

# Export side effects of wars on organized crime: The case of Mexico<sup>\*</sup>

Jesús Gorrín<sup>a</sup>, José Morales-Arilla<sup>b,\*</sup>, Bernardo Ricca<sup>c</sup>

<sup>a</sup>Warwick Business School, Coventry, UK

<sup>b</sup>Princeton University, New Jersey, USA

<sup>c</sup>Inspere, São Paulo, Brazil

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## Abstract

This paper finds that law enforcement interventions during the Mexican Drug War (MDW) hindered local export growth. We leverage exogenous variation in drug enforcement from the close election of mayors affiliated with the national ruling party during the MDW. Firms servicing the same markets but exogenously exposed to drug enforcement experienced lower export growth. The MDW decreased capital investments, eroding productivity gains in capital-intensive activities.

*Keywords:* Exports, Violence, Mexico, Regression Discontinuity.

*JEL Codes:* F14, O17, K49, K42.

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## 1. Introduction

Export competitiveness is key for countries to converge towards higher living standards.<sup>1</sup> Crime and violence are seen as important constraints on the productivity and welfare of developing economies. Consequently, governments spend significant resources fighting criminal organizations. In this paper, we show that a particular type of drug law enforcement policy – war on organized crime – can backfire and further erode local production capabilities. We focus our analysis on the Mexican Drug War (MDW), 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 Dell (2015) showed, a close election of a PAN mayor led to a disproportionate increase in homicides during the MDW. Our first exploration of the effects of a discontinuous exposure to a PAN mayor using aggregate data finds negative effects on production growth that concentrate in the manufacturing sector. This is consistent with findings from the Mexican Enterprise Survey, which show an increase in the proportion of manufacturing firms citing violence as an obstacle to production in northern Mexico – the area of the country that experienced the bulk of the violence increase.

Because violence can affect both demand and supply, our paper focuses on local exports and introduces new elements to the regression discontinuity (RD) setting to identify the effect of violence on the local capacity to supply foreign markets. We compare the export growth of the same product to the same destination from areas where the discontinuous exposure to a PAN mayor is an exogenous source of violence. Using firm-level data on exports, we

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<sup>\*</sup>Corresponding author

*Email addresses:* Jesus.Gorrin@wbs.ac.uk (Jesús Gorrín), jmoralesarilla@princeton.edu (José Morales-Arilla), BernardoOGR@insper.edu.br (Bernardo Ricca)

<sup>1</sup>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 the large amounts invested in policies that promote market access (Lederman et al., 2010).

document that export growth decreases by an average of 21% as a consequence of a PAN mayor being elected in a close election. This effect is persistent and continues into the next mayoral term, with the 6-year growth rate in exports decreasing by an average of 13%. We observe robust effects on export growth only at the intensive margin. In the context of Melitz (2003), the main implication of this finding is that violence increases the marginal costs of exporting, while the potential increase in the fixed costs of exporting is not enough to affect decisions at the extensive margin.<sup>2</sup>

A key goal of this paper is to identify the characteristics of the exporters most affected by the violence that ensued after drug enforcement operations, as well as the channels through which the MDW affected local export outcomes. Hence, we start by identifying the sectors that observed the largest erosion in export growth. We find that effects are concentrated in electronics, vehicles, and textiles. We then explore the characteristics of exporters and products most affected by the MDW and find that effects concentrate on large exporters and the exports of capital-intensive products. Lastly, we explore the channels through which the MDW altered export outcomes. As drug enforcement interventions induced increases in violence and crime, we focus on three possible mechanisms connecting enforcement with export underperformance: erosion in capital accumulation, resource misallocation away from production and into protection, and reductions in labor availability due to local disamenities. We find that the drug war decreased capital accumulation and capital expenditure in new greenfield investments. We also find evidence suggesting that violence increased the number of providers of private security services. Finally, we find negative effects on average wage growth and null effects on migration, both of which suggest that productivity drops dominate the disamenity mechanism.<sup>3</sup> Taken together, our evidence suggests that the MDW affected local export growth by eroding the accumulation of productive capital, which in turn limited potential gains in labor productivity.

Our paper contributes to the literature on the economic costs of violence, crime, and law enforcement in several ways.<sup>4</sup> First, we overcome endogeneity challenges, such as omitted variable bias and reverse causality (Dell et al., 2019 and Dix-Carneiro et al., 2018), by focusing on plausibly exogenous variation in the local exposure to drug enforcement interventions. We show that such interventions can backfire by engendering an increase in violence that yields economic side effects on export activity. Most importantly, we structure our analysis to assess the relative importance of alternative theories connecting enforcement, violence, and economic underperformance. We show that drug enforcement erodes capital accumulation and disproportionately affects large exporters of capital-intensive products. Building on this result, we conjecture that these temporary drug enforcement interventions may have caused long-term economic consequences by disrupting the process of structural transformation and productivity growth.

While a broader segment of the literature has studied the economic consequences of conflicts and terrorist activity, fewer papers study the economic consequences of violence triggered by law-and-order interventions. The distinction is relevant because, unlike civil conflicts, law enforcement activities are common in a broader set of countries. The criminal activities that increase as a consequence of these operations are likely to differ from the violence that ensues during and after civil conflicts, as the organizations involved have different structures and objectives. A final distinction is that the effects of law-and-order national policies are not restricted to conflict settings, where political uncertainty also affects firms' economic decisions. Therefore, showing the unintended economic consequences of these policies – and the mechanisms through which they operate – is an academic contribution that can help improve the decision-making of policymakers in a broad set of countries.

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<sup>2</sup>According to Melitz (2003), the fixed costs of servicing a specific market should induce firms to exit that market. There are a number of reasons to believe the MDW did not impose higher market-specific fixed costs on exporters. However, extensive margin predictions would be expected if the violence triggered by the MDW affected the fixed costs of exporting across markets. This would be the case if, for example, violence led to higher fixed premia for insurance and freight, increased fixed investments in protection, or higher costs of business travel necessary for developing and sustaining trading relationships. While our analysis lacks measures for these specific outcomes, the absence of extensive margin results suggests that the rise in these costs did not bind firms' capacity to engage in exports.

<sup>3</sup>While an erosion in capital accumulation should lead to lower wage growth as the marginal productivity of labor decreases, a local disamenity should induce higher wage growth, as workers demand a premium for their exposure to increased levels of crime and violence. We find evidence that drug enforcement led to a reduction in wage growth, and the effects are strongest and more precise for low-skill workers. This suggests that productivity drops outweigh the potential disamenity premium, especially for low-skill workers with limited outside options. Moreover, we find no effects on out-migration and in-migration patterns across skill levels.

<sup>4</sup>See, for instance, Abadie and Gardeazabal (2003) for the effects of violence on firm market values; Besley and Mueller (2012) and Frischtak and Mandel (2012) for effects on housing prices; Pshisva and Suarez (2010) for effects on corporate investment; Besley and Mueller (2018) for effects on misallocation; and Adhikari (2013) and Clemens (2017) for effects on labor force displacement. Rozo (2018) documents an increase in firm exit and a decrease in production as effects of violence, while Ksoll et al. (2022) show that electoral violence causes a decrease in exports due in part to an increase in worker absenteeism.

Our paper also contributes to a growing literature on the effects of the Mexican Drug War itself. 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; Ashby and Ramos (2013) evaluates effects on foreign direct investment; and Lindo and Padilla-Romo (2018) study the consequences of the kingpin targeting approach. Utar (2018) shows that an increase in violence driven by the drug war generates a decrease in production for the local markets but not a decrease in exports; Montoya (2016) finds that the drug 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. There are several differences between these papers and ours. First, we are the first to show that the MDW exerts a negative effect on local export capacities. Second, while other papers rely on difference-in-differences or instrumental variable methods, our study uses plausibly exogenous changes in violence to find negative export consequences of the Drug War. This distinction is particularly important in relation to the null results on exports of Utar (2018). That paper uses the interaction of drug enforcement operations in Mexico with net cocaine cultivation in Colombia as an instrument for homicides in Mexico. However, it is natural to assume that government officials would strategically target certain areas based on economic and political criteria, leading to endogeneity in the cross-section of the treatment.<sup>5</sup> For this reason, we believe our identification, which uses close electoral outcomes as a source of exogenous variation in the local exposure to violence, is a more plausible strategy to assess the effects of the MDW on local exports. Furthermore, we extend our analysis to assess the potential mechanisms through which the MDW affected local exports.

