Local Labor Market Conditions and Crime: Evidence from the Brazilian Trade Liberalization *

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Abstract

This paper estimates the effect of local labor market conditions on crime in a developing country with high crime rates. Contrary to the previous literature, which has focused exclusively on developed countries with relatively low crime rates, we find that labor market conditions have a strong effect on homicides. We exploit the 1990s trade liberalization in Brazil as a natural experiment generating exogenous shocks to local labor demand. Regions facing more negative shocks experience large relative increases in crime rates in the medium term, but these effects virtually disappear in the long term. This pattern mirrors the labor market responses to the trade shocks. Using the trade liberalization episode to design an instrumental variables strategy, we find that a 10% reduction in expected labor market earnings (employment rate $\times$ earnings) leads to an increase of 39% in homicide rates. Our results highlight an additional dimension of adjustment costs following trade shocks that has so far been overlooked in the literature.

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1 Introduction

Researchers and policy makers have long been interested in the link between labor market conditions and crime. Given the interest in this topic, it is not surprising that there is a large literature studying the extent to which labor markets affect crime. However, this literature has traditionally failed to establish and quantify this relationship within a causal framework. Indeed, omitted variable bias and reverse causality issues have posed considerable obstacles to inference in this line of work. Nevertheless, there has been progress in the past 15 years regarding identification with papers focusing on panel data and instrumental variable strategies (see Mustard, 2010, and references therein).

To date, this literature has focused exclusively on developed countries with relatively low crime rates.[1] Homicide rates in previously studied countries range from 0.7 to 4.7 cases per 100,000 inhabitants in Sweden and the United States respectively according to a 2012 report by the United Nations Office on Drugs and Crime.[2] These rates are considerably lower than those in many countries in the Latin America and the Caribbean region. As a matter of fact, among the 20 most violent countries in the world, 14 are located in the region. In 2012, homicide rates in these countries ranged from 22.1 in the Dominican Republic to 90.4 per 100,000 inhabitants in Honduras. These countries have in common poor labor market conditions, poor education systems, large levels of social inequality and a strong presence of organized crime. Therefore, we would expect labor market conditions to have much larger and severe effects on crime rates in such countries, with potentially large consequences for welfare.

This paper studies how local labor market conditions affect crime in Brazil. The country is ranked first in absolute number of homicides, exceeding 50,000 cases in 2012. That same year, Brazil was ranked 18$^{\text{th}}$ among countries with the largest homicide rates recording 25.2 cases per 100,000 inhabitants. The poor labor market conditions and high incidence of crime and violence in the country provide a unique empirical setting to investigate the effect of local labor market conditions on crime, bringing a new perspective to the topic.[3] We overcome the endogeneity issue exploring a sharp natural experiment, namely, the Brazilian trade liberalization of the 1990s which generated large and permanent local labor demand shocks across the country (Kovak, 2013; Dix-Carneiro and Kovak, 2015b). We make use of this natural experiment to design an instrumental variable strategy to estimate the effect of local labor market conditions on crime. Along the way, we also

[1] Mustard (2010) surveys studies focusing on the United States, Sweden, Australia, Sweden, France and New Zealand.

[2] Homicide rates are widely considered as a reliable barometer of all violent crime and overall crime at the national level (see Fox and Zawitz, 2000; Fajnzylber et al., 2000; Pope and Pope, 2012).

[3] The share of informal workers in Brazil accounted for 58%, 64% and 49% of the labor force in 1991, 2000 and 2010 respectively. Informality is defined as work without a government-registered labor contract, self-employment or domestic work and was calculated using the Brazilian Demographic Census.

emphasize the reduced-form specification directly relating crime to trade-induced local shocks for two reasons. First, it highlights a dimension of adjustment costs, beyond those directly associated with labor reallocation, that may follow trade liberalization episodes (Dix-Carneiro, 2014). Second, it helps to validate our identification strategy, as we will explain later.

We focus on 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.[4] The paper considers three moments in time, corresponding to three Census years: (i) 1991, describing the equilibrium in the Brazilian labor market before the trade reform; (ii) 2000, referring to the medium-term equilibrium outcome after the trade reform; and (iii) 2010, representing the long-term equilibrium. Our empirical strategy investigates how crime rates changed in each local labor market as liberalization took hold, tracing out its effects over the medium- and long-term horizons.[5] In order to do so, we construct a measure of trade-induced shocks to local labor demand based on a weighted average of sectoral tariffs, 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.

Our main result shows that the deterioration in local labor market conditions induced by the trade reform was accompanied by substantial increases in crime rates. We provide evidence in this direction and quantify this effect using an instrumental variables (IV) strategy where regional tariff changes are used as an instrument for changes in local labor market conditions. Our first stage generates results similar to those previously documented in the literature, namely, regions specialized in industries exposed to larger reductions in tariffs faced a deterioration in labor market conditions relative to the national average in the medium term (1991-2000), followed by a partial recovery in the long term (1991-2010).[6] Our second stage shows that this medium-term deterioration in local labor market conditions led to increases in crime rates. We estimate that a 0.1 log point reduction in expected labor market earnings (employment rate $\times$ earnings) leads to an increase of 0.33 log point (39 percent) in homicide rates. To put these quantitative effects in perspective, the 90th percentile in the 1991 distribution of homicide rates was 12 times as large as the 10th percentile (30 and 2.5 per 100,000 inhabitants, respectively). While OLS regressions relating changes in local crime rates to changes in local labor market con-

[4]In Appendix A, we provide evidence that homicide rates are a good proxy for the overall incidence of crime. In particular, we show that homicide rates are closely correlated with other crime rates at the local labor market level (both in levels and in changes).

[5]We conduct our analysis at the micro-region level, which is a grouping of economically integrated contiguous municipalities. Section 4 provides details.

[6]The partial recovery is due to a recovery in employment rates. The effect of the trade shock on local earnings is long-lasting.

ditions lead to non-significant results, our IV strategy points to large and significant causal effects of the labor market on crime. This highlights the importance of our identification strategy.

We also analyze a reduced-form specification where we directly regress changes in local crime rates on regional tariff changes. This reduced-form specification is interesting in itself for at least two reasons. First, it draws attention to the total effect of the trade-induced local shocks on crime rates. Second, it allows us to analyze in detail the timing of the response of crime to the change in tariffs, providing supporting evidence in favor of our key identifying assumption.

Our reduced-form results indicate that regions facing more negative trade-induced shocks went through 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 or after 2003, there is no statistically significant effect of the trade reform on crime. Our methodology allows us to trace out the dynamics of the overall response of crime rates to the trade-induced shock and to show that it closely matches the timing of the labor market effects. We also conduct a placebo exercise that confirms that region-specific trends in crime before the reform were uncorrelated with the (future) trade-induced shocks. These exercises lend further credibility to our results. Contrary to the previous literature, we are able to provide compelling evidence in support of our identification hypothesis. The benchmark specification for the reduced form indicates that regions experiencing a 0.1 log point larger reduction in tariffs (corresponding to a movement from the 90th to the 10th percentile of regional tariff changes) would experience relative increases in crime rates of 0.38 log point (46 percent) over the medium term.[7]

Our contribution to the literature is threefold. First, contrary to the existing literature, which has so far focused exclusively on developed countries with relatively low crime rates, we study a developing country with poor labor market conditions and high prevalence of crime. This is an appealing setting, since the criminogenic effect of deteriorations in labor market conditions should be much stronger and more relevant in countries with these characteristics.

