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Economic Shocks and Crime: Evidence from the Brazilian Trade Liberalization

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Economic Shocks and Crime: Evidence from the Brazilian Trade Liberalization*

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Abstract

This paper studies the effect of changes in economic conditions on crime. We exploit the 1990s trade liberalization in Brazil as a natural experiment generating exogenous shocks to local economies. We document that regions exposed to larger tariff reductions experienced a temporary increase in crime following liberalization. Next, we investigate through what channels the trade-induced economic shocks may have affected crime. We show that the shocks had significant effects on potential determinants of crime, such as labor market conditions, public goods provision, and income inequality. We propose a novel framework exploiting the distinct dynamic responses of these variables to obtain bounds on the effect of labor market conditions on crime. Our results indicate that this channel accounts for 75 to 93 percent of the effect of the trade-induced shocks on crime.

JEL Classification: J6, K42, F16

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1 Introduction

In the wake of the Great Recession, there were renewed concerns that the severe economic crisis could fuel a resurgence in crime (see Colvi, 2009, for example). These concerns echoed ideas dating back to the Great Depression of the 1930s and recent discussions about the relationship between economic crises, more broadly, and crime (Fishback et al., 2010; UNODC, 2012). The literature on economic cycles, labor market conditions, and crime has recurrently investigated these issues, but identification challenges remain open (e.g. Cook and Zarkin, 1985; Raphael and Winter-Ebmer, 2001; Finklea, 2011). Despite its relevance in the public debate and important welfare implications, there is no general agreement regarding the effect of economic shocks on criminal activity, and even less about the mechanisms through which these effects may play out.

This paper sheds light on the effect of economic conditions on crime by exploiting local economic shocks brought about by the Brazilian trade liberalization episode. Between 1990 and 1995, Brazil implemented a large-scale unilateral trade liberalization that had heterogeneous effects on local economies across the country. Regions initially specialized in industries exposed to larger tariff cuts experienced deteriorations in labor market conditions relative to the national average (Kovak, 2013; Dix-Carneiro and Kovak, 2015b). Brazil's trade liberalization had a unique feature: it was close to a once-and-for-all event, with tariffs being reduced between 1990 and 1995, and remaining approximately constant afterwards. This allows us to empirically characterize the dynamic response of crime rates to the trade-induced regional economic shocks. It also allows us to explore the timing of the responses of potential mechanisms and to assess their relevance in explaining the observed response of crime.

The Brazilian context is particularly appealing because it is characterized by high incidence of crime. In 2012, the United Nations Office on Drugs and Crime (UNODC) ranked Brazil as the number one country worldwide in absolute number of homicides, with over 50,000 occurrences per year, and 18th in homicide rates, with 25.2 homicides per 100,000 inhabitants. The Economist magazine recently compiled a list of the world's 50 most violent metropolises (cities with populations of 250,000 or more), and 32 of them are located in the country.¹ Brazil also shares many common features with other countries in Latin America and the Caribbean. According to the UNODC, among the 20 most violent countries in the world, 14 are located in the region. These countries have in common as well many other socioeconomic characteristics, such as poor labor market conditions, ineffective educational systems, and high levels of inequality. One could therefore expect economic shocks to have more severe effects on crime, with potentially larger welfare

¹<http://www.economist.com/blogs/graphicdetail/2016/03/daily-chart-18>

implications, in such settings.

Our empirical strategy investigates how crime rates evolved in each local economy as liberalization took place, tracing out its effects over the medium- and long-run horizons. In order to do so, we construct a measure of trade-induced shocks to local economies based on changes in sector-specific tariffs and on the initial sectoral composition of employment in each region, using the methodology proposed by Topalova (2010) and rationalized and refined by Kovak (2013). We refer to these trade-induced shocks as “regional tariff changes” throughout the rest of the paper. We measure crime using homicide data compiled by the Brazilian Ministry of Health, which are the only crime data that can be consistently compared across regions of the country for extended periods of time.²

We start by analyzing the direct effect of regional tariff changes on crime. Our reduced-form results indicate that regions facing larger trade-induced shocks experienced relative increases in crime rates starting in 1995, immediately after the trade reform was complete, and continued experiencing relatively higher crime growth for the following eight years. Before 1995 and after 2003, there is no statistically significant effect of the trade reform on crime. Our placebo exercises show that region-specific trends in crime before the reform were uncorrelated with the (future) trade-induced shocks. This pattern confirms that our results are capturing causal effects of the trade-induced shocks on crime. The baseline specification indicates that a region facing a *reduction* in tariffs of 0.1 log point (corresponding to a movement from the 90th to the 10th percentile of regional tariff changes) experienced a relative *increase* in its crime rate of 0.38 log point (46 percent) five years after liberalization was complete.

Having established the direct effect of these local economic shocks on crime, we move to analyze through which mechanisms these effects may have played out. We focus on three sets of factors that have been linked to crime and violence by the existing literature: (i) labor market conditions such as employment rates and earnings (Raphael and Winter-Ebmer, 2001; Gould et al., 2002; Lin, 2008; Fougère et al., 2009); (ii) public goods provision (Levitt, 1997; Schargrodsky and di Tella, 2004; Jacob and Lefgren, 2003; Lochner and Moretti, 2004; Foley, 2011); and (iii) mental health (stress or depression) and inequality (Fajnzylber et al., 2002; Bourguignon et al., 2003; Card and Dahl, 2011; Fazel et al., 2015).

First, we show that regions specialized in industries exposed to larger reductions in tariffs experienced a deterioration in labor market conditions (employment and earnings) relative to the national average in the medium run (1991-2000), followed by a partial recovery in the long run (1991-2010). The dynamic profile of this labor market response

²Section 3 and Appendix A provide evidence that homicide rates are a good proxy for the overall incidence of crime in Brazil. In addition, in the context of developing countries where underreporting is prevalent and non-random, data on homicides provide less biased measures of the changes in crime and violence (Soares, 2004).

closely mirrors that observed for crime rates.³ Next, we show that the initial deterioration in labor market conditions was accompanied by other signs of contraction in economic activity, including plant closure, reduced formal wage bill, and reduced government revenues. These dimensions are relevant because they directly affect a local government’s tax base and therefore may hinder its ability to provide public goods, which may affect crime. Indeed, we find that regions more exposed to tariff reductions also experienced relative declines in government spending and in public safety personnel, and increases in share of youth (14 to 18 years old) out of school. However, these impacts persisted and were amplified in the long run, in contrast with the recovery observed in labor market conditions as well as in crime rates. Our results also show that there were no significant effects on suicide rates, indicating that mental health and depression do not seem to have played an important role in the response of crime we document. This is an important result, given that we measure criminal activity using homicide rates. Finally, we show that inequality followed a similar path to that observed for the provision of public goods: more exposure to foreign competition was associated with increases in inequality in the medium run, which were amplified in the long run.

The effect of trade shocks on crime follows the same dynamic pattern as the effect on labor market conditions, and both are very different from the dynamic responses observed for public goods provision and inequality. This suggests that the labor market channel is essential to understand how local crime rates responded to this shock. We formalize this argument using an empirical framework in which we assume a stable long-run relationship between crime and its determinants, but the response of these determinants to the one-time trade shock may evolve over time (as it is the case). Next, we argue that, by imposing theoretical sign restrictions on the effects of these determinants, one cannot reproduce the observed dynamic effects of trade shocks on crime without attributing a major role to labor market variables, in particular to the employment rate.

Based on this framework, we develop a strategy to estimate bounds for the effect of labor market conditions on crime. Our methodological innovation shows that one can exploit the distinct dynamic effects of a single shock to achieve partial identification. The preferred estimates from our baseline specification lead to lower and upper bounds for the elasticity of crime with respect to the employment rate of, respectively, -5.6 and -4.5, both statistically significant. These imply that if a region experiences a 10-year decline in its employment rate of one standard deviation (0.07 log point), the crime rate would be expected to increase between 0.32 and 0.39 log point (37 and 48 percent). This is a large economic effect: it represents an increase equivalent to half a standard deviation of the

³Consistent with previous findings of [Dix-Carneiro and Kovak \(2015b\)](#), the long-run recovery in employment reflects increases in informal employment, while formal employment never recovers.

distribution of changes in crime rates across regions between 1991 and 2000. These bounds also indicate that labor market conditions account for 75 to 93 percent of the medium-run effect of the trade-induced economic shocks on crime and constitute the main mechanism through which liberalization affected crime.

According to our framework and theoretical restrictions, the long-run recovery in crime rates in harder hit locations was driven by the recovery in employment rates. In earlier work, [Dix-Carneiro and Kovak \(2015b\)](#) find that the long-run recovery in employment rates in harder hit locations is entirely driven by an expansion of the informal sector – employment in the formal sector never recovers. Therefore, informal employment seems to have been able to keep individuals away from crime. This result suggests that enforcement of labor regulations that tend to reduce informality but increase unemployment may exacerbate the response of crime to economic downturns.

This paper contributes to the literature in three dimensions. First, we provide credible estimates of the effect of economic shocks on criminal activity and make progress in understanding the mechanisms behind this effect. Second, we contribute to a recent but growing literature stressing adjustment costs to trade shocks beyond those associated with the labor market.⁴ The fact that crime has an important externality dimension adds particular interest to this point, since it means that the socioeconomic implications of trade shocks go beyond the costs and benefits incurred by the individuals directly affected by them. Finally, the paper contributes to the literature on the effects of labor market conditions on crime ([Raphael and Winter-Ebmer, 2001](#); [Gould et al., 2002](#); [Lin, 2008](#); [Fougère et al., 2009](#)). In contrast to the Bartik shocks typically used as local labor demand shifters in this literature, we know precisely the source of the shock (changes in import tariffs), providing a more transparent source of exogenous variation.⁵ Our results suggest that these Bartik shocks are unlikely to satisfy the exclusion restriction required by an instrumental variables estimator. The combination of our natural experiment with our empirical strategy allows us to make progress relative to the previous literature and to provide bounds on the effect of local labor market conditions on crime. This is only possible because the shock captures an event that is discrete in time and permanent, which allows us to exploit the evolution of its effects over time.

The remainder of the paper is structured as follows. Section 2 provides a background of the 1990s trade liberalization in Brazil and of its documented effect on local labor

⁴For example, recent studies have estimated the effects of trade shocks on crime ([Iyer and Topalova, 2014](#); [Che and Xu, 2016](#); [Deiana, 2016](#)), the provision of public goods ([Feler and Senses, 2016](#)), health and mortality ([McManus and Schaur, 2016](#); [Pierce and Schott, 2016](#)), household structure ([Autor et al., 2015](#)) and political outcomes ([Dippel et al., 2015](#); [Autor et al., 2016](#); [Che et al., 2016](#)).

⁵[Bartik \(1991\)](#) predicts changes in local labor demand based on national changes in industry-specific employment and wages and on each region's initial industrial structure. This procedure is widely used in labor economics to construct instruments for shifts in local labor demand.

markets. Section 3 describes the data we use and provides descriptive statistics. Section 4 presents our empirical strategy and the results related to the effect of the trade-induced regional shocks on crime. Section 5 sheds light on the mechanisms behind the relationship between the trade shocks and crime. Section 6 relates our paper to the literature on labor market conditions and crime. Finally, Section 7 closes the paper with a broader discussion and interpretation of the results.

2 Trade Liberalization and Local Economic Shocks in Brazil

2.1 The Brazilian Trade Liberalization

Starting in the late 1980s and early 1990s, Brazil undertook a major unilateral trade liberalization process which was fully implemented between 1990 and 1995. The trade reform broke with nearly one hundred years of very high barriers to trade, which were part of a deliberate import substitution policy. Nominal tariffs were not only high, but also did not represent the *de facto* protection faced by industries, since there was a complex and non-transparent structure of additional regulations. There were 42 “special regimes” allowing tariff reductions or exemptions, tariff redundancies, and widespread use of non-tariff barriers (quotas, lists of banned products, red tape), as well as various additional taxes (Kume et al., 2003). During the 1988-1989 period, tariff redundancy, special regimes, and additional taxes were partially eliminated. This constituted a first move toward a more transparent system, where tariffs actually reflected the structure of protection. However, up to that point, there was no significant change in the level of protection faced by Brazilian producers (Kume et al., 2003).

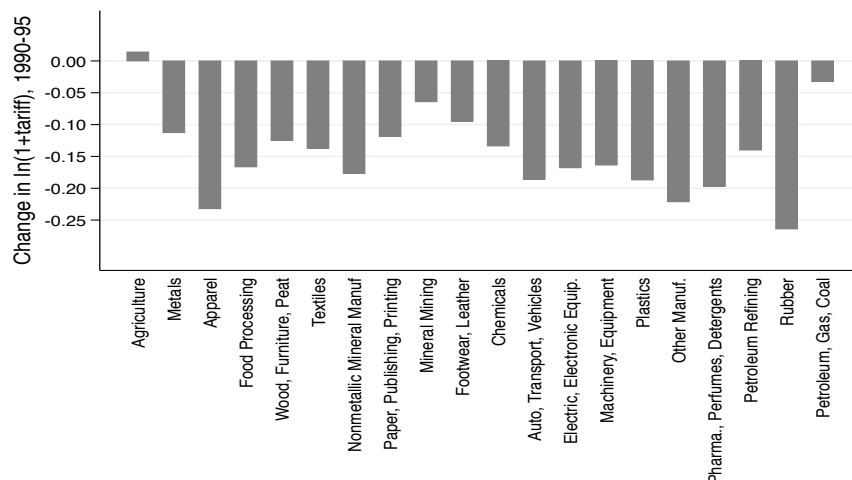
Trade liberalization effectively started in March 1990, when the newly elected president unexpectedly eliminated non-tariff barriers (e.g. suspended import licenses and special customs regime), often immediately replacing them with higher import tariffs in a process known as “tariffication” (*tarificação*, see de Carvalho, Jr., 1992). Although this change left the effective protection system unaltered, it left tariffs as the main trade policy instrument. Thus, starting in 1990, tariffs accurately reflected the level of protection faced by Brazilian firms across industries. Consequently, the tariff reductions observed between 1990 and 1995 provide a good measure of the extent and depth of the trade liberalization episode.⁶ Nominal tariff cuts were very large in some industries and the average tariff fell from 30.5 percent in 1990 to 12.8 percent in 1995.⁷ Figure 1 shows the approximate percentage

⁶Changes in tariffs after 1995 were trivial compared to the changes that occurred between 1990 and 1995. See discussion in Appendix B.

⁷We focus on changes in output tariffs to construct our measure of trade-induced local labor demand shocks (or regional tariff changes), to be formally defined in the next Section. An alternative would be to use effective rates of protection, which include information on both input and output tariffs, measuring

change in sectoral prices induced by changes in tariffs (we plot the change in the log of one plus tariffs in the figure, since this is the measure of tariff changes used in our empirical analysis).⁸ Importantly, there was ample variation in tariff cuts across sectors, which will be essential to our identification strategy. The tariff data we use throughout this paper are provided by Kume et al. (2003), and have been extensively used in the literature on trade and labor markets in Brazil.

Figure 1: Changes in $\ln(1 + \text{tariff})$, 1990-1995



Source: Dix-Carneiro and Kovak (2015b).

Finally, tariff cuts were almost perfectly correlated with pre-liberalization tariff levels (correlation coefficient of -0.90), as sectors with initially higher tariffs experienced larger subsequent reductions. This led not only to a reduction in the average tariff, but also to a homogenization of tariffs: the standard deviation of tariffs fell from 14.9 percent to 7.4 percent over the period. Baseline tariffs reflected the level of protection defined decades earlier (in 1957, see Kume et al., 2003), so this pattern lessens concerns regarding the political economy of tariff reduction, as sectoral and regional idiosyncrasies seem to be almost entirely absent (see Goldberg and Pavcnik, 2003; Pavcnik et al., 2004; Goldberg and Pavcnik, 2007, for discussions). We revisit this point when performing robustness

the effect of the entire tariff structure on value added per unit of output in each industry. At the level of aggregation used in this paper, the finest possible level that makes the industry classification of Kume et al. (2003)'s tariffs compatible with the 1991 Demographic Census, 1990-1995 changes in input tariffs are almost perfectly correlated with changes in output tariffs. Consequently, regional tariff changes computed using changes in output tariffs and using changes in effective rates of protection are also almost perfectly correlated (the correlation is greater than 0.99 when we use the effective rates of protection calculated by Kume et al. (2003)). Conducting the analysis using changes in output tariffs or effective rates of protection has little to no effect on any of the results of this paper.

⁸The price of good j , P_j , is given by $P_j = P_j^* (1 + \tau_j)$, where P_j^* is the international market price of good j and τ_j is the import tariff imposed on that good. Under a small open economy assumption, $\Delta \log(P_j) = \Delta \log(1 + \tau_j)$.

exercises in the results section.

2.2 Trade-Induced Local Economic Shocks

Our measure of local economic shocks follows the empirical literature on regional labor market effects of foreign competition, which exploits the fact that regions within a country often specialize in the production of different goods. In addition to different specialization patterns of production across space, trade shocks affect industries in varying degrees. Therefore, the interaction between sector-specific trade shocks and sectoral composition at the regional level provides a measure of trade-induced shocks to local labor demand. For example, tariffs in Apparel fell from 51.1 percent to 19.8 percent between 1990 and 1995, whereas tariffs in Agriculture increased from 5.9 percent to 7.4 percent over the same period. In the presence of substantial barriers to mobility across regions, we would expect that economic conditions would have deteriorated more in regions more specialized in harder-hit sectors.

