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

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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* D31, F13, F16, H41, K42, O17, O19)

In the wake of the Great Recession, there were renewed concerns that the severe economic crisis could fuel a resurgence in crime (see Colvin 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, Johnson, and Kantor 2010; UNODC 2012). The literature on economic cycles, labor market conditions, and crime has recurrently investigated these issues, but identification remains a major challenge (e.g., Cook and Zarkin 1985, Raphael and Winter-Ebmer 2001, and Finklea 2011). Despite its relevance in the public debate and important welfare implications, there are still open questions regarding the effect of economic shocks on criminal activity and even more on the mechanisms through which these effects may play out.

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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 2017b). 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 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 also have in common 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 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 for the following eight years. Before 1995 and after 2003, there is no statistically significant

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

[2] Section II and online Appendix C 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 nonrandom, data on homicides provide less biased measures of the changes in crime and violence (Soares 2004).

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 points (corresponding to a movement from the ninetieth to the tenth percentile of regional tariff changes) experienced a relative *increase* in its crime rate of 0.38 log points (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: labor market conditions such as employment rates and earnings (Raphael and Winter-Ebmer 2001; Gould, Weinberg, and Mustard 2002; Lin 2008; Fougère, Kramarz, and Pouget 2009); public goods provision (Levitt 2002, Schargrodsky and Di Tella 2004, Jacob and Lefgren 2003, Lochner and Moretti 2004, Foley 2011); and mental health (stress or depression) and inequality (Fajnzylber, Lederman, and Loayza 2002; Bourguignon, Nuñez, and Sanchez 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 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 the 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

[3] Consistent with previous findings of Dix-Carneiro and Kovak (2017a), the long-run recovery in employment reflects increases in informal employment, while formal employment never recovers.

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 ten-year decline in its employment rate of one standard deviation (0.07 log points), the crime rate would be expected to increase between 0.32 and 0.39 log points (37 and 48 percent). This is a large economic effect: it represents an increase equivalent to half a standard deviation of the 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 (2017a) 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.[4] 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 four 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, there is very little evidence on the effect of economic conditions on crime in developing countries with high incidence of crime. In contrast with the existing literature on the effect of economic shocks on crime, discussed later in this paper, we find substantial effects on homicide rates. Third, we contribute to a recent but growing literature stressing adjustment costs to trade shocks beyond those associated with the labor market.[5] The fact that crime has an important externality dimension adds particular interest

[4] Dix-Carneiro and Kovak (2017a) consider a worker as informal if she is informally employed by a firm (off the books and invisible to the government) or if she is self-employed. In either case, the worker does not receive the benefits or regulatory protections present in the formal labor market.

[5] For example, recent studies have estimated the effects of trade shocks on crime (Iyer and Topalova 2014, Che and Xu 2015, 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, Dorn, and Hanson 2015), and political outcomes (Dippel et al. 2017, Autor et al. 2016, Che et al. 2016).

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. Of particular interest to our paper, Che and Xu (2016) and Deiana (2016) exploit labor demand shocks across American Commuting Zones induced by import competition from China to show that regions more exposed to China trade experience relative increases in property crime. Iyer and Topalova (2014) exploit local labor demand shocks induced by the Indian trade liberalization to establish a similar result. However, none of these papers attempt to identify the channels through which the trade-induced local shocks affect crime and none detect significant effects on violent crime (they study countries with relatively low crime levels—India and the United States—when compared to Brazil or most of Latin America and the Caribbean).[6] Finally, the paper contributes to the literature on the effects of labor market conditions on crime (Raphael and Winter-Ebmer 2001; Gould, Weinberg, and Mustard 2002; Lin 2008; Fougere, Kramarz, and Pouget 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.[7] 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 II provides a background of the 1990s trade liberalization in Brazil and of its documented effect on local labor markets. Section III describes the data we use and provides descriptive statistics. Section IV presents our empirical strategy and the results related to the effect of the trade-induced regional shocks on crime. Section V sheds light on the mechanisms behind the relationship between the trade shocks and crime. Section VI relates our paper to the literature on labor market conditions and crime. Finally, Section VII closes the paper with a broader discussion and interpretation of the results.

I. Trade Liberalization and Local Economic Shocks in Brazil

A. *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.

[6] Burke, Hsiang, and Miguel (2015) also investigate the relationship between shocks to human populations and violence, but they focus on the effect of weather shocks on conflicts. Conflict and common crime are somewhat different phenomena and our paper pays particular attention to the role of labor market conditions in driving crime, so our focus is different from theirs.

[7] 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.

The trade reform broke with nearly 100 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 nontransparent 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, Piani, and Souza 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, Piani, and Souza 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 1992).[8] 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.[9] 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.[10] Figure 1 shows the approximate percentage 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).[11] 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, Piani, and Souza (2003) and have been extensively used in the literature on trade and labor markets in Brazil.[12]

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

[8] Online Appendix A shows the time series of tariffs. Note the tariff increases in 1990 for the auto and electronic equipment industries.

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

[10] We 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 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, Piani, and Souza (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, Piani, and Souza 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.

[11] The 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, $\Delta \log(P_j) = \Delta \log(1+\tau_j)$.

[12] We refer readers interested in further details behind the Brazilian trade liberalization to (Dix-Carneiro and Kovak 2017b).

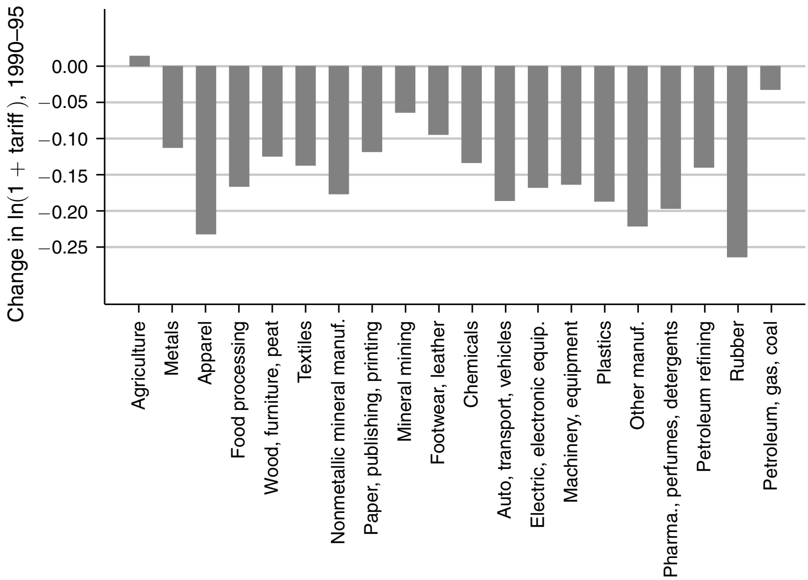

Figure 1. Changes in log(1 + tariff), 1990–1995

Source: Dix-Carneiro and Kovak (2017b)

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, Piani, and Souza 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, and Goldberg and Pavcnik 2007 for discussions). We revisit this point when performing robustness exercises in the results section.