## 2. Empirical setting

### 2.1. The Mexican political landscape and the Drug War

For most of the twentieth century, the Institutional Revolutionary Party (*Partido Revolucionario Institucional*, *PRI*) was the governing party in Mexico. In the 1990s, politicians from different parties began to win 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 (O’Neil, 2009). Importantly, violence was contained. When other parties gained power, the cartels had to negotiate with new officials and this relationship was shaken. The election of Vicente Fox (PAN) as president in 2000 triggered some institutional changes, but it was only in 2006, when Felipe Calderón (PAN) was elected president, that changes intensified. Calderón governed from December 1, 2006 until November 30, 2012. Just after taking office, he declared a war on drugs, sending the army to several provinces.

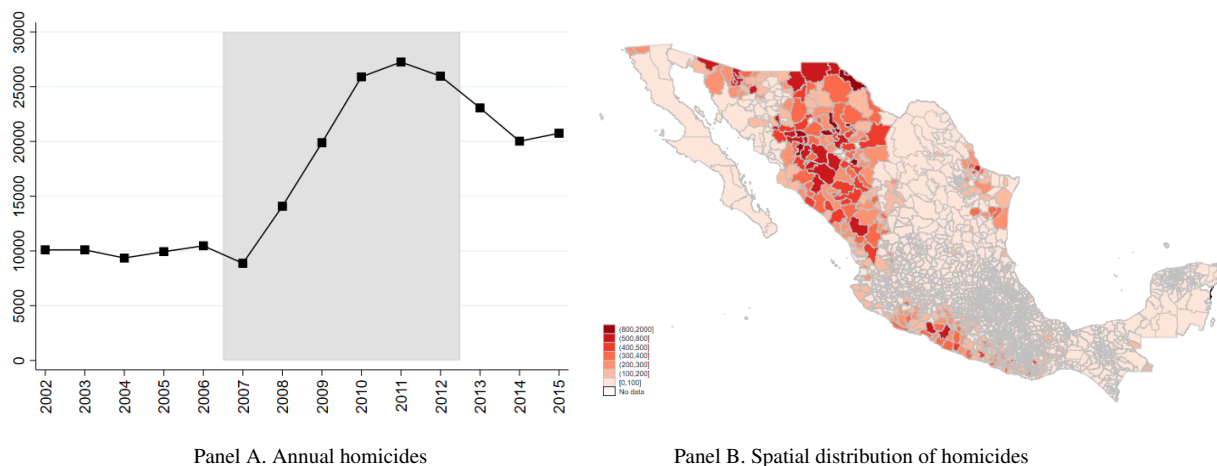
The policy had tragic consequences. The arrest or assassination of kingpins triggered disputes for territorial power and an escalation in violence. Members from the same organization or from rival cartels exploited the weakening of the leadership to try to gain control of the organization. Once in charge, new leaders had to assert their authority, often through force. There is evidence that competition between cartels intensified violence during the Mexican Drug War. Rios (2013) shows that deaths from confrontations between government authorities and criminals in a municipality are significantly positively related to future drug-related deaths in that municipality, the effect being stronger in areas where multiple cartels operate, suggesting competition plays a role. The quantitative effect in Rios (2013) is also supported by a series of case studies that illustrate that in the wake of enforcement operations by the government, there are cartel splits, retaliations, and an increase in threatening communications between cartels. Lindo and Padilla-Romo (2018) show that violence increases more after interventions that remove cartel leaders and in places where other cartels are present (i.e., competitive turfs). The effect is not driven by the interventions, but by the removal of kingpins, which the authors interpret as violence driven by competition. Dell (2015), using confidential data on trafficking routes, finds that violence increases more in areas where cartel control is more fragmented. In an unreported result, using data on cartel presence from Coscia and Rios (2012), we find that the increases in violence and the drops in export that follow a PAN close-win municipal election are strongest in areas where multiple cartels operate or where a rival cartel is operating in a neighboring municipality.

During Calderón’s administration, the number of homicides increased by 160%, from 10,452 in 2006 to 27,213 in 2011 (Panel A of Figure 1). The period of steepest growth occurred between 2007 and 2010. After this period,

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<sup>5</sup>In Section A.9 of the Online Appendix, we compare our results with those obtained using law enforcement crackdowns as the treatment.

homicides decreased slightly but remained at a number that was twice the pre-war one. Homicides were concentrated in the northern regions of the country, where the main cartels smuggle drugs into the US (Panel B of Figure 1). In reaction to the crackdown, cartels began to diversify their activities into other crimes, such as extortion, human trafficking, oil theft, kidnapping, and robbery. Consistently, survey data shows that firms were negatively affected by the ensuing violence. 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, manufacturing firms in northern Mexico became more likely to cite violence as a key constraint on their operations (Table A.7 in the Online Appendix).



**Figure 1:** Annual homicides and spatial distribution of homicides  
 Note: Panel A: time series of total homicides in Mexico. The grey area shows total homicides during Calderón’s presidency. Panel B: geographical distribution of total homicides between 2007 and 2011 per 100,000 inhabitants.

These drug-enforcement operations were mainly organized at the federal level. However, large-scale interventions require coordination with local authorities if they are to be successful, and this coordination is facilitated when national and local authorities share a political affiliation, agenda, and interests. All municipalities and states in Mexico control a police force. Mayors are ultimately in charge of the municipal police force because they appoint or remove the chief of police. The important role that mayors played in the implementation of the Drug War can be seen in practice, such as in their denouncement of extortion from cartels. From 2006 until 2014, organized crime killed 63 former mayors or mayors in office.<sup>6</sup> Importantly, at the time of the drug war, mayors were elected by popular vote in competitive polls that occur every three years. Hence, it is reasonable to assume that municipal elections are an important source of variation in the implementation of the drug war policy at the local level.

## 2.2. The role of exports in the Mexican economy and performance during the Mexican drug war

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, with the US receiving 86% of the exports. The exports/GDP ratio in Mexico was 30.4% in 2005, which is significant compared to other economies in the region. (15.2% in Brazil, 40.2% in Chile, 16.8% in Colombia, and 26.8% in Peru). During the period of the war on drugs, Mexican exports grew less in comparison to those countries (Figure A.4 in the Online Appendix). Aside from the increase in violence, many factors might explain this relative underperformance, one of which is that the MDW coincided with the collapse in international trade observed after the financial crisis of 2007-2008. As the economic contraction was heterogeneous across countries, demand shocks in the main destination countries of Mexican exports (especially the US) could account for the observed patterns. Our empirical strategy deals explicitly with this possibility.

<sup>6</sup>See <https://cnnespanol.cnn.com/2018/04/13/violencia-contra-los-alcaldes-en-mexico-mas-de-100-asesinados-desde-2006/> and <http://archivo.eluniversal.com.mx/nacion/165947.html>.

### 2.3. Data

We collect data on local electoral results from the Electoral Tribunals of each state. Municipalities located in different states held these on different dates. Elected mayors (municipal presidents) serve for three years. 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 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 the 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. Annual data on local public expenditures and revenues are from the INEGI.

We use firm-level data on exports from the Mexican Tax Administration Service (*Servicio de Administración Tributaria*, SAT).<sup>7</sup> We observe exports at the country of destination-product level; products are classified using HS 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, albeit 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 Mexican Atlas of Economic Complexity. 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, with the caveat that there may be measurement error when assigning the municipality of origin. Both datasets are available from 2004 to 2014. Because of this restriction, when studying long-term effects, we use a 6-year period.