Although the idea above was initially introduced by [Topalova \(2010\)](#), [Kovak \(2013\)](#) formalized and refined it in the context of a specific-factors model. We follow [Kovak \(2013\)](#) and define our local economic shock as the “Regional Tariff Change” in region r , which effectively measures by how much trade liberalization affected labor demand in the region. RTC_r is the average tariff change faced by region r , weighted by the importance of each sector in regional employment. Formally:

$$RTC_r = \sum_{i \in T} \psi_{ri} \Delta \log(1 + \tau_i), \text{ with}$$

$$\psi_{ri} = \frac{\frac{\lambda_{ri}}{\varphi_i}}{\sum_{j \in T} \frac{\lambda_{rj}}{\varphi_j}},$$

where τ_i is the tariff on industry i , λ_{ri} is the initial share of region r workers employed in industry i , φ_i equals one minus the wage bill share of industry i , and T denotes the set of all tradable industries (manufacturing, agriculture and mining). One of the advantages of the treatment in [Kovak \(2013\)](#) is that it explicitly shows how to incorporate non-tradable sectors into the analysis. Because non-tradable output must be consumed within the region where it is produced, non-tradable prices move together with prices of locally-produced tradable goods. Therefore, the magnitude of the trade-induced regional shock depends only on how the local tradable sector is affected (see [Kovak, 2013](#), for further discussion and details).

3 Data

3.1 Local Economies

We conduct our analysis at the micro-region level, which is a grouping of economically integrated contiguous municipalities with similar geographic and productive characteristics. Micro-regions closely parallel the notion of local economies and have been widely used as the units of analysis in the literature on the local labor market effects of trade liberalization in Brazil (Kovak, 2013; Costa et al., 2015; Dix-Carneiro and Kovak, 2015a,b; Hirata and Soares, 2015).⁹ Although the Brazilian Statistical Agency IBGE (*Instituto Brasileiro de Geografia e Estatística*) periodically constructs mappings between municipalities and micro-regions, we adapt these mappings given that municipalities change boundaries and are created and extinguished over time. Therefore, we aggregate municipalities to obtain minimally comparable areas (Reis et al., 2008) and construct micro-regions that are consistently identifiable from 1980 to 2010. This process leads to a set of 411 local economies, as in Dix-Carneiro and Kovak (2015a) and Costa et al. (2015).¹⁰ Table 1 provides descriptive statistics at the micro-region level for the main variables used in our empirical analysis. The respective data sources are discussed in the following sections.

3.2 Crime

We use homicide rates computed from mortality records as a proxy for the overall incidence of crime. These records come from DATASUS (*Departamento de Informática do Sistema Único de Saúde*), an administrative dataset from the Ministry of Health that contains detailed information on deaths by external causes classified according to the International Statistical Classification of Diseases and Related Health Problems (ICD).¹¹ We use annual data aggregated to the micro-region level from 1980 to 2010.¹²

Both the homicide rate and the total number of homicides have increased substantially

⁹A potential concern in this context would be commuting across micro-regions. But note that only 3.2 and 4.6 percent of workers lived and worked in different micro-regions in, respectively, 2000 and 2010.

¹⁰The micro-regions we use in this paper are slightly more aggregated versions than the ones in Kovak (2013) and Dix-Carneiro and Kovak (2015b) who use minimally comparable areas over shorter periods (1991 to 2000 and 1991 to 2010, respectively). As in these other papers, we drop the region containing the free trade zone of Manaus, since it was exempt from tariffs and unaffected by the tariff changes that occurred during the 1990s trade liberalization.

¹¹The ICD is published by the World Health Organization. It changed in 1996, but the series remain comparable. From 1980 through 1995, we use the ICD-9 (categories E960-E969) and from 1996 through 2010 we use the ICD-10 (categories X85-Y09).

¹²Since our econometric specifications make use of changes in logs of crime rates, we add one to the number of homicides in each region to avoid sample selection issues that would arise from dropping regions with no reported homicides in at least one year. We obtain nearly identical results when we do not add one to the number of homicides in each region. We also obtain very similar results if our measure of homicides in region r and year t is given by an average of homicides between years $t - 1$ and t . In that case, only four regions are excluded from the regressions due to zeros.

over the past 30 years in Brazil, with the homicide rate in 2010 being more than 2.5 times higher than in 1980, while the total number of homicides increased five-fold, from around 10,000 to 50,000 deaths per year. These numbers put Brazil in the first place worldwide in terms of number of homicides and in 18th place in terms of homicide rates (UNODC, 2013). The dispersion of homicide rates across micro-regions is also high: the 10th and 90th percentiles of the distribution corresponded to, respectively, 2.5 and 30 in 1991, and 2.9 and 34 in 2000.

In Figure 2, Panel (a), we show how log-changes in crime rates between 1991 and 2000 ($\Delta_{91-00} \log(CR_r)$) are distributed across local economies. Since we will be contrasting changes in the log of local crime rates to regional tariff changes (RTC_r), Figure 2 also presents the distribution of RTC_r across micro-regions (Panel (b)). It shows that there is a large degree of heterogeneity in changes in homicide rates and trade-induced shocks across regions.

One potential concern with the use of homicides to represent the overall incidence of crime is that less extreme forms of violence are typically more prevalent. In addition, economic crimes might seem more adequate categories to analyze the response of crime to deteriorations in economic conditions. Unfortunately, in the case of Brazil, police records are not compiled systematically in a comparable way at the municipality (or micro-region) level. Even for the very few states that do provide statistics at more disaggregate levels, the available series start only in the early 2000s, many years after the trade liberalization period and, therefore, are not suitable for our analysis. For these reasons, homicides recorded by the health system are the only type of crime that can be followed over extended periods of time and across all regions of the country. Homicides are also considered more reliable crime statistics in the context of developing countries, where underreporting of less serious offenses tends to be non-random and widespread (Soares, 2004).

Nevertheless, we explicitly address this concern using data from the states of São Paulo and Minas Gerais for the period between 2001 and 2011. These are the two most populous states in Brazil, comprising 32 percent of the total population, and they provide disaggregated police compiled statistics since the early 2000s for certain types of crime. Appendix A presents correlations between levels and changes in crime rates in 5-year windows between 2001 and 2011 for São Paulo and Minas Gerais, for four types of crime: homicides recorded by the health system (our dependent variable), homicides recorded by the police, violent crimes against the person (excluding homicides), and violent property crimes.¹³ We focus on violent crimes since these are supposed to suffer less from

¹³Violent property crimes refer to robberies in both states. Violent crimes against the person refer to rape in São Paulo and to rape, assaults, and attempted homicides in Minas Gerais. The data are provided by the statistical agencies of the two states (Fundação SEADE for São Paulo and Fundação João Pinheiro for Minas Gerais).

underreporting bias. Our measure of homicides is highly correlated, both in levels and in (5-year) changes, to police-recorded homicides, to property crimes, and to crimes against the person. This pattern is similar if we consider 1- or 10-year intervals as well (Tables A.2 and A.3), or if we condition on time and micro-region fixed effects (Tables A.4 and A.5). At the level of micro-regions in Brazil, homicide rates seem indeed to be a good proxy for the overall incidence of crime.

The strong correlations between homicides and other types of crime reflect the fact that property crime and drug trafficking in Brazil are usually undertaken by armed individuals, and homicides sometimes arise as collateral damage of these activities. Violence is also typically used as a way to settle disputes among agents operating in illegal markets and among common criminals (Chimeli and Soares, 2016). Even though there are no official statistics on the motivations behind homicides in Brazil, available ethnographic evidence suggest that at least 40 percent of homicides in urban areas – and possibly much more – are likely to be linked to typical economic crimes (e.g. robberies) and to illegal drug trafficking (Lima, 2000; Saporì et al., 2012).

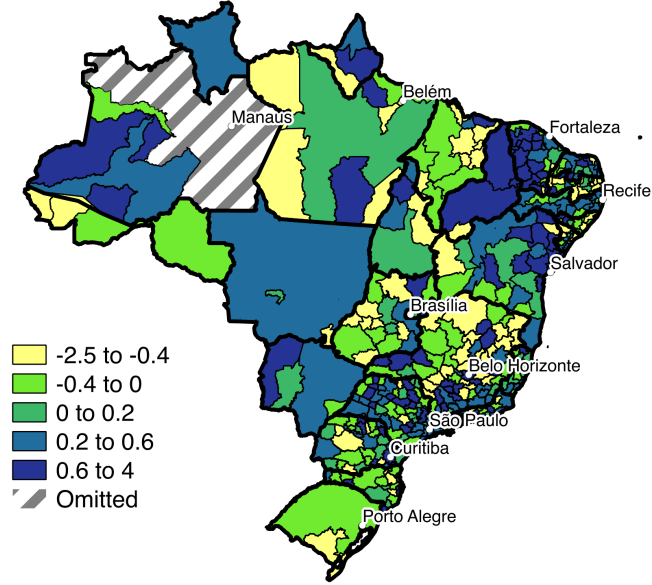
3.3 Other Variables

We use four waves of the Brazilian Demographic Census covering thirty years (1980–2010) to compute several variables of interest. First, we use the Census to construct the two main labor market outcomes at the individual level, namely, total labor market earnings and employment status (employed or not employed). We also use individual-level data to estimate per capita household income inequality and socio-demographic characteristics (education, age, and urban location) when necessary. In addition, we use the Census data to estimate the number of workers employed in occupations related to public safety in each region. These consist of jobs in the civil and military police as well as security guards. Appendix C explains in further detail other treatments we apply to some variables extracted from the Census.

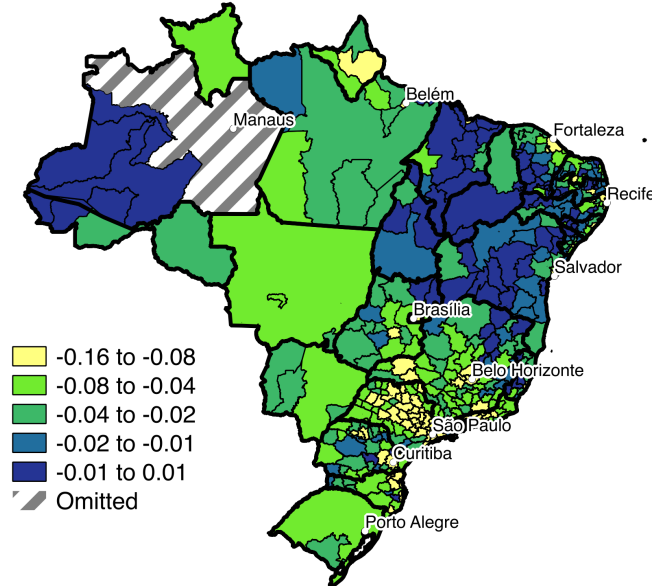
We obtain annual spending and revenue for local government from the Ministry of Finance (*Ministério da Fazenda – Secretaria do Tesouro Nacional*).¹⁴ Finally, we use the RAIS data set (*Registro Anual de Informações Sociais*) to compute the number of formal establishments and the formal wage bill for each micro-region. RAIS is an administrative data set collected by the Ministry of Labor covering the universe of formal firms and workers. Table 1 provides descriptive statistics for our main variables at the micro-region level.

¹⁴The data goes back to 1985 but it is often unreliable, partly because of measurement error due to hyperinflation and frequent missing information. For this reason we focus on data after Brazil stabilized its currency, that is, from 1994 onwards.

Figure 2: Log-Changes in Local Crime Rates and Regional Tariff Changes



(a) Distribution of Log-Changes in Local Crime Rates: 1991–2000



(b) Distribution of Regional Tariff Changes, RTC_r

Source: Crime rates correspond to homicide rates per 100,000 inhabitants computed from DATASUS (*Departamento de Informática do Sistema Único de Saúde*). Regional tariff changes, RTC_r , computed according to the formulae in Section 2.2.

Table 1: Descriptive Statistics at the Micro-Region Level

Variable	Source	1991		2000		2010	
		Mean	SD	Mean	SD	Mean	SD
Crime Rate (per 100,000 inhabitants)	DataSUS	14.2	10.7	15.8	13.1	22.4	14.5
Suicide Rate (per 100,000 inhabitants)	DataSUS	4.1	3.0	4.7	3.2	6.3	3.2
Real Monthly Earnings (2010 R\$)	Census	754.9	338.4	920.0	372.6	992.3	332.1
Employment Rate	Census	0.60	0.05	0.60	0.06	0.64	0.08
Share Young (18 to 30 years old)	Census	0.22	0.02	0.23	0.01	0.23	0.02
Share Unskilled, ≥ 18 years	Census	0.48	0.04	0.47	0.04	0.45	0.05
Share Young, Unskilled and Male	Census	0.09	0.01	0.08	0.01	0.06	0.02
Share Urban	Census	0.61	0.20	0.68	0.18	0.73	0.17
Public Safety Personnel (per 100,000 inhabitants)	Census	614	332	709	341	761	331
High School Dropouts	Census	0.55	0.09	0.32	0.06	0.26	0.04
Gini (Household Income per Capita)	Census	0.55	0.04	0.56	0.04	0.52	0.04
Population	Census	353,130	929,562	407,750	1,046,677	457,060	1,143,856
Gov. Spending per Capita (Annual, 2010 R\$) *	Finance Ministry	342.4	182.8	820.9	331.5	1,061.0	319.0
Gov. Revenue per Capita (Annual, 2010 R\$) *	Finance Ministry	325.1	161.2	862.9	348.5	1,632.8	556.7
Formal Wage Bill per Capita (Annualized, 2010 R\$)	RAIS and Census	778.2	976.4	1,299.1	1,365.4	2,743.2	2,442.2
Number of Formal Establishments	RAIS	3,050	12,709	5,015	16,569	7,197	21,597

Notes: Data on 411 micro-regions. Crime rates are computed as homicide rates per 100,000 inhabitants; suicide rates are also computed per 100,000 inhabitants; the share of unskilled individuals is computed as the fraction of individuals in the population who have completed middle school or less and are 18 years old or more; the share of public safety personnel corresponds to the fraction of the population working in public safety jobs (military and civil police, security guards); high school dropouts corresponds to the share of 14–18 year old children who are not in school; the formal wage bill for each region sums all December formal labor earnings of each year (and annualizes it multiplying by 12 months).

* Due to data quality issues, we use government spending and revenue information starting in 1994 (see text). For these variables, 1994 values are reported in the 1991 column.

4 Local Trade Shocks and Crime Rates

This section investigates if the local economic shocks brought about by the Brazilian trade liberalization translated into changes in crime rates. Given that the trade shock we exploit is discrete in time and permanent, we follow the methodology proposed by [Dix-Carneiro and Kovak \(2015b\)](#) and empirically describe the evolution of the response of crime to regional tariff changes. In Section 5, we exploit the dynamic response of crime to help distinguishing the channels through which these effects propagated.

4.1 Medium- and Long-Run Effects

A unique feature of Brazil’s trade liberalization is that it was close to a once-and-for-all event: tariffs were reduced between 1990 and 1995, but remained approximately constant afterwards. This allows us to empirically characterize the dynamic response of crime rates to the trade-induced regional economic shocks. We use the following specification to compare the evolution of crime rates in regions facing larger tariff reductions to those in regions facing smaller tariff declines:

$$\log(CR_{r,t}) - \log(CR_{r,1991}) = \xi_t + \theta_t RTC_r + \epsilon_{r,t}, \quad (1)$$

where $CR_{r,t}$ is the crime rate in region r at time $t > 1991$.¹⁵ In all specifications we cluster standard errors at the meso-region level to account for potential spatial correlation in outcomes across neighboring regions.^{16,17}

Table 2 presents estimates from equation (1) analyzing the medium-run effect, $\hat{\theta}_{2000}$, of the trade-induced local shocks on crime. We start in column 1 with a specification that corresponds to a univariate regression relating log-changes in local homicide rates to regional tariff changes, without additional controls and without weighting observations. There is a significant negative relationship between changes in homicide rates and regional tariff changes, indicating that regions that faced larger exposure to foreign competition (more negative RTC_r) also experienced increases in crime rates relative to the national average. In column 2, we follow most of the literature on crime and health and weight the same specification from column 1 by the average population between 1991 and 2000, with little noticeable change in the results.¹⁸

In column 3, we add state fixed effects to the specification from column 2 (27 fixed effects, corresponding to 26 states plus the federal district), to account for state-level

¹⁵We use 1991, instead of 1990, as the starting point because the former was a Census year. In the next section, we use Census data to analyze the response of the potential mechanisms to the trade shock and we want these two sets of results to be directly comparable. This choice is inconsequential for the results we report.