Second, we believe that our empirical exercise improves upon the existing literature on labor markets and crime providing a more convincing identification strategy. The main concerns in this context are the endogeneity of labor market conditions to crime and the presence of unobserved factors determining both simultaneously. For these reasons,

[7]Previous literature on labor markets and crime has typically focused on young and unskilled workers. In our setting, since the effect of the trade reform was roughly homogeneous across demographic groups, it makes little difference in terms of estimated coefficients if we consider all workers, or only the young or unskilled.

recent papers have used instruments for labor market conditions based on Bartik shocks, combining initial employment composition across industries and subsequent changes in aggregate employment, exchange rates, oil prices, and military contracts (Raphael and Winter-Ebmer, 2001; Gould et al., 2002; Lin, 2008; Fougère et al., 2009). Still, no paper in this literature uses a clear and well defined natural experiment. The natural experiment that we explore – the 1990s trade liberalization in Brazil – presents a series of advantages relative to the instruments that have been used previously: (i) it captures an event that is discrete in time and permanent; (ii) the exogeneity and exclusion restrictions are plausibly satisfied, meaning that it is unlikely that a major national trade reform was driven by local crime conditions and it is difficult to think of an effect of trade policy on crime that would not have worked through labor markets;[8] and (iii) the labor market implications of the trade reform have been documented in the literature to be large and, for certain outcomes, long lasting. These features of our natural experiment allow us to present direct evidence supporting our identification hypothesis and to trace out the dynamic response of crime in ways that are novel to the literature. Probably due to a combination of our improved empirical strategy and the particular context analyzed, the response of crime to labor market conditions that we document is much stronger than that documented before. We show that deteriorations in labor market conditions in Brazil are strongly associated with increases in homicide rates, while the previous literature on developed countries found robust effects of labor market conditions only on property (non-violent) crime, and a zero effect on homicide rates.

Third, we explicitly consider the link between trade shocks and crime. The links between, on one side, trade and labor markets and, on the other side, labor markets and crime are well established in the literature. However, the connection between trade-induced labor market shocks and crime has never been explored.[9] The effect of trade policy on crime is interesting in itself, since it highlights a dimension of adjustment costs, beyond those directly associated with labor reallocation, that has been overlooked in the past.

[8] Trade could also affect crime directly through the market for final goods. For example, this would be the case if trade liberalization affected the incentives for smuggling and other illegal trade, as explored by Prasad (2012). However, notice that, with a national market for final goods, these effects would tend to be homogeneous across the country (or concentrated along distribution routes). Our identification strategy relies on the differential effect that tariff reductions have on the market for factors, specifically the labor market, making use of the variation in the initial structure of employment across local labor markets. Any aggregate effect of trade liberalization on crime – or any effect not correlated with the initial structure of employment across sectors – is automatically controlled for. In any case, in the situation analyzed by Prasad (2012), incentives for illegal trade are higher under a more restrictive trade regime, generating a negative correlation between liberalization and crime in the aggregate, in the opposite direction of the relationship we investigate.

[9] The only other paper to consider a somewhat similar setting is Iyer and Topalova (2014), who analyze the effect of climate and trade-induced poverty changes on crime in India.

The remainder of the paper is structured as follows. Section 2 provides a background of the 1990s trade reform in Brazil and of its documented effect on local labor markets. Section 3 discusses our empirical framework. It starts by providing a theoretical background behind the relationships between trade and local labor markets, and local labor markets and crime, and then discusses our empirical approach and identification strategy. Section 4 describes the data we use and provides descriptive statistics. Section 5 presents the main results exploring the links between trade-induced shocks to local labor demand, labor market conditions, and crime. Finally, Section 6 closes the paper with a few concluding remarks.

2 Trade Liberalization and Local Labor Markets in Brazil

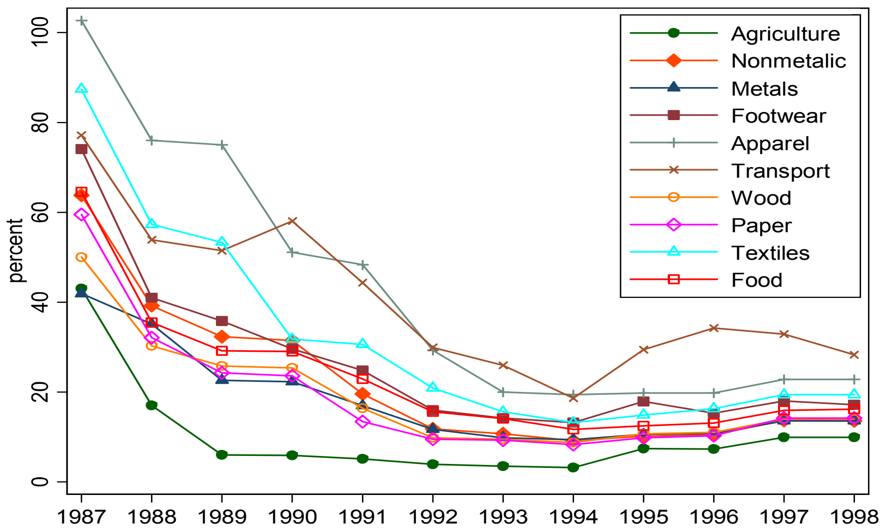
Starting in the late 1980s and early 1990s, Brazil initiated a major unilateral trade liberalization process, which was fully implemented between 1990 and 1995. The trade reform ended nearly one hundred years of high barriers to trade, which were part of a deliberate import substitution policy. Nominal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

Trade liberalization effectively started in March 1990, when the newly elected president unexpectedly eliminated non-tariff barriers (e.g. suspended import licenses and special customs regime), often immediately replacing them with higher import tariffs in a process known as "tariffication" (*tarificação*, see de Carvalho, Jr., 1992). Even though this change left the effective protection system unaltered, it transformed tariffs in 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.[10] The tariff data we use in this paper are provided by Kume et al. (2003), and have been extensively used in the previous literature on trade and labor markets in

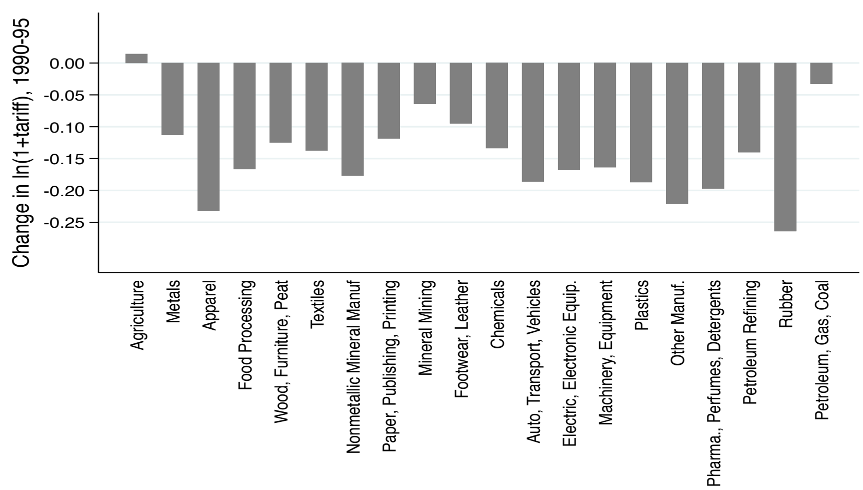
[10]Changes in tariffs after 1995 were trivial compared to the changes that occurred between 1990 and 1995. See discussion in Appendix B.

