Violence and economic disruption: Firm-level evidence from Mexico

Eduardo Montoya*

January 28, 2016

Abstract

While high levels of crime and conflict challenge many regions, limited microeconomic evidence exists to establish their impacts on firms. I study the economic consequences of recent high levels of violence associated with the Mexican drug war, relying on microdata from national business victimization surveys conducted in 2012 and 2014, and monthly panel data from 8,000 manufacturing and construction establishments in more than 70 cities between 2007 and 2013. Since 2008, violence has spread across Mexico from city to city, creating spatial and temporal variation in cities' exposures to crime. I exploit this staggered incidence to identify the firm-level impacts of drug-related violence, first within a fixed effects design, and second within a novel difference-in-differences design employing structural breaks in homicide rates and synthetic controls at the firm-level. In all sectors, I find significant declines in activity when violence increases; in the industrial sector, I find that revenue, employment and hours worked fall by 2.5-4% in the 24 months following a large structural break. But I find no significant increase in wages, no significant impacts on private security investments, and I find that the business impacts of violence persist even after controlling for economic crimes like theft. Effects are heterogeneous by firm size and sector, and consistent with greater impacts among smaller firms and non-traded goods.

*PhD candidate, Department of Agricultural and Resource Economics, University of California, Berkeley. Email: emontoya@berkeley.edu. I thank Alain de Janvry, Sofia Berto Villas-Boas, Elliott Collins, Angeli Kirk, Jeremy Magruder, Matthew Ranson, Steven Raphael, Gordon Rausser, Elisabeth Sadoulet, Leo Simon, Laura Vargas, and participants at the ARE Development Workshop and ARE Department Seminar. I gratefully acknowledge funding for this work under a Dissertation Fellowship from the UC Institute for Global Conflict and Cooperation (IGCC) and a Dissertation Research Grant from UC Institute for Mexico and the United States (UC MEXUS). Early work was conducted as a summer researcher at the Banco de México Department of Regional Economies. Many thanks to Natalia Volkow and staff at Mexico's Instituto Nacional de Estadística y Geografía (INEGI) for their tireless support. This work was accomplished through processing of microdata at INEGI's Microdata Laboratory in Mexico City. The conclusions and opinions expressed in this work are solely my responsibility as author and are not part of the official statistics of INEGI's National System of Statistical Information and Geography. Online appendix at https://are.berkeley.edu/sites/default/files/job-candidates/pdfs/JMPMontoyaModelAppendix.pdf.

1 Introduction

How do high levels of violence disrupt economic activity? Businesses face multiple threats when local violence and related crimes peak. Fear and insecurity may lead consumers to reduce purchases. Fear of extortion and theft may lead firms to adopt a lower profile, scaling back production and employment, to adopt costly security measures, or to exit. Workers may resist working and traveling after dark, demand compensating wages, or even migrate away from violence. What are the costs of such behaviors? And can we distinguish between these alternate channels?

In this paper, I study the economic consequences of recent high levels of violence associated with the Mexican drug war. I focus on understanding indirect costs—those resulting from distortions to consumer, worker, and (formal sector) firm decisions, or what I call economic disruption—rather than direct measures of lives and human capital lost, property lost, or police, military, and health system expenditures.[1] I present new evidence quantifying the job and income losses due to major increases in violent crime using monthly production data from 2007-2013 for a sample of 8,000 Mexican manufacturing and construction firms. I also present evidence of the impact of violence on private security, drawing on a large, nationally representative business victimization survey conducted in Mexico in 2012 and 2014. Finally, I characterize the distribution of impacts across firms, and exploit this same heterogeneity to infer which supply- and demand-side factors are the most likely drivers of business impacts in particular major sectors.

Empirically, the challenge is to find variation in violence that is plausibly exogenous to prevailing economic trends. Drug violence associated with the Mexican drug war has spread from city to city, for reasons that were not likely driven by local fluctuations in economic activity. In particular, much of this violence has been driven by inter-cartel rivalries over territory, exacerbated by arrests and killing of key leaders under a newly aggressive enforcement strategy since 2006.[2] Still, the dramatic increases in violence in Mexico since 2008 have coincided with the onset of, and recovery from, the global financial crisis. If firms vary in their exposure to this crisis (or other economic trends) in ways that correlate with their exposure to violence, we may conflate the effects of violence with those of prevailing economic trends.

To respond to these threats, I take two approaches. In analysis of the business victimization data, I rely on modern panel data methods, and benefit from the fact that between 2012 and 2014, the sharpest fluctuations associated with the global financial crisis had passed. Next, using the industrial production data, I adopt a novel difference-in-differences design that exploits the spatial and temporal variation in the onset of drug wars across Mexican cities, as well as the availability of detailed production data for a large pool of firms across the country during periods

[1]See Czabanski (2008) and World Bank (2009) for cogent reviews of conceptual issues, methods, and history of broader cost of crime estimates.

[2]A small body of work finds evidence that government security strategies have in some cases had the perverse effect of increasing violence by exacerbating existing rivalries among cartels, or between cartels and local law enforcement (Dell 2015; Chaidez 2014; Lindo and Padilla-Romo 2015).

that predated the onset of drug violence. As illustrated in Figure 1, the onset of drug violence was highly discontinuous in many cities, so I begin by grouping cities into those that experienced sudden drug wars, and those that did not.[3] I do this by testing for structural breaks (Bai & Perron 2003) in the monthly homicide rate that should indicate the onset of a drug war.[4] The identifying assumption in a regression discontinuity design is that potential outcomes are a smooth function of the running variable, time; but for the event of interest, they would have continued along a smooth path. As I show below, this identifying assumption is corroborated by the pattern of economic activity prior to the average structural break, which does not exhibit discontinuities. However, I go on to bolster the identification by implementing a synthetic control design. That is, for each firm in those 12 cities that did experience large structural breaks,[5] I use the large pool of firms in cities without large structural breaks to construct a synthetic control (Abadie, Diamond, Hainmueller 2010) that best replicates the behavior of the firm in the large structural break city prior to the outbreak of violence. Essentially, this method can be seen as a selection on unobservables design, in which matching on pre-intervention values of the outcome variable allow me to implicitly match on those unobservables that shape firms' reactions to fluctuating economic conditions. Because these matching procedures are at the level of the firm, I remain able to test for heterogeneous effects across characteristics in order to identify the mechanisms of impact.

Drawing on the business victimization data, I exploit changes in local homicide rates in each city between survey years to identify the impacts of violence. I find that smaller establishments are significantly more likely than large firms to report reduced business hours when homicide rates increase. Owner visits to the establishments decline similarly. This heterogeneity by size is only prominent among commercial and service establishments, and does not appear to extend to industrial firms. Strikingly, however, neither victimization nor actions to protect the establishment—hiring guards, installing alarms—increase with homicide rates. This implies that business performance suffers during periods of high violence for reasons beyond their actual victimization. Overall, the victimization surveys suggest that small retail and service establishments are most affected during episodes of high violence, while manufacturers are least affected. Reduced owner visits to the establishment are an important potential mechanism, while impacts do not appear to be driven by direct increases in private security expenditures or victimization.

Turning to actual firm-level production data, I find that even large industrial establishments

[3] Also see Figure 2, which highlights the geographic distribution of drug violence by mapping the annualized monthly homicide rate across each of 73 urban areas in June of 2007, 2009, and 2011.

[4] Narrative evidence characterizing events in each of these cities is provided in a companion paper.

[5] I.e., while structural breaks are identified based on Bai & Perron (2003), I further restrict the sample to those cities that saw increases in the average annualized monthly homicide rate exceeding 30, and that exceeded the pre-break average level by 3-4 times the pre-break standard deviation. I identify 12 cities that experienced structural breaks on this basis, but omit one because the industrial survey provided no data on establishments in that city after the break occurs.

perform poorly during episodes of high violence. Based on monthly production data from 2007-2014 for over 8,000 manufacturing and construction establishments, I find that during the first 24 months following major outbreaks of drug violence in a given city, average revenue and work hours among large industrial establishments fall by 2.5-4%. Consistent with the results of the victimization survey for the industrial sector, I find no significant differences between relatively larger and smaller industrial establishments. I also find no significant impacts on average earnings per worker or on the labor intensity of earnings.

But the divergent pattern of effects seen in the victimization survey suggests that different mechanisms may be at work in each sector. In order to understand this, I begin with a standard model of heterogeneous firms, in which price-taking or monopolistic firms hire inputs and produce output subject to firm-specific productivity levels and input prices. Crime is experienced as a common tax to one or more model primitives, but restricted to be the same across firms. Nevertheless, firms' inherent heterogeneity implies that they will react differently to common shocks, providing a lever to distinguish between alternate mechanisms through which crime may affect economic activity.

Guided by the model, I merge the business victimization survey with local averages from the economic census at the city by detailed industry by firm size category-level, and revisit both the business victimization survey and industrial data. In all sectors, I find little evidence to suggest that impacts are driven by a labor market distortion, such as reluctance to work late hours or out-migration. Thus, I argue that either demand or productivity shocks best rationalize my findings. While I am not able to distinguish between these two econometrically given current data, the distribution of impacts across firms does put structure on those shocks that may help to determine plausibility. And if we assume that either a productivity or demand shock is primarily at work in all sectors, then we may ask why such a shock would behave one way in the commercial and service sectors—i.e., strongly correlated with business size—but not in the industrial sectors. I consider a range of explanations.

This study contributes both thematically and methodologically to a still small empirical literature analyzing responses to conflict and violence at the firm-level; more broadly, it contributes to work studying the role of the external environment on firm-level productivity.[6] This remains a first order question given the importance of entrepreneurship and firm performance to local employment and growth. My results highlight violence as one aspect of the local environment that may degrade firm performance; in particular, it is suggestive that business owners are deterred from tending to their establishments. To my knowledge, this mechanism has not previously been emphasized in the empirical literature. Patterns of heterogeneity by size of establishment suggest that additional management structure may play a role in mitigating these

[6] See Syverson (2011) for a discussion of external drivers of productivity differences, including spillovers through agglomeration, impacts of market competition, and other factors.

effects. Methodologically, the combination of structural breaks and individual-level synthetic controls is novel and may be useful in other contexts; I address methodological issues including use of nearest neighbors to minimize attrition, and provide an inferential strategy to account for the cross-city dependency structure induced by the synthetic controls procedure.

This study also makes a theoretical contribution by adopting a unified modeling framework that includes multiple channels through which crime and conflict may affect firms. The model yields clear, testable predictions, formalizing the relationship between shocks in violence, firm-level outcomes, observable dimensions of heterogeneity, and demand-side characteristics. Existing empirical studies frequently emphasize a single channel through which violence affects firms, such as worker absenteeism or productivity declines. While such a specific focus may be necessitated by data limitations or justified ex post, the more flexible approach used here is well suited to distinguishing between unknown channels through which violence may affect firm behavior in diverse settings.

The remainder of the paper is structured as follows. In Section 2, I provide background on the Mexican drug war. In Section 3, I present my theoretical framework. Section 4 describes my data, and Section 5 the identification strategy. Section 6 presents results and discussion, and Section 7 concludes.

2 Background and related literature

2.1 The Mexican drug war

A range of factors over the last 40 years have led to the growth of Mexican drug trafficking organizations (DTOs), including rising consumer demand, counter-narcotics successes in other producer countries, and decades of one-party rule in Mexico (Recio 2002; Astorga and Shirk 2010; Beittel 2013; Osorio 2013).[7] Throughout, efforts to combat drug trafficking and drug violence in Mexico have led to federal deployments in Mexican cities and rural areas to quell violence or lead eradication efforts. Never, though, has drug violence in the country reached the levels it attained since 2008.

Drug trade-related violence in Mexico claimed nearly 50,000 lives between 2006 and 2011.[8]

[7] The best estimates are that Mexican DTOs earn aggregate export revenues of about \$6.6 billion annually, with cocaine (52%) and marijuana (23%) the largest contributors, followed by Colombian heroin (11%), methamphetamines (9%), and Mexican heroin (6%) (Kilmer et al. 2010, Table 5.2). Because prices rise rapidly along the supply chain after drugs enter the US, and because other countries transport drugs to the US, this is far less than American retail expenditures on the major illicit drugs, estimated at \$109 billion (ONDCP 2014). Even with the highest estimates, less than 15% of Mexican DTO revenue is believed to be from non-drug sources (International Crisis Group 2013). To give a sense of scale, Mexican GDP in 2012 was \$1.2 trillion dollars.

[8] This is based on data released by the Office of the Presidency that specifically identifies homicides related to organized crime. In official crime statistics, total intentional homicides increased from 71,000 between 2002-2007 to 113,000 between 2008-2013, a 60% increase and a difference of 40,000 homicides.

These homicides have been geographically concentrated, with individual urban areas seeing overwhelming violence against a national rate that rose from a historic low of 8.1 in 2007 to a peak of 23.5 per 100,000 residents in 2011. From 2008-2010, the border city of Ciudad Juárez saw an average homicide rate of 182; from 2011-2013, the average rate in the port city of Acapulco was 158. To put these numbers into context, no Metropolitan Statistical Area in the U.S. had a homicide rate above 30 in the 2000s; among individual U.S. cities, the highest rates were New Orleans, LA in 2007 (94), Gary, IN in 2001 (79), and Flint, MI in 2012 (61). Rates at these levels are comparable to the rates of battle death in some conflict settings: Iraq '91 (125), Bosnia-Herzegovina '92 (235), and Syria '14 (300).[9]

Figure 1 provides a detailed view of homicide rates in selected cities since 1994. Figure 2 reviews the geographic spread of violence across 73 Mexican cities since 2007. Not surprisingly, those cities that have experienced the largest outbreaks of violence tend to be strategically important, whether as points of entry into the U.S. (Ciudad Juárez, Nuevo Laredo, Tijuana), as port cities for incoming shipments of drugs and precursors from abroad (Acapulco, Mazátlan), or as transshipment cities along major trafficking routes (Chihuahua, Torreón, other cities along the Pacific coast).

Abstracting from the byzantine details of rivalries between DTOs, a small set of papers consider whether counter-drug policies have had systematic impacts on drug violence. To date, I am aware of no work that has identified changes in local economic conditions as major contributors to the outbreak of these turf wars. The most frequently cited explanation for the eruption of violence since 2008, compared to previous periods of more limited drug-related violence, has been that the aggressive military campaign initiated by Mexican president Felipe Calderón since December 2006 helped upset an already precarious equilibrium between DTOs. Empirical work thus far supports the argument that enforcement actions, including "kingpin strategies" that target leaders of criminal organizations, have been causally related to short-term increases in violence (Dell 2015; Calderón et al. 2013; Lindo & Padilla-Romo 2015), and that funding for local security investments increased violence (Chaidez 2014).

Finally, it is important to emphasize the fear, uncertainty, and disruption created by this violence. The examples in Figure 1 demonstrate that in particular cities, annualized homicide rates per 100,000 persons reached levels over 100 for extended periods of time. Additionally, the gruesome nature of the violence and its public displays were frequently intended to create fear among competing DTOs as well as the local population. Combined with an increasing number of disappearances, kidnappings and extortion, these episodes have generated intense

[9] U.S. figures based on analyses of FBI Uniform Crime Reports from 1999 to 2013; battle deaths from the Uppsala Conflict Data Program (Version 5.0-2015), and national population from the World Bank. Worldwide, across the largest cities in 127 countries between 2005 and 2012, the eight most violent cities by homicide rates were all in Latin America and the Caribbean, with Lesotho and South Africa the highest outside the region. Among 18 countries in the Americas in 2011, the percentages of total homicides related to organized crime or gangs was 30% in the median country and over 45% in the upper quartile (UNODC 2014).

media coverage. Military and federal police deployments may themselves have created local disruption.[10] Surveys show high levels of pessimism about authorities, and corresponding under-reporting of crime. In cross-sectional evidence, surveys indicate changes in daily activities. In high violence areas, people carry less cash, enjoy less nighttime entertainment, and take fewer taxis; enumerators describe the population as terrorized (Díaz-Cayeros et al. 2011). Individual-level responses are similar in high crime environments in Caribbean countries, where firm-level responses include hiring security and closing before dark (UNODC 2007). In anecdotal evidence from Mexico, firms describe voluntarily lowering their profile—specifically, removing business advertisements from the side of city buses, and cutting back on production—in order to avoid potential extortion and kidnapping. In sum, there is considerable reason to suspect that drug trade-related violence may have adverse impacts on economic activity.

2.2 Related literature

Typologies of violence frequently distinguish between deaths during war and conflict versus intentional homicides outside of war.[11] But there are similarities between civil conflicts and violence related to organized crime groups. Both are often characterized by violence that is extreme but highly localized, and fought using small arms and munitions that do not lead to the kind of physical destruction seen in inter-state wars (Blattman & Miguel 2010). Further, the relevant combatants are often distinct from civilians, such that violence is to some extent targeted rather than wholesale. Thus, I find that the most relevant literature includes work related to both organized crime and civil conflict; I briefly review these below. I then highlight related studies on individual-level impacts of drug violence in Mexico.

Firms and GDP per capita during episodes of violence. I describe three papers that study firm-level outcomes during episodes of violence, each of which emphasizes a different channel. The closest analogue to the current work is Rozo (2014). Based on an instrumental variables design, she studies manufacturing plants using annual census data in Colombia. The average firm in her data employed 82 in 1995 and 67 in 2010. The period saw a dramatic decline in violence, with the national rate falling from near 70 per 100,000 to around 35 between 1995 and 2010. She finds strong impacts—a 10% increase in the homicide rate leads to a 1.7% decline in average revenue, a much larger increase in output prices of 5.3%, and a 3.8% decline in housing

[10] E.g., army personnel surrounded and disarmed police departments in Nuevo Laredo, Reynosa, and Matamoros in Tamaulipas in January 2008; similar actions in Ciudad Juárez led to police strikes; and troops frequently employ highway checkpoints and raids (STRATFOR Mexico Security Memos: 2008-01-21, 2008-04-07)

[11] *E.g.*, based on characteristics including premeditation, motivation, context, instrumentality, and the relationship between victim and perpetrator, the UNODC (2014) classifies intentional homicide into three main typologies: homicide related to other criminal activities; homicide related to interpersonal conflict; and homicide related to socio-political agendas. Drug trade-related violence falls under the first and terrorism, war, and civil conflict under the third. See, *e.g.*, Berman & Matanock (2015) for a useful typology of insurgencies.

rents, though only a 0.7% increase in nominal wages. While Rozo is not explicit in stating that migration was the key mechanism, this is the primary channel emphasized in the conceptual framework and empirical results.[12]

In the context of post-election ethnic violence in Kenya after 2007, Ksoll, Macchiavello, and Morjaria (2014; hereafter, KMM) emphasize a related labor channel. They study 104 flower exporting firms near 16 towns (who account for over 90% of flower exports), using both production data and survey evidence. The average firm in their data employed between 456 and 480 workers. KMM report the election violence took the lives of 1,200 people[13] and displaced at least 500,000. They find a 20% decline in weekly revenues during the violence, and that worker absenteeism was the key channel affecting firms; they perform a calibration exercise to compute an implied 16% increase in operating costs. Klapper, Richmond, and Tran (2013; hereafter, KRT) study the effects of civil conflict in Ivory Coast on the formal private sector in the years preceding and immediately following the political crises in 1999-2000 and the Civil War in 2002. The average firm in their data employed 56 employees. The authors' calculations show that the number of conflicts were as high as 6 per 10,000 inhabitants in some departments.[14] KRT find that average log productivity declined between 16-23%. Like the present study, they use heterogeneity to make some inferences about channels, suggesting that increased costs of imported inputs may be one driver, with little evidence of demand effects.

Finally, two studies use within-country variation in conflict and violence to explore effects on GDP per capita. Abadie and Gardeazabal (2003) examine the effects of terrorism in the Basque Country of Spain from 1968 to 2000, a period that saw an average of 0.82 terrorism-related killings per 100,000 per year in the Basque region (with a maximum of 4.3 per 100,000 in 1980). Using a synthetic control method, they find that terrorism caused a ten percent decline in GDP per capita relative to a synthetic control. Pinotti (2012) studies an increase in mafia activity in two regions of Italy. Compared to a synthetic control, he finds that the increased mafia presence led to a differential increase in the homicide rate of up to 5 per 100,000 and a 16% decline in GDP per capita.

Using death rates as a measure of intensity, the contrast in magnitudes of impact across studies is intriguing. In Colombia in the 2000s, a decline in the national homicide rate from 70 to 35 per 100,000 implies increased real income around 6%. Taken as a proxy for GDP per capita,

[12] A model of heterogeneous firms as in Section 3 would suggest additional effects from a shock to labor costs due to migration: that log employment should decline at least as much as log revenue, and that impacts across establishments should be increasing in labor intensity of revenue. Failing to find evidence consistent with these predictions might suggest either a search for additional mechanisms, or that the model I propose is inappropriate to the Colombian setting—both useful insights.

[13] The national population in 2007 was 37M, implying a national death rate of 3.2 per 100,000. This does not account for the temporal and spatial concentration of the violence. Assuming those deaths occurred during a two week period, the annualized national rate would have been 83 per 100,000 during those weeks.

[14] Based on UCDP data, I find battle deaths at the national level as high 3.6 per 100,000 inhabits in 2002. KRT characterize the conflict as low-intensity but repeated.

the magnitude is smaller than that seen in the synthetic control studies despite much greater variation in homicide rates. In KMM, an increase in the death rate around 3 per 100,000 leads to 20% short-term declines in revenue (though the tight concentration of violence over time makes it difficult to compare with other studies). Characteristics of the violence—its type, its level, duration, and geographic concentration—as well as characteristics of the economy or outcomes studied—aggregate or firm-level measures, labor and capital mobility, state capacity, firm types, and firm sizes—may all play roles in explaining the wide variation.

Prior work on economic impacts of Mexican drug violence. Several papers have begun to explore the economic implications of drug violence in Mexico, including its aggregate effects (Robles, Calderón, Magaloni [RCM] 2013; Balmori 2014), as well as its impacts on labor markets and migration (Dell 2015; RCM 2013; BenYishay and Pearlman 2013; Velásquez 2014; Basu and Pearlman 2013), and housing prices (Ajzenman, Galiani, Seira [AGS] 2014).

Some of the largest impact estimates have come from aggregate data in synthetic control designs; but effects have not always been shown to be statistically significant. These approaches have in common that the identifying variation is coming from extreme, rather than marginal, changes in violence. RCM employ a strategy similar to the one I use in the present study. They find that municipalities experiencing sudden large increases in the number of annual homicides (340 municipalities) consume 4% and 7% less electricity in the first and second years after the increase compared to synthetic controls composed from municipalities that never experienced such increases. However, they provide no falsification tests or other inferential strategy. Key differences in our approaches include the restriction to large urban areas versus all municipalities, the use of monthly versus annual data, the use of microdata versus a proxy for aggregate outcomes, the method for classifying "treated" regions, the use of heterogeneity to distinguish channels, and the inferential strategy. Next, using annual data at the state-level, Balmori finds that per capita GDP declined between 4-13% in 11 states following the initiation of military operations. While the author concludes that average impacts were not significant in placebo tests, he emphasizes significant impacts in Chihuahua, Durango, and Guerrero.