### 2.4. Empirical strategy

The endogenous assignment of enforcement efforts toward violent regions biases the regressions of violence on enforcement. Similar concerns would arise from regressing measures of local production on violence. To address these challenges, we 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 estimate the following specification

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

where  $y_m$  denotes average homicides per 100,000 inhabitants in municipality  $m$  during the elected mayor's term,  $PANwin_m$  is a dummy variable that takes the value one when the elected mayor is from the PAN, and  $f(Margin_m, PANwin_m)$  is a polynomial on the PAN vote margin (percentage of total votes) and the dummy of PAN victory. In our main results, we restrict the sample to municipalities where the PAN won or lost by a margin smaller than 5% of the total votes in the 2007-2008 elections. We choose this pair of election years because the terms of mayors elected in those years overlap with the period that experienced the steepest growth in homicides. Indeed, as we show later, this is the only pair of years in which a close PAN election is associated with a large increase in homicides. Because it is likely that crime is under-reported in smaller municipalities, we weight regressions using the size of the 2005 population (Dell, 2015). Following anecdotal evidence that cartels diversified their activities during the war, we also test the effects on other crimes.

In the Online Appendix, we present results for different bandwidths and polynomial choices and estimate the effect of a close PAN victory using a local difference-in-differences framework with year and municipality fixed effects. Mexican elections in this period are not considered prone to manipulation. We provide empirical support for this claim in the Online Appendix. We show, using Cattaneo et al. (2018) and McCrary (2008), that there is no evidence of bunching around the discontinuity.

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<sup>7</sup>Micro-level data are not publicly available. We accessed these data at the Growth Lab at Harvard's Center for International Development.

Studying the effect of a close election 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: it can diminish the likelihood or capacity of individuals to consume certain types of goods, disrupt production by increasing costs, and drive workers out of the affected locality. We advance the existing literature by disentangling the effects on supply from the 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 controlling for external aggregate demand shocks with product-destination fixed effects (Paravisini et al., 2014). 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 following form:

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

where  $X^t_{fmpc}$  stands for the exports of product  $p$  to country of destination  $c$  in year  $t$  from a firm  $f$  that is located in municipality  $m$ , and  $\psi_{pc}$  stands for product-country of destination fixed effects. 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 specifications,  $t'$  is the third (final) year of the new administration, and  $t$  is the year when elections take place. The parameter  $\beta$  captures the percent difference in the average yearly growth factor of exports by firm-product-destination for the firms marginally exposed to a PAN mayor in their municipality. We cluster standard errors at the municipality level. We follow an analogous specification when using municipality-level data. As in Equation 1, we weight the municipality-level regressions by the 2005 population. In the Online Appendix, we show results for an OLS estimation and for a WLS estimation that uses the population in 2005 divided by the number of units (product-destination pairs) within a given municipality. As with homicides, we also provide results of difference-in-differences regressions in the Online Appendix, as well as regressions for different bandwidths and polynomial choices.

Regarding the identification assumptions behind our empirical strategy, random assignment of close PAN wins is insufficient to draw conclusions about the effects of the MDW on exports. We need to show that export under-performance was not triggered by the election of PAN itself, but by PAN’s propensity to engage in the war on drugs, and the ensuing violence that it caused.<sup>8</sup> Municipalities governed by PAN were likely to receive benefits from the federal administration (e.g. de la Garza and Lopez-Videla, 2020), and Calderón was elected on a pro-trade platform that contrasted with that of his main contender for the presidency (Bustos and Morales-Arilla, 2022). These potential biases work in the opposite direction to our identified estimates and invite the interpretation of our results as a lower bound of the true unintended export damages of drug enforcement.<sup>9</sup> Nevertheless, we investigate this conjecture empirically by performing a series of placebo tests. 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/or 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 levels of pre-war violence were less likely to be the target of anti-drug operations. Importantly, they still experienced the socio-economic policies implemented by PAN mayors. 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 sample into 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, which contains the points of entry to the United States (the main consumer

<sup>8</sup>Even if this condition is satisfied, it would be difficult to estimate the causal impact of a particular type of crime (e.g., homicides) on exports. In instrumental variables parlance, random assignment of a PAN win does not imply that the exclusion restriction is satisfied, as it may impact several dimensions of violence other than homicides that we cannot measure. Therefore, we prefer to show only reduced-form regressions.

<sup>9</sup>Another potential concern is that the MDW might have displaced export activities to other areas within Mexico. Such displacement could induce a SUTVA assumption violation if exports moved from areas with a close PAN win to areas with a close PAN loss. We believe this is highly unlikely, given the sample’s spatial dispersion (Online Appendix, Figure A.5) and that close PAN loss locations are only a small subset of potential displacement destinations. A separate concern regarding displacement is that our local estimates may not be representative of the potential aggregate effects of the MDW on total Mexican exports. In non-reported analyses, we find no effects of close PAN mayors on the change in the local share of employment for firms that operated in a single location within a state at baseline. These results suggest that drug enforcement did not induce firms to disperse their operations geographically.

market). We divide our sample of municipalities with close elections in 2007-2008 (198 municipalities) into north (99 municipalities) and south (99 municipalities) using the median of the average latitude of municipalities based on border coordinates data. 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 by reference to any cartel presence at the beginning of the drug war (31 municipalities with cartel presence, 167 without). Table A.3 in the Online Appendix shows municipality characteristics of each subsample.<sup>10</sup> Finally, we also evaluate the effect of a close election of a PAN mayor in periods outside of the MDW.

A remaining concern about these placebos is that PAN might implement different policies depending on pre-treatment municipality characteristics. An example is the diversion of local public resources from public investment in hospitals, education, and infrastructure to security in PAN municipalities that had more violence and cartel activity before the war. If this is the case, part of the drop in exports in the north and cartel-presence samples can be explained by a decline in the provision of some public goods instead of by an increase in violence. However, we find no evidence to support this possibility (Table A.17 of the Online Appendix). We also find no effect on total tax revenues.

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. Our results would also be expected if the MDW eroded the illicit narcotics trade that is often hidden as legal exports. If this were the case, we would expect results to concentrate on exports to the US and from municipalities with more emigration to the US. However, we do not find these patterns in the data (Table A.15 in the Online Appendix).

We interpret our estimates as direct supply-side effects of violence. However, this view could be compromised by potential demand spillovers for exports if local demand had been affected by the MDW. [Almunia et al. \(2021\)](#) show that negative local demand shocks can cause an increase in exports. If this were the case in our setting, our negative estimates would be attenuated, and should thus be interpreted as a lower bound for the direct effects on export supply. However, if financially-constrained exporters use cash from local revenues to finance their exporting activity, or if the production processes have economies of scale or scope, there could be negative spillovers from a depressed local demand for exports. We believe this is not the case. First, for this channel to be relevant, firms in close PAN-win municipalities and close PAN-loss municipalities should have different exposures to local demand. One such case would arise if a large share of the affected firms’ revenues comes from their own municipality. This is unlikely in our setting. For instance, our effects concentrate on electronics and vehicle exports, capital-intensive activities, and larger exporters. Plants in these segments operate at scale, so sales in their municipalities of origin are likely a small fraction of total sales. Finally, our main results are robust to including state fixed effects that capture changes to demand at a more local level (Online Appendix Section A.7).

## 2.5. Descriptive statistics

Table 1 reports summary statistics for municipalities that held elections in 2007 and 2008. Panel A shows socio-economic characteristics. By 2006, Mexico was already a violent country compared to the US. The American rate of 6 homicides per 100,000 pales against the rates observed 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. 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 those that did not elect PAN mayors. However, once the sample is limited to municipalities where the PAN won or lost by less than 5% of the total votes, the baseline characteristics are not statistically different in the treatment and control groups. The lack of difference in 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 for the firm-level data, while Panel C displays trade characteristics when we use 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

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<sup>10</sup>Municipalities in the north or with a pre-existing cartel presence show larger economic activity, according to measures such as GDP and exporting activity. The table confirms that the northern part of the sample had more violence before the war period: the average homicide rate in 2006 in the north sample is 14.4 versus 10.4 in the south sample. Finally, the table shows that the splits are not independent: of the 31 municipalities with pre-existing cartel presence, 20 are part of the north sample.

significant for the unrestricted sample; for the sample that is restricted to municipalities facing close elections, none of the differences are statistically significant. Yet, given that some differences are relatively large in economic terms, in the Online Appendix we show that results are robust when we estimate a local difference-in-differences model (Figure A.2).