Brazil. Nominal tariff cuts were very large in some industries (see Figure 1, Panel *a*) and the average tariff fell from 30.5 percent in 1990 to 12.8 percent in 1995. Panel (b) in Figure 1 shows the approximate percentage change in sectoral prices induced by changes in tariffs under the assumption of complete pass-through (we plot the change in the log of one plus tariffs in the figure, since it is the variable used in our empirical analysis). Importantly, there was ample variation in tariff cuts across sectors, which will be essential in our identification strategy.

Figure 1: Tariff changes across industries



(a) Nominal Tariffs, Hirata and Soares (2015)



(b) Changes in $\log(1 + \text{tariff})$, 1990-1995, Dix-Carneiro and Kovak (2015b)

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

A vast list of papers has investigated the labor market effects of the Brazilian trade liberalization. In the context of this study, two recent papers are especially relevant. Kovak (2013) investigates the local labor market effects of the Brazilian trade reform. Using the 1991 and 2000 waves of the Decennial Census, he shows that wages strongly declined in regions that faced larger exposure to foreign competition relative to less exposed regions. Dix-Carneiro and Kovak (2015b) complement these findings and analyze the effects of the trade-induced local shocks on earnings, employment, and informality over the medium (1991-2000) and long term (1991-2010). A robust finding that emerges from these two papers is that the local labor demand shocks induced by trade liberalization had significant and economically large effects on local wages, labor market earnings, employment, and informality, with some of these effects persisting at least until 2010.

The next section explains the existing theory behind the effects of trade liberalization on local labor markets and develops a simple model illustrating the role of labor market conditions as determinants of crime. These theoretical considerations guide our empirical strategy, which links trade-induced shocks to local labor demand to local changes in crime rates.

3 Empirical Framework

This section starts by laying out the theoretical foundations linking: (i) trade liberalization to local labor market outcomes, and (ii) local labor market market outcomes to crime. We follow the existing literature to establish the first of these links and present a simple occupational choice model to shed light on the second one. These theoretical considerations guide our empirical investigation of the effects of local labor market conditions on crime.

Our empirical strategy exploits a natural experiment, generated by the Brazilian trade reform, to analyze this issue. The reform induced large exogenous shocks to local labor demand, with substantial effects on labor market outcomes. We use this natural experiment to create an instrument to labor market conditions and also emphasize the reduced-form relationship between trade shocks and crime, which has not been analyzed in previous research but speaks directly to the burgeoning literature on the adjustment costs following trade reforms.

3.1 Trade and Local Labor Markets: Theoretical Benchmark

The empirical literature on regional labor market effects of foreign competition exploits the fact that regions within a country often specialize in the production of different goods. For Brazil, Kovak (2013) shows that 96 percent of workers in Traipu (in the state of Alagoas) produced agricultural goods in 1991. On the other hand, workers in Rio de Janeiro were mostly concentrated in Apparel, Metals and Food Processing. 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 labor market outcomes such as earnings, wages and employment would have deteriorated in Rio de Janeiro relative to Traipu's.

Although the idea above was initially introduced by Topalova (2010), Kovak (2013) formalized and refined it with a model in which industries employ labor and factors which are region- and sector-specific, produce according to constant returns to scale technologies, and behave competitively. Specific factors are exogenously fixed across regions and sectors and workers cannot move across regions. However, workers can move across industries within regions without frictions equalizing wages within each location. Tariff reductions across sectors implemented by trade liberalization reduce the prices faced by each industry. In the context of this model, Kovak (2013) shows that the effect of trade liberalization on wages at the regional level is given by:

$$\Delta \log (w_r) = RTC_r,$$

where $\Delta \log (w_r)$ is the trade-induced proportional change in the wage rate in region r and RTC_r is the "Regional Tariff Change" in region r, which effectively measures by how much trade liberalization affected labor demand in the region. RTC_r is the average tariff change faced by region r, weighted by the importance of each sector in regional employment. Formally:

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

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

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).

Dix-Carneiro and Kovak (2015b) extend Kovak (2013) and allow regional labor and specific-factors to respond to the trade-induced local shock. Their model generates the reduced-form equation below:

$$\Delta \log (w_r) = \alpha + \beta RTC_r + \varepsilon_r,$$

where the magnitude of β depends on the relative size of the adjustment of labor and specific-factors to the local shock RTC_r. This is the main specification studied in Kovak (2013) and Dix-Carneiro and Kovak (2015b), although the latter also analyze how other labor market outcomes such as formal employment, non-employment, job creation and destruction, and informality respond to regional tariff changes over different time horizons.

We focus on changes in output tariffs to construct regional tariff changes (RTC_r). However, one may wonder why we don't make use of changes in input tariffs in our analysis. One simple way to include information on both input and output tariffs involves the calculation of effective rates of protection. Effective rates of protection measure the effect of the entire tariff structure on value added per unit of output in each industry, when both intermediate and final goods are taken into account. Kume et al. (2003) have also calculated effective rates of protection, according to Corden (1974). At the level of aggregation used in this paper (the finest possible level that makes the industry classification of Kume et al. (2003)'s tariffs compatible with the 1991 Demographic Census), 1990-1995 changes in input tariffs are almost perfectly correlated with changes in output tariffs. Consequently, regional tariff changes computed using changes in output tariffs and using changes in effective rates of protection are also almost perfectly correlated (the correlation is greater than 0.99). 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.

3.2 Local Labor Markets and Crime: A Simple Model

In this section, we present a simple partial equilibrium model that illustrates how labor market conditions can directly affect crime rates. The model follows the tradition of crime as an occupational choice (Ehrlich, 1973) and delivers a sufficient statistic for the effect of labor market conditions on crime. This model serves mainly as a guide to our empirical investigation and does not intend to be an encompassing theoretical assessment of, or an original theoretical contribution to, the analysis of the relationship between labor markets and crime.

Individuals decide between looking for work or engaging in criminal activities. If individuals decide to look for work, they find a job, which pays w, with probability P_e. With probability $1 - P_e$ they do not find a job and receive zero income. If individuals decide to engage in criminal activity, they are caught with probability P_c, in which case all of their illegal income is confiscated and they get a net income of zero.[11] With probability $1 - P_c$ they are not caught and enjoy their illegal income y. Individuals are risk neutral and care about the log of expected income in addition to idiosyncratic preference shocks ϵ_i^w and ϵ_i^c which tilt individuals toward work or crime.

The utilities of looking for work and engaging in criminal activities are given, respectively, by the following expressions:

$$\begin{aligned} U_i^w &= \underbrace{\log\left(w \times P_e\right)}_{\equiv V_w} + \nu\epsilon_i^w, \\ U_i^c &= \underbrace{\log\left(y \times (1 - P_c)\right)}_{\equiv V_c} + \nu\epsilon_i^c. \end{aligned}$$

The preference shocks ϵ_i^w and ϵ_i^c follow standard Gumbel distributions and are independent from each other. In addition, $\nu > 0$ is a scale parameter determining the dispersion of these preference shocks. The crime rate is given by the fraction of individuals who choose crime over work, or $\Pr\left(U_i^c > U_i^w\right)$. Using properties of Gumbel distributions, this fraction can be written as:

$$\begin{aligned} CR &= \Pr\left(U_i^C > U_i^w\right) = \frac{e^{\frac{1}{\nu}V_c}}{e^{\frac{1}{\nu}V_c} + e^{\frac{1}{\nu}V_w}}, \\ \Rightarrow \frac{CR}{1 - CR} &= \exp\left\{\frac{1}{\nu}\left(V_c - V_w\right)\right\}, \\ \Rightarrow \log(CR) &\approx \log\left(\frac{CR}{1 - CR}\right) = \frac{1}{\nu}\left(V_c - V_w\right). \end{aligned}$$

[11] Incorporating punishment associated with being caught, or some utility flow from unemployment, would not change the qualitative implications of the model in terms of the effect of labor market variables on crime. However, these changes would not allow us to obtain the simple empirical specification in equation (2). Therefore, for simplicity, we omit these terms.