Evidence of labor market impacts has been mixed, with most studies finding effects only among subpopulations. Dell studies the effect of government crackdowns within a network model in which drug trafficking gets diverted to new areas. She finds that gaining a predicted trafficking route increases homicide rates by 1.7 per 100,000 (Table 7, Panel B), and that female labor force participation rates declines by 1.3 percentage points relative to 51% baseline, while the point estimate for men is negative but not significant (Table A-58, Panel B). Attributing this effect entirely to the change in the homicide rate, the implied impact for a 10-person increase would be 6.7pp. Dell also finds that informal sector log wages are marginally significantly affected. RCM study labor market outcomes in an IV design; they find significant overall declines in participation rates of 2.2pp for a 10-person increase in the homicide rate and a 1.5pp increase in unemployment. Velásquez uses an individual-level fixed effects design, with data in 2005/6 and 2009/10. She finds little impact on labor market outcomes of employed persons of either gender, but finds heterogeneous impacts on the self-employed by gender and occupation. Any increase in the homicide rate is correlated with a 20% decline in the likelihood that a woman self-employed in 2005 worked in the week prior to being surveyed in 2009. BenYishay and Pearlman study changes in hours worked, using fixed effects and instrumental variables designs. They find no significant effects in their preferred IV specifications.

There is limited empirical evidence of migration in response to Mexican violence. Overall, Velásquez finds no evidence that changes in violence made either men or women more likely to emigrate, but the author emphasizes effects among self-employed men and rural women. Basu and Pearlman study gross migration rates under an instrumental variables strategy and find no significant effects; they attribute this to low mobility in the Mexican population.

Finally, AGS analyze home appraisals in monthly data from 2008-2011 using a fixed effects design at the municipality-level and controlling for detailed housing characteristics. They find that only poor homes lose value. A one standard deviation increase in homicides leads to a 3% decline in the appraisal value of low-income housing.

While all of these studies advance our understanding of the economic effects of drug violence, they do not directly identify the channels of impact, and there is a risk that they may not satisfactorily address the major threat to validity in the Mexican context. The U.S. financial crisis and economic recession coincided with the dramatic increase in homicide rates after 2008. Border and other regions of Mexico that trade heavily with the U.S. can plausibly be assumed to have been more greatly affected by this recession than central and southern regions. But border and other regions that are well-situated for trade and trafficking have also borne the brunt of the increase in drug-related homicides, potentially confounding the effects of two very different causes. The instrumental variables designs rely exactly on the prediction that homicides will increase most in regions either closest to the border or most valuable as trafficking locations—but this is precisely where we would expect the U.S. recession to have had the greatest effects as well.[15] Fixed effects designs estimated during this period may be vulnerable to differential time trends in regions closer to the border and/or more reliant on U.S. trade during this period. In principle, the synthetic control designs may be better able to control for such threats, but matching at the

[15] The IV designs in RCM and Basu and Pearlman attempt to avoid this concern by exploiting temporal variation (fluctuations in Colombian cocaine seizures) interacted with spatial characteristics (distance to the U.S. or miles of federal toll roads). If, after controlling for time fixed effects, that interaction term is plausibly uncorrelated with the local effects of the U.S. recession, then it may predict plausibly exogenous variation in local homicide rates. But cocaine seizures follow a consistent upward trend after 2007, and will in turn predict a region-specific upward trend in homicides. If we believe that the effects of the U.S. recession were also stronger in border regions, then time fixed effects will not control for this simple, region-specific, spurious correlation—invalidating the IV assumptions.

municipality-level or state-level does not allow the same precision that matching at the level of individual firms in monthly data, with precisely estimated structural breaks, should provide.

3 Conceptual framework

In this section, I present a model of heterogeneous firms experiencing the impact of crime as a form of tax. While it is common in the literature to model crime and conflict in this way, I provide a unified framework that encompasses multiple forms that these taxes may take. Specifically, I consider an economy of heterogeneous firms, which may be operating in either price-taking or monopolistic sectors. The tax may fall on demand, on firm productivity, and/or upon one or more input factors, and may take various forms.

I present model predictions for selected cases below. All derivations of these comparative statics, and additional resuts, are provided in an online appendix.[16]

3.1 Firm problem

Assume that each firm's production takes the standard CES form $Q_{js}(\mathbf{X}_{js}) = A_j F_s(\mathbf{X}_{js}) = A_j \left[\sum_{i=1}^{I} \alpha_{si} X_{jsi}^{\frac{\sigma_s-1}{\sigma_s}}\right]^{\nu_s \frac{\sigma_s}{\sigma_s-1}}$, with $\sigma_s > 0$ denoting the elasticity of substitution across I inputs. Let j index firms, s index sectors, and i index inputs. Returns to scale are captured by the parameter $\nu_s > 0$, with $\nu_s = 1$ indicating constant returns to scale. Returns to scale in this specification take the form of increasing or decreasing marginal costs. When $\sigma_s \to 1$, production converges to the Cobb-Douglas form, $Q_{js}(\mathbf{X}_{js}) = A_j F_s(\mathbf{X}_{js}) = A_j \left[\prod_{i=1}^{I} X_{jsi}^{\alpha_{si}}\right]^{\nu_s}$, with $\sum_{i=1}^{I} \alpha_{si} = 1$. The A_j coefficient captures Hicks-neutral productivity. Let ω_{jsi} denote market prices for each factor of production, which the firm treats as exogenous. With the exception of the productivity term A_j, I assume that production function parameters—α_{si}, σ_s, ν_s—are common to all firms within an industry. However, input prices ω_{jsi} are allowed to be firm-specific. This is consistent with the substantial variation in input mixes seen across firms within the same industry.

Under price-taking behavior, firms take prices as given, i.e., $P_{js}(Q_{js}) = \bar{P}_s$. Alternatively, we may assume that firms face downward sloping demand curves. In particular, assume that demand is isoelastic with $Q_{js}(P_{js}) = \theta_{js} P_{js}^{-\epsilon_s}$ denoting the demand function, and $P_{js}(Q_{js}) = \theta_{js}^{1/\epsilon_s} Q_{js}^{-1/\epsilon_s}$ the inverse demand function, with $\epsilon_s > 1$. Let Y_{js} denote revenue, with $Y_{js}(\mathbf{X}_{js}) = P_{js}\left(Q_{js}(\mathbf{X}_{js})\right) Q_{js}(\mathbf{X}_{js})$. Then in either case, we can write the firm's maximization problem as

$$\max_{\{X_{jsi} \geq 0\}_i} P_{js}\left(Q_{js}(\mathbf{X}_{js})\right) Q_{js}(\mathbf{X}_{js}) - \boldsymbol{\omega}_{js} \cdot \mathbf{X}_{js} \tag{1}$$

[16] See https://are.berkeley.edu/sites/default/files/job-candidates/pdfs/JMPMontoyaModelAppendix.pdf.

To ease notation in the following, let

$$\Phi_{js} = \begin{cases} \left(\sum_{i=1}^{I} \alpha_{si}^{\sigma_s} \omega_{jsi}^{1-\sigma_s}\right)^{\frac{1}{\sigma_s - 1}} & , \ 0 < \sigma_s \neq 1 \\ \prod_{i=1}^{I} \left(\frac{\alpha_{si}}{\omega_{jsi}}\right)^{\alpha_{si}} & , \ \sigma_s = 1 \end{cases} \tag{2}$$

which can be seen as a firm-specific productivity term reflecting the benefit of access to cheaper inputs. It is the inverse of the firm-specific ideal cost index, in the sense that cost-minimizing total cost may be expressed as $C(Q) = Q^{\frac{1}{\nu}} A^{-\frac{1}{\nu}} \Phi^{-1}$.

Solutions The online appendix includes solutions under price-taking. Under monopolistic behavior and differentiated goods, let $\mu_s = \frac{\epsilon_s}{\epsilon_s - 1}$ denote a firm's markup over marginal cost, which is constrained to be the same for all firms in industry s. Let $\eta_s \equiv \frac{\mu_s}{\mu_s - \nu_s}$. In the monopolistic case, it is also true that $\eta_s = \epsilon_s/(\nu_s + \epsilon_s - \epsilon_s \nu_s)$. As will be shown below, η_s is the inverse of the share of profits in revenue. This characterization of η_s remains correct under price-taking if we let $\mu_s = 1$. Thus, positive profits requires $\mu_s > \nu_s$, which implies $\eta_s > 1$.

Solutions for revenue, input usage, output prices, and profits are given by

$$Y_{js}^* = \theta_j^{\eta_s} A_j^{\eta_s/\mu_s} \left(\frac{\nu_s}{\mu_s}\right)^{\eta_s - 1} \Phi_{js}^{\eta_s - 1} \tag{3}$$

$$X_{jsm}^* = \theta_j^{\eta_s} A_j^{\eta_s/\mu_s} \left(\frac{\nu_s}{\mu_s}\right)^{\eta_s} \Phi_{js}^{\eta_s - \sigma_s} \left(\frac{\alpha_{sm}}{\omega_{jsm}}\right)^{\sigma_s} \tag{4}$$

$$P_{js}^* = \theta_j^{(1-\nu_s)\eta_s} A_j^{-\eta_s/\epsilon_s} \left(\frac{\nu_s}{\mu_s}\right)^{-\nu_s \eta_s/\epsilon_s} \Phi_{js}^{-\nu_s \eta_s/\epsilon_s} \tag{5}$$

$$\Pi_{js}^* = (1 - \frac{\nu_s}{\mu_s}) Y_{js}^* = \eta_s^{-1} Y_{js}^* \tag{6}$$

and the solution for factor intensity of revenue, $\Omega_{jsm}^* = \omega_{jsm} X_{jsm}^* / Y_{js}^*$, is given by

$$\Omega_{jsm}^* = \frac{\nu_s}{\mu_s} \alpha_{sm}^{\sigma_s} \omega_{jsm}^{1-\sigma_s} \Phi_{js}^{1-\sigma_s} \tag{7}$$

Thus we see that η_s is equal to the inverse of the share of pure economic profit in revenue. This will be a key parameter in the comparative statics below. However crime is modeled, a robust prediction will be that firms with lower profitability (higher η_s) will be impacted more strongly by increased violence.

To provide intuition for the comparative statics below, we will need to interpret heterogeneity across firms with different levels of A_j and θ_j, the firm-level productivity and demand shift parameters. These parameters behave identically in determining firm size in equations (3) and (4), but in opposite ways in determining output price (assuming $\nu_s < 1$). Holding θ_j constant across all firms in industry s implies horizontal differentiation and a negative relationship between firm size and unit output prices. Allowing θ_j to vary across firms implies vertical (quality) differentiation and a positive correlation between firm size and unit output prices.

Empirical work tends to find a positive correlation between plant size and unit output prices (e.g. Kugler and Verhoogen 2012 in Colombian manufacturing; Faber 2014 in Mexican manufacturing). These findings are often interpreted within a framework of quality differentiation in production. Faber (2014) also identifies a positive correlation between household income and household purchase unit values for retail goods, leading to a model of vertical differentiation in production and consumption that links consumption differences across households to differences in plant technologies. Thus, for purposes of interpretation below, I will treat larger plant sizes, which are observable in my data, as synonymous with higher output prices, higher product quality and consumption by higher income consumers.

3.2 Comparative statics

3.2.1 Violence as a common treatment effect

Proportional productivity shocks Consider a productivity shock of the form $A'_j = A_j(1-\tau_A)$. Under both price-taking and monopolistic behavior, letting $\mu_s = 1$ in the case of price-taking, we can replace A_j with A'_j and differentiate with respect to τ_A to find that

$$\frac{\partial Y^*_{js}/\partial \tau_A}{Y^*_{js}} = \frac{\partial X^*_{jsm}/\partial \tau_A}{X^*_{jsm}} = \frac{\partial \Pi^*_{js}/\partial \tau_A}{\Pi^*_{js}} = -\frac{\eta_s/\mu_s}{1-\tau_A} < 0 \tag{8}$$

with $\eta_s = \frac{\mu_s}{\mu_s - \nu_s}$. In the case of monopolistic behavior, we have a further prediction about price:

$$\frac{\partial P^*_{js}/\partial \tau_A}{P^*_{js}} = \frac{\eta_s/\epsilon_s}{1-\tau_A} > 0 \tag{9}$$

We can infer the percentage decline in profits from the percentage decline in revenue. We also see that percentage impacts on revenue and input usage should be equal. Under monopolistic competition, the magnitude of the percentage impact on price will be smaller than the impact on revenue so long as $\epsilon_s > \mu_s \iff \epsilon_s > 2 \iff \mu_s < 2$. That is, as long as prices are assumed less than twice marginal cost, we should expect the magnitude of price effects to be less than the magnitude on real variables. To provide a reference point, for $\nu_s = 0.8$ and $\epsilon_s = 11$ (implying 10% markups), a 1pp increase in τ_p from 0 would result in a 3.3% decline in revenue and input usage and a 0.3% increase in price.

Intuitively, negative productivity shocks may be seen as an increase in marginal cost. But costs increase by the same percentage for all firms, in a way that is proportional to establishment size. For example, if both large and small establishments choose to close an hour early one day out of the week due to roadblocks or concerns about traveling after dark, we would observe equal proportional effects across establishments of different sizes.

Proportional demand shocks Under price-taking behavior, consider demand shocks of the form $P'_{js} = (1-\tau_p)\bar{P}_s$, or under monopolistic behavior, shocks of the form $P'_{js}(Q_{js}) = (1-\tau_p)P_{js}(Q_{js})$. Under both monopolistic and price-taking behavior (letting $\mu_s = 1$ under price-taking), we have

$$\frac{\partial Y^*_{js}/\partial\tau_p}{Y^*_{js}} = \frac{\partial X^*_{jsm}/\partial\tau_p}{X^*_{jsm}} = \frac{\partial \Pi^*_{js}/\partial\tau_p}{\Pi^*_{js}} = -\frac{\eta_s}{1-\tau_p} < 0 \tag{10}$$

while under monopolistic behavior, we have the further prediction that

$$\frac{\partial P^*_{js}/\partial\tau_p}{P^*_{js}} = -\frac{\eta_s}{1-\tau_p}(1-\nu_s) \tag{11}$$

We can infer the percentage decline in profits from the percentage decline in revenue. Again we see that percentage impacts on revenue and input usages should be equal, although now the predicted sign of price change is ambiguous, depending on returns to scale. To provide a reference point, for $\nu_s = 0.8$ and $\epsilon_s = 11$ (implying 10% markups), a 1pp increase in τ_p from 0 would result in a 3.7% decline in revenue and input usage and a 0.7% decline in price.

Intuitively, demand falls in a way that leads to equal percentage declines across high-priced and low-priced items within the same category of goods. Treating unit prices, quality, and firm size as synonymous, the prediction is that small and large businesses will be affected equally.

Predictions Let c index cities, s index industries or sectors, j index firms, and t index time periods. Let $T_{ct} = 1$ in cities during a presumed treatment event, and let $T_{ct} = 0$ in all periods in cities that never experience the treatment event, before the treatment event occurs in a given city, or after the treatment event has ended in a given city. Also let Z_{jsct} be a vector of predetermined covariates, let $\bar{Y}_j$ be a measure of firm size prior to the outbreak of violence, and let $\bar{\Omega}_{jsm}$ be a measure of the factor intensity of revenue for input m prior to the outbreak of violence. Consider regressions of the form

$$\begin{aligned}
\log Y_{jsct} &= a_1 T_{ct} + a_2 \log(\bar{Y}_j) T_{ct} + a_3 \bar{\Omega}_{jsm} + f_a(Z_{jsct}) + u_{jcst} & (12)\\
\log X^m_{jsct} &= b^m_1 T_{ct} + b^m_2 \log(\bar{Y}_j) T_{ct} + b^m_3 \bar{\Omega}_{jsm} + f^m_b(Z_{jsct}) + v^m_{jcst} & (13)\\
\log \Omega^m_{jsct} &= d^m_1 T_{ct} + d^m_2 \log(\bar{Y}_j) T_{ct} + d^m_3 \bar{\Omega}_{jsm} + f^m_d(Z_{jsct}) + w^m_{jcst} & (14)
\end{aligned}$$

and assume that $\mathbb{E}\left[u_{jcst} \times T_{ct} \mid f_a(Z_{jsct})\right] = \mathbb{E}\left[v^m_{jcst} \times T_{ct} \mid f_b(Z_{jsct})\right] = \mathbb{E}\left[w_{jcst} \times T_{ct} \mid f_d(Z_{jsct})\right] = 0$.

$$\textbf{Prediction A1}: \quad a_1 = b^m_1 < 0 \quad \forall m \tag{15}$$

$$\textbf{Prediction A2}: \quad a_2 = b^m_2 = 0 \quad \forall m \tag{16}$$

$$\textbf{Prediction A3}: \quad a_3 = b^m_3 = 0 \quad \forall m \tag{17}$$

$$\textbf{Prediction A4}: \quad d^m_1 = d^m_2 = d^m_3 = 0 \quad \forall m \tag{18}$$

3.2.2 Violence leading to greater impacts among smaller firms

Additive productivity shocks Consider shocks of the form $A'_j = A_j - t_A$. Under both price-taking and monopolistic behavior, letting $\mu_s = 1$ in the case of price-taking, we can replace A_j with A'_j and differentiate with respect to t_A to find that

$$\frac{\partial Y^*_{js}/\partial t_A}{Y^*_{js}} = \frac{\partial X^*_{jsm}/\partial t_A}{X^*_{jsm}} = \frac{\partial \Pi^*_{js}/\partial t_A}{\Pi^*_{js}} = -\frac{\eta_s/\mu_s}{A_j - t_A} < 0 \tag{19}$$

with $\eta_s = \frac{\mu_s}{\mu_s - \nu_s}$. In the case of monopolistic behavior, we have a further prediction about price:

$$\frac{\partial P^*_{js}/\partial t_A}{P^*_{js}} = \frac{\eta_s/\epsilon_s}{A_j - t_A} > 0 \tag{20}$$

Predictions in this case are similar to those under proportional productivity shocks, with one key exception. Impacts will be greater for firms with lower levels of productivity. While productivity is unobserved, firm size (by revenue or employment) is increasing in productivity and is observed. We may also think of productivity levels and firm size by revenue as being correlated with variation in product quality within a given sector. Thus, it may be inferred that small firms selling low quality goods are impacted more strongly than large firms selling high quality goods.

Intuitively, small firms behave as if they experience a larger percentage increase in marginal cost than do large firms. In anecdotal evidence, more prominent individuals, such as doctors who are more likely to be targeted for kidnapping and ransom, take steps to reduce their risk when violence increases. Examples include varying routes to work and driving lower quality vehicles. If the owners of small businesses feel as if they are more conspicuous targets than do the owners of large establishments, they will be less likely to visit the establishment or more likely to avoid keeping the establishment open after dark, generating the heterogeneous response by firm size.

Additive demand shocks In the monopolistic case, consider a demand shock of the form $\theta'_j = \theta_j - t_\theta$. Given the assumption of isoelastic demand, it is straightforward to simply replace every instance of θ_j with $\theta_j - t_\theta$, and differentiate with respect to t_θ. Thus we have that

$$\frac{\partial Y^*_{js}/\partial \tau_p}{Y^*_{js}} = \frac{\partial X^*_{jsm}/\partial \tau_p}{X^*_{jsm}} = \frac{\partial \Pi^*_{js}/\partial \tau_p}{\Pi^*_{js}} = -\frac{\eta_s}{\theta_j - t_\theta} < 0 \tag{21}$$

$$\frac{\partial P^*_{js}/\partial \tau_p}{P^*_{js}} = -\frac{\eta_s}{\theta_j - t_\theta}(1 - \nu_s) \tag{22}$$

Predictions in this case are similar to those under proportional demand shocks, but now impacts will be proportionally greater for firms with lower levels of θ_j. While θ_j is unobserved, firm size (by revenue or employment) is increasing in θ_j and is observed, which we may also think of as being correlated with higher product quality. Once again, small firms selling low quality goods are impacted more strongly than large firms selling high quality goods.

Intuitively, one demand-side explanation may be that if consumers of low-quality goods are lower income and more vulnerable when violence increases, compared to higher income consumers who purchase high-quality goods from shopping malls, this would imply a greater proportional decline in smaller establishments producing lower-quality goods.

Predictions Consider regressions of the form

$$\begin{aligned}
\log Y_{jsct} &= a_1 T_{ct} + a_2 \log(\bar{Y}_j) T_{ct} + a_3 \bar{\Omega}_{jsm} + f_a(Z_{jsct}) + u_{jcst} && (23)\\
\log X^m_{jsct} &= b^m_1 T_{ct} + b^m_2 \log(\bar{Y}_j) T_{ct} + b^m_3 \bar{\Omega}_{jsm} + f^m_b(Z_{jsct}) + v^m_{jcst} && (24)\\
\log \Omega^m_{jsct} &= d^m_1 T_{ct} + d^m_2 \log(\bar{Y}_j) T_{ct} + d^m_3 \bar{\Omega}_{jsm} + f^m_d(Z_{jsct}) + w^m_{jcst} && (25)
\end{aligned}$$

and assume that $\mathbb{E}\left[u_{jcst} \times T_{ct} \mid f_a(Z_{jsct})\right] = \mathbb{E}\left[v^m_{jcst} \times T_{ct} \mid f_b(Z_{jsct})\right] = \mathbb{E}\left[w_{jcst} \times T_{ct} \mid f_d(Z_{jsct})\right] = 0$.

$$\textbf{Prediction B1}: \quad a_1 = b^m_1 < 0 \quad \forall m \tag{26}$$

$$\textbf{Prediction B2}: \quad a_2 = b^m_2 > 0 \quad \forall m \tag{27}$$

$$\textbf{Prediction B3}: \quad a_3 = b^m_3 = 0 \quad \forall m \tag{28}$$

$$\textbf{Prediction B4}: \quad d^m_1 = d^m_2 = d^m_3 = 0 \quad \forall m \tag{29}$$

3.2.3 Violence as a labor supply shock, supply chain disruption, or other factor market distortion

In this section, I consider input price shocks of the form $\omega'_{jsi} = (1+\tau_i)\omega_{jsi}$. In the online appendix, I also consider input price shocks of the form $\omega'_{jsi} = \omega_{jsi} + t_i$.

Previous literature emphasizes impacts through worker absenteeism and out-migration, which may be modeled as an increase in workers' reservation wages. Shocks to multiple production factors are easily incorporated. Skilled and unskilled labor may be disaggregated and tested separately. The key data requirements are simply firm-level revenue and factor expenditures.

Proportional factor market price shocks Consider input price shocks of the form $\omega'_{jsi} = (1+\tau_i)\omega_{jsi}$. Under both price-taking and monopolistic behavior, letting $\mu_s = 1$ for price-taking firms, we have

$$\left.\frac{\partial \Omega^*_{jsn}/\partial \tau_m}{\Omega^*_{jsn}}\right|_{\tau_m=0} = -(1-\sigma_s)\frac{\mu_s}{\nu_s}\Omega^*_{jsm} + (1-\sigma_s)\mathbb{I}(n=m) \tag{30}$$

In the Cobb-Douglas case ($\sigma_s = 1$), even under factor price shocks, factor shares of revenue remain unchanged. In general, the sign of the derivative in equation (30) is ambiguous without further assumptions as to which input was affected, the value of σ_s, and other parameters. However, observe that factor shares will not be affected by demand or productivity shocks. Thus it remains the case that a change in any factor share is inconsistent with the demand or productivity shocks studied above. Impacts would be heterogeneous across firms with different values of Ω^*_{jsm}.

