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Violence and Opportunity in Mexico: Essays in Development Economics

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Violence and Opportunity in Mexico: Essays in Development Economics

by

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Abstract

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This dissertation combines two empirical analyses related to the importance of security in Mexico. The first paper considers the impact of a major collapse in security by studying firms in large cities during outbreaks of violence. The second paper turns to rural areas and small-scale farmers, examining the effects of a land titling program that was designed to improve tenure security and increase agricultural productivity.

Chapter 1 shows that indeed high levels of violent crime have substantial negative effects on firm outcomes. 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. Beginning primarily in 2008, violence 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.

Chapter 2, which is based on joint work with Alain de Janvry and Elisabeth Sadoulet, shows that improving property rights through land certification leads to a particular pattern of migration and changes to the structure of local economies. We use the rollout of a large-scale land certification program in Mexico from 1993 to 2006 to study how rural reforms establishing secure property rights determine patterns of migration, urbanization, and structural transformation. Certification leads higher-skilled agricultural labor to migrate,

leaving behind economies less concentrated in agriculture, yet with no significant deterioration in wages. States' manufacturing capitals see corresponding gains in urban population and agricultural employment. Average wages increase significantly in manufacturing capitals, suggesting growth and demand effects that outweigh employment competition usually associated with immigration. Sectoral wages only rise significantly in services, indicating that imperfect substitutability of labor is empirically important to understanding structural transformation and internal migration. These results also imply that natives in non-tradeable sectors are the most likely beneficiaries of increased local demand under immigration.

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Chapter 1

Violence and economic disruption: Firm-level evidence from Mexico

1.1 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

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

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 that predated the onset of drug violence. As illustrated in Figure 1.1, 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

[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).

[3] Also see Figure 1.2, 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.

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 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

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

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 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 1.2, I provide background on the Mexican drug war. In Section 1.3, I present my theoretical framework. Section 1.4 describes my data, and Section 1.5 the identification strategy. Section 1.6 presents results and discussion, and Section 1.7 concludes.

1.2 Background and related literature

1.2.1 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] These homicides have been geographically concentrated, with individual urban areas seeing

[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.

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 1.1 provides a detailed view of homicide rates in selected cities since 1994. Figure 1.2 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 1.1 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 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

[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).

[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)

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.

1.2.2 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 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

[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.

[12] A model of heterogeneous firms as in Section 1.3 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.

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, 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

[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.

[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 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.

1.3 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]

1.3.1 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 $\sigma_s \to 1$,

[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.

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

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

$$\max_{\{X_{jsi} \geq 0\}_i} P_{js}\left(Q_{js}(\mathbf{X}_{js})\right) Q_{js}(\mathbf{X}_{js}) - \boldsymbol{\omega}_{js} \cdot \mathbf{X}_{js} \tag{1.1}$$

To ease notation in the following, let

$$\Phi_{js} = \begin{cases} \left(\sum_{i=1}^{I} \alpha_{si}^{\sigma_s} \omega_{jsi}^{1-\sigma_s}\right)^{\frac{1}{\sigma_s - 1}} & ,\ 0 < \sigma_s \neq 1 \\ \prod_{i=1}^{I} \left(\frac{\alpha_{si}}{\omega_{jsi}}\right)^{\alpha_{si}} & ,\ \sigma_s = 1 \end{cases} \tag{1.2}$$

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

$$Y_{js}^* = \theta_j^{\eta_s} A_j^{\eta_s/\mu_s} \left(\frac{\nu_s}{\mu_s}\right)^{\eta_s - 1} \Phi_{js}^{\eta_s - 1} \tag{1.3}$$

$$X_{jsm}^* = \theta_j^{\eta_s} A_j^{\eta_s/\mu_s} \left(\frac{\nu_s}{\mu_s}\right)^{\eta_s} \Phi_{js}^{\eta_s - \sigma_s} \left(\frac{\alpha_{sm}}{\omega_{jsm}}\right)^{\sigma_s} \tag{1.4}$$

$$P_{js}^* = \theta_j^{(1-\nu_s)\eta_s} A_j^{-\eta_s/\epsilon_s} \left(\frac{\nu_s}{\mu_s}\right)^{-\nu_s \eta_s / \epsilon_s} \Phi_{js}^{-\nu_s \eta_s / \epsilon_s} \tag{1.5}$$

$$\Pi_{js}^* = (1 - \frac{\nu_s}{\mu_s}) Y_{js}^* = \eta_s^{-1} Y_{js}^* \tag{1.6}$$

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

$$\Omega_{jsm}^* = \frac{\nu_s}{\mu_s} \alpha_{sm}^{\sigma_s} \omega_{jsm}^{1-\sigma_s} \Phi_{js}^{1-\sigma_s} \tag{1.7}$$

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 (1.3) and (1.4), 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.

1.3.2 Comparative statics

1.3.2.1 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

$$\frac{\partial Y^*_{js}/\partial \tau_A}{Y^*_{js}} = \frac{\partial X^*_{jsm}/\partial \tau_A}{X^*_{jsm}} = \frac{\partial \Pi^*_{js}/\partial \tau_A}{\Pi^*_{js}} \quad = \quad -\frac{\eta_s/\mu_s}{1-\tau_A} < 0 \tag{1.8}$$

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

$$\frac{\partial P^*_{js}/\partial \tau_A}{P^*_{js}} \quad = \quad \frac{\eta_s/\epsilon_s}{1-\tau_A} > 0 \tag{1.9}$$

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

$$\frac{\partial Y^*_{js}/\partial \tau_p}{Y^*_{js}} = \frac{\partial X^*_{jsm}/\partial \tau_p}{X^*_{jsm}} = \frac{\partial \Pi^*_{js}/\partial \tau_p}{\Pi^*_{js}} = -\frac{\eta_s}{1-\tau_p} < 0 \tag{1.10}$$

while under monopolistic behavior, we have the further prediction that

$$\frac{\partial P^*_{js}/\partial \tau_p}{P^*_{js}} = -\frac{\eta_s}{1-\tau_p}(1-\nu_s) \tag{1.11}$$

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

$$\log Y_{jsct} = a_1 T_{ct} + a_2 \log(\bar{Y}_j) T_{ct} + a_3 \bar{\Omega}_{jsm} + f_a(Z_{jsct}) + u_{jcst} \tag{1.12}$$

$$\log X^m_{jsct} = b^m_1 T_{ct} + b^m_2 \log(\bar{Y}_j) T_{ct} + b^m_3 \bar{\Omega}_{jsm} + f^m_b(Z_{jsct}) + v^m_{jcst} \tag{1.13}$$

$$\log \Omega^m_{jsct} = d^m_1 T_{ct} + d^m_2 \log(\bar{Y}_j) T_{ct} + d^m_3 \bar{\Omega}_{jsm} + f^m_d(Z_{jsct}) + w^m_{jcst} \tag{1.14}$$

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

$$\textbf{Prediction A1}: \quad a_1 = b_1^m < 0 \quad \forall m \tag{1.15}$$

$$\textbf{Prediction A2}: \quad a_2 = b_2^m = 0 \quad \forall m \tag{1.16}$$

$$\textbf{Prediction A3}: \quad a_3 = b_3^m = 0 \quad \forall m \tag{1.17}$$

$$\textbf{Prediction A4}: \quad d_1^m = d_2^m = d_3^m = 0 \quad \forall m \tag{1.18}$$

1.3.2.2 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

$$\frac{\partial Y^*_{js}/\partial t_A}{Y^*_{js}} = \frac{\partial X^*_{jsm}/\partial t_A}{X^*_{jsm}} = \frac{\partial \Pi^*_{js}/\partial t_A}{\Pi^*_{js}} = -\frac{\eta_s/\mu_s}{A_j - t_A} < 0 \tag{1.19}$$

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

$$\frac{\partial P^*_{js}/\partial t_A}{P^*_{js}} = \frac{\eta_s/\epsilon_s}{A_j - t_A} > 0 \tag{1.20}$$

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

$$\frac{\partial Y^*_{js}/\partial \tau_p}{Y^*_{js}} = \frac{\partial X^*_{jsm}/\partial \tau_p}{X^*_{jsm}} = \frac{\partial \Pi^*_{js}/\partial \tau_p}{\Pi^*_{js}} = -\frac{\eta_s}{\theta_j - t_\theta} < 0 \tag{1.21}$$

$$\frac{\partial P^*_{js}/\partial \tau_p}{P^*_{js}} = -\frac{\eta_s}{\theta_j - t_\theta}(1 - \nu_s) \tag{1.22}$$

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

$$\log Y_{jsct} = a_1 T_{ct} + a_2 \log(\bar{Y}_j) T_{ct} + a_3 \bar{\Omega}_{jsm} + f_a(Z_{jsct}) + u_{jcst} \tag{1.23}$$

$$\log X^m_{jsct} = b^m_1 T_{ct} + b^m_2 \log(\bar{Y}_j) T_{ct} + b^m_3 \bar{\Omega}_{jsm} + f^m_b(Z_{jsct}) + v^m_{jcst} \tag{1.24}$$

$$\log \Omega^m_{jsct} = d^m_1 T_{ct} + d^m_2 \log(\bar{Y}_j) T_{ct} + d^m_3 \bar{\Omega}_{jsm} + f^m_d(Z_{jsct}) + w^m_{jcst} \tag{1.25}$$

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

$$\textbf{Prediction B1}: \quad a_1 = b^m_1 < 0 \quad \forall m \tag{1.26}$$

$$\textbf{Prediction B2}: \quad a_2 = b^m_2 > 0 \quad \forall m \tag{1.27}$$

$$\textbf{Prediction B3}: \quad a_3 = b^m_3 = 0 \quad \forall m \tag{1.28}$$

$$\textbf{Prediction B4}: \quad d^m_1 = d^m_2 = d^m_3 = 0 \quad \forall m \tag{1.29}$$

1.3.2.3 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

$$\left.\frac{\partial \Omega^*_{jsn}/\partial \tau_m}{\Omega^*_{jsn}}\right|_{\tau_m = 0} = -(1 - \sigma_s)\frac{\mu_s}{\nu_s}\Omega^*_{jsm} + (1 - \sigma_s)\mathbb{I}(n = m) \tag{1.30}$$

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 (1.30) 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

$$\frac{\partial Y^*_{js}/\partial\tau_m}{Y^*_{js}} = \frac{\partial \Pi^*_{js}/\partial\tau_m}{\Pi^*_{js}} = -(\eta_s - 1)\frac{\mu_s}{\nu_s}\Omega^*_{jsm} = -\eta_s\Omega^*_{jsm} < 0 \tag{1.31}$$

$$\frac{\partial X^*_{jsn}/\partial\tau_m}{X^*_{jsn}} = -(\eta_s - \sigma_s)\frac{\mu_s}{\nu_s}\Omega^*_{jsm} - \frac{\sigma_s}{1+\tau_m}\mathbb{I}(n = m) \tag{1.32}$$

while under monopolistic behavior, we also have that

$$\frac{\partial P^*_{js}/\partial\tau_m}{P^*_{js}} = \eta_s\frac{\mu_s}{\epsilon_s}\frac{\Omega^*_{jsm}}{1+\tau_m} > 0 \tag{1.33}$$

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 (1.31) and (1.32) 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 (1.8) and (1.10), and equations (1.19) and (1.21), 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 (1.30) 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

$$\log Y_{jsct} = a_1 T_{ct} + a_2\bar{\Omega}_{jsm} + f_a(Z_{jsct}) + u_{jcst} \tag{1.34}$$

$$\log X^m_{jsct} = b^m_1 T_{ct} + b^m_2\bar{\Omega}_{jsm} + f^m_b(Z_{jsct}) + v^m_{jcst} \tag{1.35}$$

$$\log \Omega^m_{jsct} = d^m_1 T_{ct} + d^m_2\bar{\Omega}_{jsm} + f^m_d(Z_{jsct}) + w^m_{jcst} \tag{1.36}$$

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

$$\textbf{Prediction C} \quad 0 = a_1 \tag{1.37}$$

$$-\mathbb{E}_j\left[\eta_s \times \Delta\tau_m\right] = a_2 < 0 \tag{1.38}$$

$$-\mathbb{E}_j\left[(\eta_s - \sigma_s)\frac{\mu_s}{\nu_s} \times \Delta\tau_m\right] = b^m_2 < 0 \tag{1.39}$$

$$-\mathbb{E}_j\left[\sigma_s \times \Delta\tau_m\right] = b^m_1 < 0 \tag{1.40}$$

$$-\mathbb{E}_j\left[(1 - \sigma_s)\frac{\mu_s}{\nu_s} \times \Delta\tau_m\right] = d^m_2 < 0 \tag{1.41}$$

$$\mathbb{E}_j\left[(1 - \sigma_s) \times \Delta\tau_m\right] = d^m_1 > 0 \tag{1.42}$$

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.

1.4 Data

1.4.1 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 1.2, I generate custom weights. Observe that some cities (e.g., Mexico City) encompass portions 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 1.2 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

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

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.

1.4.2 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.

1.4.3 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.

1.5 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.

1.5.1 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.

1.5.2 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 1.3. The largest identified structural breaks, and the criteria under which I have selected the cities that I focus on below, is provided in Table 1.5.

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.

1.5.3 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.

1.5.3.1 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.

1.5.3.2 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 1.3. 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]

[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

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 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 1.4 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 1.6 presents the comparison of means between firms in cities with large structural breaks, and their synthetic controls.

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largest possible pool.

1.5.3.3 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.,

$$\begin{aligned} Y_{jct} &= \beta T_{ct} + \delta Z_{jc} \times T_{ct} + \gamma Z_{jc} + \eta Z_{jc} \times I\left[Post24\right]_{ct} + \mu_{jt} + \epsilon_{jct} \\ \tilde{Y}_{jct} &= \gamma \tilde{Z}_{jc} + \eta \tilde{Z}_{jc} \times I\left[Post24\right]_{ct} + \mu_{jt} + \tilde{\epsilon}_{jct} \end{aligned} \tag{1.43}$$

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.

1.5.4 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 (1.43). Construct the Wald statistic $w = \hat{\beta}/s_{\hat{\beta}}$, using analytical standard errors clustered by treatment city.

2. Re-estimate the regression in (1.43), 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 (1.43) 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.

1.6 Results

1.6.1 Average effects

1.6.1.1 Business victimization survey

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

$$y_{jsct} = \beta \log HomRt_{ct} + \gamma Z_{jsc} + \delta \log HomRt_{ct} \times Z_{jsc} + \eta X_{jsct} + \mu_c + \lambda_{st} + e_{jsct} \tag{1.44}$$

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}.

[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.

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 1.7, 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 1.8, 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 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.

[20] See Table 1.1 for the precise wording.

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.

1.6.1.2 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:

$$y_{jsct} = \sum_i \beta_i HomRt_{isct} + \mu_j + \lambda_{st} + \sum_k \eta_{kt} Z_{jksc} + f_c(t) + \tilde{e}_{jksct} \tag{1.45}$$

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 1.9. 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 1.5 provides visual evidence comparing firms in cities with large structural breaks against their synthetic controls for four variables: homicide

[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.

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 1.5 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 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 1.3 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 1.10 uses the regression framework in subsections 1.5.3 and 1.5.4 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

[22]As shown in Figure 1.4, 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.

correlational evidence in Table 1.9, in which I find significant increases in wages. Given the stronger identifying assumptions required in Table 1.9, I regard the synthetic control estimates as more credible. Thus, based on the model in Section 1.3.2.3, in the absence of any impacts on factor intensities in Table 1.10, we would fail to reject the null hypothesis that crime has no impact on factor prices.

1.6.1.3 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 the form

$$y_{ict} = \sum_{\tau} \beta_{\tau} D_{ct}^{\tau} + \eta X_{ict} + \mu_c + \lambda_t + f_c(t) + v_{ict} \tag{1.46}$$

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 1.6 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.

1.6.1.4 Summary

Across datasets and identification strategies, the results in Section 1.6.1 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 1.10, 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).

1.6.2 Channels and heterogeneous effects

While the results in Section 1.6.1 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 1.3.2.3 (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 1.3, 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 1.6.3.

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 1.2, 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 1.11, 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 1.12, 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 1.8 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 1.3.2.3, 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 1.14 and 1.15, I consider regressions motivated by the results in Section 1.3.2.3. In Table 1.14, 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 1.15, 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 1.14 and 1.15, 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 1.3, 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 1.14, 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 1.13, 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 1.3 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 1.14 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 1.15.

In the business victimization surveys, I focus on testing whether this heterogeneity by business size remains significant across major sectors (Table 1.16). 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 1.15, 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.

1.6.3 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.

1.6.3.1 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 1.14, it is intriguing that owners visit their establishments less when violence increases. This reduced owner attention and oversight would be consistent with productivity 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.

[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.

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.

1.6.3.2 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 1.10, 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.

1.7 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 1.7 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

[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).

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 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.

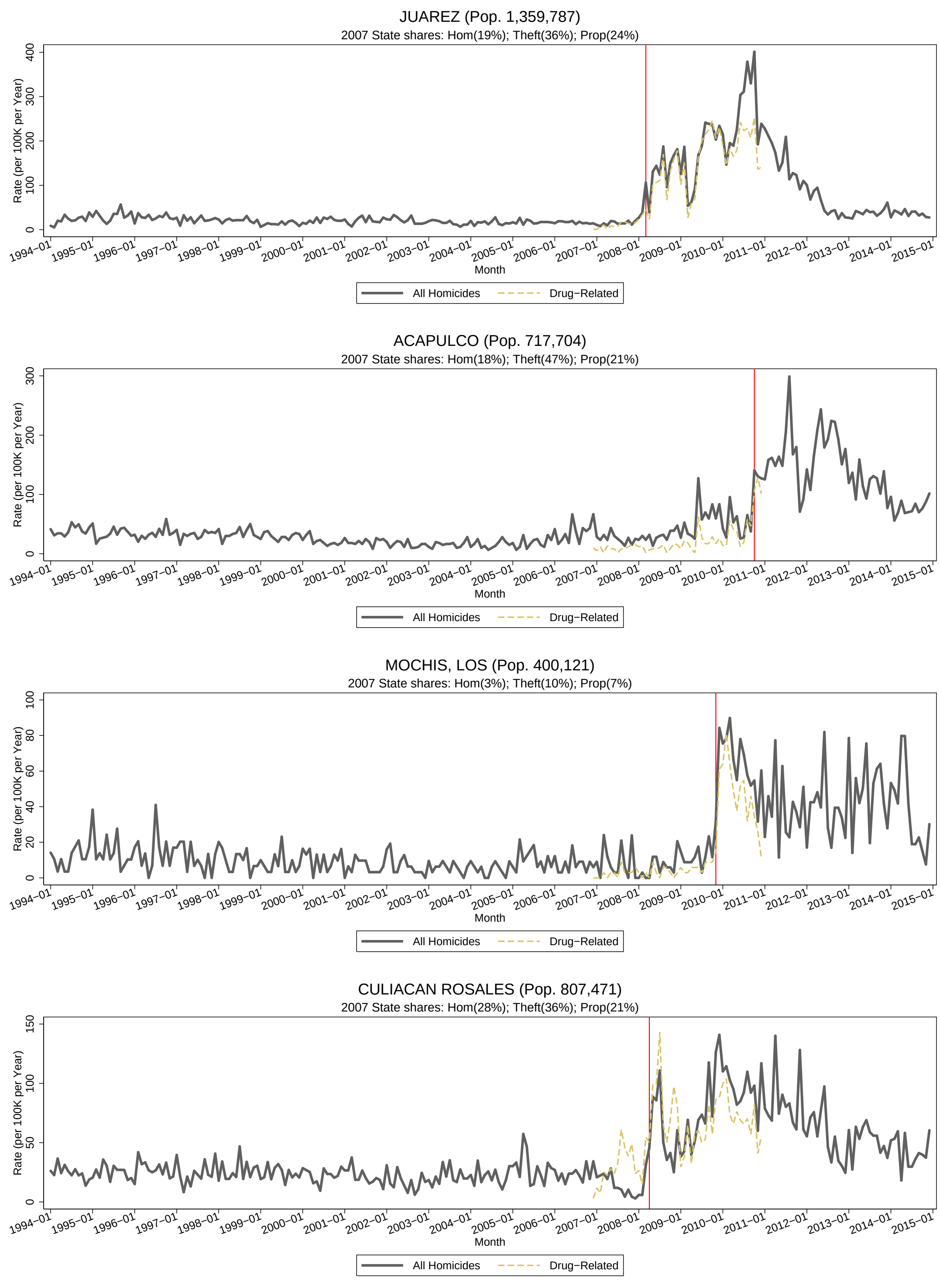
It is important to acknowledge the limitations of this work. Notably, while the business victimization survey contributes unique data and informs multiple findings, it was conducted during or after violence had peaked across the country and may not be representative of business reactions while violence 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.

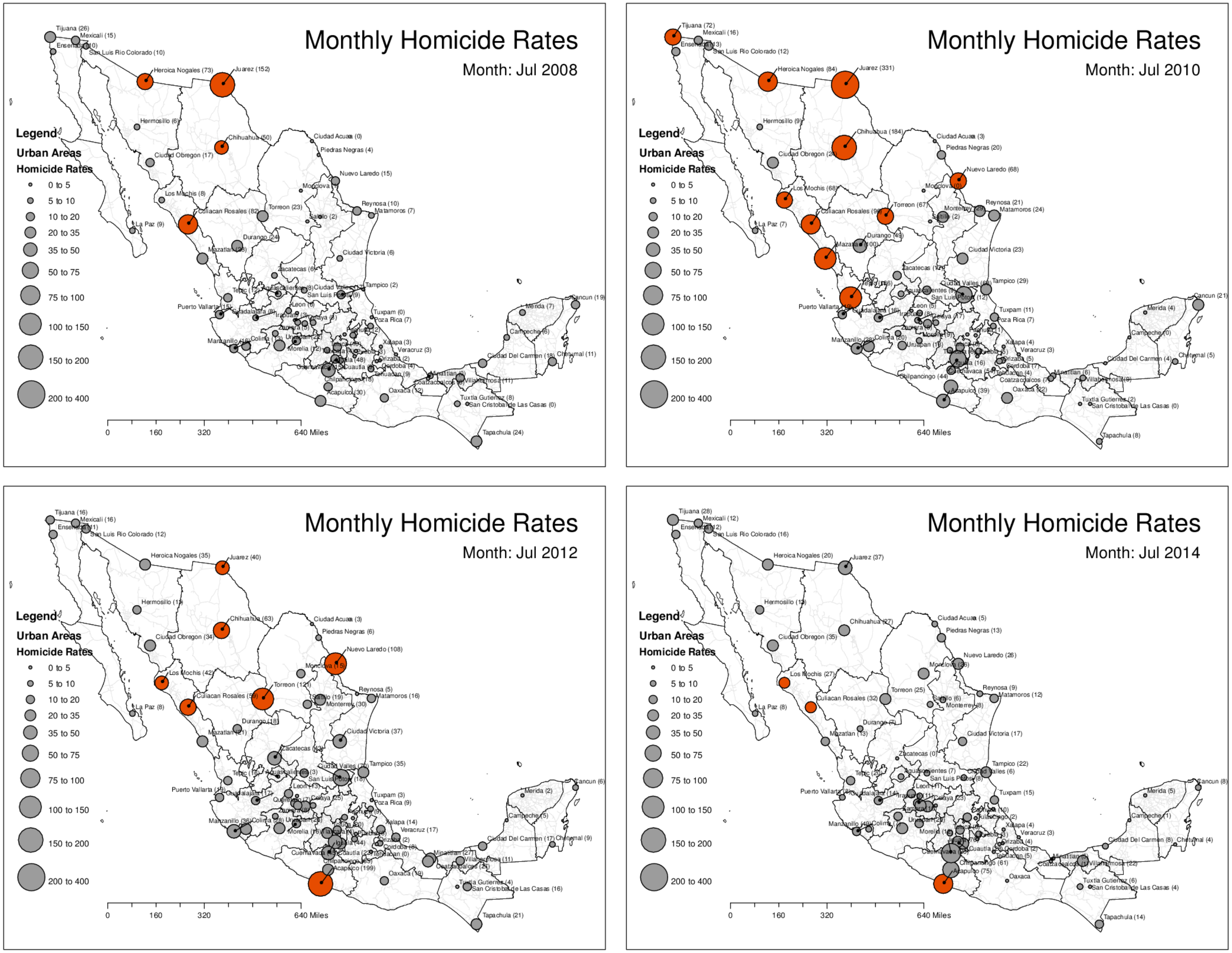
Figures and Tables

Figure 1.1: Homicide rates in selected cities



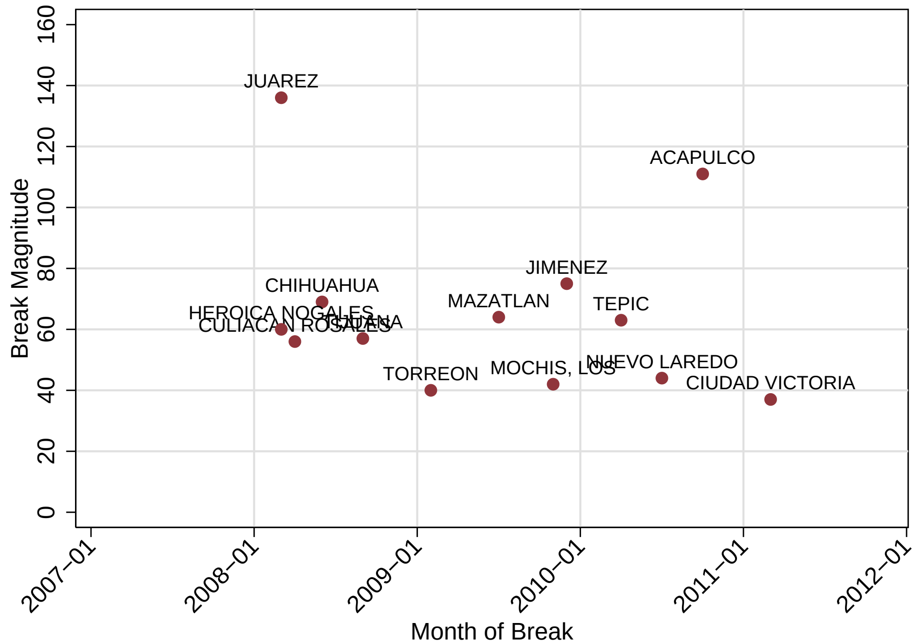
Notes: These figures illustrate the highly discontinuous nature of increases in crime in selected cities. Red lines indicate structural breaks estimated using all months from 2005 to 2013.