B. *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 previously mentioned idea 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. The variable RTC_r is the average tariff change faced by region r, weighted by the importance of each sector in regional employment. Formally:

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

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

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).

II. Data

A. *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, Garred, and Pessoa 2016; Dix-Carneiro and Kovak 2015, 2017b; Hirata and Soares 2016).[13] 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, Pimentel, and Alvarenga 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 (2015) and Costa, Garred, and Pessoa (2016).[14] Table 1 provides descriptive statistics at

[13] A 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.

[14] The micro-regions we use in this paper are slightly more aggregated versions than the ones in Kovak (2013) and Dix-Carneiro and Kovak (2017b) 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

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	13.4	10.7	15.1	13.2	21.7	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$)	Census	754.9	338.4	920.0	372.6	992.3	332.1
Employment rate	Census	0.60	0.05	0.60	0.06	0.64	0.08
Share young (18 to 30 years old)	Census	0.22	0.02	0.23	0.01	0.23	0.02
Share unskilled, ≥18 years	Census	0.48	0.04	0.47	0.04	0.45	0.05
Share young, unskilled, and male	Census	0.09	0.01	0.08	0.01	0.06	0.02
Share urban	Census	0.61	0.20	0.68	0.18	0.73	0.17
Public safety personnel (per 100,000 inhabitants)	Census	614	332	709	341	761	331
High school dropouts	Census	0.55	0.09	0.32	0.06	0.26	0.04
Gini (household income per capita)	Census	0.55	0.04	0.56	0.04	0.52	0.04
Population	Census	353,130	929,562	407,750	1,046,677	457,060	1,143,856
Gov. spending per capita (annual, 2010 R$)[a]	Finance Ministry	342.4	182.8	820.9	331.5	1,061.0	319.0
Gov. revenue per capita (annual, 2010 R$)[a]	Finance Ministry	325.1	161.2	862.9	348.5	1,632.8	556.7
Formal wage bill per capita (annualized, 2010 R$)	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: This table shows 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 correspond 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).

[a] 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.

the micro-region level for the main variables used in our empirical analysis. The respective data sources are discussed in the following sections.

B. *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).[15] We use annual data aggregated to the micro-region level from 1980 to 2010.[16]

Manaus, since it was exempt from tariffs and unaffected by the tariff changes that occurred during the 1990s trade liberalization.

[15] The 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).

[16] Since 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

Both the homicide rate and the total number of homicides have increased substantially 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 tenth and ninetieth percentiles of the distribution corresponded to, respectively, 2.5 and 30 in 1991, and 2.9 and 34 in 2000.

Figure 2 visually displays the type of variation we will be exploring in the paper. Panels A and B, respectively, show how log changes in crime rates are distributed across local economies for the pre-liberalization period (between 1980 and 1991) and for the post-liberalization period (between 1991 and 2000). Since we will be contrasting changes in the log of local crime rates to regional tariff changes (RTC_r), panel C of Figure 2 presents the distribution of RTC_r across micro-regions. As the three maps show, there is a large degree of heterogeneity in both 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 nonrandom 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. Online Appendix C presents correlations between levels and changes in crime rates in five-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.[17] We focus on violent crimes since these are supposed to suffer less from underreporting bias.

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.

[17] 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).

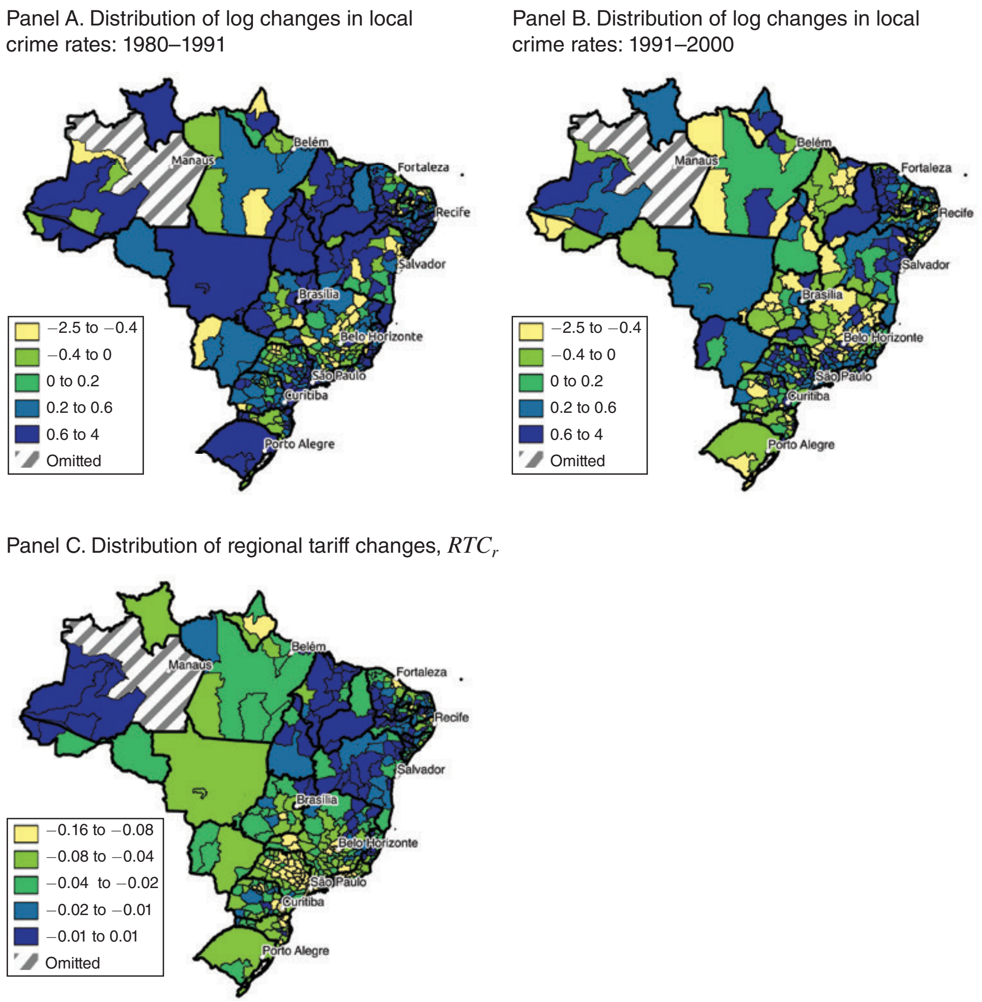

Figure 2. Pre-trends, Regional Tariff Changes, and Post-liberalization log Changes in Local Crime Rates

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, are computed according to the formulae in Section IB.