Next, under both price-taking and monopolistic behavior, letting $\mu_s = 1$ for price-taking firms, we have that

$$\frac{\partial Y^*_{js}/\partial \tau_m}{Y^*_{js}} = \frac{\partial \Pi^*_{js}/\partial \tau_m}{\Pi^*_{js}} = -(\eta_s - 1)\frac{\mu_s}{\nu_s}\Omega^*_{jsm} = -\eta_s \Omega^*_{jsm} < 0 \tag{31}$$

$$\frac{\partial X^*_{jsn}/\partial \tau_m}{X^*_{jsn}} = -(\eta_s - \sigma_s)\frac{\mu_s}{\nu_s}\Omega^*_{jsm} - \frac{\sigma_s}{1+\tau_m}\mathbb{I}(n = m) \tag{32}$$

while under monopolistic behavior, we also have that

$$\frac{\partial P^*_{js}/\partial \tau_m}{P^*_{js}} = \eta_s \frac{\mu_s}{\epsilon_s}\frac{\Omega^*_{jsm}}{1+\tau_m} > 0 \tag{33}$$

Observe that for $\sigma_s < 1$, indicating inputs that are less substitutable than Cobb-Douglas, the derivative on input usage is negative, and usage of all inputs should decline by a greater percentage then revenue. Also observe that it remains true that we can infer the average percentage decline in profits from the average percentage decline in revenue.

To provide a reference point in the monopolistic case, when $\epsilon_s = 11$, $\nu_s = 0.8$, $\sigma_s = 0.5$, and $\Omega^*_{jsm} = 0.3$, revenue would decline by 1.1% when τ_m moves from 0 to .01, while the corresponding factor demand would fall by 1.3%. Other things equal, impacts would be heterogeneous across firms with different values of Ω^*_{jsm}. In a regression, the relevant slope parameter would be $-(\eta_s - 1)\frac{\mu_s}{\nu_s}\Delta\tau_m$, or -.04, but the slope parameter will be larger depending on $\Delta\tau_m$.

Assuming $\sigma_s < 1$, equations (31) and (32) imply that if there is a shock to factor prices, then for at least one factor, the magnitude of the impact of should be greater than the observed impact on revenues. Because equations (8) and (10), and equations (19) and (21), all establish that demand and productivity shocks should have equal impacts on both revenue and input usage, we are able to test for the presence of factor market shocks regardless of the presence or absence of pure demand or productivity shocks. Equation (30) derives from essentially the same intuition, observing that factor intensities of revenue should change in the presence of a factor market shock, but not in the presence of pure demand or productivity shocks.

Predictions Assuming $\sigma_s < 1$, one straightforward prediction is that average percentage declines in input usage, for all inputs, will be weakly greater than percentage declines in revenue.

But further predictions are possible. Consider regressions of the form

$$\begin{aligned}
\log Y_{jsct} &= a_1 T_{ct} + a_2 \bar{\Omega}_{jsm} + f_a(Z_{jsct}) + u_{jcst} && (34)\\
\log X^m_{jsct} &= b^m_1 T_{ct} + b^m_2 \bar{\Omega}_{jsm} + f^m_b(Z_{jsct}) + v^m_{jcst} && (35)\\
\log \Omega^m_{jsct} &= d^m_1 T_{ct} + d^m_2 \bar{\Omega}_{jsm} + f^m_d(Z_{jsct}) + w^m_{jcst} && (36)
\end{aligned}$$

and assume that $\mathbb{E}\left[u_{jcst} \times T_{ct} \mid f_a(Z_{jsct})\right] = \mathbb{E}\left[v^m_{jcst} \times T_{ct} \mid f_b(Z_{jsct})\right] = \mathbb{E}\left[w_{jcst} \times T_{ct} \mid f_d(Z_{jsct})\right] = 0.$

$$\begin{aligned}
\textbf{Prediction C} \quad 0 &= a_1 && (37)\\
-\mathbb{E}_j\left[\eta_s \times \Delta\tau_m\right] &= a_2 < 0 && (38)\\
-\mathbb{E}_j\left[(\eta_s - \sigma_s)\frac{\mu_s}{\nu_s} \times \Delta\tau_m\right] &= b^m_2 < 0 && (39)\\
-\mathbb{E}_j\left[\sigma_s \times \Delta\tau_m\right] &= b^m_1 < 0 && (40)\\
-\mathbb{E}_j\left[(1 - \sigma_s)\frac{\mu_s}{\nu_s} \times \Delta\tau_m\right] &= d^m_2 < 0 && (41)\\
\mathbb{E}_j\left[(1 - \sigma_s) \times \Delta\tau_m\right] &= d^m_1 > 0 && (42)
\end{aligned}$$

In five equations, the four unknowns are η_s, (μ_s/ν_s), σ_s, and $\Delta\tau_m$. We can solve for the average value of $\delta = (\mu_s/\nu_s)$ as $\hat{\delta} = -d^m_2/d^m_1$. Let $\hat{\gamma} \equiv -b^m_1/d^m_1 = \hat{\sigma}_s/(1 - \hat{\sigma}_s)$. Then we can solve for the average value of $\hat{\sigma}_s$ as $\hat{\sigma}_s = \hat{\gamma}/(1+\hat{\gamma})$. We can now solve for $\Delta\hat{\tau}_m$ as $\Delta\hat{\tau}_m = d^m_1/(1-\hat{\sigma}_s)$ and solve for $\hat{\eta}_s$ as $\hat{\eta}_s = a_2/\Delta\tau_m$. Alternatively, structural estimation should deliver all of the above estimates more efficiently.

4 Data

4.1 Establishment-level data

Business victimization survey (ENVE) Repeated, cross-sectional business victimization surveys of about 30,000 establishments were conducted by INEGI[17] in 2012 and 2014. These surveys provide detailed information on business victimization rates, characteristics of the types of crimes experienced, reporting and under-reporting of crimes to official authorities, perceptions of trends, and direct economic losses as a result of insecurity. Moreover, they allow me to relate observable measures of insecurity—homicide rates—to direct victimization, perceived insecurity and self-reported actions and business impacts.

The ENVE survey is probabilistic, stratified by business size, and designed to be representative at the national and state levels. In order to present city-level averages, such as those in Table 2, I generate custom weights. Observe that some cities (e.g., Mexico City) encompass portions

[17] Instituto Nacional de Estadística y Geografía, or the National Institute of Statistics and Geography.

of multiple states. Because sampling probabilities vary across city-state-stratum combinations, I generate inverse probability weights as the ratio of census firms to surveyed firms for each combination. City-level averages are then constructed as weighted averages.

Table 2 provides initial summary statistics. Results are reported for crime rates per 100,000 persons. Questions regarding neighborhood conditions survey busineses as to whether they have observed a range of conditions in their neighborhood. Questions regarding business impacts ask whether firms have altered their behavior in response to insecurity: i.e., by reducing investment, collaborating less with other businesses, stopped handling cash, cancelled distribution routes, or reduced their business hours. The two largest business responses are seen to be reducing business hours (20%), and reducing investment plans (17%). Questions regarding victimization ask whether businesses have experienced specification types of crime, with the two most frequent responses being theft of vehicle parts (16%) and petty theft (14%). Questions regarding local institutions ask whether business are confident in various police forces, the courts, and military, and also ask them to grade their performance. For each set of questions, I also construct a summary index (Anderson 2008).

Manufacturing and construction establishments The key economic data used in the current analysis are monthly, establishment-level production data for approximately 8,000 Mexican manufacturing (EMIM) and construction (ENEC) establishments from 2007-2013, across 77 cities. The available survey data allow me to construct establishment-level measures along key dimensions of heterogeneity identified by the model, including labor intensity of revenue, average revenue product of labor, and average establishment wage rates.

The survey contains outcomes at both the establishment-level, and in the case of manufacturing firms, at the product level. Over 90% of the sample consists of manufacturing firms. Establishment-level variables include employment, production hours, and wagebill by type of worker. In the case of manufacturing firms, physical quantities produced and sold, as well as their values, are reported at the product-level. Revenues from *maquiladora* production—i.e., manufacturing conducted on behalf of third parties using their raw materials, frequently for export—are also available for manufacturing firms. For construction establishments, revenues are reported only at the establishment-level. Additional variables are available in annual datasets available from 2009, including detailed costs, electricity consumption, inventories, and fixed assets.

The survey design is primarily deterministic. In most cases, the sampling proceeds by first ranking establishments within each 6-digit industry nationally by revenue. Establishments are then included in order until some threshold level of national revenue—from 60 to 85%, depending on the industry—is captured by the survey. Thus, the survey can be thought of as a census of the largest establishments by revenue in each city, or as representative of those firms that generate the bulk of aggregate sales. On the other hand, even within manufacturing, a sizable portion of aggregate local employment is by smaller firms than those captured by the survey. The average establishment in the survey employed about 300 workers in January of 2007, while the median establishment employed about 200 workers. An establishment at the 10th percentile of the survey employed 21 employees.

4.2 Drug violence

My primary measure of insecurity is based on the annualized monthly rate of intentional homicides per 100,000 persons at the urban area-level. Intentional homicides are available at the municipality-level from 1990 to 2012 from INEGI. Monthly population estimates at the municipality-level are interpolated linearly based on annual population estimates at the municipality-level from the Mexican Ministry of Health (SALUD). Homicides and population estimates are both aggregated to the urban area-level.

Additional monthly homicide statistics at the municipality level are available from the Office of the Presidency for the period from December 2006 to December 2010. Notably, the statistics from the Presidency attempt to distinguish between homicides related to criminal rivalry; however, the short time span of these data makes it difficult to implement the structural breaks analysis relied on here. Additional monthly crime statistics from 1997-2013 are available at the municipality level from INEGI.

4.3 Other economic data

The current analysis also relies on a variety of additional economic datasets. The most comprehensive establishment-level microdata is available from the three most recent economic censuses, for calendar years 1998, 2003, and 2008. Demographic data are also available from INEGI, primarily based on population census data for calendar years 2000, 2005, and 2010.

Monthly microdata on labor market outcomes from 2005 to 2013 in 73 urban areas are obtained from the National Occupation and Employment Survey (ENOE), and include information on employment and wages, as well as the gender of the respondent, whether employment is formal or informal, the economic sector of employment, and the size of the employing firm, in addition to other economic outcomes and respondent characteristics.

5 Empirical strategy

My primary empirical strategy relies on identifying a large, discontinuous break in the time series of a city's homicide rate, and studying changes in economic activity on either side of that break. The identifying assumption is that while the set of cities in which violence erupts will be a selected sample, the precise timing of such a large increase in violence is unlikely to be the result of smoothly changing economic conditions prior to the outbreak. That is, the timing of an eruption in violence is the result of essentially random success in arresting drug kingpins—or in some other military development, or in strategic changes in the drug landscape—that are in principle unrelated to fluctuations in local economic conditions.

Some of these causes are known, while some are not. But examples of events that we know help to motivate the empirical strategy:

- **Tijuana, BC**. Counter-narcotics deployments to Tijuana under President Calderón began in January 2007. In October 2008, the leader of the local DTO was captured. This led to an internal battle for control between the leader's remaining family and a rival lieutenant, as well as a large spike in violence in that same month. The rival lieutenant was captured in January 2010; violence then declined throughout 2010.

- **Chihuahua and Sinaloa**. A breakdown between two factions of the Sinaloa DTO led to violence between those two groups throughout northern Mexico. With variations, narratives suggest this was caused by an arrest in the leadership of one of those factions in January 2008, which was seen as a betrayal by the other faction. This breakdown also led one faction of the Sinaloa DTO to launch a war against the Juárez DTO for control of the border (Astorga and Shirk 2010; Hernández 2010). Violence thus erupted in multiple cities of northern Mexico throughout early 2008, even as military deployments began to cities including Ciudad Juárez. Structural breaks are identified in Chihuahua, Sonora, and Sinaloa between March and June 2008 in the territory controlled by these DTOs.

While these cases suggest that reverse causality is unlikely to explain any observed correlations, it does not rule out spurious correlation in small samples. I consider two approaches to deal with this threat.

5.1 Fixed effects analyses

Two advantages of the victimization survey are that firms are explicitly asked to describe their responses to insecurity, and that it was conducted after the worst of the global financial crisis. To the extent that firms are able to distinguish between their direct reactions to insecurity versus to general economic conditions, such evidence should in general be less contaminated by spurious correlation.

Nevertheless, in fixed effects regressions employing the business victimization data, I include city- and year-fixed effects, as well as industry-specific and characteristic-specific flexible time trends, to provide additional robustness. In fixed effects designs employing the industrial production data, I include flexible national time trends and firm characteristic time trends, as well as city-specific linear and cubic time trends to test for additional robustness.

The explanatory variable of interest in the fixed effects regressions is either the natural logarithm of annualized monthly homicide rates, or the homicide rate parameterized as a series of bins: from 0 to 10; 11 to 20; 20 to 35; 35 to 47; 47 to 63; 61 to 116; 166 to 188; or greater than 188. The bins were constructed such that if the sample were restricted to the 12 cities subject to large structural breaks in violence, the given cutoffs would divide the available firm-months into eight evenly-sized bins.

5.2 Identifying structural breaks in city-level homicide rates

Breaks in homicide rates are identified based on an econometric literature in testing for structural breaks in time series (*e.g.*, Bai and Perron, 2003; Zeileis *et al.*, 2003). Using homicide data aggregated to the city-level, for each city I first test the null hypothesis that there was no structural break in homicide rates. That is, for each urban area, monthly homicide rates during the 108 months spanning January 2005 to December 2013 are estimated using a constant regression model, $h_t = \alpha + \epsilon_t$. For each month from January 2006 to December 2012, I estimate the relaxed model, $h_t = \alpha + \beta I(t \geq \tau) + \varepsilon_t$, and collect the resulting F-statistic for the relaxed model against the restricted model. The observed distribution of these F-statistics can then be tested against the distribution derived under the null hypothesis of no structural break.

Conditional on rejecting the null, the month with the largest F-statistic is identified as the month of the structural break. Next, I review the identified structural breaks, focusing on 12 cities that experienced the largest shocks to their homicide rates—i.e., increases in the average annualized monthly homicide rate exceeding 30, and that exceed the pre-break average level by 3-4 times the pre-break standard deviation. The date and magnitude of the estimated structural breaks is presented in Figure 3. The largest identified structural breaks, and the criteria under which I have selected the cities that I focus on below, is provided in Table 5.

While these explosions in violence are plausibly not caused by local economic changes, they may nevertheless be correlated with them. In order to control for this possibility, I go on to use the large sample of firms in all other cities to implement a form of matching.

5.3 Estimation using individual-level synthetic controls

The approach used here draws on Abadie, Diamond, and Hainmueller [ADH] (2010). I follow ADH in relying on a factor model as motivation for the synthetic controls procedure, and largely in the estimation procedure itself. However, while ADH analyze an empirical setting in which a single aggregate unit is treated, my setting involves individual-level data with multiple treatment events at the group-level. And while ADH rely on permutation tests for inference, because I have multiple treated groups I am able to exploit a clustered wild bootstrap percentile-t procedure, imposing the null hypothesis of no treatment effect (Cameron, Gelbach, Miller 2008; Webb

2014). I adapt the wild bootstrap procedure to account for the dependency structure implied by synthetic controls.

5.3.1 Synthetic controls

ADH begin by describing potential outcomes based on a factor model. Let Y_{jct}^N be the outcome that would be observed for firm j in city c and period t in the absence of the intervention I. In my setting, the "intervention" of interest is that of a large structural break in homicide rates. For ease of exposition, consider $c \in \{0, 1\}$, where $c = 0$ indicates all cities that did not experience the intervention, and $c = 1$ indicates the city that did. Let Δ_{ct} denote the impact of the intervention.

Then we can write the observed outcomes for firms in cities that do experience the intervention as

$$Y_{j1t} = Y_{j1t}^N + \Delta_{1t}$$

Because Y_{j1t} is observed, then given estimates of Y_{j1t}^N for all firms j in city 1, we can estimate Δ_{1t} as $\bar{Y}_{1t} - \bar{Y}_{1t}^N$. ADH assume that Y_{jct}^N may be described by a factor model. That is, in every time period, we can think of all firms as responding to a set of common factors or shocks, but loading on these factors in different ways. Specifically,

$$Y_{jct}^N = \delta_t + \theta_t \mathbf{Z}_j + \lambda_t \boldsymbol{\mu}_j + \varepsilon_{jct}$$

where δ_t is an unknown common factor with constant factor loadings across units, $\mathbf{Z}_j$ is an $(r \times 1)$ vector of observed covariates, θ_t is a $(1 \times r)$ vector of unknown parameters, λ_t is a $(1 \times F)$ vector of unobserved common factors, $\boldsymbol{\mu}_j$ is an $(F \times 1)$ vector of unknown factor loadings, and the error terms ε_{jct} are unobserved transitory shocks with zero mean. Firms differ in their reactions along observable characteristics $\mathbf{Z}_j$. The composite residual is given by $\lambda_t \boldsymbol{\mu}_j + \varepsilon_{jct}$, and quite generally describes firms reacting differently to unobserved factors due to unobserved, firm-level characteristics, plus a mean zero error term.

The ideal comparator for unit j would have identical values of $\mathbf{Z}_j$ and $\boldsymbol{\mu}_j$. But because this is infeasible (in particular, since characteristics $\boldsymbol{\mu}_j$ are unobserved), ADH study "synthetic controls" constructed as weighted averages of units in untreated regions. That is, let $\tilde{Y}_{j1t} = \mathbf{Y}_{0t}^T \mathbf{w}_j$ and $\tilde{\mathbf{Z}}_j = \mathbf{Z}_0^T \mathbf{w}_j$, where $\mathbf{Y}_{0t}$ is an $N_0 \times 1$ vector of outcomes for all units in untreated regions in period t, $\mathbf{Z}_0$ is an $N_0 \times r$ vector of time-invariant characteristics for all units in untreated regions, and $\mathbf{w}_j$ is an $N_0 \times 1$ vector of weights. All weights in $\mathbf{w}_j$ are between 0 and 1, and sum to 1. ADH show that the only way that a synthetic control can fit both $\mathbf{Z}_j$ and a long vector of preintervention outcomes is if it fits both $\mathbf{Z}_j$ and $\boldsymbol{\mu}_j$. Thus, compared to a standard matching estimator, the synthetic control method exploits pre-treatment outcomes to implicitly match along unobservables as well as observables. Compared to a differences-in-differences model, it allows the unobserved confounders $\boldsymbol{\mu}_j$ to have time-varying rather than fixed effects.

5.3.2 Implementation

In my setting, I observe 12 cities experiencing large structural breaks in their homicide rates, at different points in time. I assume that the only difference between these events is the date on which they occur. Under the assumption that each structural break should have similar impacts on firms, by pooling post-break outcomes across all of these breaks, we should be able to form more precise estimates of the outcomes for the "typical" large structural break in homicide rates.

City-specific match periods ADH identify the existence of a large number of preintervention periods as one key to identifying plausible matches for $\boldsymbol{\mu}_j$. For the 12 cities that I identify as experiencing the largest structural breaks, the dates of those breaks range from April 2008 (Ciudad Juarez) to November 2010 (Acapulco). The firm-level data are available from January 2007. Thus, for firms in the earliest cities, I am able to exploit at most 15 months of data during the match period, while for firms in the later cities, I able to exploit as many as 46 months. Given the relatively short intervals for matching, for firms in each city I match using all available pre-break data.

Matching The outcomes and dimensions of heterogeneity I study follow from the model in Section 3. Key outcome variables include log revenue, log employment, log hours worked, and log labor share of revenue. Key dimensions of heterogeneity include reciprocal wage per employee, reciprocal revenue per employee, and the revenue share of labor in level form.

For each firm in each city experiencing a large structural break, I begin by identifying its 5 and 20 nearest neighbors along pre-break values of relevant outcome measures and dimensions of heterogeneity. The potential donor pool consists of all firms in cities that did not experience a large structural break. I match on the entire sequence of all variables during all pre-break months, using a normalized Euclidean weighting matrix. Synthetic controls are then constructed using only each firm's nearest neighbors. This approach has three benefits: it potentially improves approximation quality after the matching period; it reduces attrition at the match-level (which will be discussed below); and it reduces the computational burden of constructing the synthetic controls. With respect to the first, ADH note that if outcomes are highly nonlinear in characteristics, and the range of those characteristics is large, interpolation biases may be reduced by restricting the comparison group to units that are similar to the exposed units before estimating the synthetic control weights.[18]

For each firm in the large structural break cities, and for each outcome variable of interest, I construct a synthetic control by identifying the weights of the nearest neighbors that minimize

[18]Notice that the similarity of the comparison group will typically be maximized by eliminating all but a few, highly similar units, while the fit of the synthetic control will be maximized by searching over the largest possible pool.

the mean-squared error of the target outcome variable during all pre-break months. Weights are restricted to the interval between 0 and 1, and required to sum to 1. I also construct weights while estimating a match-specific constant during the pre-period; in this case, the average distance between the "treated" firm and its synthetic control is guaranteed equal to zero during the pre-treatment period, and the synthetic control weights will be relatively better at matching time trends. Robustness checks include estimation with either 5 or 20 nearest neighbors, and with and without the match-specific constant.

Overlap The result of the above procedures is a synthetic control for each firm in each city that experienced a structural break. Naturally, the fit of the resulting synthetic control will vary across firms. The literature on matching emphasizes that valid estimation depends on sufficient overlap between treated and comparison units. Thus, all synthetic controls for which the mean squared error is above the 90th percentile are trimmed from the analysis.

Attrition A distinguishing feature of individual-level compared to aggregate data is the possibility of attrition. In a context with synthetic controls, if any of the firms used to construct the synthetic control exits the sample, applying the computed weights to the remaining firms may result in a drastically different counterfactual.

Necessarily, I drop the entire match from estimation in the first month in which either the treated firm or any firm with positive weight in its synthetic control exits the sample. The concern with this approach is that the sample lost to attrition may be a non-random sample of the study population, such that estimated effects will be biased relative to the average effect. Potential responses to this include some version of the Heckman selection correction, or to compare results in the unbalanced sample against those in a balanced sample. In this paper, my primary test for selection is whether in the months prior to attrition, there is a differential effect in the attrition sample.

Observe that the larger the number of firms that receive positive weight in the synthetic control, the greater is the probability of attrition. By restricting the synthetic control to at most 5 or 20 firms, I am able to reduce the rate of attrition at the match-level. Figure 4 presents the rates of attrition. In the left-hand panel, we see that within two years of January 2008, less than 5% of the sample is lost to attrition, while firms in large break cities were subject to greater attrition. In the right-hand panel, we see that after 24 months, rates of attrition among the synthetic controls range from 15% to 22%, depending on the number of nearest neighbors used.

Comparison of treated firms and synthetic controls Table 6 presents the comparison of means between firms in cities with large structural breaks, and their synthetic controls.

5.3.3 Estimation

There are at least two equivalent ways of recovering identical point estimates. I discuss inference and standard errors in the following sub-section.

Differencing The simplest approach is to difference the observed outcomes of each firm against those of its synthetic control. That is, for each firm in each period, compute $\breve{Y}_{jct} = Y_{jct} - \tilde{Y}_{jct}$. The average effect of the event is then $\bar{\Delta} = \frac{1}{T}\sum_{t>T_0}\left(\frac{1}{J_t}\sum_j \breve{Y}_{jct}\right)$, where J_t indicates the number of firms in large structural break cities active in period t. This is convenient for graphical analysis. Equivalently, we could run the regression $\breve{Y}_{jct} = \alpha + \beta T_{ct} + u_{jct}$, with T_{ct} an indicator equal to 1 after the structural break occurs. Then $\hat{\beta}$ provides our estimate of $\bar{\Delta}$. In order to control for chance differences in covariates between the treated firm and its synthetic control, construct $\breve{Z}_{jc} = Z_{jc} - \tilde{Z}_{jc}$ and run the regression $\breve{Y}_{jct} = \alpha + \beta T_{ct} + \gamma \breve{Z}_{jc} + v_{jct}$. Treatment effect heterogeneity is recovered by estimating $\breve{Y}_{jct} = \alpha + \beta T_{ct} + \gamma \breve{Z}_{jc} + \delta \breve{Z}_{jc} T_{ct} + \varepsilon_{jct}$, with $\hat{\delta}$ now providing our estimate of the linear relationship between greater values of Z and outcome Y under treatment.

