office.[18] Furthermore, municipal presidents have denounced extorsion from cartels.[19] Importantly, at the time of the drug war, mayors were elected by popular vote in competitive polls. Hence, it is reasonable to assume municipal elections are an important source of variation in the implementation of the Drug War policy at the local level. This assumption is crucial for our identification.

Regarding Mexico's political climate, Mexican parties are quite heterogeneous in their preferred social and economic policies. Among the major parties, PAN is more economically liberal and business oriented than its opponents. As evidence of this, PAN was elected on an economic platform based on globalization and an increase in foreign investment (Krauze, 2006). It was also more socially conservative on a range of issues, including drug consumption and enforcement. Its main rival in the 2006 elections, the Party of the Democratic Revolution (*Partido de la Revolución Democrática, PRD*), was suspicious of free markets and globalization. Its other rival, the PRI, located between PRD and PAN in an ideological sense.

2.2 Exports and the Mexican Economy

Mexico has a relevant trade activity. The country exports a diverse set of products. In 2005, the three largest product exports were oil (12.8%), automobiles (5.9%) and monitors and projectors (4.6%). In the same year, Mexico exported to 190 countries,

[17] See "Mexico Drug War Fast Facts" (https://edition.cnn.com/2013/09/02/world/americas/mexico-drug-war-fast-facts/index.html).

[18] Webpage: https://cnnespanol.cnn.com/2018/04/13/violencia-contra-los-alcaldes-en-mexico-mas-de-100-asesinados-desde-2006/.

[19] Webpage: http://archivo.eluniversal.com.mx/nacion/165947.html.

with the US accounting for 86% of the exports. The ratio exports/GDP in Mexico was 30.4% in 2005, which is significant compared to other economies in the region. In the same year, this ratio was 15.2% in Brazil, 23.2% in Argentina, 16.8% in Colombia, 40.2% in Chile, and 26.8% in Peru. During the great trade collapse after the financial crisis, Mexican exports suffered more in comparison to those countries. Figure A.8 in the Online Appendix shows that Mexico had the smallest growth in exports between 2005 and 2012.

2.3 Data

We collect data on local electoral results from the Electoral Tribunals of each state. Municipal elections are held every three years, and municipalities located in different states held them on different dates. We focus on municipalities with elections in 2007 and 2008 because the terms of mayors elected in those years overlap with the peak of the war. Monthly data on homicides are from the National Institute of Geography and Statistics (*Instituto Nacional de Estadística y Geografía, INEGI*), available from 1990. Data on other types of crimes tend to be noisier due to underreporting. The issue of underreporting is severe in developing countries, where both the police and victims do not report all crimes. The most reliable source of crime data at the municipality level is The National Public Security System (*Sistema Nacional de Seguridad Pública, SNSP*), which starts in 2011. Data on municipality characteristics are from the National System of Municipal Information (*Sistema Nacional de Información Municipal, SNIM*). Data on skills and incomes of workers in 2000 and 2010 are from the Census of Population and Housing Units (*Censo de Población y Vivienda*). Data on capital investment at the municipality-level in 2003, 2008 and 2013 are from the Economic Census (*Censos Económicos*). Data on greenfield investments are from fDi Markets.

We use firm-level data on exports from the Mexican Tax Administration Service (*Servicio de Administración Tributaria, SAT)*, Mexico's customs authority.[20] We observe exports at the country of destination-product level; products are classified using HS

[20]Micro-level data are not publicly available. We accessed these data at the Growth Lab at Harvard's Center for International Development.

4-digit codes. Because we can only observe the state of origin of the export transaction and our variation is at the municipality-level, we restrict the sample to firms that operate in a single municipality within a state. By imposing this restriction, we are able to identify the correct municipality of origin at the expense of excluding part of the transactions.

To guarantee that our results are not driven by sample selection, we complement the analysis of exports with publicly available municipal data from the Atlas of Economic Complexity.[21] The Atlas is constructed with the same data that we use in the firm-level analysis. The geographical distribution of exports of firms that operate in more than one municipality in a given state is assumed to be identical to the distribution of a firm's workforce as expressed in social security records. Therefore, the Atlas covers all export transactions at the expense of some measurement error when assigning the municipality of origin. Since this data is public available, we also provide replication codes.

2.4 Empirical strategy

The endogenous assignment of enforcement efforts towards violent regions makes simple regressions of violence on enforcement to be biased. Enforcement may also correlate with local unobservables, leading to omitted variable bias. Similar biases would result from regressing measures of local production on violence. To address these challenges, we first need to identify exogenous variation in enforcement and violence. One party, the PAN, implemented stronger actions against Mexican drug cartels. Following Dell (2015), we use close elections of a PAN mayor as a source of exogenous variation in the intensity of the war on drugs. We focus the analysis on the 2007 and 2008 elections, as mayors' administrations elected in those years started at the beginning of the war, and finished around its peak, in 2011. We estimate the following specification

$$y_m = \alpha + \beta PANwin_m + \delta f(Margin_m, PANwin_m) + \epsilon_m \quad (1)$$

[21] The data was developed by the Growth Lab at Harvard's Center for International Development. Access: http://complejidad.datos.gob.mx.

where m denotes municipalities, $PANwin_m$ is a dummy variable that takes value 1 when PAN wins, and $f(Margin_m, PANwin_m)$ is a polynomial on the vote margin and the dummy of PAN victory. In our main results, we restrict the sample to municipalities where PAN won or lost by a margin smaller than 5%.[22] We first associate a PAN win with an increase in homicides. Then, following anecdotal evidence that cartels diversified their activities during the war, we also test the effects on other crimes. Because it is likely that crime is under-reported in smaller municipalities, we weight for population size in 2005 (Dell, 2015). As suggested by Solon et al. (2015), we always report robust standard errors when weighting. In the Online Appendix, we also estimate the effect of a close PAN victory using a local differences-in-differences (DiD) framework with year and municipality fixed effects.

From a theoretical perspective Mexican elections in these period are considered competitive. Since municipalities are relatively large it is unlikely outcomes of close elections were subject to manipulation. In the Online Appendix we also provide empirical support for this claim. We show both using Cattaneo et al. (2018) and McCrary (2008) that there is no evidence of bunching around the discontinuity.

Studying the effect of the same shock on a measure of local production is not enough to identify the impact of violence on the production capacity of firms. Violence can affect both demand and supply. For example, violence could affect the economy by diminishing the likelihood or capacity of individuals to consume certain type of goods; it could disrupt production by increasing costs; it could could drive workers out of the affected locality. We advance the existing literature by disentangling the effects on supply from potential effects on demand. As we concentrate on exports of the same product to the same destination, we keep aggregate external demand factors fixed and estimate an effect that is driven by a drop in the ability of firms to supply foreign markets. We estimate "reduced-form" regressions using exports as dependent variables. Exploring the fact that the data are at the product-destination level, we control for external aggregate demand shocks by including product-destination fixed effects (Paravisini et al., 2014).

[22] In the Online Appendix, we present results for different margin and polynomial choices.

These dummies also control for regional specialization in serving foreign markets, an issue that gains importance in our setting because the sample of municipalities with close elections is small. Regressions take the form:

$$log\left[\frac{X^{t'}_{fmpc}}{X^{t}_{fmpc}}\right]^{\frac{1}{t'-t}} = \alpha + \beta PANwin_m + \delta f(Margin_m, PANwin_m) + \psi_{pc} + \epsilon_{fmcp} \quad (2)$$

where X^t_{fmpc} stands for the exports of firm f of product p to country of destination c, located in municipality m in baseline year t. The dependent variable captures the average yearly growth factor in total exports at the firm, product and country of destination level between years t and t'. In the majority of the specifications, t' is the third (final) year of the new administration, and t is the year when elections take place. β captures the percent difference in the average yearly growth factor of the exports by firm-product-destination for firms marginally exposed to a PAN mayor in their municipality. ψ_{pc} stands for product-country of destination fixed-effects that control for external demand. We cluster standard errors at the municipality level. We follow a similar procedure when using municipality-level data from the Atlas of Economic Complexity, estimating a regression analogous to equation 2 for municipal export growth at the market level.[23] As with homicides, we also provide results of DiD regressions in the Online Appendix.

Regarding the identification assumptions behind our empirical strategy, random assignment of close PAN wins is not enough to draw conclusions about the effects of violence on exports.[24] We need to show that the under-performance was not triggered by the election of PAN itself and the particular economic policies that the party advocates, but by the propensity to engage in the war on drugs and the ensuing violence that it caused.

[23] As in equation 1, we weight by population as of 2005 in the municipality level regressions. In the Online Appendix, we show results without weights and for a weight defined by population as of 2005 divided by the number of units (product-destination pairs) within a given municipality.

[24] In an IV setting, random assignment of a PAN win does not imply that the exclusion restriction is satisfied. A PAN win in this setting also impacts several dimensions of violence. Therefore, we prefer to show only reduced-form regressions. Nevertheless, our main export results persist if we use PAN wins as an instrument for total homicides. See Table A.1 in the Online Appendix. The comparison of the IV and OLS estimates highlights the importance of dealing with endogeneity in this setting.

To show that the effect is indeed driven by violence and not by the party's agenda, we perform two placebo tests during the period of the war on drugs. *Ex-ante* cartel presence and *ex-ante* high levels of violence were drivers of enforcement operations during the war. Locations with a PAN mayor but no cartel presence and low violence before the war were less likely to be the target of anti-drug operations. Importantly, they still experienced the economic policies implemented by the PAN. If in those locations a PAN win is not associated with a decrease in exports, then we can conclude that it is not the PAN victory itself that is causing our main result. We thus exploit heterogeneity in the potential intensity of the war on drugs by splitting our analysis in areas that experienced different levels of drug-related activity and violence before the war.

We explore the prevalence of pre-existing violence in the north of the country. Most of the drug-trafficking organizations operate in this region, where the points of entry to the US (the main consumer market) are located. Hence, we split our data into northern and southern municipalities. We complement the analysis by using data collected by Coscia and Rios (2012) on cartel presence at the municipal level in Mexico, splitting the sample using the presence of any cartel at the beginning of the drug war.[25] Finally, we also evaluate the effect of electing a PAN mayor in a close election in periods outside of the Mexican drug war.[26]

Finally, estimates of supply-side effects could be biased if local demand was affected by the Mexican drug war. Almunia et al. (2021) argue that negative local demand shocks can cause an increase in exports because short-term marginal costs decrease - a venting out channel. However, the context of their analysis - the Great Recession in Spain - is very different from the context of the Mexican drug war, and if anything,

[25] Coscia and Rios (2012) collect data from relevant web sources such as newspapers and blogs on Drug Trafficking Organizations (DTO) activities in Mexican municipalities using an automatized system. However, there are some limitations in the ability to collect information since powerful cartels can suppress it (Wainwright, 2016). This problem can be especially prevalent in badly governed municipalities. Using the measure directly to predict violence could introduce a bias. Nonetheless, it is unlikely that this potential bias is correlated with the close election outcomes. This is why the main source of variation that we use is still the close election result.