The approximation in the last line follows if $CR << 1$, which is typically the case. If we assume that the return to crime is constant over time, we obtain the following expression relating changes in $\log(CR)$ to changes in $\log(w \times P_e)$:

$$\Delta \log(CR) = -\frac{1}{\nu} \Delta \log(w \times P_e) \,. \tag{1}$$

The variable $(w \times P_e)$ summarizes the labor market conditions that affect local crime rates. We refer to this variable as "expected labor market earnings." It is important to emphasize that the model delivers the prediction that both changes in earnings and in the probability of finding a job determine changes in crime rates. Therefore, given that changes in local earnings and in local employment are usually correlated, any specification relating changes in just one of these variables to changes in crime rates – as commonly seen in the labor markets and crime literature – will also be indirectly capturing the effect of the omitted variable.

For expositional clarity, we have assumed that the gain from criminal activities does not depend on labor market conditions. If this is not the case, then the estimate of the effect of labor market conditions on crime rates will capture both a direct effect and an indirect effect through the payoff of crime.

To fix ideas, assume that the reward to crime, y, also depends on labor market conditions as follows: $y = \overline{RC}\,(w \times P_e)^{\phi}$, where $\overline{RC}$ is a constant and $\phi > 0$. Therefore:

$$\begin{aligned} \Delta \log(CR) &= \frac{1}{\nu}\left(\Delta V_c - \Delta V_w\right), \\ &= \frac{1}{\nu}\left(\phi - 1\right) \Delta \log(w \times P_e) \,. \end{aligned}$$

This extension illustrates the idea that local labor market conditions can have opposite effects on crime rates: a deterioration in local labor market conditions can increase crime through its direct impact (as illustrated by the simpler version of the model), but it can also work in the opposite direction since it may decrease the payoff from criminal activities. Given that crimes target not only income, but also accumulated wealth, we expect that the direct effect of the labor market – through opportunities of employment and legal earnings – is more relevant than the indirect effect – through potential targets for criminal activity. Still, this version of the model indicates that, from a strictly theoretical perspective, the sign of the effect of labor market conditions on crime is ultimately an empirical question.

3.3 Empirical Strategy

The effect of labor market conditions on crime is summarized by the empirical counterpart of equation (1) discussed in the previous section:

$$\Delta_{s,s'} \log (CR_r) = \mu_{s,s'} + \rho_{s,s'} \Delta_{s,s'} \log (w_r \times P_{e,r}) + u_{r,s,s'}, \tag{2}$$

where $\mu_{s,s'}$ and $\rho_{s,s'}$ are parameters, $u_{r,s,s'}$ is a random term, r indexes regions and s and s' indicate, respectively, the initial and final periods. Since we estimate our regressions considering various time intervals $[s, s']$, we also index the coefficients and the error term by s and s'. In this context, our objective is to identify the parameter $\rho_{s,s'}$. However, a simple OLS estimation of equation (2) is likely to be subject to omitted variable bias, as there may be factors that simultaneously determine local labor market conditions and crime that are not controlled for in the regression above. For example, local labor market conditions may be driven by changing urbanization or social norms, which are both likely to affect crime rates. Reverse causality from crime to labor market conditions, as when dangerous areas lead businesses to shut down and move to other regions, is also a possibility. If this were the case, the $\rho_{s,s'}$ coefficient estimated from an OLS regression would be biased and would not reflect the causal effect of labor markets on crime.

We overcome this problem using local labor demand shocks induced by the trade reform as an instrument for labor market conditions. In our first stage, we isolate the variation in local labor market conditions driven by the regional tariff changes estimating the following equation:

$$\Delta_{s,s'} \log (w_r \times P_{e,r}) = \theta_{s,s'} + \sigma_{s,s'} RTC_r + v_{r,s,s'}, \tag{3}$$

where $\theta_{s,s'}$ and $\sigma_{s,s'}$ are parameters and $v_{r,s,s'}$ is a random term. Using this IV strategy to estimate equation (2) and using RTC_r as an instrument for local labor market conditions, we are arguably able to recover an unbiased estimate of the parameter $\rho_{s,s'}$, indicating the effect of changes in expected labor market earnings on crime rates. We estimate these effects in the medium ($s = 1991$ and $s' = 2000$) and long ($s = 1991$ and $s' = 2010$) terms. Most of our analysis, though, is focused on medium-term effects, as we explain in detail in the results section.

Given the discussion from Section 2, our instrument RTC_r considers the changes in tariffs between 1990 and 1995, corresponding to the period of actual liberalization during the Brazilian trade reform. Changes in tariffs after 1995 were very modest relative to the changes implemented between 1990 and 1995. Appendix B confirms that changes in tariffs over longer time intervals in the post-1990 period (1990-2000 or 1990-2010) are very highly correlated with the changes observed between 1990 and 1995. Therefore, the choice of time interval for the calculation of RTC_r is of little consequence in terms of the qualitative results presented in the paper.

To implement the IV strategy, we adopt a two-step procedure in which we obtain region-specific log earnings and employment rates after controlling for age, gender, and education. This is important because regional changes in composition that might be correlated with regional tariff changes would lead to changes in average region-specific earnings and employment rates, even in the absence of effects of the trade shocks on the labor market. In the first step, we obtain region- and year-specific log earnings estimating the Mincerian regression below and saving the $\widehat{\log(w_{rs})}$ estimates:

$$\begin{aligned}\log(w_{irs}) &= \log(w_{rs}) + \sum_k \eta^w_{ks} I(Educ_i = k) + \gamma^w_s I(Female_i = 1) + \\ &\quad \delta^w_{1s}(age_{is} - 18) + \delta^w_{2s}(age_{is} - 18)^2 + \varepsilon^w_{irs}, \end{aligned} \tag{4}$$

where w_{irs} represents monthly labor market earnings for worker i in region r in year s, $I(Educ_i = k)$ is a dummy variable corresponding to years of schooling k, $I(Female_i = 1)$ is a dummy for gender, age_{is} indicates age, and $\log(w_{rs})$ captures the average of the log of monthly earnings across regions r and time periods s for observationally identical individuals.[12]

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

$$\begin{aligned}Emp_{irs} &= P_{e,rs} + \sum_k \eta^e_{ks} I(Educ_i = k) + \gamma^e_s I(Female_i = 1) + \\ &\quad \delta^e_{1s}(age_{is} - 18) + \delta^e_{2s}(age_{is} - 18)^2 + \varepsilon^e_{irs}, \end{aligned} \tag{5}$$

where Emp_{irs} indicates if individual i in region r was employed in year s, and $P_{e,rs}$ captures the average probability of employment across regions r and time periods s for observationally identical individuals.