Figure 1.2: Homicide rates across Mexican cities, 2008-2014



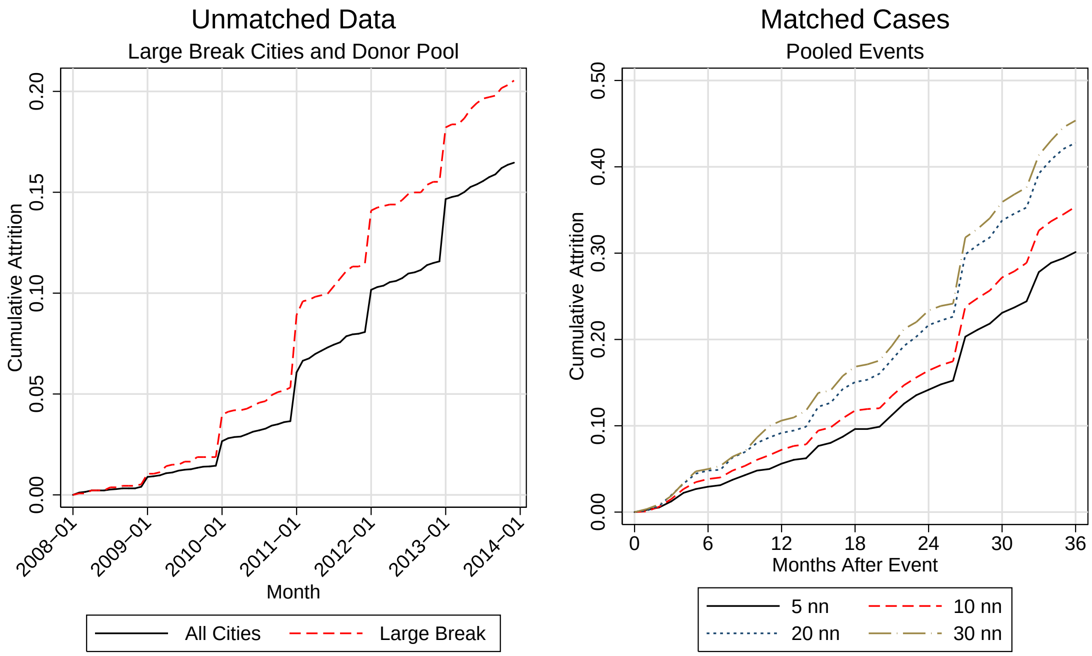
Notes: These maps illustrate the geographic progression of violence outbreaks across Mexico. Red circles indicate cities subsequent to an identified structural break.

Figure 1.3: Cities experiencing large structural breaks



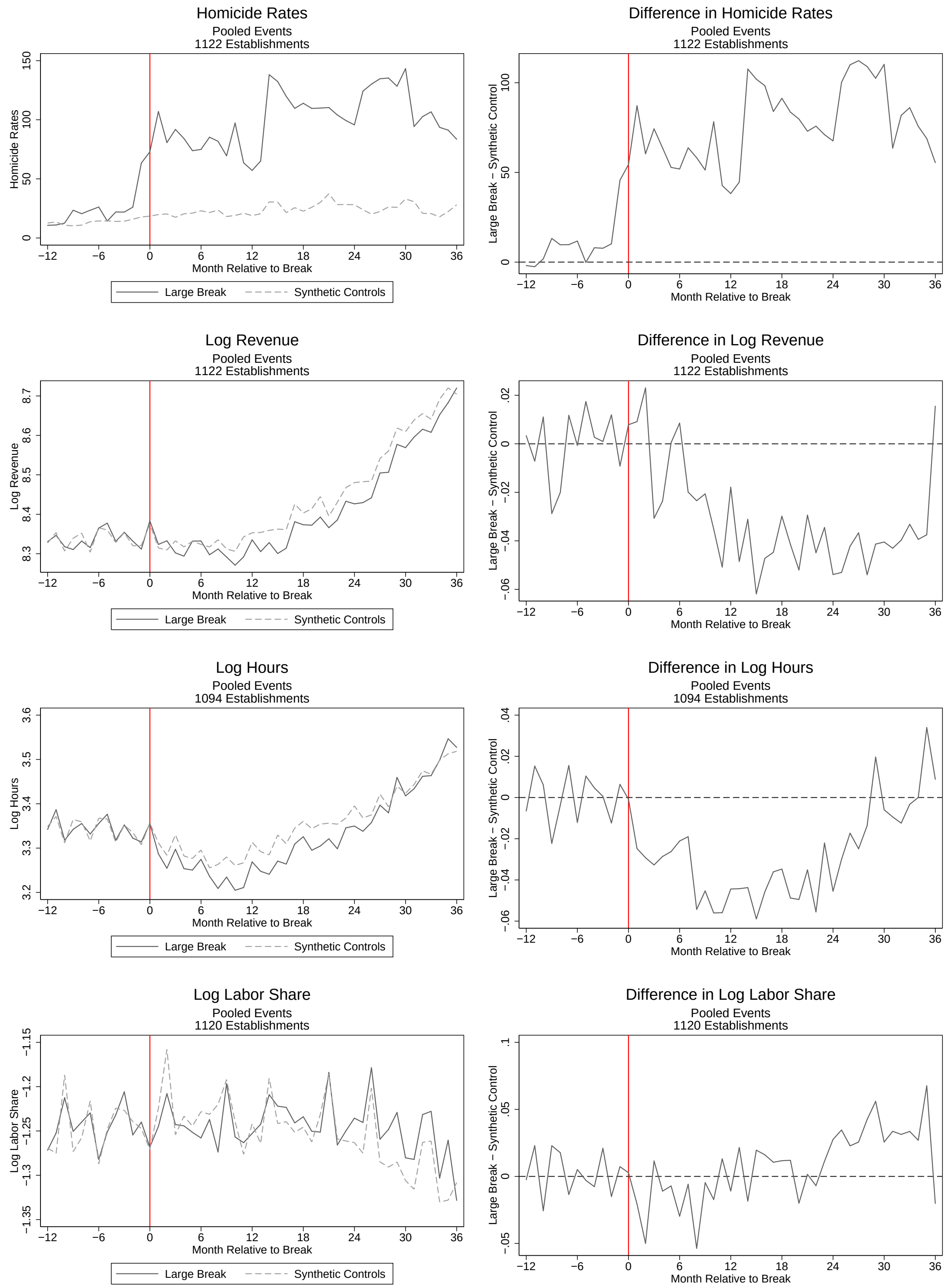
Notes: Based on analyses of homicide rates from 2005-2014 using municipality-level mortality statistics from INEGI/SINAIS and police statistics from SNSP, aggregated to the urban area-level. Structural breaks estimated using all months, but constrained to be no smaller than 15% of the sample time period. 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 1.4: Increased attrition under individual-level synthetic controls



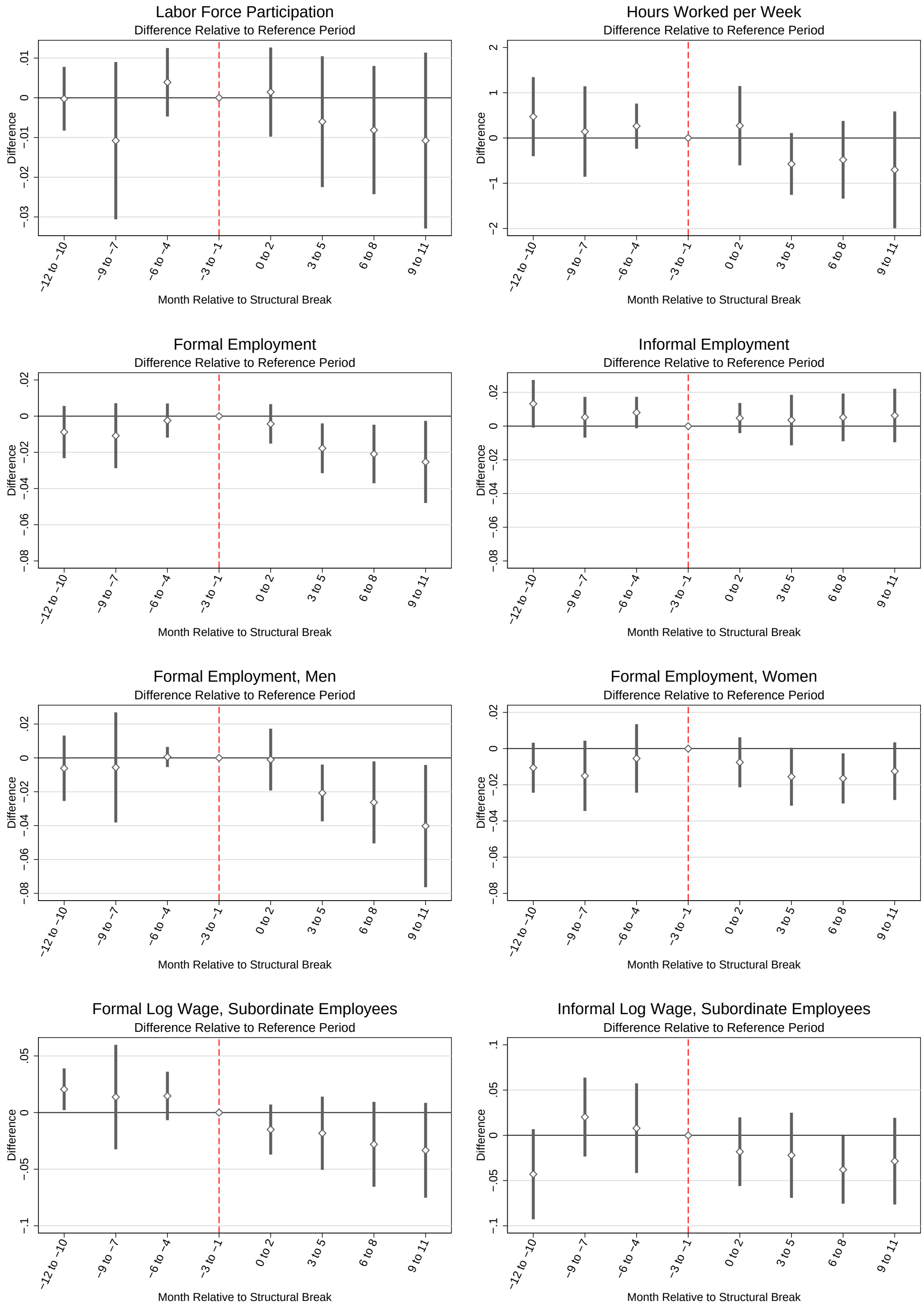
Notes: This figure illustrates that a synthetic control approach using individual-level data will tend to increase rates of attrition, but that attrition can be controlled. The figure on the left presents typical attrition in the Mexican industrial data. Given all establishments in the data as of January 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 1.5: Average outcomes among firms in structural break cities and their synthetic controls



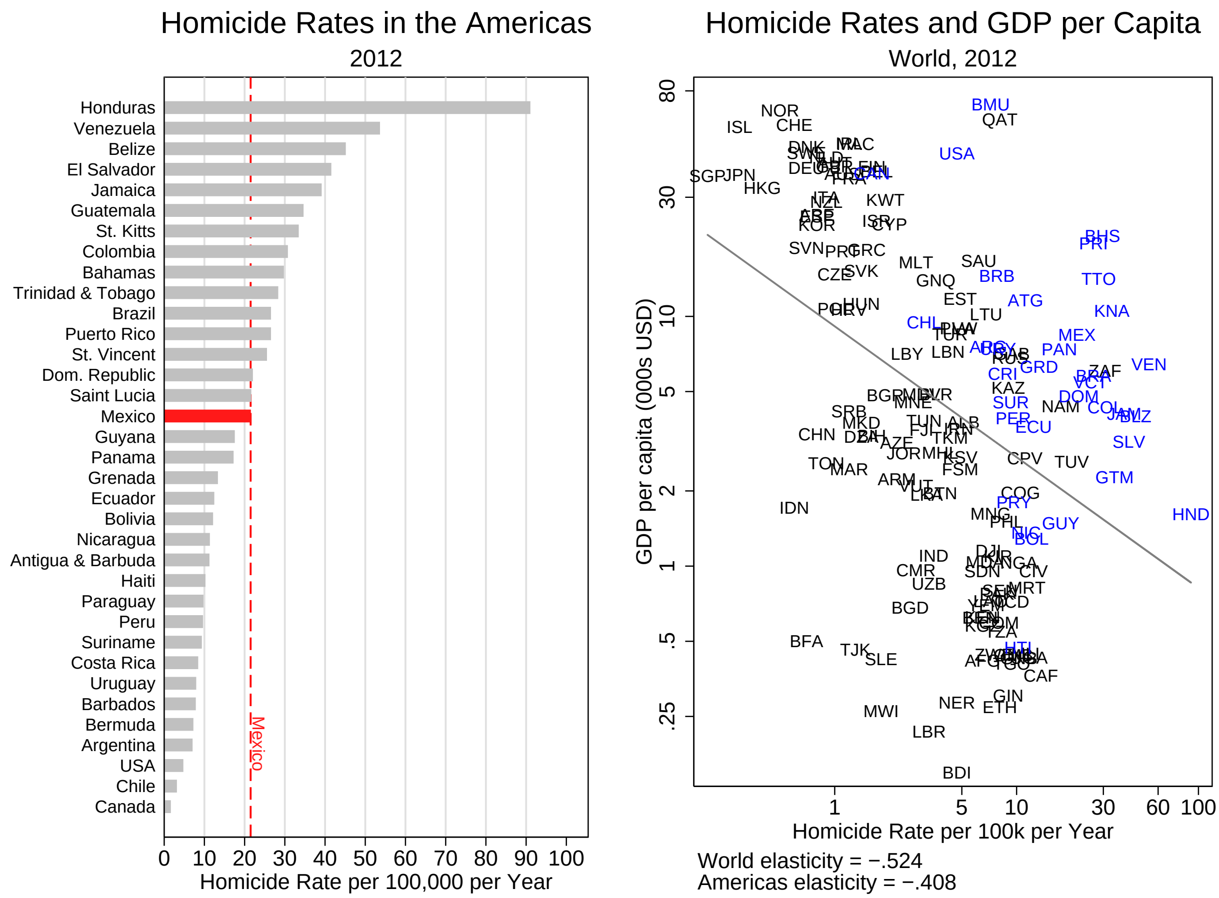
Notes: These graphs present the results of the synthetic controls exercise described in the text for selected outcomes. For each month before and after the structural break in each city, average outcomes are computed for all establishments in the structural break cities and in their synthetic controls (left column); the difference between these two averages is presented in the right column.

Figure 1.6: 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. The reference period includes the three months prior to a structural break. Household-level population weights used in all regressions. Clustering is by urban area using a wild bootstrap percentile-t procedure.

Figure 1.7: Violence and GDP per capita around the world



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

Table 1.1: 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 1.2: 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. 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 1.3: 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 1.4: Comparison between surveyed industrial plants vs. census plants, 2008

		Svy as Pct of Census			Emp per Plant		Rev per Emp	
Urban Area	Firms	Firms	Emp	Rev	Svy	Cens	Svy	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 1.5: 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 1.6: 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 1.7: 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 1.1 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 1.8: 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)
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)
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 1.1 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 1.9: Economic activity declines with violence in panel regressions, 2007-2014

	(1)	(2)	(3)	(4)	(5)
	LnRev_{jsct}	LnEmp_{jsct}	LnHrs_{jsct}	LnWagebill_{jsct}	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 1.10: 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. 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 1.11: Business victimization and private security do not increase with homicide rates, 2011-2013

	(1) VctIdx_{jsct}	(2) VctAny_{jsct}	(3) VPettyThft_{jsct}	(4) VExtrt_{jsct}	(5) ActIdx_{jsct}	(6) ActGuards_{jsct}	(7) ActAlarms_{jsct}	(8) ActInsure_{jsct}	(9) 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 1.1 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 1.12: 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 1.13: 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 1.1 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 1.14: Heterogeneity by firm characteristics in the business victimization survey, 2011-2013

	(1) BizIdx$_{jsct}$	(2) BizHours$_{jsct}$	(3) BizInvest$_{jsct}$	(4) BizOwner$_{jsct}$	(5) 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 1.1 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 1.15: 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. See Table 1.10 for additional notes. * p < .1, ** p< .05, *** p < .01

Table 1.16: 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 1.1 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.

Chapter 2

Rural reform, urbanization, and structural transformation in Mexico

2.1 Introduction

The sectoral and spatial transitions from agriculture into manufacturing and services, and from rural to urban areas, have long been considered central features of economic development (Clark 1940, Harris and Todaro 1970, Duarte and Restuccia 2010). A wide variety of patterns exist in the way these features may interact, depending on whether structural transformation is driven by technological progress and income effects, relative input or output prices, or other sources, and whether there is international trade, and internal goods and labor mobility (Herrendorf et al. 2014). Yet while substantial theoretical and empirical macroeconomic literatures have arisen, the microeconomic foundations of the structural transformation and its relation to rural-urban migration remain poorly understood (Foster and Rosenzweig 2008).

In this paper, we argue that rural reforms establishing secure property rights to agricultural land drive a particular pattern of structural and spatial transformation. We consider a regime in which access to agricultural land is contingent on both the owner's presence and his continued active use of the land. Because migration requires surrending this land without compensation, such policies restrict geographic labor mobility and lead to an inefficient allocation of labor to agriculture. A land titling program confers effective ownership, establishing secure property rights, enabling land sales, and relaxing the opportunity cost of migration (de Janvry et al. 2015). In this context, we argue that higher-skilled agricultural labor exploits the opportunity to migrate away from their origin municipalities, leaving behind economies less concentrated in agriculture, yet with no significant deterioration in wages. States' manufacturing capitals see corresponding gains in urban population and agricultural employment. Average wages increase significantly in our setting, which we attribute to growth and demand effects from immigrants with a preference for urban amenities that outweigh any employment competition. By sector, wages only rise significantly in services, confirming that imperfect substitutability of labor—across sectors, and/or between immigrants and natives—is empirically important to the process of structural transformation and internal migration. Finally, in such an environment, native employees in the non-tradeables sector are the most likely beneficiaries of increased local demand associated with immigration.

Our empirical evidence draws on Mexico's experience under agricultural land reform. In Mexico's first agricultural reform, from 1914–1992, agrarian communities called ejidos were created by expropriating large private landholdings and reallocating land to groups of peasant farmers. These lands were then managed by an ejido assembly, which granted community members use-based rights to cultivate individual agricultural plots, but no right to rent or sell agricultural land. The scale of this first reform was massive, and resulted in about half of Mexico's total land area and rural population (Dell 2012) living and working under a system of incomplete property rights. In anticipation of NAFTA, Mexico conducted a second agricultural reform in an effort to improve agricultural efficiency. This was a large-scale land certification program, called the Programa de Certificación de Derechos Ejidales y Titulación de Solares, or PROCEDE. The program was rolled out nationwide from 1993 to 2006 to issue certificates of ownership over ejido land, and was as large in scope as the first agricultural reform, with all but a small subset of ejidos certified by 2006.

We use Mexico's implementation of PROCEDE from 1993–2006 as a large-scale natural experiment to examine the impacts on migration and structural transformation that result in moving from use-based land rights to certificate-based land rights. We rely on a fixed effects specification that essentially compares changes in municipalities that had larger shares of their population in early-certified ejidos versus municipalities with smaller shares of their population in early-certified ejidos. In all specifications, we control for time-varying trends associated with the overall ejido share of the population, eliminating time-trending effects correlated with this observable difference in municipality composition. Moreover, when possible in our municipality-level specifications, we exploit only within-state variation across municipalities in early-certification shares, eliminating the concern that time-trending unobservables at the state-level might be simultaneously correlated with early-certification shares and our outcomes of interest. Thus, the main threat to identification is time-trending unobservables that vary differentially within a state across municipalities that have larger and smaller early-certification shares. We provide falsification tests suggesting that changes in migration over time were not correlated with the program's rollout.

In describing our results, we group outcomes according to "origin" or "net emitting" localities and municipalities versus "destination" or "net receiving" localities and municipalities. More specifically, we distinguish between outcomes within the manufacturing capitals of states as compared to the average municipality. The average municipality in our data became a net emitting municipality. Over 40% of its population was rural, with a substantial portion of its population attached to ejidos as PROCEDE was rolled out. As a result, the average municipality experienced significant outmigration as the labor constraints attached to ejido land use were relaxed. But these migrants had to move somewhere. To understand the implications of their movements for destination economies, we examine outcomes within the manufacturing capital of each state—defined as the single municipality in each state with the largest base of manufacturing employment in 1990. Typically, this will also be the population and services capitals of the state, and therefore represents access to all of the amenities, employment and consumption options typically afforded by an urban environment. In focusing on the entire municipality rather than its principal city, we allow for the possibility that migrants move to rural areas in the periphery of cities, rather than to the cities themselves. We indeed find that manufacturing capitals tend to gain population in response to PROCEDE. Thus, we describe manufacturing capitals as net receiving economies, and study the effects associated with immigration to this particular, important type of migrant destination.

Our first set of results relate to the effects of rural reform and outmigration on the average, net emitting municipality. The average municipality in our data is 43% rural in 1990. Almost 10% of its population consisted of ejidatarios that were eventually certified, with about 5% of its population certified prior to 2000.[1] From 1990 to 2000, the average municipality growth rate was about 9% per decade. The population growth rate was slower in municipalities with greater percentages of the population in ejidos—on average, 2% below trend per decade. But outmigration due to PROCEDE further reduced the population growth rate, with point estimates suggesting an additional 1% decline in population growth. These population effects were concentrated in rural areas, which grew 2% below trend per decade, while there was no significant effect in urban areas.[2]

In these net emitting municipalities, we find that migration tends to be selective for the better-educated and those with higher incomes. There is no change in the lower income population (minimum wage or below), but significant declines in the middle (from 1 to 5 times the minimum wage) and higher income (over 5 times the minimum wage) populations. This is consistent with selective outmigration by better-educated, higher-income persons.[3] Further, the economy becomes less agricultural in employment. Yet there is no significant change in urbanization or aggregate growth in any major sector. While we cannot cleanly identify welfare impacts, it is notable that wages do not fall significantly in any sector despite the presumed loss in demand that should have accompanied the decline in population. Further, the sectoral balance within the remaining population represents a more diverse economy, in principle less vulnerable to volatility in agricultural markets. There is heterogeneity across subgroups within the municipality. While we find no effect on aggregate employment overall, agricultural and services jobs indeed decline significantly within rural areas. Changes in sectoral employment shares are significant within urban areas, but not rural areas; they are also significant among males, but not females. This is consistent with greater intersectoral and geographic mobility among men than women. Finally, even across subgroups, there are no significant changes in wages within net emitting municipalities.

Our second set of results relate to the effects of rural reform and immigration on states' manufacturing capitals, which are net receiving municipalities. Here we find significantly greater overall and urban population growth in response to PROCEDE. Aggregate agricultural employment increases significantly; point estimates are positive in other sectors but smaller and not significant. This suggests that native and immigrant laborers are not homogeneous within urban labor markets, and/or that there are important switching costs.

[1] The relevant ejido population also include posesionarios and avecindados. In total, these comprise 13% of the average municipality population, with about 7% of population certified prior to 2000.

[2] Regression point estimates are scaled by the average ejido and early-certified shares in presenting these results. Note that while point estimates suggest some degree of urbanization, this impact was not significant.

[3] We may also infer that PROCEDE did not increase local returns to education sufficiently to reverse this selection effect. For example, if agricultural productivity increased substantially, we might expect more educated households would be best able to exploit this increase. In this case, migration might have been selective for lower income rather than high income populations.

But the influx of population is not sufficient to change the economic structure—employment shares by sector do not change significantly. On the other hand, average wages increase significantly.[4] But by sector, only service sector wages increase significantly. The fact that wage impacts can be sector-specific again points to nonhomogeneous labor and intersectoral adjustment costs that prevent workers from switching sectors to exploit the higher wages. The fact that wages for services employees in particular increase may be explained in at least two ways. Because both agricultural and manufactured goods are tradable, even a balanced increase in local demand across sectors due to population growth would lead to stronger wage increases within services, so long as labor is not fully mobile across sectors. Alternatively, incoming migrant preferences may be biased toward services compared to the local population. In this case, price increases in services may reflect a composition effect consistent with selection effects for higher income migrants with a preference for amenities only available in cities. In either case, while we cannot directly identify welfare impacts, it seems likely that native households within the services sector are the most likely beneficiaries of increased local demand resulting from immigration.

The remainder of the paper is organized as follows. In Section 2.2 we provide details on the history of land reform in Mexico. Section 2.3 describes the data. Section 2.4 presents the identification strategy and results. Section 2.5 provides additional robustness checks and Section 2.6 concludes.

2.2 Land reform in Mexico

A major grievance of insurgent groups during the Mexican Revolution was the expropriation of indigenous lands by elites for incorporation into large estates. Mexico's first land reform was thus a response to demands by peasant revolutionaries, establishing constitutional provisions allowing large estates to be purchased or expropriated and reallocated to the landless. The result would be a 75-year land redistribution program, from 1914 to 1992, among the largest in the world, involving over 50% of Mexican territory (Yates 1981).