In Figure A.5 of the Online Appendix, we show the geographical distribution of all municipalities 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 is regionally dispersed. This is important for our identification because this undermines the possibility that regional shocks, and not the treatment, drive our results.<sup>11</sup>

### 3. Main results

#### 3.1. Effects on violence

In Panel A of Figure 2, we plot the evolution of total homicides in municipalities with a close PAN win versus municipalities with a close PAN loss in the 2007-2008 elections. From 2002 to 2007, homicides were stable in both groups. Between 2007 and 2010, homicides increased sharply in *both* groups, but the increase was much more pronounced in municipalities with a close PAN win. From 2011 on, the two groups started to follow a similar downward trend; despite that decrease, homicides were still much more frequent in 2014 than in pre-war years.

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 2007-2008 elections causes an increase in homicides of between 25.9 and 41.2 per 100,000 population. In municipalities with a close PAN loss, the homicide rate is 15.7. Therefore, a PAN win led to an increase in the homicide rate of between 65% and 162%.<sup>12</sup>

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 which are unrelated to the war on drugs. To rule out this possibility, we run the two contemporaneous placebo tests described in 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 northern part of the sample, 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 pairs of elections between 2004 and 2010. Effects are mainly present in municipalities that closely elect a PAN mayor in the 2006-2007 and 2007-2008 election groups; that is, mayors with terms that coincide with the period of sharpest growth in homicides. As a result, throughout the paper, we focus on those election years.<sup>13</sup> 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.

In the Online Appendix Figure A.1, we present results for a difference-in-differences specification that confirms the previous results and shows no pre-trends in the entire sample or in any of the subsamples (north, south, cartel, no cartel). We also provide evidence that a close PAN victory caused an increase in crimes other than homicides (Online Appendix, Tables A.5 and A.6).

#### 3.2. Motivating evidence on economic effects

A first question regarding the economic effects of violence in this empirical setting is whether local production growth was eroded by the close election of a PAN mayor. Using economic census data, we show in Table A.19 of the Online Appendix that we cannot detect an overall effect with sufficient precision. However, when we restrict the data

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<sup>11</sup>Moreover, this minimizes the possibility of displacement between treated and control municipalities in our close election sample, which could constitute a potential SUTVA violation.

<sup>12</sup>Figure A.6 in the Online Appendix presents the graphical representation of the results. Our sample of close elections is slightly different from that in Dell (2015). In Dell's paper, there is an additional restriction imposed 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 statistical significance.

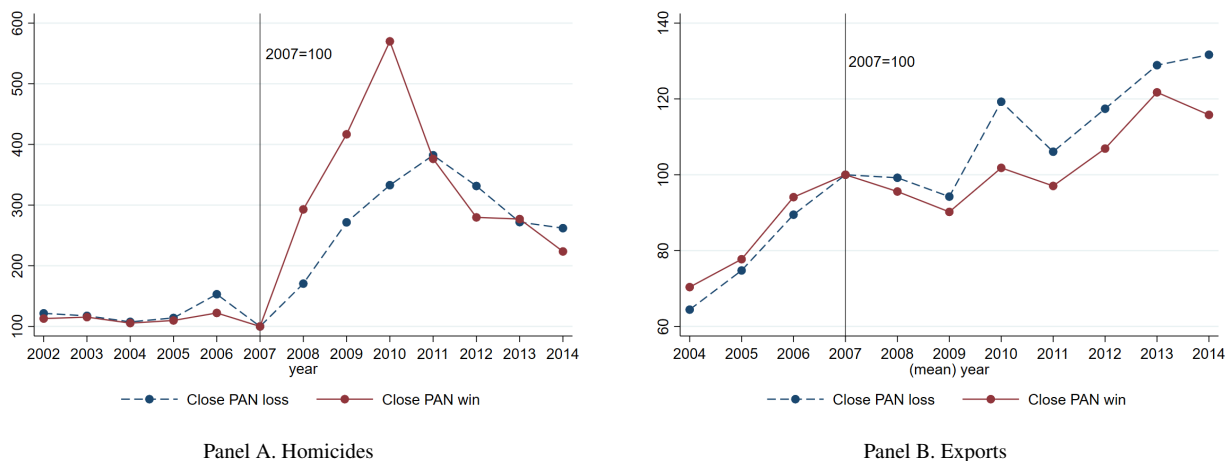
<sup>13</sup>In the Online Appendix, we consider different bandwidths (7%, 6%, 4% and 3%) and degrees of the RD polynomial (2<sup>nd</sup>, 3<sup>rd</sup>, and 4<sup>th</sup>). See Table A.4.



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

**Table 1:** Baseline characteristics

Note: 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% of the total votes. 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.



**Figure 2:** Evolution of total homicides and total exports in municipalities with close PAN elections in 2007-2008  
 Note: Panel A (Panel B) depicts the evolution of total homicides (total exports) in municipalities where the PAN won or lost by a margin smaller than 5% of the total votes in the 2007 and 2008 elections. In both panels, we fix the level in 2007 at 100. We plot homicides from 2002 and exports from 2004 due to data availability.

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Effect after the 07 and 08 elections</i>						
Mean if PAN loss	15.7	15.7	19.1	11.5	15.8	15.4
PAN win	25.9** (12.7)	41.2** (19.0)	43.0** (20.8)	-10.7 (9.1)	44.9* (22.4)	11.7 (12.2)
Linear polynomial	No	Yes	Yes	Yes	Yes	Yes
Municipalities	All	All	North	South	Cartel	No cartel
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
<i>Panel B: Effect after different election years</i>						
Mean if PAN loss	12.2	8.5	13.5	15.7	13.6	18.5
PAN win	-0.8 (3.1)	0.5 (2.0)	30.9* (16.8)	41.2** (19.0)	2.6 (5.2)	7.7* (4.2)
Linear polynomial	Yes	Yes	Yes	Yes	Yes	Yes
Municipalities	All	All	All	All	All	All
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

**Table 2:** Homicides

Note: The table reports estimates of Equation 1. Weights are determined by population size in 2005. The dependent variable is the average annual homicides per 100,000 population in the three years following local elections. In Panel A, columns 1-2, the sample is comprised of municipalities where PAN won or lost by a margin smaller than 5% of the total votes in the 2007 and 2008 elections (198 municipalities). In Panel A, columns 3-4, this sample is divided into two parts using the median of the (average) latitude of the municipalities: north (99 municipalities) and south (99 municipalities). In Panel A, column 5, we report effects in municipalities with pre-existing cartel participation measured in 2007 (31 municipalities, as identified by *Coscia and Rios, 2012*), while in column 6, we report effects in municipalities with no pre-existing cartel activity (167 municipalities). In Panel B, the sample is comprised of municipalities where PAN won or lost by a margin smaller than 5% of the total votes in the election years that were used to perform the estimation. Robust standard errors are reported in parentheses.

to specific industries, we find that production growth in manufacturing activities dropped in areas marginally exposed to a close PAN mayor, while results for activities in services, construction, and retail remain absent. The table shows that both production and production per worker dropped in the manufacturing sector, which is characterized by a larger dependence on capital.

The observed erosion in manufacturing activity is consistent with evidence from the Mexican World Bank Enterprise Survey between 2006 and 2009. This survey is mainly urban and excludes firms operating in the primary sector of the economy. It provides important measures regarding firms' operations and perceived constraints on economic activity just before the start of the MDW (2006) and at its peak (2009). In the Online Appendix, Table A.7, we document an increase in the number of manufacturing firms located in the north that regard crime as an obstacle to their operations. We find negative impacts on other measures, such as firms' time costs of dealing with regulatory hurdles and whether they identify courts as a major obstacle.