Once we collect the $\widehat{\log(w_{rs})}$ and $\widehat{P}_{e,rs}$ estimates, we compute a local labor market index given by $\log(\widehat{w_{rs} \times P_{e,rs}}) = \widehat{\log(w_{rs})} + \log\left(\widehat{P}_{e,rs}\right)$, which can be approximately interpreted as the log of expected earnings from the model in the previous section and can be used to estimate equations (2) and (3).

We also estimate reduced-form relationships connecting changes in crime directly to the regional tariff changes. The reduced-form regressions are given by the following specification:

$$\Delta_{s,s'} \log(CR_r) = \xi_{s,s'} + \kappa_{s,s'} RTC_r + \epsilon_{r,s,s'}, \tag{6}$$

[12] Appendix D conducts the same type of analysis focusing on hourly wages instead of earnings. Results are very similar.

where $\xi_{s,s'}$ and $\kappa_{s,s'}$ are parameters and $\epsilon_{r,s,s'}$ is a random term.

The reduced-form exercise is of particular interest in our context for a couple of reasons. First, it highlights an additional dimension of adjustment costs following trade reforms that has so far been overlooked in the literature. Second, while we observe labor market data only every ten years (census years), we have homicide data for every year between 1980 and 2010. Therefore, the reduced-form analysis allows us to closely examine the timing of the relationship between regional tariff changes and crime. This exercise is useful in two ways: (i) to perform placebo tests before the liberalization period; and (ii) to trace out the specific dynamics of change in crime rates after the reform. Both analyses provide evidence in support of our identification strategy. We conduct a series of exercises estimating equation (6) using combinations of s and s' in different periods between 1980 and 2010. The trade shock that we explore is discrete and permanent. Therefore, this strategy can trace out the dynamic response of crime to labor demand shocks in a way that has not been done before in the literature.

4 Data

4.1 Local Labor Markets

We conduct our analysis at the micro-region level, which is a grouping of economically integrated contiguous municipalities with similar geographic and productive characteristics. The definition of a micro-region closely parallels the notion of a local labor market and has been widely used as the unit of analysis in the literature on the local labor market effects of trade liberalization in Brazil (Kovak, 2013; Costa et al., 2014; Dix-Carneiro and Kovak, 2015a,b; Hirata and Soares, 2015).[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 et al., 2008) and construct micro-regions that are consistently identifiable from 1980 to 2010. This process leads to a set of 411 local labor markets, as in Dix-Carneiro and Kovak (2015a).[14]

4.2 Crime

We use homicide rates computed from mortality records as a proxy for the overall incidence of crime. These records come from DATASUS (*Departamento de Informática*

[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] We drop the region containing the free trade zone of Manaus, since it was exempt from tariffs and unaffected by the tariff changes that occurred during the 1990s trade liberalization.

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.

Our main dependent variable is computed as the log-change in the crime rate of region r between years s and s', as follows:

$$\Delta_{s,s'} \log (CR_r) = \log \left(CR_{r,s'}\right) - \log (CR_{r,s})$$

where

$$CR_{r,s} \equiv \frac{100,000 \times \text{Total Homicides}_{r,s}}{\text{Population}_{r,s}}.$$

As we focus on changes in logs, 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. Throughout the paper, we consider the crime rate per 100,000 inhabitants, as in the above expression.

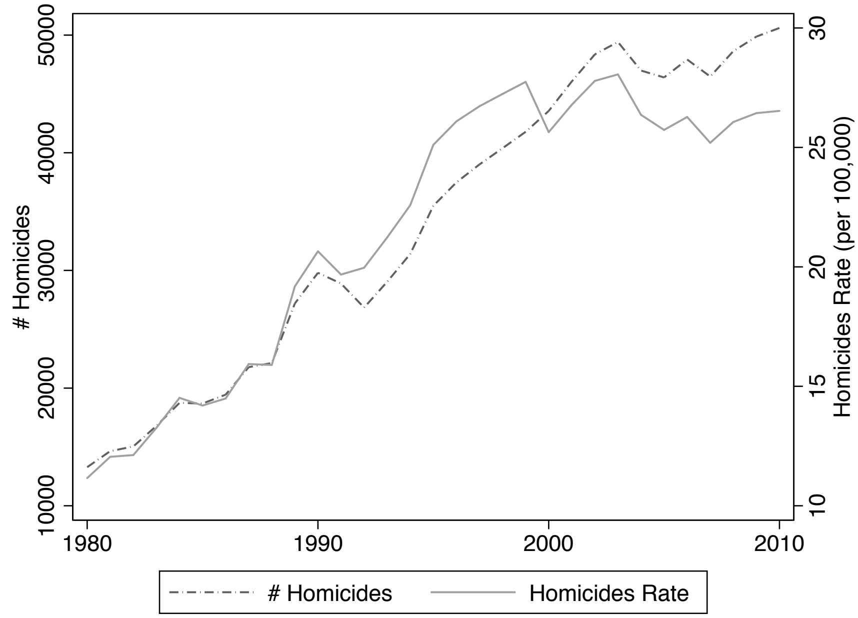
Figure 2 shows the evolution of the homicide rate, as well as of the total number of homicides, in Brazil between 1980 and 2010. As the figure shows, both variables have increased substantially over the past 30 years, with the homicide rate in 2010 being more than 2.5 times higher than in 1980, while the total number of homicides increased 5-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 the 18th place in terms of homicide rates (UNODC, 2013). The dispersion of homicide rates across micro-regions is also extremely high: the 10th and 90th percentiles of the distribution corresponded to, respectively, 2.5 and 30 in 1991, and 2.9 and 34 in 2000.

In Figure 3, Panel (a), we show how log-changes in crime rates ($\Delta_{91-00} \log (CR_r)$) are distributed across local labor markets for the period 1991–2000. Since we will be contrasting changes in the log of local crime rates to regional tariff changes (RTC_r), Figure 3 also presents the distribution of RTC_r across micro-regions (Panel (b)). It shows that there is a large degree of heterogeneity in changes in homicide rates and trade-induced shocks across regions.

One potential concern with the use of homicides to represent the overall incidence of crime is that they are relatively rare and extreme outcomes. More common types of crime and less extreme forms of violence are much more prevalent than homicides. Unfortunately, in the case of Brazil, police records are not compiled systematically in a

[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).

Figure 2: Homicide Rates and Total Number of Homicides: 1980–2010



Source: Micro data from DATASUS (*Departamento de Informática do Sistema Único de Saúde*). Homicide rates per 100,000 inhabitants.

comparable way at the municipality (or micro-region) level. Even for the very few states that do provide this type of 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.

Appendix A mitigates this concern and shows (in two different contexts) that levels and changes in local homicide rates are strongly correlated with levels and changes, respectively, in other types of crime at the local level. The Brazilian state of Minas Gerais provides municipality level information between 2000 and 2010 on occurrences recorded by the police for four types of crime: homicides, violent crimes against the person (excluding homicides), violent property crimes, and minor offenses. Minas Gerais is a good case study as it is the second most populous state in Brazil, with over 800 municipalities and 64 micro-regions, which allows us to assess how the levels and changes in these crime categories are associated with the corresponding patterns observed in the homicide data compiled by the health system. The analysis in the Appendix shows that our measure of homicides is highly correlated, both in levels and in changes, to police-recorded homicides, to property crimes, and to crimes against the person. At the level of local labor markets, homicide rates are indeed a good proxy for the overall incidence of crime. We also conduct an analogous analysis using state-level data from the United States between 1980 and 2010 (from the FBI Uniform Crime Reports) and obtain similar qualitative results. We conclude that the correlation of homicides and other crime categories across regions is not a peculiar feature of the Brazilian state of Minas Gerais. Indeed, violent property crime, burglaries, and drug crimes are usually undertaken by armed individuals, and homicides sometimes arise as unplanned outcomes of these activities. Violence is also typically thought of as a way to settle disputes among agents operating in illegal markets and among common criminals. In addition, involvement in crime may increase the use of violence in other social settings.