[26] The north/south and the cartel/non-cartel splits are not independent. In the Online Appendix, we show that 20 out of the 31 municipalities with pre-existing cartel presence are part of the north sample. We also show that municipalities in different groups can differ across socio-economic characteristics.

this channel would attenuate our estimates. Nevertheless, if financially constrained exporters use cash from local revenues to finance their exporting activity, or if the the production processes have economies of scale or scope, there could be negative spillovers from local demand to exports.[27] Noting that any changes in demand at the *country level* affect both treatment and control municipalities, for this channel to be relevant, revenues from sales to the municipality of origin must be significant. This is not likely the case in our setting. For instance, we find large effects on electronics and vehicles exports. Plants in these sectors operate at a high scale, so that sales to municipalities of origin are likely a small fraction of total sales. Furthermore, we explore the sectoral composition of aggregate effects in the economic census, finding negative effects of a close PAN win in manufacturing which are absent in non-tradable activities that are driven by local demand. Moreover, our main results are robust to including region fixed effects that capture changes to demand at a more local level. Therefore, we argue that our estimates identify a causal effect of violence on the local capacity to supply foreign markets.

Another potential concern is that our results are obtained during the "great trade collapse" that followed the financial crisis of 2007-2008. This fact is not a threat to the internal validity of our exercise as the source of variation is cross-sectional, and in our main specification we control for demand shocks using product-destination dummies. However, we cannot rule out a different result in an alternative context (external validity). Finally, if some of the exporting activity relocates to the control group, the stable unit treatment value assumption (SUTVA) would be violated in our setting, leading to an overestimation of the effect. We believe this is not a concern as the exporting activity would have to relocate to the restricted set of municipalities that had a close PAN loss (111 out of a total of 2454 municipalities in Mexico).

[27] Local demand can be relevant if, for instance, industries cluster in a given municipality.

2.5 Descriptive statistics

Table 1 reports summary statistics for municipalities that held elections in 2007 and 2008. Panel A shows socioeconomic characteristics of each Mexican municipality. In terms of population, municipalities are small. They have, on average, 35 thousand inhabitants, while the average county in the US has 100 thousand inhabitants. Furthermore, by 2006 Mexico was already a violent country in relation to the US. The American rate of 6 homicides per 100,000 pales in comparison to 11.7 in Mexico. However, compared to some Latin American countries, such as Brazil (26), Colombia (37), Venezuela (49), and El Salvador (58), Mexico's homicide rate was relatively small in 2006 (Berthet and Lopez, 2011). Although PAN was already an important party, only 27% of municipalities had an incumbent PAN mayor. Municipalities that elected PAN mayors are richer and less violent than municipalities that did not elect PAN mayors. However, once the sample is limited to municipalities where PAN won or lost by a small margin, the baseline characteristics are not statistically different in the treatment and control groups. Moreover, the loss of power caused by the restriction of the sample does not drive the results. For all significantly different variables in the unrestricted sample, we see smaller differences when we restrict to the 5% spread. The lack of difference on observables provides reassuring evidence in favor of the assumption of random assignment in close PAN victories.

Panel B of Table 1 shows the characteristics of trade variables of the firm-level data, while Panel C displays trade characteristics when we use the municipality-level data. Municipalities where PAN was elected tend to export more *ex-ante*, and to more markets, defined as product-destination pairs. In general, the differences are statistically significant for the unrestricted sample; for the sample that is restricted to municipalities facing close elections, all differences are not statistically significant. Yet, as some differences are still relatively large in economic terms, in the Online Appendix we show that results are robust when we estimate a local differences-in-differences model with year and municipality fixed effects.

In the Online Appendix, we show the geographical distribution of all municipal-

ities in which elections took place in 2007 and 2008. When we restrict the sample to municipalities with close elections, the distribution of PAN losses and wins are regionally dispersed. This is important for our identification because this undermines the possibility that regional shocks, and not the treatment, drive our results. Also in the Online Appendix, we present municipality characteristics of municipalities located in the north or with pre-existing cartel presence. In comparison to municipalities in the south or with no pre-existing cartel presence, these municipalities show a larger economic activity, according to measures such as GDP and exporting activity. Moreover, two thirds of municipalities with cartel presence are located in the north.

3 Effects on violence

3.1 Homicides

We report estimates of Equation 1 in columns 1-2, Panel A, of Table 2. The outcome variable is the annual average of homicides over the new incumbent's term. A close PAN victory in the elections of 2007 and 2008 causes an increase between 25.9 and 41.2 in homicides per 100,000 population. In municipalities with a close PAN loss, the homicide rate is 15.7. Therefore, a PAN win is associated with an increase between 65% and 162%. In Panel A of Figure 3, we plot the evolution of total homicides in municipalities with a close PAN win versus in municipalities with close PAN loss. Homicides increase sharply in both groups but the increase is more pronounced in municipalities with a close PAN win. Figure A.10 in the Online Appendix presents the graphical representation of the results.[28]

Part of the results obtained using the 2007 and 2008 elections could be the result of new policies implemented by the PAN in this period that were not related to the war on drugs. To rule out this possibility, we run the two contemporaneous placebo

[28]Our sample of close elections is slightly different from Dell (2015). In Dell's paper there is an additional restriction given by the availability of confidential data on drug transportation routes. In our paper this restriction is not necessary. Even with this difference, the results are very similar in economic magnitude and in statistical significance.

tests described in the empirical strategy (subsection 2.4). In columns 3-6, Panel A, of Table 2, we show that the effect of a close PAN win on the homicide rate is only present in the north of Mexico, with an effect of 43, and in municipalities with pre-existing cartel presence, with an effect of 44.9. In the south, the effect on the homicide rate is negative (-10.7) but not statistically significant, and in municipalities without pre-existing cartel presence, the effect is positive, but not statistically significant and with a smaller magnitude (11.7).

In Panel B of Table 2, we test whether a close PAN victory is associated with higher homicides in periods outside the war on drugs. We estimate the effects of a close PAN win for all elections between 2003 and 2011. Effects are mainly present in municipalities that elect a PAN win mayor in the 2006, 2007 and 2008 elections, that is, mayors with terms that overlap with the peak of the war. Outside this period, effects are much smaller in magnitude and lack statistical significance. This suggests that a PAN victory by itself did not cause higher violence at the municipality level. The main driver of violence was the combination of a PAN victory with the implementation of the war on drugs.

Table A.5 in the Online Appendix reports the same regressions when we restrict the sample to municipalities where PAN won or lost by different margins (7%, 6%, 4% and 3%). The results are consistent. Results are also robust to increasing the degree of the RD polynomial.

3.2 Other crimes

A natural question is whether the incidence of other types of crime also increased. Given how the drug war triggered inter-gang competition for areas experiencing government crackdowns, it is plausible for such increased gang presence to induced a spike in criminal activities beyond homicides. Increased competition and drug enforcement might also lead gangs to seek revenues in other criminal activities. There are some limitations in documenting the effects on other crimes. Data is noisier due to under-reporting. Furthermore, the most reliable source publishes crime statistics per munic-

ipality only from 2011. Therefore, differently from the homicides case, for which we could assess the impact over the mayoral term, we can only test the impact on the level observed in 2011..

Table 3 reports results for six different types of crime. A close PAN win in 2007 and 2008 is associated with higher levels of extortion, robbery that target individuals and firms, displacement and property damages in 2011. Effects are stronger in the north sample and in regions with pre-existing cartel presence. We find no effect on kidnappings. For personal injuries, effects are confined to the north sample and to regions with pre-existing cartel presence.

To test whether this effect is restricted to a close PAN win during the war period, we perform two tests. First, we use the 2004 and 2005 elections. Ideally, we would like to test the effect on other crimes in 2008, but since the data are available from 2011, we study the effect on the level in 2011. Table A.6 in the Online Appendix shows that a PAN win in those elections is in general not associated with higher levels of crime in 2011. On the contrary, for certain types crime, a PAN win is associated with lower levels of crime in 2011. We also run a test using the 2010 and 2011 elections on crime in 2014. Most of the term of mayors elected in those years took place after the war on drugs. We also find no impact.

The results suggest that a close PAN win during the war on drugs is associated with higher levels of homicides and other crimes. We also find effects on crimes that affect firms directly, such as extortion, robbery and property damages.

4 Economic effects

4.1 Motivating evidence

A first question regarding the economic effects of violence in this empirical setting is whether local production growth was eroded by the assignment of a close PAN mayor. Table 4 shows that we cannot detect an overall effect with sufficient precision. However, separating economic activities uncovers an important heterogeneity. While no effect

is detected for non-tradable activities, production growth in manufacturing activities dropped in areas marginally exposed to a close PAN mayor.

This heterogeneity is consistent with survey evidence from the Mexican Enterprise Survey from 2006 and 2009. This survey provides spatial and industrial heterogeneity in a number of important measures regarding firms' operations and perceived constraints on economic activity right before the start of the Mexican drug war (2006) and at its peak (2009). We focus on the change in firms responses in the north of the country - where the bulk of the violence associated with the Mexican drug war occurred -, and evaluate the heterogeneity between manufacturing firms and those operating in other sectors.[29] Panel A in Table 5 shows responses regarding crime as an obstacle to firms' operations. As we can see, the growth in the crime obstacle score was strongest in the north for manufacturing firms (columns 1-3), and the growth in the number of firms identifying crime as their main constraint is also greatest for manufacturing firms (columns 4-6). Panel B provides estimates on firms' time costs of dealing with regulatory hurdles and on whether firms identify Courts as a mayor obstacle. These results suggest that concerns over these issues did not grow disproportionately in the north, and show no detectable heterogeneities between manufacturing and other firms.

Seeing how these results point towards manufacturing activities hurting disproportionately from the increase in violence, we now look at the change in the exports of goods from the restricted sample of municipalities with close elections of PAN mayors, as in this data we will be able to better control for domestic and international demand factors by comparing exports of the same goods to the same countries originating from municipalities exposed to different levels of violence during the Mexican drug war.

4.2 Main results

Table 6 shows estimates of Equation 2. Yearly firm-level export growth drops by 8% over the 3-year term of PAN mayors elected in close elections in the years 2007 and

[29]The Enterprise Survey is mainly urban and excludes firms operating in the primary sector of the economy. Hence, manufacturing firms are bench-marked against firms operating in non-tradable industries - basically commerce and construction.

2008. The estimate increases to 14% when we control for a linear RD polynomial on both sides of the discontinuity, while the inclusion of destination dummies does not change the magnitude of the effect. In our our preferred specification, which includes controls for foreign demand shocks by including product-destination dummies, the effect increases in magnitude to 21%. As a comparison, the yearly firm-level export growth of firms in municipalities that do not elect a PAN mayor is 3%. The drop in export growth is persistent, as it does not vanish in the next mayoral term. When extending the period to 6 years (two mayoral terms), we find that firm-level export growth drops by 13% annually.[30] Because the firm-level data do not include the universe of exports, we also run the same regressions on a sample of municipality-level exports and verify our results are not driven by sample selection. Table 6 also reports estimates at the municipality level. Results show the same patterns, albeit with slightly smaller magnitudes. Yearly municipality export growth drops by 15% over 3-years and do not vanish in the next mayoral term, dropping by 8% yearly over a 6-year period.[31]

A potential concern is that our results might be driven by policies implemented by PAN mayors that are not related to the drug war. To show this is not the case, we estimate a series of placebo regressions. In Table 7, we estimate the contemporaneous placebos using the north vs south split and the pre-existing cartel presence vs no pre-existing cartel presence split.[32] Results lose precision with the firm-level data when we restrict the sample to municipalities in the south or with no pre-existing cartel presence. However, the municipality-level data confirms that the negative effect on exports is only present in the regions that experienced a surge in violence: municipalities in the north and with pre-existing cartel presence. In municipalities in the south or with no pre-existing cartel presence, a close PAN win is associated with a positive export growth, albeit only statistically significant in the sample of municipalities with no pre-existing cartel presence.