Expropriated lands were organized into agrarian communities called ejidos (Sanderson 1984). Ejido land included individual parcels available to community members under use-based rights, common property lands for grazing and forestry, and residential plots. Land sales and hired labor were prohibited. Importantly, community members (or "ejidatarios") were required to use the land productively (Cordova 1974). Land left idle for two years could be taken away, essentially imposing a permanent obligation that the ejidatario and his family had to cultivate the land or lose access. This rule was enforced by a state-level Agrarian Commission external to the ejido, which was charged with implementing federal legislation and responsible for land expropriations and reallocations.

Mexico's second land reform was a response to the impending implementation of NAFTA and elimination of import tariffs on agricultural goods. Land reforms establishing security of property rights to agricultural land were seen as essential in order to promote long-term investment by ejidatarios and maintain competitiveness (Heath 1990). Among its key provisions were the establishment of a national program, PROCEDE, to provide ejidatarios

[4] This is notable in contrast to work studying the effects of rural-urban immigration following adverse rural income shocks (Kleemans and Magruder 2015)

with certificates to their land; to provide certificate-holders with rights to rent their plots, sell to other members of the ejido, hire labor, and fallow land; and to provide a mechanism to convert certificates into full private property (de Janvry et al. 1997). PROCEDE was rolled out nationally from 1993 to 2006, eventually certifying 92% of ejidos.

From an evaluation standpoint, the ideal program implementation would have been to randomly assign the year in which ejidos received certification through PROCEDE. In practice, de Janvry et al. [2014] show that where certification was completed earlier, ejidos were smaller, had a larger share of their land in parcels, were closer to large cities, were wealthier, had fewer nonvoting members, and were in municipalities that shared the political party of the state governor.

We adopt several strategies to address identification concerns. All regressions control for state-by-year time trends. This eliminates the concern that time-trending unobservables at the state-level might be simultaneously correlated with early-certification shares and our outcomes of interest. In addition, differences between early- and late-certified ejidos are not a threat to econometric identification if they are uncorrelated with the economic outcomes of interest. Thus, we verify that changes over time in migration prior to the program were not correlated with the date program completion. While we study multiple economic outcomes in addition to migration, this is the key economic channel through which we hypothesize most of our economic effects occur, as well as the only outcome variable for which we have sufficient pre-program data to perform this test. Finally, we interact fixed municipality characteristics found to be correlated with program completion with time effects in order to account for the possibility that migration and other economic outcomes changed for reasons related to these characteristics.

2.3 Data

Information on the rollout of PROCEDE is based on a set of ejido digital maps created during the certification process. GIS ejido boundaries are available for the 26,481 ejidos that completed the program during the period from 1993–2006. The digital maps, as well as administrative data for 28,614 ejidos including the date of certification and the number of ejido community members were obtained from the National Agrarian Registry (RAN). Of these ejidos, 20,524 (or 71%) were certified from 1993–1999, while 8,090 (or 28%) were certified after 1999.

Next, the primary economic data in the analysis are the 1980, 1990, and 2000 population censuses carried out by INEGI. Population data are available for all years; demographic and employment data are available for 1990 and 2000. Demographics include literacy, education, and housing characteristics. Employment outcomes include labor force participation, employment, and sector of occupation. The population census categorizes employment of persons 12 and older into three groups: the primary sector (agriculture, ranching, forestry, and fishing), the secondary sector (construction, mining, manufacturing, and electricity), and the tertiary sector (commerce, communications, transportation, services, public administration, and defense). Nationally, 23%, 28%, and 46% of employment in 1990 were in the primary, secondary, and tertiary sectors, respectively. Employment became more service-based and less agricultural by 2000, with employment shares of 16%, 28%, and 53% in the primary, secondary, and tertiary sectors, respectively.

The census data are available at the state, municipality, and locality levels. These data may be merged with information on the rollout of PROCEDE at the state and municipality levels using geographic identifiers available in both datasets. To conduct analyses at the locality-level, ejidos and localities must be matched spatially. We considered the locality to match an ejido if the centroid of the locality was located inside the boundaries of one of the ejidos in the GIS database. This process matched 27,334 localities to 11,581 different ejidos. Of these ejidos, 8,454 (or 73%) were certified from 1993–1999, and 3,100 (or 27%) after 1999.

Third, we use the 10% microdata samples prepared by IPUMS and INEGI from the 1990 and 2000 population censuses. Geographic identifiers are available at the municipality-level. The 1990 data are a self-weighting sample of private dwellings extracted from the full census microdata. The 2000 census incorporates a short form completed by enumeration and a long form completed by sampling; the microdata are provided for the long form. The 2000 sample was stratified geographically and sampled clusters of dwellings within strata. The final microdata dataset is a two-year pooled cross-section of income, work hours, occupation, and demographics for about 8 million persons in 1990 and 10 million persons in 2000. Weighted averages are constructed at the municipality level using the survey weights, as well as by subgroups including urban, rural, male and female subpopulations. When needed, we construct municipality aggregates, such as total agricultural income earnings, by computing average income per agricultural employee in the microdata, multiplied by the complete census count of primary sector employees in that municipality.[5]

Fourth, we use data on annual real GDP by state and industry from INEGI for 1993 and 2000. We group industries into the primary, secondary, and tertiary sectors as in the population census.[6] Nationally, 6%, 26%, and 68% of GDP in 1993 were in the primary, secondary, and tertiary sectors, respectively; this would become 5%, 28%, and 67% by 2000.

2.4 Results

2.4.1 Locality analysis

First, we use the matched 1990 and 2000 locality-level population censuses. The locality-level analysis captures both migration of individuals and entire families. Three key characteristics of this dataset are its inclusion of localities of all sizes and levels of income, its geographical coverage (nationwide), and its time span (up to seven years with a certificate).

We first compare the evolution of local outcomes over time in a standard two-period fixed effects regression. Let i index localities, j index ejidos, m index municipalities, s index states, and t index time. Then we estimate:

[5] Nominal values are used throughout. In the regression analyses, any currency effects of the 1994 peso crisis and other price changes are absorbed by the state-by-year fixed effects.

[6] The industries defined for state-level GDP are: 1) agriculture, forestry, and fishing; 2) mining; 3) manufacturing; 4) construction; 5) electricity, gas, and water; 6) commerce, restaurants, and hotels; 7) transportation, storage, and communication; 8) financial services; and 9) other services. We define industry 1 as the primary sector, combine industries 2 through 5 as the secondary sector, and all others as the tertiary sector.

$$y_{ijmst} = \mu_j + \eta Year2000_t + \delta EarlyCert_j \times Year2000_t + \varepsilon_{ijmst} \tag{2.1}$$

where y_{ijmst} is an outcome of interest, such as population or employment, the μ_j coefficients are ejido fixed effects, the variable $Year2000_t$ is an indicator equal to 0 in 1990 (before any certification) and equal to 1 in year 2000 (after certification had begun), and $EarlyCert_j$ is an indicator equal to 1 if the ejido was certified prior to 2000, and 0 otherwise. This is a standard, two-period, difference-in-difference regression where identification comes from changes in outcomes correlated with changes in certification status between 1990 and 2000. Any time-invariant ejido characteristic that is correlated with the program rollout is accounted for by the municipality fixed effects μ_j. Time trends that are common across ejidos are accounted for by the time effect η.

Table 2.1 reprises key results from de Janvry et al. [2015]. The dependent variable is the total population (or its logarithm) of locality i in year t. The regression results show that the program induced migration at the locality level. The first row in the table shows that ejido localities lost around 9.6 persons or 21 percent of their population between 1990 and 2000 (the time effect). The coefficients on the interaction term in the second row indicate that PROCEDE was associated with an additional reduction in population of approximately 3–4 individuals, in a setting where the average locality has 99 individuals (Column 1), or 4 percent of its population (Column 2). As a falsification test, we use 12,455 localities with available population in 1980 to estimate a version of the above regression for the period 1980–1990. The estimate in column 3 indicates that the difference in population change in the 1980–1990 decade between early and late certified localities was very small and not significant. This similarity in pre-program population trends suggests that our estimate is not driven by pre-1990 differences in population change between early program and late program areas.

Table 2.2 explores selected employment outcomes in the locality after certification, i.e., among the population that remains behind. Column 1 indicates that certification leads to a 10% decline in the labor force (i.e., the population economically active, which includes both the employed and those actively seeking work). On the other hand, Column 2 shows that there is no change in the population not economically active. Interpreted as selection, this implies that active labor force participants are more likely to emigrate than the inactive population. Column 3 confirms that overall employment also declines by 10% following certification. Columns 4–6 consider log employment within the primary, secondary, and tertiary sectors. Only primary sector employment changes significantly, declining by 14%. That the percentage declines in employment are considerably larger than the percentage declines in population, with no offsetting increases in workers outside of the labor force, suggests it is primarily employed agricultural workers who emigrate.

Table 2.3 demonstrates that the local labor market looks significantly different after certification. There is a significant 3pp decline in the share of employment in agriculture, and a 1pp increase in the share of employment. Notably, however, this economic restructuring results from the decline in agricultural employment, rather than due to growth in manufacturing employment.

Finally, Table 2.4 shows that educational indicators in the locality decline after certification. This suggests that the more educated members of the population were most likely to emigrate. Other welfare indicators have mixed signs. The percentage of households with dirt floors and lacking sewage connections declined, while the percentage lacking electrical connections increased. While we cannot definitively distinguish selection effects versus direct impacts, it may be that persons with the ability to migrate were most likely to reside in electrified households. The remaining population is nevertheless able to invest in private durables, like improved flooring and water connections.

2.4.2 Municipality analysis

Next, we use the year 1990 and 2000 municipality-level datasets described above. While the municipality remains a very local geographic area, it allows us to study aggregate effects of certification beyond the ejido. Note that while analyses at the locality-level highlight changes in net migration, aggregate effects also depend on changes in gross migration patterns. Moreover, to the extent that ejido out-migrants choose to relocate within the same municipality, we can study the reallocation of individuals across occupations and across space, rather than the effects of population loss. Limitations are the loss in power due to moving the unit of observation farther away from the ejido itself, and the need for stronger identifying assumptions.

In our basic specification, we exploit only within-state variation across municipalities in certification intensity to identify impacts. Let m index municipalities, s index states, and t index time. Then we estimate:

$$\begin{aligned} y_{mst} &= \mu_m + \tilde{\eta}_{st} + \gamma Year2000_t \times PctEjidoMun90_{ms} \\ &\quad + \delta Year2000_t \times PctEarlyMun90_{ms} + \epsilon_{mst} \end{aligned} \tag{2.2}$$

where y_{mst} is an outcome of interest, such as population or wage earnings from manufacturing. The coefficients μ_m and η_{st} are municipality and state-by-year fixed effects, respectively, and ε_{mst} is a random error term. Robust standard errors are clustered at the municipality-level for estimation. The variables $PctEjidoMun90_{ms}$ and $PctEarlyMun90_{ms}$ are continuous variables capturing the percentage of the municipality population located in ejidos in 1990, and located in ejidos that were certified prior to the year 2000, respectively. The variable $Year2000_t$ is defined as above. This is a standard fixed effects regression where identification is coming from changes in outcomes correlated with changes in certification status between 1990 and 2000. Any time-invariant municipality characteristic that is correlated with the program rollout is accounted for by the municipality fixed effects μ_m. Any time trends that are common to municipalities of a given state are accounted for by the state-by-year fixed effects η_{st}. Finally, it may be that ejidos simply have a different time trend than other populations. Such ejido-specific time trends are accounted for by the coefficient β on the interaction term $Year2000_t \times PctEjidoMun90_{ms}$. The coefficient of interest is δ, which captures the effect of greater certification on y_{mst}. The identifying assumption is thus that any time-varying characteristic of municipalities that affects y_{mst} is uncorrelated with the speed of the rollout in that municipality. We provide support for the validity of this identification assumption in Section 2.5.

Equivalently, let $\Delta y_{ms} = y_{ms,2000} - y_{ms,1990}$. Then we can also recover γ and δ from the first-differenced regression:

$$\Delta y_{ms} \quad = \quad \eta_s + \gamma PctEjidoMun_{ms} + \delta PctEarlyMun_{ms} + e_{ms} \tag{2.3}$$

where $\eta_s = \tilde{\eta}_{s,2000} - \tilde{\eta}_{s,1990}$ and $e_{ms} = \Delta\epsilon_{mst}$. Robust standard errors are used in the estimation.

Municipio population and urbanization

We first test whether certification leads to changes in population at the municipality-level. Given the outmigration observed in the locality analyses, a precise zero at this level would suggest that migrants leaving the ejido tend to stay within the borders of the municipality.

Table 2.5 presents the results. In fact, municipalities with relatively larger shares of their population in early-certified ejidos still tend to lose population, suggesting that ejido migrants do not stop at the municipality borders. Point estimates in Column 1 indicate that a 1-percentage point increase in the early-certified share of the population results in a loss of 0.2 percent of the population. In the average municipality, 5% of the population was certified between 1993 and 1999. This implies that the average municipality was about 1% smaller in 2000 than it would have been absent PROCEDE. Some portion of this will be directly attributable to out-migration by ejidatarios, avecindados and posesionarios and their families. In addition, it may be that slower growth of ejidos leads to network effects that reduce population growth from non-ejido rural areas, such as through trade relationships.

Columns 2 and 3 focus on population changes within the urban and rural areas of the municipality. Consistent with intuition, we find more negative effects among rural populations than urban. The coefficient on urban populations is small and insignificant, whereas the point estimate for rural populations is significant and indicates an elasticity of -0.4. Scaled by the average early-certified share of the population, this implies that the rural population was about 2% smaller than it would have been in 2000 absent PROCEDE. Columns 4 and 5 focus on the population in those localities that could be definitively matched to ejidos. We expect a more negative impact among early-certified localities than among late-certified localities. While point estimates are consistent with this prediction, neither coefficient is statistically significant.[7] Finally, Column 6 tests whether PROCEDE increased urbanization at the municipality-level, either through net out-migration by rural populations, or through movement of rural populations into the city. The relevant point estimate is positive, but small and not significant.

Thus, the regressions in Table 2.5 suggest that while PROCEDE indeed had a strong impact on net migration from rural areas, the average municipality was not well-equipped to retain an increasingly mobile population. There is no evidence of urbanization. But results also suggest that economic effects may be heterogeneous across municipalities, as well as across population subgroups within municipalities.

[7] We might also expect impacts among early-certified localities to be at least as negative as impacts for the rural population. But because ejidos do not correspond precisely to localities, this prediction is ambiguous. This depends on the proportion of ejidatarios within the matched localities as compared to the proportion within the general rural population. In addition, matches between ejidos and localities were only identified in a smaller set of municipalities, and thus represent a different local average treatment effect.

Municipio employment by income level

How does the distribution of income change as outmigration increases?

Columns 1-3 of Table 2.6 report log employment at the municipality-level within three income categories: persons earnings less than the minimum wage (LnLow); persons earnings up to five times the minimum wage (LnMid); and persons earning more than 5 times the minimum wage (LnHi). Columns 4-6 then report the corresponding percentages of the municipality population within each of the same categories (PctLow, PctMid, PctHi). The population includes all employed persons over 12 years of age who reported an income level.

In Column 1 we find no significant effect on the population at the lowest income level. Yet the middle and upper income levels both decline in Columns 2 and 3, either significantly or marginally significantly. Point estimates are large, suggesting that the higher income population is about 7% smaller than it would have been absent PROCEDE. These changes are also reflected in the percentages in the Columns 4-6, as we see that the share of employment at the highest income levels declines significantly. Because the higher income is a relatively small share of the population, however, change in the share of the population at the highest income levels is relatively small—the implied decline is only -0.14pp.

Thus, the regressions in Table 2.6 indicate that outmigration due to PROCEDE was relatively concentrated among higher income employmees. The data are consistent with selective outmigration by wealthier households; ultimately, however, we cannot distinguish between selective migration versus welfare changes among non-migrants.

Municipio sectoral employment and earnings

Does outmigration also imply job loss across all sectors of the municipality economy? Or do some sectors flourish while others decline?

Columns 1–3 of Table 2.7 report the effects of certification on aggregate employment within the primary, secondary, and tertiary sectors of the economy, respectively. Notably, none of the individual sectors exhibits significant aggregate effects in Columns 1–3. Point estimates are negative for agriculture and positive for the other sectors; elasticities are small, ranging from -0.08 in agriculture to 0.07 in services. These compare to an elasticity of -0.2 on population in Table 2.5. The smaller and insignificant point estimates here are consistent with a prediction that persons or households with more stable employment are less likely to migrate. However, notice the contrast with employment effects at the locality level in Table 2.2. The analysis there suggested that employed agricultural workers are more likely to leave the ejido than non-labor force participants; the analysis here suggests that those employed agricultural workers who leave the ejido tend to relocate within the municipality, while the non-labor force participants who leave the ejido are most likely to migrate beyond its borders. Finally, Columns 4–6 report the effects on aggregate income (i.e., total wage earnings or labor expenditure) within the same sectors. At this level of aggregation, there are no significant impacts.

Next, we consider whether PROCEDE led to significant changes in the relative importance of different sectors of the economy. Specifically, Table 2.8 examines the sectoral shares of aggregate employment and aggregate labor income. Scaled by the average level of early-certification, Columns 1 and 4 indicate that municipality economies became less agricultural in both their aggregate employment (-0.8pp) and income shares (-1.2pp). Each of the other two sectors gained about 0.5pp.

Thus, the regressions in Table 2.7 indicate that at the municipality-level, neither aggregate employment nor aggregate labor earnings by sector change significantly. Comparing Table 2.5, this is consistent with the idea that persons and households with more stable employment are less likely to emigrate. On the other hand, there is consistent evidence that local economies become relatively less dependent on agriculture, and more dependent on manufacturing and services.

Municipio wages

Does outmigration lead to labor scarcity and wage increases among the remaining population? Or does it result primarily in a loss of local demand and wage declines?

Table 2.9 regresses sectoral wages on PROCEDE exposure. There are no statistically significant impacts in any sector. Assuming labor is paid its marginal revenue product, there is no evidence of a change in labor revenue productivity.

2.4.3 Municipality subgroups

The diverse effect on population groups in Table 2.5 raise the possibility that outcomes may vary depending on the subgroup analyzed within the municipality. Thus, we repeat the municipality regressions for subgroups including urban, rural, male or female subpopulations.

Subgroup analysis of municipio sectoral employment and earnings

The regressions in Table 2.7 provided little evidence of impacts on aggregate employment or labor earnings. Table 2.10 considers whether impacts may have been concentrated among particular subpopulations, including urban, rural, male, or female subgroups.

Looking across the four subgroups, we find significant impacts on aggregate employment only within the rural subpopulations. Point estimates are negative across all sectors, and of the same order of magnitude, suggesting relatively consistent declines across all sectors. Both agricultural and service sector employment decline significantly, with elasticities of -0.4 and -0.7, respectively. The implied job losses are thus about -2% and -3.5 in these two sectors, respectively. The concentration of aggregate job losses within the rural sector is consistent with the significant declines in rural population compared to other groups in Table 2.5. There are also indications that women may be particularly affected. There is a marginally significant decline in aggregate female employment, and a significant decline in aggregate labor earnings by women.

Next, Table 2.11 considers the effects on sectoral shares by subgroup. Despite the aggregate employment losses in the rural sector, because these losses were relatively consistent across sectors, there is no significant change in employment or income shares. On the other hand, while the impacts on aggregate employment by sector were not individually significant in urban areas, point estimates varied widely. Thus we see significant declines in urban agricultural shares of employment, and increases in services. The point estimates imply that agricultural employment share declines by about 0.5pp, while the services shares increases by about 0.5pp. Male populations also saw significant changes in employment shares. Employment becomes significantly more concentrated in manufacturing (by 0.5pp), as do labor earnings (by 0.9pp). While employment shares do not change significantly for women, labor earnings also become significantly more concentrated in manufacturing (by 1.5pp).

Thus, Tables 2.10 and 2.11 show that aggregate employment losses were concentrated within rural populations. Yet, the clearest changes in the sectoral composition of employment occurred in urban areas, and among males. In particular, labor markets in urban areas became significantly less agricultural (-0.5pp) and more service-based (0.5pp). Male employment became significantly less agriculture-dependent (-0.6pp) and more manufacturing-based (0.5pp). Female labor earnings become significantly more manufacturing-based (1.5pp)

Subgroup analysis of municipio wages

Table 2.12 shows that across urban, rural, male, or female subgroups, there continue to be no significant impacts on overall or sectoral wages. Assuming labor is paid its marginal revenue product, there is no evidence of a change in labor revenue productivity in any sector.

2.4.4 Municipality manufacturing capitals

While the average municipality in Table 2.5 was not well-equipped to retain an increasingly mobile population, it seems likely that losses by the average municipality will be gains to another municipality. In this section, we allow for the possibility that migrating populations may converge on the largest municipality in a given state. In this case, outcomes within the largest municipality will depend not only on the early-certified share of the population that resides within its borders, but also on the total early-certified population throughout the state. Thus, we are interested in an interaction between a municipality's relative position within the state, and the total potential migrant population.

We focus on the municipality in each state with the largest manufacturing employment in 1990. For 26 of 32 states, the largest municipality in manufacturing is also the largest in services, and it is typically also the population capital. This municipality therefore represents access to all of the amenities, employment and consumption options typically afforded by an urban environment. In focusing on the entire municipality rather than its principal city, we allow for the possibility that migrants move to rural areas in the periphery of cities, rather than to the cities themselves.

Our regression specification is an extension of (2.3):

$$\begin{aligned}\Delta y_{ms} \;=\;& \eta_s + \gamma PctEjidoMun_{ms} + \delta PctEarlyMun_{ms} + \beta I(MostMfgMun_{ms}) \\ & + \psi I(MostMfgMun_{ms}) \times PctEarlyState_s + v_{ms}\end{aligned} \tag{2.4}$$

where $I(MostMfgMun_{ms})$ is an indicator equal to 1 if municipality m contains the most manufacturing employment in state s in 1990, and $I(MostMfgMun_{ms}) \times PctEarlyState_s$ is an indicator with the early-certified share of population at the state-level. The coefficient of interest is ψ. The v_{ms} term is a random error, clustered at the state-level.

Outcomes within the largest municipality are allowed to depend on the early-certified share of the population in the same way that outcomes do in all other municipalities. But we have relaxed the specification in (2.3) by allowing the largest municipality in each state to also have a systematically different time trend for reasons that may be outside of our model; this is captured by β. Finally, we allow for the possibility that populations tend to converge on a particular municipality using the interaction term $I(MostMfgMun_{ms}) \times PctEarlyState_s$.

Heterogeneity in municipio population and urbanization

Do the manufacturing capitals within states tend to gain or lose population in response to PROCEDE? Table 2.13 tests this using regression (2.4). Population outcomes are regressed on indicators for the most important municipality for manufacturing. The coefficients indicate that greater exposure to PROCEDE at the state-level in fact leads to net population growth within these municipalities.

Thus, the regressions in Table 2.13 support the hypothesis that rural populations tend to converge toward the important manufacturing and services municipalities within the state.

Heterogeneity in municipio sectoral employment and earnings

Given our finding that the manufacturing capitals tend to gain population, we now ask whether they also gain jobs and earnings, and if so, in which sectors.

Table 2.14 regresses log aggregate employment by sector against the indicator of municipality importance. On average, the normal trend is that employment grows more slowly in manufacturing capitals than in other areas of the state. But when the leading manufacturing municipalities are exposed to relatively greater PROCEDE populations at the state-level, log agricultural employment increases significantly. Results on aggregate labor expenditures are also significant. Log agricultural earnings, and log services earnings both grow significantly relative to trend. In Table 2.15, we also consider whether sectoral shares change significantly in the manufacturing capitals. But here we find no significant effects.

Thus, the results in Tables 2.14 and 2.15 support the hypothesis that as rural populations converge toward the important manufacturing and servicies municipalities, aggregate agricultural employment increases significantly. Agricultural earnings and services sector earnings also increase.

Heterogeneity in municipio wages

Finally, we ask whether wages tended to increase or decrease in response to increased immigration, and whether these effects were the same across sectors.

Table 2.16 regresses monthly average wage against our indicator of municipality importance. Strikingly, Column 1 indicates that on average in manufacturing capitals, wages tend to increase at the same time as the large immigration implied by Table 2.13.

One possible explanation for this result may be that the kind of immigration we observe does not lead to a substantial net increase in employment competition. For example, better off ejidatarios with a preference for urban amenities might sell their plots and migrate to land nearer to the city. This is not growth, as it merely relocates an agricultural job from one location to another within the same state. Depending on composition effects, this move may or may not affect the average agricultural wage, but it would not lead to significant employment competition for services and manufacturing jobs. On the other hand, consumption demand from this immigrant household could drive up local prices, in particular for non-tradeable goods.

This interpretation is consistent with the significant increase in agricultural employment observed in manufacturing capitals in Table 2.14. This hypothesis might also imply that we should observe a significant increase in service sector wages. But notice a prediction about sector-specific wage effects depends on imperfect substitutability of labor across sectors. If workers can easily move between sectors, then an increase in non-tradeables prices will drive up wages in all sectors, and we will not observe sector-specific effects. Thus, asking whether wage increases are concentrated in the service sector is a joint test of the hypotheses that the local demand effects of immigration outweigh employment competition, and that frictions exist which prevent easy movement of local workers between sectors.

Thus, Columns 2-4 of Table 2.16 examine changes in sector-specific wages in manufacturing capitals. Consistent with the above hypothesis, we find that only service sector wages increase significantly. Yet the story is not so precise as the simple hypothesis above. Although the point estimates on agricultural and manufacturing wages are not significant, both are positive, consistent with some level of substitutability across sectors. In fact, the point estimate on agricultural wages is almost as large as the point estimate in services. This may indicate greater substitutability between agricultural and services employment than between manufacturing and the other two sectors, or it may indicate composition effects in the agricultural sector due to higher-income immigrants. Under the interpretation that intersectoral labor mobility is low, these results suggest that natives in the services sector are most likely to benefit from immigration. Policies that increase intersectoral labor mobility would tend to make the local demand benefits more equally shared across natives.