4.3 Labor Market Outcomes

We use four waves of the Brazilian Demographic Census covering thirty years (1980–2010). We consider two main labor market outcomes at the individual level, namely, total labor market earnings and employment status (employed or not employed), but also investigate hourly wages. We use information on individuals' age, gender and schooling to control for compositional effects in the two-step procedure described in the previous section. Further details on data treatment can be found in the Appendix C.

Table 1 shows some well-known facts about the Brazilian labor market. Even though average schooling increased steadily over time, it remained very low in 2010 (slightly below 8 years). Similarly, labor market earnings and more clearly hourly wages increased substantially in real terms. The employment rate remained stable between 1991 and 2000 and increased by 6 percentage points between 2000 and 2010, reflecting the expansion experienced by the Brazilian economy in the 2000s. Regarding the distribution of labor market outcomes across micro-regions, Table 2 reveals substantial inequality. Hourly wages and earnings show great dispersion across micro-regions, with large changes over time. There are also sizable disparities in employment rates, with the difference between the 90th and 10th percentiles changing from 11 percentage points in 1991 to 19 in 2010. As a consequence, there is also a large degree of heterogeneity in our measure of local labor market conditions – i.e. expected earnings – across micro-regions.

Table 1: Labor Market Descriptive Statistics

	1991		2000		2010	
	Mean	Obs.	Mean	Obs.	Mean	Obs.
Years of Schooling	5.39 (4.36)	8,977,535	6.54 (4.37)	11,365,956	7.85 (4.58)	12,633,332
Age	35.22 (12.47)	8,983,092	35.86 (12.57)	11,475,673	37.25 (12.74)	12,633,332
Female	0.51 (0.5)	8,983,092	0.51 (0.5)	11,475,673	0.51 (0.5)	12,633,332
Real Hourly Wage (2010 R$)	6.16 (15.08)	5,303,585	7.27 (24.98)	6,486,763	9.19 (58.84)	7,744,805
Real Monthly Earnings (2010 R$)	1,118.21 (2,384.95)	5,303,585	1,309.1 (4,342.57)	6,486,763	1,359.41 (3,432.94)	7,744,805
Employment Rate	0.62 (0.48)	8,983,092	0.61 (0.49)	11,475,673	0.67 (0.47)	12,633,332

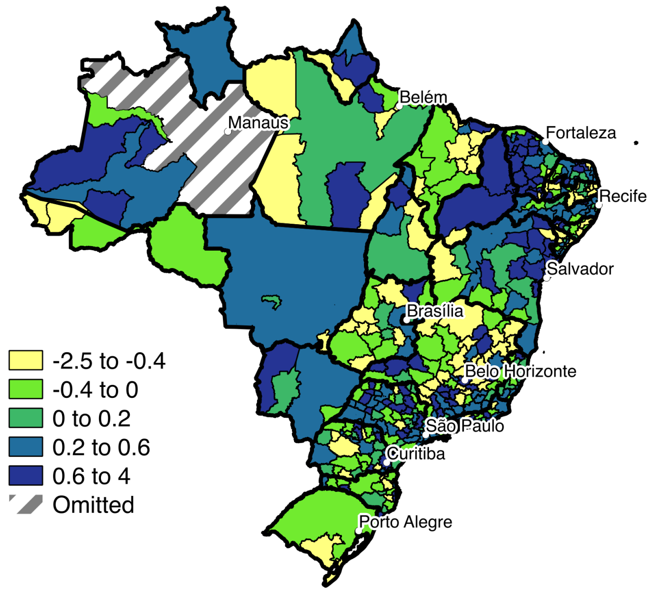
Source: Decennial Census. Standard deviations in parentheses. Average exchange rate in 2010: 1 US$ = 1.76 R$ (International Financial Statistics).

Table 2: Distribution of Labor Market Outcomes Across Micro-Regions

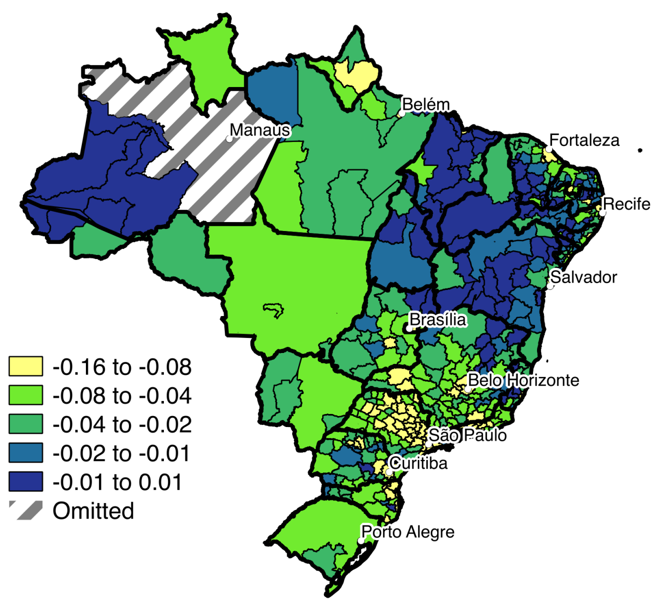
	10th Perc.	50th Perc.	90th Perc.
		1991	
Real Hourly Wage (2010 R$)	2.1	3.9	6.6
Real Monthly Earnings (2010 R$)	364.7	717.1	1218.4
Employment Rate	0.55	0.60	0.66
Expected Earnings (2010 R$)	204.8	436.2	785.5
		2000	
Real Hourly Wage (2010 R$)	2.9	4.8	7.6
Real Monthly Earnings (2010 R$)	473.0	906.1	1395.3
Employment Rate	0.53	0.60	0.66
Expected Earnings (2010 R$)	266.2	549.0	910.9
		2010	
Real Hourly Wage (2010 R$)	4.1	6.2	9.1
Real Monthly Earnings (2010 R$)	585.8	1009.1	1411.0
Employment Rate	0.54	0.65	0.73
Expected Earnings (2010 R$)	320.7	649.5	1005.4

Source: Decennial Census. Expected Earnings in region r equals the average real monthly earnings times the employment rate in region r. Average exchange rate in 2010: $1US\$ = 1.76R\$$ (International Financial Statistics).

Figure 3: Log-Changes in Local Crime Rates and Regional Tariff Changes



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



(b) Distribution of Regional Tariff Changes, RTC_r

Source: Crime rates correspond to homicide rates per 100,000 inhabitants computed from DATASUS (*Departamento de Informática do Sistema Único de Saúde*). Regional tariff changes, RTC_r, computed according to the formulae in Section 3.