In practice, I also include two endpoint coefficients that adjust for the fact that the structural break cities are unbalanced in event-time. Thus, in both groups, a coefficient $\underline{\beta}$ is estimated for all months more than a year before the event, and a coefficient $\bar{\beta}$ is estimated for all months more than 24 months after the event. Now the coefficient β can be seen as a difference in average outcomes during the 24 months after the structural break versus average outcomes during the 12 months prior to the event. When estimating heterogeneous effects, similar endpoint coefficients shoud be estimated by interacting the $\breve{Z}_{jc}$ characteristics with an indicator variable for months after 24 months, and earlier than 12 months.

Matched pairs Rather than differencing the data, it is possible to include the complete time series for both firm j and its synthetic control, where the data for firm j in period t consists of two observations, one for $c = 1$ and one for $c = 0$ denoting the synthetic control. E.g.,

$$\begin{aligned} Y_{jct} &= \beta T_{ct} + \delta Z_{jc} \times T_{ct} + \gamma Z_{jc} + \eta Z_{jc} \times I\left[Post24\right]_{ct} + \mu_{jt} + \epsilon_{jct} \\ \tilde{Y}_{jct} &= \gamma \tilde{Z}_{jc} + \eta \tilde{Z}_{jc} \times I\left[Post24\right]_{ct} + \mu_{jt} + \tilde{\epsilon}_{jct} \end{aligned} \tag{43}$$

By including the match-specific, flexible time trend μ_{jt}, we will recover point estimates for β that are identical to those above. One potential advantage is that by estimating the regression in level form, it is possible to generate predicted values and residuals in levels—this will be needed to perform the wild bootstrap procedures described below. Additionally, this approach makes it possible to compare point estimates under relaxed models. For example, rather than estimate a match-specific, flexible time trend, it may be of interest to estimate a flexible time trend at the level of each treatment city along with match fixed effects, or simply a set of time- and match-fixed effects.

Notice that in this case, separate endpoint coefficients can be calculated for each group. When estimating heterogeneous effects, the characteristic of interest should be interacted with an indicator for months after 24 months, and earlier than 12 months, separately for each group. Then the coefficient δ captures only the differential effect of higher levels of Z_{jc} in a structural break city during the 24 months after a structural break versus its differential effect in the 12 months prior to the structural break.

5.4 Inferential strategy

Given 12 treatment cities, I depart from ADH in my preferred inference strategy.

My primary strategy is to implement a wild cluster bootstrap, imposing the null hypothesis of no effect (Cameron, Gelbach, Miller 2008; Webb 2014). Analytical standard errors clustered at the level of each treatment city would be problematic for at least two reasons. First, because clustered standard errors are only justified asymptotically, the small number of clusters will likely lead to over-rejection. Second, standard errors clustered by treatment city fail to account for the dependency structure across treatment cities that will be implied by the synthetic controls procedure. In order to account for the first issue, it would be sufficient to implement a wild cluster bootstrap at the treatment city-level. In order to also account for the second, I follow the procedure below.

1. Estimate the regression in (43). Construct the Wald statistic $w = \hat{\beta}/s_{\hat{\beta}}$, using analytical standard errors clustered by treatment city.

2. Re-estimate the regression in (43), without estimating β, and collect the appropriate residuals, $\hat{\mathbf{u}}$

   (a) Based on the estimated values for μ_{jt} (and any addtional controls), compute predicted values for Y_{jct}, i.e., for the "treated firms." Also compute the residuals.

   (b) Next, compute predicted values for all donor firms that have positive weight as synthetic controls for the treated firms. Notice that a single donor firm may have positive weight as a synthetic control for multiple treated firms. In this case, each instance of the control firm will initially receive a different predicted value. But it cannot be a reasonable bootstrap DGP for a single donor firm to have multiple outcomes in each period. Thus, in each period, each donor firm is assigned a weighted average of all predicted values that have been generated for it. The weighting for each value is equal to the synthetic control weight for that instance, divided by the sum of all synthetic control weights observed for that control firm in that period. After completing this procedure, no matter how many times a given donor firm appears as a synthetic control, each donor firm will have a single predicted value, and a single residual.

3. Construct a wild bootstrap replicate, clustered by origin city

   (a) Resample from the constructed residuals, clustered by origin city. That is, for the vector of residuals for all firms in each origin city g, form $\hat{\mathbf{u}}_g^{R*} = \hat{\mathbf{u}}_g^R \xi_g$, where ξ_g is a random variable with support on $\pm\sqrt{1/2}$, ± 1, and $\pm\sqrt{3/2}$, with equal probability. Also construct replicate values of the dependent variable equal to the predicted value plus $\hat{\mathbf{u}}_g^{R*}$

   (b) For each treated firm, apply the appropriate synthetic control weights to the replicate values of donor firms in order to form a wild bootstrap replicate of the synthetic control.

4. Re-estimate regression (43) for each bootstrap replicate, and keep the Wald statistic w_b^*.

5. Repeat steps 3 and 4 at least 500 times. Reject $\beta_0 = 0$ at level α if and only if $w < w^*_{[\alpha/2]}$ or $w > w^*_{[1-\alpha/2]}$, where $w^*_{[q]}$ denotes the qth quantile of $w_1^*, \ldots, w_B^*$.

Notice that in a number of cases, I am interested in both a base effect and an interaction term. In this case, a separate set of residuals is constructed for each parameter of interest, in each case imposing the null that that coefficient is equal to zero.

6 Results

6.1 Average effects

6.1.1 Business victimization survey

I begin by reviewing the business victimization survey. In this section, I run regressions of the form:

$$y_{jsct} = \beta \log HomRt_{ct} + \gamma Z_{jsc} + \delta \log HomRt_{ct} \times Z_{jsc} + \eta X_{jsct} + \mu_c + \lambda_{st} + e_{jsct} \tag{44}$$

where y_{jsct} is an individual-level outcome of interest, such as whether the establishment has been victimized, or hired guards, installed alarms, or reduced its business hours in the last year. The indices describe firms j in industry/sector s in city c and period t. The variable $\log HomRt_{ct}$ is my primary proxy for drug-related insecurity, with the coefficient β denoting the average effect of interest. The vector Z_{jsc} contains predetermined covariates, such as business size, labor intensity of revenue, and average wage in 2008.[19] Heterogeneous effects are captured by δ. The vector X_{jsct} contains time-varying covariates at the establishment- or city-level, such as other crime rates, or firm-level perceptions or victimization outcomes. The variables μ_c and λ_{st} denote city- and industry-year-fixed effects, respectively.

The identifying assumption is that conditional on city- and industry-year-fixed effects, changes in $\log HomRt_{ct}$ are uncorrelated with e_{jsct}.

Business hours and owner visits decline when violence increases. After asking businesses whether they have been affected by the various forms of theft, fraud, extortion, kidnapping, and property damage above, they are then asked whether, during the reference year, related insecurity led them to reduce business hours ($BizHours_{jsct}$) or investment ($BizInvest_{jsct}$), or led to greater absenteeism by owners ($BizOwner_{jsct}$), or cancellation of distribution plans ($BizDistrib_{jsct}$).[20] Based on individual-level responses to these questions, I construct a summary index, $BizIdx_{jsct}$, based on Anderson (2008).

In Table 7, I regress each of these business impact measures on the log of the annual homicide rate at the city-level, denoted $LnHomRt_{ct}$. Fixed effects control for time-invariant factors common to each city, national time trends that might vary across each 4-digit industry, and national trends that might vary depending on firm characteristics. The results indicate that greater homicide rates are significantly related to declines in the likelihood that businesses maintained normal production hours over the last year. In order to interpret the magnitude of the effect, I scale the point estimate by the inter-quartile range of the explanatory variable, finding that a business in a city at the 75th percentile of homicide rates would be 4.1 percentage points less likely to maintain normal production hours than one at the 25th percentile. Given that the average firm has a 14.1 percent likelihood, variation in homicide rates implies a 29% decline in the likelihood of maintaining normal business hours. Impacts on the business impacts index variable are marginally significant, while impacts on the other business impact variables are not significant.

In Table 8, I consider whether the economic effects of increased homicide rates might be driven entirely by correlation with other types of crimes, such as business theft. Thus, I include additional measures of annual crime rates at the city-level, such as $LnBizTheftRt_{ct}$, denoting the log of the business theft rate, as well as measures of the overall theft rate $LnTheftRt_{ct}$, and

[19]The notation indicates that these characteristics are known at the establishment-level. But recall that production data from the 2008 economic census are merged with the business victimization microdata at the detailed industry by city by firm size category-level. Firm size categories include microenterprises (fewer than 10 employees), and small, medium, and large categories, for which the thresholds vary slightly depending on the industry. For ease of exposition, I will, for example, refer to "labor-intensive establishments," rather than "industry-city-size categories with greater average labor intensity per establishment." This simplification is reflected in the notation. Observe that given city- and industry-by-year-fixed effects, coefficients on firm characteristics are identified based on within-city and within-industry variation.

[20]See Table 1 for the precise wording.

the overall rate of property crimes $LnPropCrimeRt_{ct}$. Finally, I construct a summary index, $VictimIdx_{jsct}$, for the set of victimization questions in the business victimization survey. Observe that while the $VictimIdx_{jsct}$ variable captures each firm's direct experience with economic crimes, the city-level statistics may better describe the general atmosphere of crime and insecurity.

I find that increased homicide rates continue to have strong impacts on firm behavior independently of any correlation with economic crimes. Holding exposure to other crime constant, a greater homicide rate leads to highly significant declines in the likelihood of either maintaining normal production hours or normal levels of owner visits, and to declines in the overall business impacts index. Scaling the effects by the inter-quartile range of the homicide rate, we see declines in the likelihood of maintaining normal business hours of 4.3 percentage points (31% decline), and declines in the likelihood of normal owner visits of 1.9 percentage points (23% decline). We can also compare the effects of the homicide rate to that of other types of crime. Homicide rates do not have as strong an effect as direct victimization as captured by the $VictimIdx_{jsct}$ variable; this has a large impact on all outcome varibles, including 5.3 and 4.0 percentage point declines in the likelihood of maintaining business hours and owner visits. Homicide rates have a stronger effect on production hours and owner visits than the local business theft rate $LnBizTheftRt_{ct}$, but a weaker effect on business investment and distribution choices.

These results suggest that between 2011 and 2013, operating in an environment with elevated homicide rates took a toll on economic activity. These impacts may be due to a variety of factors, such as declines in demand, increased costs of private security, or worker demand for compensating wages. Before turning to an investigation of mechanisms, I attempt to corroborate these reductions in economic activity using actual production data between 2007 and 2013, during periods with greater variation in levels of violence than those seen between 2011 and 2013.

6.1.2 Industrial production data

In this section, I consider monthly establishment-level data for manufacturing and construction firms from 2007-2013.

In monthly panel data, greater violence is correlated with less activity. In the panel data, I run regressions of the form:

$$y_{jsct} = \sum_i \beta_i HomRt_{isct} + \mu_j + \lambda_{st} + \sum_k \eta_{kt} Z_{jksc} + f_c(t) + \tilde{e}_{jksct} \tag{45}$$

where y_{jsct} is an individual-level outcome of interest, such as log revenue or log employment. The indices describe firms j in industry/sector s in city c and period t. The variables $HomRt_{isct}$ parameterize the annualized, monthly homicide rate into a series of bins, subscripted by i, providing my proxy for drug-related insecurity. The coefficients β_i denote the average effect for each level of the homicide rate. The vector Z_{jksc} contains predetermined covariates for business characteristics indexed by k, such as business size, labor intensity of revenue, and average wage observed for each establishment in 2007. Each of these k characteristics is interacted with a flexible time trend, η_{kt}. The variable $f_c(t)$ contains a city-specific linear time trend. The variables μ_j and λ_{st} denote city- and industry-year-fixed effects, respectively. The identifying assumption is that conditional on μ_j, λ_{st}, $\eta_{kt} \times Z_{jksc}$, $f_c(t)$, changes in $HomRt_{isct}$ are uncorrelated with changes in $\tilde{e}_{jsct}$.

Results are presented in Table 9. In addition to city- and industry-by-year fixed effects, and interactions of firm characteristics with a flexible time trend, I include a city-specific linear time trend. For the higher levels of violence, the coefficients β_i are highly significant and indicate that greater violence is correlated with greater reductions in revenue, employment, work hours, and earnings, but positively correlated with the average wage. For the highest category of violence, results are consistent with declines in revenue of 6% and declines in employment and work hours of 4.6% and 3.8%, respectively.

Graphical evidence of declines in economic activity following structural breaks. I now turn to evidence based on individual-level synthetic controls and structural breaks in homicide rates.[21] Each row of Figure 5 provides visual evidence comparing firms in cities with large structural breaks against their synthetic controls for four variables: homicide rates, log revenue, log hours, and log wagebill. Graphs on the left column depict outcomes in levels for each group, while graphs on the right present the difference between group averages. The two graphs in the first row of Figure 5 show that violence among firms in cities with large structural breaks exceeded that of their synthetic controls' cities by more than 50 homicides per 100,000 for almost all months over the next 3 years, and sometimes as much as 100 homicides per 100,000.

In the remaining rows, focus first on the left column. During the 12 months prior to the break, there is no visual evidence of a break in average outcomes that precedes the structural break in violence. This suggests that economic outcomes were varying smoothly at the time of the structural break, and is consistent with the assumption that causality does not run from breaks in economic activity to structural breaks in violence. Next, observe that for the revenue and work hours variables, there is an apparent decline in slope for the first 12 months following the structural break, before average economic activity turns upward again. Average outcomes among the synthetic control firms for the revenue and work hours variables also decline following the structural break, but less sharply. The growth in average outcomes after 12 months may reflect economic recovery in spite of continued violence, but it may also reflect in part the increasing

[21] Graphical results are presented for the synthetic controls analysis based on 5 nearest neighbors, and with synthetic control weights estimated while including a constant. The visual evidence is similar when 10 or 20 nearest neighbors are used, and whether the constant is estimated or omitted during the synthetic controls estimation.

importance of attrition.[22] On the other hand, in the final row, we see that the average labor intensity of revenue at industrial establishments does not appear to follow a trend after the structural break.

In the differenced graphs in the right column, average outcomes for revenue and work hours variables among firms in structural break cities decline for about 12 months relative to their synthetic controls, reaching levels about 4-5% lower. The differential in log revenue remains relatively constant through month 35, while the differential in employment and work hours begins to close after about month 18. By contrast, the labor intensity of revenue does not decline strongly after a structural break. Recall from the conceptual framework in Section 3 that factor intensities of revenue will respond to factor price changes, but should not respond to demand or Hicks-neutral productivity shocks. Thus, the final row indicates little evidence of a major change in factor prices following a structural break, but is consistent with demand or productivity shocks being the major source of impacts.

Regression evidence of declines in economic activity following structural breaks. Table 10 uses the regression framework in subsections 5.3 and 5.4 to estimate impacts and provide inference. The most robust impact appears to be log production hours per establishment, with impacts ranging from -3.8% and -3% when estimated with 5 and 20 nearest neighbors, respectively, and statistically significant at the 5% level in both panels for all inferential strategies. When estimated with 5 nearest neighbors, impacts on log employment and log revenue are marginally significant under the wild bootstrap clustered by origin city. When estimated with 20 nearest neighbors, only log revenue is significant, though at the 5% level. Nevertheless, in all cases, point estimates are consistent with declines in activity.

Across both panels, and for all inferential strategies, I found no evidence of significant impacts on the labor intensity of revenue or on wage rates. This is in contrast to the correlational evidence in Table 9, in which I find significant increases in wages. Given the stronger identifying assumptions required in Table 9, I regard the synthetic control estimates as more credible. Thus, based on the model in Section 3.2.3, in the absence of any impacts on factor intensities in Table 10, we would fail to reject the null hypothesis that crime has no impact on factor prices.

6.1.3 Labor market data

Next, I test for corroborating evidence of impacts in non-firm datasets including the ENOE labor market survey. The ENOE spans 73 cities from 2005 to 2013 and includes individuals in the full set of cities with structural breaks used above.

In a dataset restricted to those cities that experienced structural breaks, I run regressions of

[22] As shown in Figure 4, by 24 months after the event, close to 15% of the sample has been lost to attrition even when only 5 nearest neighbors are selected.

the form

$$y_{ict} = \sum_{\tau} \beta_\tau D_{ct}^{\tau} + \eta X_{ict} + \mu_c + \lambda_t + f_c(t) + v_{ict} \tag{46}$$

where the subscript i indicates individuals, c indexes cities, and t index time. The D_{ct}^{τ} are a series of event-time dummies equal to one when the structural break is τ months away in a given city, with $D_{ct}^{\tau} \equiv \mathbb{I}(t - BreakMth_c = \tau)$ and $BreakMth_c$ indicating the month of the structural break in city c. The coefficients of interest are the β_τ values, X_{ict} is a vector of predetermined covariates, and μ_c and λ_t are city and time fixed effects, respectively. The control $f_c(t)$ is a city-specific linear or quadratic time trend. The identifying assumption is that, conditional on fixed effects and city-specific linear or quadratic time trends, the timing of structural breaks in each city can be treated as randomly assigned. This assumption implies the prediction that $\beta_\tau = 0$ for all $\tau < 0$. I use the three months prior to the break as the reference period given the ENOE's quarterly structure. Figure 6 presents point estimates and 95% confidence intervals.

In the labor market data, point estimates are consistent with declines in formal employment of about 2%. Employment declines are driven by the formal sector, with overall informal employment remaining largely unmoved. Employment losses are larger among men than women, but measured imprecisely. Notably, in these data, wages decline for both formal and informal jobs. Compared to the industrial surveys, the sample of individuals in the labor market data are employed in a greater range of sectors and business sizes, and will include persons who remain employed by moving to lower-paying jobs, while the industrial surveys are essentially restricted to workers who remain employed at the same large manufacturing establishments.

6.1.4 Summary

Across datasets and identification strategies, the results in Section 6.1 indicate that violence leads to a decline in economic activity. Analysis of nationally representative business victimization surveys between 2011 and 2013 suggest that businesses operating in a violent atmosphere are significantly less likely to maintain hours of operation (4pp). Holding other types of crime constant, greater violence is also correlated with absentee owners (2pp). Focusing on large industrial establishments for which we have production data, between 2007-2013, I find consistent evidence in fixed effects regressions and in an analysis incorporating synthetic controls that production activity declines with violence. While point estimates and statistical significance are somewhat sensitive to specification, the most robust impacts I find are that labor hours decline by about 3-4pp in the 24 months following a structural break in violence.

However, I find no credible evidence of a factor price shock driving production cost increases. A significant impact on labor intensity of revenue would allow us to reject the null hypothesis that factor prices do not change significantly in response to increased violence, even in the presence of demand or Hicks-neutral productivity shocks; in Table 10, I fail to reject the null. There is no significant increase on observed average earnings per employee in the industrial production data in the synthetic controls analysis, which would provide some evidence of a labor supply shock—that is, the average wage of a worker who is able to remain employed at the same industrial plant does not increase. In fact, additional labor market data provide evidence of a decline in wages following an increase in the homicide rate (the difference may be a result of the greater range of sectors and business sizes included in the labor market data, greater exposure to demand shocks in those sectors, or the result of individuals switching to lower-paying jobs).

6.2 Channels and heterogeneous effects

While the results in Section 6.1 indicate that business activity declines with violence, they do not identify the channels through which these impacts occur. The absence of a significant impact on labor intensity of revenue and wage rates in the synthetic controls analysis suggests that factor prices do not change, but may not be the most powerful test of this channel.

The first possibility I consider is that particular types of firms may be directly targeted in economic crimes (theft, property damage, extortion) that are correlated with increased violence. A related possibility is that fear of direct victimization may lead firms to incur private security costs. Increased marginal costs of production would then imply reduced output and reduced usage of other inputs. I do not find evidence that this is the case. That is, I find no significant correlation between increased homicide rates and business victimization by economic crimes or private security measures such as hiring guards and installing alarms. I investigate this further below.

I next consider whether increased violence operates as some form of input shock. The primary possibility I consider is that crime constitutes a labor market shock in which workers become reluctant to work or travel during violent periods, demand compensating wages, or even migrate away in the presence of violence (KMM 2010; Rozo 2014). The conceptual framework in Section 3.2.3 (and results in the online appendix) indicate that such a shock would lead to heterogeneous effects, depending on factors such as a firm's labor intensity of revenue, wage rate, and the revenue productivity of its labor. I take these predictions to the data, but in neither the business victimization surveys nor in the industrial production data do I find evidence consistent with this possibility.

Finally, I consider whether violence behaves as a demand or productivity shock. Based on the conceptual framework in Section 3, the primary observable prediction of (additive) demand and productivity shocks is that impacts should be heterogeneous along firm characteristics that are proportional to TFP, such as total employment or total revenue. In the business victimization survey, I find that small firms are significantly more adversely affected when homicide rates increase in the trade and services sectors. This is consistent with an additive or productivity shock. However, I find no evidence of heterogeneity by size among the industrial establishments in the business victimization survey, or in the monthly production data for industrial establishments. This is consistent with a (multiplicative) demand or productivity shock that is proportional to firm size.

I turn to the evidence now, and discuss the findings in Section 6.3.

Are impacts driven by business victimization and increased private security costs, or by other types of crime? In the business victimization survey, businesses are asked if they were affected by various forms of theft, fraud, extortion, kidnapping, and property damage during the reference year. They are also asked whether they undertook a variety of private security measures, such as hiring guards, installing security alarms, buying insurance, or changing doors, windows, and locks. As shown in Table 2, 37% of establishments report some form of victimization, with the most frequent types being theft of vehicle parts (16%), petty theft (14%), and extortion (8%). Among the most common private security changes (not shown) are installing alarms (27%), hiring guards (15%), and buying insurance (10%). Using the individual-level responses, I construct a summary index for each set of questions—*VictimIdx* and *ActIdx*.

In Table 11, I correlate these dependent variables with measures of insecurity and with firm characteristics. Notably, across all of these variables, I find no evidence that an increase in the homicide rate is correlated with either direct victimization or with private security measures.

One concern may be that the business victimization surveys were conducted after the greatest increases in violence had occurred. Thus, I also test for a correlation between homicide rates and other types of crime using official crime statistics that do span the same period during which structural breaks in homicide rates were occurring. In Table 12, I find limited evidence of a correlation between homicide rates and economic crimes. In Panel A, I use monthly data at the city-level. Unfortunately, monthly data at the city-level are only available beginning in 2011, although state-level data are available for a longer time period. However, because the city-level data span the same period as the victimization survey, they can be used to corroborate those results. I find little correlation with homicide rates and other major types of crimes in city-level data from 2011-2014. In Panel B, I use data at the state-level for the same time period, and again find no significant correlation. In Panel C, I do find significant correlations using state-level data from 2005-2014, spanning the periods before and after the major increases in violence beginning in 2008. Given the time span, I control for city-specific quadratic time trends. I find no significant impact on the local business theft rate. I do find small, marginally significant effects on property crimes and general theft. However, the largest correlation is with abduction rates.

Tentatively, this suggests that the primary channels through which violence affects economic activity may not be through economic crimes against firms or through increased private security costs. To be clear, this does not imply that economic crimes like theft do not have substantial effects independently of violence; rather, it is consistent with the finding in Table 8 that violence affects activity independently of any correlation with economic crimes like theft.