Thus, Table 2.16 indicates that wages in manufacturing capitals increased significantly in response to immigration caused by PROCEDE. There are indications that wage increases were concentrated in services. In combination with the increase in agricultural employment seen in Table 2.14, these results offer an intruiging pattern of urbanization and effects of immigration.

2.5 Robustness checks

2.5.1 State analysis

Section 2.4.2 showed that PROCEDE induced population loss at the level of the average municipality, while section 2.4 suggested that results were heterogeneous across municipalities. Aggregating the analysis to the state-level may thus allow us to capture the full impacts of labor reallocation throughout the state. As in the municipality analysis, limitations include the loss in power due to moving the unit of observation even farther away from the ejido, and the need for stronger identifying assumptions.

At the state-level, we run fixed effects regressions based on equation (2.2), replacing the state-by-year time effects with a single time fixed effect. To improve robustness, we allow for heterogeneous time trends in rural areas. Due to the small number of clusters, our preferred inference relies on a wild bootstrap procedure based on Cameron, Gelbach, and Miller (2008).

Tables 2.17–2.20 report the results. In many respects, the results are similar to those in the municipality analysis. We briefly review the key results. First, population losses are no longer statistically significant. On the other hand, urbanization increases significantly, consistent with the analysis in section 2.4. Aggregate employment does not change significantly in any sector, but agricultural GDP declines. The agricultural shares of employment and GDP decline significantly, implying reductions of about 3pp and 1pp, respectively. There are no significant changes in GDP per capita or per employee in any sector.

Thus, moving the analysis to the state-level largely corroborates the results of the municipality analyses.

2.5.2 Placebo tests

The main threat to identification in the above analyses is a correlation between the intensity of rollout of PROCEDE and the time path of economic changes in the municipality or state. The estimated program effect would be biased if PROCEDE was correlated with pre-program changes in given economic outcomes.

To investigate this, we use a standard regression of pre-program changes in economic variables of interest on PROCEDE exposure. That is, for example, we regress changes in (log) population from 1980-1990 on the early-certified share of the population using the same regression specifications as above. Our identifying assumption requires that there should not be any significant relationship during this period. Ideally, we would be able to run this placebo test for all economic variables studied. But the only economic variables available prior to 1990 are the population variables.

Tables 2.21-2.23 report the results. In both the municipality-level and state-level regressions, there is no significant trend in population associated with early certification for any subgroup of interest, corroborating the identifying assumptions.

2.6 Conclusion

Understanding the processes of migration, urbanization and structural transformation remains central to economic development. In this paper, we argue that rural reforms establishing secure property rights to agricultural land drive a particular pattern of structural and spatial transformation.

We considered a regime in which access to agricultural land was contingent on both the owner's presence and his continued active use of the land. Because migration requires surrending land with no opportunity for compensation, such policies restrict geographic labor mobility and led to an inefficient allocation of labor to agriculture. While previous work established that security of property rights led to an important increase in outmigration (de Janvry et al. 2015), here we establish the economic consequences of this migration. We showed that certification led higher-skilled agricultural labor to migrate away from their origin municipalities, and left behind economies that were less concentrated in agriculture. This is structural transformation without growth, but it is also out-migration without blight, as there is no significant deterioration in wages among the remaining population.

Yet while the average municipality lost population, we showed that within states, the most important municipality for manufacturing employment typically saw gains in urban population and agricultural employment. Average wages increased significantly, suggesting that in this context, immigrants brought with them growth and demand effects that outweighed any employment competition. This is notable in contrast to work studying the effects of rural-urban immigration following adverse rural income shocks; e.g., Kleemans and Magruder [2015] find that such immigration leads to declines in employment rates and lower wages. This points to an important difference in effects depending on the type of immigration. While we cannot distinguish formal and informal employment in our context, we are able to distinguish primary, secondary, and tertiary sectors. We found that wages only rose significantly in services. We argue that sector-specific wage effects speak to imperfect substitutability of labor—across sectors, and/or between immigrants and natives—as an empirically important element in the processes of structural transformation and internal migration. Finally, in an environment with perfect substitutability of labor, native employees in the non-tradeables sector are the most likely beneficiaries of increased local demand associated with immigration.

Tables

Locality analysis

Table 2.1: Effect of PROCEDE on locality-level population

	(1) Pop	(2) LnPop	(3) LnPop
I(Year='00)	-9.631***	-0.207***	
	(1.001)	(0.0105)	
I(Cert93-99)xI(Year='00)	-3.689***	-0.0404***	
	(1.148)	(0.0128)	
I(Year='90)			-0.209***
			(0.0125)
I(Cert93-99)xI(Year='90)			-0.00819
			(0.0148)
Mean Dep Var	99.11	4.271	4.416
Observations	34656	34656	24910
R-squared	0.0142	0.0355	0.0332

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Notes: Standard errors clustered by ejido. An observation is a locality-year. Dependent variable in column 1 is locality-level population, and log locality-level population in all others. Regressions in columns (1) and (2) based on 17,328 localities that were matched to ejidos, had population data in both the 1990 and 2000 censuses, and had a population of more than 20 in 1990. Regression in column (3) is based on 12,455 localities with available population data in 1980 and with a population larger than 20 in 1980. **Sources**: Population census data for 1990 and 2000 from INEGI. Locality-level PROCEDE classifications from de Janvry *et al.* (2015).

Table 2.2: Selective migration for employed, agricultural workers

	(1) LnPEA	(2) LnNPEA	(3) LnEmp	(4) LnEmp1	(5) LnEmp2	(6) LnEmp3
I(Year≥'00)	0.000349	-0.0265**	0.0319**	-0.0767***	0.258***	0.349***
	(0.0142)	(0.0116)	(0.0149)	(0.0164)	(0.0256)	(0.0200)
I(EarlyCert)xI(Year≥'00)	-0.0991***	-0.0144	-0.0997***	-0.136***	-0.000652	0.0158
	(0.0173)	(0.0142)	(0.0180)	(0.0195)	(0.0305)	(0.0240)
Mean Dep Var	2.925	3.382	2.900	2.579	1.196	1.056
R-squared	0.00403	0.00102	0.00211	0.0185	0.0335	0.0722
Observations	32861	33298	32760	32075	19362	19896
Municipalities	1499	1499	1499	1497	1437	1443
Ejidos	9489	9490	9486	9464	7892	8189
Localities	17246	17286	17229	17121	12604	12841

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Notes: Standard errors clustered by ejido. An observation is a locality-year. Dependent variables in columns are log persons economically active, log persons not economically active, log persons employed, and columns (4) through (6) contain log persons employed by sector. The sectors are primary (agriculture), secondary (construction, mining, manufacturing, electricity), and tertiary sectors (commerce, communication, transportation, services, public administration, defense). **Interpreted as selection, population declines are driven by out-migration of employed agricultural workers.** All regressions control for ejido fixed effects. **Sources**: Population census data for 1990 and 2000 from INEGI. Locality-level PROCEDE classifications from de Janvry *et al.* (2015).

Table 2.3: Out-migration due to PROCEDE significantly changed local employment structure

	(1) PctPEA	(2) PctEmp	(3) PctEmp1	(4) PctEmp2	(5) PctEmp3
I(Year≥'00)	0.791*	3.493***	-6.612***	3.919***	5.088***
	(0.434)	(0.286)	(0.590)	(0.353)	(0.267)
I(EarlyCert)xI(Year≥'00)	-1.896***	-0.589*	-2.966***	0.997**	0.458
	(0.508)	(0.331)	(0.680)	(0.425)	(0.324)
Mean Dep Var	39.41	97.34	74.42	12.59	9.899
R-squared	0.00230	0.0359	0.0608	0.0328	0.0644
Observations	33328	32860	32759	32759	32759
Municipalities	1499	1499	1499	1499	1499
Ejidos	9491	9489	9486	9486	9486
Localities	17288	17245	17228	17228	17228

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Notes: Standard errors clustered by ejido. An observation is a locality-year. Dependent variables in columns are percentage of the population economically active, percentage of the economically active population that is employed, and columns (3) through (5) contain the percentage of employed persons by sector. The sectors are primary (agriculture), secondary (construction, mining, manufacturing, electricity), and tertiary sectors (commerce, communication, transportation, services, public administration, defense). **Out-migration due to PROCEDE significantly changed local employment structure. Necessarily, out-migration by agricultural workers led to communities that were relatively less agrarian and more industrial.** All regressions control for ejido fixed effects. **Sources**: Population census data for 1990 and 2000 from INEGI. Locality-level PROCEDE classifications from de Janvry *et al.* (2015).

Table 2.4: Selective migration for the more educated and better-off

	(1) MargIdx	(2) Illit	(3) NoSch	(4) Dirt	(5) NoSewr	(6) NoElec
I(Year≥'00)	-0.401***	-2.864***	-2.303***	-11.54***	-10.95***	-32.48***
	(0.0105)	(0.178)	(0.189)	(0.416)	(0.508)	(0.771)
I(EarlyCert)xI(Year≥'00)	0.0205*	1.192***	0.829***	-0.833*	-2.882***	1.933**
	(0.0123)	(0.196)	(0.216)	(0.495)	(0.601)	(0.927)
Mean Dep Var	0.451	14.86	15.21	57.77	87.20	54.82
R-squared	0.183	0.0325	0.0185	0.125	0.159	0.283
Observations	32847	33334	33334	33334	33334	33334
Municipalities	1499	1499	1499	1499	1499	1499
Ejidos	9489	9491	9491	9491	9491	9491
Localities	17243	17288	17288	17289	17289	17289

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Notes: Standard errors clustered by ejido. An observation is a locality-year. Dependent variables in columns are the: marginality index, illiteracy rate, and population shares that have no school education, live in homes with dirt floors, do not have sewage connections, and do not have electricity. **Interpreted as selection, the increased marginality, illiteracy, and declines in education suggest that the more educated and better-off in the community are most likely to migrate**. All regressions control for ejido fixed effects. **Sources**: Population census data for 1990 and 2000 from INEGI. Locality-level PROCEDE classifications from de Janvry *et al*. (2015).

Municipality analysis

Table 2.5: Municipio populations decline

	(1) Δ LnPop	(2) Δ LnUrb	(3) Δ LnRur	(4) Δ LnEarly	(5) Δ LnLate	(6) Δ PctUrb
I(Year='00)						
x PctEjido90	-0.00209***	-0.00344***	-0.000805	-0.000273	-0.00102	-0.0540***
	(0.000385)	(0.000744)	(0.00107)	(0.00245)	(0.00201)	(0.0205)
x PctEarly90	-0.00190***	-0.000623	-0.00408**	-0.00106	0.000182	0.0396
	(0.000577)	(0.001000)	(0.00171)	(0.00278)	(0.00440)	(0.0274)
R-squared	0.220	0.156	0.169	0.120	0.129	0.0947
Observations	2378	2378	2225	1374	1043	2378
Mean Dep Var	0.0915	0.106	0.159	-0.0813	-0.0780	0.570
Mean PctRur90	43.80	43.80	46.81	49.37	52.82	43.80
Mean PctEjido90	9.604	9.604	9.598	9.964	10.71	9.604
Mean PctEarly90	4.872	4.872	4.976	6.550	4.660	4.872

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Notes: Robust standard errors. An observation is a municipality. Dependent variables in columns are differences from 1990 to 2000 in: log population, log urban population, log rural population, log population in early-certified ejidos, log population in late-certified ejidos, and the urban share of the population. Ejido and early-certified population shares are calculated as a percentage of the total municipality population. All specifications control for heterogeneous time trends associated with ejido populations (shown), and state-specific trends (not shown). **Sources**: Population census data for 1990 and 2000 from INEGI. Data on ejido certification from RAN.

Table 2.6: Municipio higher income populations decline

	(1) Δ LnLow	(2) Δ LnMid	(3) Δ LnHi	(4) Δ PctLow	(5) Δ PctMid	(6) Δ PctHi
I(Year='00)						
x PctEjido90	-0.000713	0.000428	0.00791	-0.00933	0.0325	-0.0232***
	(0.000910)	(0.00155)	(0.00766)	(0.0273)	(0.0262)	(0.00507)
x PctEarly90	0.00125	-0.00543**	-0.0145*	0.0786	-0.0488	-0.0298***
	(0.00136)	(0.00254)	(0.00878)	(0.0505)	(0.0490)	(0.00971)
R-squared	0.296	0.111	0.0655	0.112	0.0941	0.304
Observations	2376	2376	2275	2377	2377	2377
Mean Dep Var	0.149	0.428	0.452	-4.466	3.267	1.198
Mean PctRur90	43.83	43.83	44.57	43.82	43.82	43.82
Mean PctEjido90	9.595	9.603	9.165	9.608	9.608	9.608
Mean PctEarly90	4.858	4.876	4.796	4.874	4.874	4.874

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Notes: Robust standard errors. An observation is a municipality. Dependent variables in columns 1 to 3 are differences from 1990 to 2000 in log population 12+ years, employed, and with incomes that are: up to minimum wage; from 1 to 5 times the minimum wage; over 5 times the minimum wage. Dependent variables in columns 4 to 6 are differences from 1990 to 2000 in percentages of the population 12+ years, according to the same three categories. Ejido and early-certified population shares are calculated as a percentage of the total municipality population. All specifications control for heterogeneous time trends associated with ejido populations (shown), and state-specific trends (not shown). **Sources**: Population census data for 1990 and 2000 from INEGI; income data for 1990 and 2000 from IPUMS and INEGI. Data on ejido certification from RAN.

Table 2.7: Municipio aggregate employment and earnings do not change significantly

	(1) Δ LnEmp1	(2) Δ LnEmp2	(3) Δ LnEmp3	(4) Δ LnInc1	(5) Δ LnInc2	(6) Δ LnInc3
I(Year='00)						
x PctEjido90	0.000375	0.00420**	0.000466	-0.000515	0.00233	0.000719
	(0.000831)	(0.00191)	(0.00146)	(0.00336)	(0.00258)	(0.00288)
x PctEarly90	-0.000817	0.000523	0.000702	-0.00493	0.00550	0.00310
	(0.00131)	(0.00278)	(0.00183)	(0.00574)	(0.00448)	(0.00416)
R-squared	0.234	0.0946	0.123	0.0422	0.0450	0.0258
Observations	2210	2210	2210	2210	2210	2210
Mean Dep Var	-0.0373	0.548	0.660	-5.891	-5.079	-4.860
Mean PctRur90	44.64	44.64	44.64	44.64	44.64	44.64
Mean PctEjido90	9.157	9.157	9.157	9.157	9.157	9.157
Mean PctEarly90	4.875	4.875	4.875	4.875	4.875	4.875

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Notes: Robust standard errors. An observation is a municipality. Dependent variables are differences from 1990 to 2000 in: columns (1) through (3) are log employment in each sector, while columns (4) through (6) contain log aggregate income. The sectors are primary (agriculture), secondary (construction, mining, manufacturing, electricity), and tertiary sectors (commerce, communication, transportation, services, public administration, defense). Ejido and early-certified population shares are calculated as a percentage of the total municipality population. All specifications control for heterogeneous time trends associated with ejido populations (shown), and state-specific trends (not shown). **Sources**: Population census data for 1990 and 2000 from INEGI; income data for 1990 and 2000 from IPUMS and INEGI. Data on ejido certification from RAN.

Table 2.8: Municipio economies become relatively less agricultural

	(1) Δ PctEmp1	(2) Δ PctEmp2	(3) Δ PctEmp3	(4) Δ PctInc1	(5) Δ PctInc2	(6) Δ PctInc3
I(Year='00)						
x PctEjido90	-0.0647**	0.0868***	-0.0236	-0.147**	0.141***	0.00619
	(0.0290)	(0.0208)	(0.0187)	(0.0616)	(0.0513)	(0.0612)
x PctEarly90	-0.164***	0.0835**	0.0893***	-0.249**	0.115*	0.134
	(0.0444)	(0.0334)	(0.0278)	(0.101)	(0.0679)	(0.104)
R-squared	0.131	0.149	0.144	0.0504	0.0616	0.0236
Observations	2210	2210	2210	2210	2210	2210
Mean Dep Var	-11.38	3.726	8.516	-15.24	2.482	12.76
Mean PctRur90	44.64	44.64	44.64	44.64	44.64	44.64
Mean PctEjido90	9.157	9.157	9.157	9.157	9.157	9.157
Mean PctEarly90	4.875	4.875	4.875	4.875	4.875	4.875

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Notes: Robust standard errors. An observation is a municipality. Dependent variables are differences from 1990 to 2000 in: columns (1) through (3) are the percentages of employed persons in each sector, while columns (4) through (6) contain percentages of aggregate income. The sectors are primary (agriculture), secondary (construction, mining, manufacturing, electricity), and tertiary sectors (commerce, communication, transportation, services, public administration, defense). Ejido and early-certified population shares are calculated as a percentage of the total municipality population. All specifications control for heterogeneous time trends associated with ejido populations (shown), and state-specific trends (not shown). **Sources**: Population census data for 1990 and 2000 from INEGI; income data for 1990 and 2000 from IPUMS and INEGI. Data on ejido certification from RAN.

Table 2.9: Municipio wages do not change significantly

	(1) Δ LnWage	(2) Δ LnWage1	(3) Δ LnWage2	(4) Δ LnWage3
I(Year='00)				
x PctEjido90	0.0000899	-0.000890	-0.00187	0.000254
	(0.00193)	(0.00319)	(0.00226)	(0.00266)
x PctEarly90	0.000884	-0.00411	0.00498	0.00240
	(0.00286)	(0.00561)	(0.00335)	(0.00368)
R-squared	0.0433	0.0426	0.0334	0.0348
Observations	2210	2210	2210	2210
Mean Dep Var	-5.573	-5.854	-5.626	-5.521
Mean PctRur90	44.64	44.64	44.64	44.64
Mean PctEjido90	9.157	9.157	9.157	9.157
Mean PctEarly90	4.875	4.875	4.875	4.875

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Notes: Robust standard errors. An observation is a municipality. Dependent variables are differences from 1990 to 2000 in: log earnings per employee, and log earnings per employee in the primary (agriculture), secondary (construction, mining, manufacturing, electricity), and tertiary sectors (commerce, communication, transportation, services, public administration, defense). Ejido and early-certified population shares are calculated as a percentage of the total municipality population. All specifications control for heterogeneous time trends associated with ejido populations (shown), and state-specific trends (not shown). **Sources**: Population census data for 1990 and 2000 from INEGI; income data for 1990 and 2000 from IPUMS and INEGI. Data on ejido certification from RAN.

Heterogeneity by subgroup

Table 2.10: Municipio sectoral aggregates, by subgroup

	(1) Δ LnEmp1	(2) Δ LnEmp2	(3) Δ LnEmp3	(4) Δ LnInc1	(5) Δ LnInc2	(6) Δ LnInc3
I(Year='00)			*Rural areas*			
x PctEjido90	0.000921	0.00418	0.000717	0.000240	0.00279	0.00317
	(0.00135)	(0.00292)	(0.00212)	(0.00354)	(0.00349)	(0.00307)
x PctEarlyCert90	-0.00433**	-0.00335	-0.00699**	-0.0122*	-0.000527	-0.00612
	(0.00200)	(0.00447)	(0.00301)	(0.00669)	(0.00664)	(0.00500)
Observations	2059	2022	2034	2044	1972	1982
R-squared	0.233	0.0790	0.119	0.0642	0.0571	0.0531
I(Year='00)			*Urban areas*			
x PctEjido90	0.000149	0.00426**	-0.000663	-0.00541	-0.000680	-0.00142
	(0.00124)	(0.00183)	(0.00159)	(0.00681)	(0.00446)	(0.00469)
x PctEarlyCert90	-0.00143	-0.000134	-0.000687	-0.00555	0.00313	0.000326
	(0.00187)	(0.00326)	(0.00266)	(0.00808)	(0.00600)	(0.00596)
Observations	2210	2199	2209	1409	1408	1411
R-squared	0.0892	0.0851	0.0842	0.0575	0.0845	0.0472
I(Year='00)			*Male populations*			
x PctEjido90	-0.000810	0.00160	0.00133	-0.00104	-0.00112	0.00353
	(0.00100)	(0.00251)	(0.00184)	(0.00339)	(0.00302)	(0.00357)
x PctEarlyCert90	-0.00169	0.00225	-0.000683	-0.00646	0.00529	0.00237
	(0.00147)	(0.00325)	(0.00283)	(0.00581)	(0.00447)	(0.00552)
Observations	2210	2194	2196	2210	2191	2192
R-squared	0.127	0.0523	0.0687	0.0411	0.0363	0.0325
I(Year='00)			*Female populations*			
x PctEjido90	0.00478	-0.00553	0.00175	0.00783	-0.00528	0.00392
	(0.00515)	(0.00421)	(0.00304)	(0.0105)	(0.00591)	(0.00486)
x PctEarlyCert90	-0.0141*	0.00374	-0.00113	-0.0318**	0.00394	-0.00370
	(0.00811)	(0.00656)	(0.00370)	(0.0155)	(0.0106)	(0.00634)
Observations	1644	1773	2135	1383	1742	2117
R-squared	0.175	0.0648	0.0680	0.0719	0.0655	0.0194

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Notes: Robust standard errors. See Table 2.7 for additional notes.

Table 2.11: Municipio sectoral shares, by subgroup

	(1) Δ PctEmp1	(2) Δ PctEmp2	(3) Δ PctEmp3	(4) Δ PctInc1	(5) Δ PctInc2	(6) Δ PctInc3
I(Year='00)			*Rural areas*			
x PctEjido90	-0.0253	0.0574	-0.0464**	-0.107*	0.112*	-0.00484
	(0.0414)	(0.0368)	(0.0228)	(0.0644)	(0.0588)	(0.0660)
x PctEarlyCert90	-0.0120	0.0170	0.000791	-0.140	0.0815	0.0589
	(0.0655)	(0.0518)	(0.0319)	(0.109)	(0.0870)	(0.116)
Observations	2064	2064	2064	1928	1928	1928
R-squared	0.111	0.0941	0.107	0.0354	0.0577	0.0197
I(Year='00)			*Urban areas*			
x PctEjido90	-0.0777**	0.0849***	-0.0149	-0.228	0.142*	0.0856
	(0.0335)	(0.0220)	(0.0279)	(0.143)	(0.0810)	(0.123)
x PctEarlyCert90	-0.107**	0.0343	0.0947**	-0.284*	0.133	0.151
	(0.0521)	(0.0362)	(0.0380)	(0.162)	(0.115)	(0.148)
Observations	2210	2210	2210	1405	1405	1405
R-squared	0.0800	0.137	0.0748	0.0814	0.0876	0.0520
I(Year='00)			*Male populations*			
x PctEjido90	-0.0661*	0.0616**	0.00446	-0.104	0.0620	0.0422
	(0.0380)	(0.0274)	(0.0199)	(0.0685)	(0.0632)	(0.0579)
x PctEarlyCert90	-0.127**	0.0913**	0.0354	-0.375***	0.192**	0.183**
	(0.0563)	(0.0434)	(0.0324)	(0.0970)	(0.0871)	(0.0866)
Observations	2210	2210	2210	2175	2175	2175
R-squared	0.0743	0.0819	0.0827	0.0453	0.0534	0.0295
I(Year='00)			*Female populations*			
x PctEjido90	0.00567	-0.00828	0.00262	-0.150	-0.138	0.287
	(0.0751)	(0.0691)	(0.0798)	(0.154)	(0.107)	(0.188)
x PctEarlyCert90	0.126	0.0819	-0.208*	-0.211	0.319**	-0.108
	(0.117)	(0.0966)	(0.119)	(0.194)	(0.150)	(0.242)
Observations	2176	2176	2176	1215	1215	1215
R-squared	0.0897	0.0510	0.0701	0.0524	0.0716	0.0742

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Notes: Robust standard errors. See Table 2.8 for additional notes.

Table 2.12: Municipio sectoral wages, by subgroup

	(1) Δ LnWage	(2) Δ LnWage1	(3) Δ LnWage2	(4) Δ LnWage3
I(Year='00)		*Rural areas*		
x PctEjido90	0.00262	0.000317	-0.000583	0.00368
	(0.00206)	(0.00322)	(0.00246)	(0.00276)
x PctEarlyCert90	-0.00127	-0.00607	0.00297	-0.000129
	(0.00322)	(0.00588)	(0.00391)	(0.00396)
Observations	2144	2128	2073	2075
R-squared	0.0525	0.0503	0.0386	0.0445
I(Year='00)		*Urban areas*		
x PctEjido90	-0.00322	-0.00310	-0.00731**	-0.00420
	(0.00382)	(0.00648)	(0.00340)	(0.00382)
x PctEarlyCert90	0.00101	-0.00587	0.00587	0.00388
	(0.00469)	(0.00760)	(0.00459)	(0.00511)
Observations	1412	1409	1408	1411
R-squared	0.0600	0.0548	0.0611	0.0560
I(Year='00)		*Male populations*		
x PctEjido90	0.000157	-0.000229	-0.00262	0.00231
	(0.00207)	(0.00321)	(0.00219)	(0.00345)
x PctEarlyCert90	0.00593	-0.00477	0.00351	0.00365
	(0.00579)	(0.00573)	(0.00356)	(0.00488)
Observations	2210	2210	2191	2192
R-squared	0.0364	0.0370	0.0246	0.0302
I(Year='00)		*Female populations*		
x PctEjido90	-0.00207	-0.00195	-0.000919	0.00157
	(0.00315)	(0.00901)	(0.00440)	(0.00395)
x PctEarlyCert90	-0.000395	-0.0119	0.00222	-0.00222
	(0.00481)	(0.0130)	(0.00745)	(0.00503)
Observations	2164	1383	1742	2117
R-squared	0.0456	0.0593	0.0606	0.0278

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Notes: Robust standard errors. See Table 2.9 for additional notes.