Our measure of homicides is highly correlated, both in levels and in (five-year) changes, to police-recorded homicides, to property crimes, and to crimes against the person. This pattern is similar if we consider one- or ten-year intervals as well (Tables C.2 and C.3) or if we condition on time and micro-region fixed effects (Tables C.4 and C.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 2017). 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; Sapori, Sena, and da Silva 2012).

C. *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. Online Appendix D explains in further detail other treatments we apply to some variables extracted from the census.

We obtain annual spending and revenue at the municipality level from the Ministry of Finance (*Ministério da Fazenda – Secretaria do Tesouro Nacional*).[18] These data are then aggregated at the micro-region level. Finally, we use the RAIS dataset (*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 dataset 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.

III. 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 (2017b) and empirically describe the evolution of the response of crime to regional tariff changes. In Section IV, we exploit the dynamic response of crime to help distinguish the channels through which these effects propagated.

[18] The data go 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.

TABLE 2—REGIONAL TARIFF CHANGES AND LOG CHANGES IN LOCAL CRIME RATES: 1991–2000

Dep. var.: $\Delta_{91-00}\log(CR_r)$	OLS (1)	OLS (2)	OLS (3)	OLS (4)	2SLS (5)
RTC_r	−1.976	−2.444	−3.838	−3.769	−3.853
	(0.822)	(0.723)	(1.426)	(1.365)	(1.403)
$\Delta_{80-91}\log(CR_r)$				−0.303	0.0683
				(0.0749)	(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^2	0.013	0.052	0.346	0.406	–

Notes: Standard errors (in parentheses) are adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. In column 1, observations are not weighted; in column 2, observations are weighted by population; column 3 adds state fixed effects to column 2; column 4 adds pre-trends to column 3; column 5 shows two-stage least squares, with an instrument for $\Delta_{80-91}\log(CR_r)$ (see text).

Source: DATASUS data

A. *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:

$$(1) \qquad \log(CR_{r,t}) - \log(CR_{r,1991}) = \theta_t RTC_r + \alpha_{s,t} + \epsilon_{r,t},$$

where $CR_{r,t}$ is the crime rate in region r at time $t > 1991$ and $\alpha_{s,t}$ are state-time fixed effects. There are several reasons why we should control for state fixed effects. First, by constitutional mandate, several policies and institutions in Brazil are decentralized to state governments (for example, public security, health and educational policies, and part of the justice system). Second, minimum wages started to be state specific in 2001. Therefore, controlling for state fixed effects accounts for these state-level policies, which are likely to be correlated with local economic conditions. Finally, Brazil has a large territory with states in very different stages of development. Our specification explores variation in RTC_r across micro-regions within states, providing a more transparent analysis and better treatment-control comparisons.[19, 20] In all specifications, we cluster standard errors at the meso-region

[19] We use 1991, instead of 1990, as the base year 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.

[20] In practice, we estimate equation (1) year by year. This method shares similarities with the local projections method of Jordà (2005) as we both sequentially estimate our models over different horizons. However, there are

level to account for potential spatial correlation in outcomes across neighboring regions.[21]

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.[22]

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 changes potentially driven by state-specific policies. 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 preexisting 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 preexisting 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 between changes in crime rates and regional tariff changes is not driven by preexisting trends.

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

important conceptual differences. First, for each fixed horizon, our method exploits cross-sectional variation to estimate θ_t, whereas Jordà (2005) exploits time series variation. Second, our method projects changes in crime directly onto preconstructed shocks (RTC), whereas the local projections method does not exploit such variation.

[21] Meso-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 1980–2010 period.

[22] In 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 the population size (see, for example, Deschênes and Moretti 2009 and Burgess et al. 2011).

Table 3—Regional Tariff Changes and log Changes in Local Crime Rates: 1991–2010

Dep. var.: $\Delta_{91-10}\log(CR_r)$	OLS (1)	OLS (2)	OLS (3)	OLS (4)	2SLS (5)
RTC_r	5.293	6.668	−1.324	−1.198	−1.340
	(1.494)	(2.899)	(2.454)	(2.265)	(2.437)
$\Delta_{80-91}\log(CR_r)$				−0.514	0.0681
				(0.0902)	(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^2	0.066	0.133	0.642	0.702	–

Notes: Standard errors (in parentheses) are adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. In column 1, observations are not weighted; in column 2, observations are weighted by population; column 3 adds state fixed effects to column 2; column 4 adds pre-trends to column 3; column 5 shows two-stage least squares, with an instrument for $\Delta_{80-91}\log(CR_r)$ (see text).

Source: DATASUS data

points, so an increase in crime rates of 0.38 log points 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.[23]

One important concern with our estimates is that the RTC_r shocks may be correlated with preexisting trends in the outcome of interest. For this reason, Tables 2 and 3 included preexisting trends in log crime rates as an additional control to rule out that the estimated effects were driven by a (coincidental) correlation between preexisting trends and (future) regional tariff changes. The results show that pre-trends have no effect on our estimates of interest, indicating that preexisting 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

[23] The *positive* and statistically significant coefficients obtained in columns 1 and 2 are driven by state-level policies in the states of São Paulo and Rio de Janeiro, which account for approximately 30 percent of Brazil's population. From the mid-2000s onward, the state of São Paulo implemented, for reasons not related to our natural experiment, very successful policies to reduce crime (see de Mello and Schneider 2010). The state of Rio de Janeiro also experienced a very particular shock between 2000 and 2010: the major influx of resources from the discovery and exploration of oil reserves along the Brazilian coast. This was clearly unrelated to the trade liberalization but had major implications for economic activity and public expenditures, including in public security, at the state level.