### 3.3. Effects on exports

Table 3 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 2008. That 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 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 did not elect a PAN mayor is 3%. The drop in export growth is persistent, in that it does not vanish in the next mayoral term: when we extend the period to 6 years, we find that firm-level export growth drops by 13% annually. Because the firm-level data do not include the universe of exports, we also run the same regressions on the sample of municipality-level exports to verify that our results are not driven by sample selection. Results show the same patterns, albeit with slightly smaller magnitudes. Yearly municipality export growth drops by 15% over three years and by 8% yearly over six years.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Firm-level				Municipality-level			
<i>Panel A: Exports, 3-year growth</i>								
Mean if PAN loss	0.03				0.07			
PAN win	-0.08*** (0.01)	-0.14*** (0.05)	-0.14** (0.06)	-0.21** (0.09)	-0.08*** (0.03)	-0.18*** (0.03)	-0.19*** (0.03)	-0.15*** (0.02)
Observations	17,256	17,256	17,248	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
<i>Panel B: Exports, 6-year growth</i>								
Mean if PAN loss	0.03				0.06			
PAN win	-0.017 (0.02)	-0.14*** (0.05)	-0.12*** (0.04)	-0.13*** (0.05)	-0.04 (0.03)	-0.13*** (0.04)	-0.13*** (0.04)	-0.08*** (0.02)
Observations	14,236	14,236	14,226	12,345	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
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

**Table 3:** Exports

Note: The table reports estimates of Equation 2. Columns 1-4 report results from the data at the firm-product-destination level, and columns 5-8 report results from the data 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% of the total votes 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.

In Table 4, we incorporate placebo tests using the north versus south and the pre-existing cartel presence versus no pre-existing cartel presence splits. For the firm-level data, results lose precision when we restrict the sample to municipalities in the south or to those with no pre-existing cartel presence, as these regions have fewer single-plant exporters. However, the municipality-level data confirms that the negative effect on exports is only present in the

regions that experienced a relative 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 positive export growth, albeit this effect is only statistically significant in the sample of municipalities with no pre-existing cartel presence. In Table A.14 of the Online Appendix, we show that these differences are statistically significant when, instead of restricting the sample across segments of the data, we use the full sample to estimate interactions between subsample dummies and the regression discontinuity terms.

	(1)	(2)	(3)	(4)
<i>Panel A: Firm-level</i>				
Mean if PAN loss	0.03	0.01	0.03	0.002
PAN win	-0.13* (0.07)	0.87 (2.22)	-0.21** (0.09)	-2.86 (2.74)
Observations	14,574	273	14,686	114
R-squared	0.13	0.82	0.14	0.90
<i>Panel B: Municipality-level</i>				
Mean if PAN loss	0.06	0.11	0.07	0.07
PAN win	-0.13*** (0.02)	0.04 (0.03)	-0.15*** (0.02)	0.03* (0.02)
Observations	14,120	2,790	13,889	3,133
R-squared	0.59	0.80	0.57	0.75
Linear RD Polynomial	Yes	Yes	Yes	Yes
Product-destination FE	Yes	Yes	Yes	Yes
Municipalities	North	South	Cartel	No cartel

**Table 4:** Exports, regional and pre-existing cartel presence heterogeneity

Note: The table reports estimates of Equation 2. In Panel A, we report results from the data at the firm-product-destination level, and in Panel B, we report estimates from the data 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% of the total votes in the 2007 and 2008 elections, and (ii) the value exported is positive in the years used to compute export growth (election year and three years later). In columns 1-2, we split the original sample of 198 municipalities into north (99 municipalities) and south (99 municipalities) using the median of the (average) latitude of the municipalities that had a close election in 2007 and 2008. Despite using the median to divide the sample, the sample size in columns 1 and 2 is not the same, as municipalities in the north tend to export to more product-destination pairs. In columns 3-4, we split the sample according to the presence of cartels in 2007 using data constructed by [Coscia and Rios \(2012\)](#): cartel presence (31 municipalities) and no cartel presence (167 municipalities).

A relevant point is that we cannot explicitly address the role of drug smuggling. Part of the effect could be explained by a decrease in the illegal drug exports that were often hidden in legal export shipments. We provide indirect evidence that this is not a likely explanation for our results. In Table A.15 of the Online Appendix, we show that negative effects are not specific to exports to the US, which is the main export destination for illegal narcotics. Similarly, we find that results are not specific to municipalities that have a relatively large share of their population living in the US.

In Table 5, we analyze whether the negative effect of a close PAN win is limited to the war on drugs period. Leveraging the fact that our data range from 2004 to 2014, we compute the effect of a close PAN win over time. We start with the sample of municipalities with close elections in 2004-2005 and finish with the sample of municipalities with close elections in 2010-2011. The results show that a PAN win is not associated with a decrease in exports outside of the war period. Interestingly, the two limit periods (2004-2005 and 2010-2011) yield positive and statistically significant results, which is consistent with the idea that outside of the MDW, municipalities under PAN mayors did not suffer a decline in exports. Furthermore, the negative effects of a PAN win on exports concentrate on the 2006-2007 and 2007-2008 elections, which coincide with the strongest effects on homicides (see Table 2, Panel B).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Elections	04 - 05	05 - 06	06 - 07	07 - 08	08 - 09	09 - 10	10 - 11
<i>Panel A: Log(exports 2nd year of the term/exports election year)</i>							
Mean if PAN loss	-0.23	-0.06	-0.05	-0.04	0.03	-0.01	-0.02
PAN win	0.13*** (0.06)	0.14 (0.27)	-0.13 (0.11)	-0.26*** (0.12)	0.11 (0.21)	0.15 (0.13)	0.26* (0.15)
Observations	15,554	1,527	18,540	16,598	3,906	16,156	11,714
R-squared	0.15	0.39	0.15	0.14	0.50	0.21	0.16
<i>Panel B: Log(exports 3rd year of the term/exports election year)</i>							
Mean if PAN loss	-0.17	-0.09	0.01	0.03	0.03	0.04	0.05
PAN win	0.10* (0.05)	0.19 (0.26)	-0.09 (0.11)	-0.21*** (0.08)	-0.18 (0.11)	-0.10 (0.10)	0.31* (0.17)
Observations	15,193	1,319	17,006	15,335	3,852	15,549	11,188
R-squared	0.15	0.45	0.16	0.15	0.50	0.21	0.16
<i>Panel C: Log(average exports during the 3-year term/exports election year)</i>							
Mean if PAN loss	0.03	0.29	0.55	0.60	0.60	0.52	0.49
PAN win	0.25*** (0.11)	0.15 (0.58)	-0.19 (0.16)	-0.38*** (0.12)	0.03 (0.35)	0.21 (0.24)	1.18* (0.72)
Observations	10,216	854	12,719	11,599	2,463	11,511	8,659
R-squared	0.17	0.47	0.15	0.15	0.55	0.22	0.15
Linear RD polynomial	Yes	Yes	Yes	Yes	Yes	Yes	0.15
Product-destination FE	Yes	Yes	Yes	Yes	Yes	Yes	0.15

**Table 5:** Firm exports, time-series evolution of the effect

Note: 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 the effects on the log of the 2-year export growth factor. Panel B shows the effects on the log of the 3-year export growth factor. Panel C shows the effects on the log of the growth factor of the average 3-year exports.

### 3.4. Extensive margin

Differences in 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 the combination of product and country of destination) might also be relevant.<sup>14</sup> Moreover, comparing extensive and intensive margin results sheds light on the degree to which violence works as a marginal cost or a fixed cost to exporting.

Table 6 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. Results show a negative (that is, a PAN win is associated with fewer disappearances) but not statistically significant effect. These results remain largely the same when we evaluate the change in the number of firms supplying each market from a given municipality, which incorporates exporter entry choices into the extensive margin analysis (Table A.16 in the Online Appendix). The table also shows results for the north versus south and cartel versus non-cartel splits. With the exception of the north, where effects are negative and statistically significant, results are unchanged.