A relevant point is that we cannot explicitly address the role of drug smuggling.

[30] We cannot test the effect over a longer period since 2014 is the last year of our data.

[31] In the Online Appendix, we show that results are robust for different weighting choices.

[32] The north-south segmentation was determined by the median latitude among the municipalities with close elections.

It can be tempting to interpret a decrease in export growth as a natural consequence of the effectiveness of the Mexican Drug War. We provide indirect evidence that this is not a likely explanation. In a non-reported regression we separate the effect across Mexico's main trading partners. The negative effects are similar when we restrict to destinations where drug smuggling from Mexico is a problem such as US, and the EU. The results are in line with the decrease in export growth to other destinations such as China. This suggests that a decrease in drug smuggling is not the main cause of the decrease in export growth.

In Table 8, we analyse whether the negative effect of a close PAN win is only contingent to the war on drugs period. Exploring the fact that our data ranges from 2004 to 2014, we compute the effect of a close PAN win over time. We start with the 2004-2005 elections, and finish with the 2010-2011 elections.[33] The results show that a PAN win is not associated with a decrease in exports outside of the war period. Figure 4 presents these results graphically. Results are similar using the municipality-level data (Tables A.10, A.15 and A.20 in the Online Appendix).

4.3 Extensive margin

Differences on the intensive margin alone might not be representative of changes in total exports. The extensive margin, that is, the number of relationships between firms and markets (defined here as product-destination) might also be of relevance. For example, if the number of relationship appearances decrease or the number of relationship disappearances increase with a close PAN win, the intensive margin results might be underestimating the effect of violence on exports. Moreover, comparing extensive and intensive margin results sheds light on the cost nature of violence, that is, whether it affects marginal or fixed costs.

Table 9 evaluates the effects of a marginal PAN victory on the probability of a firm losing an export relationship with a foreign country for a given product. For the entire sample of municipalities with close elections, results show a negative (that is, a

[33] We always use close elections in two consecutive years to increase sample size.

PAN win is associated with less disappearances) but not statistically significant effect when product-destination dummies are included. These results remain largely the same when we evaluate the change in the number of single-plant firms supplying each market from a given municipality (Table A.7 in the Online Appendix). In the context of the negative and significant effects observed at the intensive margin, this evidence suggests that firms adapt to the increasingly violent environment by reducing the intensity of their ongoing export relationships, but not by disproportionately rescinding on these relationships. This finding can be interpreted as consequence of increasing marginal costs of exporting, assuming there exist fixed and sunk costs of developing export relationships.[34] The lack of effects on exit decisions suggests fixed costs of exporting did not change as violence increased during the Mexican drug war.

Table 9 also shows results for the north vs south and cartel vs non-cartel splits. With the exception of the north, where effects are negative and statistically significant, results are unchanged. To asses the impact on total exports, we perform two exercises. First, in Panel B of Figure 3, we show that aggregate exports (which includes relationships appearances and disappearances) in close PAN win and close PAN loss municipalities grow at a very similar rate 3-years before the election. After the close election we observe a divergence: total exports of municipalities with a close PAN win experience smaller growth. We see a large short-term effect, clearly pronounced in the year 2010. The figure also shows the persistence of the effect: the difference in growth remains in 2014.

Then, in the Online Appendix (Tables A.2 and A.3) we provide descriptive statistics on the values of exports that come from the appearance of new relationships and from the disappearance of old ones. We can see that continuing relationships (the intensive margin) account for the bulk of exports, highlighting the importance of our intensive margin results.

[34]For a theoretical motivation behind the margins of adjustment, see Melitz (2003), and for an estimation on the relevance of each method of adjustment in trade, see Helpman et al. (2008).

4.4 Robustness and comparison with DiD regressions

In the Online Appendix, we show that results are robust to different bandwidths and degrees of the RD polynomial. In the main regressions, we avoid using region or state fixed effects because they are not important for the identification of the effect of a close PAN win on violence. In particular, possibly due to spillovers, effects on violence are severely reduced. However, in the reduced-form regressions, these fixed effects could help us to control for local demand: instead of controlling implicitly for country-level demand shocks, we now control for region or state demand shocks. We perform these exercises in the Online Appendix. When the first-stage survives to the inclusion of these fixed effects, we still observe a drop in exports. Using the Economic Census, we also find smaller effects for non-tradable sectors that are arguably more affected by local demand.[35]

We also provide additional evidence on the importance of our control for foreign demand. In Table 6, we show that if we do not control for foreign demand, we obtain a coefficient of -0.18 in the municipally-level regressions. Once we add controls for foreign demand, the magnitude drops from -0.18 to -0.15. However, this reduction in magnitude can be due to omitted variable bias or sample selection, because singletons are dropped once we include product-destination dummies. To assess which channel drives the results, in the Online Appendix we run the regression without foreign demand controls on the sample without singletons (Table A.8). The coefficient increases in magnitude to -0.20, meaning that sample bias, if anything, moves the coefficient in the opposite direction, and showing that omitted variable bias plays an important role.

Furthermore, we show that our results are present in differences-in-differences regressions using close PAN wins as the treatment and show that results are unchanged. To see how our empirical strategy deals with the endogeneity of the location of enforcement operations, we collect data on the location of federal operations and use this information to classify treated municipalities. We find that operations lead to a positive effect on homicides, but null or even positive effects on exports. We argue that

[35] We describe the Economic Census data in detail in Section 6.

these results are biased because, differently from a close PAN win, the deployment of law enforcement operations is not exogenous. We present these exercises in the Online Appendix.

A potential concern regarding the interpretation of our results is whether the results are driven by ordinary exporters or by export-processing firms. This is an important qualification, as Lu (2010) and Dai et al. (2016) have found that export-processing firms are less productive and less capital intensive than domestic producers in China. Given the existence of export-processing firms in Mexico it could be reasonable to consider whether the effects are driven by export-processing firms in our setting. Our micro-level export data does not allow us to distinguish ordinary exports from export-processing exports. Hence, we rely on aggregate census data, which offers municipality manufacturing revenue information, identifying the portion of revenues that derive from export-processing activities ('maquila'). It is important to mention that 'maquila' activities only account for 14% of total revenues, making it unlikely for them to drive these negative effects. Table 4 shows negative effects of the MDW in revenues in production and revenues in the manufacturing sector, which remain unaffected after excluding 'maquila' revenues. Therefore, it is reasonable to assume the effects are not driven by export-processing firms.

4.5 Aggregate effects

A final question is whether the drug war resulted in a decrease in total exports. Aggregating results has two downsides: (i) we cannot control for demand shocks using product-destination dummies, leading to composition effects due to a small sample and remaining (yet not statistically different) differences in baseline trade activity (this is specially important as the sample period coincides with the fallout of the 2007-2008 financial crisis); and (ii) sample size decreases.

Our estimates show a large effect if we aggregate our firm-level data across markets. Table 10 evaluates whether violence exposure affects total exports of single-plant firms, finding that firms assigned to a close PAN mayor experienced an export growth ratio

50% lower. Moreover, these results are larger and more precise in municipalities with baseline cartel presence and in the north. Results of the RD estimation using the data at the municipality-level data are less precise. A local DiD analysis, however, confirm negative effects (see Online Appendix). Finally, Panel B of Figure 3 shows that total exports coming from municipalities with a close PAN victory and from municipalities with a close of PAN defeat share a similar trajectory until 2007; however, after 2007, total exports from municipalities with a close PAN win start to grow less.

5 The economic victims of violence: effects across product and firm characteristics

So far we document a negative effect of violence on the local capacity to supply foreign markets. We now discuss the mechanisms through which violence may have eroded local export growth. There are many possible channels connecting violence to firms' under-performance. First, predation may force firms to reallocate resources from production towards protection.[36] Violence may also work as a local disamenity, conditioning the hiring of workers.[37] Violence may also erode the capacity to attract capital for new projects and existing activities, affecting capital-dependent activities disproportionately. Finally, crime may disrupt coordination-intensive productions processes that rely on the transportation and assembly of fragmented and complementary inputs.[38]

In order to assess the relevance of these channels, we study which production char-

[36]This mechanism underlies canonical interpretations on the economic effects of violence and crime, such as Tullock (1967). Besley and Mueller (2018) estimate macro-level effects of insecurity building on Enterprise Survey microdata from different countries. For Mexico, they find that large firms are disproportionately affected by predation.

[37]Violence may operate as a local disamenity by increasing the perceived probability of being victimized. To achieve between-city spatial equilibrium, local disamenities should lead to migration and to a relative increase in real wages.

[38]Firms relying on transportation services outside the scope of private protection efforts may be more vulnerable to criminal predation. Coordination-intensive production may also be relatively vulnerable. According to "O-Ring" models of production with complementary tasks (Kremer, 1993), output depends on the probability of success in such tasks. From this perspective, violence could be seen as decreasing the probability of success in all tasks. As a consequence, complex production in activities that rely on a higher number of tasks could be disproportionately affected by violence, as they face a relative increase in the probability that any of its relevant tasks fails.

acteristics associate with a stronger erosion in export growth. Specifically, we analyze the heterogeneity of the effects of violence by economic sector, exporter size, capital and skill intensity, finance dependence, transportation dependence and input and occupation fragmentation. We find that large exporters and exports of capital dependent products were hit hardest. These results are consistent with an increasingly predatory environment that disproportionately affects larger exporters and erodes the local capacity to attract capital investment. These results are largely inconsistent with the transportation/coordination channel.

To further study the mechanisms through which the Mexican drug war may have affected economic outcomes. We evaluate the effects of violence on the prevalence of private protection services, on capital accumulation and greenfield FDI, and on labor costs. Our evidence confirms the relevance of the predation and capital-attraction channels, as we observe an increase in the number of security providers, a decrease in asset accumulation and a drop in capital expenditures in areas with a narrow PAN victory. Our results are also inconsistent with the disamenity channel, as a close PAN victory led to a drop in wages.

5.1 Heterogeneity: firm and product characteristics

We first explore who the economic victims of violence are, evaluating how the effects of the Mexican drug war concentrate across economic sectors and exporter characteristics. Figure 5 shows the marginal effects of a PAN win on firm-level export growth by broad product categories. The most affected product categories are textiles, vehicles and electronics. The latter two represent some of the largest, most advanced and capital intensive product categories in the Mexican export mix.

We study the heterogeneity in the effects of violence along firm and product characteristics. For exporter characteristics, we split our data around the median employment size and average wage paid by Mexican exporters in 2007. Similarly, we split our sample at median product values for different measures characterizing production processes

and technologies.[39] The measures were selected to be indicative of the potential channels through which violence might be affecting export growth. Specifically:

- Product Complexity: This metric from Hausmann et al. (2014) empirically approximates the productive capabilities required to export a product competitively from a given country. Competitiveness in complex products associates with faster economic growth at the country level. Table A.28 in the Online Appendix reports the 10 products with highest and lowest complexity.
- Capital dependence: This metric from Shirotori et al. (2010) captures the Revealed Capital Intensity of a given product from international trade patterns and national capital endowments of competitive exporters.
- Human capital dependence: Also from Shirotori et al. (2010), this measure captures the Revealed Human Capital Intensity of a product from international trade and national human capital endowment patterns.
- Finance dependence: This metric from Rajan and Zingales (1998), measures a product's dependence on external capital for its production. Cash crops with fast turnaround – like tobacco – are in the bottom of the finance dependence list, while sectors that require long-term risky investments and higher working capital – like drugs and medicines – are in the top of the list.
- Trucking dependence: We build this metric as the share of trucking services in a product's input mix, as measured in the US input-output tables.
- Input fragmentation: We calculate this metric as the inverse of the Hirschman-Herfindahl index of concentration in input shares for each product, as measured in the US input-output tables.
- Occupation fragmentation: We calculate this metric as the inverse of the Hirschman-Herfindahl index of concentration in the employment of an industry across occupations, as measured in the US input-output tables.