¹⁶Meso-regions are groupings of micro-regions and are defined by the Brazilian Statistical Agency IBGE. Note that we also need to aggregate a few IBGE meso-regions to make them consistent over the

Table 2: Regional Tariff Changes and Log-Changes in Local Crime Rates: 1991–2000

Dep. Var.: $\Delta_{91-00} \log(CR_r)$	OLS	OLS	OLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)
RTC_r	-1.976** (0.822)	-2.444*** (0.723)	-3.838*** (1.426)	-3.769*** (1.365)	-3.853*** (1.403)
$\Delta_{80-91} \log(CR_r)$				-0.303*** (0.0749)	0.0683 (0.129)
State Fixed Effects	No	No	Yes	Yes	Yes
Kleibergen-Paap Wald rk F statistic					54.2
Observations	411	411	411	411	411
R-squared	0.013	0.052	0.346	0.406	–

Notes: DATASUS data. Standard errors (in parentheses) adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. Columns: (1) Observations are not weighted; (2) Observations are weighted by population; (3) Adds state fixed effects to (2); (4) Adds pre-trends to (3); (5) Two-Stage Least Squares, with an instrument for $\Delta_{80-91} \log(CR_r)$ (see text).

Significant at the *** 1 percent, ** 5 percent, * 10 percent level.

changes potentially driven by state-specific policies.^{19,20} The magnitude of the coefficient increases by more than 50 percent and remains strongly significant. This indicates that some of the states that faced greater exposure to foreign competition following liberalization also displayed other time varying characteristics that contributed to reduce crime, initially biasing the coefficient toward zero.

In columns 4 and 5 we estimate the same specification from column 3, but controlling for log-changes in local homicide rates between 1980 and 1991. This specification addresses concerns about pre-existing trends in region-specific crime rates that could be correlated with (future) trade-induced local shocks. In column 4 we include this variable as an additional control and estimate the equation by OLS. A potential problem with this procedure is that the log of 1991 crime rates appears both in the right and left hand side of the estimating equation, potentially introducing a mechanical bias and contaminating all of the remaining coefficients. We address this problem in column 5, where we instrument pre-existing trends $\Delta_{80-91} \log(CR_r)$ with $\log\left(\frac{\text{Total Homicides}_{r,1990}}{\text{Total Homicides}_{r,1980}}\right)$. In either case, there is very little change in the coefficient of interest, indicating that the estimated relationship

1980-2010 period.

¹⁷In practice, we estimate equation (1) year by year.

¹⁸In the health literature, the realized mortality rate from a certain condition is often seen as an estimator for the underlying mortality probability. The variance of this estimator is inversely proportional to population size (see, for example, Deschenes and Moretti, 2009 and Burgess et al., 2014).

¹⁹By constitutional mandate, several policies and institutions in Brazil are decentralized to state governments (for example, public security, and part of the justice system, and of health and educational policies). Therefore, controlling for state fixed effects accounts for these unobserved policies, which are likely to be correlated with local economic conditions.

²⁰By adding state fixed effects, we exploit variation in RTC_r across micro-regions within states.

between changes in crime rates and regional tariff changes is not driven by pre-existing trends.

The effect of regional tariff changes on crime rates is considerable. Moving a region from the 90th percentile to the 10th percentile of the distribution of regional tariff changes means a change in RTC_r equivalent to -0.1 log point. Column 3 of Table 2 predicts that this movement would be accompanied by an increase in crime rates of 0.38 log point, or 46 percent. To put this effect into perspective, note that the standard deviation of $\Delta_{91-00} \log(CR_r)$ across regions is of 0.7 log point, so an increase in crime rates of 0.38 log point is equivalent to an increase of approximately half a standard deviation in decadal changes in log crime rates.

Table 3 reproduces the same exercises from Table 2, but focuses on the long-run effect of regional tariff changes, $\hat{\theta}_{2010}$. As opposed to the results in Table 2, columns 1 and 2 indicate a positive and statistically significant relationship between the log-changes in crime rates and regional tariff changes. However, once we control for state fixed effects (columns 3 to 5), the coefficients become negative, much smaller in magnitude than the medium-run coefficients, and not statistically significant. As before, this changing pattern in the long-run coefficient indicates that states experiencing more negative shocks also experienced other changes that tended to reduce crime. Once we control for common state characteristics, there is no noticeable relationship between log-changes in crime rates and regional tariff changes over the 1991-2010 interval.

Table 3: Regional Tariff Changes and Log-Changes in Local Crime Rates: 1991–2010

Dep. Var.: $\Delta_{91-10} \log(CR_r)$	OLS	OLS	OLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)
RTC_r	5.293*** (1.494)	6.668** (2.899)	-1.324 (2.454)	-1.198 (2.265)	-1.340 (2.437)
$\Delta_{80-91} \log(CR_r)$				-0.514*** (0.0902)	0.0681 (0.227)
State Fixed Effects	No	No	Yes	Yes	Yes
Kleibergen-Paap Wald rk F statistic					52.2
Observations	411	411	411	411	411
R-squared	0.066	0.133	0.642	0.702	–

Notes: DATASUS data. Standard errors (in parentheses) adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. Columns: (1) Observations are not weighted; (2) Observations are weighted by population; (3) Adds state fixed effects to (2); (4) Adds pre-trends to (3); (5) Two-Stage Least Squares, with an instrument for $\Delta_{80-91} \log(CR_r)$ (see text). Significant at the *** 1 percent, ** 5 percent, * 10 percent level.

One important concern with our estimates is that the RTC_r shocks may be correlated

with pre-existing trends in the outcome of interest. For this reason, Tables 2 and 3 included pre-existing trends in log crime rates as an additional control to rule out that the estimated effects were driven by a (coincidental) correlation between pre-existing trends and (future) regional tariff changes. The results show that pre-trends have no effect on our estimates of interest, indicating that pre-existing trends are not likely to be a challenge to our identification strategy. Table 4 corroborates this conclusion and shows that regional tariff changes are uncorrelated with pre-trends by directly regressing pre-liberalization changes in crime on (future) trade shocks. In all specifications, the coefficients are small in magnitude, with opposite signs to those from Table 2, and not statistically significant.

Table 4: 1980-1991 Log-Changes in Crime Rates and Regional Tariff Changes – Placebo Tests

Dep. Var.: $\Delta_{80-91} \log(CR_r)$	(1)	(2)	(3)
RTC_r	0.727 (1.096)	0.200 (1.409)	0.162 (0.893)
State Fixed Effects	No	No	Yes
Observations	411	411	411
R-squared	0.002	0.000	0.426

Notes: DATASUS data. Standard errors (in parentheses) adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. Columns: (1) Observations are not weighted; (2) Observations are weighted by population; (3) Adds state fixed effects to (2).

Significant at the *** 1 percent, ** 5 percent, * 10 percent level.

It is important to emphasize that the estimation of θ_t in equation (1) can only reveal *relative* effects of Brazil’s trade liberalization on crime. This is a well-known limitation of reduced-form estimates in the presence of important general equilibrium effects, which is a common feature of all trade and local labor markets literature. These general equilibrium effects, common to all units, will be absorbed in the intercept ξ_t . Therefore, we cannot make statements about the total effect of the trade reform on the national crime level without imposing restrictive theoretical assumptions. A full structural model quantifying absolute effects of trade on crime is out of the scope of this paper and is suggested as future work on the topic. Nevertheless, the variation we explore reveals the relationship between local economic shocks and crime rates by comparing regions with different degrees of exposure to the trade shock.

4.2 Dynamic Effects

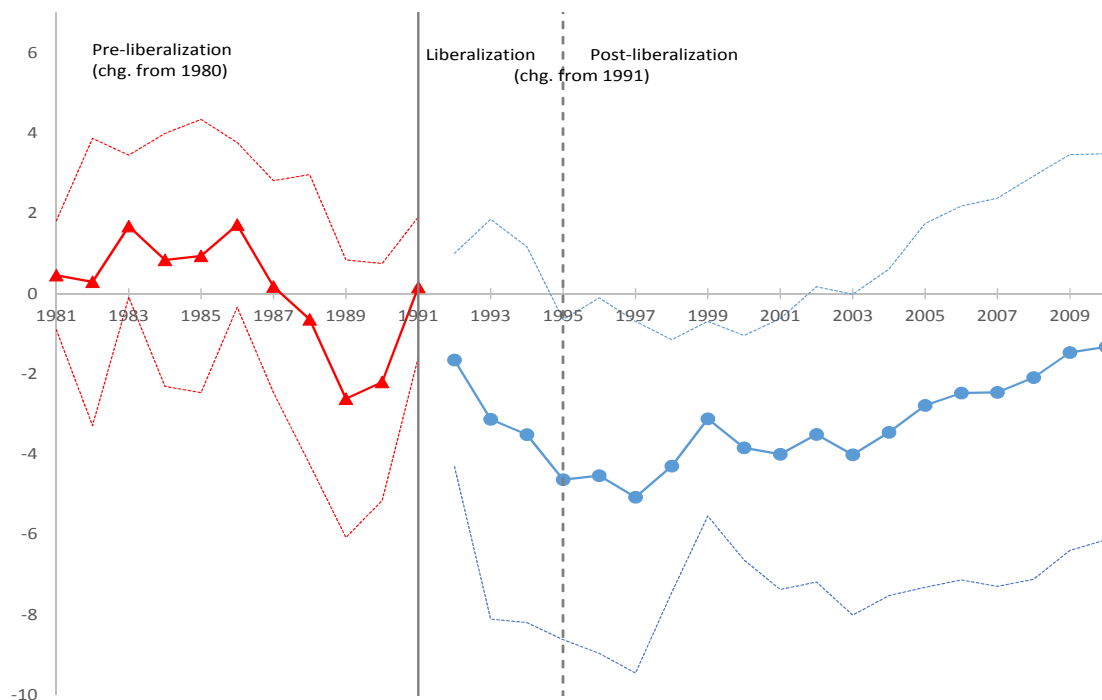
The previous section documented that the trade-induced local shocks had a strong effect on crime rates, but that the effect was temporary. Regions that were hit harder by liberalization experienced relative increases in crime rates in the medium run (1991 to 2000), but these increases vanished in the long run (1991 to 2010). Here, we confirm this pattern by plotting the yearly evolution of the effect of the trade shocks on crime ($\hat{\theta}_t$ for $t = 1992, \dots, 2010$) in Figure 3. Given that we view liberalization approximately as a one-time permanent shock that unfolded between 1990 and 1995, we interpret the evolution of $\hat{\theta}_t$ as the empirical dynamic response of crime rates to the local shocks RTC_r . The points in the figure for 2000 and 2010 correspond to the RTC_r coefficients in columns (3) of Tables 2 and 3. The circular blue markers in Figure 3 show that harder-hit regions experienced gradual increases in crime relative to the national average over the years immediately following the end of trade liberalization, but these increases eventually receded. Note that we present coefficient estimates for 1992-94, but these should be interpreted with care, as liberalization was still an ongoing process during these intermediate years.²¹

Figure 3 also shows a series of pre-liberalization coefficients, in which the dependent variable is the change in log crime rates between 1980 and the year listed on the x-axis, and the independent variable is RTC_r . None of these coefficients is statistically significant, corroborating the conclusion that pre-existing trends in regional crime rates were uncorrelated with the shocks induced by trade liberalization.

Together, the results from this section indicate that the liberalization-induced economic shocks had a strong causal effect on crime rates over the short and medium runs, but that this effect vanished in the long run. We now investigate through what channels these local economic shocks affected crime.

²¹However, the tariff cuts were almost fully implemented by 1993, so these early coefficients are still informative regarding liberalization's short-run effects. When regressing RTC_r on an alternate version measuring tariff changes from 1990-93, the R^2 is 0.93.

Figure 3: Dynamic Effects of Regional Tariff Changes on Log-Changes in Local Crime Rates



Each point reflects an individual regression coefficient, $\hat{\theta}_t$ following (1), where the dependent variable is the change in regional log crime rates and the independent variable is the regional tariff change (RTC_r). Note that RTC_r always reflects tariff changes from 1990-1995. For blue circles, the changes are from 1991 to the year listed on the x-axis. For red triangles, the changes are from 1980 to the year listed. All regressions include state fixed effects. Negative estimates imply larger crime increases in regions facing larger tariff reductions. Vertical bars indicate that liberalization began in 1991 and was complete by 1995. Dashed lines show 95 percent confidence intervals. Standard errors adjusted for 91 mesoregion clusters.

5 How Did the Trade Shocks Affect Crime?

5.1 Potential Mechanisms

An established literature shows that regions exposed to increased foreign competition tend to experience deteriorations in labor market conditions (Autor et al., 2013; Kovak, 2013; Dix-Carneiro and Kovak, 2015b). The link between labor market conditions (employment and earnings) and crime has also been extensively explored (Raphael and Winter-Ebmer, 2001; Gould et al., 2002; Lin, 2008; Fougère et al., 2009). Therefore, labor market conditions constitute a natural channel through which increased foreign competition may have affected crime rates. Nevertheless, local shocks leading to reductions in labor demand can also affect crime in other ways. Negative shocks to local economic activity can reduce government revenues and, consequently, impact the provision of public

goods, which can directly affect crime rates.²² Finally, poor labor market conditions can also affect crime indirectly, through increased inequality or deteriorated mental health due to stress or depression (Fajnzylber et al., 2002; Bourguignon et al., 2003; Card and Dahl, 2011; Fazel et al., 2015). The latter can be important in our setting because we are using homicides to measure crime rates.

In this section, we examine how liberalization affected variables belonging to these three sets of determinants and discuss their relative importance in explaining the reduced-form response of crime rates to the local trade shocks. Specifically, we estimate equations similar to (1), but use variables capturing these various channels as dependent variables, instead of crime rates. All left hand side variables are transformed using the natural logarithm, so estimated responses can be interpreted as elasticities with respect to regional tariff changes.²³

Panel A in Table 5 presents the results for the effect of regional tariff changes on labor market earnings in columns 1 and 2 and on employment rates in columns 3 and 4, for the 1991-2000 and the 1991-2010 periods, respectively.²⁴ The results show that regions facing greater exposure to foreign competition after the liberalization episode (more negative RTC_r) experienced relative reductions in earnings in the medium run (2000), followed by a timid recovery in the long run (2010). The point estimate of the impact on earnings is reduced by 10 percent and loses precision between 2000 and 2010, although the coefficients are not statistically different. In turn, the effect on employment rates is temporary, being large and significant in 2000 but vanishing in 2010. The point estimates indicate that a change in regional tariffs of -0.1 log point would lead to a 0.064 log-point reduction in the employment rate in 2000, with the effect vanishing in 2010. The stronger effect of liberalization on the labor market in 2000 when compared to 2010 mirrors the profile found in the previous section for the response of local crime to regional tariff changes.

Dix-Carneiro and Kovak (2015b) show that the long-run recovery in employment rates experienced by harder-hit regions reflects relative increases in informal employment, while formal employment keeps falling. They also emphasize that the effects of liberalization on local *formal* sector earnings is permanent and gradually magnified over time. However, *overall* local earnings (including formal and informal workers) partially recover in the long run, as we corroborate with the evidence presented here (despite small differences in specifications).²⁵

In Panel B of Table 5, we consider other economic consequences of the local tariff

²²For example, there is ample evidence on the role of police presence, schooling, and welfare payments in preventing crime (Levitt, 1997; Schargrodsky and di Tella, 2004; Jacob and Lefgren, 2003; Lochner and Moretti, 2004; Foley, 2011).

²³Remember that regional tariff changes are measured in terms of log points.

²⁴Changes in our regional employment and earnings variables are net of composition, so that changes in these variables reflect changes in regional labor market conditions for observationally equivalent indi-

Table 5: Investigation of Potential Mechanisms

Panel A: Labor Market Outcomes					
	Earnings		Employment Rate		
	(1)	(2)	(3)	(4)	
	1991-2000	1991-2010	1991-2000	1991-2010	
RTC_r	0.527*** (0.123)	0.460* (0.243)	0.643*** (0.0627)	-0.0510 (0.102)	
R-squared	0.731	0.737	0.528	0.637	
Panel B: Government Revenue and Tax Base					
	Gov. Revenue per Capita		Wage Bill per Capita		# Formal Establishments
	(1)	(2)	(3)	(4)	(5)
	1991-2000	1991-2010	1991-2000	1991-2010	1991-2000
RTC_r	1.500* (0.803)	2.330*** (0.585)	4.695*** (0.482)	8.963*** (0.643)	2.519*** (0.304)
R-squared	0.476	0.543	0.569	0.768	0.718
Panel C: Provision of Public Goods					
	Gov. Spending per Capita		Public Safety Personnel		High School Dropouts
	(1)	(2)	(3)	(4)	(5)
	1991-2000	1991-2010	1991-2000	1991-2010	1991-2000
RTC_r	3.153*** (0.665)	5.184*** (0.617)	0.940*** (0.246)	1.519*** (0.400)	-0.354* (0.200)
R-squared	0.592	0.724	0.390	0.444	0.479
Panel D: Miscellaneous					
	Suicide Rates		Income Inequality (Gini)		
	(1)	(2)	(3)	(4)	
	1991-2000	1991-2010	1991-2000	1991-2010	
RTC_r	1.551 (1.138)	2.148 (2.017)	-0.252*** (0.0740)	-0.753*** (0.166)	
R-squared	0.301	0.482	0.468	0.535	

Notes: All left-hand-side variables are given by the changes of logs over the indicated period. Public Safety Personnel and High School Dropouts are both measured per capita. Income inequality is measured by the Gini coefficient of per capita household income. Standard errors (in parentheses) adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. 411 micro-region observations, except for 3 to 4 missing values in government spending and revenue. Observations are weighted by population. All specifications control for state-period fixed effects. Significant at the *** 1 percent, ** 5 percent, * 10 percent level.

shocks. The table analyzes the impact on government revenues (per capita), number of operating formal establishments (with positive employment), and formal wage bill (per

viduals (for details on this procedure, see Appendix C.1).