Are impacts consistent with violence as a shock to labor supply? As already noted, one test for whether violence behaves as a factor price shock is to test whether the labor intensity of revenue changes following an increase in violence. Above, I found no evidence of this. In this section, I consider an alternate test based on heterogeneity of impacts to input usages across firms.

As shown in Section 3.2.3, if we assume that crime behaves as common, percentage increase in the implicit wage rate required to bring workers in to the establishment, we should find firms whose revenue streams are most dependent on wage labor are most adversely affected. If we assume that crime behaves as a common, absolute increase in the implicit wage rate, we should find that the magnitude of impacts depend on the revenue lost per unit of labor if they do not work, and on the level of the implicit wage rate at the establishment (see online appendix).

In Tables 14 and 15, I consider regressions motivated by the results in Section 3.2.3. In Table 14, for purposes of this test, I focus primarily on indicators of work hours (*BizHours*) and investment (*BizInvest*) as the most direct analogues to input usage in the business victimization surveys. I find no evidence in the business victimization surveys that inverse revenue productivity of labor or labor intensity of revenue are correlated with greater impacts on these indicators of input usage. Controlling for establishment size, I do find that establishments with lower wage rates are more likely to receive continued visits by the owner and maintain the same distribution routes. However, scaling these coefficients by the interquartile range of the corresponding explanatory variables, the magnitudes appear to be economically unimportant.

In Table 15, I focus on demand for labor hours among industrial establishments. The regressions in columns (1), (3), and (5) estimate both a base effect and an interaction term, while the regressions in columns (2), (4), and (6) omit the base effect. While it is most consistent with the model predictions to omit the base effect, such regressions would risk conflating the average impact of crime as a demand shock with its impact as a factor price shock. Focusing on columns (1), (3), and (5), once again I find no evidence of heterogeneous impacts by labor share of revenue or the inverse revenue productivity of labor.

Are impacts heterogeneous by size? In Tables 14 and 15, I also consider whether crime behaves as a demand or productivity shock of various forms. Here, I describe findings based on the business victimization survey.

As shown in Section 3, a proportional demand shock implies that log revenue and log input usages will be affected equally, and predicts no heterogeneity along correlates of TFP such as log employment or log revenue. On the other hand, while additive demand or productivity shocks also predict that log revenue and log input usages will be affected equally, they predict that small firms will be most adversely affected by violence. Thus, I reject a proportional demand/productivity shock in favor of an additive demand shock if impacts are heterogeneous by log revenue or log employment.

In Table 14, I focus once again on indicators of work hours (*BizHours*) and investment (*BizInvest*) in the business victimization survey, pooling firms across all sectors. I find that large firms are less affected by an increase in the homicide rate. Comparing establishments at the 25th and 75th percentiles by employment, smaller establishments would be 3.3pp less likely to maintain normal business hours after experiencing the same increase in the homicide rate. Thus, based on the victimization survey, in this pooled sample of firms in all industries, I reject that violence acts as a proportional demand/productivity shock, in favor of the alternate hypothesis that violence behaves as an additive demand or productivity shock.

Is there heterogeneity across major economic sectors? In Table 13, I compare impacts across sectors for two outcome variables, the work hours (*BizHours*) and overall business impacts index (*BizIdz*) variables.

For both variables, I find that services are most affected, while industrial firms are least affected. Along the business impacts index, which captures variation across all business variables, the magnitude of impacts are clearly largest among services, next largest among commercial firms, and least among industrial firms. Along the business hours variable, impacts are roughly equal for both industrial firms and commercial firms.

Taking industrial establishments as the traded goods sector, and services as non-traded, the difference in impacts is consistent with a model in which local demand shocks are more important for non-traded goods rather than for traded goods. However, the model in Section 3 also implies that firms in industries with greater profit shares (lower values of η_s) will be affected less than firms in more competitive industries. Thus, taking industrial firms as more profitable than services would also be consistent with this finding.

Does heterogeneity along firm characteristics vary across major economic sectors? I now re-estimate the regressions in Table 14 for each major sector in the business victimization survey: industry, wholesale and retail trade, and services. I also review evidence based on the industrial production data in Table 15.

In the business victimization surveys, I focus on testing whether this heterogeneity by business size remains significant across major sectors (Table 16). In fact, I find that it is most prominent among establishments in the retail and wholesale trade sectors, where base effects and heterogeneity by size are large and significant. For the industrial sector, there is no evidence of effects in the base variable or of heterogeneity.

Returning to the industrial production data in Table 15, I find that my results are consistent with those of the victimization surveys: there is no evidence of heterogeneous effects by establishment size. It is important to recall that while the industrial surveys focus on much larger firms than those in the business victimization surveys, there nevertheless remains variation by size that should serve to identify such impacts.

6.3 Discussion

The preceding results support that high levels of violence may reduce economic activity.

However, the effects of violence appear to be independent of any increase in crimes that directly target firms, and they do not appear to lead to an increase in private security costs. There is also little evidence, in the Mexican setting, that drug violence behaves as a shock to labor costs; that is, firms that depend more heavily on labor do not appear to be more strongly affected than firms that rely less on labor. The most consistent interpretation of the data is that drug violence in the Mexican setting behaves as a demand or productivity shock. But the form of these demand or productivity shocks varies by major economic sector. Within the trade and services sectors, I find that smaller firms are impacted more than proportionally compared to large firms—consistent with additive demand or additive productivity shocks. In the industrial sector, I find that small and large firms are impacted roughly proportionally—consistent with proportional demand or productivity shocks.[23]

Already these findings constitute new evidence of the ways that a local economy is impacted by violence. But they remain reduced form in the sense that they do not explain why the impacts of violence have these particular characteristics in each sector. If violence is primarily a demand or productivity shock, why are small firms most strongly affected in the services and trade sectors, but not in the industrial sector? Below, I consider some possibilities.

6.3.1 Violence as a productivity or demand shock

Violence as a productivity shock. If management at low TFP trade and services establishments is differentially affected when violence increases, but management at industrial establishments is affected in a proportional way across both low and high TFP establishments, this would be consistent with the above results.

Based on Table 14, it is intriguing that owners visit their establishments less when violence increases. This reduced owner attention and oversight would be consistent with productivity

[23]Studying the impacts of drug violence on manufacturing plants in Colombia, Rozo (Nov 2014, footnote 41) also finds that impacts do not vary by production levels. Thus, our results agree in this empirical finding. In the Mexican context, I argue these impacts are consistent with a demand or productivity shock, and have relied on other evidence to argue there is no evidence that costs are driven by a labor market shock. In her setting, she relies on other evidence to argue that drug violence creates upward pressure on firm costs through increased labor costs and out-migration.

declines, and we again see that smaller establishments are most likely to to have absentee owners when violence increases. This finding is also consistent with anecdotal evidence suggesting that firms voluntarily attempt to lower their profile when violence increases—removing advertisements from the sides of buses, reducing production—in order to lower their exposure to organized crime. Indeed, another way that owners reduce their involvement may be through reduced business hours.

Within the commercial and services sectors, it may be that low TFP establishments are largely those that require the owner's presence in order to operate effectively—e.g., single-employee retail establishments vs. large establishments which depend on some layer of middle management. In this case, when owners reduce their involvement, this would lead to the observed differential impact among low TFP establishments versus high TFP establishments in the trade and services sectors. But then it remains to explain why establishments in the industrial sector are instead impacted proportionally. One possibility is that due to greater competition within the manufacturing sector, the range of variation in TFP is lower among manufacturing establishments than in retail trade and services. Thus, it could simply be that the kinds of low productivity establishments that are so heavily dependent on owner involvement in the retail and services sectors, are less common in the manufacturing sector.

Violence as a demand shock. If the types of consumers that purchase products at low TFP trade and services establishments are differentially affected when violence increases, but consumers of products at low and high TFP industrial establishments are affected in a proportional way, this would explain the above results. Consider a stylized scenario. First, suppose that both low and high income consumers purchase goods at trade and services establishments, but only higher income consumers purchase manufactured goods. Low TFP establishments sell low quality products, which are only purchased by low income consumers. Finally, suppose that low income consumers are most affected when violence increases. This would be sufficient to explain the above outcomes.

While it may be possible to explain heterogeneity by size within the trade and services sectors in various ways, perhaps the more puzzling result is that in the industrial sector, heterogeneous establishments are all affected proportionally. It is as if preferences for manufacturing goods are homothetic, and high levels of violence behave as a negative income shock. Informally, one possibility is that consumers of manufactured goods are most likely to be other manufacturers. It may be that linkages within the sector lead to declines in demand that are proportional across firms of different sizes and levels of productivity.

6.3.2 Assessing the magnitude of economic disruption

To put these results into context, I compare the one-year value of jobs lost to the value of lives lost and the value of housing price declines. Necessarily, these exercises involve strong assumptions.

I begin by estimating the value of lost jobs based on estimates from this paper. From Table 10, I use the lower of the two point estimates for lost jobs, at -2.8pp per structural break. While my estimates are constructed for a population of industrial firms, I will assume that all formal sectors of the economy are similarly affected. Based on population estimates by age group published by the Mexican Ministry of Health for 2008, I assume that 65% of the population is of working age (15-64). Based on World Bank estimates for 2008, I assume a 61% labor force participation rate, and 3.5% unemployment. For a population of 100,000 this would imply a loss of 680 jobs and an increase in the unemployment rate to 5.5%. But this will be an overestimate of unemployment if people move to informal or part-time jobs. I will account for this by scaling down the value of lost wages that I assign to each lost job. In World Bank data, nominal GDP in 2008 was $9,500 per capita and $25,400 per person employed. In the Mexican economic census for 2008, wages and benefits per hired employee were $8,900, while in the survey data I use here the average wage is about $7,600 in 2007. GDP per person employed would seem the best measure of the total economic value of a job, but it may not reflect the ability to draw down savings or take informal or part-time employment when a job is lost. (It will also reflect general equilibrium implications not captured in the comparison measures I construct.) Thus I assume that all formal sector workers find a part-time job at half their previous wage and economic value, resulting in lost wages of $4,450. Under these assumptions, the implied cost of economic disruption for one year is $27 per capita (or 0.33% GDPpc).

Next, I compute a value for the mortality cost. As a measure of the increased mortality risk following a large structural break, I use an average value of 60 per 100,000. (Using 20 as an estimate of the pre-break homicide rate, the percentage increase is 200%. The implied elasticity of employment with respect to the homicide rate thus implies that a 10% increase in the homicide rate would result in employment declines of 0.14%.) Heinle, Molzahn, and Shirk (2015) document an average age per homicide victim of 32. For this age group in Mexico, Martínez and Aguilera (2013) use methods based on Murphy and Topel (2006) to estimate lower and upper bounds on the value of a life year at $15,000 and $45,000 in 2004 USD. Taking the midpoint at $37,600 in 2008 USD, the per capita mortality cost is $23 (or 0.2% GDPpc). Thus, the cost of economic disruption for one year is comparable to the flow value of lost lives.

To provide a second benchmark, I compare the cost of housing value declines based on AGS (2014). The authors' estimates imply that a 10% increase in the homicide rate would result in a 0.12% decline in the value of poor quality homes. For a 200% increase, the implied loss of value would be 2.4%. Based on a 2010 population of 117.9 million, as well as 28.6 million homes out of which 10% are poor (reported in AGS), one would expect 2,400 poor homes per 100,000 persons. At an average appraised value of \$24,000, the implied loss of housing values would be about \$14 per capita (or 0.14% GDPpc). Thus, the cost of economic disruption for one year is about double the loss in home values.

7 Conclusion

This paper studies the economic consequences of recent high levels of drug violence in Mexico. But its results are relevant to many countries, both more and less developed, that struggle with crime and conflict. Drug trade-related violence has hardly been unique to Mexico. Among 18 countries in the Americas in 2011, the percentages of total homicides related to organized crime or gangs was 30% in the median country and over 45% in the upper quartile. Across the largest cities in 127 countries between 2005 and 2012, the eight most violent cities were all in Latin America and the Caribbean.[24] Worldwide, Figure 7 documents a substantial negative correlation between greater homicide rates at the national level and GDP per capita.

Across two datasets and identification strategies, I find evidence that economic activity among formal firms declines when violence increases. Surprisingly, these impacts do not appear to be the result of an increase in crimes that directly target firms, and they do not appear to be due to an increase in private security costs. There is also little evidence in the Mexican setting that drug violence behaves as a shock to labor costs; that is, firms that depend more heavily on labor do not appear to be more strongly affected than firms that rely less on labor. The most consistent interpretation of the data is that drug violence in the Mexican setting behaves as a demand or productivity shock. But the form of these demand or productivity shocks varies by major economic sector. Within the trade and services sectors, I find that smaller firms are impacted more than proportionally compared to large firms—consistent with additive shocks. In the industrial sector, I find that small and large are impacted roughly proportionally—consistent with proportional demand or productivity shocks. I also find that firms in the retail and wholesale trade sectors are impacted more strongly than firms in the industrial sector. This is consistent with model predictions under the assumption that economic profits are larger in the industrial sector than in the other sectors.

Putting my results into context, a back-of-the-envelope calculation suggests that the cost of economic disruption in affected cities (\$27 per capita) is of about the same magnitude as the annual mortality cost (\$23 per capita) and about double the magnitude of the total loss in home value (\$14 per capita). Despite massive increases in the level of violence, estimates of economic impact in the Mexican setting appear to be lower than those seen in other settings, such as

[24]Including: Basseterre, Saint Kitts and Nevis (131.6 in 2011); Caracas, Venezuela (130.5 in 2007); Guatemala City, Guatemala (121.3 in 2007); Kingston, Jamaica (111.5 in 2007); Belize City, Belize (105.1 in 2011); Tegucigalpa, Honduras (102.2 in 2011). Outside of Latin America and the Caribbean, the top homicide rates were in Maseru, Lesotho (64.1 in 2007) and Cape Town, South Africa (61 in 2006). Estimates from UNODC (2014).

Colombia and Italy. Understanding why this is the case remains an important area for further work.

With respect to mechanisms, it is particularly striking that business owners become less likely to visit their establishments when violence increases. Within the services and trade sectors, these impacts are heterogeneous by firm size, with owners of small establishments most affected. If the performance of small businesses depends more on owner presence—perhaps due to the availability of middle managers at larger businesses—then a decline in owner visits would explain the disproportionate effects on small businesses.

It is important to acknowledge the limitations of this work. Notably, while the business victimization survey contributes unique data and informs multiple findings, it was conducted during or after violence had peaked across the country and may not be representative of business reactions while violence was increasing most strongly. In addition, the empirical analysis of these data relies on stronger identifying assumptions than the analysis of the industrial production data and labor market outcomes.

This work contributes to a small but growing body of work attempting to understand the microeconomic costs of conflict and violence on economic activity. More broadly, this work contributes to an understanding of the ways that the external environment may impact firm productivity (Syverson 2011; Bartelsman, Haltiwanger, Scarpetta 2010). In particular, it points to entrepreneurial attention as an important and variable component of productivity, and highlights that its importance varies across firms.

These results suggest that in addition to their direct impacts on well-being, crime and violence should be considered important determinants of economic performance.

References

Abadie, Alberto, Alexis Diamond, and Jens Hainmueller, "Synthetic control methods for comparative case studies: estimating the effect of California's tobacco control program," *Journal of the American Statistical Association*, 2010, *105*, 493.

— **and Javier Gardeazabal**, "The economic costs of conflict: a case study of the Basque Country," *American Economic Review*, 2003, *93* (1), 113.

Ajzenman, Nicolas, Sebastian Galiani, and Enrique Seira, "On the distributed costs of drug-related homicides," Working Paper 364, Center for Global Development 2014.

Anderson, Michael L, "Multiple inference and gender differences in the effects of early intervention: a reevaluation of the Abecedarian, Perry Preschool, and Early Training projects," *Journal of the American Statistical Association*, 2008, *103* (484), 1481.

Bai, Jushan and Pierre Perron, "Computation and analysis of multiple structural change models," *Journal of Applied Econometrics*, 2003, *18*, 1.

Balmori, Jose, "The economic consequences of the Mexican drug war," 2014. Mimeo, Cornell University.

Bartelsman, Eric, John Haltiwanger, and Stefano Scarpetta, "Cross-country and within-country differences in the business climate," *International Journal of Industrial Organization*, 2010, *28*, 368.

BenYishay, Ariel and Sarah Pearlman, "Homicide and work: the impact of Mexico's drug war on labor market participation," 2013. Mimeo, Vassar College.

Berman, Eli and Aila M Matanock, "The empiricists' insurgency," *Annual Review of Political Science*, 2015, *18*, 443.

Blattman, Christopher and Edward Miguel, "Civil war," *Journal of Economic Literature*, 2010, *48* (1), 3.

Busso, Matías, Lucía Madrigal, and Carmen Pagés, "Productivity and resource misallocation in Latin America," Working Paper IDB-WP-306, Inter-American Development Bank 2012.

Cameron, A Colin, Jonah B Gelbach, and Douglas L Miller, "Bootstrap-based improvements for inference with clustered errors," *The Review of Economics and Statistics*, 2008, *90* (3), 414.

Chaidez, Lilia, "More police funding, more violence? Regression discontinuity evidence," 2014. Job Market Paper, Department of Agricultural and Resource Economics, University of California, Berkeley.

Collier, Paul and Marguerite Duponchel, "The economic legacy of civil war: firm-level evidence from Sierra Leone," *Journal of Conflict Resolution*, 2012, *57* (1), 65.

Czabanski, Jacek, *Estimates of cost of crime: History, methodologies, and implications*, Berlin: Springer-Verlag.

Díaz-Cayeros, Alberto, Beatriz Magaloni, Aila Matanock, and Vidal Romero, "Living in fear: Social penetration of criminal organizations in Mexico," 2011. Unpublished.

Dell, Melissa, "Trafficking networks and the Mexican drug war," *American Economic Review*, 2015, *105* (6), 1738.

Faber, Benjamin, "Trade liberalization, the price of quality, and inequality: Evidence from Mexican store prices," 2014. Draft, University of California, Berkeley.

Hammitt, James K and María Eugena Ibarrarán, "The economic value of fatal and non-fatal occupational risks in Mexico City using actuarial- and perceived-risk estimates," *Health Economics*, 2006, *15*, 1329.

Heinle, Kimberly, Cory Molzahn, and David A. Shirk, "Drug violence in Mexico: Data and analysis through 2014," Technical Report, Department of Political Science and International Relations, University of San Diego 2015.

Hjort, Jonas, "Ethnic divisions and production in firms," *Quarterly Journal of Economics*, 2014, p. 1899.

Ibragimov, Rustam and Ulrich K Müller, "t-statistic based correlation and heterogeneity robust inference," *Journal of Business and Economic Statistics*, 2010, *28* (4), 453.

ICG, "Peña Nieto's challenge: criminal cartels and rule of law in Mexico," Latin America Report 48, International Crisis Group March 2013.

Kilmer, Beau, Jonathan P Caulkins, Brittany M Bond, and Peter H Reuter, "Reducing drug trafficking revenues and violence in Mexico: would legalizing marijuana in California help?," RAND Corporation Occasional Paper, RAND Corporation, International Programs and Drug Policy Research Center 2010.

Klapper, Leora, Christine Richmond, and Trang Tran, “Civil conflict and firm performance: evidence from Côte d’Ivoire,” Technical Report, Finance and Private Sector Development Team, Development Research Group, World Bank.

Ksoll, Christopher, Rocco Macchiavello, and Ameet Morjaria, “Guns and roses: Flower exports and electoral violence in Kenya,” June 2014. Draft.

Kugler, Maurice and Eric Verhoogen, “Prices, plant size, and product quality,” *Review of Economic Studies*, 2012, *79*, 307.

Lindo, Jason M and María Padilla-Romo, “Kingpin approaches to fighting crime and community violence: Evidence from Mexico’s drug war,” NBER Working Paper 21171, The National Bureau of Economic Research 2015.

Martínez, Gabriel and Nelly Aguilera, “The value of longevity in Mexico,” *Economía Mexicana*, 2013, *2*, 341.

Murphy, Kevin M and Robert H Topel, “The value of health and longevity,” *Journal of Political Economy*, 2006, *114* (5), 871.

ONDCP, “What America’s users spend on illegal drugs: 2000-2010,” Technical Report, Office of Research and Data Analysis, Office of National Drug Control Policy 2014. Prepared by RAND Corporation: B Kilmer, S Everingham, J Caulkins, G Midgette, R Pacula, P Reuter, R Burns, B Han, R Lundberg.

Pinotti, Paolo, “The economic consequences of organized crime: Evidence from southern Italy,” Working Paper 868, Banca d’Italia 2012.

Robles, Gustavo, Gabriela Calderón, and Beatriz Magaloni, “The economic consequences of drug trafficking violence in Mexico,” 2013. Draft.

Rozo, Sandra V, “Is murder bad for business and real income? The effects of violent crime on economic activity,” 2014. Job Market Paper, Department of Economics, UCLA.

Severnini, Edson R, “The power of hydroelectric dams: Agglomeration spillovers,” 2014. Working paper, Carnegie Mellon University.

Syverson, Chad, “What determines productivity?,” *Journal of Economic Literature*, 2011, *49* (2), 326.

UNODC, “Global study on homicide 2013: Trends, context, and data,” Report, United Nations Office of Drugs and Crime 2014.

— **and World Bank**, “Crime, violence, and development: Trends, costs, and policy options in the Caribbean,” Joint Report, United Nations Office of Drugs and Crime, and World Bank Latin America and Caribbean Region 2007.

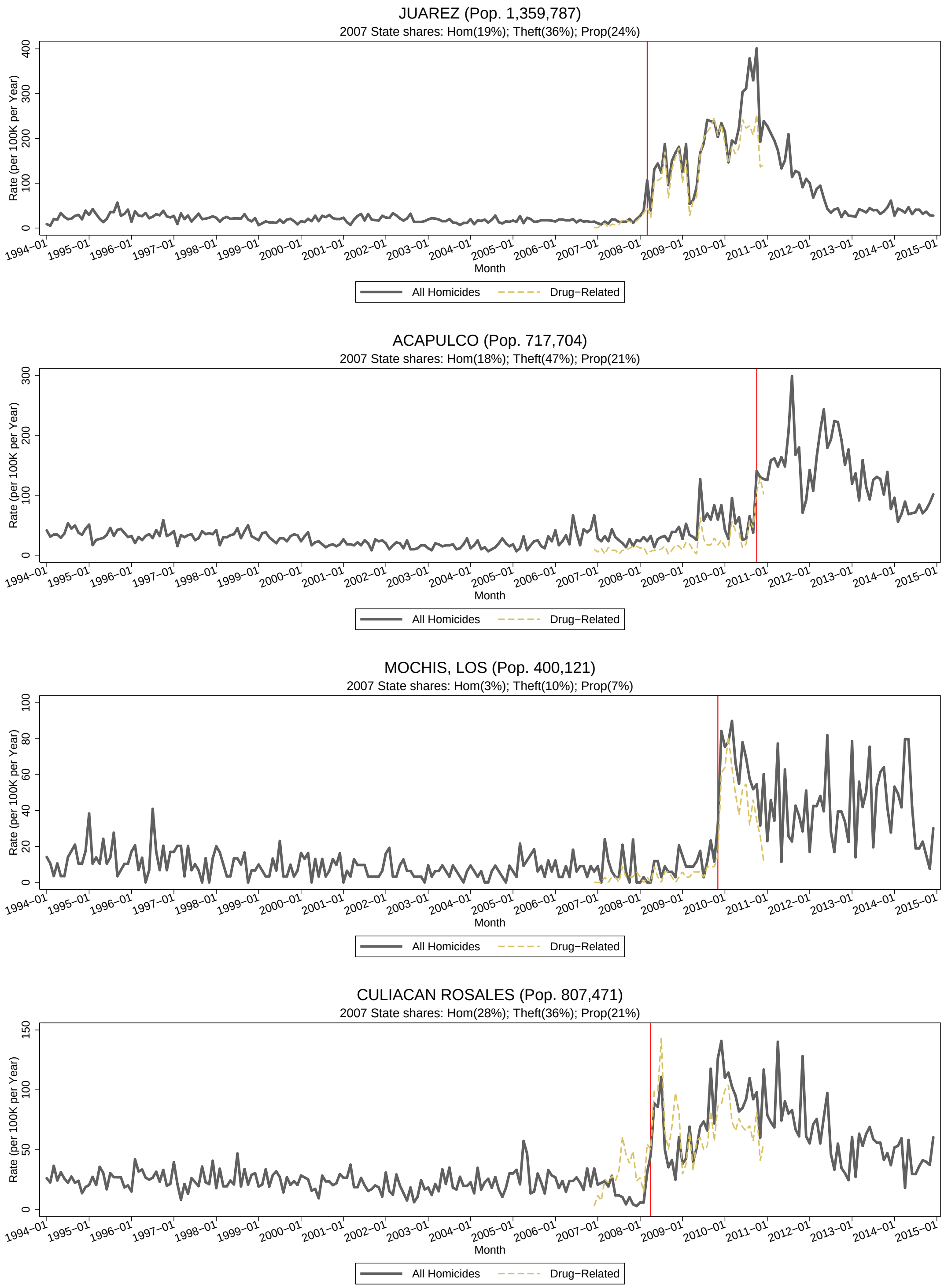
Velásquez, Andrea, “The economic burden of crime: Evidence from Mexico,” 2014. Job Market Paper, Duke University.