5 Results

5.1 Trade Liberalization and Local Crime Rates

Table 3 presents the results from our reduced-form specification analyzing the medium-term effect of trade-induced local shocks on crime. The table shows the $\widehat{\kappa}_{91-00}$ coefficient from equation (6), which captures the impact of the regional tariff changes, RTC_r, on changes in the log of local homicide rates between 1991 and 2000. We cluster standard errors at the meso-region level to account for potential spatial correlation in outcomes across neighboring regions.[16] 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. The table shows that there is a significant negative relationship between changes in homicide rates and regional tariff changes, indicating that labor markets that experienced the largest exposure to foreign competition (more negative RTC_r) also experienced relative increases in crime rates. In Columns 2 and 3, we weight the same specification from Column 1 by, respectively, the inverse of the variance of the dependent variable and the average population between 1991 and 2000.[17] The choice of weights has little influence on our point estimates, so we follow most of the literature on crime and health and use population weights in the remainder of our specifications.[18]

In Column 4, we add state fixed effects to the specification from Column 3 (27 fixed effects, corresponding to 26 states plus the federal district), to account for state-level changes potentially driven by state-specific policies.[19] The magnitude of the coefficient increases by more than 50 percent and remains strongly significant. This indicates that some of the states that faced more exposure to foreign competition following the reform also displayed other varying characteristics that contributed to reduce crime, initially biasing the coefficient toward zero.

In Columns 5 and 6 we estimate the same specification from Column 4, but we also control for log-changes in local homicide rates between 1980 and 1991. This specification

[16] Meso-regions are groupings of micro-regions and are defined by the Brazilian Statistical Agency IBGE. Note that we also need to slightly aggregate the IBGE meso-regions to make them consistent over the 1980-2010 period.

[17] Note that, although we have the universe of homicides within a given region, the population of that region must be estimated using the Census. We compute the variance of region-specific population in 1991 and 2000 and apply the delta-method in order to obtain the variance of our left-hand-side variable.

[18] In the health literature, the realized mortality rate from a certain condition is often used an estimator for the underlying mortality probability. The variance of this estimator is inversely proportional to population size (see, for example Deschenes and Moretti (2009) and Burgess et al. (2014)). Our results remain virtually identical if we adopt any of the other two alternatives.

[19] By constitutional mandate, the main police forces and public security policies in Brazil are decentralized to state governments. Therefore, controlling for state fixed effects controls for these unobserved policies, which are likely to be correlated with local economic conditions.

Table 3: 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)	OLS (5)	2SLS (6)
RTC_r	-1.976**	-2.588***	-2.444***	-3.838***	-3.769***	-3.853***
	(0.822)	(0.779)	(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	No	Yes	Yes	Yes
First Stage F-Stat						54.19
Observations	411	411	411	411	411	411
R-squared	0.013	0.060	0.052	0.346	0.406	–

Notes: DATASUS data. Standard errors (in parentheses) adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. Columns: (1) Observations are not weighted; (2) Observations are weighted by the inverse of the variance of the dependent variable; (3) Observations are weighted by population; (4) Adds state fixed effects to (3); (5) Adds pre-trends to (4); (6) Two-Stage Least Squares, with an instrument for $\Delta_{80-91}\log(CR_r)$ (see text).
Significant at the *** 1 percent, ** 5 percent, * 10 percent level.

addresses concerns about pre-existing trends in region-specific crime rates that could be correlated with (future) trade-induced local shocks. In Column 5 we include this variable as an additional control and estimate the equation by OLS. A potential problem with this procedure is that the 1991 log of 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 6, where we instrument pre-existing trends $\Delta_{80-91}\log(CR_r)$ with $\log\left(\frac{\text{Total Homicides}_{r,1990}}{\text{Total Homicides}_{r,1980}}\right)$. In either case, there is very little change in the coefficient of interest, indicating that the estimated relationship between changes in crime rates and regional tariff changes is not driven by pre-existing trends. The effect of regional tariff changes on changes in crime rates is sizable. Moving a region from the 90th percentile to the 10th percentile of regional tariff changes means a change in RTC_r equivalent to -0.1 log point. Column 4 of Table 3 predicts that this movement would be accompanied by an increase in crime rates of 0.38 log point, or 46 percent.

Table 4 reproduces the same exercises from Table 3, but focuses on the long-term effect (1991-2010) of regional tariff changes. Differently from the results in Table 3, Columns 1 to 3 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 4 to 6), the coefficients become negative, smaller in magnitude than the medium term coefficients, and not statistically significant. As before, this changing pattern in the 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 and over the 1991-2010 interval.

We conclude that trade-induced local shocks had a strong effect on local crime rates, but that the effect was temporary. Regions facing more negative shocks go through relative increases in crime rates in the medium term (1991 to 2000). However, this effect appears to vanish in the long term (1991 to 2010). It is important to emphasize that the estimation of $\kappa_{s,s'}$ in equation (6) does not deliver absolute effects of the trade liberalization on crime. This is a well known limitation of difference-in-differences estimates when the treatment assignment is likely to generate important general equilibrium effects that spills over to other units – which is certainly the case in this large scale trade reform. These general equilibrium effects, common to all units, will be absorbed in the intercept $\xi_{s,s'}$. Therefore, we cannot make statements about the total effect of trade reform on the national level of crime.

Table 4: 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)	OLS (5)	2SLS (6)
RTC_r	5.293***	6.976**	6.668**	-1.324	-1.198	-1.340
	(1.494)	(2.857)	(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	No	Yes	Yes	Yes
First Stage F-Stat						52.2
Observations	411	411	411	411	411	411
R-squared	0.066	0.151	0.133	0.642	0.702	–

Notes: DATASUS data. Standard errors (in parentheses) adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. Columns: (1) Observations are not weighted; (2) Observations are weighted by the inverse of the variance of the dependent variable; (3) Observations are weighted by population; (4) Adds state fixed effects to (3); (5) Adds pre-trends to (4); (6) Two-Stage Least Squares, with an instrument for $\Delta_{80-91}\log(CR_r)$ (see text).
Significant at the *** 1 percent, ** 5 percent, * 10 percent level.

5.2 Placebo Exercise and Dynamic Effects

One important concern in difference-in-differences estimates is that the treatment assignment may be correlated with pre-existing trends in the outcome of interest. For this reason, Tables 3 and 4 included pre-existing trends in log crime rates as an additional control to rule out that the estimated effects were driven by a (coincidental) correlation between pre-existing trends and (future) regional tariff changes. The results showed that pre-trends had no effect on our estimates of interest, indicating that pre-existing trends are not likely to be a challenge to our identification strategy. Here, we go one step further and analyze the timing of the response of crime to the regional tariff changes.

First, we conduct a placebo exercise where we project changes in the log of local crime rates between 1980 and 1991 onto future regional tariff changes (RTC_r). If pre-existing trends are indeed a concern, this regression would yield statistically significant results. We replicate the specifications from the first four columns in Table 3. Results are presented in Table 5. All coefficients are very small in magnitude, with opposite signs to those from Table 3, and none is statistically significant. Indeed, pre-existing trends do not seem to be a challenge to the identification strategy.