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	0.200	0.162
	(1.096)	(1.409)	(0.893)
State fixed effects	No	No	Yes
Observations	411	411	411
R^2	0.002	0.000	0.426

Notes: Standard errors (in parentheses) are adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. In column 1, observations are not weighted; in column 2, observations are weighted by population; column 3 adds state fixed effects to column 2.

Source: DATASUS data

shocks. In all specifications, the coefficients are small in magnitude, with opposite signs to those from Table 2, and not statistically significant. Finally, we conduct additional robustness exercises in Tables E.1 and E.2 of online Appendix E.1. In these exercises, we sequentially control for initial region characteristics such as pre-trends in crime rates and 1991 levels of the following socio-demographic variables: household per capita income inequality, employment rate, share of males, share of young (less than 30 years old), share of unskilled, share of manufacturing, and share of population in urban areas. The patterns documented in Tables 2 and 3 are robust to these additional controls.

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 state-period effects $\alpha_{s,t}$. Therefore, we cannot make statements about the total effect of the trade reform on the national crime level without imposing restrictive theoretical assumptions. In particular, the main findings in Tables 2 and 3 tell us that the relative effect dissipates over time, but this does not necessarily imply anything about the aggregate effect of liberalization. For example, the relative effect could be temporary but the aggregate effect permanent. 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.

B. *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).

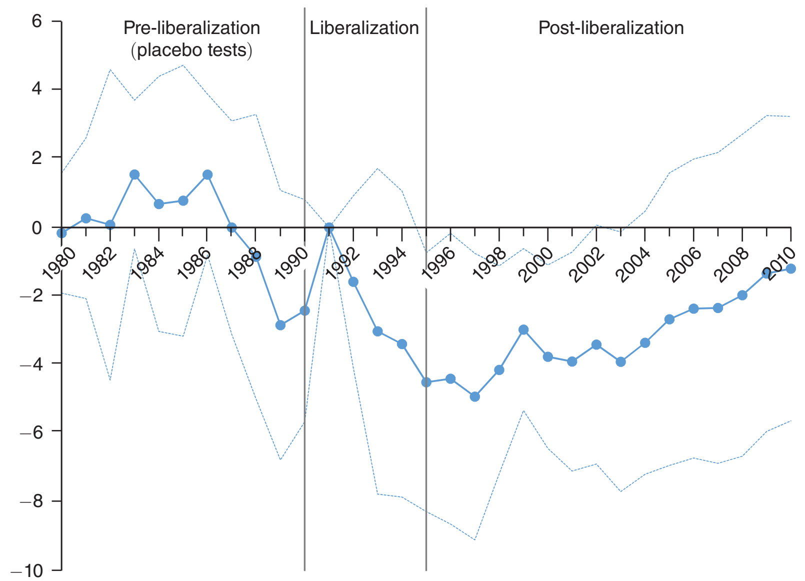

FIGURE 3. DYNAMIC EFFECTS OF REGIONAL TARIFF CHANGES ON LOG CHANGES IN LOCAL CRIME RATES

Notes: 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) and $t = 1980, \ldots, 1991, \ldots, 2010$. Note that RTC_r always reflects tariff changes from 1990–1995. 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 are adjusted for 91 meso-region clusters.

Here, we confirm this pattern by plotting the yearly evolution of the effect of the trade shocks on crime ($\hat{\theta}_t$ for $t = 1992, \ldots, 2010$) 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. Figure 3 shows 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 1992–1994, but these should be interpreted with care, as liberalization was still an ongoing process during these intermediate years.[24] Figure 3 also plots placebo tests—estimates of θ_t for $t = 1980, \ldots, 1990$. None of these coefficients is statistically significant, corroborating the conclusion that preexisting trends in regional crime rates were uncorrelated with the shocks induced by trade liberalization.

[24] However, 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 1990–1993, the R^2 is 0.93.

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.

IV. How Did the Trade Shocks Affect Crime?

A. *Potential Mechanisms*

An established literature shows that regions exposed to increased foreign competition tend to experience deteriorations in labor market conditions (Autor, Dorn, and Hanson 2013; Kovak 2013; Dix-Carneiro and Kovak 2017b). The link between labor market conditions (employment and earnings) and crime has also been extensively explored (Raphael and Winter-Ebmer 2001; Gould, Weinberg, and Mustard 2002; Lin 2008; Fougére, Kramarz, and Pouget 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.[25] Finally, poor labor market conditions can also affect crime indirectly, through increased inequality or deteriorated mental health due to stress or depression (Fajnzylber, Lederman, and Loayza 2002; Bourguignon, Nuñez, and Sanchez 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.[26]

Panel A in Table 5 presents the results for the effect of regional tariff changes on labor market earnings per employed worker in columns 1 and 2 and on employment rates in columns 3 and 4, for the 1991–2000 and the 1991–2010 periods, respectively.[27] 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

[25] For example, there is ample evidence on the role of police presence, schooling, and welfare payments in preventing crime (Levitt 2002, Schargrodsky and Di Tella 2004, Jacob and Lefgren 2003, Lochner and Moretti 2004, Foley 2011).

[26] Remember that regional tariff changes are measured in terms of log points.

[27] 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 (for details on this procedure, see online Appendix D.1). Worker-level labor market earnings sum across all labor market earnings obtained within a month.

Table 5—Investigation of Potential Mechanisms

Panel A. Labor market outcomes

	Earnings		Employment rate	
	1991–2000	1991–2010	1991–2000	1991–2010
	(1)	(2)	(3)	(4)
RTC_r	0.527	0.460	0.643	−0.0510
	(0.123)	(0.243)	(0.0627)	(0.102)
R^2	0.731	0.737	0.528	0.637

Panel B. Government revenue and tax base

	Gov. revenue per capita		Wage bill per capita		Number of formal establishments	
	1991–2000	1991–2010	1991–2000	1991–2010	1991–2000	1991–2010
	(1)	(2)	(3)	(4)	(5)	(6)
RTC_r	1.500	2.330	4.695	8.963	2.519	4.319
	(0.803)	(0.585)	(0.482)	(0.643)	(0.304)	(0.351)
R^2	0.476	0.543	0.569	0.768	0.718	0.793

Panel C. Provision of public goods

	Gov. spending per capita		Public safety personnel		High school dropouts	
	1991–2000	1991–2010	1991–2000	1991–2010	1991–2000	1991–2010
	(1)	(2)	(3)	(4)	(5)	(6)
RTC_r	3.153	5.184	0.940	1.519	−0.354	−2.397
	(0.665)	(0.617)	(0.246)	(0.400)	(0.200)	(0.291)
R^2	0.592	0.724	0.390	0.444	0.479	0.666

Panel D. Miscellaneous

	Suicide rates		Income inequality (Gini)	
	1991–2000	1991–2010	1991–2000	1991–2010
	(1)	(2)	(3)	(4)
RTC_r	1.551	2.148	−0.252	−0.753
	(1.138)	(2.017)	(0.0740)	(0.166)
R^2	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. Changes in regional employment and total labor market earnings per worker are net of composition (see online Appendix D.1). 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) are adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. There are 411 micro-region observations, except for three to four missing values in government spending and revenue. Observations are weighted by population. All specifications control for state-period fixed effects.