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 or starting these relationships. This finding can be interpreted as a consequence of increasing the marginal costs of exporting. Assuming there are fixed costs to maintaining or establishing export relationships, the lack of net effects on exit and entry decisions suggests these costs did not change substantially as violence increased.<sup>15</sup> An alternative explanation for our findings is that firms value solid relationships and their reputation in foreign trade transactions (Macchiavello and Morjaria, 2015), and may choose to continue to service customers even after negative shocks, especially if they expect the violence disruption to be temporary or less severe in the future. This interpretation is feasible but is only partial. In models that view the exporting decision as a real option, we would expect the higher non-recoverable fixed costs of exporting (sunk costs) to generate a larger inactive region on both entry and exit.<sup>16</sup> This can explain a lack of extensive margin effects even in the presence of increases in the fixed costs of exporting. However, the real option channel would not fully explain the significant negative effects we find in the intensive margin. It would also predict a decrease in entry because waiting becomes more valuable, but the data do not support this prediction (Table A.16 in the Online Appendix).

	(1)	(2)	(3)	(4)	(5)
Mean if PAN loss	0.52	0.55	0.40	0.55	0.33
PAN win	-0.07 (0.07)	-0.16*** (0.05)	-0.14 (0.39)	-0.05 (0.07)	-0.18 (0.71)
Observations	37,686	36,100	705	36,353	475
R-squared	0.29	0.26	0.83	0.27	0.86
Linear RD Polynomial	Yes	Yes	Yes	Yes	Yes
Product-destination FE	Yes	Yes	Yes	Yes	Yes
Municipalities	All	North	South	Cartel	No cartel

**Table 6:** Firm exports, extensive margin

Note: The table provides estimates at the firm-product-destination level, where the outcome variable is a binary variable that takes value one if a firm ceased exporting a given product to a given country. 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. In columns 2-3, we split the original sample of 198 municipalities into north (99 municipalities) and south (99 municipalities) using the median of the (average) latitude of the municipalities that had a close election in 2007 and 2008. In columns 4-5, we split the sample according to the presence of cartels in 2007 using data constructed by Coscia and Rios (2012): cartel presence (31 municipalities) and no cartel presence (167 municipalities).

<sup>14</sup>In Tables A.1 and A.2 of the Online Appendix, 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.

<sup>15</sup>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).

<sup>16</sup>See Dixit (1989a), Dixit (1989b), and Baldwin and Krugman (1989) for seminal work on this topic.

### 3.5. Robustness checks

In Section A4 of the Online Appendix, we show that results are robust to different bandwidths, degrees of the RD polynomials, weighting choices (including OLS), and adding more election years (2007-2010) to our baseline specification. Furthermore, we show that our results are present in difference-in-differences regressions using close PAN wins as the treatment (Figure A.2). The difference-in-differences results are reassuring for two reasons: (i) we formally show the absence of pre-trends in the estimation sample and in the subsamples used to perform the placebo tests (north, south, cartel, no cartel); (ii) despite not being statistically significant, we do observe large differences in foreign trade variables between the close PAN win and close PAN loss municipalities.

In the main regressions, we do not use state fixed effects because they are not instrumental in our identification. However, in Table A.24 of the Online Appendix, in addition to explicitly controlling for foreign demand (with product-destination dummies) and implicitly controlling for countrywide domestic demand shocks (within-country comparison), we further control for state demand shocks that could potentially affect the capacity of firms to supply foreign markets. We perform these exercises by adding state fixed effects to the regressions. Possibly due to spillovers, the effects on violence are severely reduced. When the effect on homicide survives the inclusion of these fixed effects, we still observe a drop in exports.

We also provide additional evidence on the importance of our control for foreign demand. In Table 3, we show that if we do not control for foreign demand, we obtain a coefficient of -0.18 in the municipality-level regressions. Once we add controls for foreign demand, the magnitude drops to -0.15. However, this reduction in magnitude may be due to omitted variable bias or sample selection because singletons (product-destination pairs that have only one municipality as a supplier) are dropped once we include product-destination dummies. To assess which channel drives the results, we run regressions with and without foreign demand controls on the same sample without singletons (Online Appendix Table A.11). The coefficient obtained in the estimation without foreign demand controls in the sample without singletons increases in magnitude to -0.20, meaning that sample bias, if anything, moves the coefficient in the opposite direction, and shows that omitted variable bias plays an important role. Such bias would not exist if we studied the effect of random PAN victories in a sufficiently large sample. However, the sample size is a concern in regression discontinuity settings, and some imbalances can naturally arise. Moreover, if some regions, due to economies of scale, specialize in serving specific markets, the need for a larger sample size becomes even more critical. The coupling of RDD estimates with foreign demand controls provides more reassurance that our effects are not due to imbalances or composition effects.

This is the first paper to document a negative effect of violence on exports in the context of the MDW. A key reason for this finding is that our paper addresses a classic endogeneity concern related to the use of enforcement operations as a source of variation to identify the effects of violence. 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 the treated municipalities. We find that enforcement operations led to a positive effect on homicides, but null or even positive effects on exports. We argue that these results are biased because, unlike a close PAN win, the deployment of law enforcement operations is not exogenous. We present these exercises in Section A9 of 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, given the findings of Lu (2010) and Dai et al. (2016) that export-processing firms in China are less productive and less capital-intensive than the country's domestic producers. Given the existence of export-processing firms in Mexico, it is 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 that identifies the portion of revenues derived from export-processing activities (*maquila*). Online Appendix Table A.19 shows negative effects of the MDW on revenues in the manufacturing sector, which remain unaffected after excluding *maquila* revenues. Therefore, it is reasonable to assume the effects are not entirely driven by export-processing firms.

### 3.6. Aggregate effects

To assess the impact on total exports, we perform two additional exercises. First, in Panel B of Figure 2, we plot the evolution of aggregate exports (which includes relationship appearances and disappearances) in municipalities

with a close PAN win and a close PAN loss in the 2007-2008 elections. Exports in both groups grow at a very similar rate between 2004 and 2007. In 2008 and 2009 (the aftermath of the financial crisis), both groups experience a decline in exports but the decline in close PAN win municipalities is slightly more pronounced. In 2010 and 2011, there is a strong divergence, with PAN win municipalities underperforming in comparison to PAN loss municipalities.

We then estimate regressions to gauge the impact on total exports. Aggregating results has two downsides: First, we cannot control for demand shocks using product-destination dummies. This may lead to composition effects due to specialization and the remaining differences (though not statistically different) in baseline trade activity. Second, the sample size decreases. Our estimates show a large effect if we aggregate our firm-level data across markets. Table 7 evaluates whether violence exposure affects total exports of single-plant firms, finding that firms assigned to a PAN close-win mayor experienced an export growth ratio that is 50% lower. Moreover, these results are larger and more precise in municipalities with baseline cartel presence and in the north. The results of the estimation using municipality-level data are less precise. A local difference-in-differences analysis, however, confirms negative effects on total exports (Online Appendix Table A.3).

	(1)	(2)	(3)	(4)	(5)
PAN win	-0.18** (0.08)	-0.23*** (0.08)	0.12 (0.26)	-0.17* (0.09)	-0.13 (0.12)
Observations	1,485	1,417	68	1,418	67
R-squared	0.00	0.00	0.02	0.00	0.10
Linear RD Polynomial	Yes	Yes	Yes	Yes	Yes
Product-destination FE	Yes	Yes	Yes	Yes	Yes
Municipalities	All	North	South	Cartel	No cartel

**Table 7:** Firm exports, aggregate effects

Note: The table reports  $\beta$ 's of the regression  $y_{fm} = \alpha + \beta PANwin_m + \gamma_1 Margin_m + \gamma_2 PANwin_m \times Margin_m + \epsilon_m$ , 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 a municipality where PAN won or lost by a margin smaller than 5% of the total votes in local elections between 2007 and 2008. Standard errors are clustered at the municipality level.