Municipality manufacturing capitals

Table 2.13: Municipio populations, heterogeneity by manufacturing capital

	(1) Δ LnPop	(2) Δ LnUrb	(3) Δ LnRur	(4) Δ PctUrb
I(Year='00)				
x PctEjido90	-0.00201***	-0.00338***	-0.000625	-0.0549***
	(0.000275)	(0.000535)	(0.000739)	(0.00913)
x PctEarly90	-0.00175*	-0.000514	-0.00371*	0.0381***
	(0.00103)	(0.000754)	(0.00182)	(0.0129)
x I(MostMfg90)	0.0244	-0.00706	-0.224	-1.020
	(0.0456)	(0.0504)	(0.394)	(1.129)
I(Year='00) x StatePctEarly90				
x I(MostMfg90)	0.0312**	0.0296**	0.148	-0.107
	(0.0126)	(0.0129)	(0.127)	(0.337)
R-squared	0.226	0.158	0.177	0.0952
Observations	2378	2378	2225	2378
Clusters	32	32	32	32
Mean Dep Var	0.0915	0.106	0.159	0.570
Mean PctRur90	43.80	43.80	46.81	43.80
Mean PctEjido90	9.604	9.604	9.598	9.604
Mean PctEarly90	4.872	4.872	4.976	4.872

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Notes: Standard errors clustered by state. Tests of heterogeneity with respect to an indicator variable identifying the single municipality with the largest employment in a given sector, interacted with the early-certified share of the population at the state-level. See Table 2.5 for additional notes.

Table 2.14: Municipio sectoral aggregates, heterogeneity by manufacturing capital

	(1)	(2)	(3)	(4)	(5)	(6)
	Δ LnEmp1	Δ LnEmp2	Δ LnEmp3	Δ LnInc1	Δ LnInc2	Δ LnInc3
I(Year='00)						
x PctEjido90	0.000338	0.00415***	0.000401	-0.000392	0.00237	0.000714
	(0.00120)	(0.00109)	(0.00169)	(0.00158)	(0.00172)	(0.00181)
x PctEarly90	-0.000846	0.000464	0.000606	-0.00466	0.00559	0.00314
	(0.00166)	(0.00276)	(0.00202)	(0.00644)	(0.00565)	(0.00434)
x I(MostMfg90)	-0.245***	-0.179**	-0.163**	-0.216	-0.114	-0.261***
	(0.0643)	(0.0854)	(0.0669)	(0.178)	(0.105)	(0.0912)
I(Year='00) x StatePctEarly90						
x I(MostMfg90)	0.0605**	0.0368	0.0254	0.111**	0.0489	0.0772***
	(0.0235)	(0.0260)	(0.0197)	(0.0413)	(0.0314)	(0.0254)
R-squared	0.237	0.0951	0.125	0.0430	0.0452	0.0263
Observations	2210	2210	2210	2210	2210	2210
Clusters	32	32	32	32	32	32
Mean Dep Var	-0.0373	0.548	0.660	-5.891	-5.079	-4.860
Mean PctRur90	44.64	44.64	44.64	44.64	44.64	44.64
Mean PctEjido90	9.157	9.157	9.157	9.157	9.157	9.157
Mean PctEarly90	4.875	4.875	4.875	4.875	4.875	4.875

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Notes: Standard errors clustered by state. Tests of heterogeneity with respect to an indicator variable identifying the single municipality with the largest employment in a given sector, interacted with the early-certified share of the population at the state-level. See Table 2.7 for additional notes.

Table 2.15: Municipio sectoral shares, heterogeneity by manufacturing capital

	(1) Δ PctEmp1	(2) Δ PctEmp2	(3) Δ PctEmp3	(4) Δ PctInc1	(5) Δ PctInc2	(6) Δ PctInc3
I(Year='00)						
x PctEjido90	-0.0585	0.0834***	-0.0268	-0.139***	0.139***	0.000699
	(0.0366)	(0.0233)	(0.0199)	(0.0347)	(0.0263)	(0.0408)
x PctEarly90	-0.153***	0.0778**	0.0840***	-0.236***	0.110	0.125*
	(0.0505)	(0.0358)	(0.0239)	(0.0828)	(0.0699)	(0.0680)
x I(MostMfg90)	7.968***	-4.836***	-4.313***	10.34***	-1.635	-8.703***
	(1.901)	(1.661)	(1.024)	(2.165)	(1.854)	(2.461)
I(Year='00) x StatePctEarly90						
x I(MostMfg90)	-0.121	0.201	0.125	-0.102	-0.496	0.598
	(0.657)	(0.542)	(0.299)	(0.555)	(0.687)	(0.693)
R-squared	0.141	0.154	0.152	0.0549	0.0623	0.0263
Observations	2210	2210	2210	2210	2210	2210
Clusters	32	32	32	32	32	32
Mean Dep Var	-11.38	3.726	8.516	-15.24	2.482	12.76
Mean PctRur90	44.64	44.64	44.64	44.64	44.64	44.64
Mean PctEjido90	9.157	9.157	9.157	9.157	9.157	9.157
Mean PctEarly90	4.875	4.875	4.875	4.875	4.875	4.875

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Notes: Standard errors clustered by state. Tests of heterogeneity with respect to an indicator variable identifying the single municipality with the largest employment in a given sector, interacted with the early-certified share of the population at the state-level. See Table 2.8 for additional notes.

Table 2.16: Municipio sectoral wages, heterogeneity by manufacturing capital

	(1) Δ LnWage	(2) Δ LnWage1	(3) Δ LnWage2	(4) Δ LnWage3
I(Year='00)				
x PctEjido90	0.000168	-0.000730	-0.00178	0.000313
	(0.00109)	(0.00143)	(0.00153)	(0.00165)
x PctEarly90	0.00104	-0.00381	0.00513	0.00253
	(0.00378)	(0.00599)	(0.00356)	(0.00364)
x I(MostMfg90)	-0.0359	0.0291	0.0646	-0.0981*
	(0.0481)	(0.174)	(0.0581)	(0.0526)
I(Year='00) x StatePctEarly90				
x I(MostMfg90)	0.0398***	0.0506	0.0121	0.0518***
	(0.0117)	(0.0387)	(0.0153)	(0.0176)
R-squared	0.0438	0.0433	0.0337	0.0352
Observations	2210	2210	2210	2210
Clusters	32	32	32	32
Mean Dep Var	-5.573	-5.854	-5.626	-5.521
Mean PctRur90	44.64	44.64	44.64	44.64
Mean PctEjido90	9.157	9.157	9.157	9.157
Mean PctEarly90	4.875	4.875	4.875	4.875

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Notes: Standard errors clustered by state. Tests of heterogeneity with respect to an indicator variable identifying the single municipality with the largest employment in a given sector, interacted with the early-certified share of the population at the state-level. See Table 2.9 for additional notes.

State-level analysis

Table 2.17: State-level urbanization increases

	(1) LnPop	(2) LnUrb	(3) LnRur	(4) LnEarly	(5) LnLate	(6) PctUrb
	Analytical standard errors in parentheses					
I(Year='00)	0.461***	0.501***	0.384***	0.0261	-0.0253	5.223***
	(0.0502)	(0.0512)	(0.120)	(0.160)	(0.127)	(0.849)
I(Year='00)xPctRur90	-0.00429	-0.00254	-0.00583	-0.00223	0.00377	0.0264
	(0.00271)	(0.00292)	(0.00434)	(0.00525)	(0.00532)	(0.0572)
I(Year='00)xPctEjido90	0.0134	0.00412	0.0386	0.0219	-0.0127	-0.614
	(0.0160)	(0.0173)	(0.0259)	(0.0285)	(0.0291)	(0.366)
I(Year='00)xPctEarlyCert90	-0.0253	-0.00665	-0.0499*	-0.00478	0.0135	1.203***
	(0.0164)	(0.0174)	(0.0290)	(0.0508)	(0.0525)	(0.317)
	Wild bootstrap p-values in brackets					
I(Year='00)	0.461***	0.501***	0.384**	0.0261	-0.0253	5.223***
	[0.002]	[0.002]	[0.020]	[0.933]	[0.827]	[0.002]
I(Year='00)xPctRur90	-0.00429*	-0.00254	-0.00583	-0.00223	0.00377	0.0264
	[0.080]	[0.466]	[0.216]	[0.679]	[0.486]	[0.689]
I(Year='00)xPctEjido90	0.0134	0.00412	0.0386	0.0219	-0.0127	-0.614
	[0.456]	[0.849]	[0.136]	[0.515]	[0.629]	[0.154]
I(Year='00)xPctEarlyCert90	-0.0253	-0.00665	-0.0499	-0.00478	0.0135	1.203***
	[0.176]	[0.749]	[0.168]	[0.931]	[0.855]	[0.008]
R-squared	0.585	0.575	0.429	0.0244	0.0249	0.452
Observations	93	93	93	93	93	93
Mean Dep Var	14.36	13.97	12.91	10.07	8.957	69.10
Mean PctRural90	27.32	27.32	27.32	27.32	27.32	27.32
Mean PctEjido90	4.954	4.954	4.954	4.954	4.954	4.954
Mean PctEarlyCert90	3.099	3.099	3.099	3.099	3.099	3.099

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Notes: Standard errors and wild bootstrap p-values clustered by state. An observation is a state-year. Dependent variables in columns are log population, log urban population, log rural population, log population in early-certified ejidos, log population in late-certified ejidos, and the urban share of the population. Ejido and early-certified population shares are calculated as a percentage of the total state population. All specifications control for year-fixed effects and heterogeneous time trends associated with rural and ejido populations (shown), and municipality and state-fixed effects (not shown). For each presented variable, clustered wild bootstrap p-values are based on Cameron, Gelbach, and Miller (2008) with 1000 replications and Rademacher weights. **Sources**: Population census data for 1990 and 2000 from INEGI. Data on ejido certification from RAN.

Table 2.18: State-level agricultural output declines

	(1) LnEmp1	(2) LnEmp2	(3) LnEmp3	(4) LnGDP1	(5) LnGDP2	(6) LnGDP3
	Analytical standard errors in parentheses					
I(Year='00)	-0.0457	0.477***	0.513***	0.139**	0.508***	0.341***
	(0.0999)	(0.0585)	(0.0450)	(0.0611)	(0.0581)	(0.0293)
I(Year='00)xPctRur90	-0.00164	-0.00513***	0.000487	0.00130	-0.00111	-0.00234**
	(0.00350)	(0.00166)	(0.00196)	(0.00233)	(0.00289)	(0.00102)
I(Year='00)xPctEjido90	0.0411*	0.0339***	0.0112	0.00455	-0.0354**	-0.0134**
	(0.0205)	(0.00846)	(0.0122)	(0.0123)	(0.0162)	(0.00606)
I(Year='00)xPctEarlyCert90	-0.0543	-0.0152	-0.00492	-0.0355**	0.00771	0.00419
	(0.0328)	(0.0142)	(0.0167)	(0.0131)	(0.0134)	(0.00810)
	Wild bootstrap p-values in brackets					
I(Year='00)	-0.0457	0.477***	0.513***	0.139*	0.508***	0.341***
	[0.669]	[0.002]	[0.002]	[0.078]	[0.002]	[0.002]
I(Year='00)xPctRur90	-0.00164	-0.00513**	0.000487	0.00130	-0.00111	-0.00234*
	[0.641]	[0.016]	[0.799]	[0.655]	[0.711]	[0.064]
I(Year='00)xPctEjido90	0.0411	0.0339**	0.0112	0.00455	-0.0354*	-0.0134*
	[0.110]	[0.012]	[0.392]	[0.715]	[0.080]	[0.098]
I(Year='00)xPctEarlyCert90	-0.0543	-0.0152	-0.00492	-0.0355**	0.00771	0.00419
	[0.290]	[0.276]	[0.811]	[0.016]	[0.545]	[0.663]
R-squared	0.203	0.946	0.965	0.362	0.859	0.930
Observations	62	62	62	62	62	62
Mean Dep Var	11.65	11.94	12.54	14.43	15.63	16.57
Mean PctRural90	27.32	27.32	27.32	27.32	27.32	27.32
Mean PctEjido90	4.954	4.954	4.954	4.954	4.954	4.954
Mean PctEarlyCert90	3.099	3.099	3.099	3.099	3.099	3.099

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Notes: Standard errors and wild bootstrap p-values clustered by state. An observation is a state-year. Dependent variables in columns (1) through (3) are log employment in each sector, while columns (4) through (6) contain log GDP. The sectors are primary (agriculture), secondary (construction, mining, manufacturing, electricity), and tertiary sectors (commerce, communication, transportation, services, public administration, defense). Ejido and early-certified population shares are calculated as a percentage of the total state population. The state-level GDP values are based on data for 1993 and 2000, while the population values are available for census years 1990 and 2000. All specifications control for year-fixed effects and heterogeneous time trends associated with rural and ejido populations (shown), and state-fixed effects (not shown). For each presented variable, clustered wild bootstrap p-values are based on Cameron, Gelbach, and Miller (2008) with 1000 replications and Rademacher weights. **Sources**: Population census data for 1990 and 2000, and national accounts data for 1993 and 2000, from INEGI. Data on ejido certification from RAN.

Table 2.19: State economies become relatively less agricultural

	(1) PctEmp1	(2) PctEmp2	(3) PctEmp3	(4) PctGDP1	(5) PctGDP2	(6) PctGDP3
	Analytical standard errors in parentheses					
I(Year='00)	-2.075**	-0.301	1.912	-1.244**	3.797***	-2.554***
	(0.878)	(1.677)	(1.557)	(0.476)	(0.860)	(0.659)
I(Year='00)xPctRur90	-0.118***	-0.0256	0.158***	0.00909	0.0156	-0.0246
	(0.0371)	(0.0393)	(0.0364)	(0.0194)	(0.0480)	(0.0357)
I(Year='00)xPctEjido90	-0.0383	0.318*	-0.0968	0.213*	-0.498*	0.286
	(0.240)	(0.180)	(0.186)	(0.105)	(0.278)	(0.206)
I(Year='00)xPctEarlyCert90	-1.041***	0.330	0.633*	-0.354***	0.189	0.165
	(0.373)	(0.312)	(0.325)	(0.125)	(0.209)	(0.173)
	Wild bootstrap p-values in brackets					
I(Year='00)	-2.075*	-0.301	1.912	-1.244**	3.797***	-2.554***
	[0.056]	[0.877]	[0.264]	[0.014]	[0.006]	[0.006]
I(Year='00)xPctRur90	-0.118***	-0.0256	0.158***	0.00909	0.0156	-0.0246
	[0.002]	[0.549]	[0.008]	[0.705]	[0.783]	[0.484]
I(Year='00)xPctEjido90	-0.0383	0.318	-0.0968	0.213*	-0.498	0.286
	[0.845]	[0.170]	[0.615]	[0.082]	[0.120]	[0.220]
I(Year='00)xPctEarlyCert90	-1.041**	0.330	0.633	-0.354**	0.189	0.165
	[0.050]	[0.366]	[0.122]	[0.016]	[0.330]	[0.330]
R-squared	0.975	0.378	0.913	0.535	0.579	0.444
Observations	62	62	62	62	62	62
Mean Dep Var	22.78	26.84	47.33	9.296	26.79	63.92
Mean PctRural90	27.32	27.32	27.32	27.32	27.32	27.32
Mean PctEjido90	4.954	4.954	4.954	4.954	4.954	4.954
Mean PctEarlyCert90	3.099	3.099	3.099	3.099	3.099	3.099

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Notes: Standard errors and wild bootstrap p-values clustered by state. An observation is a state-year. Dependent variables in columns (1) through (3) are the percentages of employed persons in each sector, while columns (4) through (6) contain percentages of GDP. The sectors are primary (agriculture), secondary (construction, mining, manufacturing, electricity), and tertiary sectors (commerce, communication, transportation, services, public administration, defense). Ejido and early-certified population shares are calculated as a percentage of the total state population. The state-level GDP values are based on data for 1993 and 2000, while the population values are available for census years 1990 and 2000. All specifications control for year-fixed effects and heterogeneous time trends associated with rural and ejido populations (shown), and state-fixed effects (not shown). For each presented variable, clustered wild bootstrap p-values are based on Cameron, Gelbach, and Miller (2008) with 1000 replications and Rademacher weights. **Sources**: Population census data for 1990 and 2000, and national accounts data for 1993 and 2000, from INEGI. Data on ejido certification from RAN.

Table 2.20: State-level sectoral productivities do not change significantly

	(1) LnGDPpc	(2) LnGDP1pe	(3) LnGDP2pe	(4) LnGDP3pe
	Analytical standard errors in parentheses			
I(Year='00)	0.0667*	0.184	0.0313	-0.172***
	(0.0386)	(0.127)	(0.0488)	(0.0598)
I(Year='00)xPctRur90	0.00169	0.00293	0.00402	-0.00283
	(0.00204)	(0.00424)	(0.00312)	(0.00213)
I(Year='00)xPctEjido90	-0.0239**	-0.0365	-0.0693***	-0.0246**
	(0.0113)	(0.0249)	(0.0179)	(0.0119)
I(Year='00)xPctEarlyCert90	0.0148	0.0188	0.0230	0.00911
	(0.00951)	(0.0404)	(0.0164)	(0.0155)
	Wild bootstrap p-values in brackets			
I(Year='00)	0.0667	0.184	0.0313	-0.172**
	[0.114]	[0.210]	[0.519]	[0.020]
I(Year='00)xPctRur90	0.00169	0.00293	0.00402	-0.00283
	[0.422]	[0.547]	[0.256]	[0.290]
I(Year='00)xPctEjido90	-0.0239*	-0.0365	-0.0693***	-0.0246*
	[0.088]	[0.216]	[0.006]	[0.058]
I(Year='00)xPctEarlyCert90	0.0148	0.0188	0.0230	0.00911
	[0.192]	[0.623]	[0.226]	[0.641]
R-squared	0.253	0.336	0.638	0.898
Observations	62	62	62	62
Mean Dep Var	2.535	2.778	3.690	4.030
Mean PctRural90	27.32	27.32	27.32	27.32
Mean PctEjido90	4.954	4.954	4.954	4.954
Mean PctEarlyCert90	3.099	3.099	3.099	3.099

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Notes: Standard errors and wild bootstrap p-values clustered by state. An observation is a state-year. Dependent variables in columns are: log GDP per capita, and log GDP per employee in the primary (agriculture), secondary (construction, mining, manufacturing, electricity), and tertiary sectors (commerce, communication, transportation, services, public administration, defense). Ejido and early-certified population shares are calculated as a percentage of the total state population. The state-level GDP values are based on data for 1993 and 2000, while the population values are available for census years 1990 and 2000. All specifications control for year-fixed effects and heterogeneous time trends associated with rural and ejido populations (shown), and state-fixed effects (not shown). For each presented variable, clustered wild bootstrap p-values are based on Cameron, Gelbach, and Miller (2008) with 1000 replications. **Sources**: Population census data for 1990 and 2000, and national accounts data for 1993 and 2000, from INEGI. Data on ejido certification from RAN.

Placebo tests

Table 2.21: Municipio results supported by PLACEBO test prior to certification

	(1) Δ LnPop	(2) Δ LnUrb	(3) Δ LnRur	(4) Δ LnEarly	(5) Δ LnLate	(6) Δ PctUrb
I(Year='90)						
x PctEjido90	-0.00410***	-0.00568***	-0.00367***	-0.00372	-0.0000815	-0.104***
	(0.000670)	(0.00120)	(0.00129)	(0.00270)	(0.00311)	(0.0366)
x PctEarly90	-0.00136	-0.00102	0.000332	0.00276	-0.00357	-0.0404
	(0.00126)	(0.00167)	(0.00210)	(0.00326)	(0.00635)	(0.0688)
R-squared	0.162	0.100	0.145	0.153	0.133	0.103
Observations	2371	2368	2145	1192	870	2371
Mean Dep Var	0.169	0.226	0.181	0.204	0.225	2.767
Mean PctRur90	43.79	43.79	48.08	50.82	54.13	43.79
Mean PctEjido90	9.587	9.584	9.538	10.06	10.87	9.587
Mean PctEarly90	4.874	4.874	4.951	6.612	4.771	4.874

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Notes: Robust standard errors. An observation is a municipality. Dependent variables in columns are differences from 1980 to 1990 in log population, log urban population, log rural population, log population in early-certified ejidos, log population in late-certified ejidos, and the urban share of the population. Ejido and early-certified population shares are calculated as a percentage of the total municipality population. **Prior to certification, there is no significant trend in population associated with early certification for any population variable of interest.** All specifications control for heterogeneous time trends associated with ejido populations (shown), and state-specific trends (not shown). **Sources**: Population census data for 1980 and 1990 from INEGI. Data on ejido certification from RAN.

Table 2.22: State-level results supported by PLACEBO test prior to certification

	(1) LnPop	(2) LnUrb	(3) LnRur	(4) LnEarly	(5) LnLate	(6) PctUrb
	Analytical standard errors in parentheses					
I(Year='90)	-0.0206	0.0229	-0.0283	0.154**	0.198***	3.759***
	(0.0154)	(0.0246)	(0.0231)	(0.0637)	(0.0671)	(1.337)
I(Year='90)xPctRur90	0.00125**	0.00151*	0.00243***	-0.000342	-0.000249	-0.0135
	(0.000588)	(0.000811)	(0.000834)	(0.00226)	(0.00166)	(0.0396)
I(Year='90)xPctEjido90	0.00545	0.00208	0.00243	0.00536	0.00192	-0.196
	(0.00426)	(0.00360)	(0.00837)	(0.0139)	(0.0104)	(0.238)
I(Year='90)xPctEarlyCert90	-0.00709	-0.00815*	-0.00952	-0.00208	-0.00584	-0.166
	(0.00564)	(0.00414)	(0.0110)	(0.0285)	(0.0211)	(0.289)
	Wild bootstrap p-values in brackets					
I(Year='90)	-0.0206	0.0229	-0.0283	0.154**	0.198***	3.759***
	[0.180]	[0.382]	[0.244]	[0.014]	[0.008]	[0.008]
I(Year='90)xPctRur90	0.00125*	0.00151	0.00243***	-0.000342	-0.000249	-0.0135
	[0.064]	[0.110]	[0.008]	[0.859]	[0.915]	[0.757]
I(Year='90)xPctEjido90	0.00545	0.00208	0.00243	0.00536	0.00192	-0.196
	[0.296]	[0.585]	[0.827]	[0.733]	[0.865]	[0.432]
I(Year='90)xPctEarlyCert90	-0.00709	-0.00815	-0.00952	-0.00208	-0.00584	-0.166
	[0.316]	[0.146]	[0.414]	[0.979]	[0.775]	[0.657]
R-squared	0.00477	0.00899	0.00940	0.131	0.166	0.0426
Observations	93	93	93	93	93	93
Mean Dep Var	14.36	13.97	12.91	10.07	8.957	69.10
Mean PctRural90	27.32	27.32	27.32	27.32	27.32	27.32
Mean PctEjido90	4.954	4.954	4.954	4.954	4.954	4.954
Mean PctEarlyCert90	3.099	3.099	3.099	3.099	3.099	3.099

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Notes: Standard errors and wild bootstrap p-values clustered by state. An observation is a state-year. Dependent variables in columns are log population, log urban population, log rural population, log non-ejido population, log population in late-certified ejidos, and the urban share of the population. Ejido and early-certified population shares are calculated as a percentage of the total state population. **Prior to certification, there is no significant trend in population associated with early certification for any population variable of interest.** All specifications control for year-fixed effects and heterogeneous time trends associated with rural and ejido populations (shown), and municipality and state-fixed effects (not shown). For each presented variable, clustered wild bootstrap p-values are based on Cameron, Gelbach, and Miller (2008) with 1000 replications and Rademacher weights. **Sources**: Population census data for 1990 and 2000 from INEGI. Data on ejido certification from RAN.

Table 2.23: Municipio heterogeneity results supported by PLACEBO test prior to certification

	(1) Δ LnPop	(2) Δ LnUrb	(3) Δ LnRur	(4) Δ PctUrb
I(Year='90)				
x PctEjido90	-0.00405***	-0.00563***	-0.00365**	-0.103**
	(0.000481)	(0.000719)	(0.00141)	(0.0388)
x PctEarly90	-0.00128	-0.000936	0.000372	-0.0394
	(0.00130)	(0.00176)	(0.00158)	(0.0432)
x I(MostMfg90)	0.0458	-0.0111	0.0778	0.782
	(0.0582)	(0.0770)	(0.107)	(2.611)
I(Year='90) x StatePctEarly90				
x I(MostMfg90)	0.00831	0.0252	-0.0126	0.0306
	(0.0274)	(0.0246)	(0.0282)	(0.661)
R-squared	0.163	0.101	0.145	0.103
Observations	2371	2368	2145	2371
Clusters	32	32	31	32
Mean Dep Var	0.169	0.226	0.181	2.767
Mean PctRur90	43.79	43.79	48.08	43.79
Mean PctEjido90	9.587	9.584	9.538	9.587
Mean PctEarly90	4.874	4.874	4.951	4.874

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Notes: Standard errors clustered by state. See Table 2.21 for additional notes.

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Appendix A

Appendix to Chapter 1

A.1 Model and solutions

For a given firm, production takes the standard CES form $Q = F(\mathbf{X}) = A\left[\sum_{i=1}^{I} \alpha_i X_i^{\frac{\sigma-1}{\sigma}}\right]^{\nu\frac{\sigma}{\sigma-1}}$, with $\sigma > 0$ denoting the elasticity of inputs across the I inputs. Returns to scale are captured by $\nu > 0$, with $\nu = 1$ indicating constant returns to scale. The A coefficient captures Hicks-neutral total factor productivity. When $\sigma \to 1$, production converges to the Cobb-Douglas form, $F(\mathbf{X}) = A\left[\prod_{i=1}^{I} X_i^{\alpha_i}\right]^{\nu}$, with $\sum_{i=1}^{I} \alpha_i = 1$.