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 points 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 (2017a) and Dix-Carneiro and Kovak (2017b) 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).[28]

In panel B of Table 5, we consider other economic consequences of the local tariff shocks. The table analyzes the impact on government revenues (per capita), number of operating formal establishments (with positive employment), and formal wage bill (per 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 points would lead to a reduction of 0.15 log points in government revenues in the medium run and 0.23 in the long run. These results are also consistent with Dix-Carneiro and Kovak (2017b), who document that formal employment and the number of formal establishments gradually decline in adversely affected regions relative to the national average.[29]

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 points, the number of public safety personnel (per capita) is reduced by 0.094 log points 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

[28] 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 (2017a) such as how observations are weighted or the exact definition of labor earnings.

[29] We emphasize that micro-regions are not administrative regions, they are collections of municipalities constructed by the Brazilian Statistical Agency (IBGE) that closely mirror what we understand by a local labor market. The revenue of a municipality comes from transfers from the state and federal governments but also from their own resources. Revenues directly raised by the municipalities come from three forms of taxation: property tax (Imposto Predial e Territorial Urbano—IPTU); tax on services (Imposto Sobre Serviços—ISS); and tax on property sales, transfers, or donations (Imposto sobre a Transmissão de Bens Imóveis—ITBI). Own resources represent an important share of total revenues. Using data on revenues' sources for 1995 and 2000, we find that 27 percent of the resources of a typical micro-region are raised directly by the municipalities that constitute it (the standard deviation across micro-regions is of approximately 17 percent). The raw data is available at http://www.tesouro.fazenda.gov.br/pt_PT/contas-anuais.

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 points 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.

B. *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 III. We argue that by assuming a stable long-run relationship between these variables and crime; by imposing theoretical sign restrictions on their effects on crime; and 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.

Empirical Framework.—Informed by the literature on the socioeconomic 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:

$$\Delta_t \log(CR_r) = \beta^w \Delta_t \log(w_r) + \beta^e \Delta_t \log(P_{e,r}) + \beta^g \Delta_t \log(GovSp_r) + \beta^{ps} \Delta_t \log(PS_r) + \beta^h \Delta_t \log(HSDrop_r) + \beta^i \Delta_t \log(Ineq_r) + \alpha_{s,t} + \eta_{r,t}, \tag{2}$$

where Δ_t refers to long changes over time, t indexes the period over which changes are computed, 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, $Ineq$ to per capita household income inequality, and $\alpha_{s,t}$ are state-period fixed effects.[30] We also assume that $E(\eta_{r,t} | RTC_r, D_s \times D_t) = 0$, that is, conditional on state-period indicators $(D_s \times D_t)$, RTC_r affects crime only through the remaining variables in the right-hand side of equation (2).[31]

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.[32] 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 and state indicators, we can always write

$$\Delta_1 \log(CR_r) = \theta_1 RTC_r + \delta_{s,1} + \varepsilon_{r,1},$$

$$\Delta_2 \log(CR_r) = \theta_2 RTC_r + \delta_{s,2} + \varepsilon_{r,2},$$

where θ_1, θ_2 are projection coefficients, $\delta_{s,1}$ and $\delta_{s,2}$ are state fixed effects, and

$$E(\varepsilon_{r,1} | RTC_r, D_s \times D_1) = E(\varepsilon_{r,2} | RTC_r, D_s \times D_2) = 0$$

[30] In robustness checks, we include in the right-hand side of equation (2) changes in demographic variables such as the share of young and unskilled males and the share of the population living in urban areas. Our framework and derivation are robust to the inclusion of these additional covariates. We omit those in our main derivations to simplify the exposition.

[31] We 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. Also, note that the variables in panel B of Table 5 are not included in the right-hand side of equation (2). The responses of these variables corroborate the results obtained in panel C, but government revenue, the number of establishments, and the formal wage bill per capita should not have any effect on crime rates after we include direct measures of the provision of public goods such as those displayed in panel C.

[32] In 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, $E(u|x) = 0$ (we omit the constant for clarity).

by construction. In fact, these are the equations that we estimated in Tables 2 and 3, when we effectively projected medium- and long-run changes in crime onto RTC_r and state fixed effects 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 interpreted 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 following equations:

$$\Delta_1 \log(X_r) = b_1^X RTC_r + \upsilon_{s1}^X + u_{r,1}^X,$$

$$\Delta_2 \log(X_r) = b_2^X RTC_r + \upsilon_{s2}^X + u_{r,2}^X,$$

where $\upsilon_{s,1}^X$ and $\upsilon_{s,2}^X$ are state-period fixed effects and

$$E(u_{r,1}^X | RTC_r, D_s \times D_1) = E(u_{r,2}^X | RTC_r, D_s \times D_2) = 0$$

by construction. Again, these statistical decompositions can always be carried out, regardless of the covariance and relationship structures between the variables in equation (2).

Substituting the relationship for each of the X variables of interest in equation (2) and collecting terms, one obtains

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

for $t = 1, 2$.

Given the assumption that $E(\eta_{r,t} | RTC_r, D_s \times D_t) = 0$ and the fact that $E(u_{r,t}^X | RTC_r, D_s \times D_t) = 0$ by construction, it follows that $E(\omega_{r,t} | RTC_r, D_s \times D_t) = 0$. By the uniqueness of the projection of $\Delta_t \log(CR_r)$ onto RTC_r and $D_s \times D_t$, it must also be the case that

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

In words, if we have a stable and linear relationship between crime and its underlying determinants, the vector $\boldsymbol{\theta}$ 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 βs solely based on medium- and long-run responses to the RTC_r shocks. In this case, we can estimate the θs and the bs, but we cannot identify the βs. 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 IVB.