[39] All these measures are converted into the 1992 version of the Harmonized System of product classification. Some products in our data cannot be matched to the relevant scores, which alters the overall sample size in some of our specifications.

Table A.29 in the Online Appendix shows the correlations between all product-specific characteristics. We observe a positive and high correlation between complexity, capital and human capital dependence. This is expected, as complexity captures the specificity and diversity in the capabilities required for an economic activity.[40] Likewise, if there are complementarities between long-term capital and human capital, it is natural that both measures are highly correlated. External finance and occupational fragmentation are positively but less strongly correlated to each other and to complexity, capital and human capital dependence. Finally, transport service dependence and input fragmentation are highly correlated with each other, but largely orthogonal to all other measures.

Table 11 shows estimates of the effects of a PAN victory in a close local election in 2007 and 2008 on export growth, conditioning for exporter and product characteristic groups. Results in Panel A suggest that the negative effects of the war on drugs are more detrimental for more complex and larger exporters. Firms exporting high complexity products from close PAN win municipalities suffer a 27% decrease in export growth. The results are not significant for low complexity products. A PAN win leads to a 27% decrease in export growth for large exporters, while small exporters suffer no significant change. We observe some evidence of stronger effects of violence on firms paying relatively lower wages.

Panel B shows results around the capital, human capital and external finance dependence margins. Firms producing capital intensive products suffer a drop of 32% in export growth. There is no statistically significant effect in products with low capital intensity. Firms exporting products with high-skill dependence experienced a decrease of 27% in export growth, while the estimate for products that require low skill levels is smaller and not statistically significant. Regarding external finance dependence, we find a 21% drop in export growth associated with a marginal PAN victory. While the estimate for low-finance dependence products is not significant, magnitudes are simi-

[40] See Hausmann et al. (2014). Table A.27 in the Online Appendix shows that exporters that had a complex product as main export in 2007 have a larger workforce, pay higher average wages and export larger amounts.

lar to high-finance dependence products. Overall, results in Panel B are most consistent with violence affecting exports in capital-dependent products.

Panel C shows estimates around the transport dependence, input fragmentation and occupation fragmentation dimensions. Our results suggest that exports most dependent on trucking services were not affected by a marginal assignment to a PAN mayor, while exports least dependent on such services were greatly affected. Similarly, products with low input fragmentation seem to have been affected most by the war on drugs. Finally, there seems to be no heterogeneity across products' level of occupation fragmentation.

Observing stronger negative effects of violence for larger firms, for complex exports, and for capital dependent products is consistent with the predation and capital attraction channels. It is aldo consistent with the findings of (Besley and Mueller, 2018), who find that larger firms in Mexico are more affected by predation and misallocation from production to protection. Moreover, firms may be facing higher costs for procuring labor, and capital dependent activities may be hurting disproportionately from investment reductions in violence-affected regions. We explore these potential mechanisms in the next section. Importantly, observing stronger effects of violence on sectors with concentrated inputs or with low trucking dependence, along with balanced effects across the occupational fragmentation dimension, is inconsistent with the coordination-transportation channel.

6 Mechanisms

In this section, we proceed to evaluate whether the Mexican drug war altered local labor costs, local capacity to attract capital investment and the local prevalence of private security services.

6.1 Effects on labor costs

Violence can act as a disammenity at the local level, leading workers in more dangerous environments to demand an income premium and thus imposing an additional burden on firms. This would be especially relevant for workers that have better outside options.[41] On the other hand, violence can also make labor less productive by increasing absenteeism or by reducing capital availability. This effect on the marginal productivity of labor would express itself by a drop in salary growth. To study the prevalence of these channels, we collect data on the 2000 and 2010 Mexican population censuses.

We separate workers in skill groups based on their educational attainment. In Mexico, high school requires 12 years of accumulated approved years of studies. A college degree requires on average 17 years of accumulated approved years of studies. We define *low skill* workers as those that do not have a high school degree or equivalent (i.e., less than 12 years of schooling), *high skill* as those that have at least a university degree (17 or more years of schooling), and *medium skill* as those that have between 12 and 17 years of schooling. The census data also allow us to split individuals into workers employed by a firm, self-employed and business owners. This split is relevant because the income premium is mostly present in the first group.

In Table 12, we study the effect of a close PAN win in the 2007-2008 elections on income growth between 2000 and 2010. For workers, we find no effect for the high skill group. For both the middle and low skill groups of workers, the effect is negative and significant, and the magnitude is larger for the low skill group. This result is consistent with the idea that high skill workers are more difficult to replace and thus might demand a premium to remain in regions affected by surges in violence, while middle and low skill groups are easier to replace and thus experience a smaller salary growth. For non-workers, all skill groups are negatively affected. Moreover, differently from the results on workers, the high skill group of non-workers is the group that suffers

[41] Part of the (real) income premium can also come indirectly in the form of lower prices, such as real estate or product prices (Rozo, 2018). We do not have data on local prices to test this channel.

the largest drop in income growth.

Table 12 also reports the results for the north-south and cartel-non cartel splits. The effects are mostly present in municipalities that had pre-existing cartel activity or located in the north, where a PAN win increased violence during the drug war period. The table also shows the results of the effect of a PAN win in the 2004-2005 elections. In this electoral cycle the war on drugs was not implemented. For these municipalities, a PAN win is associated with an increase in salary growth. These heterogeneities show that these negative effects are likely caused by the war on drugs, and not other policies implemented by PAN. In the online appendix we estimate the effects of a close PAN win on migration patterns. We find no evidence that a close PAN win is associated to an increase in emigration or a decrease in immigration. Overall, these results suggest that, for low and middle skill workers, the productivity channel dominates the disamenity channel in explaining the effects of violence on labor costs during the Mexican drug war. For high skill workers, salaries do not grow less in comparison to the control group, suggesting that firms that rely on this type of labor as an input are more adversely affected.

6.2 Effects on local firms' capital and employment decisions

We now study how local aggregates of firms' capital accumulation decisions were affected by the Mexican Drug War. We use the 2003, 2008 and 2013 waves of the Mexican Economic Census.[42] Even though the war started in 2006 and the peak of violence occurred in 2010 and 2011, given the data restrictions, we use the growth of capital accumulation variables between 2008 and 2013 to learn about the effects of the drug war. We pursue similar placebo strategies, expecting the effects of a PAN win in 2007-2008 only in areas with baseline cartel presence and in northern Mexico. Moreover, we expect that PAN victories in 2003-2004 do not yield similar effects in the growth of firms' outcomes between 2003 and 2008, with the caveat that disruptions started in 2006.

[42]The Mexican Economic Census is a census of productive units outside of agriculture activities. It is run every five years by the Mexican Statistics Authority (INEGI), which publishes aggregate results at the wave-municipality-industry level.

Regarding capital accumulation, Panel A in Table 13 shows the effects of a PAN win on local investment levels. We observe negative effects, but only statistically significant at 10%, on the full sample. Splitting the sample between northern and southern Mexico reveals a negative effect of a PAN win on investment in the north, but a positive effect in the south, where a PAN victory did not lead to a spike in violence. We find negative but not statistically significant effects in municipalities with baseline cartel presence. The effect of a PAN win in the placebo period is positive and not statistically significant. With regards to fixed capital formation, Panel B shows large negative and significant effects of a PAN victory, which concentrate in cartel areas and in northern Mexico, and are absent for the placebo period. Panel C shows results for the value of fixed assets. The result is negative but not statistically significant in the full sample, but in the north the effect is negative and statistically significant.

We also use the Economic Census to study employment outcomes. Panel D shows no statistically significant effects of a PAN win on overall employment growth, while Panel E shows negative effects of a close PAN win on average wage growth. This negative effect concentrates in northern municipalities and in municipalities with pre-existing cartel presence. In Table A.24 of the Online Appendix, we study effects on the average wage growth of blue collar and white collar workers.[43] In the full sample, we find larger negative effects for white collar workers. However, the north-south and the cartel- non cartel splits provide less clear results.

Overall, these results confirm our findings on the effects of violence on investments and on capital-intensive exports. Firms in locations exogenously exposed to the violence triggered by the Mexican Drug war invested less and reduced their fixed capital accumulation. On the other hand, we do not find effects on overall employment, but find negative effects on average wage growth, disputing the disamenity channel connecting violence to labor market outcomes. We also find larger effects for white collar workers. Although the samples and dates used to measure salary growth are different,

[43] According to the Mexican Economic Census, blue-collar workers refer to "production, services and sales personnel", while white collar workers refer to "directors, administration and accounting personnel".

if white-collar workers are the more skilled individuals, these effects contrast with the results documented in the previous subsection.

6.3 Effects on greenfield investment CAPEX

Another channel possibly connecting violence and export under-performance during the Mexican Drug War is the erosion in the local capacity to attract external capital. Assessing this hypothesis requires yearly local investment data.We use data from fDi Markets, a Financial Times' service with a comprehensive database of crossborder greenfield investments worldwide. fDi Markets collects information on the capital expenditures (CAPEX) in all new investment projects, as well as the number of new jobs created.

We first look at greenfield projects located in the municipalities with close elections. Panel A in Table 14 shows regression discontinuity estimates of the effect of a PAN victory in 2007/2008 on the CAPEX, number of new jobs and the capital per worker of the average greenfield investment between 2007 and 2012, and between 2007 and 2010. The CAPEX of the average greenfield project was $178 million lower under closely elected PAN mayors. There was no statistically significant effect of a PAN mayor on the number of jobs created, and the average capital per new job was reduced by $650,000. Panel B shows similar estimates for greenfield investments between 2004 and 2009 and between 2004 and 2007 in municipalities with close elections between 2004 and 2005, finding either a null or positive effects of a closely elected PAN mayor on the average CAPEX of greenfield investments.

We then build an aggregate dataset of the CAPEX, new jobs and number of new projects received by a Mexican municipality between 2007 and 2012. After restricting our sample to municipalities with close elections in 2007 or 2008, we retain CAPEX data for 14 municipalities.[44] Panel A of Table A.22 in the Online Appendix provides difference in means and regression discontinuity estimates, showing that a PAN vic-

[44]The low sample size is due to the fact that fDi Markets did not document any greenfield investment between 2007 and 2012 for most municipalities with close elections in 2007/2008.

tory in 2007/2008 associates with a drop of $5,740 in CAPEX investments per capita.[45] We find no statistically significant effects on the number of new jobs or projects. Panel B shows no statistically significant effect of a PAN victory in 2004/2005 on the CAPEX per capita, new jobs or new projects received by municipalities between 2004 and 2009.

These results suggest that municipalities marginally exposed to violence had a harder time attracting external capital for new productive activities, but attracted about the same number of new projects and jobs. Investors seem to have committed less capital to their new productive projects in more violent areas. If existing exporters reacted as greenfield investors did and reduced capital commitments on their operations, the negative consequences of violence on exports would be greater for capital-dependent exporters. This is consistent with the heterogeneous results documented before.