²⁵ Although results are consistent across papers, note that there are small differences in specifications between the results shown in Table 5 and the results discussed by [Dix-Carneiro and Kovak \(2015b\)](#) such as how observations are weighted or the exact definition of labor earnings.

capita). In the medium run (columns 1, 3, and 5), we observe effects analogous to those seen in the labor market: regions facing greater exposure to foreign competition experience relative reductions in government revenue, in the number of formal establishments, and in the formal wage bill. However, the long-run effects are very different: while overall labor market effects tend to dissipate, the impacts on these economic activity indicators are permanent and amplified over time. For example, a change in regional tariffs of -0.1 log point would lead to a reduction of 0.15 log point in government revenues in the medium run, and 0.23 in the long run. These results are also consistent with [Dix-Carneiro and Kovak \(2015b\)](#), who document that formal employment and the number of formal establishments gradually declines in adversely affected regions relative to the national average.

These findings are relevant because they speak to the local government’s ability to provide public goods. Panel C in Table 5 investigates this point and shows that the long-run contraction in economic activity in the formal sector was followed by a reduction in the provision of public goods. Government spending (per capita), the number of workers employed in jobs related to public safety (as a fraction of the population), and the share of youth aged 14-18 out of school (high-school dropouts) experience relative deteriorations in regions facing larger tariff shocks. As in Panel B, these effects increase substantially between 2000 and 2010. For example, in response to a change in regional tariffs of -0.1 log point, the number of public safety personnel (per capita) is reduced by 0.094 log-point between 1991 and 2000, and by 0.15 between 1991 and 2010. It is worth noting that rather than thinking of these three variables as independent factors potentially determining crime, we consider them as different manifestations of a single phenomenon taking place during this period: the reduced capacity of the state to provide public goods due to reduced government revenues.

The last set of variables we analyze is related to other indirect channels through which deteriorations in labor market conditions (caused by the trade shocks) may have affected crime. Panel D in Table 5 looks at the responses of inequality (measured by the Gini coefficient for per capita household income) and suicide rates to the local trade shocks. Regarding suicides, results are not statistically significant and point estimates do not indicate deteriorations in mental health as a result of adverse economic shocks (if anything, larger exposure to the shock is associated with a lower suicide rate, although not significantly). However, we find patterns for the response of inequality similar to those documented for the economic outcomes in Panels B and C. Regions facing greater exposure to foreign competition also experience relative increases in inequality, which are enhanced in the long run: a -0.1 change in RTC_r is associated with increases of 0.025 log point in the Gini coefficient in the medium run and 0.075 in the long run.

Taken together, the results from Table 5 suggest that three sets of factors – labor market conditions, public goods provision, and inequality – may have intermediated the effect of trade shocks on crime. Among these, only labor market conditions display dynamic responses similar to those documented for crime rates. In harder-hit regions, employment rates and earnings decline sharply in the medium run, concomitantly with the increase in crime, and then recover – partially in the case of earnings and fully for employment rates – as crime also recedes to the national trend. Public goods provision and inequality, quite differently, experience deteriorations that are magnified over time. Once these dynamics are taken into account, it seems difficult to rationalize the response of crime to the regional tariff shocks without resorting to the labor market as a key intervening mechanism. We formalize this argument in the next section.

5.2 Separating Mechanisms

The previous section showed that the RTC_r shocks are significantly associated with a host of potential mechanisms that could have intermediated the effect of trade liberalization on crime. Here, we propose a framework that attempts to shed light on the role of these mechanisms in explaining the effects we documented in Section 4. We argue that (1) by assuming a stable long-run relationship between these variables and crime, (2) by imposing theoretical sign restrictions on their effects on crime, and (3) by exploiting the distinct dynamic responses of these variables to RTC_r , we can conclude that a substantial part of the effect of RTC_r on crime must have been materialized through labor market conditions, especially employment rates.

5.2.1 Empirical Framework

Informed by the literature on the socio-economic determinants of crime and in light of the evidence from Table 5, we consider three broad categories of mechanisms through which liberalization may have affected crime: labor market conditions (earnings and employment rates), provision of public goods (government spending, public safety personnel, and high-school dropouts), and inequality. From now on, we assume that the RTC_r shock could have affected local crime rates only through these mechanisms. More precisely, we assume that there is a stable long-run relationship between crime and these variables, described by the following equation:

$$\begin{aligned} \Delta \log(CR_r) &= \beta^w \Delta \log(w_r) + \beta^e \Delta \log(P_{e,r}) + \beta^g \Delta \log(GovSp_r) \\ &+ \beta^{ps} \Delta \log(PS_r) + \beta^h \Delta \log(HSDrop_r) + \beta^i \Delta \log(Ineq_r) + \eta_r, \end{aligned} \quad (2)$$

where Δ refers to long changes over time, w refers to labor market earnings, P_e to employment rates, $GovSp$ to government spending, PS to public safety personnel, $HSDrop$ to youth (14-18) out of school, which we call high-school dropouts, and $Ineq$ to per capita household income inequality. We also assume that $Cov(RTC_r, \eta_r) = 0$, that is, RTC_r affects crime only through the variables in the right hand side of equation (2).²⁶

We rely on equation (2) to dissect the mechanisms behind the medium- and long-run effects of RTC_r on crime. First, note that we can decompose the medium- and long-run changes in crime into a projection onto RTC_r and a residual orthogonal to RTC_r .²⁷ To save on notation, let period 1 denote 1991-2000 and period 2 denote 1991-2010. By projecting medium- and long-run changes in crime onto RTC_r , we can always write:

$$\begin{aligned}\Delta_1 \log(CR_r) &= \theta_1 RTC_r + \varepsilon_{r,1} \\ \Delta_2 \log(CR_r) &= \theta_2 RTC_r + \varepsilon_{r,2}\end{aligned}$$

where θ_1 and θ_2 are projection coefficients, and $Cov(RTC_r, \varepsilon_{r,1}) = Cov(RTC_r, \varepsilon_{r,2}) = 0$ by construction.²⁸ In fact, these are the equations that we estimated in Tables 2 and 3, when we effectively projected changes in crime onto RTC_r using Ordinary Least Squares. If the effect of the local trade shocks on crime is intermediated by other variables, such as the ones in the right hand side of equation (2), θ_1 and θ_2 can be seen as reduced-form effects of RTC_r on changes in crime in the medium and long run.

Now consider the variables $X_r \in \{w_r, P_{e,r}, GovSp_r, PS_r, HSDrop_r, Ineq_r\}$ on the right hand side of equation (2). Our Ordinary Least Squares regression coefficients in Table 5 are given by the coefficients b_1^X and b_2^X in the equations below:

$$\begin{aligned}\Delta_1 \log(X_r) &= b_1^X RTC_r + u_{r,1}^X \\ \Delta_2 \log(X_r) &= b_2^X RTC_r + u_{r,2}^X\end{aligned}$$

where $Cov(RTC_r, u_{r,1}^X) = Cov(RTC_r, u_{r,2}^X) = 0$ by construction.

Substituting the relationship for each of the X variables of interest in equation (2)

²⁶We can also think of this relationship as a more parsimonious specification relating crime only to the three broad categories mentioned before: labor market conditions, public good provision, and inequality. From this perspective, the variables listed in equation (2) would be alternative proxies for these channels linking economic shocks to crime.

²⁷In general, for any two variables z and x , we can always express z as a function of x and a residual orthogonal to x : $z = \alpha x + u$, where $\alpha = E(zx)/E(x^2)$ and, by construction, $Cov(x, u) = 0$ (we omit the constant for clarity).

²⁸We omit the constant and other controls such as state fixed effects for clarity of exposition.

and collecting terms, one obtains:

$$\begin{aligned}\Delta_t \log(CR_r) = & \left(\beta^w b_t^w + \beta^e b_t^e + \beta^g b_t^g + \beta^{ps} b_t^{ps} + \beta^h b_t^h + \beta^i b_t^i \right) RTC_r \\ & + \underbrace{\beta^w u_{r,t}^w + \beta^e u_{r,t}^e + \beta^g u_{r,t}^g + \beta^{ps} u_{r,t}^{ps} + \beta^h u_{r,t}^h + \beta^i u_{r,t}^i + \eta_{r,t}}_{\equiv \omega_{r,t}}\end{aligned}$$

for $t = 1, 2$.

Given the assumption that $Cov(RTC_r, \eta_{r,t}) = 0$ and the fact that RTC_r is uncorrelated with the u residuals by construction, it follows that $Cov(RTC_r, \omega_{r,t}) = 0$. By the uniqueness of the projection of $\Delta_t \log(CR_r)$ onto RTC_r , it must also be the case that

$$\begin{aligned}\begin{pmatrix} \theta_1 \\ \theta_2 \end{pmatrix} = & \beta^w \begin{pmatrix} b_1^w \\ b_2^w \end{pmatrix} + \beta^e \begin{pmatrix} b_1^e \\ b_2^e \end{pmatrix} + \beta^g \begin{pmatrix} b_1^g \\ b_2^g \end{pmatrix} + \\ & \beta^{ps} \begin{pmatrix} b_1^{ps} \\ b_2^{ps} \end{pmatrix} + \beta^h \begin{pmatrix} b_1^h \\ b_2^h \end{pmatrix} + \beta^i \begin{pmatrix} b_1^i \\ b_2^i \end{pmatrix}.\end{aligned}\quad (3)$$

In words, if we have a stable and linear relationship between crime and its underlying determinants, the vector θ giving the medium- and long-run reduced-form effects of RTC_r on crime must be given by a linear combination of the vectors describing the reduced-form effects of RTC_r on each of the determinants of crime (where the weights are given by the parameters β^j). Without additional assumptions, this observation is not of much help and simply reflects that we cannot identify the β 's solely based on medium- and long-run responses to the RTC_r shocks. In this case, we can estimate the θ 's and the b 's, but we cannot identify the β 's. However, if we are able to impose theoretical restrictions on the β coefficients from equation (2), expression (3) may be valuable in shedding light on the relevance of some of the factors under consideration. We follow this direction in Section 5.2.2.

Equation (3) highlights the limits to identification in our setting if we do not resort to additional assumptions. However, it also highlights the power of exploiting distinct dynamic effects of a single shock to achieve the identification of multiple coefficients. The general message is that with enough observations over time and distinct dynamic responses of the right hand side variables to the shock, full identification could in principle be achieved. To be specific, suppose we had seven data points instead of just three (1991, 2000 and 2010). In that case, it might have been possible to achieve full identification with this method, provided a full rank condition was met (meaning that the dynamic responses of the right hand side variables in equation (2) were sufficiently heterogeneous). We would have a six-dimensional θ vector in the left hand side and six-dimensional b vectors in the right hand side, that is, six equations with six unknowns.

5.2.2 Theoretical Restrictions and Bounds on the Effect of Labor Market Conditions on Crime

The classical theoretical formulation of the decision to participate in illegal activities developed by [Ehrlich \(1973\)](#) predicts that better opportunities in the legal market, higher probability of apprehension (police presence), and lower inequality reduce participation into crime.²⁹ An increase in the number of high school drop-outs should increase crime due to reduced incapacitation and worsened future labor market opportunities, as formally analyzed by [Lochner \(2011\)](#). Finally, increases in government spending indicate improved provision of public goods and are likely to be associated with greater police presence and better schools, and, consequently, to reductions in crime. All of these relationships are supported by the available empirical evidence on the effects of police ([Levitt, 1997](#); [Scharrotsky and di Tella, 2004](#)), schooling ([Jacob and Lefgren, 2003](#); [Lochner and Moretti, 2004](#)), inequality ([Fajnzylber et al., 2002](#); [Bourguignon et al., 2003](#)), and labor market conditions ([Raphael and Winter-Ebmer, 2001](#); [Gould et al., 2002](#)) on crime.

Therefore, the theoretical and empirical literature suggests that $\beta^w \leq 0$ (growing wages do not lead to increases in crime), $\beta^e \leq 0$ (growing employment does not lead to increases in crime), $\beta^g \leq 0$ (growing government expenditures do not lead to increases in crime), $\beta^{ps} \leq 0$ (expanding police forces do not lead to increases in crime), $\beta^h \geq 0$ (more high school dropouts does not lead to reductions in crime), and $\beta^i \geq 0$ (growing inequality does not lead to reductions in crime). Note that these sign restrictions are in the form of weak inequalities, so that each of these effects are allowed to be zero. Let us assume that these restrictions are valid and, for ease of exposition, define $\tilde{\beta}^j = |\beta^j|$, with $j \in \{w, e, g, ps, h, i\}$, so that we can write:

$$\begin{pmatrix} \theta_1 \\ \theta_2 \end{pmatrix} = \tilde{\beta}^w \begin{pmatrix} -b_1^w \\ -b_2^w \end{pmatrix} + \tilde{\beta}^e \begin{pmatrix} -b_1^e \\ -b_2^e \end{pmatrix} + \tilde{\beta}^g \begin{pmatrix} -b_1^g \\ -b_2^g \end{pmatrix} \\ + \tilde{\beta}^{ps} \begin{pmatrix} -b_1^{ps} \\ -b_2^{ps} \end{pmatrix} + \tilde{\beta}^h \begin{pmatrix} b_1^h \\ b_2^h \end{pmatrix} + \tilde{\beta}^i \begin{pmatrix} b_1^i \\ b_2^i \end{pmatrix}, \quad (4)$$

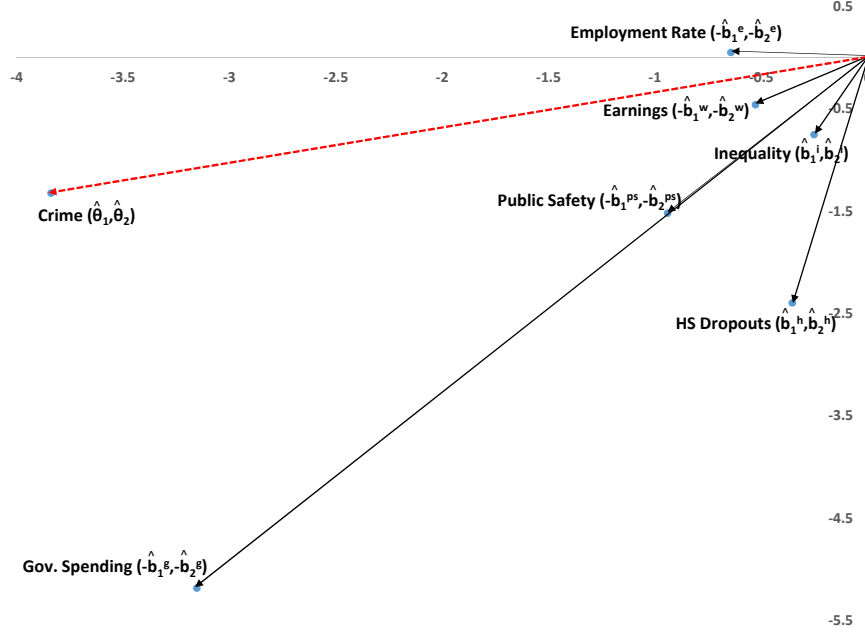
and $\tilde{\beta}^j \geq 0$ for $j \in \{w, e, g, ps, h, i\}$. In words, the vector $\boldsymbol{\theta}$ must be generated by a **positive** linear combination of vectors $\{-b^w, -b^e, -b^g, -b^{ps}, b^h, b^i\}$.

Figure 4 plots our estimated $\hat{\mathbf{b}}^j$ vectors, multiplied by the signs indicated in equation (4). In the figure, the horizontal axis represents the medium-run effect of RTC_r and the vertical axis represents the long-run effect. The figure also plots the estimated reduced-

²⁹In this model, the effect of the labor market on the intensive margin of crime is more ambiguous. Nevertheless, the evidence indicates that there is much more variation in crime at the extensive than at the intensive margin ([Blumstein and Visher, 1986](#)).

form medium- and long-run effects of RTC_r on crime (vector $\hat{\theta}$).

Figure 4: Medium *versus* Long-Run Effects of RTC on Different Channels

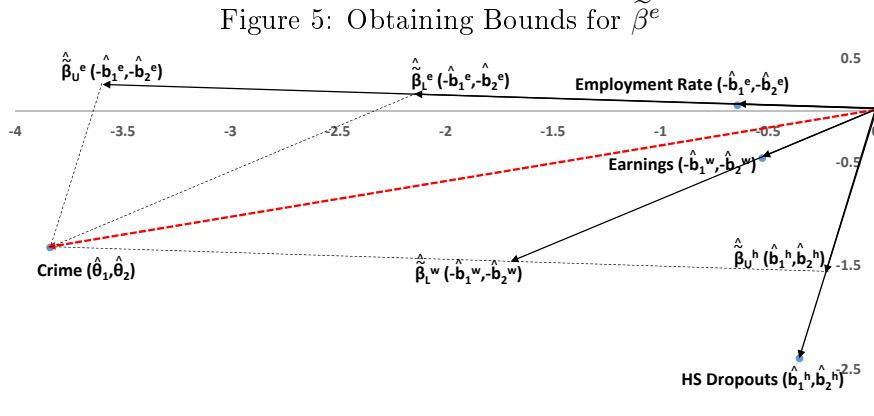


The horizontal axis represents the medium-term effects and the vertical axis represents long-term effects of RTC_r on each outcome estimated in Tables 2, 3 and 5. See text and equation (4) for details.