The partial derivatives of F with respect to input X_i are given by

$$F_i(\mathbf{X}) = \begin{cases} \nu A\left(\sum_{i=1}^{I} \alpha_i X_i^{\frac{\sigma-1}{\sigma}}\right)^{\nu\frac{\sigma}{\sigma-1}-1} \alpha_i X_i^{-1/\sigma} = \nu Q \alpha_i X_i^{-1/\sigma}\left(\sum_{i=1}^{I} \alpha_i X_i^{\frac{\sigma-1}{\sigma}}\right)^{-1} & , \sigma \neq 1 \\ \nu A\left(\prod_{i=1}^{I} X_i^{\alpha_i}\right)^{\nu} \alpha_i X_i^{-1} = \nu Q \alpha_i X_i^{-1} & , \sigma = 1 \end{cases} \tag{A.1}$$

while the output elasticities are given by

$$F_i(\mathbf{X})\frac{X}{Q} = \begin{cases} \nu \alpha_i X_i^{(\sigma-1)/\sigma}\left(\sum_{i=1}^{I} \alpha_i X_i^{\frac{\sigma-1}{\sigma}}\right)^{-1} & , \sigma \neq 1 \\ \nu \alpha_i & , \sigma = 1 \end{cases}$$

To ease notation in the following, let

$$\Phi = \begin{cases} \left(\sum_{i=1}^{I} \alpha_i^{\sigma} \omega_i^{1-\sigma}\right)^{\frac{1}{\sigma-1}} & , 0 < \sigma \neq 1 \\ \prod_{i=1}^{I}\left(\frac{\alpha_i}{\omega_i}\right)^{\alpha_i} & , \sigma = 1 \end{cases} \tag{A.2}$$

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

A.1.1 Cost minimization

Let ω_i denote the firm-specific price for each factor of production, which the firm treats as exogenous. For a given level of output $\bar{Q}$, the firm's cost minimization problem can be written as

$$\min_{\{X_i \geq 0\}} \sum_{i=1}^{I} \omega_i X_i + \lambda \left[\bar{Q} - F(\mathbf{X})\right] \tag{A.3}$$

The first order conditions for cost minimization imply that $F_i(\mathbf{X}) = \lambda \omega_i \; \forall i$. Taking the ratio of first order conditions for inputs m and k, we see that in an optimum, the relative factor proportions must satisfy $\frac{\omega_k}{\omega_m} = \frac{\alpha_k}{\alpha_m}\left(\frac{X_m}{X_k}\right)^{1/\sigma}$ for all $\sigma > 0$, or re-writing,

$$X_k \;\; = \;\; X_m \left(\frac{\omega_m}{\alpha_m}\right)^{\sigma} \left(\frac{\alpha_k}{\omega_k}\right)^{\sigma}, \; \sigma > 0 \tag{A.4}$$

It follows that cost-minimizing factor shares of total cost are constant for all levels of output and TFP levels,

$$\frac{\omega_i X_i^*}{\sum_{i=1}^{I} \omega_i X_i^*} = \frac{\omega_i \left[X_m \left(\frac{\omega_m}{\alpha_m}\right)^{\sigma} \left(\frac{\alpha_i}{\omega_i}\right)^{\sigma}\right]}{\sum_{i=1}^{I} \omega_i \left[X_m \left(\frac{\omega_m}{\alpha_m}\right)^{\sigma} \left(\frac{\alpha_i}{\omega_i}\right)^{\sigma}\right]} = \frac{\alpha_i^{\sigma} \omega_i^{1-\sigma}}{\sum_{i=1}^{I} \alpha_i^{\sigma} \omega_i^{1-\sigma}}, \; \sigma > 0 \tag{A.5}$$

In order to obtain factor demands, we can substitute for X_i in the production constraint:

$$\bar{Q} \equiv \begin{cases} A\left(\sum_{i=1}^{I} \alpha_i \left[X_m \left(\frac{\omega_m}{\alpha_m}\right)^{\sigma} \left(\frac{\alpha_i}{\omega_i}\right)^{\sigma}\right]^{\frac{\sigma-1}{\sigma}}\right)^{\nu \frac{\sigma}{\sigma-1}} = AX_m^{\nu} \left(\frac{\omega_m}{\alpha_m}\right)^{\nu\sigma} \left[\sum_{i=1}^{I} \alpha_i^{\sigma} \omega_i^{1-\sigma}\right]^{\nu \frac{\sigma}{\sigma-1}} & , \; 0 < \sigma \neq 1 \\ A\left[\prod_{i=1}^{I} X_m^{\alpha_i} \left(\frac{\omega_m}{\alpha_m}\right)^{\alpha_i} \left(\frac{\alpha_i}{\omega_i}\right)^{\alpha_i}\right]^{\nu} = AX_m^{\nu} \left(\frac{\omega_m}{\alpha_m}\right)^{\nu} \left[\prod_{i=1}^{n} \left(\frac{\alpha_i}{\omega_i}\right)^{\alpha_i}\right]^{\nu} & , \; \sigma = 1 \end{cases} \tag{A.6}$$

and then solve for X_m^*, finding:

$$X_m^* = \begin{cases} \left(\frac{\bar{Q}}{A}\right)^{\frac{1}{\nu}} \left(\frac{\alpha_m}{\omega_m}\right)^{\sigma} \left[\sum_{i=1}^{I} \alpha_i^{\sigma} \omega_i^{1-\sigma}\right]^{\frac{\sigma}{1-\sigma}} & , \; 0 < \sigma \neq 1 \\ \left(\frac{\bar{Q}}{A}\right)^{\frac{1}{\nu}} \left(\frac{\alpha_m}{\omega_m}\right) \prod_{i=1}^{I} \left(\frac{\omega_i}{\alpha_i}\right)^{\alpha_i} & , \; \sigma = 1 \end{cases} \tag{A.7}$$

or

$$X_m^* = Q^{\frac{1}{\nu}} A^{-\frac{1}{\nu}} \Phi^{-\sigma} \left(\frac{\alpha_m}{\omega_m}\right)^{\sigma} \tag{A.8}$$

In order to obtain the minimum cost function, we can substitute the optimal factor demands into the cost function such that $C(\bar{Q}, \boldsymbol{\omega}) = \sum_{i=1}^{n} \omega_i X_i^*$, or:

$$C(\bar{Q}, \boldsymbol{\omega}) = \begin{cases} \left(\frac{\bar{Q}}{A}\right)^{\frac{1}{\nu}} \left[\sum_{i=1}^{I} \alpha_i^{\sigma} \omega_i^{1-\sigma}\right]^{\frac{1}{1-\sigma}} & , \; 0 < \sigma \neq 1 \\ \left(\frac{\bar{Q}}{A}\right)^{\frac{1}{\nu}} \prod_{i=1}^{I} \left(\frac{\omega_i}{\alpha_i}\right)^{\alpha_i} & , \; \sigma = 1 \end{cases} \tag{A.9}$$

or more simply,

$$C(\bar{Q}, \boldsymbol{\omega}) = Q^{\frac{1}{\nu}} A^{-\frac{1}{\nu}} \Phi^{-1} \tag{A.10}$$

Then the marginal cost of output is given by

$$c(\bar{Q}, \boldsymbol{\omega}) = \begin{cases} \frac{1}{\nu} A^{-\frac{1}{\nu}} \left(\sum_{i=1}^{I} \alpha_i^{\sigma} \omega_i^{1-\sigma}\right)^{\frac{1}{1-\sigma}} \bar{Q}^{\frac{1-\nu}{\nu}} & , \; 0 < \sigma \neq 1 \\ \frac{1}{\nu} A^{-\frac{1}{\nu}} \prod_{i=1}^{I} \left(\frac{\omega_i}{\alpha_i}\right)^{\alpha_i} \bar{Q}^{\frac{1-\nu}{\nu}} & , \; \sigma = 1 \end{cases} \tag{A.11}$$

or more simply,

$$c(Q,\boldsymbol{\omega}) = \frac{1}{\nu} Q^{\frac{1-\nu}{\nu}} A^{-\frac{1}{\nu}} \Phi^{-1} \tag{A.12}$$

Thus, it can also be seen that the ratio of average cost to marginal cost is equal to the returns to scale, ν.

Finally, recalling that marginal products are given by equation (A.1), we can now define cost-minimizing marginal products. It can be shown that

$$\begin{aligned}
\left(\sum_{i=1}^{I} \alpha_i X_i^{\frac{\sigma-1}{\sigma}}\right)^{-1} &= A^{\frac{1}{\nu}} A^{-\frac{1}{\nu}\frac{1}{\sigma}} Q^{-\frac{1}{\nu}} Q^{\frac{1}{\nu}\frac{1}{\sigma}} \\
X_m^{-1} &= Q^{-\frac{1}{\nu}} A^{\frac{1}{\nu}} \Phi^{\sigma} \left(\frac{\omega_m}{\alpha_m}\right)^{\sigma} \\
X_m^{-\frac{1}{\sigma}} &= Q^{-\frac{1}{\nu}\frac{1}{\sigma}} A^{\frac{1}{\nu}\frac{1}{\sigma}} \Phi \left(\frac{\omega_m}{\alpha_m}\right) \\
X_m^{-\frac{1}{\sigma}} \left(\sum_{i=1}^{I} \alpha_i X_i^{\frac{\sigma-1}{\sigma}}\right)^{-1} &= A^{\frac{1}{\nu}} Q^{-\frac{1}{\nu}} \Phi \left(\frac{\omega_m}{\alpha_m}\right)
\end{aligned}$$

and thus, cost-minimizing marginal products are given by

$$F_m = \nu \omega_m A^{\frac{1}{\nu}} Q^{\frac{\nu-1}{\nu}} \Phi, \quad \forall m \tag{A.13}$$

while the cost-minimizing output elasticity is given by

$$\frac{\partial Q}{\partial X_m} \frac{X_m^*}{Q} = \nu \omega_m A^{\frac{1}{\nu}} Q^{-\frac{1}{\nu}} \Phi \left[Q^{\frac{1}{\nu}} A^{-\frac{1}{\nu}} \Phi^{-\sigma} \left(\frac{\alpha_m}{\omega_m}\right)^{\sigma} \right] = \nu \alpha_m^{\sigma} \omega_m^{1-\sigma} \Phi^{1-\sigma} \tag{A.14}$$

A.1.2 Profit maximization

Under price-taking behavior, firms take prices as given, i.e., $P(Q) = \bar{P}$. Alternatively, we may assume that firms face downward sloping demand curves. In particular, assume that demand is isoelastic with $Q(P) = \theta^{\epsilon} P^{-\epsilon}$ denoting the demand function, and $P(Q) = \theta Q^{-1/\epsilon}$ the inverse demand function, with $\epsilon > 1$. Notice we can treat the firm as choosing Q to maximize profits, with input demands then determined based on cost minimization. Thus, we write the firm's maximization problem as

$$\max_{Q \geq 0} P(Q)\, Q - C(Q) \tag{A.15}$$

Let $\varepsilon(Q) = -(\partial Q/\partial P)(P/Q)$, and let $\mu(Q) = \varepsilon(Q)/(\varepsilon(Q)-1)$. The first order condition requires $P(Q) + (\partial P/\partial Q)Q \equiv c(Q)$, or $P(Q)\,[1 - 1/\varepsilon(Q)] \equiv c(Q)$, or $P(Q) \equiv \mu(Q)c(Q)$.

In the competitive case, $\partial P/\partial Q = 0$ and we have that firms choose Q such that price equals marginal cost. In the monopolistic case, given isoelastic demand, $\varepsilon(Q) = \epsilon$ and $\mu(Q) = \mu = \epsilon/(\epsilon - 1)$. This implies the familiar constant markup over marginal cost condition and implicitly defines Q^* as that value such that $P(Q) \equiv \mu c(Q)$.

Price-taking. Cost-minimizing marginal costs are given by equation (A.11). Assuming $\nu < 1$, setting marginal cost equal to price and solving for quantity, optimal output levels are given by

$$Q^* = \begin{cases} A^\eta (\nu P)^{\nu\eta} \left[\left(\sum_{i=1}^I \alpha_i^\sigma \omega_i^{1-\sigma} \right)^{\frac{1}{\sigma-1}} \right]^{\nu\eta} & , 0 < \sigma \neq 1 \\ A^\eta (\nu P)^{\nu\eta} \left[\prod_{i=1}^I \left(\frac{\alpha_i}{\omega_i} \right)^{\alpha_i} \right]^{\nu\eta} & , \sigma = 1 \end{cases} \tag{A.16}$$

or, for all $\sigma > 0$,

$$Q^* = A^\eta \nu^{\nu\eta} P^{\nu\eta} \Phi^{\nu\eta} \tag{A.17}$$

with $\eta = \frac{1}{1-\nu}$. Notice that $\nu < 1$ implies $\eta > 1$, with $\eta \to \infty$ as $\nu \to 1$. As will be shown below, η can be seen as the inverse of the share of variable profits in revenue. Thus, as $\nu \to 1$ and $\eta \to \infty$, the variable profit share goes to 0, highlighting that decreasing returns to scale are a necessary condition for positive profits under price-taking.

Also notice that $\eta - 1 = \frac{1}{1-\nu} - \frac{1-\nu}{1-\nu} = \frac{\nu}{1-\nu} = \nu\eta$, such that $\nu\eta + 1 = \eta$. Using this fact, we see that optimal revenue, PQ^*, is given by

$$Y^* = \begin{cases} (AP)^\eta \nu^{\eta-1} \left[\left(\sum_{i=1}^I \alpha_i^\sigma \omega_i^{1-\sigma} \right)^{\frac{1}{\sigma-1}} \right]^{\eta-1} & , 0 < \sigma \neq 1 \\ (AP)^\eta \nu^{\eta-1} \left[\prod_{i=1}^I \left(\frac{\alpha_i}{\omega_i} \right)^{\alpha_i} \right]^{\eta-1} & , \sigma = 1 \end{cases} \tag{A.18}$$

or, for all $\sigma > 0$,

$$Y^* = A^\eta P^\eta \nu^{\eta-1} \Phi^{\eta-1} \tag{A.19}$$

Input demands may be calculated substituting target output levels from equation (A.16) into the equation for cost-minimizing factor demands in equation (A.7):

$$X_m^* = \begin{cases} A^{-\frac{1}{\nu}} \left(\frac{\alpha_m}{\omega_m} \right)^\sigma \left[\sum_{i=1}^I \alpha_i^\sigma \omega_i^{1-\sigma} \right]^{\frac{\sigma}{1-\sigma}} \left(A^\eta (\nu P)^{\nu\eta} \left[\left(\sum_{i=1}^I \alpha_i^\sigma \omega_i^{1-\sigma} \right)^{\frac{1}{\sigma-1}} \right]^{\nu\eta} \right)^{\frac{1}{\nu}} & , 0 < \sigma \neq 1 \\ A^{-\frac{1}{\nu}} \left(\frac{\alpha_m}{\omega_m} \right) \left[\prod_{i=1}^I \left(\frac{\omega_i}{\alpha_i} \right)^{\alpha_i} \right] \left(A^\eta (\nu P)^{\nu\eta} \left[\prod_{i=1}^I \left(\frac{\alpha_i}{\omega_i} \right)^{\alpha_i} \right]^{\nu\eta} \right)^{\frac{1}{\nu}} & , \sigma = 1 \end{cases}$$

which can be simplified

$$X_m^* = \begin{cases} \left(\frac{\alpha_m}{\omega_m} \right)^\sigma A^{\frac{\eta-1}{\nu}} (\nu P)^\eta \left[\left(\sum_{i=1}^I \alpha_i^\sigma \omega_i^{1-\sigma} \right)^{\frac{1}{\sigma-1}} \right]^{-\sigma} \left[\left(\sum_{i=1}^I \alpha_i^\sigma \omega_i^{1-\sigma} \right)^{\frac{1}{\sigma-1}} \right]^{\eta} & , 0 < \sigma \neq 1 \\ \left(\frac{\alpha_m}{\omega_m} \right) A^{\frac{\eta-1}{\nu}} (\nu P)^\eta \left[\prod_{i=1}^I \left(\frac{\alpha_i}{\omega_i} \right)^{\alpha_i} \right]^{-1} \left[\prod_{i=1}^I \left(\frac{\alpha_i}{\omega_i} \right)^{\alpha_i} \right]^{\eta} & , \sigma = 1 \end{cases}$$

Observe that $\eta - 1 = \frac{1}{1-\nu} - \frac{1-\nu}{1-\nu} = \frac{\nu}{1-\nu} = \nu\eta$, and $(\eta-1)/\nu = \eta$. Recall that $\nu < 1$ implies $\eta > 1$, with $\eta \to \infty$ as $\nu \to 1$. Then finally,

$$X_m^* = \begin{cases} \left(\frac{\alpha_m}{\omega_m} \right)^\sigma (\nu AP)^\eta \left[\left(\sum_{i=1}^I \alpha_i^\sigma \omega_i^{1-\sigma} \right)^{\frac{1}{\sigma-1}} \right]^{\eta-\sigma} & , 0 < \sigma \neq 1 \\ \left(\frac{\alpha_m}{\omega_m} \right) (\nu AP)^\eta \left[\prod_{i=1}^I \left(\frac{\alpha_i}{\omega_i} \right)^{\alpha_i} \right]^{\eta-1} & , \sigma = 1 \end{cases} \tag{A.20}$$

or, for all $\sigma > 0$,

$$X_m^* = \left(\frac{\alpha_m}{\omega_m} \right)^\sigma (\nu AP)^\eta \Phi^{\eta-\sigma} \tag{A.21}$$

And substituting optimal output levels into the expression for cost-minimizing total costs, equation (A.9), we have

$$C(Q^*) \quad = \quad A^{-\frac{1}{\nu}} \left[A^\eta P^{\eta-1} \nu^{\eta-1} \Phi^{\nu\eta}\right]^{\frac{1}{\nu}} \Phi^{-1} = A^{\frac{\eta-1}{\nu}} P^{\frac{\eta-1}{\nu}} \nu^{\frac{\eta-1}{\nu}} \Phi^{\eta-1} = A^{\frac{\nu\eta}{\nu}} P^{\frac{\nu\eta}{\nu}} \nu^{\frac{\nu\eta}{\nu}} \Phi^{\eta-1}$$

or,

$$C(Q^*) \quad = \quad A^\eta P^\eta \nu^\eta \Phi^{\eta-1} \tag{A.22}$$

and observe that total revenue is equal to total cost times $1/\nu$, i.e., $Y^* = (1/\nu)C^*$.

Then variable profits are given by

$$\Pi^*_{\text{var}} = A^\eta P^\eta \nu^{\eta-1} \Phi^{\eta-1} - A^\eta P^\eta \nu^\eta \Phi^{\eta-1} = A^\eta P^\eta \nu^{\eta-1} \Phi^{\eta-1} (1-\nu) \tag{A.23}$$

which highlights that variable profits can only be positive for $\nu < 1$. Variable profits can also be written

$$\Pi^*_{\text{var}} = (1-\nu) Y^* \tag{A.24}$$

Collecting equations in their simplest forms, we have

$$Q^* = A^\eta P^{\eta-1} \nu^{\eta-1} \Phi^{\nu\eta} \tag{A.17 revisited}$$
$$Y^* = A^\eta P^\eta \nu^{\eta-1} \Phi^{\eta-1} \tag{A.19 revisited}$$
$$X^*_m = \left(\frac{\alpha_m}{\omega_m}\right)^\sigma \nu^\eta A^\eta P^\eta \Phi^{\eta-\sigma} \tag{A.21 revisited}$$

and

$$C^* = A^\eta P^\eta \nu^\eta \Phi^{\eta-1} \tag{A.22 revisited}$$
$$\Pi^*_{\text{var}} = A^\eta P^\eta \nu^{\eta-1} \Phi^{\eta-1} (1-\nu) \tag{A.23 revisited}$$

Monopolistic. In the monopolistic case, firms choose quantities such that the implied price is equal to marginal cost times a markup. Given isoelastic demand with $Q(P) = \theta^\epsilon P^{-\epsilon}$, and $P(Q) = \theta Q^{-1/\epsilon}$, optimal output levels are implied by

$$Q \quad s.t. \quad \theta Q^{-1/\epsilon} = \begin{cases} \frac{\mu}{\nu} A^{-\frac{1}{\nu}} \left(\sum_{i=1}^I \alpha_i^\sigma \omega_i^{1-\sigma}\right)^{\frac{1}{1-\sigma}} \bar{Q}^{\frac{1-\nu}{\nu}} & , \ 0 < \sigma \neq 1 \\ \frac{\mu}{\nu} A^{-\frac{1}{\nu}} \prod_{i=1}^I \left(\frac{\omega_i}{\alpha_i}\right)^{\alpha_i} \bar{Q}^{\frac{1-\nu}{\nu}} & \sigma = 1 \end{cases}$$

with $\mu = \epsilon/(\epsilon - 1)$, which implies that

$$Q^* = \begin{cases} A^\eta \theta^{\nu\eta} \left(\frac{\nu}{\mu}\right)^{\nu\eta} \left[\left(\sum_{i=1}^I \alpha_i^\sigma \omega_i^{1-\sigma}\right)^{\frac{1}{\sigma-1}}\right]^{\nu\eta} & , \ 0 < \sigma \neq 1 \\ A^\eta \theta^{\nu\eta} \left(\frac{\nu}{\mu}\right)^{\nu\eta} \left[\prod_{i=1}^I \left(\frac{\alpha_i}{\omega_i}\right)^{\alpha_i}\right]^{\nu\eta} & , \ \sigma = 1 \end{cases} \tag{A.25}$$

or, for all $\sigma > 0$,

$$Q^* = A^\eta \theta^{\nu\eta} \left(\frac{\nu}{\mu}\right)^{\nu\eta} \Phi^{\nu\eta} \tag{A.26}$$

with $\eta = \frac{\epsilon}{\nu+\epsilon-\epsilon\nu} = \frac{\mu}{\mu-\nu}$. In general, the sign of η is ambiguous. But as will be shown below, η can once again be seen as the inverse of the share of variable profits in revenue. Thus, positive profits requires $\eta > 0$, or equivalently, $\mu > \nu$. Notice that $\eta > 1$, with $\nu \to \mu \implies \eta \to \infty$.