Webb, Matthew D, “Reworking wild bootstrap based inference for clustered errors,” Working Paper 1315, Department of Economics, Queen’s University November 2014.

WorldBank, “The costs of violence,” Report, Conflict, Crime, and Violence Team, Social Development Department, The World Bank 2009. Stergios Skaperdas, Rodrigo Soares, Alys Willman, Stephen C Miller.

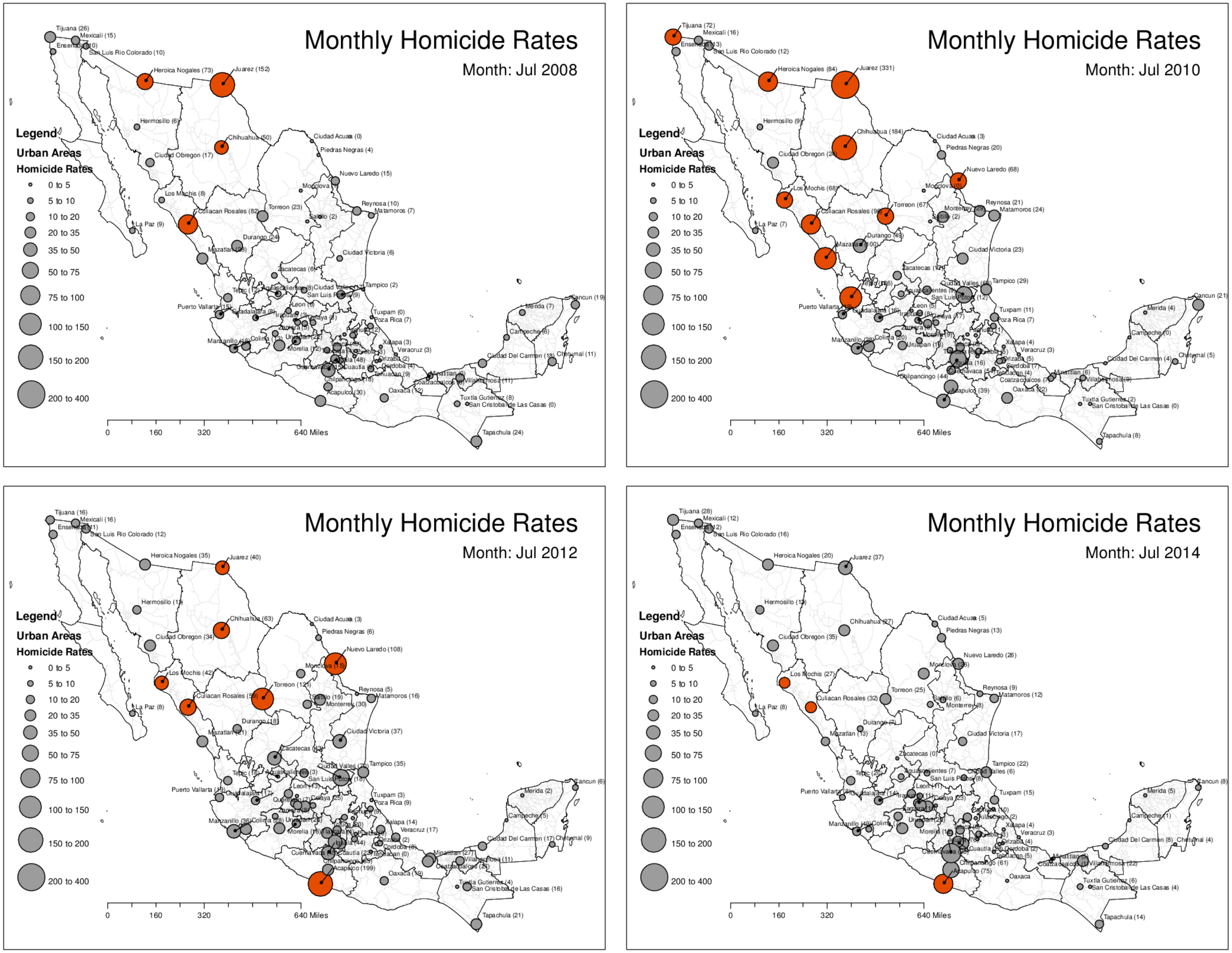
Figures and Tables

Figure 1: Homicide rates in selected cities



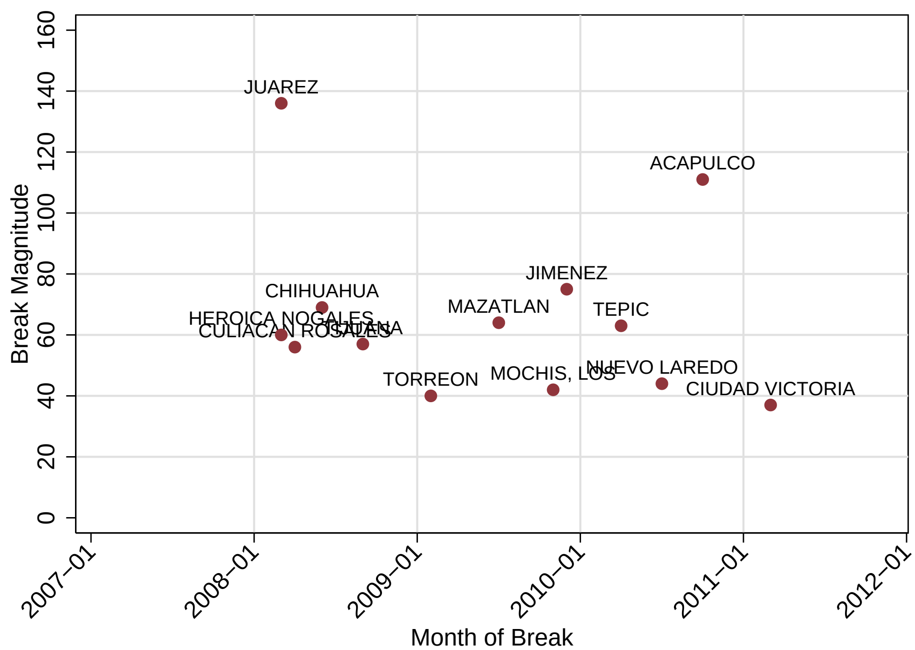
Notes: These figures illustrate the highly discontinuous nature of increases in crime in selected cities. Overall homicide counts from 1994-2013 based on municipality-level mortality statistics from INEGI/SINAIS, aggregated to the urban area-level; counts for 2014 based on municipality-level police statistics for intentional homicides from SNSP, with municipality-specific adjustments based on the ratio observed in the last 6 months of 2013. Drug-related homicide counts based on data from the Office of the Presidency avaiable from December 2006 through 2010. Monthly population counts based on linear interpolation of municipality-level annual population estimates from CONAPO. Red lines indicate structural breaks estimated using all months from 2005 to 2013.

Figure 2: Homicide rates across Mexican cities, 2008-2014



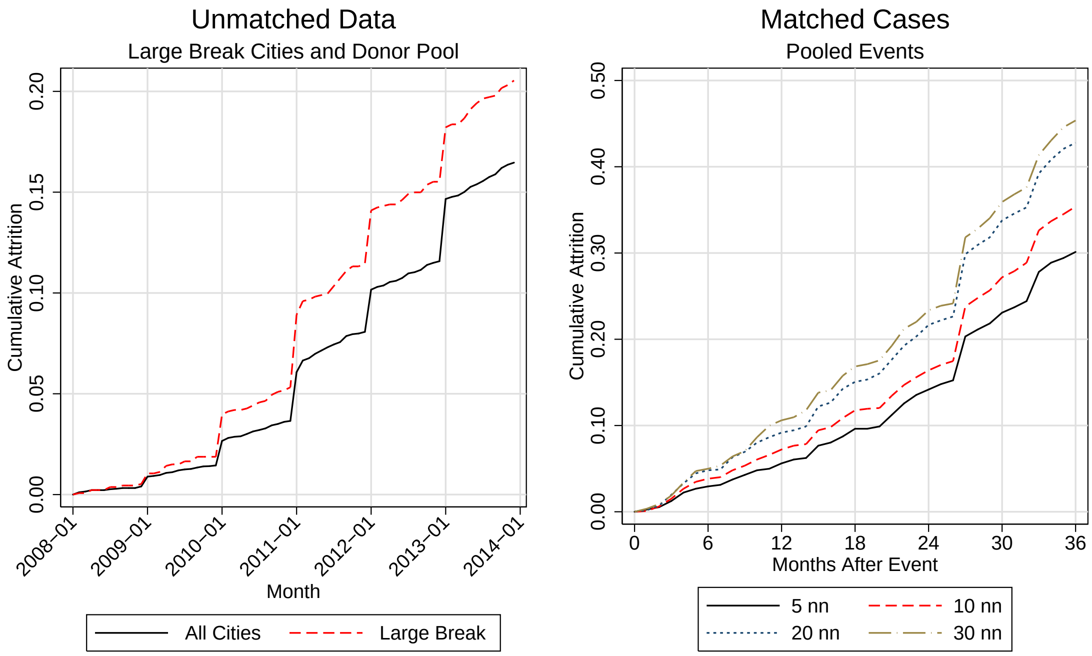
Notes: These maps illustrate the geographic progression of violence outbreaks across Mexico. Red circles indicate cities subsequent to an identified structural break. Reported rates are the 3-month average annualized monthly homicide rate for June, July, and August. Coloration is removed in the first month that the 6-month average falls below 35, and remains below 35 for the following 6 months.

Figure 3: Cities experiencing large structural breaks



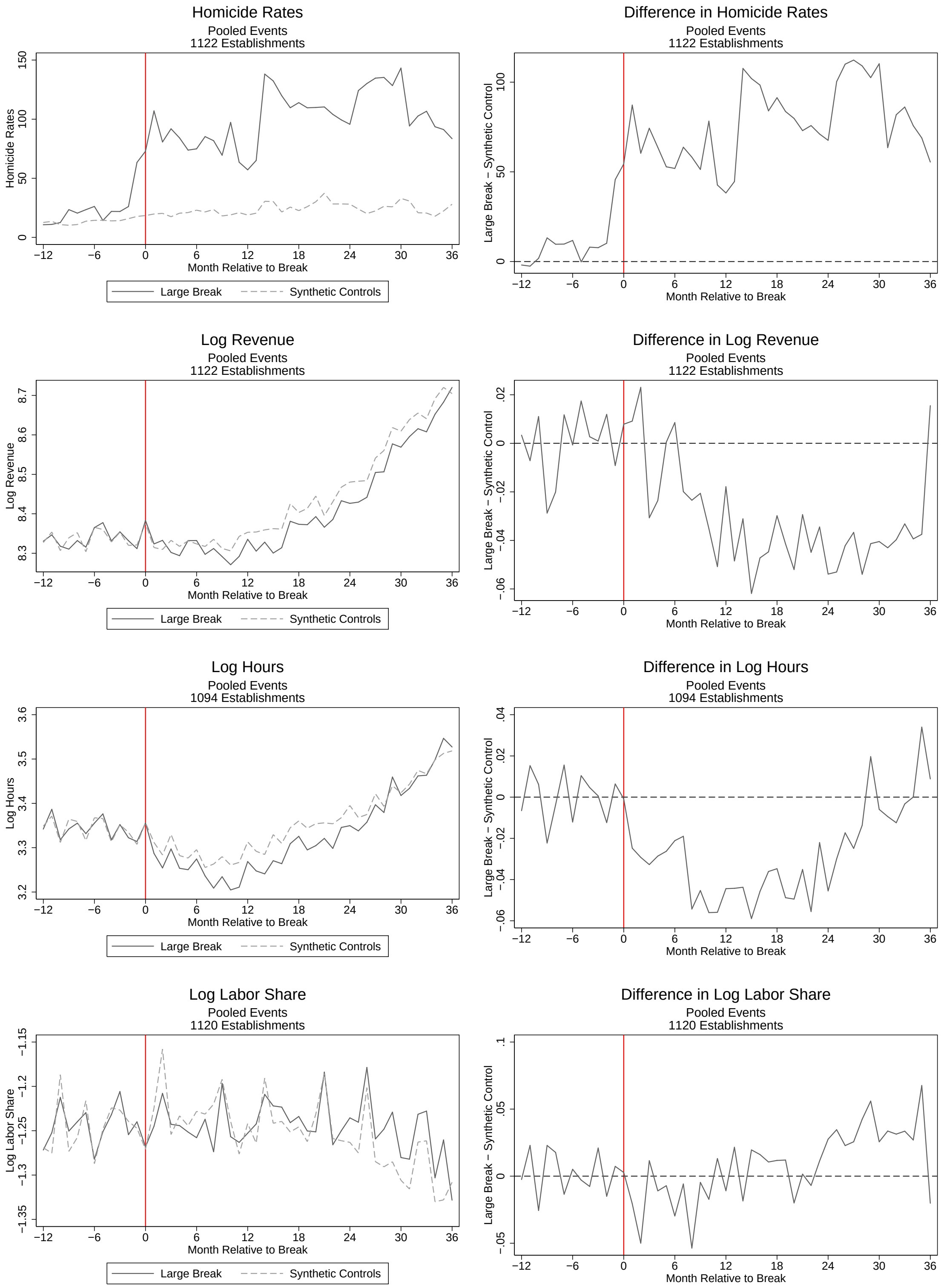
Notes: Based on analyses of homicide rates from 2005-2014 using municipality-level mortality statistics from INEGI/SINAIS and police statistics from SNSP, aggregated to the urban area-level. Structural breaks estimated using all months, but constrained to be no smaller than 15% of the sample time period. Breaks are considered statistically significant if p-values are less than .05 under all of the max, average, and exponential F-tests. Break magnitudes are calculated as the difference in average homicide rates during the 24 months after vs. before the identified break.

Figure 4: Increased attrition under individual-level synthetic controls



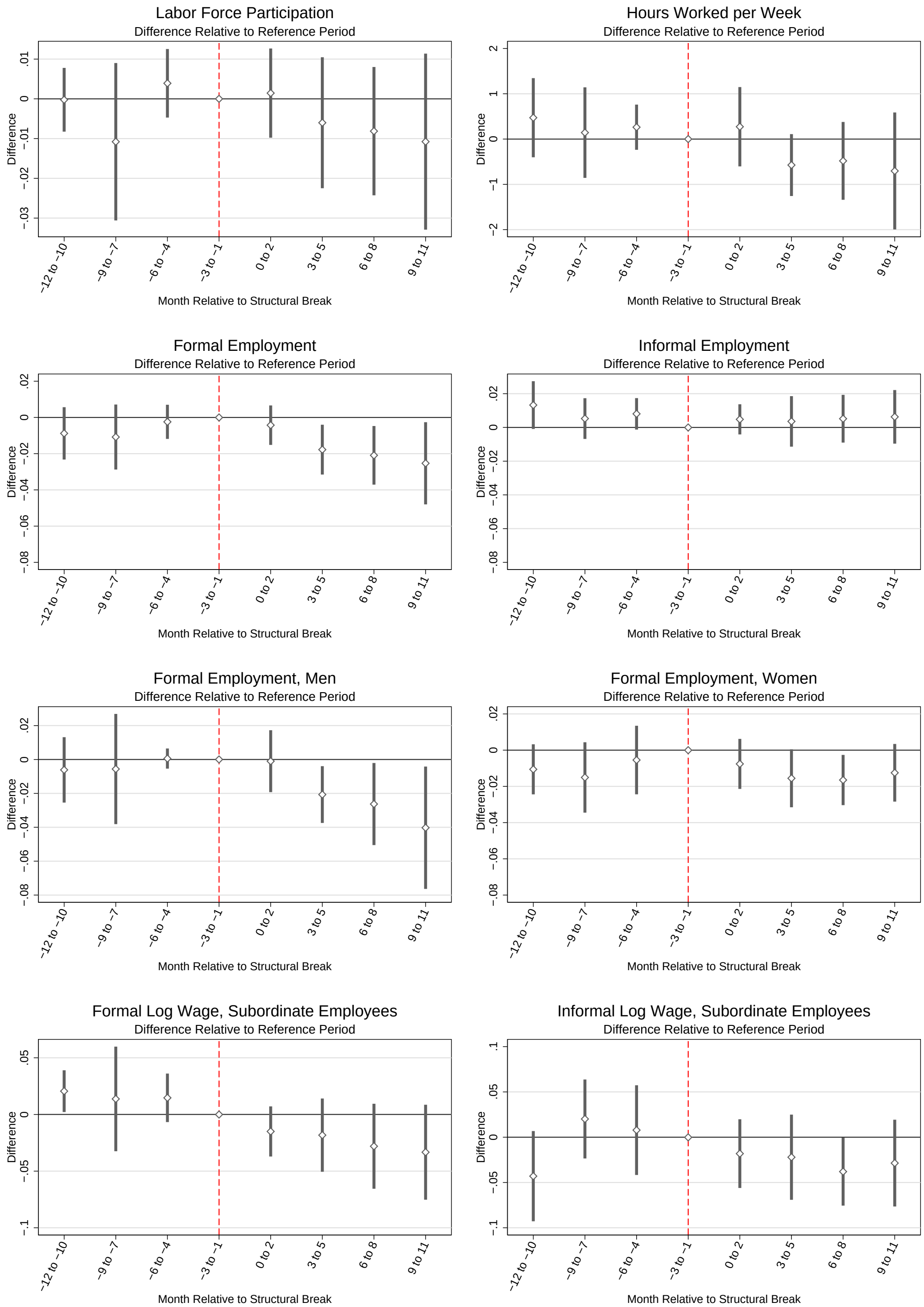
Notes: This figure illustrates that a synthetic control approach using individual-level data will tend to increase rates of attrition, but that attrition can be controlled. The figure on the left presents typical attrition in the Mexican industrial data. Given all establishments in the data as of January 2008, the black line plots cumulative attrition, which remains less than 20% even 6 years later. The dashed red line demonstrates that attrition in cities with large structural breaks was greater, but still less than 20% for almost the entire period. The figure on the right plots cumulative attrition under the synthetic control approach, analyzing how many months after a structural break a given matched case (i.e., the establishment in the large break city and all establishments within its synthetic control) remains in the data. Attrition rates are much higher in the right panel. However, by limiting the time period analyzed, and by constructing the synthetic control from a small number of high-quality matches, it is possible to reduce attrition. *Sources*: Based on analyses of the EMIM and ENEC; structural breaks identified based on municipality-level mortality statistics from INEGI/SINAIS.

Figure 5: Average outcomes among firms in structural break cities and their synthetic controls



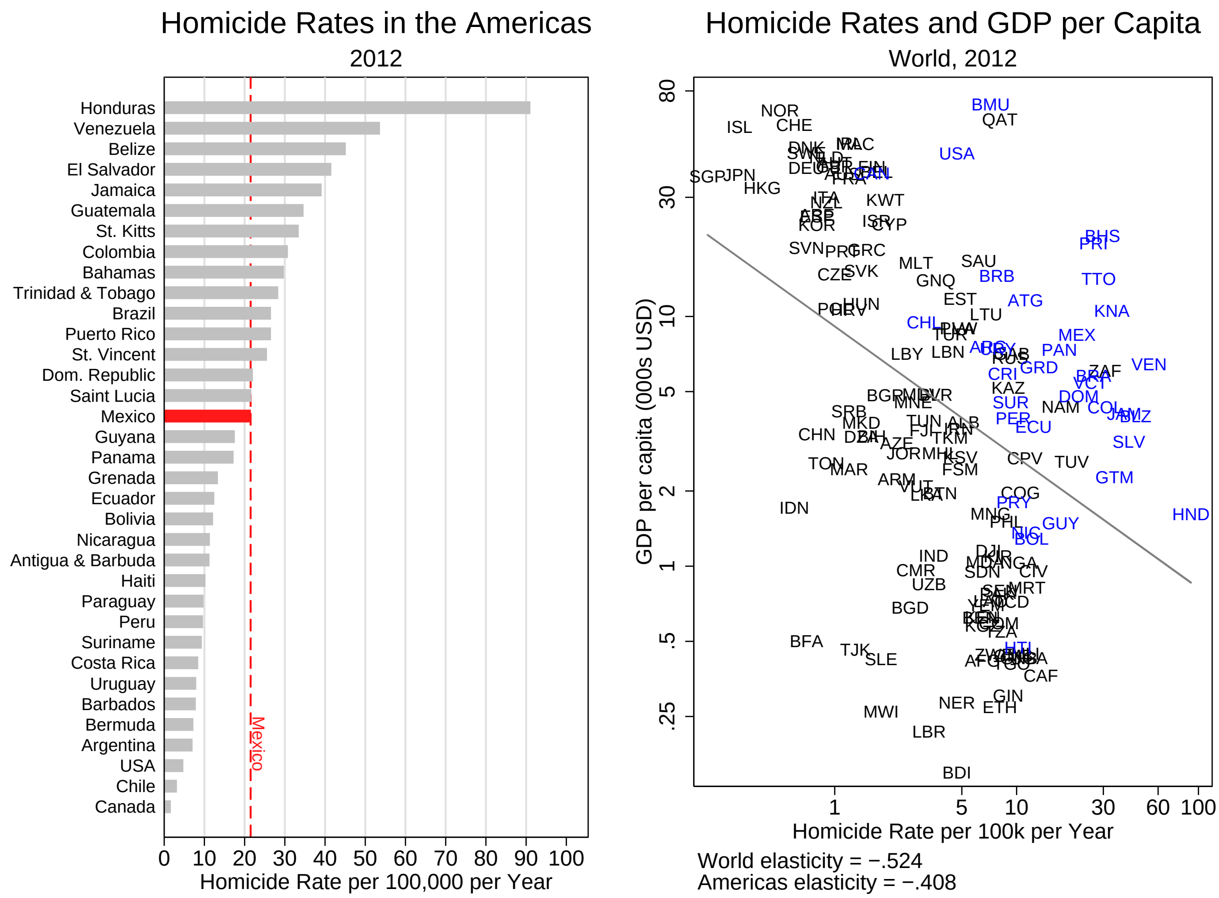
Notes: These graphs present the results of the synthetic controls exercise described in the text for selected outcomes. Twelve "large break" cities are identified based on estimation of structural breaks in their homicide rates. For each establishment in each large break city, and each dependent variable (excluding homicide rates), a group of 5 nearest neighbors is constructed from establishments in cities without structural breaks. A synthetic control for each establishment in each large break city is constructed from its respective neighbor group. The bottom decile of synthetic controls by pre-break mean squared error are dropped from the analysis, as are exact matches. For each month before and after the structural break in each city, average outcomes are computed for all establishments in the structural break cities and in their synthetic controls (left column); the difference between these two averages is presented in the right column. Inference based on the wild bootstrap procedure described in text is presented in Table 10. *Sources*: Based on analyses of the EMIM and ENEC surveys; structural breaks identified based on municipality-level mortality statistics from INEGI/SINAIS.