Two immediate conclusions arise from an inspection of Figure 4. First, note that according to our theoretical restrictions, the documented dynamic responses of crime to liberalization cannot be **solely** explained by the effect of liberalization on earnings, public goods provision and inequality. Mathematically, no **positive** linear combination of vectors $\{-\hat{b}^w, -\hat{b}^g, -\hat{b}^{ps}, \hat{b}^h, \hat{b}^i\}$ can generate $\hat{\theta}$, as $\hat{\theta}$ does not belong to the cone spanned by these vectors. Second, since $\hat{\theta}$ does belong to the cone spanned by $\{-\hat{b}^e, -\hat{b}^w, -\hat{b}^g, -\hat{b}^{ps}, \hat{b}^h, \hat{b}^i\}$, employment rates **must** play a role in explaining the effects of trade shocks on crime. Therefore, according to our framework and theoretical sign restrictions, we must have $\tilde{\beta}^e > 0$ or $\beta^e < 0$. It is also important to note that although our framework and theoretical sign restrictions *predict* that $\theta \in \{-b^e, -b^w, -b^g, -b^{ps}, b^h, b^i\}$ the empirical analysis does not make such an assumption. Consequently, the configuration shown in Figure 4 is consistent with the theoretical sign restrictions we impose.

A closer inspection of Figure 4 reveals that we can impose bounds on $\tilde{\beta}^e$ by expressing $\hat{\theta}$ as a positive linear combination of $-\hat{b}^e$ with the two outermost vectors in the cone spanned by $\{-\hat{b}^w, -\hat{b}^g, -\hat{b}^{ps}, \hat{b}^h, \hat{b}^i\}$, $-\hat{b}^w$ and $-\hat{b}^h$. The lower bound is obtained by expressing $\hat{\theta}$ as a positive linear combination of $-\hat{b}^e$ and \hat{b}^w , while the upper bound is obtained by a positive linear combination of $-\hat{b}^e$ and \hat{b}^h . This procedure is illustrated in

Figure 5, which shows geometrically how we can estimate an upper bound $\widetilde{\beta}_U^e$ and a lower bound $\widetilde{\beta}_L^e$ for $\widetilde{\beta}^e$, based on the configuration of vectors shown in Figure 4.



The horizontal axis represents the medium-term effects and the vertical axis represents long-term effects of RTC_r on each outcome estimated in Tables 2, 3 and 5. See text and equation (4) for details. $\widetilde{\beta}_L^e$ is obtained by expressing $\widehat{\theta}$ as a positive linear combination of $-\widehat{b}^w$ and $-\widehat{b}^e$. $\widetilde{\beta}_U^e$ is obtained expressing $\widehat{\theta}$ as a linear combination of \widehat{b}^h and $-\widehat{b}^e$.

More rigorously, assuming that the configuration of the **population** projection coefficients θ and b is similar to the one obtained for their empirical counterparts (pictured in Figure 4) Appendix D shows that:

$$\underbrace{\frac{-\theta_1 b_2^w + \theta_2 b_1^w}{b_1^e b_2^w - b_1^w b_2^e}}_{\widetilde{\beta}_L^e} < \widetilde{\beta}^e < \underbrace{\frac{-\theta_1 b_2^h + \theta_2 b_1^h}{b_1^e b_2^h - b_1^h b_2^e}}_{\widetilde{\beta}_U^e}. \quad (5)$$

It is easy to show that $\widetilde{\beta}_L^e$ solves:

$$\begin{pmatrix} \theta_1 \\ \theta_2 \end{pmatrix} = \widetilde{\beta}_L^w \begin{pmatrix} -b_1^w \\ -b_2^w \end{pmatrix} + \widetilde{\beta}_L^e \begin{pmatrix} -b_1^e \\ -b_2^e \end{pmatrix},$$

and that $\widetilde{\beta}_U^e$ solves:

$$\begin{pmatrix} \theta_1 \\ \theta_2 \end{pmatrix} = \widetilde{\beta}_U^h \begin{pmatrix} b_1^h \\ b_2^h \end{pmatrix} + \widetilde{\beta}_U^e \begin{pmatrix} -b_1^e \\ -b_2^e \end{pmatrix}.$$

In words, these expressions confirm that we can obtain a lower bound for $\widetilde{\beta}^e$ by finding the linear combination between $-\mathbf{b}^w$ and $-\mathbf{b}^e$ that generates θ . Similarly, we obtain an upper bound for $\widetilde{\beta}^e$ by finding the linear combination between \mathbf{b}^h and $-\mathbf{b}^e$ that generates θ . Since $\beta^e = -\widetilde{\beta}^e$, equation (5) leads to:

$$\underbrace{\frac{\theta_1 b_2^w - \theta_2 b_1^w}{b_1^e b_2^w - b_1^w b_2^e}}_{\beta_U^e} > \beta^e > \underbrace{\frac{\theta_1 b_2^h - \theta_2 b_1^h}{b_1^e b_2^h - b_1^h b_2^e}}_{\beta_L^e}.$$

We estimate these lower and upper bounds for β^e , the effect of employment rates on crime, using the empirical counterparts of their elements:

$$\begin{aligned}\hat{\beta}_U^e &= \frac{\hat{\theta}_1 \hat{b}_2^w - \hat{\theta}_2 \hat{b}_1^w}{\hat{b}_1^e \hat{b}_2^w - \hat{b}_1^w \hat{b}_2^e} \\ \hat{\beta}_L^e &= \frac{\hat{\theta}_1 \hat{b}_2^h - \hat{\theta}_2 \hat{b}_1^h}{\hat{b}_1^e \hat{b}_2^h - \hat{b}_1^h \hat{b}_2^e}.\end{aligned}$$

It is convenient to note that $\hat{\beta}_U^e$ solves:

$$\begin{pmatrix} \hat{\beta}_U^w \\ \hat{\beta}_U^e \end{pmatrix} = \begin{pmatrix} \hat{b}_1^w & \hat{b}_1^e \\ \hat{b}_2^w & \hat{b}_2^e \end{pmatrix}^{-1} \begin{pmatrix} \hat{\theta}_1 \\ \hat{\theta}_2 \end{pmatrix}, \quad (6)$$

and that $\hat{\beta}_L^e$ solves:

$$\begin{pmatrix} \hat{\beta}_L^h \\ \hat{\beta}_L^e \end{pmatrix} = \begin{pmatrix} \hat{b}_1^h & \hat{b}_1^e \\ \hat{b}_2^h & \hat{b}_2^e \end{pmatrix}^{-1} \begin{pmatrix} \hat{\theta}_1 \\ \hat{\theta}_2 \end{pmatrix}. \quad (7)$$

Appendix E shows that equation (6) is **algebraically equivalent** to a Two-Stage Least Squares (2SLS) estimator relating changes in employment rates and earnings to changes in crime rates. This 2SLS estimator is obtained stacking medium- and long-run changes, and instruments are given by $RTC \times Period_{91-00}$ and $RTC \times Period_{91-10}$.³⁰ Similarly, equation (7) is algebraically equivalent to an analogous 2SLS estimator relating changes in employment rates and the share of high-school dropouts to changes in crime rates.

The interpretation of the bounds estimators as 2SLS estimators is informative. Suppose we estimate a regression relating crime rates to employment rates and earnings by 2SLS, using $RTC \times Period_{91-00}$ and $RTC \times Period_{91-10}$ as instruments and ignoring the rest of the potential channels in equation (2). In that case, according to the sign restrictions we imposed in Section 5.2.1, we would obtain an **upward** biased estimate for β^e , as this 2SLS estimator converges to $\beta_U^e > \beta^e$. On the other hand, suppose we estimate a regression relating crime rates to employment rates and share of high-school dropouts by 2SLS, using $RTC \times Period_{91-00}$ and $RTC \times Period_{91-10}$ as instruments and ignoring the rest of the potential channels in equation (2). According to our sign restrictions, we

³⁰ $Period_{t-t'}$ is a dummy variable indicating if an observation relates to period $t - t'$.

would obtain a **downward** biased estimate for β^e , as this 2SLS estimator converges to $\beta_L^e < \beta^e$.

Table 6 shows our estimates for the bounds on the effect of employment rates on crime. According to our baseline specification – obtained using the vectors depicted in Figure 4 – we obtain bounds between -5.6 (lower bound) and -3.3 (upper bound 1). Although the upper bound estimate is economically significant (we interpret magnitudes at the end of this section), its standard error is very large so that we cannot reject that it is zero. Once we take sampling error into account, the reduced-form estimates $\hat{\mathbf{b}}^w$ and $\hat{\mathbf{b}}^e$ are close to collinear, so that the matrix with columns $\hat{\mathbf{b}}^w$ and $\hat{\mathbf{b}}^e$ in equation (6) is close to singular. This leads to large standard errors for both $\hat{\beta}_U^w$ and $\hat{\beta}_U^e$. Essentially, this means that employment rates and earnings responded very similarly to the trade shocks, so that there is little room to distinguish whether the liberalization-induced labor market effects on crime played out through earnings or employment rates.

Table 6: Bounds on The Effect of Employment Rates on Crime

Panel A: Baseline Specification		
Upper Bound 1	Upper Bound 2	Lower Bound
-3.307 (3.205)	-4.473*** (1.386)	-5.595*** (1.925)
Panel B: Adding Demographic Controls		
Upper Bound 1	Upper Bound 2	Lower Bound
-4.298** (2.013)	-4.309** (1.870)	-4.818*** (1.627)

Notes: As noted in the text, upper and lower bounds are algebraically equivalent to 2SLS estimators. Standard errors are outcomes of 2SLS regressions relating crime rates to employment and earnings, public safety or high-school dropouts. All specifications stack 1991-2000 and 1991-2010 changes and control for state-period fixed effects. Standard errors are clustered at the meso-region level.

Additional notes: Upper bound 1 combines employment with earnings; Upper bound 2 combines employment with public safety; Lower bound combines employment with high-school dropouts. See text for details.

Significant at the *** 1 percent, ** 5 percent, * 10 percent level.

In our discussion of Figure 4, we argued that employment rates must have non-zero weight in explaining the dynamic response of crime to the trade shocks. Therefore, since we cannot separate the effect of employment rates from the effect of earnings, we measure labor market conditions (more broadly) solely with employment rates. We do so with the understanding that the employment effects we measure are likely to capture both employment rate effects as well as earnings effects. If we omit $\Delta \log(w_r)$ from the right hand side of equation (2), it is easy to see in Figure 4 that we can obtain a lower bound

for $\tilde{\beta}^e$ by expressing θ as a positive linear combination of $-\mathbf{b}^{ps}$ and $-\mathbf{b}^e$. Since $\tilde{\beta}^e = -\beta^e$, a lower bound for $\tilde{\beta}^e$ leads to an upper bound for β^e . Details are found in Appendix D. In that case, the upper bound estimator for β^e is given by (upper bound 2):

$$\hat{\beta}_U^e = \frac{\hat{\theta}_1 \hat{b}_2^{ps} - \hat{\theta}_2 \hat{b}_1^{ps}}{\hat{b}_1^e \hat{b}_2^{ps} - \hat{b}_1^{ps} \hat{b}_2^e},$$

so that $\hat{\beta}_U^e$ solves:

$$\begin{pmatrix} \hat{\beta}_U^{ps} \\ \hat{\beta}_U^e \end{pmatrix} = \begin{pmatrix} \hat{b}_1^{ps} & \hat{b}_1^e \\ \hat{b}_2^{ps} & \hat{b}_2^e \end{pmatrix}^{-1} \begin{pmatrix} \hat{\theta}_1 \\ \hat{\theta}_2 \end{pmatrix}. \quad (8)$$

With the understanding that if we exclude earnings from the right hand side of equation (2) employment rates measure labor market conditions as a whole, we obtain bounds on the effect of labor market conditions between -5.6 and -4.5 (see Table 6).

Our baseline specifications in Tables 2, 3 and 5 only use state-period fixed effects as controls. We conduct this same exercise by adding controls such as changes in the share of young and unskilled males in the population (male individuals who are between 18 and 30 years old, and with less than eight years of education) and changes in the urbanization rate (share of population living in urban settings). These demographic controls intend to capture compositional changes in the population that can affect crime (see, for example, Glaeser et al., 1996 and Levitt, 1999). The $\hat{\theta}$ and $\hat{\mathbf{b}}$ estimates arising from these new exercises are shown in Table F.1 in the Appendix. Figure F.1 shows that the configuration of vectors that arises from this exercise is similar to the one in Figure 4, so that our method still applies.

Panel B of Table 6 shows the resulting bounds. We obtain bounds between -4.8 and -4.3. Interestingly, in this case, we are able to separate the effect of employment rates from the effect of earnings, as the resulting vectors $\hat{\mathbf{b}}^e$ and $\hat{\mathbf{b}}^w$ grow further apart (see Figure F.1). In addition, the upper bound on β^e is very similar if we combine employment rates with either earnings (upper bound 1) or public safety personnel (upper bound 2) to compute it. Nevertheless, we do not want to over emphasize this finding, and keep measuring labor market conditions more broadly using employment rates, and omitting earnings from equation (2).

We now use the estimates of our benchmark specification in Panel A of Table 6 to interpret the magnitude of the estimated effect of labor market conditions (measured by employment rates) on crime. For example, if $\log(P_e)$ is reduced by 0.07 log point (the standard deviation of $\Delta_{91-00} \log(P_e)$ across regions), the crime rate is expected to increase between $-4.5 \times -0.07 = 0.32$ and $-5.6 \times -0.07 = 0.39$ log point (37 and 48 percent). Alternatively, consider a region facing a RTC_r shock of -0.1 log point, which is the 90-10 gap in the distribution of RTC_r . According to Table 5, this would lead to a

reduction in the employment rate of 0.064 log-point in the medium run, relative to the national average. In turn, Table 6 indicates that this would be associated with a relative increase between $-4.5 \times -0.064 = 0.29$ and $-5.6 \times -0.064 = 0.36$ log point (33 to 43 percent) in crime rates five years following the end of liberalization. Consequently, labor market conditions account for 75 to 93 percent of the medium-run effect of the trade-induced economic shocks on crime and constitute the main mechanism through which the tariff-induced shocks affected crime.³¹

6 Relationship with the Literature on Labor Market Conditions and Crime

As we mentioned throughout the paper, there is a large literature measuring the effect of local unemployment rates on crime. This literature typically estimates this effect by exploiting local labor demand shifters measured with Bartik shocks as instruments for labor market conditions. However, this literature has abstracted from other potential mechanisms through which local labor demand shocks may affect crime – for example, through changes in government spending, police forces or inequality. It is therefore natural to ask: if we had assumed employment rates to be the **sole** mechanism through which trade shocks affected crime rates and applied a 2SLS estimator using the *RTC* shocks as instruments, mimicking the path this literature has followed, how would this estimate compare with the bounds we obtained in Table 6?

We perform this exercise adding one innovation. Given that the *RTC_r* shocks had distinct dynamic effects on many variables of interest, we can construct two instrumental variables and confront employment rates against each of the remaining channels in equation (2), one by one. In other words, we can estimate regressions such as:

$$\Delta \log(CR_r) = \beta^e \Delta \log(P_{e,r}) + \beta^X \Delta \log(X_r) + \eta_r^X \quad (9)$$

where $X \in \{w, GovSp, PS, HSDrop, Ineq\}$. For improved efficiency, we stack 1991-2000 and 2000-2010 changes instead of 1991-2000 and 1991-2010 changes, otherwise the η_r^X error terms would be automatically correlated across time as the latter periods overlap. Since we cluster standard errors at the meso-region level, our standard errors are robust to the correlation of errors across neighboring regions and over time. We employ 2SLS and

³¹The total reduced-form effect of a *RTC_r* shock of -0.1 log point is to increase crime rates by $-3.85 \times -0.1 = 0.385$ log point. Labor market conditions account for a fraction between $\frac{\beta_L^e \times b_1^e \times -0.1}{\theta_1 \times -0.1} = \frac{-4.5 \times 0.64 \times -0.1}{-3.85 \times -0.1} = 0.75$ and $\frac{\beta_L^e \times b_1^e \times -0.1}{\theta_1 \times -0.1} = \frac{-5.6 \times 0.64 \times -0.1}{-3.85 \times -0.1} = 0.93$ of this effect. Remember that $\theta_1 = \beta^e b_1^e + \beta^g b_1^g + \beta^{ps} b_1^{ps} + \beta^h b_1^h + \beta^i b_1^i$ (see equation (3)) and that we are measuring labor market conditions with employment rates only.

$RTC_r \times I(\text{period} = 1991 - 2000)$ and $RTC_r \times I(\text{period} = 2000 - 2010)$ as instruments. All specifications control for state-period fixed effects. Results are shown in Tables 7 and 8.