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 $\boldsymbol{\theta}$ vector in the left-hand side and six-dimensional $\mathbf{b}$ vectors in the right-hand side, that is, six equations with six unknowns.

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.[33] An increase in the number of high school dropouts should increase crime due to reduced incapacitation and worsened future labor market opportunities, as formally analyzed by Lochner (2010). 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 2002, Schargrodsky and Di Tella 2004), schooling (Jacob and Lefgren 2003, Lochner and Moretti 2004), inequality (Fajnzylber, Lederman, and Loayza 2002; Bourguignon, Nuñez, and Sanchez 2003), and labor market conditions (Raphael and Winter-Ebmer 2001; Gould, Weinberg, and Mustard 2002) on crime.

Therefore, the theoretical and empirical literature suggests that $\beta^w \leq 0$ (higher wages do not lead to increases in crime), $\beta^e \leq 0$ (higher employment rate does not lead to increases in crime), $\beta^g \leq 0$ (higher 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 $\beta^i \geq 0$ (higher inequality does not lead to reductions in crime). Note that these

[33] In 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 et al. 1986).

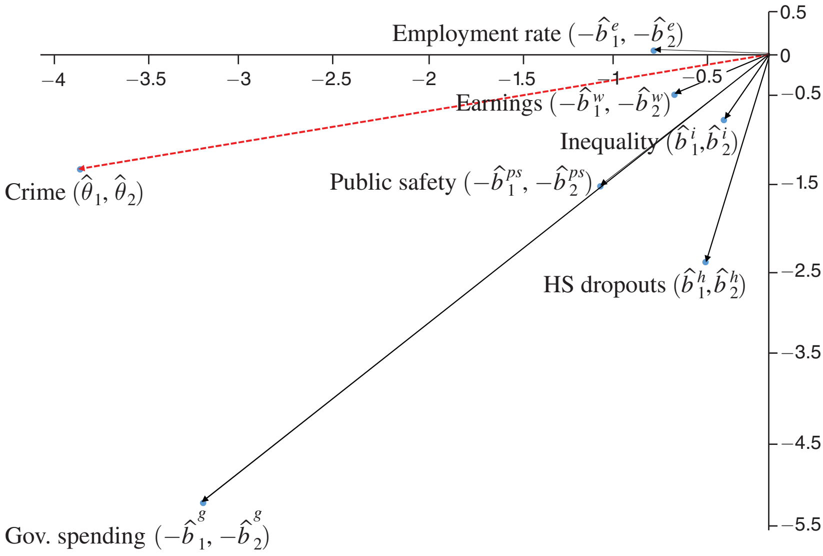

Figure 4. Medium- versus Long-Run Effects of *RTC* on Different Channels

Notes: 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.

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

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

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

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

Two immediate conclusions arise from an inspection of Figure 4. First, note that 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 $\{-\hat{\mathbf{b}}^w, -\hat{\mathbf{b}}^g, -\hat{\mathbf{b}}^{ps}, \hat{\mathbf{b}}^h, \hat{\mathbf{b}}^i\}$ can generate $\hat{\boldsymbol{\theta}}$, as $\hat{\boldsymbol{\theta}}$ does not belong to the cone spanned by these vectors. Second, since $\hat{\boldsymbol{\theta}}$ does belong to the cone spanned by $\{-\hat{\mathbf{b}}^e, -\hat{\mathbf{b}}^w, -\hat{\mathbf{b}}^g, -\hat{\mathbf{b}}^{ps}, \hat{\mathbf{b}}^h, \hat{\mathbf{b}}^i\}$, employment rates **must** play a role in explaining

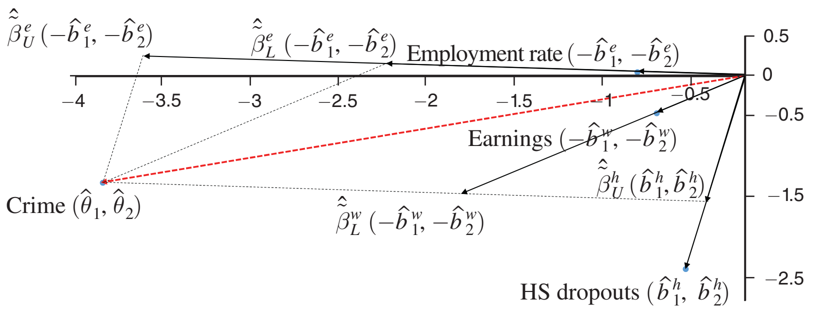

Figure 5. Obtaining Bounds for $\tilde{\beta}^e$

Notes: 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. Here, $\hat{\tilde{\beta}}_L^e$ is obtained by expressing $\hat{\boldsymbol{\theta}}$ as a positive linear combination of $-\hat{\mathbf{b}}^w$ and $-\hat{\mathbf{b}}^e$. Also, $\hat{\tilde{\beta}}_U^e$ is obtained expressing $\hat{\boldsymbol{\theta}}$ as a linear combination of $\hat{\mathbf{b}}^h$ and $-\hat{\mathbf{b}}^e$.

the effects of trade shocks on crime. Therefore, according to our framework and theoretical sign restrictions, we must have $\tilde{\beta}^e > 0$ or, equivalently, $\beta^e < 0$. It is also important to note that although our framework and theoretical sign restrictions *predict* that $\boldsymbol{\theta} \in \{-\mathbf{b}^e, -\mathbf{b}^w, -\mathbf{b}^g, -\mathbf{b}^{ps}, \mathbf{b}^h, \mathbf{b}^i\}$ 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 $\hat{\boldsymbol{\theta}}$ as a positive linear combination of $-\hat{\mathbf{b}}^e$ with the two outermost vectors in the cone spanned by $\{-\hat{\mathbf{b}}^w, -\hat{\mathbf{b}}^g, -\hat{\mathbf{b}}^{ps}, \hat{\mathbf{b}}^h, \hat{\mathbf{b}}^i\}$, $-\hat{\mathbf{b}}^w$, and $-\hat{\mathbf{b}}^h$. The lower bound is obtained by expressing $\hat{\boldsymbol{\theta}}$ as a positive linear combination of $-\hat{\mathbf{b}}^e$ and $\hat{\mathbf{b}}^w$, while the upper bound is obtained by a positive linear combination of $-\hat{\mathbf{b}}^e$ and $\hat{\mathbf{b}}^h$. This procedure is illustrated in Figure 5, which shows geometrically how we can estimate an upper bound $\hat{\tilde{\beta}}_U^e$ and a lower bound $\hat{\tilde{\beta}}_L^e$ for $\tilde{\beta}^e$, based on the configuration of vectors shown in Figure 4.