6.4 Effects on private provision of security services

Violence can erode economic activity by displacing local resources away from production, as insecurity leads economic agents to increase their spending on protection services. Through this "predation" channel, the violence increase would cause an increase in the presence of private security providers. This channel was studied by Besley and Mueller (2018), who find stronger effects for larger firms in Mexico. This result is consistent with our finding that larger firms are more affected.

We do not have data on protection expenditures, such as guards, equipment, fences, etc. We provide indirect evidence using the population and economic censuses. In Table 15, we use occupation data from the population census to assess the growth in the number of guards and their salaries as a function of a marginal PAN victory. Even though salaries grow less in PAN win municipalities, which is consistent with the findings documented previously, we do observe an increase an the number of guards, especially in areas with pre-existing cartel presence. In the Online Appendix, we use the economic census to study the effects on the number of plants, number of employees

[45] We provide the additional difference in means estimates as a more flexible specification of the model, given the low sample size. While the magnitude of the effect on CAPEX is smaller ($2,630 per capita), the conclusion that a PAN victory associates with lower CAPEX only in the period of the Mexican Drug War remains.

and in the average wages in the security industry. Because the sample size drops dramatically in this exercise, the results are only suggestive. We find an increase in the number of plants and an increase in the number of employees working with security monitoring services.

7 Conclusion

The Mexican Drug War has drawn widespread attention because of the scale of its consequences. We confirm the results in Dell (2015), who provides evidence that homicides increase disproportionately in municipalities where the rollout of war efforts was supported by PAN mayors. We take a step further and assess how the Drug War affected the real economy. We document a negative change in trade patterns, with export growth decreasing significantly after a close PAN win. We interpret our results as evidence of external effects of violence, since they are not observed outside the temporal and geographic context of the Drug War.

By leveraging from close elections and comparing exports of the same product to the same destination, we are able to disentangle effects on supply from demand factors, and study how violence affects the capacity of firms to serve external markets. Hence, we provide a methodological contribution to identify the economic effects of violence. We also provide new evidence on the relationship between violence and trade. Using firm-level microdata, we find that firms locating in a municipality that was exposed to a PAN mayor faced lower export growth rates, but we do not find a higher probability of firm exit from product-country markets. This is consistent with the view that violence increases the marginal costs of exporting, but does not affect significantly the fixed costs of sustaining trading relationships.

A key objective in this study is to identify the channels connecting increased violence to economic outcomes. First, we find that large exporters and exports of complex and capital-intensive products were affected disproportionately. Second, we find a reduction in capital accumulation and wages, and we find suggestive evidence that vio-

lence led to an increase in the number security service providers. These results suggest that, as the roll out of the war led to local predatory environments, firms mobilized resources from production to protection and limited the inflows of capital, reducing productivity and disproportionately affecting capital-intensive activities and large exporters.

Our main results suggest that violence can negatively affect the capacity of local economies to supply foreign markets. Importantly, the increase in violence was a consequence of a government policy. In the case of Mexico, the policy did not only cost lives, but damaged large firms and capital-intensive activities, both of which are key for economic growth.

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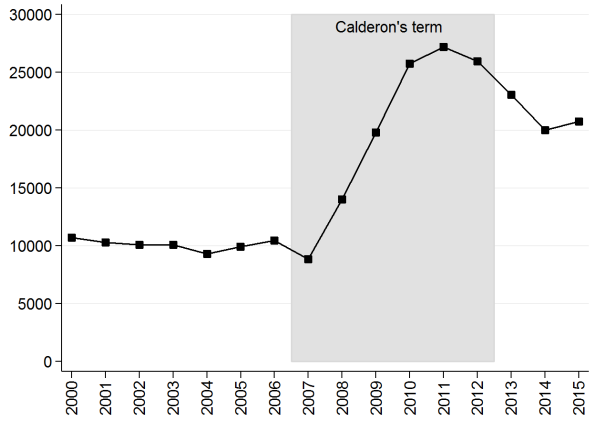
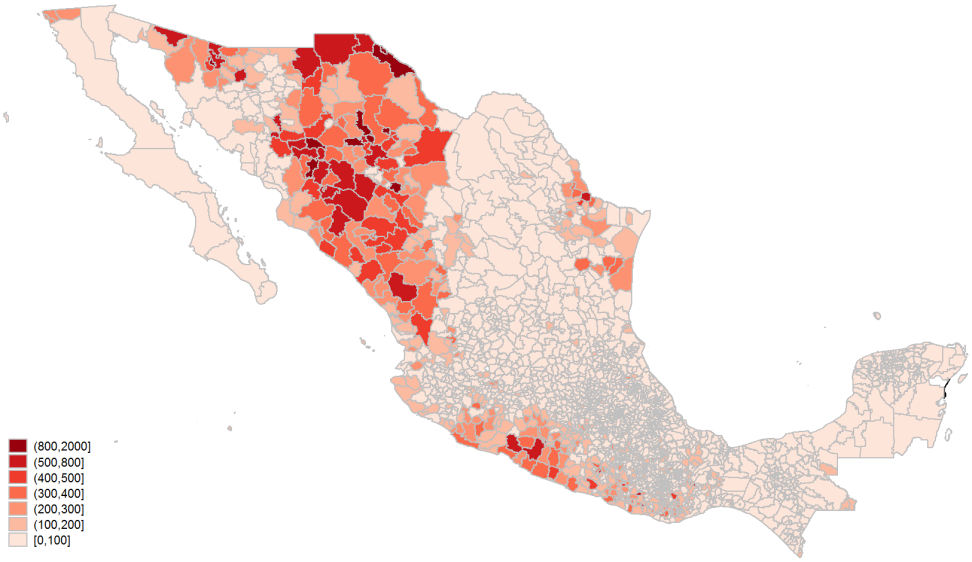
Figures

Figure 1: Annual homicides



Notes: This figure shows the time series of total homicides in Mexico. The grey area shows total homicides during Calderón's presidency.

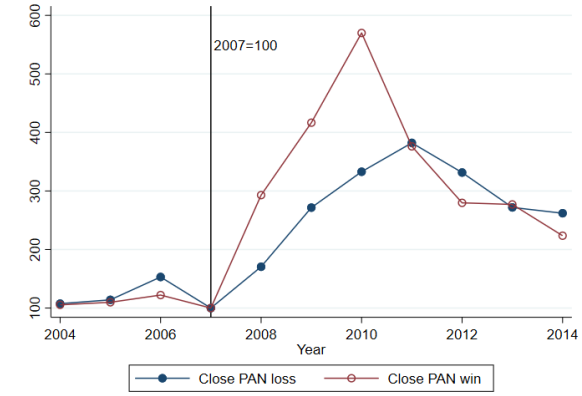
Figure 2: Spatial distribution of homicides



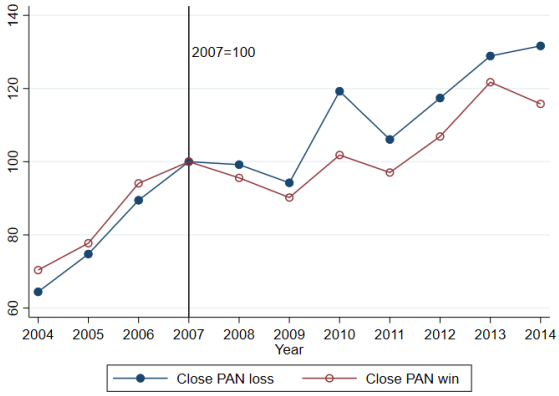
Notes: The figure depicts the geographical distribution of total homicides between 2007 and 2011 per 100,000 inhabitants.

Figure 3: Evolution of total homicides and total exports in municipalities with close PAN elections

Panel A. Homicides

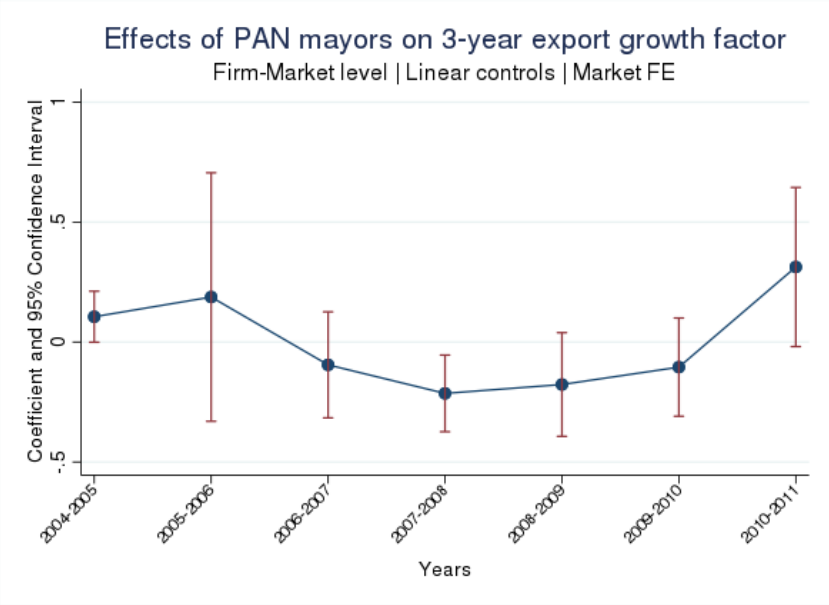


Panel B. Exports



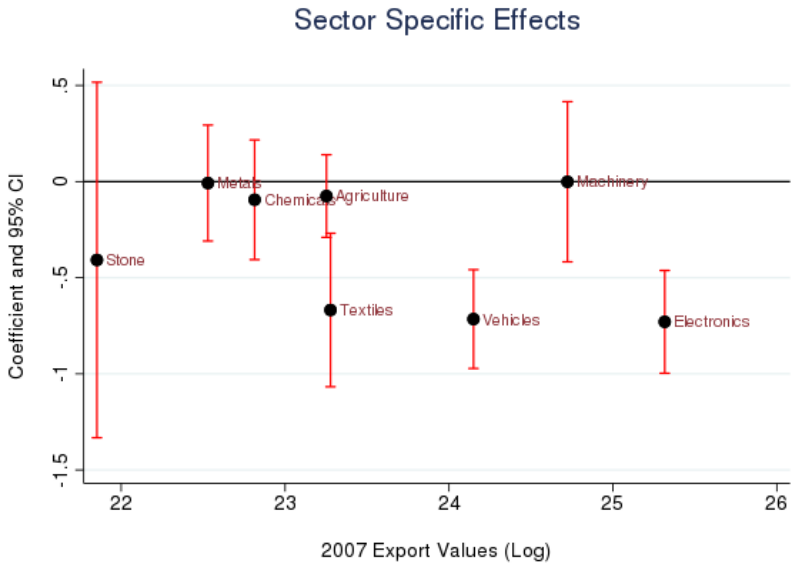
Notes: Panel A depicts the evolution of total homicides in municipalities where PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections. Panel B depicts the evolution of total exports in municipalities where PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections.

Figure 4: Effects of a PAN mayor in different periods: 3- year growth



Notes: RDD estimates of the effect of a close local PAN victory in each election window on the 3-year log export growth are presented in the y-axis. Confidence intervals are presented at a 95% level. The data for exports is formed by triples of firm, product, and country of destination.

Figure 5: Sector specific effects



Notes: RDD estimates of the effect of a close local PAN victory on the log export growth in each product category are presented in the y-axis. Confidence intervals are presented at a 95% level. The log of total exports from single-plant firms for each category in 2007 is presented in the x-axis. The data for exports is formed by triples of firm, product, and country of destination.