Table 7: Employment Rates Against Alternative Mechanisms

Dep. Var.: $\Delta \log(CR_r)$	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta \log(P_{e,r})$	-4.501*** (1.348)	-2.995 (3.371)	-4.428*** (1.319)	-4.329*** (1.374)	-5.562*** (1.928)	-5.063*** (1.523)
$\Delta \log(w_r)$		-3.6242 (5.764)				
$\Delta \log(\text{Gov. Spending}_r)$			-0.3165 (0.421)			
$\Delta \log(\text{Public Safety}_r)$				-1.120 (1.393)		
$\Delta \log(\text{HS Dropout}_r)$					0.730 (0.887)	
$\Delta \log(\text{Inequality}_r)$						2.304 (3.253)
Observations	822	822	816	822	822	822
K-P rk LM statistic	21.72	4.441	17.54	7.570	15.19	16.44
p-value	0.000	0.035	0.000	0.006	0.000	0.000
K-P rk Wald F statistic	53.75	1.945	29.82	6.957	32.54	13.16
A-R Wald test F-stat	8.403	8.403	8.427	8.403	8.403	8.403
A-R Wald test p-value	0.000	0.000	0.000	0.000	0.000	0.000

Notes: Decennial Census data. Standard errors (in parentheses) adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. Observations are weighted by population. All specifications stack 1991-2000 and 2000-2010 changes, control for state-period fixed effects and use $RTC_r \times I(\text{period}=91-00)$ and $RTC_r \times I(\text{period}=00-10)$ as instruments for the alternative mechanisms. Estimation method is Two-Stage Least Squares. There are 6 missing values for government spending in column (3).

Significant at the *** 1 percent, ** 5 percent, * 10 percent level.

Column 1 in Table 7 shows the 2SLS estimate of the effect of employment rates on crime if we use $RTC_r \times I(\text{period} = 1991 - 2000)$ and $RTC_r \times I(\text{period} = 2000 - 2010)$ as instruments. This specification is similar to what the previous literature on the topic has adopted, except for the choice of specific instruments. In this case, we obtain an estimate of -4.5. Therefore, in the context of our study, we obtain an estimate that is similar to the upper bound for the effect of labor market conditions on crime. However, it goes without saying that this provides no information on the size of the bias in other studies.

Columns 2 to 6 in Table 7 sequentially confront employment rates against competing mechanisms. Although this constitutes a step beyond what the literature on labor markets and crime has typically considered, these regressions must still be interpreted with caution. When we confront employment rates with public safety personnel, for example, we do not impose sign restrictions on β^e and β^{ps} as we did in Section 5.2.2, but we cannot strictly

Table 8: Employment Rates Against Alternative Mechanisms: Adding Controls

Dep. Var.: $\Delta \log(CR_r)$	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta \log(P_{e,r})$	-4.482*** (1.266)	-4.896** (2.226)	-4.591*** (1.445)	-4.668*** (1.620)	-4.202*** (1.544)	-4.360*** (1.259)
$\Delta \log(w_r)$		0.9391 (3.786)				
$\Delta \log(\text{Gov. Spending}_r)$			0.1229 (0.523)			
$\Delta \log(\text{Public Safety}_r)$				0.5274 (2.328)		
$\Delta \log(\text{HS Dropout}_r)$					-0.223 (0.971)	
$\Delta \log(\text{Inequality}_r)$						-0.600 (2.488)
$\Delta \log(\text{Share YUM}_r)$	-0.274 (0.415)	-0.236 (0.421)	-0.375 (0.642)	-0.455 (0.954)	-0.288 (0.435)	-0.238 (0.408)
$\Delta \log(\text{Share Urban}_r)$	-1.119*** (0.397)	-1.182*** (0.309)	-1.219*** (0.350)	-1.227*** (0.392)	-1.188*** (0.310)	-1.182*** (0.303)
Observations	822	822	816	822	822	822
K-P rk LM statistic	22.43	5.872	15.03	3.607	12.52	16.29
p-value	0.000	0.015	0.000	0.057	0.000	0.000
K-P rk Wald F statistic	65.89	3.185	22.82	2.785	36.71	18.89
A-R Wald test F-stat	8.473	8.473	8.512	8.473	8.473	8.473
A-R Wald test p-value	0.000	0.000	0.000	0.000	0.000	0.000

Notes: Decennial Census data. Standard errors (in parentheses) adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. Observations are weighted by population. All specifications stack 1991-2000 and 2000-2010 changes, control for state-period fixed effects and use $RTC_r \times I(\text{period}=91-00)$ and $RTC_r \times I(\text{period}=00-10)$ as instruments for the alternative mechanisms. Estimation method is Two-Stage Least Squares. There are 6 missing values for government spending in column (3). YUM stands for Young, Unskilled and Male.

Significant at the *** 1 percent, ** 5 percent, * 10 percent level.

rule out that a combination of the remaining variables in equation (2) is an important determinant of crime, therefore biasing our estimates. Nonetheless, the stability of the β^e estimates in the sequential estimation of (9) for each competing mechanism gives us more confidence that, indeed, labor market conditions constituted an important mechanism through which the trade shocks affected crime. The only instance where the estimate of β^e is non-significant is when we confront employment rates with earnings. In that case, as we discussed, we cannot separate the effect of employment rates from the effect of earnings, as they are affected by the RTC_r shocks in very similar ways over the medium and long runs. Table 8 reproduces the same exercises in Table 7, but also controls for changes in demographic variables. We obtain very similar estimates.

7 Discussion

This paper exploits the local economic shocks induced by the Brazilian trade liberalization episode to provide credible estimates of the effect of economic conditions on criminal activity. We take advantage of two key features of Brazil’s liberalization to make progress in understanding the mechanisms behind this effect: (i) the discreteness and persistence of the shock; and (ii) its heterogeneous dynamic effects on the potential mechanisms behind the response of crime rates. We provide a framework that exploits these elements to argue that it is difficult to rationalize the observed response of crime to the trade shocks without attributing a key role to labor market variables, in particular to the employment rate.

By linking trade-induced shocks to crime, this paper contributes to a growing literature on the effects of trade beyond the labor market and documents a new dimension of adjustment costs that may follow trade shocks. Analyses of these adjustment costs have typically focused on frictions impeding or slowing the reallocation of resources needed to generate production gains from trade (Artuç et al., 2010; Coşar, 2013; Dix-Carneiro, 2014) or on workers whose labor market trajectories are adversely affected by trade (Menezes-Filho and Muendler, 2011; Autor et al., 2014; Dix-Carneiro and Kovak, 2015b; Utar, 2015). Since crime generates substantial externalities, our results add a relevant dimension to these adjustment costs by showing that the consequences of trade shocks go beyond the individuals directly affected by them.

It is worthwhile to stress one important aspect of our approach. Most of the literature on labor markets and crime resorts to some sort of regional economic shocks – such as Bartik shocks – as a source of exogenous variation. The evidence from Section 5 indicates that local economic shocks affecting the labor market are likely to be correlated with other dimensions that may also be relevant determinants of crime rates (such as public goods provision and inequality). This suggests that the instruments used in the previous literature do not satisfy the exclusion restriction required by an IV estimator. This is precisely why we explore the distinct dynamic responses of the various potential mechanisms in order to be able to provide bounds for the causal effect of labor market conditions on crime. In the context of our study, the traditional IV estimates of the effect of labor market conditions on crime is very similar to what our methodology delivered as an upper bound (lower bound for the **magnitude** of the effect).

We documented that regions facing greater exposure to foreign competition experienced gradual increases in crime relative to the national average over the years immediately following the trade liberalization, but that these increases in crime eventually receded. Our analysis presents evidence that the recovery of the labor market in these harder-hit

regions played a key role in reducing crime in the long run. Interestingly, [Dix-Carneiro and Kovak \(2015b\)](#) show that the long-run recovery in employment rates was entirely due to an expansion of the informal sector. In this context, informal employment seems to have been enough to keep individuals away from crime. Enforcement of labor regulations that tend to reduce informality but increase unemployment could therefore exacerbate the response of crime to economic downturns. Analogously, the evidence we provide also highlights the importance of counter-cyclical policies, by improving labor market prospects, as instruments to fight crime. The costs of economic downturns – or of low employability in general – go beyond those faced by the individuals who directly suffer from worsened labor market opportunities. In such circumstances, there is a potential welfare enhancing role for government interventions that are successful in improving labor market outcomes.

Finally, we focus on a developing country with high levels of violence and document an economically large response of homicide rates to local labor market conditions. There are a few possible explanations for the large response of homicide rates that we estimate, which contrast to largely zero effects on violent crime found in the previous literature (which focused exclusively in developed countries with low crime rates). Our natural experiment and empirical framework combined lead to a more transparent identification of the effect of labor market conditions on crime than the empirical strategies that have been used so far. In addition, we explore the context of a developing country with high incidence of crime and poor labor market conditions, in sharp contrast to the developed country context that has been the focus of previous research. The first of these factors probably allows us to more precisely estimate the response of crime to labor market outcomes, while the second provides a setting where the response of crime is likely to be stronger. The evidence suggests that the criminogenic effect of deteriorations in labor market conditions is indeed more extreme and policy relevant in developing countries with poor labor market conditions and high levels of violence.

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Web Appendix – Not for Publication

A Homicide Rates as a Proxy for Overall Criminal Activity

This section investigates to what extent local homicide rates constitute a good proxy for overall criminal activity. We examine data from Minas Gerais and São Paulo, the two most populous states in Brazil, which account for 32 percent of Brazil’s total population. These constitute the very few Brazilian states publishing disaggregate crime data from police-compiled statistics since the early 2000s at the municipality level. We have data for four types of crime: homicides recorded by the health system (our dependent variable), homicides recorded by the police, violent crimes against the person (excluding homicides), and violent property crimes. Violent property crimes refer to robberies in both states. Violent crimes against the person refer to rape in São Paulo and to rape, assaults, and attempted homicides in Minas Gerais. The data are provided by the statistical agencies of the two states (Fundação SEADE for São Paulo and Fundação João Pinheiro for Minas Gerais).

We start by examining how the rates of different types of crime recorded by the police correlate with the homicide rates used in our empirical analysis for a 5-year interval. As Table A.1 shows, our measure of homicides is highly correlated, both in levels and in changes, to police-recorded homicides, to property crimes, and to crimes against the person.

Table A.2 shows the results in log-levels for both São Paulo and Minas Gerais using yearly data and 10-year intervals. Table A.3 shows correlations for log-changes for both states and the same time intervals. Homicide rates measured by the police and the health system are highly correlated, with a strongly significant correlation that ranges from 0.84 to 0.92. Both measures of homicides are also strongly and significantly correlated with crimes against the person and property crimes, but particularly so with the latter. It is worth noting that the correlations in Panel B of Table A.3 should be interpreted with caution given the small number of observations used to generate them.

Tables A.4 and A.5 relate our measure of homicide rates (from the health system) to the rates of crimes against the person, property crimes and homicides measured by the police. These regressions control for micro-region and year fixed effects, so we focus on how changes in our measure of criminal activity, relative to aggregate crime trends, relate to changes in other measures of crime within regions. The first three columns show results in line with those from Tables A.1, A.2 and A.3. Even after we account for micro-region fixed effects and common trends in crime, homicide rates measured by the health system are strongly correlated with homicides recorded by the police, crimes against the person, and property crimes. Moreover, these correlations are stronger when we restrict attention to longer time windows. Columns 4 and 5 progressively include the different measures of crime rates on the right hand side.

In sum, Table A.1 and the results presented in this section indicate that local homicide rates measured by the health system (DATASUS) are indeed systematically correlated with local overall crime rates recorded by the police.

Table A.1: Correlation Between Homicide Rates And Other Crime Measures: Micro-Regions of São Paulo and Minas Gerais, 5-year intervals (2001, 2006 and 2011)

Log-Levels				
	$\log(CR_r)$	$\log(HomPol_r)$	$\log(Person_r)$	$\log(Property_r)$
São Paulo				
$\log(CR_r)$	1			
$\log(HomPol_r)$	0.849***	1		
$\log(Person_r)$	0.204***	0.223***	1	
$\log(Property_r)$	0.611***	0.490***	0.286***	1
Observations			186	
Minas Gerais				
$\log(CR_r)$	1			
$\log(HomPol_r)$	0.889***	1		
$\log(Person_r)$	0.580***	0.711***	1	
$\log(Property_r)$	0.716***	0.644***	0.633***	1
Observations			192	
Log-Changes				
	$\Delta_5 \log(CR_r)$	$\Delta_5 \log(HomPol_r)$	$\Delta_5 \log(Person_r)$	$\Delta_5 \log(Property_r)$
São Paulo				
$\Delta_5 \log(CR_r)$	1			
$\Delta_5 \log(HomPol_r)$	0.700***	1		
$\Delta_5 \log(Person_r)$	0.513***	0.483***	1	
$\Delta_5 \log(Property_r)$	0.348***	0.415***	0.455***	1
Observations			124	
Minas Gerais				
$\Delta_5 \log(CR_r)$	1			
$\Delta_5 \log(HomPol_r)$	0.675***	1		
$\Delta_5 \log(Person_r)$	0.435***	0.359***	1	
$\Delta_5 \log(Property_r)$	0.393***	0.294***	0.783***	1
Observations			128	

Notes: Data are provided by the statistical agencies of the two states (Fundação SEADE for São Paulo and Fundação João Pinheiro for Minas Gerais). Observations are weighted by region-specific population. CR_r is the homicide rate measured by the health system (DATASUS), $HomPol_r$ is the homicide rate measured by the police, $Person_r$ is the rate of crimes against the person, and $Property_r$ is the rate of property crimes. Notation: $\Delta_s y = y_{t+s} - y_t$.

Significant at the *** 1 percent, ** 5 percent, and * 10 percent level.

Table A.2: Correlation Between Homicide Rates And Other Crime Measures: Micro-Regions of São Paulo and Minas Gerais, 2000–2010

Panel A: Yearly data				
	$\log(CR_r)$	$\log(HomPol_r)$	$\log(Person_r)$	$\log(Property_r)$
São Paulo				
$\log(CR_r)$	1			
$\log(HomPol_r)$	0.884***	1		
$\log(Person_r)$	0.371***	0.376***	1	
$\log(Property_r)$	0.633***	0.542***	0.329***	1
Observations			682	
Minas Gerais				
$\log(CR_r)$	1			
$\log(HomPol_r)$	0.916***	1		
$\log(Person_r)$	0.658***	0.740***	1	
$\log(Property_r)$	0.733***	0.652***	0.613***	1
Observations			704	
Panel B: 10-year intervals (2001 and 2011)				
São Paulo				
$\log(CR_r)$	1			
$\log(HomPol_r)$	0.844***	1		
$\log(Person_r)$	0.0793	0.0138	1	
$\log(Property_r)$	0.614***	0.460***	0.299***	1
Observations			124	
Minas Gerais				
$\log(CR_r)$	1			
$\log(HomPol_r)$	0.859***	1		
$\log(Person_r)$	0.518***	0.687***	1	
$\log(Property_r)$	0.723***	0.645***	0.623***	1
Observations			128	

Notes: Data are provided by the statistical agencies of the two states (Fundação SEADE for São Paulo and Fundação João Pinheiro for Minas Gerais). Observations are weighted by region-specific population. CR_r is the homicide rate measured by the health system (DATASUS), $HomPol_r$ is the homicide rate measured by the police, $Person_r$ is the rate of crimes against the person, and $Property_r$ is the rate of property crimes.

Significant at the *** 1 percent, ** 5 percent, and * 10 percent level.

Table A.3: Correlation Between Log-Changes in Homicide Rates and Other Crime Measures: Micro-Regions of São Paulo and Minas Gerais, 2000–2010

Panel A: Yearly data				
	$\Delta_1 \log(CR_r)$	$\Delta_1 \log(HomPol_r)$	$\Delta_1 \log(Person_r)$	$\Delta_1 \log(Property_r)$
São Paulo				
$\Delta_1 \log(CR_r)$	1			
$\Delta_1 \log(HomPol_r)$	0.586***	1		
$\Delta_1 \log(Person_r)$	0.257***	0.338***	1	
$\Delta_1 \log(Property_r)$	0.147***	0.153***	0.139***	1
Observations			620	
Minas Gerais				
$\Delta_1 \log(CR_r)$	1			
$\Delta_1 \log(HomPol_r)$	0.621***	1		
$\Delta_1 \log(Person_r)$	0.163***	0.130***	1	
$\Delta_1 \log(Property_r)$	0.229***	0.188***	0.400***	1
Observations			640	
Panel B: 10-year intervals (2001 and 2011)				
	$\Delta_{10} \log(CR_r)$	$\Delta_{10} \log(HomPol_r)$	$\Delta_{10} \log(Person_r)$	$\Delta_{10} \log(Property_r)$
São Paulo				
$\Delta_{10} \log(CR_r)$	1			
$\Delta_{10} \log(HomPol_r)$	0.755***	1		
$\Delta_{10} \log(Person_r)$	0.569***	0.0595	1	
$\Delta_{10} \log(Property_r)$	0.478***	0.382***	0.290**	1
Observations			62	
Minas Gerais				
$\Delta_{10} \log(CR_r)$	1			
$\Delta_{10} \log(HomPol_r)$	0.478***	1		
$\Delta_{10} \log(Person_r)$	0.259**	0.196	1	
$\Delta_{10} \log(Property_r)$	0.308**	0.115	0.154	1
Observations			64	

Notes: Data are provided by the statistical agencies of the two states (Fundação SEADE for São Paulo and Fundação João Pinheiro for Minas Gerais). Observations are weighted by region-specific population. CR_r is the homicide rate measured by the health system (DATASUS); $HomPol_r$ is the homicide rate measured by the police; $Person_r$ is the rate of crimes against the person; and $Property_r$ is the rate of property crimes. Notation: $\Delta_s y = y_{t+s} - y_t$.