Figure 6: Local labor market outcomes



Notes: These figures report point estimates and pseudo-95% confidence intervals from event study regressions analyzing labor market outcomes before and after a structural break in homicide rates. In a dataset restricted to those cities that experienced structural breaks, I run regressions of the form $y_{ict} = \sum_{\tau} \beta_{\tau} D^{\tau}_{ct} + \eta X_{ict} + \mu_c + \lambda_t + f_c(t) + v_{ict}$, where the subscript i indicates individuals, c indexes cities, and t index time. The D^{τ}_{ct} are a series of event-time dummies equal to one when the structural break is τ months away in a given city, μ_c and λ_t are city and month fixed effects, and the control $f_c(t)$ is a city-specific linear time trend. The figures report the β coefficients. The reference period includes the three months prior to a structural break. Household-level population weights used in all regressions. Data include 12 cities with structural breaks. Clustering is by urban area using a wild bootstrap percentile-t procedure, imposing the null hypothesis of no effect. Standard errors and confidence intervals are constructed so that they would reproduce the computed p-value in a t-test with the appropriate degrees of freedom. *Sources*: Based on analyses of monthly labor market microdata from 2005 to 2013 from the ENOE; structural breaks identified based on municipality-level mortality statistics from INEGI/SINAIS.

Figure 7: Violence and GDP per capita around the world



Notes: Latin American countries highlighted in blue. Homicide rates from UNODC (2014) and GDP per capita figures from the World Bank Databank.

Table 1: Business victimization survey key questions

Neighborhood Conditions: Tell me if in the neighborhood of the establishment there is/are currently: 1) Gangs or violent groups; 2) Vandalism of establishments; 3) Property invasion; 4) Drug use; 5) Frequent theft or assaults of establishments; 6) Drug sales; 7) Prostitution; 8) Kidnappings; 9) Homicides; 10) Extortion by criminals; 11) Protection payments to criminals; 12) Extortion of establishments by authorities; 13) Other

Actions to Improve Security: During REFERENCE YEAR, in order to protect itself from crime, did the establishment take actions such as: 1) Changing doors or windows; 2) Changing or installing locks; 3) Installing bars or fences; 4) Purchasing safes or security rooms; 5) Installing alarms or security cameras; 6) Installing GPS locators; 7) Installing defenses against IT attacks; 8) Hiring guards or private security; 9) Creating an area within the establishment responsible for security; 10) Purchasing insurance; 11) Purchasing a guard dog; 12) Relocating the establishment; 13) Other

Victimization: During REFERENCE YEAR, did the establishment suffer directly situation X described on the card? 1) Total vehicle theft; 2) Theft of vehicle accessories, parts, or tools; 3) Theft of store merchandise while in transit; 4) Petty theft of store merchandise; 5) Major theft of store merchandise; 6) Other theft; 7) Delivery of products without payment (Fraud); 8) IT system attacks; 9) Threats and pressure of any form for money or goods; 10) Abduction of a business owner for money or goods; 11) Property damage

Business Impacts: During REFERENCE YEAR, as a result of the situations or crimes above, did you: 1) Cancel plans to grow your establishment (investment); 2) Stop marketing through or doing business with other businesses; 3) Stop managing cash on the premises of this establishment; 4) Reduce hours of production or marketing of goods and services; 5) Cancel distribution routes or sales of your products; 6) Did the owners stop visiting the establishment?

Table 2: Business victimization summary statistics

	Mean	SD	Min	Max
Population	947,019	2,387,568	80,560	19,834,376
Population (excluding D.F.)	675,195	757,289	80,560	4,572,929
Population (median)	440,848	.	440,848	440,848
Homicide Rate	25	31	1	196
Property Crimes Rate	282	188	10	1111
Extortion Rate	7	6	0	28
Violent Theft Rate	188	166	2	809
Business Theft Rate	89	68	1	404
Household Theft Rate	163	150	8	904
Violent Crimes Rate	367	217	20	1149
Global Summary Index	0.121	0.387	-0.810	1.277
Neighborhood: Index	-0.042	0.317	-0.818	0.695
Neighborhood: Gangs	0.396	0.152	0.000	0.885
Neighborhood: Vandalism	0.363	0.139	0.003	0.776
Neighborhood: Prpty Invasion	0.117	0.071	0.000	0.321
Neighborhood: Drug Use	0.441	0.146	0.044	0.895
Neighborhood: Robbery	0.505	0.152	0.079	0.903
Neighborhood: Drug Sales	0.297	0.125	0.058	0.774
Neighborhood: Prostitution	0.183	0.080	0.001	0.474
Neighborhood: Kidnapping	0.161	0.116	0.000	0.561
Neighborhood: Homicide	0.205	0.125	0.000	0.543
Neighborhood: Extortion	0.295	0.147	0.005	0.615
Neighborhood: Protection Payments	0.122	0.112	0.000	0.438
Neighborhood: Extortion by Auth.	0.083	0.066	0.000	0.343
Business: Index	-0.094	0.433	-1.701	0.587
Business: Less Investment	0.176	0.129	0.000	0.620
Business: Less with Others	0.107	0.114	0.000	0.911
Business: Stop Handling Cash	0.126	0.097	0.000	0.530
Business: Reduce Hours	0.202	0.158	0.000	0.860
Business: Cancel Distribution	0.083	0.105	0.000	0.917
Business: Owner Absent	0.072	0.082	0.000	0.424
Business: Other	0.025	0.055	0.000	0.433
Victimization: Index	0.224	0.229	-1.191	0.585
Victimization: Any crime	0.377	0.142	0.059	0.990
Victimization: Veh. Theft	0.056	0.056	0.000	0.356
Victimization: Veh. Parts	0.162	0.161	0.000	1.000
Victimization: Merch. in Transit	0.035	0.034	0.000	0.190
Victimization: Petty Theft	0.143	0.091	0.008	0.606
Victimization: Extortion	0.082	0.070	0.000	0.425
Victimization: Property Dmg	0.018	0.021	0.000	0.105
Institutions: Confidence Index	-0.015	0.304	-1.265	0.768
Institutions: Performance Index	0.033	0.320	-1.177	0.774
N	140			

Notes: Based on analyses of microdata from the ENVE 2012 and 2014, economic census 2009, and crime statistics from the SNSP. Datasets restricted to 80 urban areas defined by INEGI. Summary index variables in the ENVE constructed using individual-level data pooled across 2012 and 2014 surveys. Custom survey weights for aggregation of each year of the ENVE data to the urban area-level based on the economic census 2009; averages constructed only for those urban areas in which all census strata are represented in the survey, resulting in 140 urban area-years.

Table 3: Manufacturing and construction firms, 2007

	Mean	Median	SD	Min	Max
Industry-level averages					
Est per 2-digit industry (4)	2,171	2,186	723	1,284	3,028
Est per 3-digit industry (24)	362	353	187	46	740
Est per 4-digit industry (96)	90	65	98	4	633
Est per 6-digit industry (274)	32	21	37	1	272
Establishment-level averages					
Employees	277	99	627	1	13,588
Hours (000s)	648.1	232.0	1,439.8	0.1	31,828.0
Hours per Emp per Day	6.63	6.53	1.61	0.31	32.88
Revenue (USD 000s)	22,144	3,602	121,405	0	4,824,623
Wagebill (USD 000s)	2,508	623	7,360	0	334,224
Wage per Emp (USD 000s)	7.6	6.1	6.4	0.0	310.4
Labor Share of Rev	0.290	0.181	1.114	0.000	86.665
Revenue per Emp (USD 000s)	89.4	32.6	1,074.7	0.1	97,909.5
Emp/(000s Rev) Ratio	0.055	0.031	0.179	0.000	11.250
Emp/(000s Wage) Ratio	0.200	0.165	0.348	0.003	22.500
Establishment-level averages (Winsorized)					
Employees	259	99	456	2	2,948
Hours (000s)	605.4	232.0	1,048.2	4.9	6,634.0
Hours per Emp per Day	6.59	6.53	1.07	3.80	11.60
Revenue (USD 000s)	16,917	3,602	40,442	19	275,480
Wagebill (USD 000s)	2,263	623	4,521	6	28,739
Wage per Emp (USD 000s)	7.4	6.1	4.7	1.3	27.8
Labor Share of Rev	0.256	0.181	0.227	0.013	1.266
Revenue per Emp (USD 000s)	69.7	32.6	114.4	2.7	783.2
Emp/(000s Rev) Ratio	0.048	0.031	0.056	0.001	0.365
Emp/(000s Wage) Ratio	0.186	0.165	0.114	0.036	0.747
Establishment-level averages (log)					
Ln Employees	4.575	4.596	1.482	0.000	9.517
Ln Hours (000s)	5.434	5.447	1.483	-2.189	10.368
Ln Hours per Emp per Day	1.867	1.877	0.243	-1.181	3.493
Ln Revenue (USD 000s)	8.142	8.189	1.966	-0.762	15.389
Ln Wagebill (USD 000s)	6.412	6.434	1.783	-2.015	12.720
Ln Wage per Emp (USD 000s)	1.837	1.801	0.606	-3.114	5.738
Ln Labor Share of Rev	-1.730	-1.708	0.952	-8.853	4.462
Ln Revenue per Emp (USD 000s)	3.567	3.485	1.144	-2.420	11.492
Ln Emp/(000s Rev) Ratio	-3.567	-3.485	1.144	-11.492	2.420
Ln Emp/(000s Wage) Ratio	-1.837	-1.801	0.606	-5.738	3.114

Notes: Based on analyses of monthly, establishment-level surveys of Mexican manufacturing (EMIM) and construction (ENEC) firms across 80 defined urban areas. Surveys include the largest establishments by revenue at the national level until 6-digit industry-specific thresholds of national coverage are reached; the sample thus represents the largest firms in each industry in each city. Monthly data for 2007 are aggregated for each establishment, with ratios such as labor share of revenue computed based on total annual revenue and total annual wagebill. Values are Winsorized at the 1st and 99th percentiles.

Table 4: Comparison between surveyed industrial plants vs. census plants, 2008

Urban Area	Firms	Svy as Pct of Census: Firms	Svy as Pct of Census: Emp	Svy as Pct of Census: Rev	Emp per Plant: Svy	Emp per Plant: Cens	Rev per Emp: Svy	Rev per Emp: Cens
Acapulco, Gro	17	2.3	41.4	65.7	212	12	978	616
Chihuahua, Chih	144	10.4	64.3	65.1	346	56	717	708
Ciudad Victoria, Tamps	23	4.3	42.0	38.5	221	23	352	384
Culiacan Rosales, Sin	59	4.7	34.8	64.0	157	21	1,257	685
Heroica Nogales, Son	64	28.6	72.2	70.5	431	171	240	246
Jimenez, Chih	0	0	0.0	0.0	—	23	—	171
Juarez, Chih	275	21.7	80.5	76.6	674	182	303	319
Los Mochis, Sin	31	5.6	75.7	41.1	337	25	367	676
Mazatlan, Sin	29	5.1	49.9	68.9	219	22	1,268	919
Nuevo Laredo, Tamps	34	10	64.2	51.9	424	66	300	372
Tepic, Nay	30	3.3	26.1	37.7	117	15	835	579
Tijuana, BC	438	30.2	85.5	76.6	329	116	418	467
Torreon, Coah	224	12.8	69.4	89.1	238	44	2,127	1,655

Notes: This table shows that the industrial surveys capture a small percentage of total industrial census establishments in each city, but a large percentage of economic activity. By number of employees per establishments, surveyed establishments tend to be much larger than the average establishment in the census. In terms of revenue per employee, surveyed establishments are more comparable. Based on analyses of monthly, establishment-level surveys of Mexican manufacturing (EMIM) and construction (ENEC) firms, and economic census data for the year 2008, across 80 defined urban areas. The census data are limited to the same set of 6-digit industries covered by the survey data. The first column indicates the number of establishments in the survey. The third through fifth columns indicate what percentage of total census establishments, employees, and revenue in each city are captured by the survey. The sixth and seventh columns compare the average number of employees per establishment in the survey data versus in the census data. The final two columns compare the average revenue per employee in the survey data versus in the census data.

Table 5: Structural Breaks in Homicide Rates

	Urban Area	Pop (000s)	Firms	Break	ΔHR	$\Delta HR > 30$	$\Delta HR > 3\sigma$	$\Delta HR > 4\sigma$
1	Juarez, Chih	1,360	275	2008-03	136	1	1	1
2	Acapulco, Gro	718	17	2010-10	111	1	1	1
3	Chihuahua, Chih	787	144	2008-06	69	1	1	1
4	Mazatlan, Sin	416	29	2009-07	64	1	1	1
5	Tepic, Nay	348	30	2010-04	63	1	1	1
6	Heroica Nogales, Son	204	64	2008-03	60	1	1	1
7	Tijuana, BC	1,490	438	2008-09	57	1	1	1
8	Culiacan Rosales, Sin	807	59	2008-04	56	1	1	1
9	Los Mochis, Sin	400	31	2009-11	42	1	1	1
10	Torreon, Coah	1,050	224	2009-02	40	1	1	1
11	Ciudad Victoria, Tamps	304	23	2011-03	37	1	1	1
12	Jimenez, Chih	41	0	2009-12	75	1	1	0
13	Nuevo Laredo, Tamps	371	34	2010-07	44	1	0	0

ΔHR = increase in homicide rate

Notes: Based on analyses of homicide rates from 2005-2014 using municipality-level mortality statistics from INEGI/SINAIS and police statistics from SNSP, aggregated to the urban area-level. Structural breaks estimated using all months, but constrained to be no smaller than 15% of the sample time period. Breaks are considered statistically significant if p-values are less than .05 under all of the max, average, and exponential F-tests. Break magnitudes are calculated as the difference in average homicide rates during the 24 months after vs. before the identified break. The pre-break standard deviation of the annualized monthly homicide rate is computed using all months prior to the estimated structural break.

Table 6: Comparison of establishments in large break cities vs. synthetic controls, 2007

	Comparison	Large Breaks	Diff	Diff/SD
Log Homicide Rate	2.25	2.38	0.134***	0.414
Log Employment	4.94	4.94	-0.001	-0.000
Log Revenue	8.35	8.33	-0.024	-0.014
Log Hours	3.30	3.30	0.001	0.001
Log Wagebill	7.04	7.05	0.011	0.006
Ln Labor Share of Rev	-1.317	-1.289	0.028	0.034
Ln Wage per Employee	11.495	11.506	0.010	0.024
Ln Revenue per Employee	12.811	12.793	-0.018	-0.019
Labor Share of Rev	0.369	0.401	0.032***	0.118
Emp/(000s Wage) Ratio	0.011	0.011	0.000	0.008
Emp/(000s Rev) Ratio	0.004	0.004	0.000***	0.117
N	2246			

Notes: This table shows that synthetic controls constructed to replicate one dependent variable (log employment), lead to balance across most other characteristics. Differences in characteristics that are less well-balanced may controlled for in the regression specification. An observation is either an individual establishment observed in 2007, or its synthetic control. The third column reports the absolute difference in the given variable, as well as the statistical significance for a t-test across the two groups. The fourth column reports that standardized magnitude of the difference, i.e., the difference divided by the standard deviation. Of the variables listed, all are used in identifying 5 nearest neighbors except the final three: labor share of revenue (in level form), and the employee-to-wage and employee-to-revenue ratios. In this table, synthetic control weights are constructed to replicate log employment in monthly data. For each establishment, monthly values of revenue, hours, and wage payments are summed, while employment is averaged. Ratios are constructed at the establishment-level, logs are taken, outcomes are weighted to construct the synthetic controls, and finally outcomes are averaged within each group. * p < 0.1 ** p < .05 *** p < .01.

Table 7: In business victimization surveys, economic activity declines with violence, 2011-2013

	(1)	(2)	(3)	(4)	(5)
	BizIdx_{jsct}	BizHours_{jsct}	BizInvest_{jsct}	BizOwner_{jsct}	BizDistrib_{jsct}
LnHomRt_{ct}	-0.0533*	-0.0317***	-0.00289	-0.00881	0.00124
	(0.0306)	(0.00773)	(0.00713)	(0.00735)	(0.00535)
City, Year FE	Yes	Yes	Yes	Yes	Yes
4-digit X Year	Yes	Yes	Yes	Yes	Yes
FirmChar X Year	Yes	Yes	Yes	Yes	Yes
Crime	No	No	No	No	No
R-squared	0.106	0.117	0.0903	0.103	0.0808
Observations	15540	15327	15253	14600	12019
Clusters	77	77	77	77	77
MeanDepVar	-0.0230	-0.141	-0.134	-0.0807	-0.0837
LnHomRt X IQR	-0.0687	-0.0410	-0.00372	-0.0114	0.00159

Notes: Standard errors in parentheses clustered by urban area. This table shows that increases in homicide rates at the city-level ($LnHomRt_{ct}$) correlate with declines in self-reported business activity in the ENVE even after controlling for common time trends. An observation is an individual establishment in 2011 or 2013, based on a repeated cross-section. Dependent variables are indicated at the top of each column; see Table 1 for question phrasing. The summary index variable in the ENVE is constructed using individual-level data pooled across both years. Across all columns, a more positive value of the dependent variable indicates fewer adverse business impacts. That is, the binary dependent variables in columns 2-5 take a value of -1 if businesses reduced their hours of operations, or owners visited their establishments less, etc., or 0 otherwise. Point estimates are scaled by the inter-quartile range of corresponding independent variable below the table; for example, an increase in the log homicide rate of that magnitude would imply a 4.1 percentage point greater likelihood of reducing business hours. Production characteristics including firm size, labor productivity, and labor intensity of revenue are merged with the ENVE based on census averages for 2008 at the 6-digit industry by firm size category by city level (establishments in the ENVE are categorized as microenterprise, small, medium, or large). All regressions control for city-, year-, and industry-fixed effects at the 4-digit level, industry-specific time trends, and firm characteristic time trends. *Sources*: Based on analyses of microdata from the ENVE 2012 and 2014, economic census 2009, municipality-level mortality statistics from INEGI/SINAIS, and crime statistics from the SNSP. All datasets are restricted to as many as 80 urban areas defined by INEGI. * $p < 0.1$ ** $p < .05$ *** $p < .01$.

Table 8: Violence reduces economic activity independently of other crime, 2011-2013

	(1)	(2)	(3)	(4)	(5)
	BizIdx_{jsct}	BizHours_{jsct}	BizInvest_{jsct}	BizOwner_{jsct}	BizDistrib_{jsct}
LnHomRt_{ct}	**-0.0786****	**-0.0327*****	**-0.00936**	**-0.0149*****	**-0.00511**
	(0.0302)	(0.00751)	(0.00734)	(0.00514)	(0.00799)
LnTheftRt_{ct}	-0.0120	0.0252*	0.00555	-0.0143	-0.00749
	(0.0803)	(0.0138)	(0.0178)	(0.0153)	(0.0171)
$\text{LnPropCrimeRt}_{ct}$	-0.113	-0.0250	-0.0397*	-0.0391*	-0.0294
	(0.0810)	(0.0163)	(0.0213)	(0.0219)	(0.0202)
LnBizTheftRt_{ct}	-0.0985**	-0.0244***	-0.0249*	-0.00896	-0.0228***
	(0.0399)	(0.00736)	(0.0129)	(0.00725)	(0.00680)
$\text{VictimIndex}_{jsct}$	-0.213***	-0.0450***	-0.0542***	-0.0358***	-0.0513***
	(0.0144)	(0.00428)	(0.00474)	(0.00402)	(0.00352)
City, Year FE	Yes	Yes	Yes	Yes	Yes
4-digit X Year	Yes	Yes	Yes	Yes	Yes
FirmChar X Year	Yes	Yes	Yes	Yes	Yes
R-squared	0.189	0.160	0.143	0.140	0.131
Observations	13846	13677	13642	13034	10963
Clusters	77	77	77	77	77
MeanDepVar	-0.0275	-0.138	-0.134	-0.0840	-0.0871
LnHomRt X IQR	**-0.102**	**-0.0425**	**-0.0122**	**-0.0194**	**-0.00665**
LnBizTheft X IQR	-0.0933	-0.0231	-0.0236	-0.00887	-0.0212
VictimIdx X IQR	-0.250	-0.0526	-0.0633	-0.0397	-0.0600

Notes: Standard errors in parentheses clustered by urban area. This table shows that increases in homicide rates at the city-level ($LnHomRt_{ct}$) correlate with declines in self-reported business activity in the ENVE even after controlling for common time trends and other types of crime. An observation is an individual establishment in 2011 or 2013, based on a repeated cross-section. Dependent variables are indicated at the top of each column; see Table 1 for question phrasing. The summary index variable in the ENVE is constructed using individual-level data pooled across both years. Across all columns, a more positive value of the dependent variable indicates fewer adverse business impacts. That is, the binary dependent variables in columns 2-5 take a value of -1 if businesses reduced their hours of operations, or owners visited their establishments less, etc., or 0 otherwise. A more positive value of the victimization index implies less victimization. Point estimates are scaled by the inter-quartile range of corresponding independent variable below the table. For example, an increase in the log homicide rate of the magnitude of its inter-quartile range would imply a 4.2 percentage point greater likelihood of reducing business hours. Production characteristics including firm size, labor productivity, and labor intensity of revenue are merged with the ENVE based on census averages for 2008 at the 6-digit industry by firm size category by city level (establishments in the ENVE are categorized as microenterprise, small, medium, or large). All regressions control for city-, year-, and industry-fixed effects at the 4-digit level, industry-specific time trends, and firm characteristic time trends. *Sources*: Based on analyses of microdata from the ENVE 2012 and 2014, economic census 2009, municipality-level mortality statistics from INEGI/SINAIS, and crime statistics from the SNSP. All datasets are restricted to as many as 80 urban areas defined by INEGI. * p < 0.1 ** p < .05 *** p < .01.