Tables

Table 1: Baseline characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	Total Sample			Spread 5%		
	PAN won	PAN lost	P-val.	PAN won	PAN lost	P-val.
Panel A: Sociodemographic characteristics						
Population 2005	38396	34270	0.54	59232	42934	0.44
	(126163)	(89949)		(190580)	(103344)	
Population density	162.9	149.4	0.61	209.6	188.14	0.75
(2005)	(385.1)	(380.8)		(465.8)	(466.3)	
PAN incumbent	0.28	0.26	0.49	0.31	0.32	0.84
	(0.45)	(0.44)		(0.47)	(0.47)	
GDP per capita	5996	5683	0.09	6085	6228	0.74
(USD, 2005)	(2942)	(2613)		(3360)	(2759)	
Mean years of	6.1	5.9	0.16	6.1	6.1	0.97
schooling, 2005	(1.4)	(1.4)		(1.4)	(1.4)	
Homicide rate	9.3	12.3	0.04	12.03	12.6	0.86
(2006)	(19.1)	(21.1)		(27.8)	(21.6)	
Observations	257	1159		87	111	
Panel B: Trade characteristics, municipality aggregates of firm-level data						
Total exports, 2006	402.9	139.4	0.15	865.6	484.0	0.61
(in millions USD)	(2392)	(1109)		(3751)	(1754)	
Exports: number	7.7	7.4	0.87	10.9	10.6	0.94
of countries	(14.2)	(11.9)		(20.0)	(15.2)	
Exports: number of pairs	97.9	61.4	0.19	191.9	123.8	0.53
product-country	(351.7)	(175.4)		(526.2)	(280.6)	
Exports: number of	30.3	13.1	0.04	62.1	21.0	0.25
exporters	(120.1)	(41.0)		(186.8)	(49.6)	
Observations	87	286		32	31	
Panel C: Trade characteristics, municipality-level data						
Total exports, 2006	194.7	54.7	0.03	450.4	168.9	0.29
(in millions USD)	(1580.3)	(709)		(2528.9)	(1104.5)	
Exports: number	19.5	18.9	0.71	22.6	22.6	1
of countries	(22.5)	(19.3)		(27.2)	(23.6)	
Exports: number of pairs	115.8	69	0 .04	211.3	120.5	0.31
product-country	(533)	(260)		(803.2)	(449)	
Observations	257	1159		87	111	

Notes: In Panels A and C, columns 1-2 report means for all municipalities in which elections occurred in 2007 and 2008, while columns 4-5 restrict the sample to municipalities where PAN won or lost by a margin smaller than 5%. In Panel B, we aggregate the firm-level data at the municipality-level. Sample size drops because of the restriction to include only exports of single plant firms within a state. Columns 3 and 6 report p-values of t-tests on the difference in means between the PAN win and PAN loss sample. Standard errors are reported in parentheses.

Table 2: Homicides

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Effect after the 07 and 08 elections						
Mean if PAN loss	15.69	15.69	19.08	11.50	15.82	15.43
PAN win	25.90**	41.22**	43.02**	-10.73	44.87*	11.68
	(12.65)	(18.98)	(20.75)	(9.14)	(22.35)	(12.21)
Linear polynomial	No	Yes	Yes	Yes	Yes	Yes
Sample - region	All	All	North	South	Cartel	No cartel
Sample - elections	07 - 08	07 - 08	07 - 08	07 - 08	07 - 08	07 - 08
Observations	198	198	99	99	31	167
R-squared	0.17	0.25	0.25	0.06	0.36	0.02
Panel B: Effect after different election years						
Mean if PAN loss	12.18	8.49	13.53	15.69	13.55	18.53
PAN win	-0.81	0.50	30.92*	41.22**	2.56	7.69*
	(3.09)	(1.99)	(16.77)	(18.98)	(5.23)	(4.23)
Linear polynomial	Yes	Yes	Yes	Yes	Yes	Yes
Sample - region	All	All	All	All	All	All
Sample - elections	04 - 05	05 - 06	06 - 07	07 - 08	08 - 09	09 - 10
Observations	247	96	262	198	147	392
R-squared	0.12	0.01	0.19	0.25	0.06	0.06

Notes: Columns 1-6 report WLS regressions. Weights are determined by population size in 2005. The dependent variable is average annual homicides per 100,000 population in the three years following local elections. In Panel A, columns 1 and 2, the sample is comprised of municipalities where PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections. In Panel A, columns 3 and 4, this sample is divided into two parts using the median of the (average) latitude of the municipalities. In panel A, column 3, we report results for the northern municipalities, while in Panel A, column 4, we report results for the southern municipalities. In Panel A, column 5, we report effects in municipalities with pre-existing cartel participation measured in 2007 (as identified by Coscia and Rios (2012)). In Panel A, column 6, we report effects in municipalities with no pre-existing cartel activity. In Panel B, the sample is comprised of municipalities where PAN won or lost by a margin smaller than 5% in the election years used to perform the estimation. Robust standard errors are reported in parentheses.

Table 3: Other crimes

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Extortion			*Panel B: Displacement, property damages*		
Mean if PAN loss		5.4			126.5	
PAN win	4.6*	-3.4**	-3.0	260.4***	-112.7***	-28.2
	(2.7)	(1.7)	(2.7)	(86.5)	(36.4)	(57.5)
PAN win x North		6.0			366.4***	
		(4.3)			(112.2)	
PAN win x Cartel			8.1**			303.9***
			(4.0)			(116.2)
Observations	139	139	139	139	139	139
R-squared	0.17	0.34	0.38	0.38	0.52	0.58
	Panel C: Robbery businesses			*Panel D: Robbery individuals*		
Mean if PAN loss		63.3			484.5	
PAN win	75.6*	-76.7***	1.1	901.8***	-330.6***	17.0
	(44.2)	(27.5)	(22.1)	(297.7)	(92.8)	(150.7)
PAN win x North		153.8**			1,211.1***	
		(60.5)			(326.1)	
PAN win x Cartel			55.8			870.0***
			(50.1)			(328.8)
R-squared	0.19	0.36	0.50	0.29	0.48	0.58
	Panel E: Kidnapping			*Panel F: Personal injury*		
Mean if PAN loss		1.3			170.3	
PAN win	1.4	-0.1	1.0	191.8	-141.3***	-88.3
	(1.0)	(0.6)	(1.1)	(119.9)	(39.3)	(53.5)
PAN win x North		1.4			322.0**	
		(1.7)			(152.2)	
PAN win x Cartel			0.4			278.6*
			(1.6)			(156.8)
R-squared	0.10	0.13	0.13	0.22	0.37	0.48

Notes: Columns 1-6 report weighted regressions. Weights are determined by population size in 2005. In all panels the dependent variables are averages of a certain crime type per 100,000 population in 2011. In panel A the dependent variable is extortion; in Panel B, displacements and property damages; in panel C, robberies that targeted business establishments; in Panel D, robberies that targeted business individuals; in Panel E, kidnapping; and in Panel F, personal injuries. For all regressions, the sample is comprised of municipalities where crime data is available and where PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections. All regressions include a linear RD polynomial on the margin of victory in the elections. In columns 2 and 4, we add to the model a dummy (its main effects and interactions with Margin, PAN win, and Margin x PAN win) that equals 1 if a municipality is located in the north (splitting the sample into two using the median of the average latitude of the municipalities); In columns 3 and 6, we add to the model a dummy (its main effects and interactions with Margin, PAN win, and Margin x PAN win) that equals 1 if a municipality has cartel presence in 2007. Robust standard errors are reported in parentheses.

Table 4: Tradable and non-tradable

	(1)	(2)	(3)
	Production	Revenues	Revenues, excluding maquila services
Panel A: All industries			
PANwin	-0.13	-0.04	
	(0.16)	(0.13)	
Observations	197	198	
R-squared	0.04	0.03	
Panel A: Manufacturing			
PANwin	-0.402*	-0.414*	-0.407*
	(0.233)	(0.239)	(0.244)
Observations	192	192	192
R-squared	0.067	0.069	0.067
Panel B: Services and construction			
PANwin	-0.12	-0.10	
	(0.07)	(0.08)	
Observations	195	195	
R-squared	0.01	0.01	

Notes: The table reports β's of the regression $y_m = \alpha + \beta PANwin_m + \delta_1 Margin_m + \delta_2 PANwin_m \times Margin_m + \epsilon_m$, where y_m stands for the log of the growth factor in production, total revenues, revenues from maquiladoras and revenues from non-maquiladoras between 2008 and 2013. Regressions are weighted by population size in 2005. The sample is comprised of municipalities where PAN won or lost by a margin smaller than 5% in local elections between 2007 and 2008. Robust standard errors are reported in parentheses.

Table 5: Enterprise Survey

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
	Crime obstacle score			Crime is the worst obstacle		
Baseline (South, 2006)	1.182***	1.254***	1.171***	0.0823***	0.0988***	0.0799***
	(0.178)	(0.110)	(0.213)	(0.00706)	(0.0109)	(0.00711)
North	0.0814	-0.282*	0.124	-0.0493	-0.0616***	-0.0472
	(0.240)	(0.132)	(0.279)	(0.0306)	(0.0165)	(0.0325)
2009	0.433**	0.0384	0.734**	0.0430	-0.0629***	0.126**
	(0.194)	(0.191)	(0.316)	(0.0320)	(0.0106)	(0.0530)
North × 2009	0.896**	1.153***	0.878*	0.0312	0.115***	-0.0379
	(0.325)	(0.265)	(0.437)	(0.0459)	(0.0193)	(0.0640)
Observations	2,948	2,286	662	2,947	2,281	666
R-squared	0.082	0.056	0.127	0.009	0.015	0.039
Sectors	All	Manufactures	Other	All	Manufactures	Other
Panel B						
	Hours spent on regulation			Courts are a mayor obstacle		
Baseline (South, 2006)	18.02***	17.89***	18.05***	0.120**	0.103***	0.123*
	(3.554)	(2.865)	(3.801)	(0.0546)	(0.0201)	(0.0602)
North	13.81**	3.336	15.00***	-0.108*	-0.0636***	-0.114*
	(4.622)	(3.188)	(4.954)	(0.0552)	(0.0202)	(0.0609)
2009	1.518	5.474	-2.089	0.224*	0.259***	0.205
	(5.199)	(6.757)	(4.682)	(0.104)	(0.0680)	(0.129)
North × 2009	-22.60***	-19.05**	-17.96***	-0.136	-0.0899	-0.182
	(6.330)	(7.367)	(5.899)	(0.105)	(0.0725)	(0.130)
Observations	2,889	2,250	639	2,840	2,199	641
R-squared	0.028	0.030	0.041	0.096	0.064	0.092
Sectors	All	Manufactures	Other	All	Manufactures	Other

Notes: This table provides average response estimates for Firms surveyed in Mexico's enterprise surveys of 2006 and 2009. In both panels, the first row provides the average response of southern firms in 2006 and the next three rows provide differences in these averages associated to northern firms, to firms surveyed in 2009, and to their interaction. Panel A covers questions on crime as an obstacle to firms' operations, while panel B addresses questions on regulation and courts as obstacles. For each of the questions, the first column provides estimates for the full sample of firms, while the second and third columns respectively restrict the samples of firms to those operating in manufacturing activities or other activities. Survey-provided weights for each firm are used to calculate the respective averages, and standard errors are clustered at the region level.