More rigorously, assuming that the configuration of the **population** projection coefficients $\boldsymbol{\theta}$ and $\mathbf{b}$ is similar to the one obtained for their empirical counterparts (pictured in Figure 4) Appendix F shows that

$$\underbrace{\frac{-\theta_1 b_2^w + \theta_2 b_1^w}{b_1^e b_2^w - b_1^w b_2^e}}_{\tilde{\beta}_L^e} < \tilde{\beta}^e < \underbrace{\frac{-\theta_1 b_2^h + \theta_2 b_1^h}{b_1^e b_2^h - b_1^h b_2^e}}_{\tilde{\beta}_U^e}. \tag{5}$$

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

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

and that $\tilde{\beta}_U^e$ solves

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

In words, these expressions confirm that we can obtain a lower bound for $\tilde{\beta}^e$ by finding the linear combination between $-\mathbf{b}^w$ and $-\mathbf{b}^e$ that generates $\boldsymbol{\theta}$. Similarly, we obtain an upper bound for $\tilde{\beta}^e$ by finding the linear combination between $\mathbf{b}^h$ and $-\mathbf{b}^e$ that generates $\boldsymbol{\theta}$. Since $\beta^e = -\tilde{\beta}^e$, 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:

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

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

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

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

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

Appendix G 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 $RTC \times Period_{91-10}$.[34] 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 IVB, we would obtain an **upward** biased estimate for β^e, as this 2SLS estimator converges to $\beta_U^e > \beta^e$.

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

Table 6—Bounds on the Effect of Employment Rates on Crime

Upper bound 1	Upper bound 2	Lower bound
Panel A. Baseline specification		
−3.307	−4.473	−5.595
(3.205)	(1.386)	(1.925)
Panel B. Adding demographic controls		
Upper bound 1	Upper bound 2	Lower bound
−4.298	−4.309	−4.818
(2.013)	(1.870)	(1.627)

Notes: As noted in the text, upper and lower bounds are algebraically equivalent to 2SLS estimators. Standard errors (in parentheses) 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. Upper bound 1 combines employment with earnings; upper bound 2 combines employment with public safety; the lower bound combines employment with high school dropouts. See text for details.

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

The method we develop here shares similarities with the macroeconomics literature estimating structural vector autoregressions (VARs). This literature typically needs to impose theoretical restrictions on structural parameters in order to map reduced-form estimates to structural estimates. Prominent examples of theoretical restrictions that can be imposed on structural VARs to obtain identification are Sims (1980), Blanchard and Quah (1989), and Uhlig (2005). However, macroeconomists typically impose enough assumptions to achieve exact identification of the structural parameters of interest. Our approach of imposing inequality constraints on structural parameters to achieve partial identification is similar to the approach Leamer (1981) follows to obtain bounds on the elasticities of demand and supply after imposing that the demand function must slope downward and the supply function slopes upward. It is also important to mention that the approach we follow finds theoretical bounds on the effect of employment rates on crime and performs inference on these bounds—so we are able to say that the effect is not too small or too large. We do not proceed to find a confidence set containing that effect with a prespecified probability. Finally, our paper relates to a large literature on mediation analysis, which attempts to identify mechanisms through which a treatment affects an outcome variable.[35] In this literature, a paper that is of particular interest to our study is Dippel et al. (2017) who develop an econometric method to identify through which mechanism import competition affected political outcomes in Germany.

[35] See, for example, Imai, Keele, and Tingley (2010); Pearl (2014); and Heckman and Pinto (2015).

Results.—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 similarly (in a statistical sense) 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.

In our discussion of Figure 4, we argued that employment rates must have nonzero 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 $\boldsymbol{\theta}$ 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 F. In that case, the upper bound estimator for β^e is given by (upper bound 2):

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

leading to 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 8 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, Sacerdote, and Scheinkman 1996 and Levitt 1999). The $\hat{\boldsymbol{\theta}}$ and $\hat{\mathbf{b}}$ estimates arising from these new exercises are shown in Table H.3 in the online Appendix. Figure H.6 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 H.6). 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.

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 points (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 points (37 and 48 percent). Alternatively, consider a region facing a RTC_r shock of -0.1 log points, 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 points 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 points (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.[36]

We conclude this section calling attention to the fact that the procedure we develop is constructive and built over the estimates we obtained in Tables 2, 3, and 5. It is instructive to discuss instances when our procedure is not going to be as informative. First, suppose that X is a variable belonging to the right-hand side of equation (2). If the $\mathbf{b}^X$ vector (with medium- and long-run responses to RTC) belonged to the cone spanned by $\mathbf{b}^e$ and $\boldsymbol{\theta}$ or just above $\mathbf{b}^e$ clockwise (but in the same quadrant), the procedure would not be as informative as it currently is—we would only be able to say that a combination between employment rates and X is essential to generate the dynamic response of crime to the trade shocks. Also, if one of the vectors $\mathbf{b}^w$, $\mathbf{b}^g$, $\mathbf{b}^{ps}$, $\mathbf{b}^h$, $\mathbf{b}^i$ in Figure 4 were in another quadrant, the procedure would also fail, as the response of employment rates would no longer be essential to generate the response of crime to trade shocks. It is equally important to emphasize that while our procedure is able to impose bounds on the effect of employment rates on crime, it is not able to impose bounds on the remaining variables. The only thing we can say is that changes in the provision of public goods and inequality account for 7 percent to 25 percent of the medium-run effect of RTC on crime that we document in Table 2.

V. 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

[36] The total reduced-form effect of a RTC_r shock of –0.1 log points is to increase crime rates by $-3.85 \times -0.1 = 0.385$ log points. Labor market conditions account for a fraction between $\frac{\beta^e_U \times b^e_1 \times -0.1}{\theta_1 \times -0.1} = \frac{-4.5 \times 0.64 \times -0.1}{-3.85 \times -0.1} = 0.75$ and $\frac{\beta^e_L \times b^e_1 \times -0.1}{\theta_1 \times -0.1} = \frac{-5.6 \times 0.64 \times -0.1}{-3.85 \times -0.1} = 0.93$ of this effect. Remember that $\theta_1 = \beta^e b^e_1 + \beta^g b^g_1 + \beta^{ps} b^{ps}_1 + \beta^h b^h_1 + \beta^i b^i_1$ (see equation (3)) and that we are measuring labor market conditions with employment rates only.