## 4. How did the MDW affect exports? Heterogeneity and mechanism analyses

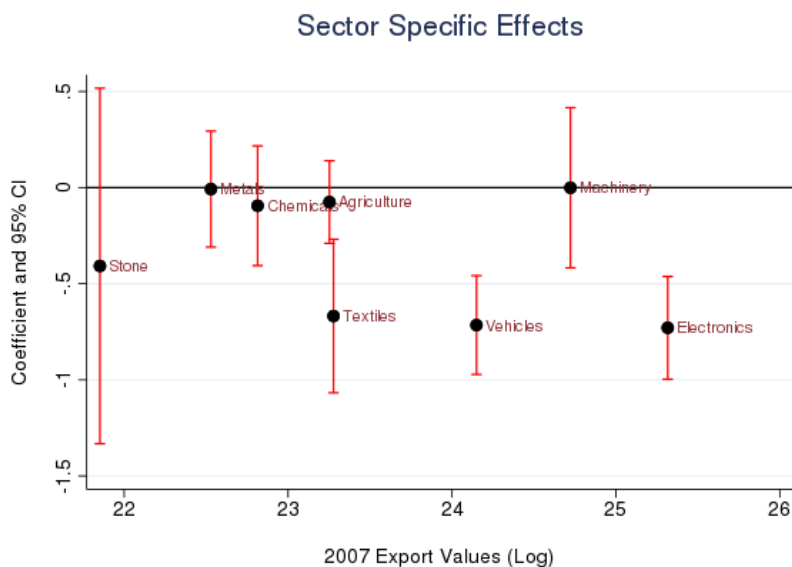
### 4.1. Concentration of effects across product and exporter characteristics

We now explore how the effects of the MDW were distributed across different product categories and exporter characteristics. Figure 3 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 most capital-intensive product categories in the Mexican export mix.

We then split our data around the median employment size of Mexican exporters in 2007 and the median of product capital intensity.<sup>17</sup> We show heterogeneity results across these dimensions in Table 8. Columns 1-3 provide evidence that suggests that large exporters were more affected. When we restrict the sample to product-destination export observations that originate from large firms, we estimate that a PAN win leads to a 27% decrease in export growth, while the restriction to small exporters leads to smaller and statistically insignificant results. Despite these results for the subsamples, the coefficient of the interaction of the dummy of PAN win with the dummy for large exporters is not statistically significant in the entire sample. Columns 4-6 show that the exports of more capital-intensive products suffered more during the war. 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. These patterns are confirmed when we estimate the coefficient of the interaction of the dummy of PAN win with the dummy for high capital-dependent products in the entire sample.

<sup>17</sup>Product-specific measures of capital intensity come from [Shirotori et al. \(2010\)](#), and capture the revealed capital intensity of a given product from the international trade patterns and national capital endowments of competitive exporters. Products are categorized as having or not having high capital intensity along the median score across product categories.





**Figure 3:** Sector specific effects

Note: RDD estimates of the effect of a close local PAN victory on the log of 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 on the x-axis. The export data is formed by triples of firm, product, and destination country.

The fact that effects seem to concentrate on capital-dependent products is consistent with the view that the MDW may have eroded exports by limiting capital accumulation. Separately, stronger effects on large firms are 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. We now explore these and other potential explanations by exploring the mechanisms through which the MDW affected different local economic outcomes.

#### 4.2. Testing predictions for the disamenity channel

Violence can act as a disamenity, leading workers in more dangerous environments to demand an income premium and thus imposing an additional burden on firms. This is especially relevant for workers that have better outside options. We collect salary and income information from the 2000 and 2010 population censuses. We separate workers into skill groups based on educational attainment. In Mexico, a high school degree requires 12 years of accumulated approved years and a college degree requires 17 years of accumulated approved years. We define *low-skill* workers as those who do not have a high school degree or equivalent (fewer than 12 years of schooling), *high-skill* as those who have at least a university degree (17 or more years of schooling), and *medium-skill* as those in between. The census data also allow us to split individuals into workers employed by a firm and other occupations (self-employed and business owners). This split is relevant because the income premium is mostly present in the first group.

In Table 9, we study the effect of a close PAN win in the 2007-2008 elections on income growth between 2000 and 2010. For low- and medium-skill workers, we find negative effects on wage growth, indicating that salary effects caused by drops in labor productivity dominate a potential disamenity premium. However, we do not find effects for the high-skill group, which is consistent with the idea that high-skill workers have better outside options, and tend to do better at negotiating wage adjustments after local productivity drops in violent areas. Low-skill workers, on the other hand, are easier to replace and thus fared worse in terms of salary adjustments. For self-employed individuals and business owners, all schooling groups are negatively affected, which is also consistent with a negative productivity shock in the areas most affected by the MDW.

Table 9 also reports the results for the north versus south and cartel versus no cartel splits. The effects are mostly present in municipalities in the northern part of the sample and with pre-existing cartel activity, where a PAN win increased violence during the drug war period (see Table 2, Panel A). Table 9 also shows the results of the effect of a

	(1)	(2)	(3)	(4)	(5)	(6)
PANwin	-0.27*** (0.08)	-0.04 (0.21)	-0.04 (0.20)	-0.32*** (0.06)	-0.06 (0.10)	-0.06 (0.10)
PANwin x Large			-0.23 (0.21)			
PANwin x High cap. dep.						-0.26*** (0.08)
Observations	13,793	1,153	14,946	9,596	5,574	15,170
R-squared	0.08	0.17	0.09	0.07	0.10	0.08
Linear RD Polynomial	Yes	Yes	Yes	Yes	Yes	Yes
Product-destination FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Large	Small	All	High cap. dep.	Low cap. dep.	All

**Table 8:** Firm exports, heterogeneity

Note: Table reports estimates at the firm-product-destination level, where the outcome variable is the 3-year log of 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 (2) restricts the sample to exporters above (below) the median exporter employment size in 2007. Column 4 (5) in panel B restricts the sample to high (low) capital-intensity products. The capital dependence measure comes from [Shirotori et al. \(2010\)](#), who estimate the revealed capital intensity of the product from the international trade patterns and national capital endowments of their competitive exporters.

close PAN win in the 2004–2005 elections, which increased the wage growth rates. These results confirm that a close PAN victory was associated with worse economic outcomes only when it resulted in an increase in violence.

Another key prediction of the disamenity channel is that violence should induce local workers to take job opportunities in other areas. Again, this effect on migration should be greater on high-skill individuals with better outside options. In [Tables A.22 and A.23](#) of the Online Appendix, we estimate the effects of a close PAN win on population growth and migration patterns. We find that a close PAN win associates with an increase in population growth. This effect, which goes against the expectations of the disamenity channel, is not unique to the region and period of the MDW. Similarly, we see no increase in emigration or a decrease in immigration. Taken together, these results suggest that the main predictions from the disamenity channel (higher wages and population outflows) seem not to explain the observed patterns in the data. Nevertheless, the absence of negative effects on wages observed for high-skill workers highlights the importance of their relative outside options.

#### 4.3. Testing predictions for the capital accumulation channel

The MDW may have induced firms to stop or reduce their local investments in order to minimize their exposure to violence and extortion. A relative erosion in capital accumulation should induce a drop in the productivity and compensation of complementary inputs, such as labor. The evidence found thus far for how the effects of the MDW concentrate on capital-intensive activities and for how wage growth is hampered is consistent with this channel. We now study how local aggregates of firms' capital accumulation decisions were affected by the MDW. We use the 2003, 2008, and 2013 waves of the Mexican Economic Census. Even though the war started in 2006 and violence peaked in 2010 and 2011, we are restricted by the data to using the growth of capital accumulation variables between 2008 and 2013 to learn about the effects of the drug war.

Regarding capital accumulation, Panel A of [Table 10](#) shows a large negative effect on gross fixed capital formation. Investigating the effects on each of the subsamples, we show that the negative consequences of a close PAN victory concentrate in the northern part of the sample and in locations with pre-war cartel activity. We then study the effects of the stock of fixed assets in Panel B. The result is negative but not statistically significant in the full sample; however, in the north sample, the effect is negative and statistically significant.