In order to obtain optimal revenue, first observe that revenue is equal to $Q \times P(Q)$, so that the revenue function is $Y(Q) = \theta Q^{\frac{\epsilon-1}{\epsilon}}$. Next, observe that $\eta - 1 = \frac{\mu}{\mu-\nu} - 1 = \frac{\nu}{\mu-\nu} = \frac{\nu}{\mu}\eta$. Thus, $\theta\theta^{\frac{\nu}{\mu}\eta}$ equals $\theta\theta^{\eta-1}$ equals θ^{η}. Now substituting Q^* directly into the revenue function, we have

$$Y^* = \begin{cases} \theta^{\eta}(A^{\frac{1}{\mu}})^{\eta}\left(\frac{\nu}{\mu}\right)^{\eta-1}\left[\left(\sum_{i=1}^{I}\alpha_i^{\sigma}\omega_i^{1-\sigma}\right)^{\frac{1}{\sigma-1}}\right]^{\eta-1} & , 0<\sigma\neq 1 \\ \theta^{\eta}(A^{\frac{1}{\mu}})^{\eta}\left(\frac{\nu}{\mu}\right)^{\eta-1}\left[\prod_{i=1}^{I}\left(\frac{\alpha_i}{\omega_i}\right)^{\alpha_i}\right]^{\eta-1} & , \sigma = 1 \end{cases} \tag{A.27}$$

or, for all $\sigma > 0$,

$$Y^* = \theta^{\eta}(A^{\frac{1}{\mu}})^{\eta}\left(\frac{\nu}{\mu}\right)^{\eta-1}\Phi^{\eta-1} \tag{A.28}$$

Moreover, it will be useful to observe that given ϵ (and θ), we can simply invert the revenue function to infer quantities:

$$Q^* = (Y^*)^{\frac{\epsilon}{\epsilon-1}}\theta^{-\frac{\epsilon}{\epsilon-1}} \tag{A.29}$$

In order to derive a convenient expression for optimal pricing, observe that $\frac{\nu}{\epsilon}\eta - 1 = \frac{\nu}{\epsilon+\nu-\epsilon\nu} - 1 = (\nu-1)\eta$. Then substituting Q^* into the inverse demand function, it can be shown that

$$P^* = \begin{cases} A^{-\frac{1}{\epsilon}\eta}\theta^{\eta-\nu\eta}\left(\frac{\nu}{\mu}\right)^{-\frac{\nu}{\epsilon}\eta}\left[\left(\sum_{i=1}^{I}\alpha_i^{\sigma}\omega_i^{1-\sigma}\right)^{\frac{1}{\sigma-1}}\right]^{-\frac{\nu}{\epsilon}\eta} & , 0<\sigma\neq 1 \\ A^{-\frac{1}{\epsilon}\eta}\theta^{\eta-\nu\eta}\left(\frac{\nu}{\mu}\right)^{-\frac{\nu}{\epsilon}\eta}\left[\prod_{i=1}^{I}\left(\frac{\alpha_i}{\omega_i}\right)^{\alpha_i}\right]^{-\frac{\nu}{\epsilon}\eta} & , \sigma = 1 \end{cases} \tag{A.30}$$

or, for all $\sigma > 0$,

$$P^* = A^{-\frac{1}{\epsilon}\eta}\theta^{\eta-\nu\eta}\left(\frac{\nu}{\mu}\right)^{-\frac{\nu}{\epsilon}\eta}\Phi^{-\frac{\nu}{\epsilon}\eta} \tag{A.31}$$

Again, it will be useful to observe that given ϵ (and θ), we can use the demand function to infer prices from revenue:

$$P^* = \theta\left[(Y^*)^{\frac{\epsilon}{\epsilon-1}}\theta^{-\frac{\epsilon}{\epsilon-1}}\right]^{-1/\epsilon} = (Y^*)^{-\frac{1}{\epsilon-1}}\theta^{\frac{\epsilon}{\epsilon-1}} \tag{A.32}$$

Input demands may be calculated substituting target output levels from equation (A.25) into the equation for cost-minimizing factor demands in equation (A.7):

$$X_m^* = \begin{cases} A^{-\frac{1}{\nu}}\left(\frac{\alpha_m}{\omega_m}\right)^{\sigma}\left[\sum_{i=1}^{I}\alpha_i^{\sigma}\omega_i^{1-\sigma}\right]^{\frac{\sigma}{1-\sigma}}\left(A^{\eta}\theta^{\nu\eta}\left(\frac{\nu}{\mu}\right)^{\nu\eta}\left[\left(\sum_{i=1}^{I}\alpha_i^{\sigma}\omega_i^{1-\sigma}\right)^{\frac{1}{\sigma-1}}\right]^{\nu\eta}\right)^{\frac{1}{\nu}} & , 0<\sigma\neq 1 \\ A^{-\frac{1}{\nu}}\left(\frac{\alpha_m}{\omega_m}\right)\left[\prod_{i=1}^{I}\left(\frac{\omega_i}{\alpha_i}\right)^{\alpha_i}\right]\left(A^{\eta}\theta^{\nu\eta}\left(\frac{\nu}{\mu}\right)^{\nu\eta}\left[\prod_{i=1}^{I}\left(\frac{\alpha_i}{\omega_i}\right)^{\alpha_i}\right]^{\nu\eta}\right)^{\frac{1}{\nu}} & , \sigma = 1 \end{cases}$$

which we can simplify

$$X_m^* = \begin{cases} \left(\frac{\alpha_m}{\omega_m}\right)^{\sigma}A^{\frac{\eta-1}{\nu}}\left(\frac{\nu}{\mu}\right)^{\eta}\theta^{\eta}\left[\left(\sum_{i=1}^{I}\alpha_i^{\sigma}\omega_i^{1-\sigma}\right)^{\frac{1}{\sigma-1}}\right]^{-\sigma}\left[\left(\sum_{i=1}^{I}\alpha_i^{\sigma}\omega_i^{1-\sigma}\right)^{\frac{1}{\sigma-1}}\right]^{\eta} & , 0<\sigma\neq 1 \\ \left(\frac{\alpha_m}{\omega_m}\right)A^{\frac{\eta-1}{\nu}}\left(\frac{\nu}{\mu}\right)^{\eta}\theta^{\eta}\left[\prod_{i=1}^{I}\left(\frac{\alpha_i}{\omega_i}\right)^{\alpha_i}\right]^{-1}\left[\prod_{i=1}^{I}\left(\frac{\alpha_i}{\omega_i}\right)^{\alpha_i}\right]^{\eta} & , \sigma = 1 \end{cases}$$

Recall that $\eta - 1 = \frac{\mu}{\mu-\nu} - 1 = \frac{\nu}{\mu-\nu} = \frac{\nu}{\mu}\eta$, and $(\eta-1)/\nu = \frac{1}{\mu}\eta$. Then we can write $A^{\frac{\eta-1}{\nu}} = (A^{\frac{1}{\mu}})^{\eta}$, and

$$X_m^* = \begin{cases} \left(\frac{\alpha_m}{\omega_m}\right)^{\sigma} \theta^{\eta} (A^{\frac{1}{\mu}})^{\eta} \left(\frac{\nu}{\mu}\right)^{\eta} \left[\left(\sum_{i=1}^{I} \alpha_i^{\sigma} \omega_i^{1-\sigma}\right)^{\frac{1}{\sigma-1}}\right]^{\eta-\sigma} & , \ 0 < \sigma \neq 1 \\ \left(\frac{\alpha_m}{\omega_m}\right) \theta^{\eta} (A^{\frac{1}{\mu}})^{\eta} \left(\frac{\nu}{\mu}\right)^{\eta} \left[\prod_{i=1}^{I} \left(\frac{\alpha_i}{\omega_i}\right)^{\alpha_i}\right]^{\eta-1} & , \ \sigma = 1 \end{cases} \tag{A.33}$$

or, for all $\sigma > 0$,

$$X_m^* = \left(\frac{\alpha_m}{\omega_m}\right)^{\sigma} \theta^{\eta} (A^{\frac{1}{\mu}})^{\eta} \left(\frac{\nu}{\mu}\right)^{\eta} \Phi^{\eta-\sigma} \tag{A.34}$$

And substituting optimal output levels into the expression for cost-minimizing total costs, equation (A.9), we have

$$C(Q^*) \quad = \quad A^{-\frac{1}{\nu}} \left[A^{\eta} \theta^{\nu\eta} \left(\frac{\nu}{\mu}\right)^{\nu\eta} \Phi^{\nu\eta}\right]^{\frac{1}{\nu}} \Phi^{-1} = A^{\frac{\eta-1}{\nu}} \theta^{\eta} \left(\frac{\nu}{\mu}\right)^{\eta} \Phi^{\eta-1}$$

or,

$$C(Q^*) \quad = \quad (A^{\frac{1}{\mu}})^{\eta} \theta^{\eta} \left(\frac{\nu}{\mu}\right)^{\eta} \Phi^{\eta-1} \tag{A.35}$$

and observe the implication that total revenue is equal to total cost times μ/ν, i.e., $Y^* = (\mu/\nu)C^*$.

Thus, variable profits are given by

$$\Pi_{\text{var}}^* \quad = \quad Y^* - C^* = (A^{\frac{1}{\mu}})^{\eta} \theta^{\eta} \left(\frac{\nu}{\mu}\right)^{\eta-1} \Phi^{\eta-1} - (A^{\frac{1}{\mu}})^{\eta} \theta^{\eta} \left(\frac{\nu}{\mu}\right)^{\eta} \Phi^{\eta-1}$$

or

$$\Pi_{\text{var}}^* = (A^{\frac{1}{\mu}})^{\eta} \theta^{\eta} \left(\frac{\nu}{\mu}\right)^{\eta-1} \Phi^{\eta-1} \left(1 - \frac{\nu}{\mu}\right) \tag{A.36}$$

which highlights that variable profits can only be positive when $\mu > \nu$. (Equivalently, observe that Gorodnichenko (2012) defines ν/μ as the returns to scale in the revenue function, which will imply a negative profit share in revenue if it exceeds unity; this is identical to the result here.) Variable profits can also be written

$$\Pi_{\text{var}}^* = \left(1 - \frac{\nu}{\mu}\right) Y^* \tag{A.37}$$

Collecting equations in their simplest forms, we have

$$Q^* = A^{\eta} \theta^{\nu\eta} \left(\frac{\nu}{\mu}\right)^{\nu\eta} \Phi^{\nu\eta} \tag{A.26 revisited}$$

$$Y^* = A^{\frac{\eta}{\mu}} \theta^{\eta} \left(\frac{\nu}{\mu}\right)^{\eta-1} \Phi^{\eta-1} \tag{A.28 revisited}$$

$$P^* = A^{-\frac{\eta}{\epsilon}} \theta^{\eta-\nu\eta} \left(\frac{\nu}{\mu}\right)^{-\frac{\nu}{\epsilon}\eta} \Phi^{-\frac{\nu}{\epsilon}\eta} \tag{A.31 revisited}$$

$$X_m^* = \left(\frac{\alpha_m}{\omega_m}\right)^{\sigma} A^{\frac{\eta}{\mu}} \theta^{\eta} \left(\frac{\nu}{\mu}\right)^{\eta} \Phi^{\eta-\sigma} \tag{A.34 revisited}$$

and

$$C^* = A^{\frac{\eta}{\mu}} \theta^{\eta} \left(\frac{\nu}{\mu}\right)^{\eta} \Phi^{\eta-1} \tag{A.35 revisited}$$

$$\Pi^*_{\text{var}} = A^{\frac{\eta}{\mu}} \theta^{\eta} \left(\frac{\nu}{\mu}\right)^{\eta-1} \Phi^{\eta-1} \left(1 - \frac{\nu}{\mu}\right) \tag{A.36 revisited}$$

A.1.3 Selected ratios

Factor intensity of revenue. Let $\Omega^*_m = \omega_i X^*_i / Y^*$ denote the factor of intensity of revenue of input m. Letting $\mu = 1$ in the price-taking case, then for both the monopolistic and price-taking cases, for all $\sigma > 0$, we can write

$$\Omega^*_m = \frac{\nu}{\mu} \alpha^{\sigma}_m \omega^{1-\sigma}_m \Phi^{1-\sigma} = \left(\frac{\partial Q}{\partial X_m} \frac{X^*_m}{Q}\right) / \mu \tag{A.38}$$

Notice that Ω^*_m is equal to the cost-minimizing output elasticity in equation (A.14), divided by the markup. That is, observing a given firm's factor share of revenue, we also know that firm's output elasticity up to a scale factor. Under perfect competition, Ω^*_m is exactly the firm's output elasticity, while in the presence of markups, this measure of output elasticities will be downward biased. More generally, if we assume or estimate a common markup across firms within an industry, we can recover firm-specific output elasticities. However, if the common markup assumption is untrue, then firms with higher than average markups will have downward-biased estimated output elasticities.

Alternatively, given an assumed or estimated common output elasticity at the industry level, we can recover firm-specific markups (e.g., De Loecker 2011). However, recall that firm-specific output elasticities (which result from allowing either firm-specific ω_i or α_i parameters, or both) are needed to rationalize variation in input mixes within the same detailed industry. If the common output elasticity assumption is untrue, then firms with higher than average output elasticities at a given level of factor intensity will have upward-biased markups.

Factor-output ratio / inverse average revenue product. It follows immediately that for both the monopolistic and price-taking cases, for all $\sigma > 0$, we can write

$$(y^*_m)^{-1} = \frac{X^*_m}{Y^*} = \frac{\nu}{\mu} \alpha^{\sigma}_m \omega^{-\sigma}_m \Phi^{1-\sigma} \tag{A.39}$$

Taking the derivative with respect to ω_m, we see that $\partial(X^*_m/Y^*)/\partial\omega_m < 0$. Thus, the inverse average revenue product should be decreasing with wage, or increasing with inverse wage. More intuitively, the implication is that the average revenue product, like the marginal revenue product, is increasing in the wage rate.

A.2 Comparative statics of productivity shocks

A.2.1 Proportional productivity shock, $A' = A(1-\tau_A)$

Consider a productivity shock of the form $A' = A(1-\tau_A)$. So that replacing A with A' and differentiating with respect to τ_A, we have

$$\frac{\partial Q^*/\partial \tau_A}{Q^*} = \frac{\partial Y^*/\partial \tau_A}{Y^*} = \frac{\partial X_m^*/\partial \tau_A}{X_m^*} = \frac{\partial \Pi_{\mathrm{var}}^*/\partial \tau_A}{\Pi_{\mathrm{var}}^*} = -\frac{\eta}{1-\tau_A} \tag{A.40}$$

$$\frac{\partial \Omega_m^*/\partial \tau_A}{\Omega_m^*} = 0 \tag{A.41}$$

with $\eta = \frac{1}{1-\nu}$. Recall that $\nu < 1$ implies $\eta > 1$, with $\eta \to \infty$ as $\nu \to 1$.

Under monopolistic competition, we have

$$\frac{\partial Q^*/\partial \tau_A}{Q^*} = -\frac{\eta}{1-\tau_A} \tag{A.42}$$

$$\frac{\partial P^*/\partial \tau_A}{P^*} = \frac{\eta/\epsilon}{1-\tau_A} \tag{A.43}$$

$$\frac{\partial Y^*/\partial \tau_A}{Y^*} = \frac{\partial X_m^*/\partial \tau_A}{X_m^*} = \frac{\partial \Pi_{\mathrm{var}}^*/\partial \tau_A}{\Pi_{\mathrm{var}}^*} = -\frac{\eta/\mu}{1-\tau_A} \tag{A.44}$$

$$\frac{\partial \Omega_m^*/\partial \tau_A}{\Omega_m^*} = 0 \tag{A.45}$$

with $\eta = \frac{\epsilon}{\nu+\epsilon-\epsilon\nu} = \frac{\mu}{\mu-\nu}$.

A.2.2 Additive productivity shock, $A' = A - t_A$

Consider a productivity shock of the form $A' = A - t_A$. Under price-taking behavior, we have that

$$\frac{\partial Q^*/\partial t_A}{Q^*} = \frac{\partial Y^*/\partial t_A}{Y^*} = \frac{\partial X_m^*/\partial t_A}{X_m^*} = \frac{\partial \Pi_{\mathrm{var}}^*/\partial t_A}{\Pi_{\mathrm{var}}^*} = -\frac{\eta}{A-t_A} \tag{A.46}$$

$$\frac{\partial \Omega_m^*/\partial t_A}{\Omega_m^*} = 0 \tag{A.47}$$

with $\eta = \frac{1}{1-\nu}$. Recall that $\nu < 1$ implies $\eta > 1$, with $\eta \to \infty$ as $\nu \to 1$.

Under monopolistic behavior, we have that

$$\frac{\partial Q^*/\partial t_A}{Q^*} = -\frac{\eta}{A-t_A} \tag{A.48}$$

$$\frac{\partial P^*/\partial t_A}{P^*} = \frac{\eta/\epsilon}{A-t_A} \tag{A.49}$$

and

$$\frac{\partial Y^*/\partial t_A}{Y^*} = \frac{\partial X_m^*/\partial t_A}{X_m^*} = \frac{\partial \Pi_{\mathrm{var}}^*/\partial t_A}{\Pi_{\mathrm{var}}^*} = -\frac{\eta/\mu}{A-t_A} \tag{A.50}$$

$$\frac{\partial \Omega_m^*/\partial t_A}{\Omega_m^*} = 0 \tag{A.51}$$

with $\eta = \frac{\epsilon}{\nu+\epsilon-\epsilon\nu} = \frac{\mu}{\mu-\nu}$.

A.3 Comparative statics of factor price shocks

A.3.1 Proportional factor price shock, $\omega'_m = (1+\tau_m)\omega_m$

Consider a proportional factor price shock, $\omega'_m = (1+\tau_m)\omega_m$. In this case, we can write

$$\Phi = \begin{cases} \left[\sum_{i=1}^{I-1} \alpha_i^\sigma \omega_i^{1-\sigma} + \alpha_m^\sigma \omega_m^{1-\sigma}(1+\tau_m)^{1-\sigma}\right]^{\frac{1}{\sigma-1}} & , 0<\sigma\neq 1 \\ \left[\prod_{i=1}^{I}\left(\frac{\alpha_i}{\omega_i}\right)^{\alpha_i}\right](1+\tau_m)^{-\alpha_m} & , \sigma=1 \end{cases} \tag{A.52}$$

with, for all $\sigma > 0$ (and letting $\mu = 1$ under price-taking), we have

$$\frac{\partial\Phi}{\partial\tau_m} = -\Phi\alpha_m^\sigma\omega_m^{1-\sigma}(1+\tau_m)^{-\sigma}\Phi^{1-\sigma} \tag{A.53}$$

$$\Phi^{-1}\frac{\partial\Phi}{\partial\tau_m} = -\alpha_m^\sigma\omega_m^{1-\sigma}(1+\tau_m)^{-\sigma}\Phi^{1-\sigma} = -\frac{\mu}{\nu}(1+\tau_m)^{-1}\Omega_m^* \tag{A.54}$$

Under price-taking behavior, noting that $\eta - 1 = \frac{1}{1-\nu} - \frac{1-\nu}{1-\nu} = \frac{\nu}{1-\nu} = \nu\eta$, we have that

$$\frac{\partial Q^*/\partial\tau_m}{Q^*} = \nu\eta\Phi^{-1}\frac{\partial\Phi}{\partial\tau_m} = -(\eta-1)\frac{1}{\nu}\frac{\Omega_m^*}{1+\tau_m} = -\eta\frac{\Omega_m^*}{1+\tau_m}$$

and

$$\frac{\partial Y^*/\partial\tau_m}{Y^*} = \frac{\partial\Pi_{\text{var}}^*/\partial\tau_m}{\Pi_{\text{var}}^*} = (\eta-1)\Phi^{-1}\frac{\partial\Phi}{\partial\tau_m} = -(\eta-1)\frac{1}{\nu}\frac{\Omega_m^*}{1+\tau_m} = -\eta\frac{\Omega_m^*}{1+\tau_m} \tag{A.55}$$

$$\frac{\partial X_m^*/\partial\tau_m}{X_m^*} = (\eta-\sigma)\Phi^{-1}\frac{\partial\Phi}{\partial\tau_m} - \frac{\sigma}{1+\tau_m} = -(\eta-\sigma)\frac{1}{\nu}\frac{\Omega_m^*}{1+\tau_m} - \frac{\sigma}{1+\tau_m} \tag{A.56}$$

$$\frac{\partial X_n^*/\partial\tau_m}{X_n^*} = (\eta-\sigma)\Phi^{-1}\frac{\partial\Phi}{\partial\tau_m} = -(\eta-\sigma)\frac{1}{\nu}\frac{\Omega_m^*}{1+\tau_m} \tag{A.57}$$

Under monopolistic behavior, we have that

$$\frac{\partial Q^*/\partial\tau_m}{Q^*} = \nu\eta\Phi^{-1}\frac{\partial\Phi}{\partial\tau_m} = -\mu\eta\frac{\Omega_m^*}{1+\tau_m}$$

$$\frac{\partial P^*/\partial\tau_m}{P^*} = -\frac{\nu}{\epsilon}\eta\Phi^{-1}\frac{\partial\Phi}{\partial\tau_m} = \frac{\mu\eta}{\epsilon}\frac{\Omega_m^*}{1+\tau_m}$$

and

$$\frac{\partial Y^*/\partial\tau_m}{Y^*} = \frac{\partial\Pi_{\text{var}}^*/\partial\tau_m}{\Pi_{\text{var}}^*} = (\eta-1)\Phi^{-1}\frac{\partial\Phi}{\partial\tau_m} = -(\eta-1)\frac{\mu}{\nu}\frac{\Omega_m^*}{1+\tau_m} = -\eta\frac{\Omega_m^*}{1+\tau_m} \tag{A.58}$$

$$\frac{\partial X_m^*/\partial\tau_m}{X_m^*} = (\eta-\sigma)\Phi^{-1}\frac{\partial\Phi}{\partial\tau_m} - \frac{\sigma}{1+\tau_m} = -(\eta-\sigma)\frac{\mu}{\nu}\frac{\Omega_m^*}{1+\tau_m} - \frac{\sigma}{1+\tau_m} \tag{A.59}$$

$$\frac{\partial X_n^*/\partial\tau_m}{X_n^*} = (\eta-\sigma)\Phi^{-1}\frac{\partial\Phi}{\partial\tau_m} = -(\eta-\sigma)\frac{\mu}{\nu}\frac{\Omega_m^*}{1+\tau_m} \tag{A.60}$$

Under both price-taking and monopolistic behavior, we have that

$$\frac{\partial\Omega_n^*/\partial\tau_m}{\Omega_n^*} = (1-\sigma)\Phi^{-1}\frac{\partial\Phi}{\partial\tau_m} = -(1-\sigma)\frac{\mu}{\nu}\frac{\Omega_m^*}{1+\tau_m} \tag{A.61}$$

$$\frac{\partial\Omega_m^*/\partial\tau_m}{\Omega_m^*} = (1-\sigma)\Phi^{-1}\frac{\partial\Phi}{\partial\tau_m} + \frac{1-\sigma}{1+\tau_m} = -(1-\sigma)\frac{\mu}{\nu}\frac{\Omega_m^*}{1+\tau_m} + \frac{1-\sigma}{1+\tau_m} \tag{A.62}$$

A.3.2 Additive factor price shock, $\omega'_m = \omega_m + t_m$

Consider input price shocks of the form $\omega'_m = \omega_m + t_m$. In this case, we can write

$$\Phi = \begin{cases} \left[\sum_{i=1}^{I-1} \alpha_i^\sigma \omega_i^{1-\sigma} + \alpha_m^\sigma (\omega_m + t_m)^{1-\sigma}\right]^{\frac{1}{\sigma-1}} & , \ 0 < \sigma \neq 1 \\ \left[\prod_{i=1}^{I-1} \left(\frac{\alpha_i}{\omega_i}\right)^{\alpha_i} \alpha_m^{\alpha_m}\right] (\omega_m + t_m)^{-\alpha_m} & , \ \sigma = 1 \end{cases} \tag{A.63}$$

with, for all $\sigma > 0$ (and letting $\mu = 1$ under price-taking), we have

$$\frac{\partial \Phi}{\partial t_m} = -\Phi \alpha_m^\sigma (\omega_m + t_m)^{-\sigma} \Phi^{1-\sigma} \tag{A.64}$$

$$\Phi^{-1} \frac{\partial \Phi}{\partial t_m} = -\alpha_m^\sigma (\omega_m + t_m)^{-\sigma} \Phi^{1-\sigma} = -\frac{\mu}{\nu} \frac{X_m^*}{Y^*} \tag{A.65}$$

Under price-taking behavior, noting that $\eta - 1 = \frac{1}{1-\nu} - \frac{1-\nu}{1-\nu} = \frac{\nu}{1-\nu} = \nu\eta$, we have that

$$\frac{\partial Q^*/\partial t_m}{Q^*} = \nu\eta \Phi^{-1} \frac{\partial \Phi}{\partial t_m} = -(\eta - 1)\frac{1}{\nu}(y_m^*)^{-1} = -\eta (y_m^*)^{-1}$$

and

$$\frac{\partial Y^*/\partial t_m}{Y^*} = \frac{\partial \Pi_{\text{var}}^*/\partial t_m}{\Pi_{\text{var}}^*} = (\eta - 1)\Phi^{-1}\frac{\partial \Phi}{\partial t_m} = -(\eta - 1)\frac{1}{\nu}(y_m^*)^{-1} = -\eta (y_m^*)^{-1} \tag{A.66}$$

$$\frac{\partial X_m^*/\partial t_m}{X_m^*} = (\eta - \sigma)\Phi^{-1}\frac{\partial \Phi}{\partial t_m} - \frac{\sigma}{\omega_m + \tau_m} = -(\eta - \sigma)\frac{1}{\nu}(y_m^*)^{-1} - \sigma(\omega_m + t_m)^{-1} \tag{A.67}$$

$$\frac{\partial X_n^*/\partial t_m}{X_n^*} = (\eta - \sigma)\Phi^{-1}\frac{\partial \Phi}{\partial t_m} = -(\eta - \sigma)\frac{1}{\nu}(y_m^*)^{-1} \tag{A.68}$$

Under monopolistic behavior, we have that

$$\frac{\partial Q^*/\partial \tau_m}{Q^*} = \nu\eta \Phi^{-1}\frac{\partial \Phi}{\partial t_m} = -\mu\eta (y_m^*)^{-1} \tag{A.69}$$

$$\frac{\partial P^*/\partial \tau_m}{P^*} = -\frac{\nu}{\epsilon}\eta \Phi^{-1}\frac{\partial \Phi}{\partial t_m} = \frac{\mu\eta}{\epsilon}(y_m^*)^{-1} \tag{A.70}$$

and

$$\frac{\partial Y^*/\partial t_m}{Y^*} = \frac{\partial \Pi_{\text{var}}^*/\partial t_m}{\Pi_{\text{var}}^*} = (\eta - 1)\Phi^{-1}\frac{\partial \Phi}{\partial t_m} = -(\eta - 1)\frac{\mu}{\nu}(y_m^*)^{-1} = -\eta (y_m^*)^{-1} \tag{A.71}$$

$$\frac{\partial X_m^*/\partial t_m}{X_m^*} = (\eta - \sigma)\Phi^{-1}\frac{\partial \Phi}{\partial t_m} - \frac{\sigma}{\omega_m + \tau_m} = -(\eta - \sigma)\frac{\mu}{\nu}(y_m^*)^{-1} - \sigma(\omega_m + t_m)^{-1} \tag{A.72}$$

$$\frac{\partial X_n^*/\partial t_m}{X_n^*} = (\eta - \sigma)\Phi^{-1}\frac{\partial \Phi}{\partial t_m} = -(\eta - \sigma)\frac{\mu}{\nu}(y_m^*)^{-1} \tag{A.73}$$

Under both price-taking and monopolistic behavior, we have that

$$\frac{\partial \Omega_n^*/\partial \tau_m}{\Omega_n^*} = (1 - \sigma)\Phi^{-1}\frac{\partial \Phi}{\partial \tau_m} = -(1 - \sigma)\frac{\mu}{\nu}(y_m^*)^{-1} \tag{A.74}$$

$$\frac{\partial \Omega_m^*/\partial \tau_m}{\Omega_m^*} = (1 - \sigma)\Phi^{-1}\frac{\partial \Phi}{\partial \tau_m} + \frac{1 - \sigma}{\omega_m + \tau_m} = -(1 - \sigma)\frac{\mu}{\nu}(y_m^*)^{-1} + (1 - \sigma)(\omega_m + \tau_m)^{-1} \tag{A.75}$$

A.4 Comparative statics of demand shocks with no change in elasticities

A.4.1 Proportional demand shocks, $P' = (1-\tau_p)P$

In the price-taking case, consider a demand shock of the form $P' = (1-\tau_p)P$. Under price-taking behavior, we can write

$$Y^* = A^\eta P^\eta (1-\tau_p)^\eta \nu^{\eta-1}\Phi^{\eta-1}$$
$$X_m^* = \left(\frac{\alpha_m}{\omega_m}\right)^\sigma \nu^\eta A^\eta P^\eta (1-\tau_p)^\eta \Phi^{\eta-\sigma}$$

Thus, we have

$$\frac{\partial Q^*/\partial\tau_p}{Q^*} = -\frac{\eta-1}{1-\tau_p} \tag{A.76}$$
$$\frac{\partial P/\partial\tau_p}{P} = -\frac{1}{1-\tau_p} \tag{A.77}$$
$$\frac{\partial Y^*/\partial\tau_p}{Y^*} = \frac{\partial X_m^*/\partial\tau_p}{X_m^*} = \frac{\partial\Pi_{\text{var}}^*/\partial\tau_p}{\Pi_{\text{var}}^*} = -\frac{\eta}{1-\tau_p} \tag{A.78}$$

with $\eta = \frac{1}{1-\nu}$. Recall that $\nu < 1$ implies $\eta > 1$, with $\eta \to \infty$ as $\nu \to 1$.