Table 6: Exports

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Firm-level				Municipality-level			
Panel A: Exports, 3-year growth								
Mean if PAN loss	0.03				0.07			
PAN win	-0.08***	-0.14***	-0.14**	-0.21**	-0.08***	-0.18***	-0.19***	-0.15***
	(0.01)	(0.05)	(0.06)	(0.09)	(0.03)	(0.03)	(0.03)	(0.02)
Linear RD Polynomial	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Destination FE	No	No	Yes	No	No	No	Yes	No
Product-destination FE	No	No	No	Yes	No	No	No	Yes
Observations	17256	17256	17248	15170	21,435	21,435	21,424	18,267
R-squared	0.00	0.00	0.02	0.15	0.00	0.00	0.03	0.58
Panel B: Exports, 6-year growth								
Mean if PAN loss	0.03				0.06			
PAN win	-.017	-0.14***	-0.12***	-0.13***	-0.04	-0.13***	-0.13***	-0.08***
	(0.02)	(0.05)	(0.04)	(0.05)	(0.03)	(0.04)	(0.04)	(0.02)
Observations	14236	14236	14226	12345	20,513	20,513	20,497	17,579
R-squared	0.00	0.00	0.03	0.17	0.00	0.01	0.03	0.59

Notes: Columns 1-4 report RDD estimates at the firm-product-destination level, and columns 5-8 report RDD estimates at the municipality-product-destination level. Standard errors are clustered at the municipality level. The sample is comprised of municipalities where (i) PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections and (ii) the value exported is positive in the years used to compute export growth. Panel A shows effects on the log of the 3-year export growth factor, and Panel B shows effects on the log of the 6-year export growth factor.

Table 7: Exports, regional and pre-existing cartel presence heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Firm-level				Municipality-level			
Panel A: Pre-existing cartel presence								
Mean if PAN loss	0.03				0.07			
PAN win	-0.09***	-0.13***	-0.12**	-0.21**	-0.08**	-0.18***	-0.19***	-0.15***
	(0.01)	(0.04)	(0.05)	(0.09)	(0.03)	(0.03)	(0.03)	(0.02)
Linear RD Polynomial	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Destination FE	No	No	Yes	No	No	No	Yes	No
Product-destination FE	No	No	No	Yes	No	No	No	Yes
Observations	15849	15849	15840	14686	17,058	17,058	17,045	13,889
R-squared	0.00	0.00	0.02	0.14	0.00	0.00	0.03	0.57
Panel B: No pre-existing cartel presence								
Mean if PAN loss	0.002				0.07			
PAN win	0.04	-0.38	-0.44	-2.86	-0.01	-0.02	-0.03	0.03*
	(0.07)	(0.31)	(0.33)	(2.74)	(0.01)	(0.03)	(0.02)	(0.02)
Observations	1407	1407	1398	114	4,377	4,377	4,359	3,133
R-squared	0.00	0.00	0.19	0.90	0.00	0.00	0.10	0.75
Panel C: North								
Mean if PAN loss	0.03				0.06			
PAN win	-0.09***	-0.14***	-0.15**	-0.13*	-0.07**	-0.21***	-0.21***	-0.13***
	(0.01)	(0.05)	(0.06)	(0.07)	(0.03)	(0.06)	(0.05)	(0.02)
Observations	15631	15631	15621	14574	17,068	17,068	17,053	14,120
R-squared	0.00	0.00	0.02	0.13	0.00	0.00	0.03	0.59
Panel D: South								
Mean if PAN loss	0.01				0.11			
PAN win	-0.02	-0.17	-0.19	0.87	-0.05***	0.04*	0.05**	0.04
	(0.07)	(0.14)	(0.19)	(2.22)	(0.02)	(0.02)	(0.02)	(0.03)
Observations	1625	1625	1620	273	4,367	4,367	4,349	2,790
R-squared	0.00	0.00	0.18	0.82	0.00	0.00	0.10	0.80

Notes: Columns 1-4 report RDD estimates at the firm-product-destination level, and columns 5-8 report RDD estimates at the municipality-product-destination level. Standard errors are clustered at the municipality level. The sample is comprised of municipalities where (i) PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections and (ii) the value exported is positive in the years used to compute export growth. Panel A (B) restricts the sample to municipalities with (without) cartel presence in 2007 using data constructed by Coscia and Rios (2012). Panel C (D) restricts the sample to northern (southern) municipalities.

Table 8: Firm exports, time-series evolution of the effect

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Elections	04 - 05	05 - 06	06 - 07	07 - 08	08 - 09	09 - 10	10 - 11
Panel A: Log(exports 2nd year of the term/exports election year)							
Mean if PAN loss	-0.23	-0.06	-0.05	-0.04	0.03	-0.01	-0.02
PAN win	0.13***	0.14	-0.13	-0.26***	0.11	0.15	0.26*
	(0.06)	(0.27)	(0.11)	(0.12)	(0.21)	(0.13)	(0.15)
Observations	15554	1527	18540	16598	3906	16156	11714
R-squared	0.15	0.39	0.15	0.14	0.50	0.21	0.16
Panel B: Log(exports 3rd year of the term/exports election year)							
Mean if PAN loss	-0.17	-0.09	0.01	0.03	0.03	0.04	0.05
PAN win	0.10*	0.19	-0.09	-0.21***	-0.18	-0.10	0.31*
	(0.05)	(0.26)	(0.11)	(0.08)	(0.11)	(0.10)	(0.17)
Observations	15193	1319	17006	15335	3852	15549	11188
R-squared	0.15	0.45	0.16	0.15	0.50	0.21	0.16
Panel C: Log(average exports during the 3-year term/exports election year)							
Mean if PAN loss	0.03	0.29	0.55	0.60	0.60	0.52	0.49
PAN win	0.25***	0.15	-0.19	-0.38***	0.03	0.21	1.18*
	(0.11)	(0.58)	(0.16)	(0.12)	(0.35)	(0.24)	(0.72)
Observations	10216	854	12719	11599	2463	11511	8659
R-squared	0.17	0.47	0.15	0.15	0.55	0.22	0.15

Notes: Columns 1-7 report RDD estimates at the firm-product-destination level for elections in each pair of contiguous years between 2004-2005 and 2010-2011. Standard errors are clustered at the municipality level. The sample is comprised of municipalities where (i) PAN won or lost by a margin smaller than 5% in local elections during the relevant pair of years and (ii) the value exported is positive in the years used to compute export growth. Panel A shows effects on the log of the 2-year export growth factor. Panel B shows effects on the log of the 3-year export growth factor. Panel C shows effects on the log of the growth factor of the average 3-year exports.

Table 9: Firm exports, extensive margin

	(1)	(2)	(3)	(4)
Panel A: Main effects				
Mean if PAN loss		0.52		
PAN win	0.09***	-0.04	-0.07	-0.07
	(0.04)	(0.09)	(0.06)	(0.07)
Linear RD Polynomial	No	Yes	Yes	Yes
Destination FE	No	No	Yes	No
Product-destination FE	No	No	No	Yes
Observations	41566	41566	41540	37686
R-squared	0.01	0.01	0.12	0.29
Panel B: Pre-existing cartel presence				
Mean if PAN loss		0.55		
PAN win	0.06*	-0.06	-0.04	-0.05
	(0.03)	(0.08)	(0.06)	(0.07)
Observations	39274	39274	39247	36353
R-squared	0.00	0.01	0.11	0.27
Panel C: No pre-existing cartel presence				
Mean if PAN loss		0.33		
PAN win	0.38***	-0.29*	-0.003	-0.18
	(0.07)	(0.15)	(0.11)	(0.71)
Observations	2292	2292	2281	475
R-squared	0.08	0.11	0.30	0.86
Panel D: North				
Mean if PAN loss		0.55		
PAN win	0.06*	-0.18***	-0.15***	-0.16***
	(0.03)	(0.08)	(0.05)	(0.05)
Observations	38826	38826	38798	36100
R-squared	0.00	0.01	0.11	0.26
Panel E: South				
Mean if PAN loss		0.40		
PAN win	0.14	-0.16***	-0.01	-0.14
	(0.10)	(0.05)	(0.08)	(0.39)
Observations	2740	2740	2724	705
R-squared	0.00	0.08	0.24	0.83

Notes: Columns 1-4 report RDD estimates at the firm-product-destination level, where the outcome variable is a binary that identifies whether a triple disappeared between 2007 and 2010. Standard errors are clustered at the municipality level. The sample is comprised of municipalities where (i) PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections and (ii) the value exported for the triple was positive in 2007. Panel A does not restrict the sample of municipalities. Panel B (C) restricts the sample to municipalities with (without) cartel presence in 2007 using data constructed by Coscia and Rios (2012). Panel D (E) restricts the sample to northern (southern) municipalities.

Table 10: Firm exports, aggregate effects

	(1)	(2)	(3)	(4)	(5)
Mean if PAN lost	-0,151	-0,111	-0,549	-0,127	-0,272
PANwin	-0.53**	-0.50*	-0,398	-0.705***	0,369
	(0.226)	(0.258)	(0.370)	(0.241)	(0.780)
Sample	Full	Cartels	No Cartels	North	South
Observations	1485	1418	67	1417	68
R-squared	0,003	0,003	0,097	0,004	0,022

Notes: The table reports β's of the regression $y_{fm} = \alpha + \beta PANwin_m + \delta_1 Margin_m + \delta_2 PANwin_m \times Margin_m + \epsilon_m$, where y_{fm} stands for the log of the growth factor in total firm exports in municipality m. The sample is comprised of single-plant firms within a state located in municipalities where PAN won or lost by a margin smaller than 5% in local elections between 2007 and 2008. Robust standard errors are reported in parentheses.

Table 11: Firm exports, heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Complexity and firm Characteristics							
Mean if PAN loss	0.021	0.025	0.007	0.031	-0.168	0.022	0.009
PANwin	-0.206**	-0.269***	-0.124	-0.273***	-0.0398	-0.183	-0.296*
	(0.092)	(0.080)	(0.095)	(0.088)	(0.249)	(0.118)	(0.161)
Observations	15170	10952	4218	13793	1153	13499	1409
R-squared	0.15	0.15	0.17	0.16	0.39	0.16	0.33
Product Group	Full sample	Complex	Non-complex	Large	Small	High wages	Low wages
Panel B: Dependence on capital, human capital and finance							
Mean if PAN loss	0.021	0.021	0.021	0.019	0.029	0.020	0.026
PANwin	-0.206**	-0.320***	-0.0613	-0.265**	-0.121	-0.211**	-0.190
	(0.092)	(0.068)	(0.106)	(0.098)	(0.094)	(0.092)	(0.120)
Observations	15170	9596	5574	10200	4970	10737	4433
R-squared	0.15	0.15	0.16	0.14	0.18	0.16	0.12
Product Group	Full sample	High capital intensity	Low capital intensity	High human capital intensity	Low human capital intensity	High finance dependence	Low finance dependence
Panel C: Trucking and fragmentation of production process							
Mean if PAN loss	0.021	0.003	0.032	0.010	0.038	0.019	0.024
PANwin	-0.206**	0.0568	-0.377***	-0.142	-0.282***	-0.211*	-0.194**
	(0.092)	(0.190)	(0.043)	(0.159)	(0.064)	(0.111)	(0.082)
Observations	15170	5878	9292	8802	6368	8312	6858
R-squared	0.15	0.16	0.15	0.14	0.17	0.17	0.13
Product Group	Full sample	High trucking dependence	Low trucking dependence	High input fragmentation	Low input fragmentation	High occupation fragmentation	Low occupation fragmentation