We also use the economic census to study employment outcomes. Panel C shows no statistically significant effects of a PAN win on overall employment growth, but negative effects seem to concentrate in the north of the country. Panel D confirms the overall negative effects of a close PAN win on average wage growth. This negative effect concentrates in northern municipalities and in municipalities with a pre-existing cartel presence.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel A: High skill</i>							
PANwin	0.00 (0.01)	0.02 (0.02)	0.01 (0.02)	0.01 (0.01)	0.01 (0.01)	-0.10*** (0.02)	0.03*** (0.01)
Observations	165	84	81	29	136	110	194
R-squared	0.12	0.25	0.02	0.34	0.01	0.34	0.10
<i>Panel B: Medium skill</i>							
PANwin	-0.03*** (0.01)	-0.03*** (0.01)	0.00 (0.01)	-0.03*** (0.01)	0.00 (0.01)	-0.03** (0.01)	0.02*** (0.01)
Observations	198	99	99	31	167	181	243
R-squared	0.12	0.20	0.02	0.44	0.06	0.10	0.05
<i>Panel C: Low skill</i>							
PANwin	-0.05*** (0.02)	-0.05*** (0.02)	-0.01 (0.01)	-0.05*** (0.02)	-0.02 (0.01)	-0.06*** (0.02)	0.05** (0.02)
Observations	198	99	99	31	167	198	246
R-squared	0.28	0.29	0.03	0.37	0.07	0.15	0.25
Occupation	Worker	Worker	Worker	Worker	Worker	Self-employed / business owner	Worker
Municipalities	All	North	South	Cartel	No cartel	All	All
Elections	07-08	07-08	07-08	07-08	07-08	07-08	04-05

**Table 9:** Individual income growth by skill-occupation decomposition

Note: The table reports  $\beta$ 's of the regression  $\log(y_{m2010}/y_{m2000})^{1/10} = \alpha + \beta PANwin_m + \gamma_1 Margin_m + \gamma_2 PANwin_m \times Margin_m + \epsilon_m$ , where  $y_{mt}$  is the average income of a skill-occupation group in municipality  $m$  in year  $t$ . The data come from the population censuses of the years 2000 and 2010 and include all economic sectors. 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 then weight regressions by the size of the 2005 population. The sample is comprised of municipalities where (i) PAN won or lost by a margin smaller than 5% of the total votes in the 2007 and 2008 elections (or 2004 and 2005 in column 7), and (ii) the value of the variable was positive in 2000 and 2010. Columns 1, 6, and 7 show estimates for an unrestricted sample. In columns 2 and 3, we split the original sample (198 municipalities) into north (99 municipalities) and south (99 municipalities). In columns 4 and 5, we split the original sample into municipalities with (31 municipalities) and without (167 municipalities) cartel presence in 2007.

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 MDW invested less and reduced their fixed capital accumulation. While we do not find robust effects on overall employment, we confirm negative effects on average wage growth.

Another channel possibly connecting violence and export under-performance during the Mexican drug war is the erosion in the local capacity to attract foreign capital. Assessing this hypothesis requires yearly local foreign investment data. We use data from fDi Markets, a proprietary service from the Financial Times, that has a comprehensive database of cross-border 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 look at greenfield projects located in municipalities with close elections. Panel A in Table A.18 of the Online Appendix 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 for the average greenfield investment between 2007 and 2012 (columns 1-3) and between 2007 and 2010 (columns 4-6). The CAPEX of the average greenfield project was \$178 million lower under closely elected PAN mayors. The effect of a close-win PAN mayor on the number of jobs created was either null or positive, and the average capital per new job was reduced by at least \$650,000. Panel B provides estimates for the same specification for close elections between 2004/2005 on the growth of the same outcomes between 2004 and 2009 (columns 1-3) and between 2004 and 2007 (columns 4-6). Results show either a null or positive effect of a closely-elected PAN mayor on the average CAPEX of greenfield investments. These results seem to confirm the findings above regarding the erosion of capital accumulation in areas disproportionately affected by the MDW.

#### 4.4. Testing predictions from the misallocation channel

Violence can erode economic activity by diverting local resources away from production and toward protection, as insecurity leads economic agents to increase their spending on security services. Through this “misallocation”

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Gross fixed capital formation</i>						
Mean if PAN lost	0.02	0.01	0.03	-0.00	0.06	0.01
PANwin	-0.24** (0.10)	-0.42*** (0.11)	0.14 (0.12)	-0.28* (0.14)	0.01 (0.09)	0.12 (0.12)
Observations	182	94	88	29	153	222
R-squared	0.12	0.29	0.08	0.21	0.04	0.04
<i>Panel B: Fixed assets</i>						
Mean if PAN lost	0.02	0.04	-0.01	0.01	0.02	0.11
PANwin	-0.04 (0.09)	-0.15** (0.06)	0.17 (0.11)	-0.06 (0.12)	0.02 (0.06)	-0.01 (0.02)
Observations	198	99	99	31	167	246
R-squared	0.10	0.23	0.20	0.20	0.00	0.01
<i>Panel C: Employment</i>						
Mean if PAN lost	0.01	0.01	0.00	0.01	0.01	0.03
PANwin	0.01 (0.01)	-0.03* (0.02)	0.05* (0.03)	0.00 (0.02)	-0.01 (0.03)	0.01 (0.02)
Observations	198	99	99	31	167	243
R-squared	0.02	0.06	0.08	0.11	0.02	0.01
<i>Panel D: Average wage</i>						
Mean if PAN lost	0.04	0.03	0.05	0.03	0.05	0.05
PANwin	-0.05*** (0.01)	-0.04** (0.02)	-0.01 (0.03)	-0.04** (0.01)	-0.03 (0.02)	-0.01 (0.01)
Observations	198	99	99	31	167	243
R-squared	0.10	0.09	0.04	0.23	0.07	0.01

**Table 10:** Economic census, capital

Note: The table reports  $\beta$ 's of the regression  $\log(y_{m,t'}/y_{m,t})^{1/5} = \alpha + \beta PANwin_m + \gamma_1 Margin_m + \gamma_2 PANwin_m \times Margin_m + \epsilon_m$ , where  $t' = t + 5$  and  $y_{m,t}$  is the value of a particular variable in municipality  $m$ , year  $t$ . The data come from the economic censuses of the years 2003, 2008, and 2012. We weight regressions by the 2005 population. 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 2004 and 2005 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. In columns 2 and 3, we split the sample (198 municipalities) into north (99 municipalities) and south (99 municipalities). In columns 4 and 5, we split the sample into municipalities with (31 municipalities) and without (167 municipalities) cartel presence in 2007. Panel A shows effects on the local gross fixed capital formation, Panel B on the value of fixed assets, Panel C on employment, and Panel D on average wages.

channel, violence should induce 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.

While we do not have data on firms' protection expenditures, we are able to provide indirect evidence on this channel using the occupation of individual workers as registered in the population census. In [Table A.20](#) of the Online Appendix, we use occupation information to assess the growth in the number of security guards and their salaries as a function of a close PAN victory. Even though salaries grow less in PAN win municipalities – which is consistent with the findings documented in the previous two subsections – we observe an increase in the number of guards, especially in areas with a pre-existing cartel presence. Although indirect, this evidence is consistent with the view that the MDW displaced local resources from production and into protection.

## 5. Conclusion

The Mexican drug war has attracted widespread research attention because of the scale of its consequences on violence and crime. We go a step further by assessing how the MDW affected the real economy. We document a negative change in trade patterns, with export growth decreasing significantly after a close PAN win within the period

and geographic focus of the MDW. We interpret our results as evidence of the export effects of the violence triggered by the MDW.

Leveraging close elections to compare exports of the same product to the same destination allows us to abstract from the potential demand factors, and focus on how violence affects the capacity of firms to supply external markets. We also provide new evidence on the relationship between violence and trade. Using firm-level microdata, we find that firms located in a municipality that was exposed to a PAN mayor faced lower export growth rates, but we do not find effects on firms' decisions to exit or enter foreign markets. This is consistent with the view that violence increases the marginal costs of exporting but does not significantly affect the fixed costs of sustaining trading relationships.

A key objective of this study was to identify the channels connecting the MDW to economic outcomes. First, we find that large exporters and the exports of capital-intensive products were affected disproportionately. Second, we find a reduction in capital accumulation and wages, and evidence suggesting that violence led to an increase in the number of security service providers. These results indicate that as the rollout of the war led to local predatory environments, firms mobilized resources from production to protection while limiting capital inflows, constraining labor productivity growth and disproportionately affecting the capital-intensive activities of larger local 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 not only cost lives but damaged large firms and capital-intensive activities, both of which are key to sustained economic growth.

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