In the monopolistic case, consider a demand shock of the form $P'(Q) = (1-\tau_p)P(Q)$. Given the assumption of isoelastic demand, $P(Q) = \theta Q^{-1/\epsilon}$, it is straightforward to simply replace every instance of θ with $\theta(1-\tau_p)$, and differentiate with respect to τ_p. Thus, we can write

$$Q^* = A^\eta (1-\tau_p)^{\nu\eta}\theta^{\nu\eta}\left(\frac{\nu}{\mu}\right)^{\nu\eta}\Phi^{\nu\eta}$$
$$Y^* = A^{\frac{\eta}{\mu}}\theta^\eta (1-\tau_p)^\eta \left(\frac{\nu}{\mu}\right)^{\eta-1}\Phi^{\eta-1}$$
$$P^* = A^{-\frac{\eta}{\epsilon}}\theta^{\eta-\nu\eta}(1-\tau_p)^{\eta-\nu\eta}\left(\frac{\nu}{\mu}\right)^{-\frac{\nu}{\epsilon}\eta}\Phi^{-\frac{\nu}{\epsilon}\eta}$$
$$X_m^* = \left(\frac{\alpha_m}{\omega_m}\right)^\sigma A^{\frac{\eta}{\mu}}\theta^\eta(1-\tau_p)^\eta\left(\frac{\nu}{\mu}\right)^\eta \Phi^{\eta-\sigma}$$

Under monopolistic competition, observe that $\eta - \nu\eta = \eta(1-\nu)$, so that we have

$$\frac{\partial Q^*/\partial\tau_p}{Q^*} = -\frac{\nu\eta}{1-\tau_p} \tag{A.79}$$
$$\frac{\partial P^*/\partial\tau_p}{P^*} = -\frac{\eta}{1-\tau_p}(1-\nu) \tag{A.80}$$
$$\frac{\partial Y^*/\partial\tau_p}{Y^*} = \frac{\partial X_m^*/\partial\tau_p}{X_m^*} = \frac{\partial\Pi_{\text{var}}^*/\partial\tau_p}{\Pi_{\text{var}}^*} = -\frac{\eta}{1-\tau_p} \tag{A.81}$$

with $\eta = \frac{\epsilon}{\nu+\epsilon-\epsilon\nu} = \frac{\mu}{\mu-\nu}$. Notice that for $\nu < 1$, $(\partial P^*/\partial\tau_p)/P^* < 0$, but the magnitude of the derivative will be smaller than the impacts on revenue and input demands. Then increasing returns to scale implies that prices will increase with a demand shock.

A.4.2 Additive shock to demand shifters, $\theta' = \theta - t_\theta$

In the monopolistic case, consider a demand shock of the form $\theta' = \theta - t_\theta$. Given the assumption of isoelastic demand, $P(Q) = \theta Q^{-1/\epsilon}$, it is straightforward to simply replace every instance of θ with $\theta - t_\theta$, and differentiate with respect to t_θ. Thus, we can write

$$\begin{aligned}
Q^* =& A^\eta (\theta - t_\theta)^{\nu\eta} \left(\frac{\nu}{\mu}\right)^{\nu\eta} \Phi^{\nu\eta} \\
Y^* =& A^{\frac{\eta}{\mu}} (\theta - t_\theta)^{\eta} \left(\frac{\nu}{\mu}\right)^{\eta-1} \Phi^{\eta-1} \\
P^* =& A^{-\frac{\eta}{\epsilon}} (\theta - t_\theta)^{(1-\nu)\eta} \left(\frac{\nu}{\mu}\right)^{-\frac{\nu}{\epsilon}\eta} \Phi^{-\frac{\nu}{\epsilon}\eta} \\
X_m^* =& \left(\frac{\alpha_m}{\omega_m}\right)^{\sigma} A^{\frac{\eta}{\mu}} (\theta - t_\theta)^{\eta} \left(\frac{\nu}{\mu}\right)^{\eta} \Phi^{\eta-\sigma}
\end{aligned}$$

Under monopolistic competition, observe that $\eta - \nu\eta = \eta(1-\nu)$, so that we have

$$\frac{\partial Q^*/\partial \tau_p}{Q^*} = -\frac{\nu\eta}{\theta - t_\theta} \tag{A.82}$$

$$\frac{\partial P^*/\partial \tau_p}{P^*} = -\frac{\eta}{\theta - t_\theta}(1-\nu) \tag{A.83}$$

$$\frac{\partial Y^*/\partial \tau_p}{Y^*} = \frac{\partial X_m^*/\partial \tau_p}{X_m^*} = \frac{\partial \Pi_{\text{var}}^*/\partial \tau_p}{\Pi_{\text{var}}^*} = -\frac{\eta}{\theta - t_\theta} \tag{A.84}$$

with $\eta = \frac{\epsilon}{\nu + \epsilon - \epsilon\nu} = \frac{\mu}{\mu - \nu}$. Notice that for $\nu < 1$, $(\partial P^*/\partial \tau_p)/P^* < 0$, but the magnitude of the derivative will be smaller than the impacts on revenue and input demands. Then increasing returns to scale implies that prices will increase with a demand shock.

A.5 Comparative statics of demand shocks that do change elasticities

A.5.1 Vertical demand distortions of the form $P'(Q) = P(Q) - t_p$

A.5.1.1 Implicit solutions.

In the price-taking case, it is straightforward to derive explicit solutions under vertical demand shocks of the form $P' = P - t_p$. One simply substitutes P' for P in any of price-taking solutions.

In the monopolistic case, consider a vertical demand shock, $P'(Q) = P(Q) - t_p$. Profits are given by $(P(Q) - t_p)\, Q - C(Q) = P(Q)Q - (C(Q) - t_p Q)$. Optimal output quantities $Q^*(t_p)$ in the monopolistic case must be defined implicitly as

$$Q \text{ s.t. } \begin{cases} \theta Q^{-1/\epsilon} - \mu \left[\frac{1}{\nu} A^{-\frac{1}{\nu}} \left(\sum_{i=1}^{I} \alpha_i^{\sigma} \omega_i^{1-\sigma} \right)^{\frac{1}{1-\sigma}} Q^{\frac{1-\nu}{\nu}} \right] - \mu t_p = 0 & , 0 < \sigma \neq 1 \\ \theta Q^{-1/\epsilon} - \mu \left[\frac{1}{\nu} A^{-\frac{1}{\nu}} \prod_{i=1}^{I} \left(\frac{\omega_i}{\alpha_i} \right)^{\alpha_i} Q^{\frac{1-\nu}{\nu}} \right] - \mu t_p = 0 & , \sigma = 1 \end{cases} \tag{A.85}$$

By the implicit function theorem, for all $\sigma > 0$, it can be shown that

$$\partial Q^* / \partial t_p \big|_{t_p = 0} = -\mu \nu \eta \frac{Q^*}{P^*} \tag{A.86}$$

with

$$P^*(t_p) = \theta Q^*(t_p)^{-\frac{1}{\epsilon}} - t_p \tag{A.87}$$

$$Y^*(t_p) = \left[\theta Q^*(t_p)^{-\frac{1}{\epsilon}} - t_p \right] Q^*(t_p) = \theta Q^*(t_p)^{\frac{\epsilon-1}{\epsilon}} - t_p Q^*(t_p) \tag{A.88}$$

$$X_m^*(t_p) = \left[Q^*(t_p) \right]^{\frac{1}{\nu}} A^{-\frac{1}{\nu}} \left(\frac{\alpha_m}{\omega_m} \right)^{\sigma} \Phi^{-\sigma} \tag{A.89}$$

$$C^*(t_p) = \left[Q^*(t_p) \right]^{\frac{1}{\nu}} A^{-\frac{1}{\nu}} \Phi^{-1} \tag{A.90}$$

$$\Pi_{\text{var}}^*(t_p) = \theta Q^*(t_p)^{\frac{\epsilon-1}{\epsilon}} - t_p Q^*(t_p) - \left[Q^*(t_p) \right]^{\frac{1}{\nu}} A^{-\frac{1}{\nu}} \Phi^{-1} \tag{A.91}$$

A.5.1.2 Comparative statics.

In the price-taking case, consider a demand shock of the form $P' = P - t_p$. Then we have

$$\frac{\partial Q^* / \partial t_p}{Q^*} = -\frac{\eta - 1}{P - t_p} \tag{A.92}$$

$$\frac{\partial P / \partial t_p}{P} = -\frac{1}{P - t_p} \tag{A.93}$$

$$\frac{\partial Y^* / \partial t_p}{Y^*} = \frac{\partial X_m^* / \partial t_p}{X_m^*} = \frac{\partial \Pi_{\text{var}}^* / \partial t_p}{\Pi_{\text{var}}^*} = -\frac{\eta}{P - t_p} \tag{A.94}$$

with $\eta = \frac{1}{1-\nu}$. Recall that $\nu < 1$ implies $\eta > 1$, with $\eta \to \infty$ as $\nu \to 1$.

In the monopolistic case, consider a vertical demand shock, $P'(Q) = P(Q) - t_p$. Optimal output quantities $Q^*(t_p)$ in the monopolistic case must be defined implicitly as in equation

(A.85), as are optimal revenues and input demands. Consider revenue from equation (A.88), taking the derivative with respect to t_p, evaluated where $t_p = 0$:

$$\begin{aligned}
\left.\frac{\partial Y^*/\partial t_p}{Y^*}\right|_{t_p=0} &= \frac{1}{Y^*}\left(\left[\frac{\epsilon-1}{\epsilon}\theta\left(Q^*\right)^{-\frac{1}{\epsilon}}\right]\frac{\partial Q^*}{\partial t_p} - Q^*\right) \\
&= \frac{1}{Y^*}\left(\frac{1}{\mu}P(Q)\frac{\partial Q^*}{\partial t_p} - Q^*\right) = \frac{1}{Y^*}\left(\frac{1}{\mu}P(Q)\left[-\mu\nu\eta\frac{Q^*}{P^*}\right] - Q^*\right) \\
&= -\frac{Q^*}{Y^*}\left(\nu\eta+1\right)
\end{aligned}$$

or finally, noting that $\nu\eta + 1 = \frac{\epsilon\nu}{\epsilon+\nu-\epsilon\nu} + \frac{\epsilon+\nu-\epsilon\nu}{\epsilon+\nu-\epsilon\nu} = \frac{\epsilon+\nu}{\epsilon+\nu-\epsilon\nu} = \eta + \frac{\nu}{\epsilon}\eta$,

$$\left.\frac{\partial Y^*/\partial t_p}{Y^*}\right|_{t_p=0} = -\frac{\nu\eta+1}{P^*} = -\left(\nu\eta+1\right)\left(Y^*\right)^{\frac{1}{\epsilon-1}}\theta^{-\frac{\epsilon}{\epsilon-1}} \tag{A.95}$$

Taking the derivative of input demands from equation (A.89), we have

$$\left.\frac{\partial X_m^*/\partial t_p}{X_m^*}\right|_{t_p=0} = \frac{1}{\nu}(Q^*)^{-1}\left(-\mu\nu\eta\frac{Q^*}{P^*}\right) = -\frac{\mu\eta}{P^*} = -\mu\eta(Y^*)^{\frac{1}{\epsilon-1}}\theta^{-\frac{\epsilon}{\epsilon-1}} \tag{A.96}$$

while the derivative of price in equation (A.87) is given by

$$\left.\frac{\partial P^*/\partial t_p}{P^*}\right|_{t_p=0} = \frac{1}{P^*}\left(-\frac{1}{\epsilon}\frac{P^*}{Q^*}\frac{\partial Q}{\partial t_p} - 1\right) = \frac{\mu\nu\eta/\epsilon - 1}{P^*} = \left(\mu\nu\eta/\epsilon - 1\right)\left(Y^*\right)^{\frac{1}{\epsilon-1}}\theta^{-\frac{\epsilon}{\epsilon-1}} \tag{A.97}$$

Finally, taking the derivative of variable profits in equation (A.91), it is immediate from the envelope theorem that

$$\left.\partial\Pi^*_{\text{var}}/\partial t_p\right|_{t_p=0} = -Q^* = -(Y^*)^{\frac{\epsilon}{\epsilon-1}}\theta^{-\frac{\epsilon}{\epsilon-1}}$$

and

$$\left.\frac{\partial\Pi^*_{\text{var}}/\partial t_p}{\Pi^*_{\text{var}}}\right|_{t_p=0} = -\frac{(Y^*)^{\frac{\epsilon}{\epsilon-1}}\theta^{-\frac{\epsilon}{\epsilon-1}}}{\left(\frac{\mu-\nu}{\mu}\right)Y^*} = -\theta^{-\frac{\epsilon}{\epsilon-1}}\left(\frac{\mu}{\mu-\nu}\right)\left(Y^*\right)^{\frac{1}{\epsilon-1}} = -\eta(P^*)^{-1} \tag{A.98}$$

Collecting the derivatives in their simplest forms for the monopolistic case, we have

$$\left.\frac{\partial Q^*/\partial t_p}{Q^*}\right|_{t_p=0} = -\frac{\mu\nu\eta}{P^*} \tag{A.99}$$

$$\left.\frac{\partial P^*/\partial t_p}{P^*}\right|_{t_p=0} = \frac{\mu\nu\eta/\epsilon - 1}{P^*} \tag{A.100}$$

and

$$\left.\frac{\partial Y^*/\partial t_p}{Y^*}\right|_{t_p=0} = -\frac{\nu\eta+1}{P^*} \tag{A.101}$$

$$\left.\frac{\partial X_m^*/\partial t_p}{X_m^*}\right|_{t_p=0} = -\frac{\mu\eta}{P^*} \tag{A.102}$$

$$\left.\frac{\partial\Pi^*_{\text{var}}/\partial t_p}{\Pi^*_{\text{var}}}\right|_{t_p=0} = -\frac{\eta}{P^*} \tag{A.103}$$

A.5.2 Horizontal demand distortions of the form $Q'(P) = Q(P) - t_q$

A.5.2.1 Implicit solutions.

In the price-taking case, naturally there cannot be a horizontal demand shock. Thus, I focus only on monopolistic firms.

Consider horizontal demand distortions of the form $Q'(P) = Q(P) - t_q$, with corresponding inverse demand $P'(Q) = \theta(Q + t_q)^{-1/\epsilon}$. Optimal output quantities $Q^*(t_q)$ in the monopolistic case are defined implicitly as

$$Q \ s.t. \begin{cases} \theta(Q+t_q)^{-\frac{1}{\epsilon}} - \frac{1}{\epsilon}\theta(Q+t_q)^{-\frac{1+\epsilon}{\epsilon}}Q - \left[\frac{1}{\nu}A^{-\frac{1}{\nu}}\left(\sum_{i=1}^{I}\alpha_i^{\sigma}\omega_i^{1-\sigma}\right)^{\frac{1}{1-\sigma}}Q^{\frac{1-\nu}{\nu}}\right] = 0 & , \ 0 < \sigma \neq 1 \\ \theta(Q+t_q)^{-\frac{1}{\epsilon}} - \frac{1}{\epsilon}\theta(Q+t_q)^{-\frac{1+\epsilon}{\epsilon}}Q - \left[\frac{1}{\nu}A^{-\frac{1}{\nu}}\prod_{i=1}^{I}\left(\frac{\omega_i}{\alpha_i}\right)^{\alpha_i}Q^{\frac{1-\nu}{\nu}}\right] = 0 & , \ \sigma = 1 \end{cases} \tag{A.104}$$

By the implicit function theorem, for all $\sigma > 0$,

$$\partial Q^*/\partial t_q\big|_{t_q=0} = \frac{\nu\eta/\epsilon}{\epsilon - 1} \tag{A.105}$$

with

$$P^*(t_q) = \theta\left(Q^*(t_q) + t_q\right)^{-\frac{1}{\epsilon}} \tag{A.106}$$

$$Y^*(t_q) = \theta\left(Q^*(t_q) + t_q\right)^{-\frac{1}{\epsilon}}Q^*(t_q) \tag{A.107}$$

$$X_m^*(t_q) = \left[Q^*(t_q)\right]^{\frac{1}{\nu}}A^{-\frac{1}{\nu}}\left(\frac{\alpha_m}{\omega_m}\right)^{\sigma}\Phi^{-\sigma} \tag{A.108}$$

$$C^*(t_q) = \left[Q^*(t_q)\right]^{\frac{1}{\nu}}A^{-\frac{1}{\nu}}\Phi^{-1} \tag{A.109}$$

$$\Pi^*_{\text{var}}(t_q) = \theta\left(Q^*(t_q) + t_q\right)^{-\frac{1}{\epsilon}}Q^*(t_q) - \left[Q^*(t_q)\right]^{\frac{1}{\nu}}A^{-\frac{1}{\nu}}\Phi^{-1} \tag{A.110}$$

A.5.2.2 Comparative statics.

Consider a horizontal demand shock of the form $Q'(P) = Q(P) - t_q = \theta^{\epsilon}P^{-\epsilon} - t_q$, with and corresponding inverse demand $P'(Q) = \theta(Q+t_q)^{-1/\epsilon}$. Optimal output quantities $Q^*(t_q)$ in the monopolistic case must be defined implicitly as in equation (A.104), as are optimal revenues and input demands.

Consider revenue from equation (A.107), taking the derivative with respect to t_q:

$$\begin{aligned} \left.\frac{\partial Y^*/\partial t_q}{Y^*}\right|_{t_q=0} &= \frac{1}{Y^*}\left[-\frac{1}{\epsilon}\frac{P^*}{Q^*}\left(\frac{\partial Q^*}{\partial t_q} + 1\right)Q^* + P^*\frac{\partial Q^*}{\partial t_q}\right] = \frac{P^*}{Y^*}\left[-\frac{1}{\epsilon}\left(\frac{\nu\eta/\epsilon}{\epsilon-1} + 1\right) + \frac{\nu\eta/\epsilon}{\epsilon-1}\right] \\ &= \frac{1}{Q^*}\left[-\frac{1}{\epsilon}\frac{\nu\eta/\epsilon}{\epsilon-1} - \frac{1}{\epsilon} + \frac{\nu\eta/\epsilon}{\epsilon-1}\right] = \frac{1}{Q^*}\left[\frac{\nu\eta/\epsilon}{\epsilon-1}\left(-\frac{1}{\epsilon} + 1\right) - \frac{1}{\epsilon}\right] \\ &= \frac{1}{Q^*}\left[\frac{\nu\eta/\epsilon}{\epsilon-1}\left(\frac{\epsilon-1}{\epsilon}\right) - \frac{1}{\epsilon}\right] = \frac{1}{Q^*}\left[\nu\eta/\epsilon\left(\frac{1}{\epsilon}\right) - \frac{1}{\epsilon}\right] \\ &= \frac{1}{Q^*}\frac{1}{\epsilon}\left(\frac{\nu\eta}{\epsilon} - 1\right) = \frac{1}{Q^*}\frac{1}{\epsilon}(\nu-1)\eta \end{aligned}$$

or finally,

$$\left.\frac{\partial Y^*/\partial t_q}{Y^*}\right|_{t_q=0} = -(1-\nu)\frac{\eta}{\epsilon}(Q^*)^{-1} = -(1-\nu)\frac{\eta}{\epsilon}(Y^*)^{-\frac{\epsilon}{\epsilon-1}}\theta^{\frac{\epsilon}{\epsilon-1}} \tag{A.111}$$

Taking the derivative of input demands from equation (A.108), we have

$$\left.\frac{\partial X_m^*/\partial t_q}{X_m^*}\right|_{t_q=0} = \frac{1}{X^*}\left[\frac{1}{\nu}X^*(Q^*)^{-1}\left(\frac{\nu\eta/\epsilon}{\epsilon-1}\right)\right] = \frac{\eta/\epsilon}{\epsilon-1}(Q^*)^{-1} = \frac{\eta/\epsilon}{\epsilon-1}(Y^*)^{-\frac{\epsilon}{\epsilon-1}}\theta^{\frac{\epsilon}{\epsilon-1}} \tag{A.112}$$

while the derivative of price in equation (A.106) is given by

$$\begin{aligned}
\left.\frac{\partial Y^*/\partial t_q}{Y^*}\right|_{t_q=0} - \left.\frac{\partial Q^*/\partial t_q}{Q^*}\right|_{t_q=0} &= -(1-\nu)\frac{\eta}{\epsilon}(Q^*)^{-1} - \frac{\nu}{\epsilon-1}\frac{\eta}{\epsilon}(Q^*)^{-1} \\
&= -\frac{\eta}{\epsilon}(Q^*)^{-1} + \nu\frac{\eta}{\epsilon}(Q^*)^{-1} - \frac{\nu}{\epsilon-1}\frac{\eta}{\epsilon}(Q^*)^{-1} \\
&= -\frac{\eta(\epsilon-1)}{\epsilon(\epsilon-1)}(Q^*)^{-1} + \frac{\nu\eta(\epsilon-1)}{(\epsilon-1)\epsilon}(Q^*)^{-1} - \frac{\nu}{\epsilon-1}\frac{\eta}{\epsilon}(Q^*)^{-1} \\
&= -\frac{\eta(\epsilon-1)}{\epsilon(\epsilon-1)}(Q^*)^{-1} + \frac{\epsilon\nu\eta-\nu\eta}{(\epsilon-1)\epsilon}(Q^*)^{-1} - \frac{\nu\eta}{(\epsilon-1)\epsilon}(Q^*)^{-1} \\
&= \frac{\epsilon\nu\eta-\nu\eta}{(\epsilon-1)\epsilon}(Q^*)^{-1} + \frac{\eta-\epsilon\eta}{\epsilon(\epsilon-1)}(Q^*)^{-1} - \frac{\nu\eta}{(\epsilon-1)\epsilon}(Q^*)^{-1} \\
&= (\epsilon\nu-\nu-\epsilon+1-\nu)\frac{\eta}{(\epsilon-1)\epsilon}(Q^*)^{-1} \\
&= \left(-\frac{\epsilon}{\eta}+1-\nu\right)\frac{\eta}{(\epsilon-1)\epsilon}(Q^*)^{-1} \\
&= -\frac{1}{\epsilon-1}(Q^*)^{-1} + \frac{1-\nu}{\epsilon-1}\frac{\eta}{\epsilon}(Q^*)^{-1}
\end{aligned}$$

such that

$$\left.\frac{\partial P^*/\partial t_q}{P^*}\right|_{t_q=0} = -\frac{1}{\epsilon-1}(Q^*)^{-1} + \frac{1-\nu}{\epsilon-1}\frac{\eta}{\epsilon}(Q^*)^{-1} \tag{A.113}$$

Notice that this derivative is unambiguously negative. When $\nu > 1$, this is obvious. When $\nu < 1$, observe that the derivative of revenue remains unambiguously negative, while the quantity derivative in equation (A.105) is unambiguously positive. Given that the derivative of revenue is the sum of derivative of quantity and prices, it must be that the derivative of price is negative and greater in absolute value than the quantity derivative, as well as greater in absolute value than the revenue derivative.

Finally, taking the derivative of variable profits in equation (A.91), it is immediate from the envelope theorem that

$$\partial\Pi^*_{\text{var}}/\partial t_q|_{t_q=0} = -\frac{1}{\epsilon}P^* = -\frac{1}{\epsilon}(Y^*)^{-\frac{1}{\epsilon-1}}\theta^{\frac{\epsilon}{\epsilon-1}}$$

and

$$\left.\frac{\partial\Pi^*_{\text{var}}/\partial t_p}{\Pi^*_{\text{var}}}\right|_{t_p=0} = -\frac{1}{\epsilon}\frac{(Y^*)^{-\frac{1}{\epsilon-1}}\theta^{\frac{\epsilon}{\epsilon-1}}}{\left(\frac{\mu-\nu}{\mu}\right)Y^*} = -\frac{1}{\epsilon}\theta^{\frac{\epsilon}{\epsilon-1}}\left(\frac{\mu}{\mu-\nu}\right)(Y^*)^{-\frac{\epsilon}{\epsilon-1}} = -\frac{\eta}{\epsilon}(Q^*)^{-1} \tag{A.114}$$

Collecting the derivatives in their simplest forms for the monopolistic case, we have

$$\left.\frac{\partial Q^*/\partial t_q}{Q^*}\right|_{t_q=0} = \frac{\nu}{\epsilon-1}\frac{\eta}{\epsilon}(Q^*)^{-1} \tag{A.115}$$

$$\left.\frac{\partial P^*/\partial t_q}{P^*}\right|_{t_q=0} = -\frac{1}{\epsilon-1}(Q^*)^{-1} + \frac{1-\nu}{\epsilon-1}\frac{\eta}{\epsilon}(Q^*)^{-1} \tag{A.116}$$

and

$$\left.\frac{\partial Y^*/\partial t_q}{Y^*}\right|_{t_q=0} = -(1-\nu)\frac{\eta}{\epsilon}(Q^*)^{-1} \tag{A.117}$$

$$\left.\frac{\partial X_m^*/\partial t_q}{X_m^*}\right|_{t_q=0} = \frac{1}{\epsilon-1}\frac{\eta}{\epsilon}(Q^*)^{-1} \tag{A.118}$$

$$\left.\frac{\partial \Pi_{\mathrm{var}}^*/\partial t_q}{\Pi_{\mathrm{var}}^*}\right|_{t_q=0} = -\frac{\eta}{\epsilon}(Q^*)^{-1} \tag{A.119}$$