Table 9: Economic activity declines with violence in panel regressions, 2007-2014

	(1) LnRev_{jsct}	(2) LnEmp_{jsct}	(3) LnHrs_{jsct}	(4) LnWagebill_{jsct}	(5) LnWageRt_{jsct}
$I(11 < \text{HomRt}_{ct} \leq 20)$	-0.000606	0.000998	-0.000749	-0.000783	-0.00162
	(0.00525)	(0.00351)	(0.00300)	(0.00340)	(0.00114)
$I(20 < \text{HomRt}_{ct} \leq 35)$	-0.00588	-0.00118	-0.00489	0.00262	0.00337
	(0.00717)	(0.00436)	(0.00417)	(0.00528)	(0.00225)
$I(35 < \text{HomRt}_{ct} \leq 47)$	-0.00910	-0.00648	-0.00981*	0.0000672	0.00591*
	(0.00876)	(0.00669)	(0.00513)	(0.00698)	(0.00313)
$I(47 < \text{HomRt}_{ct} \leq 63)$	-0.0119	-0.0151*	-0.0157**	-0.00752	0.00633
	(0.0110)	(0.00813)	(0.00643)	(0.00712)	(0.00398)
$I(63 < \text{HomRt}_{ct} \leq 116)$	-0.0210	-0.0220**	-0.0217***	-0.0119*	0.00959
	(0.0146)	(0.00936)	(0.00696)	(0.00636)	(0.00656)
$I(116 < \text{HomRt}_{ct} \leq 188)$	-0.0309**	-0.0217***	-0.0218***	-0.00619	0.0145**
	(0.0121)	(0.00795)	(0.00726)	(0.00762)	(0.00627)
$I(\text{HomRt}_{ct} > 188)$	-0.0666***	-0.0465***	-0.0389***	-0.0175**	0.0259***
	(0.0144)	(0.00998)	(0.00911)	(0.00808)	(0.00770)
Firm, Mth FE	Yes	Yes	Yes	Yes	Yes
NAICS4-mth FE	Yes	Yes	Yes	Yes	Yes
FirmChars-Mth FE	Yes	Yes	Yes	Yes	Yes
City-Linear	Yes	Yes	Yes	Yes	Yes
FirmChars-City-Linear	Yes	Yes	Yes	Yes	Yes
R-squared	0.917	0.955	0.950	0.957	0.849
Observations	662106	662106	662106	662106	662106
Clusters	78	78	78	78	78
Firms	8655	8655	8655	8655	8655

Notes: Standard errors in parentheses clustered by urban area. This table shows that months in which homicide rates are high relative to the average homicide rate for the city are correlated with less observed production activity in establishment surveys. Dependent variables are indicated at the top of each column, and an observation is an establishment-month. The omitted category includes city-months with annualized homicide rates between 0 and 10 per 100,000 population. The data range is from January 2007 to December 2014. All regressions include establishment and month fixed effects and industry-specific flexible time trends at the 4-digit level. Results are similar when controls are limited to these fixed effects; for robustness, the specifications here include flexible time trends interacted with establishment production characteristics observed during 2007 including: log employees, labor share of revenue, inverse wage per employee, and inverse revenue per employee. City-specific linear time trends, and city-specific linear time trends interacted with firm-specific characteristics, are also estimated. *Sources*: Based on analyses of producer microdata for manufacturing (EMIM) and construction (ENEC) establishments, and municipality-level mortality statistics from INEGI/SINAIS. All datasets are restricted to as many as 80 urban areas defined by INEGI. * $p < 0.1$ ** $p < .05$ *** $p < .01$.

Table 10: Economic activity declines after structural breaks in homicide rates, 2007-2014

	(1)	(2)	(3)	(4)	(5)
	LnHrs_{jsct}	LnEmp_{jsct}	LnRev_{jsct}	LnRevShrL_{jsct}	LnWageRt_{jsct}
Panel A: Five nearest neighbors with constant					
I(LargeBreak)xI(Post24)	**-.0386**	**-.0357**	**-.0251**	**-.00844**	**.00534**
Analytical, break city	[.0102]**	[.0144]**	[.0603]*	[.524]	[.31]
	(.0122)	(.0121)	(.0118)	(.0128)	(.005)
Wild, break city	[.0359]**	[.0519]*	[.108]	[.615]	[.391]
	(.0184)	(.0184)	(.0156)	(.0168)	(.00623)
Wild, origin city	[.028]**	[.0639]*	[.0879]*	[.585]	[.367]
	(.0176)	(.0193)	(.0147)	(.0155)	(.00593)
Observations	153,104	157,678	155,698	154,660	153,744
Origin cities	36	38	39	38	39
Panel B: Twenty nearest neighbors with constant					
I(LargeBreak)xI(Post24)	**-.0297**	**-.0282**	**-.0371**	**-.00501**	**-.002**
Analytical, break city	[.00679]***	[.0363]**	[.00132]***	[.487]	[.694]
	(.00874)	(.0117)	(.00843)	(.00695)	(.00494)
Wild, break city	[.0279]**	[.0798]*	[.016]**	[.595]	[.739]
	(.0135)	(.0161)	(.0154)	(.00943)	(.00599)
Wild, origin city	[.0319]**	[.104]	[.012]**	[.471]	[.699]
	(.0138)	(.0174)	(.0148)	(.00696)	(.00516)
Observations	138,646	143,802	142,544	140,780	140,160
Origin cities	46	42	46	45	44

Notes: Regressions based on a dataset containing the full time series for each establishment in each city experiencing a large structural breaks in its homicide rates, and the full time series for its synthetic control. The specification is

$$\begin{aligned} Y_{jct} &= \beta T_{ct} + \delta Z_{jc} \times T_{ct} + \gamma Z_{jc} + \eta Z_{jc} \times I\left(Post24\right)_{ct} + \mu_{jt} + \epsilon_{jct} \\ \tilde{Y}_{jct} &= \gamma \tilde{Z}_{jc} + \eta \tilde{Z}_{jc} \times I\left(Post24\right)_{ct} + \mu_{jt} + \tilde{\epsilon}_{jct} \end{aligned}$$

where $T_{ct} = I(LargeBreak) \times I\left(Post24\right)_{ct}$. Point estimates $\hat{\beta}$ are reported in bold, and indicate the average percentage change in the dependent variable during the 24 months after the event compared to the 12 months prior to the event. Analytical standard errors in parentheses and p-values in brackets, clustered by large structural break city. Next, p-values from a clustered wild bootstrap percentile-t procedure are reported, with the bootstrap clustered by large structural break city, and with t-stats computed using analytical standard errors clustered by large structural break city, and residuals generated under the null of no treatment effect. Finally, p-values from a similar clustered wild bootstrap procedure, but with the bootstrap clustered by the origin city of each establishment as described in text. For the wild boostrap procedures, standard errors in parentheses are constructed such that they would reproduce the reported p-values for that coefficient in a Wald test with standard normal critical values. * p < .1, ** p< .05, *** p < .01

Table 11: Business victimization and private security do not increase with homicide rates, 2011-2013

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	VctIdx_{jsct}	VctAny_{jsct}	VPettyThft_{jsct}	VExtrt_{jsct}	ActIdx_{jsct}	ActGuards_{jsct}	ActAlarms_{jsct}	ActInsure_{jsct}	ActMoved_{jsct}
LnHomRt_{ct}	0.00817	0.00550	0.00245	0.00580	-0.00724	-0.00377	0.00347	-0.00688	0.000660
	(0.0177)	(0.0128)	(0.00730)	(0.0105)	(0.0372)	(0.00893)	(0.00862)	(0.0137)	(0.00142)
City,Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
4digit FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.123	0.120	0.202	0.0428	0.183	0.171	0.172	0.107	0.0190
Observations	32903	32903	32834	32806	28204	27079	27593	27643	27260
Clusters	72	72	72	72	72	72	72	72	72
MeanDepVar	0.0953	0.460	0.198	0.0890	0.00511	0.170	0.292	0.122	0.00770
LnHomRtXIQR	0.0102	0.00686	0.00305	0.00724	-0.00904	-0.00471	0.00433	-0.00859	0.000824

Notes: Standard errors in parentheses clustered by urban area. This table shows that after controlling for common time trends, changes in homicide rates at the city-level ($LnHomRt_{ct}$) between 2011 and 2013 were not strongly correlated with self-reported victimization by economic crimes (columns 1-4) or self-reported adoption of the private security measures (columns 5-9) in the ENVE. Dependent variables are indicated at the top of each column; see Table 1 for question phrasing. An observation is an individual establishment in 2011 or 2013, based on a repeated cross-section. Summary index variables in the ENVE constructed using individual-level data pooled across 2012 and 2014 surveys. In column 1, less victimization implies a more positive index value. In column 7, more actions to protect the establishment imply a more positive value. All other dependent variables are binary variables. Point estimates are scaled by the inter-quartile range of corresponding independent variable below the table; for example, an increase in the log homicide rate of this magnitude would imply a 0.7 percentage point increase in the likelihood of some form of victimization. Production characteristics are merged with the ENVE based on census averages for 2008 at the 6-digit industry by firm size category by city level (establishments in the ENVE are categorized as microenterprise, small, medium, or large). All regressions control for city-, year-, and industry-fixed effects at the 4-digit level. *Sources*: Based on analyses of microdata from the ENVE 2012 and 2014, economic census 2009, and municipality-level mortality statistics from INEGI/SINAIS. All datasets are restricted to at most 80 urban areas defined by INEGI. * $p < 0.1$ ** $p < .05$ *** $p < .01$.

Table 12: Correlations between homicide rates and other crimes in official statistics, 2005-2014

	$\text{Ln(PropCrimeRt)}_{ct}$	Ln(TheftRt)_{ct}	$\text{Ln(BizTheftRt)}_{ct}$	Ln(AbductRt)_{ct}
	Panel A: Monthly, city-level data, 2011-2014			
LnHomRt_{ct}	-0.00189	0.00404	-0.0000405	0.00147
	(0.0143)	(0.0102)	(0.0112)	(0.0287)
N	3679	3693	3594	1333
	Panel B: Monthly, state-level data, 2011-2014			
LnHomRt_{ct}	-0.0230	0.00613	0.0296	0.0855
	(0.0185)	(0.0184)	(0.0384)	(0.0751)
N	1536	1536	1529	1094
	Panel C: Monthly, state-level data, 2005-2014			
LnHomRt_{ct}	-0.0340*	0.0461*	0.0485	0.174**
	(0.0207)	(0.0252)	(0.0315)	(0.0693)
N	3828	3840	3812	2245

Notes: Standard errors in parentheses clustered by urban area. This table uses official crime statistics to test for a correlation between changes in homicide rates and other types of crime at the city- and state-levels. It shows that during the time span covered by the ENVE, 2011-2013, after controlling for common time trends, there was not a significant relationship between homicide rates and major categories of economic crimes. Over the period that included homicide spikes, the strongest correlation is with abduction rates. Dependent variables are indicated at the top of each column. An observation is a city-month in Panel A, and a state-month in Panels B and C. All regressions include city- (or state-) fixed effects, month-fixed effects, and city- (or state-)specific linear time trends. The state-level regression spanning 2005 to 2014 includes a state-specific quadratic time trend. The p-values in Panel C are constructed using a wild bootstrap procedure imposing the null hypothesis of no effect, with standard errors reported that would reproduce the resulting p-values in a Wald test. *Sources*: Based on analyses of municipality-level mortality statistics from INEGI/SINAIS and crime statistics from the SNSP. All datasets are restricted to at most 80 urban areas defined by INEGI. * p < 0.1 ** p < .05 *** p < .01.

Table 13: Heterogeneity by sector in the business victimization survey, 2011-2013

	Pooled	Industry	Commerce	Services
	Panel A: Dependent variable is "BizIdx"			
LnHomRt$_{ct}$	**-0.0786****	**-0.00268**	**-0.0657****	**-0.149*****
	(0.0302)	(0.0484)	(0.0305)	(0.0500)
R-squared	0.189	0.217	0.174	0.222
Observations	13846	2757	6395	4689
MeanDepVar	-0.0275	-0.0428	0.00743	-0.0664
LnHomRt X IQR	**-0.102**	**-0.00355**	**-0.0848**	**-0.192**
	Panel B: Dependent variable is "BizHrs"			
LnHomRt$_{ct}$	**-0.0327*****	**-0.0250***	**-0.0258*****	**-0.0474*****
	(0.00751)	(0.0149)	(0.00936)	(0.0132)
R-squared	0.160	0.184	0.162	0.177
Observations	13677	2723	6344	4605
MeanDepVar	-0.138	-0.105	-0.148	-0.142
LnHomRt X IQR	**-0.0425**	**-0.0331**	**-0.0332**	**-0.0611**
City, Year FE	Yes	Yes	Yes	Yes
4-digit X Year	Yes	Yes	Yes	Yes
FirmChar X Year	Yes	Yes	Yes	Yes
Crime	Yes	Yes	Yes	Yes

Notes: Standard errors in parentheses clustered by urban area. This table shows that increases in homicide rates at the city-level ($LnHomRt_{ct}$) have a greater impact on self-reported business activity among retail and services establishments than an on industrial establishments. An observation is an individual establishment in 2011 or 2013, based on a repeated cross-section. Dependent variables are indicated for each panel; see Table 1 for question phrasing. The summary index variable in the ENVE is constructed using individual-level data pooled across both years. Across all columns, a more positive value of the dependent variable indicates fewer adverse business impacts. That is, the binary dependent variables in Panel B takes a value of -1 if businesses reduced their hours of operations, or 0 otherwise. Point estimates are scaled by the inter-quartile range of corresponding independent variable at the bottom of each panel. For example, an increase in the log homicide rate of the magnitude of its inter-quartile range would imply a 4.2 percentage point greater likelihood of reducing business hours. Production characteristics including firm size, labor productivity, and labor intensity of revenue are merged with the ENVE based on census averages for 2008 at the 6-digit industry by firm size category by city level (establishments in the ENVE are categorized as microenterprise, small, medium, or large). All regressions control for city-, year-, and industry-fixed effects at the 4-digit level, industry-specific time trends, and firm characteristic time trends. Controls for other forms of crime at the city-by-year level include log business theft rates, overall theft rates, and property crime rates. *Sources*: Based on analyses of microdata from the ENVE 2012 and 2014, economic census 2009, municipality-level mortality statistics from INEGI/SINAIS, and crime statistics from the SNSP. All datasets are restricted to as many as 80 urban areas defined by INEGI. * p < 0.1 ** p < .05 *** p < .01.

Table 14: Heterogeneity by firm characteristics in the business victimization survey, 2011-2013

	(1)	(2)	(3)	(4)	(5)
	BizIdx_{jsct}	BizHours_{jsct}	BizInvest_{jsct}	BizOwner_{jsct}	BizDistrib_{jsct}
LnHomRt_{ct}	-0.147***	-0.0543***	-0.0287**	-0.0283***	-0.0178**
	(0.0340)	(0.0105)	(0.0119)	(0.00722)	(0.00875)
x Ln Avg Emp$_{jsc}$	**0.0242*****	**0.00710*****	**0.00729*****	**0.00473*****	**0.00407****
	(0.00612)	(0.00172)	(0.00263)	(0.00172)	(0.00173)
x Inv Wage_{jsc}	0.0707*	-0.00621	0.00810	0.0626***	0.0402**
	(0.0383)	(0.0140)	(0.0186)	(0.0171)	(0.0177)
x Avg Labor Share_{jsc}	-0.0291	0.0101	-0.0102	-0.0255*	-0.0131
	(0.0521)	(0.0149)	(0.0169)	(0.0130)	(0.0166)
City, Year FE	Yes	Yes	Yes	Yes	Yes
4-digit X Year	Yes	Yes	Yes	Yes	Yes
FirmChar X Year	Yes	Yes	Yes	Yes	Yes
Crime	Yes	Yes	Yes	Yes	Yes
R-squared	0.190	0.161	0.143	0.141	0.132
Observations	13846	13677	13642	13034	10963
Clusters	77	77	77	77	77
MeanDepVar	-0.0275	-0.138	-0.134	-0.0840	-0.0871
IQR(LnHomRt)	1.301	1.301	1.301	1.301	1.302
LnAvgEmp(dH-dL)	**0.113**	**0.0333**	**0.0342**	**0.0216**	**0.0197**
InvAvWage(dH-dL)	0.00529	-0.000466	0.000593	0.00496	0.00225
AvRevShr(dH-dL)	-0.00733	0.00250	-0.00256	-0.00633	-0.00297

Notes: Standard errors in parentheses clustered by urban area. This table tests for heterogeneity by firm characteristics, and shows that when point estimates are scaled by ranges of the relevant independent variables, only heterogeneity by size is statistically significant and economically important. The positive coefficient on log average employment indicates that large firms are less affected across all outcome variables. Dependent variables are indicated at the top of each column; see Table 1 for question phrasing. An observation is an individual establishment in 2011 or 2013, based on a repeated cross-section. Summary index variables in the ENVE constructed using individual-level data pooled across 2012 and 2014 surveys. Across all columns, a more positive value of the dependent variable indicates fewer adverse business impacts. That is, the binary dependent variables in columns 2-5 take a value of -1 if businesses reduced their hours of operations, or owners visited their establishments less, etc., or 0 otherwise. Point estimates are scaled below the table and report the value of $dH - dL$, where dH describes the change in business activity for an establishment at the 75th percentile of the given characteristic (e.g. size) experiencing a 2 standard deviation change in the log homicide rate, versus the change in business activity for an establishment at the 25th percentile of the same characteristic (denoted dL). Thus, the table shows that a large establishment is 3.3 percentage points less likely to report reducing business hours than a small establishment. Production characteristics are merged with the ENVE based on census averages for 2008 at the 6-digit industry by firm size category by city level (establishments in the ENVE are categorized as microenterprise, small, medium, or large). All regressions control for city-fixed and an industry-by-year flexible time trend at the 4-digit level, as well as a flexible time trend interacted with all characteristics being tested. *Sources*: Based on analyses of microdata from the ENVE 2012 and 2014, economic census 2009, municipality-level mortality statistics from INEGI/SINAIS , and crime statistics from the SNSP. All datasets are restricted to at most 80 urban areas defined by INEGI. * p < 0.1 ** p < .05 *** p < .01.

Table 15: Heterogeneous effects in the industrial production data, 2007-2014

	(1)	(2)	(3)	(4)	(5)	(6)
	LnHrs_{jsct}	LnHrs_{jsct}	LnHrs_{jsct}	LnHrs_{jsct}	LnHrs_{jsct}	LnHrs_{jsct}
Panel A: Five nearest neighbors with constant						
I(LargeBreak)xI(Post24)	-.0272		-.041***			
	(.0455)		(.0142)			
	[.551]		[.00399]			
x Log Revenue '07	-.00134	-.00423*				
	(.0105)	(.00238)				
	[.898]	[.0758]				
x Labor Share of Rev '07			.0149	-.0303		
			(.0354)	(.0469)		
			[.675]	[.519]		
Observations	153,104	153,104	153,104	153,104		
Origin cities	36	36	36	36		
Panel B: Twenty nearest neighbors with constant						
I(LargeBreak)xI(Post24)	-.00368		-.0534***		-.0488**	
	(.0358)		(.0185)		(.0202)	
	[.918]		[.00399]		[.016]	
x Log Revenue '07	-.00309	-.00348**				
	(.00798)	(.00162)				
	[.699]	[.0319]				
x Labor Share of Rev '07			.0653	-.0016		
			(.0406)	(.0173)		
			[.108]	[.926]		
x Inv Rev Prod of Labor '07					5.17	.193
					(3.25)	(1.35)
					[.112]	[.886]
Observations	138,646	138,646	138,646	138,646	138,646	138,646
Origin cities	46	46	46	46	46	46

Notes: This table tests for heterogeneity consistent with additive output shocks (heterogeneity by size), or with labor supply shocks (labor intensity of revenue and inverse revenue productivity of labor), and shows that in the industrial production data there is no evidence of heterogeneity along these characteristics. Regressions based on a dataset containing the full time series for each establishment in each city experiencing a large structural breaks in its homicide rates, and the full time series for its synthetic control. The specification is

$$\begin{aligned} Y_{jct} &= \beta T_{ct} + \delta Z_{jc} \times T_{ct} + \gamma Z_{jc} + \eta Z_{jc} \times I\left(Post24\right)_{ct} + \mu_{jt} + \epsilon_{jct} \\ \tilde{Y}_{jct} &= \gamma \tilde{Z}_{jc} + \eta \tilde{Z}_{jc} \times I\left(Post24\right)_{ct} + \mu_{jt} + \tilde{\epsilon}_{jct} \end{aligned}$$

where $T_{ct} = I(LargeBreak) \times I\left(Post24\right)_{ct}$. Point estimates $\hat{\beta}$ and $\hat{\eta}$ are reported. p-values in brackets are from a clustered wild bootstrap percentile-t procedure, with the bootstrap clustered by origin city as described in the text, with t-stats computed using analytical standard errors clustered by large structural break city, and residuals generated under the null of no treatment effect. Standard errors in parentheses are constructed such that they would reproduce the reported p-values for that coefficient in a Wald test with standard normal critical values. * p < .1, ** p< .05, *** p < .01

Table 16: Heterogeneity by firm characteristics and sector, 2011-2013

	Dependent Variable: BizHours$_{jsct}$			
	Pooled	Industry	Commerce	Services
LnHomRt$_{ct}$	-0.0543***	-0.0273	-0.0476***	-0.0711***
	(0.0105)	(0.0293)	(0.0134)	(0.0188)
x Ln Avg Emp$_{jsc}$	0.00710***	0.00303	0.00600**	0.00842**
	(0.00172)	(0.00390)	(0.00293)	(0.00384)
x Inv Wage$_{jsc}$	-0.00621	-0.144	-0.0228	0.00904
	(0.0140)	(0.155)	(0.0271)	(0.0101)
x Avg Labor Share$_{jsc}$	0.0101	-0.0200	0.0856	0.00442
	(0.0149)	(0.0413)	(0.0667)	(0.0233)
City, Year FE	Yes	Yes	Yes	Yes
4-digit X Year	Yes	Yes	Yes	Yes
FirmChar X Year	Yes	Yes	Yes	Yes
Crime	Yes	Yes	Yes	Yes
R-squared	0.161	0.185	0.163	0.177
Observations	13677	2723	6344	4605
Clusters	77	73	76	74
MeanDepVar	-0.138	-0.105	-0.148	-0.142
IQR(LnHomRt)	1.301	1.323	1.291	1.291
LnAvgEmp(dH-dL)	0.0333	0.0129	0.0254	0.0344
InvAvWage(dH-dL)	-0.000466	-0.00356	-0.00277	0.000755
AvRevShr(dH-dL)	0.00250	-0.00410	0.00951	0.00179

Notes: Standard errors in parentheses clustered by urban area. This table tests for heterogeneity by firm characteristics, across sectors. The dependent variables is BizHours, indicating whether or not the establishment reduced production hours in response to insecurity; see Table 1 for question phrasing. An observation is an individual establishment in 2011 or 2013, based on a repeated cross-section. A more positive value of the dependent variable indicates fewer adverse business impacts. That is, the binary dependent variable takes a value of -1 if businesses reduced their hours of operations, or 0 otherwise. Point estimates are scaled below the table and report the value of $dH - dL$, where dH describes the change in business activity for an establishment at the 75th percentile of the given characteristic (e.g. size) experiencing a 2 standard deviation change in the log homicide rate, versus the change in business activity for an establishment at the 25th percentile of the same characteristic (denoted dL). Thus, the table shows that a large establishment in the pooled sample is 3.3 percentage points less likely to report reducing business hours than a small establishment. Production characteristics are merged with the ENVE based on census averages for 2008 at the 6-digit industry by firm size category by city level (establishments in the ENVE are categorized as microenterprise, small, medium, or large). All regressions control for city-fixed and an industry-by-year flexible time trend at the 4-digit level, as well as a flexible time trend interacted with all characteristics being tested. *Sources*: Based on analyses of microdata from the ENVE 2012 and 2014, economic census 2009, municipality-level mortality statistics from INEGI/SINAIS , and crime statistics from the SNSP. All datasets are restricted to at most 80 urban areas defined by INEGI. * p < 0.1 ** p < .05 *** p < .01.