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

$$\Delta_t \log(CR_r) = \beta^e \Delta_t \log(P_{e,r}) + \beta^X \Delta_t \log(X_r) + \alpha_{s,t} + \eta^X_{r,t}, \quad (8)$$

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 $\eta^X_{r,t}$ 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 $RTC_r \times I(period = \text{1991–2000})$ and $RTC_r \times I(period = \text{2000–2010})$ as instruments. All specifications control for state-period fixed effects $(\alpha_{s,t})$. Results are shown in Tables 7 and 8.

Column 1 in Table 7 shows the 2SLS estimate of the effect of employment rates on crime if we use $RTC_r \times I(period = \text{1991–2000})$ and $RTC_r \times I(period = \text{2000–2010})$ 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 IVB, but we cannot strictly 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 (8) 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 nonsignificant 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 statistically similar ways over the medium and long runs. Table 8 reproduces the same exercises in Table 7, but also controls for

Table 7—Employment Rates against Alternative Mechanisms

Dep. var.: Δ log(CR_r)	(1)	(2)	(3)	(4)	(5)	(6)
Δ log ($P_{e,r}$)	−4.501	−2.995	−4.428	−4.329	−5.562	−5.063
	(1.348)	(3.371)	(1.319)	(1.374)	(1.928)	(1.523)
Δ log (w_r)		−3.6242				
		(5.764)				
Δ log (Gov. spending$_r$)			−0.3165			
			(0.421)			
Δ log (Public safety$_r$)				−1.120		
				(1.393)		
Δ log (HS dropout$_r$)					0.730	
					(0.887)	
Δ log (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*-statistic	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: Standard errors (in parentheses) are 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 $RTC_r \times I(period = 91-00)$ and $RTC_r \times I(period = 00-10)$ as instruments for the alternative mechanisms. The estimation method is two-stage least squares. There are six missing values for government spending in column 3.

Source: Decennial Census data

changes in demographic variables. We obtain very similar estimates. The only major difference is that we are now able to separate the effect of employment rates from the effect of earnings on crime (column 2), consistent with the results on panel B of Table 6.

At this point, it is important to compare our results with what the literature on labor market conditions and crime has typically found. Mustard (2010) surveys that literature, which has focused exclusively on developed countries with moderate rates of crime. Most of the papers exploiting panel data and IV strategies tend to find statistically significant and economically important effects of labor market conditions on property crime. However, no statistically significant effect on homicides has been detected. These findings contrast with ours, where we find substantial effects of labor market conditions on homicides.

VI. 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: the discreteness and persistence of the shock and 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

Table 8—Employment Rates against Alternative Mechanisms: Adding Controls

Dep. var.: $\Delta\log(CR_r)$	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta\log(P_{e,r})$	−4.481	−4.896	−4.591	−4.668	−4.202	−4.360
	(1.266)	(2.226)	(1.445)	(1.620)	(1.544)	(1.259)
$\Delta\log(w_r)$		0.9391				
		(3.786)				
Δ log(Gov. spending$_r$)			0.1229			
			(0.523)			
Δ log(Public safety$_r$)				0.5274		
				(2.328)		
Δ log(HS dropout$_r$)					−0.223	
					(0.971)	
Δ log(Inequality$_r$)						−0.600
						(2.488)
Δ log(Share YUM$_r$)	−0.274	−0.236	−0.375	−0.455	−0.288	−0.238
	(0.415)	(0.421)	(0.642)	(0.954)	(0.435)	(0.408)
Δ log(Share Urban$_r$)	−1.119	−1.182	−1.219	−1.227	−1.188	−1.182
	(0.397)	(0.309)	(0.350)	(0.392)	(0.310)	(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-statistic	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: Standard errors (in parentheses) are 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 $RTC_r \times I(period = 91\text{–}00)$ and $RTC_r \times I(period = 00\text{–}10)$ as instruments for the alternative mechanisms. The estimation method is two-stage least squares. There are six missing values for government spending in column 3. YUM stands for young, unskilled, and male.

Source: Decennial Census data

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, Chaudhuri, and McLaren 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, Utar 2015, Dix-Carneiro and Kovak 2017a). 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.

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 episode, 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 (2017a) show that the long-run employment recovery in harder hit locations was driven by slow transitions into informal employment. Therefore, it seems that lower quality jobs in the informal sector were enough to keep individuals from moving into crime. This conclusion has two implications. First, in the context we analyzed, idleness appears to be an important driver of crime. Second, stricter enforcement of labor regulations may slow down the recovery of the labor market as a whole, as the supply of informal sector jobs becomes constrained. Our results suggest that this can exacerbate the response of crime to economic downturns. In the context of Brazil, our findings suggest that more lax enforcement of labor regulations and assistance to displaced workers to quickly find reemployment may help dampen the increase in crime during times of economic hardship.

These results also suggest that employment rates are more important drivers of homicide rates than income. This contrasts with findings by Gould, Weinberg, and Mustard (2002) who report that, in the United States, depressed earnings of young unskilled males have a larger effect on property crime than unemployment. To our knowledge, this is the only other paper attempting to separate the effect of unemployment from that of earnings. However, Gould, Weinberg, and Mustard (2002) do not allow for effects of the economic shocks through channels other than the labor market (such as public goods provision and inequality). Indeed, to our knowledge, ours is the first paper trying to disentangle the different mechanisms (beyond labor market conditions) through which economic shocks may affect crime.

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 IV 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). Further research in this direction should be carried out to check whether similar conclusions would hold in other contexts.

It is also important to highlight the limits to interpretation imposed by our empirical strategy. At first sight, our findings may seem to suggest that public safety personnel played a negligible role in driving homicide rates over the period we study. This result should be seen with extreme caution. In Brazil, both the military police, responsible for ostensive patrolling, and the civil police, responsible for investigations, are managed with full autonomy by state governments. So, decisions related to hiring, allocation, equipment, and crime-fighting strategies are made at the state level. Therefore, a large chunk of the effect of police on crime is likely to be absorbed by our state-time fixed effects. Similarly, our identification relies on a difference-in-difference logic to uncover the relative effect of the trade-induced shocks on crime. So we cannot speak to the aggregate effects of trade liberalization on crime, since any potential impact taking place at the national level—be it positive or negative—would be immediately washed away by our empirical strategy.

Finally, our study focuses on a developing country with high levels of violence and documents 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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