Notes: Table reports RDD estimates at the firm-product-destination level, where the outcome variable is the 3-year log export growth between 2007 and 2010 as a function of a close PAN win between 2007 and 2008. Standard errors are clustered at the municipality level. The sample is comprised of municipalities where (i) PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections and (ii) the value exported for the triple was positive in 2007 and 2010. Column 1 shows estimates for an unrestricted sample in all panels. Column 2 (3) in panel A restricts the sample to high-complexity (low-complexity) products. Column 4 (5) restricts the sample to exporters above (below) the median exporter employment size in 2007. Column 6 (7) restricts the sample to exporters above (below) the median exporter average wage in 2007. Column 2 (3) in panel B restricts the sample to high (low) capital-intensity products. Column 4 (5) restricts the sample to high (low) skill intensity products. Column 6 (7) restricts the sample to high (low) external finance dependent products. Column 2 (3) in panel C restricts the sample to high (low) trucking dependence products. Column 4 (5) restricts the sample to products with high (low) input fragmentation. Column 6 (7) restricts the sample to products relying on activities with high (low) occupation fragmentation. Product Complexity: This metric from Hausmann et al. (2013) empirically approximates the difficulty of exporting a product competitively from a given country. Capital dependence: This metric from Shirotori et al. (2010) captures the Revealed Capital Intensity of the product from international trade patterns and national capital endowments of their competitive exporters. Human capital dependence: Also from Shirotori et al. (2010), this measure captures the Revealed Human Capital Intensity of the product from international trade and national human capital endowment patterns. Finance dependence: This metric from Rajan and Zingales (1998), measures a product's dependence in external capital for its production. Trucking dependence: We build this metric according to a product's appeared dependence on trucking services as measured in the US input-output tables. Input fragmentation: We build this metric as the inverse Herfindahl index in a product's input set as observed in the US input-output tables. Occupation fragmentation: We build this metric as the inverse Herfindahl index in a product's occupation set as observed in the US Occupational Employment Statistics.

Table 12: Individual income growth by skill-occupation decomposition

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: High skill												
	Worker						Other (self-employed, entrepreneur, owner)					
PANwin	0.03	0.17	0.10	0.05	0.12	0.34***	-0.95***	-1.09***	-0.27	-0.85***	-0.80**	0.41
	(0.11)	(0.15)	(0.16)	(0.14)	(0.15)	(0.09)	(0.17)	(0.23)	(0.40)	(0.14)	(0.36)	(0.27)
Sample	Full	North	South	Cartel	Non-cartel	Full	Full	North	South	Cartel	Non-cartel	Full
Elections	07-08	07-08	07-08	07-08	07-08	04-05	07-08	07-08	07-08	07-08	07-08	04-05
Observations	165	84	81	29	136	194	113	55	58	29	84	150
R-squared	0.12	0.25	0.02	0.34	0.01	0.10	0.33	0.43	0.02	0.65	0.10	0.04
Panel B: Medium skill												
	Worker						Other (self-employed, entrepreneur, owner)					
PANwin	-0.27***	-0.32***	0.01	-0.34***	0.01	0.23***	-0.51***	-0.62***	0.02	-0.51***	-0.15	0.59***
	(0.07)	(0.10)	(0.10)	(0.07)	(0.10)	(0.08)	(0.12)	(0.16)	(0.20)	(0.13)	(0.21)	(0.17)
Observations	198	99	99	31	167	243	193	96	97	31	162	239
R-squared	0.12	0.20	0.02	0.44	0.06	0.05	0.17	0.23	0.05	0.37	0.02	0.11
Panel C: Low skill												
	Worker						Other (self-employed, entrepreneur, owner)					
PANwin	-0.51***	-0.50***	-0.11	-0.54***	-0.15	0.49**	-0.63***	-0.81***	-0.07	-0.61**	-0.29*	0.74***
	(0.17)	(0.17)	(0.09)	(0.19)	(0.11)	(0.20)	(0.24)	(0.28)	(0.21)	(0.27)	(0.16)	(0.28)
Observations	198	99	99	31	167	246	198	99	99	31	167	246
R-squared	0.28	0.29	0.03	0.37	0.07	0.25	0.17	0.25	0.05	0.25	0.03	0.23

Notes: The table reports β's of the regression $log(y_{m2010}/y_{m2000}) = \alpha + \beta PANwin_m + \delta_1 Margin_m + \delta_2 PANwin_m \times Margin_m + \epsilon_m$, where y_{mt} is the average income of a skill-occupation group in municipality m in year t. The data come from the population census of years 2000 and 2010. Since this census is a survey, we follow the recommendation of the Mexican Statistical Institute (INEGI). We aggregate data at the municipality level using the weights provided by INEGI. We group individuals in the category "high skil" if they have 17 or more years of schooling; "medium skil" if they have between 12 and 17 years of schooling; and "low skil" if they have less than 12 years of schooling.

Table 13: Economic census, capital

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Investment growth (log)						
Mean if PAN lost	-0.10	-0.23	0.05	-0.21	0.13	0.32
PANwin	-0.86*	-2.00***	1.50**	-1.11	0.55	0.08
	(0.51)	(0.76)	(0.68)	(0.76)	(0.52)	(0.39)
Sample	All	North	South	Cartel	No cartel	All
Elections	07-08	07-08	07-08	07-08	07-08	03-04
Growth	08-13	08-13	08-13	08-13	08-13	03-08
Observations	172	84	88	28	144	311
R-squared	0.07	0.26	0.17	0.13	0.04	0.02
Panel B: Gross fixed capital formation growth (log)						
Mean if PAN lost	0.08	0.05	0.13	-0.02	0.29	0.28
PANwin	-1.21**	-2.09***	0.69	-1.42*	0.07	0.20
	(0.50)	(0.55)	(0.61)	(0.71)	(0.46)	(0.44)
Observations	182	94	88	29	153	311
R-squared	0.12	0.29	0.08	0.21	0.04	0.02
Panel C: Fixed assets growth (log)						
Mean if PAN lost	0.08	0.20	-0.07	0.06	0.11	0.56
PANwin	-0.21	-0.75**	0.86	-0.28	0.09	0.02
	(0.43)	(0.32)	(0.56)	(0.61)	(0.28)	(0.13)
Observations	198	99	99	31	167	345
R-squared	0.10	0.23	0.20	0.20	0.00	0.01
Panel D: Employment growth (log)						
Mean if PAN lost	0.01	0.01	0.01	0.00	0.03	0.22
PANwin	0.05	-0.00	0.13**	0.06	-0.01	-0.07
	(0.04)	(0.06)	(0.06)	(0.05)	(0.07)	(0.08)
Observations	198	99	99	31	167	345
R-squared	0.03	0.04	0.10	0.13	0.03	0.01
Panel E: Average wage growth (log)						
Mean if PAN lost	0.22	0.18	0.27	0.20	0.28	0.20
PANwin	-0.25***	-0.35***	0.07	-0.24***	-0.19	0.02
	(0.07)	(0.09)	(0.15)	(0.08)	(0.15)	(0.06)
Observations	198	99	99	31	167	342
R-squared	0.07	0.13	0.01	0.24	0.02	0.00

Notes: Table reports RD estimates at the municipality level, where the outcome is the log growth factor of a specific variable. The sample is comprised of municipalities where (i) PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections (or 2003 and 2004 in column 6) and (ii) the value of the variable was positive in 2008 and 2013 (or 2003 and 2008 in column 6). Columns 1 and 6 show estimates for an unrestricted sample. Column 2 (3) restricts the sample to northern (southern) municipalities. Column 4 (5) restricts the sample to municipalities with (without) cartel presence in 2007. Panel A shows effects on overall local investment; Panel B on the local gross fixed capital formation; Panel C on the value of fixed assets; Panel D on employment; and Panel E on average wages.

Table 14: FDI - project level

	(1)	(2)	(3)	(4)	(5)	(6)
	CAPEX (MM USD)	New jobs per 1,000 inhabitants	Capital per new job (1,000 USD)	CAPEX (MM USD)	New jobs per 1,000 inhabitants	Capital per new job (1,000 USD)
Panel A: PAN mayors during the war on drugs - project level						
Mean if PAN loss	115.53	328.76	286.15	92.26	304.98	319.03
PANwin	-177.5***	100.5	-650.4**	-106.1***	281.4***	-736.3**
	(33.52)	(91.95)	(267.8)	(16.6)	(69.05)	(312.8)
Observations	174	174	174	111	111	111
R-squared	0.053	0.017	0.185	0.039	0.028	0.234
Elections	07-08	07-08	07-08	07-08	07-08	07-08
Period	07-12	07-12	07-12	07-10	07-10	07-10
Panel B: PAN mayors before the war on drugs - project level						
Mean if PAN loss	96.18	390.90	305.26	56.88	342.81	201.27
PANwin	-35.81	-49.88	27.06	66.65**	-46.27	146.1*
	(69.86)	(94.18)	(100.3)	(28.75)	(130)	(68.32)
Observations	114	114	114	63	63	63
R-squared	0.014	0.009	0.062	0.022	0.01	0.019
Elections	04-05	04-05	04-05	04-05	04-05	04-05
Period	04-09	04-09	04-09	04-07	04-07	04-07

Notes: Table reports RD estimates at the greenfield project level, where the outcome is the value of the relevant variable. The sample is comprised of greenfield projects located in municipalities where PAN won or lost by a margin smaller than 5% in the relevant period. Panel A shows the effects of a close PAN victory in 2007-2008 on average project values between 2007 and 2012 (columns 1-3) or between 2007 and 2010 (columns 4-6). Panel B shows similar effects of a close PAN win in 2004-2005 on average project values between 2004 and 2009 (columns 1-3) or between 2004 and 2007 (columns 4-6). Columns 1 and 4 assess effects on a project's CAPEX levels. Columns 2 and 5 evaluate effects on a project's number of new jobs. Columns 3 and 6 show effects on the average project's capital per new created job.

Table 15: Private security: guards

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Guards average salary growth (log)						
Mean if Pan loss	0.69	0.65	0.75	0.66	0.77	0.45
PANwin	-0.70***	-0.78***	-0.38**	-0.79***	-0.29	0.11
	(0.18)	(0.20)	(0.18)	(0.21)	(0.19)	(0.10)
Sample	Full	North	South	Cartel	Non-cartel	Full
Elections	07-08	07-08	07-08	07-08	07-08	04-05
Observations	160	86	74	29	131	182
R-squared	0.37	0.48	0.06	0.66	0.03	0.15
*Panel B: (#guards_2010 - #guards_2000)*1000/Pop_05*						
Mean if Pan loss	1.96	2.90	0.80	2.34	1.21	2.55
PANwin	2.15**	1.49	1.01	2.35***	0.00	-1.78*
	(0.89)	(0.99)	(1.10)	(0.84)	(1.18)	(1.03)
Observations	198	99	99	31	167	246
R-squared	0.12	0.08	0.07	0.27	0.06	0.11

Notes: The table reports β's of the regression $y_m = \alpha + \beta PANwin_m + \delta_1 Margin_m + \delta_2 PANwin_m \times Margin_m + \epsilon_m$. The data come from the population census of years 2000 and 2010. Since this census is a survey, we follow the recommendation of the Mexican Statistical Institute (INEGI) and aggregate data at the municipality level using the weights provided by INEGI. In Panel A, the dependent variable is log of the average income of guards in 2010 divided by the average income of guards in 2000. In Panel B, the dependent variable is number of guards in 2010 minus the number of guards in 2000, per 1,000 population as of 2005.