Significant at the *** 1 percent, ** 5 percent, and * 10 percent level.

Table A.4: Conditional Correlations between Homicide Rates and Other Crime Rates: Micro-Regions of São Paulo, 2000–2010

Panel A: Yearly Data					
Dep. Var.: $\log(CR_r)$	(1)	(2)	(3)	(4)	(5)
$\log(Person_r)$	0.313*** (0.0444)			0.285*** (0.0498)	0.279*** (0.0532)
$\log(Property_r)$		0.613*** (0.149)		0.565*** (0.147)	0.178*** (0.0620)
$\log(HomPol_r)$			0.482*** (0.0444)		0.448*** (0.0341)
Observations	682	682	682	682	682
R^2 Within	0.743	0.746	0.845	0.772	0.875
R^2 Between	0.474	0.681	0.830	0.758	0.902

Panel B: 5-year intervals (2000, 2005 and 2010)					
Dep. Var.: $\log(CR_r)$	(1)	(2)	(3)	(4)	(5)
$\log(Person_r)$	0.451*** (0.0712)			0.400*** (0.0799)	0.391*** (0.0543)
$\log(Property_r)$		0.638*** (0.192)		0.467*** (0.170)	0.0877 (0.0995)
$\log(HomPol_r)$			0.456*** (0.0561)		0.426*** (0.0403)
Observations	186	186	186	186	186
R^2 Within	0.762	0.728	0.845	0.779	0.898
R^2 Between	0.458	0.657	0.799	0.736	0.855

Panel C: 10-year intervals (2000 and 2010)					
Dep. Var.: $\log(CR_r)$	(1)	(2)	(3)	(4)	(5)
$\log(Person_r)$	0.552*** (0.116)			0.455*** (0.133)	0.490*** (0.0491)
$\log(Property_r)$		1.023*** (0.288)		0.732*** (0.267)	0.131 (0.115)
$\log(HomPol_r)$			0.466*** (0.0615)		0.431*** (0.0357)
Observations	124	124	124	124	124
R^2 Within	0.820	0.795	0.887	0.849	0.960
R^2 Between	0.316	0.684	0.721	0.710	0.782

Notes: Data from Fundação SEADE. 62 micro-regions in the State of São Paulo. Robust standard errors in parentheses (clustered at the micro-region level). All regressions control for micro-regions and year fixed effects. CR_r is the homicide rate measured by the health system (DATASUS), $HomPol_r$ is the homicide rate measured by the police, $Person_r$ is the rate of violent crimes against the person, and $Property_r$ is the rate of property crimes. Violent property crimes refer to robberies, violent crimes against the person refer to rape.

Significant at *** 1 percent, ** 5 percent, and * 10 percent.

Table A.5: Conditional Correlations between Homicide Rates and Other Crime Rates: Micro-Regions of Minas Gerais, 2000–2010

Panel A: Yearly Data					
Dep. Var.: $\log(CR_r)$	(1)	(2)	(3)	(4)	(5)
$\log(Person_r)$	0.280*** (0.0710)			0.158** (0.0652)	0.0588 (0.0479)
$\log(Property_r)$		0.305*** (0.0983)		0.292*** (0.0891)	0.214*** (0.0628)
$\log(HomPol_r)$			0.751*** (0.0527)		0.706*** (0.0450)
Observations	703	704	704	703	703
R^2 Within	0.286	0.306	0.537	0.325	0.566
R^2 Between	0.625	0.200	0.792	0.402	0.857

Panel B: 5-year intervals (2000, 2005 and 2010)					
Dep. Var.: $\log(CR_r)$	(1)	(2)	(3)	(4)	(5)
$\log(Person_r)$	0.320** (0.133)			0.260* (0.138)	0.157 (0.117)
$\log(Property_r)$		0.252** (0.103)		0.179* (0.101)	0.205*** (0.0736)
$\log(HomPol_r)$			0.713*** (0.0863)		0.693*** (0.0765)
Observations	192	192	192	192	192
R^2 Within	0.544	0.537	0.667	0.553	0.692
R^2 Between	0.486	0.194	0.656	0.498	0.726

Panel C: 10-year intervals (2000 and 2010)					
Dep. Var.: $\log(CR_r)$	(1)	(2)	(3)	(4)	(5)
$\log(Person_r)$	0.335* (0.191)			0.278 (0.176)	0.178 (0.162)
$\log(Property_r)$		0.392** (0.184)		0.348* (0.174)	0.304** (0.139)
$\log(HomPol_r)$			0.638*** (0.156)		0.567*** (0.152)
Observations	128	128	128	128	128
R^2 Within	0.634	0.646	0.696	0.663	0.729
R^2 Between	0.428	0.247	0.535	0.446	0.673

Notes: Data from Fundação João Pinheiro. 64 micro-regions in the State of Minas Gerais. Robust standard errors in parentheses (clustered at the micro-region level). All regressions control for micro-regions and year fixed effects. CR_r is the homicide rate measured by the health system (DATASUS), $HomPol_r$ is the homicide rate measured by the police, $Person_r$ is the rate of violent crimes against the person, and $Property_r$ is the rate of property crimes. Property crimes refer to robberies, crimes against the person refer to rape, assaults, and attempted homicides.

Significant at *** 1 percent, ** 5 percent, and * 10 percent.

B Tariff Changes after 1995

This paper treats the 1990-1995 changes in tariffs induced by the trade liberalization as a once-and-for-all shock. Indeed, changes in tariffs after 1995 are trivial relative to the changes that occurred between 1990 and 1995. This section provides evidence supporting this claim.

The data on tariffs used in the paper are from Kume et al. (2003). These data have been extensively used by previous papers in the literature on trade and labor markets in Brazil.³² However, these data only cover the period 1987-1998. In order to show how post-liberalization tariff changes relate to changes induced by the trade reform, we use data from UNCTAD TRAINS, which cover the entire period from 1990 to 2010. Equipped with these data, we compute regional tariff changes using sectoral tariff changes between 1990 and 1995 ($RTC_{r,90-95}$), 1990 and 2000 ($RTC_{r,90-00}$) and 1990 and 2010 ($RTC_{r,90-10}$). Table B.1 shows that regional tariff changes over longer horizons, $RTC_{r,90-00}$ and $RTC_{r,90-10}$, are almost perfectly correlated with $RTC_{r,90-95}$ (elasticities are all larger than 0.8 and R-squared's are all larger than 0.92). This implies that changes in tariffs between 1990 and 1995 can indeed be considered as permanent without substantially affecting any of our qualitative or quantitative results.

Table B.1: Regional Tariff Changes 1990-1995 vs. Regional Tariff Changes 1990-2000 and 1990-2010

Dep. Var.:	$RTC_{r,90-00}$ (1)	$RTC_{r,90-00}$ (2)	$RTC_{r,90-10}$ (3)	$RTC_{r,90-10}$ (4)
$RTC_{r,90-95}$	0.970*** (0.00359)	0.985*** (0.00311)	0.844*** (0.0113)	0.802*** (0.0114)
Observations Weighted By Population	No	Yes	No	Yes
Observations	411	411	411	411
R-squared	0.994	0.996	0.931	0.923

Notes: Regional Tariff Changes (RTC_r) over different horizons computed from UNCTAD TRAINS data. $RTC_{r,90-95}$ uses changes in sectoral tariffs between 1990 and 1995; $RTC_{r,90-00}$ uses changes in sectoral tariffs between 1990 and 2000; and $RTC_{r,90-10}$ uses changes in sectoral tariffs between 1990 and 2010. UNCTAD TRAINS tariffs at the product level were aggregated into 44 industries compatible with the 1991 Brazilian Demographic Census. Aggregation was performed using simple averages. These industry-level tariffs were then used in the calculation of RTC_r . Standard errors in parentheses.

Significant at the *** 1 percent, ** 5 percent, and * 10 percent level.

³²See Menezes-Filho and Muendler (2011), Kovak (2013), Dix-Carneiro and Kovak (2015a), Dix-Carneiro and Kovak (2015b) and Hirata and Soares (2015).

C Data Procedures

C.1 Regional Employment and Earnings Net of Compositional Effects

Changes in our regional employment and earnings variables are net of composition, so that changes in these variables reflect changes in regional labor market conditions for observationally equivalent individuals and do not reflect changes in composition. This section describes how we use individual-level Census data to compute region-specific log earnings and employment rates netting out compositional effects.

We obtain region- and year-specific log-earnings by estimating the Mincer regression below and saving the $\hat{\omega}_{rs}$ estimates:

$$\begin{aligned} \log(w_{irs}) &= \omega_{rs} + \sum_k \eta_{ks}^w I(Educ_i = k) + \gamma_s^w I(Female_i = 1) + \\ &\quad \delta_{1s}^w (age_{is} - 18) + \delta_{2s}^w (age_{is} - 18)^2 + \varepsilon_{irs}^w, \end{aligned} \quad (10)$$

where w_{irs} represents *total* monthly labor market earnings for worker i in region r in year s , $I(Educ_i = k)$ is a dummy variable corresponding to years of schooling k , $I(Female_i = 1)$ is a dummy for gender, age_{is} indicates age, and ω_{rs} captures the average of the log of monthly earnings net of composition in region r and time period s . Finally, ε_{irs}^w is an error term. We use $\hat{\omega}_{rs}$ as our measure of log-earnings in region r in year s .

Region- and year-specific employment rates are obtained in a similar fashion, by estimating the linear probability model below and saving the $\hat{\pi}_{rs}$ estimates:

$$\begin{aligned} Emp_{irs} &= \pi_{rs} + \sum_k \eta_{ks}^e I(Educ_i = k) + \gamma_s^e I(Female_i = 1) + \\ &\quad \delta_{1s}^e (age_{is} - 18) + \delta_{2s}^e (age_{is} - 18)^2 + \varepsilon_{irs}^e, \end{aligned} \quad (11)$$

where Emp_{irs} indicates if individual i in region r was employed in year s , π_{rs} captures the average probability of employment net of composition in region r and time period s , and ε_{irs}^e is an error term. We use $\hat{\pi}_{rs}$ as our measure of the employment rate in region r in year s .

C.2 Employment Rates

The question in the Census questionnaire regarding work status changed between 1991 and 2000, remaining the same in 2010. In 1991 the question was "Have you worked in all or part of the past 12 months?", while in 2000 and 2010 the question related to the surveys' reference week. There is no widely used procedure to make these questions comparable, so we adopt the following strategy to construct a comparable variable across Censuses' waves.

In 1991 we define $Emp_{irt} = 1$ if the individual answers yes to "Have you worked in all or part of the previous 12 months?" and zero otherwise. For 2000 and 2010, we define $Emp_{irt} = 1$ if: (a) the individual worked for pay in the reference week; or (b) the individual had a job during the reference week, but for some reason did not work that week; or (c) the individual helped (without pay) a household member in her job or was an intern or apprentice; or (d) the individual helped (without pay) a household member engaged in agricultural activities; or (e) the individual worked in agricultural activities to

supply food to household members; and $Emp_{irt} = 0$ otherwise. The answer "yes" to the 1991 question embeds all of the cases above.

D Derivation of Bounds for β^e

Throughout this section, we will use the notation $Cone(\mathbf{v}_1, \mathbf{v}_2, \dots, \mathbf{v}_n)$ to denote the cone spanned by vectors $\mathbf{v}_1, \mathbf{v}_2, \dots, \mathbf{v}_n$, which consists of all **positive** linear combinations of these vectors. In section 5.2.1, we obtained equation (4), which we reproduce below:

$$\begin{pmatrix} \theta_1 \\ \theta_2 \end{pmatrix} = \tilde{\beta}^w \begin{pmatrix} -b_1^w \\ -b_2^w \end{pmatrix} + \tilde{\beta}^e \begin{pmatrix} -b_1^e \\ -b_2^e \end{pmatrix} + \tilde{\beta}^g \begin{pmatrix} -b_1^g \\ -b_2^g \end{pmatrix} \\ + \tilde{\beta}^{ps} \begin{pmatrix} -b_1^{ps} \\ -b_2^{ps} \end{pmatrix} + \tilde{\beta}^h \begin{pmatrix} b_1^h \\ b_2^h \end{pmatrix} + \tilde{\beta}^i \begin{pmatrix} b_1^i \\ b_2^i \end{pmatrix},$$

with $\tilde{\beta} \geq 0$, which means that $\boldsymbol{\theta}$ belongs to the cone spanned by vectors $-\mathbf{b}^w, -\mathbf{b}^e, -\mathbf{b}^g, -\mathbf{b}^{ps}, \mathbf{b}^h$ and \mathbf{b}^i – which we denote $Cone(-\mathbf{b}^w, -\mathbf{b}^e, -\mathbf{b}^g, -\mathbf{b}^{ps}, \mathbf{b}^h, \mathbf{b}^i)$. This is a theoretical relationship on the true population parameters, but note that empirically:

$$\hat{\boldsymbol{\theta}} \in Cone(-\hat{\mathbf{b}}^w, -\hat{\mathbf{b}}^e, -\hat{\mathbf{b}}^g, -\hat{\mathbf{b}}^{ps}, \hat{\mathbf{b}}^h, \hat{\mathbf{b}}^i) = Cone(-\hat{\mathbf{b}}^e, \hat{\mathbf{b}}^h),$$

where the last equality follows from

$$\{-\hat{\mathbf{b}}^w, -\hat{\mathbf{b}}^g, -\hat{\mathbf{b}}^{ps}, \hat{\mathbf{b}}^i\} \in Cone(-\hat{\mathbf{b}}^e, \hat{\mathbf{b}}^h).$$

However, $\hat{\boldsymbol{\theta}} \notin Cone(-\hat{\mathbf{b}}^w, \hat{\mathbf{b}}^h)$ and $Cone(-\hat{\mathbf{b}}^w, \hat{\mathbf{b}}^h)$ is the largest cone spanned by

$$\{-\hat{\mathbf{b}}^w, -\hat{\mathbf{b}}^g, -\hat{\mathbf{b}}^{ps}, \hat{\mathbf{b}}^h, \hat{\mathbf{b}}^i\}.$$

Also note from Figure 4 that any element $\mathbf{y} \in Cone(-\hat{\mathbf{b}}^w, \hat{\mathbf{b}}^h)$ has $\mathbf{y} < \mathbf{0}$. These relationships are based on **estimates**.

Based on these empirical results, we make the assumptions below, regarding **population** projection coefficients. These just reflect that we assume that the configuration of population vectors is similar to the configuration of estimated vectors.

Assumption 1 $\boldsymbol{\theta} \in Cone(-\mathbf{b}^w, -\mathbf{b}^e, -\mathbf{b}^g, -\mathbf{b}^{ps}, \mathbf{b}^h, \mathbf{b}^i)$

Assumption 2 $Cone(-\mathbf{b}^w, -\mathbf{b}^e, -\mathbf{b}^g, -\mathbf{b}^{ps}, \mathbf{b}^h, \mathbf{b}^i) = Cone(-\mathbf{b}^e, \mathbf{b}^h)$, that is, $Cone(-\mathbf{b}^e, \mathbf{b}^h)$ is the largest cone spanned by $\{-\mathbf{b}^w, -\mathbf{b}^e, -\mathbf{b}^g, -\mathbf{b}^{ps}, \mathbf{b}^h, \mathbf{b}^i\}$

Assumption 3 $\boldsymbol{\theta} \notin Cone(-\mathbf{b}^w, \mathbf{b}^h)$ and $Cone(-\mathbf{b}^w, \mathbf{b}^h)$ is the largest cone spanned by $\{-\mathbf{b}^w, -\mathbf{b}^g, -\mathbf{b}^{ps}, \mathbf{b}^h, \mathbf{b}^i\}$

Assumption 4 $-\mathbf{b}^w, \mathbf{b}^h < \mathbf{0}$

Assumption 1 guarantees that a solution to equation (4) with $\tilde{\beta} \geq 0$ exists. Together with Assumptions 2 and 3, it also guarantees that $\tilde{\beta}^e > 0$. Given Assumptions 1, 2 and 3, Assumption 4 is not strictly necessary for us to be able to find bounds for $\tilde{\beta}^e$, but it is satisfied by the empirical counterparts and facilitates our derivation.

Define \mathbf{y} as:

$$\begin{pmatrix} y_1 \\ y_2 \end{pmatrix} = \tilde{\beta}^w \begin{pmatrix} -b_1^w \\ -b_2^w \end{pmatrix} + \tilde{\beta}^g \begin{pmatrix} -b_1^g \\ -b_2^g \end{pmatrix} \\ + \tilde{\beta}^{ps} \begin{pmatrix} -b_1^{ps} \\ -b_2^{ps} \end{pmatrix} + \tilde{\beta}^h \begin{pmatrix} b_1^h \\ b_2^h \end{pmatrix} + \tilde{\beta}^i \begin{pmatrix} b_1^i \\ b_2^i \end{pmatrix},$$

So that

$$\begin{pmatrix} \theta_1 \\ \theta_2 \end{pmatrix} = \tilde{\beta}^e \begin{pmatrix} -b_1^e \\ -b_2^e \end{pmatrix} + \begin{pmatrix} y_1 \\ y_2 \end{pmatrix}$$

and $\mathbf{y} \in \text{Cone}(-\mathbf{b}^w, \mathbf{b}^h)$. Rewriting:

$$\begin{pmatrix} \theta_1 - y_1 \\ \theta_2 - y_2 \end{pmatrix} = \tilde{\beta}^e \begin{pmatrix} -b_1^e \\ -b_2^e \end{pmatrix} \\ \Rightarrow \begin{cases} -\tilde{\beta}^e b_1^e = \theta_1 - y_1 \\ -\tilde{\beta}^e b_2^e = \theta_2 - y_2 \end{cases} \\ \Rightarrow \tilde{\beta}^e = \frac{\theta_2 - y_2}{-b_2^e} = \frac{\theta_1 - y_1}{-b_1^e} \\ \Rightarrow y_2 = -\frac{b_2^e}{b_1^e}(\theta_1 - y_1) + \theta_2$$

It is easy to see graphically on Figure 4 that, $\mathbf{y} \in \text{Cone}(-\mathbf{b}^w, \mathbf{b}^h)$ and $-\mathbf{b}^w, \mathbf{b}^h < 0$ leads to $\frac{b_2^w}{b_1^w} < \frac{y_2}{y_1} < \frac{b_2^h}{b_1^h}$ and $y_2, y_1 < 0$. Using $\frac{b_2^w}{b_1^w} < \frac{y_2}{y_1} < \frac{b_2^h}{b_1^h}$ and $y_1 < 0$ we get:

$$\begin{aligned} \frac{b_2^w}{b_1^w} y_1 &> -\frac{b_2^e}{b_1^e}(\theta_1 - y_1) + \theta_2 > \frac{b_2^h}{b_1^h} y_1 \\ \Rightarrow \left(\frac{b_2^w}{b_1^w} - \frac{b_2^e}{b_1^e} \right) y_1 &> -\frac{b_2^e}{b_1^e} \theta_1 + \theta_2 > \left(\frac{b_2^h}{b_1^h} - \frac{b_2^e}{b_1^e} \right) y_1 \end{aligned}$$

Assume that $\left(\frac{b_2^w}{b_1^w} - \frac{b_2^e}{b_1^e} \right) > 0$ and $\left(\frac{b_2^h}{b_1^h} - \frac{b_2^e}{b_1^e} \right) > 0$ – this is met by the empirical counterparts. We obtain:

$$\frac{-\frac{b_2^e}{b_1^e} \theta_1 + \theta_2}{\left(\frac{b_2^h}{b_1^h} - \frac{b_2^e}{b_1^e} \right)} > y_1 > \frac{-\frac{b_2^e}{b_1^e} \theta_1 + \theta_2}{\left(\frac{b_2^w}{b_1^w} - \frac{b_2^e}{b_1^e} \right)}$$

Remember that $\beta^e = \frac{\theta_1 - y_1}{b_1^e}$ and assume that $b_1^e > 0$ – this is also met by the empirical counterparts. We obtain:

$$\frac{\theta_1 b_2^h - \theta_2 b_1^h}{b_1^e b_2^h - b_1^h b_2^e} < \beta^e < \frac{\theta_1 b_2^w - \theta_2 b_1^w}{b_1^e b_2^w - b_1^w b_2^e}$$

These bounds can be estimated with:

$$\hat{\beta}_L^e = \frac{\hat{\theta}_1 \hat{b}_2^h - \hat{\theta}_2 \hat{b}_1^h}{\hat{b}_1^e \hat{b}_2^h - \hat{b}_1^h \hat{b}_2^e}$$

$$\hat{\beta}_U^e = \frac{\hat{\theta}_1 \hat{b}_2^w - \hat{\theta}_2 \hat{b}_1^w}{\hat{b}_1^e \hat{b}_2^w - \hat{b}_1^w \hat{b}_2^e}$$

If earnings ($\Delta \log(w_r)$) are excluded from equation (2), then we can obtain an alternative upper bound for β^e following the same steps as above. First, note that

$$\hat{\theta} \in \text{Cone}(-\hat{\mathbf{b}}^e, \hat{\mathbf{b}}^h)$$

but

$$\hat{\theta} \notin \text{Cone}(-\hat{\mathbf{b}}^{ps}, \hat{\mathbf{b}}^h)$$

and that $\text{Cone}(-\hat{\mathbf{b}}^{ps}, \hat{\mathbf{b}}^h)$ is the largest cone spanned by $\{-\hat{\mathbf{b}}^g, -\hat{\mathbf{b}}^{ps}, \hat{\mathbf{b}}^h, \hat{\mathbf{b}}^i\}$. This leads us to make assumptions similar to 1-3, which essentially imply that the configuration of population vectors $-\mathbf{b}^e, -\mathbf{b}^g, -\mathbf{b}^{ps}, \mathbf{b}^h, \mathbf{b}^i$ is similar to the configuration of their empirical counterparts.

Assumption 5 $\theta \in \text{Cone}(-\mathbf{b}^e, -\mathbf{b}^g, -\mathbf{b}^{ps}, \mathbf{b}^h, \mathbf{b}^i) = \text{Cone}(-\mathbf{b}^e, \mathbf{b}^h)$

Assumption 6 $\text{Cone}(-\mathbf{b}^e, -\mathbf{b}^g, -\mathbf{b}^{ps}, \mathbf{b}^h, \mathbf{b}^i) = \text{Cone}(-\mathbf{b}^e, \mathbf{b}^h)$, that is, $\text{Cone}(-\mathbf{b}^e, \mathbf{b}^h)$ is the largest cone spanned by $\{-\mathbf{b}^e, -\mathbf{b}^g, -\mathbf{b}^{ps}, \mathbf{b}^h, \mathbf{b}^i\}$

Assumption 7 $\theta \notin \text{Cone}(-\mathbf{b}^{ps}, \mathbf{b}^h)$ and $\text{Cone}(-\mathbf{b}^{ps}, \mathbf{b}^h)$ is the largest cone spanned by $\{-\mathbf{b}^g, -\mathbf{b}^{ps}, \mathbf{b}^h, \mathbf{b}^i\}$

Assumption 8 $-\mathbf{b}^{ps}, \mathbf{b}^h < 0$

With Assumptions 5 to 8 replacing Assumptions 1 to 4, we follow the same procedure above to obtain the following upper bound for β^e :

$$\frac{\theta_1 b_2^{ps} - \theta_2 b_1^{ps}}{b_1^e b_2^{ps} - b_1^{ps} b_2^e}$$

Which can be estimated with:

$$\hat{\beta}_U^e = \frac{\hat{\theta}_1 \hat{b}_2^{ps} - \hat{\theta}_2 \hat{b}_1^{ps}}{\hat{b}_1^e \hat{b}_2^{ps} - \hat{b}_1^{ps} \hat{b}_2^e}$$

So that $\hat{\beta}_U^e$ solves:

$$\begin{pmatrix} \hat{\beta}_U^{ps} \\ \hat{\beta}_U^e \end{pmatrix} = \begin{pmatrix} \hat{b}_1^{ps} & \hat{b}_1^e \\ \hat{b}_2^{ps} & \hat{b}_2^e \end{pmatrix}^{-1} \begin{pmatrix} \hat{\theta}_1 \\ \hat{\theta}_2 \end{pmatrix}$$

E Bounds $\hat{\beta}_U^e$ and $\hat{\beta}_L^e$ as 2SLS Estimators

This section shows that equation (6) defines an estimator that is algebraically equivalent to a 2SLS estimator where (1) the estimating equation stacks medium- and long-run changes; and (2) instruments are given by $RTC \times Period_{91-00}$ and $RTC \times Period_{91-10}$. Without loss of generality, we ignore exogenous covariates to simplify the exposition.

Suppose we want to estimate the model below, where we stack medium-run changes (Δ_1) and long-run changes (Δ_2) and employ 2SLS with $RTC \times Period_1$ and $RTC \times Period_2$ as instruments. $Period_1$ indicates if observations relate to medium-run changes (1991-2000) and $Period_2$ indicates if observations relate to long-run changes (1991-2010).

$$\begin{pmatrix} \Delta_1 \log(CR) \\ \Delta_2 \log(CR) \end{pmatrix} = \beta^e \begin{pmatrix} \Delta_1 \log(P_e) \\ \Delta_2 \log(P_e) \end{pmatrix} + \beta^w \begin{pmatrix} \Delta_1 \log(w) \\ \Delta_2 \log(w) \end{pmatrix} + \varepsilon \quad (12)$$

First stage equations are:

$$\begin{pmatrix} \Delta_1 \log(w) \\ \Delta_2 \log(w) \end{pmatrix} = (RTC \times Period_1 \quad RTC \times Period_2) \begin{pmatrix} b_1^w \\ b_2^w \end{pmatrix} + u^w$$

$$\begin{pmatrix} \Delta_1 \log(P_e) \\ \Delta_2 \log(P_e) \end{pmatrix} = (RTC \times Period_1 \quad RTC \times Period_2) \begin{pmatrix} b_1^e \\ b_2^e \end{pmatrix} + u^e,$$

where b_1^X is the medium-run effect of RTC on variable X , and b_2^X is the long-run effect. In matrix notation:

$$\begin{pmatrix} \Delta_1 \log(w) & \Delta_1 \log(P_e) \\ \Delta_2 \log(w) & \Delta_2 \log(P_e) \end{pmatrix} = (RTC \times Period_1 \quad RTC \times Period_2) \begin{pmatrix} b_1^w & b_1^e \\ b_2^w & b_2^e \end{pmatrix} + (u^w \quad u^e)$$

First stage predictions are given by:

$$\begin{pmatrix} \widehat{\Delta_1 \log(w)} & \widehat{\Delta_1 \log(P_e)} \\ \widehat{\Delta_2 \log(w)} & \widehat{\Delta_2 \log(P_e)} \end{pmatrix} = \underbrace{(RTC \times Period_1 \quad RTC \times Period_2)}_Z \underbrace{\begin{pmatrix} \widehat{b}_1^w & \widehat{b}_1^e \\ \widehat{b}_2^w & \widehat{b}_2^e \end{pmatrix}}_{\widehat{\mathbf{b}}} = Z\widehat{\mathbf{b}}$$

By definition, the 2SLS estimator of β^e and β^w in equation (12) are given by the projection coefficients of $\begin{pmatrix} \Delta_1 \log(CR) \\ \Delta_2 \log(CR) \end{pmatrix}$ onto $\begin{pmatrix} \widehat{\Delta_1 \log(w)} & \widehat{\Delta_1 \log(P_e)} \\ \widehat{\Delta_2 \log(w)} & \widehat{\Delta_2 \log(P_e)} \end{pmatrix} = Z\widehat{\mathbf{b}}$.

$$\begin{pmatrix} \widehat{\beta}^w \\ \widehat{\beta}^e \end{pmatrix}^{2SLS} = (\widehat{\mathbf{b}}' Z' Z \widehat{\mathbf{b}})^{-1} \widehat{\mathbf{b}}' Z' \begin{pmatrix} \Delta_1 \log(CR) \\ \Delta_2 \log(CR) \end{pmatrix}$$

The reduced-form estimates – projection coefficients of $(\Delta_1 \log(CR) \quad \Delta_2 \log(CR))'$ onto the instruments Z – is given by:

$$\begin{pmatrix} \widehat{\theta}_1 \\ \widehat{\theta}_2 \end{pmatrix} = (Z'Z)^{-1} Z' \begin{pmatrix} \Delta_1 \log(CR) \\ \Delta_2 \log(CR) \end{pmatrix}$$

$$(Z'Z) \begin{pmatrix} \hat{\theta}_1 \\ \hat{\theta}_2 \end{pmatrix} = Z' \begin{pmatrix} \Delta_1 \log(CR) \\ \Delta_2 \log(CR) \end{pmatrix}$$

Rewriting:

$$\begin{aligned} \begin{pmatrix} \hat{\beta}^w \\ \hat{\beta}^e \end{pmatrix}^{2SLS} &= (\hat{\mathbf{b}}' Z' Z \hat{\mathbf{b}})^{-1} \hat{\mathbf{b}}' (Z' Z) \begin{pmatrix} \hat{\theta}_1 \\ \hat{\theta}_2 \end{pmatrix} \\ &= \hat{\mathbf{b}}^{-1} \begin{pmatrix} \hat{\theta}_1 \\ \hat{\theta}_2 \end{pmatrix} \\ &= \begin{pmatrix} \hat{b}_1^w & \hat{b}_1^e \\ \hat{b}_2^w & \hat{b}_2^e \end{pmatrix}^{-1} \begin{pmatrix} \hat{\theta}_1 \\ \hat{\theta}_2 \end{pmatrix} \end{aligned}$$

The right hand side of the above equation is equal to the right hand side of equation (6).

F Vector Configuration With Demographic Controls

This appendix checks if the configuration of vectors $\{-\hat{\mathbf{b}}^e, -\hat{\mathbf{b}}^w, -\hat{\mathbf{b}}^g, -\hat{\mathbf{b}}^{ps}, \hat{\mathbf{b}}^h, \hat{\mathbf{b}}^i\}$ is similar to the one pictured in Figure 4 once we add demographic controls such as changes in urbanization rates and changes in the fraction of the population who is young (18 to 30 years old), unskilled (eighth grade completed or less) and male. Table F.1 displays the regression results, and Figure F.1 shows the configuration of these vectors, confirming that the configuration of estimated vectors – controlling for demographic changes – is similar to those in Figure 4.

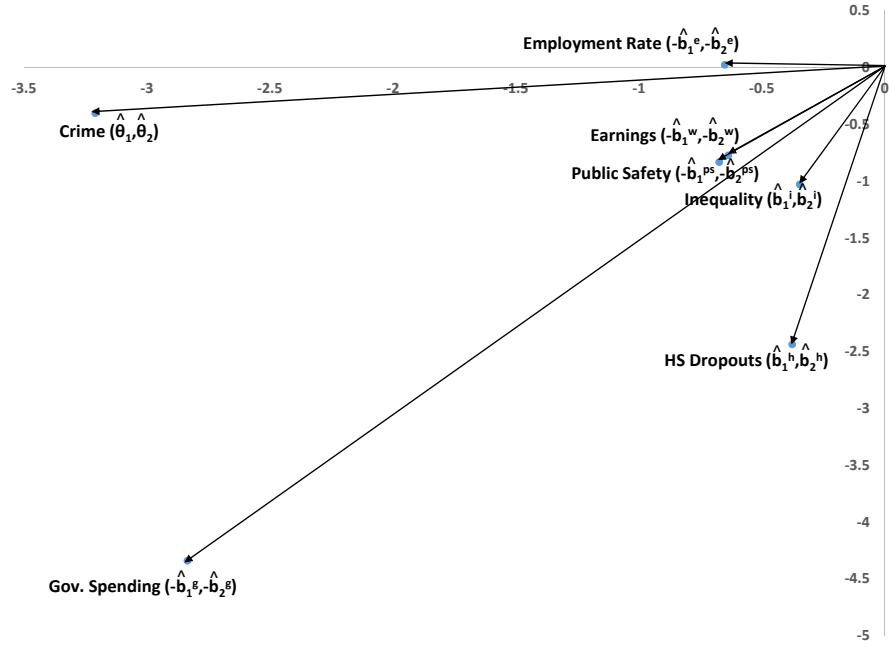
Table F.1: Medium- and Long-Run Effects of RTC – Controlling for Demographic Changes

Dep. Var.:	$\Delta \log(CR_r)$ (1)	$\Delta \log(P_{e,r})$ (2)	$\Delta \log(w_r)$ (3)	$\Delta \log(PS_r)$ (4)	$\Delta \log(GovSp_r)$ (5)	$\Delta \log(HSDrop_r)$ (6)	$\Delta \log(Ineq_r)$ (7)
$RTC_r \times Period_{91-00}$	-3.210** (1.243)	0.650*** (0.0682)	0.636*** (0.121)	0.673* (0.351)	2.835*** (0.676)	-0.376* (0.193)	-0.346*** (0.0640)
$RTC_r \times Period_{91-10}$	-0.402 (2.422)	-0.0245 (0.120)	0.772*** (0.220)	0.831 (0.544)	4.340*** (0.776)	-2.436*** (0.280)	-1.028*** (0.143)
$\Delta \log(\text{Share YUM}_r)$	0.274 (0.439)	-0.0304 (0.0367)	-0.241*** (0.0554)	0.447*** (0.118)	0.581** (0.252)	0.00573 (0.0546)	0.218*** (0.0388)
$\Delta \log(\text{Share Urban}_r)$	-0.841*** (0.311)	0.00940 (0.0357)	0.00852 (0.0466)	0.0562 (0.213)	0.0400 (0.131)	0.0204 (0.0966)	-0.0127 (0.0289)
Observations	822	822	822	822	815	822	822
R-squared	0.584	0.821	0.929	0.462	0.681	0.676	0.664

Notes: Decennial Census data. Standard errors (in parentheses) adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. Observations are weighted by population. All specifications stack 1991-2000 and 1991-2010 changes and control for state-period fixed effects. There are 6 missing values for government spending in column (3).

Significant at the *** 1 percent, ** 5 percent, * 10 percent level.

Figure F.1: Medium *versus* Long-Run Effects of RTC on Different Channels – Controlling for Demographic Changes



The horizontal axis represents the medium-term effects and the vertical axis represents long-term effects of RTC_r on each outcome estimated in Tables 2, 3 and 5. See text and equation (4) for details.