Table 5: 1980-1991 Log-Changes in Crime Rates and Regional Tariff Changes – Placebo Tests

Dep. Var.: $\Delta_{80-91} \log(CR_r)$	(1)	(2)	(3)	(4)
RTC_r	0.727	0.257	0.200	0.162
	(1.096)	(1.443)	(1.409)	(0.893)
State Fixed Effects	No	No	No	Yes
Observations	411	411	411	411
R-squared	0.002	0.001	0.000	0.426

Notes: DATASUS data. Standard errors (in parentheses) adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. Columns: (1) Observations are not weighted; (2) Observations are weighted by the inverse of the variance of the dependent variable; (3) Observations are weighted by population; (4) Adds state fixed effects to (3).
Significant at the *** 1 percent, ** 5 percent, * 10 percent level.

We can also explore the dynamics of the response of crime to the trade-induced shocks. Not only this exercise lends additional credibility to the results, but it also sheds light on the nature of the effects of the trade liberalization on crime. Since we have yearly homicide data for the period between 1980 and 2010, we can expand the placebo exercise for the entire pre-reform period (1980–1991), as well as trace out the cumulative effects of the liberalization episode during the post-reform period (1992–2010).[20]

We conduct a dynamic placebo exercise by fixing $s = 1980$ in equation (6) and estimating $\kappa_{s,s'}$ for $s' = 1981, ..., 1991$. We also estimate the dynamic effects of regional tariff changes on local crime rates fixing $s = 1991$ and estimating $\kappa_{s,s'}$ for $s' = 1992, ..., 2010$. All of the specifications control for state fixed effects and observations are weighted by population (as in Column 4 from Tables 3 and 4).

[20] For each year, we estimate the population of a micro-region with a linear interpolation using the last Census wave before that year and the first Census wave after it.

Dynamic placebo effects ($\widehat{\kappa}_{1980,s'}$, for $s' = 1981, ..., 1991$) and dynamic effects ($\widehat{\kappa}_{1991,s'}$, for $s' = 1992, ..., 2010$) are portrayed in Figure 4, with their respective confidence intervals. The red line with triangular markers refers to the placebo exercise and the blue line with circular markers refers to the dynamic effects. The figure indicates that the none of the estimated placebo effects are statistically significant. On the other hand, $\widehat{\kappa}_{1991,s'}$ is uniformly negative between $s' = 1992$ and $s' = 2010$. However, its magnitude gradually increases between 1992 and 1997, and starts to converge back to zero in 1998. Also, $\widehat{\kappa}_{1991,s'}$ is statistically significant only between 1995 and 2003 (with the exception of 2002).

Figure 4: Dynamic Effects of Regional Tariff Changes on Log-Changes in Local Crime Rates

Note: Coefficients fromequation (6); dynamic placebo effects given by $\widehat{\kappa}_{1980,s'}$, for $s' = 1981, ..., 1991$ (red triangular markers) and dynamic effects given by $\widehat{\kappa}_{1991,s'}$, for $s' = 1992, ..., 2010$ (blue circular markers).

Together, the results from this section indicate that the trade reform had a temporary effect on crime rates over the short and medium terms and that this effect vanished in the long term. This pattern is very reassuring, given the timing of the labor market effects of the Brazilian trade reform already documented in the literature and that we further investigate in the next section. Indeed, the dynamic response of crime to regional tariff changes mimics the medium- and long-term effects of regional tariff changes on local labor market conditions. These two pieces of evidence give strong empirical support to the argument that the trade-induced local shocks affected crime only through the labor market, which is essential to our identification strategy.

5.3 Trade Liberalization and Local Labor Markets

While sections 5.1 and 5.2 established a connection between regional tariff changes and crime, this section examines the mechanism through which this effect occurred. Specifically, we investigate the effect of the trade reform on local labor markets. Part of this analysis is similar to the first stage of our later IV estimation. Our methodology, explained in detail in Section 3, is analogous to the strategy adopted by previous papers in this literature, such as Kovak (2013) and Dix-Carneiro and Kovak (2015b).

In Table 6, we investigate the effect of regional tariff changes on total labor market earnings in Columns 1 and 2, on employment rates in Columns 3 and 4, and on our index of labor market conditions (expected labor market earnings) in Columns 5 and 6, for the 1991-2000 and the 1991-2010 periods, respectively. The table shows that labor markets experiencing more exposure to foreign competition after the liberalization episode (more negative RTC_r) also experienced permanent (relative) reductions in earnings, lasting up to 2010. The point estimates indicate that the reduction in earnings was magnified between 2000 and 2010, but this difference is not statistically significant. The effect on employment rates, on its turn, was temporary, being large and significant in 2000 and vanishing in 2010.[21]

The estimated effect of regional tariff changes on our index of labor market conditions, shown in Columns 5 and 6, is a combination of the effects on earnings and employment, given that our index is the product of these two variables.[22] We find that increased exposure to foreign competition reduced expected labor market earnings in 2000 and in 2010, but that the effect in 2000 was almost twice as large as that observed in 2010. The recovery in employment rates between 2000 and 2010 contributed to substantially reduce the impact of liberalization on expected earnings in the long term. The point estimates indicate that a change in regional tariffs of -0.1 would lead to a 12 percent reduction in expected labor market earnings in 2000 and a 6.6 percent reduction in 2010.

The stronger effect of the trade reform on the labor market in 2000 as compared to 2010 is reassuring, since it mimics the profile found in the previous section for the response of local crime to regional tariff changes. Our key identifying assumption is that regional tariff

[21] Dix-Carneiro and Kovak (2015b) show that the recovery in employment rates in harder hit regions took place mostly in the informal sector. Using longitudinal data on formal sector employment and cross-sectional data from the Census, their results suggest that trade-displaced workers in these regions went through periods of unemployment in the short and medium terms, but eventually found employment in the informal sector.

[22] Note that the coefficients on RTC_r in Columns 1 and 3 of Table 6 do not sum exactly to Column 5 because weights are different across specifications. The same observation holds for Columns 2, 4 and 6.

Table 6: Regional Tariff Changes and Evolution of Labor Market Conditions

Dep. Var.:	$\Delta \log(w_r)$		$\Delta \log(P_{e,r})$		$\Delta \log(w_r \times P_{e,r})$	
	1991-2000	1991-2010	1991-2000	1991-2010	1991-2000	1991-2010
	(1)	(2)	(3)	(4)	(5)	(6)
RTC_r	0.567***	0.668**	0.648***	0.0147	1.187***	0.659**
	(0.120)	(0.279)	(0.0713)	(0.102)	(0.134)	(0.317)
Observations	411	411	411	411	411	411
R-squared	0.701	0.683	0.495	0.635	0.704	0.687

Notes: Decennial Census data. Standard errors (in parentheses) adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. Observations are weighted by the inverse of the squared standard error of the estimated change in the dependent variable. All specifications control for state fixed effects.
Significant at the *** 1 percent, ** 5 percent, * 10 percent level.

changes affected crime rates only through their effects on local labor markets. Given that the effect of regional tariff changes on labor market outcomes is stronger in the medium than in the long term, our identification implies that the effect on local crime should also be stronger in the medium than in the long term. This mirror pattern adds credibility to the claim that the trade shock is instrumental in providing a source of identification for the causal effect of labor market conditions on crime. We further investigate this issue explicitly in the next subsection.

Note that we used labor market earnings as our measure of labor income in this section. In Appendix Table D.2, we investigate the effect of regional tariff changes on hourly wages, and use wages instead of earnings to construct our index of labor market conditions. Overall, the results are very similar. The only difference is that the point estimate of the effect of the change in regional tariffs on wages is stronger in the medium than in the long term (0.7 vs 0.4).

5.4 Changes in Local Labor Market Conditions and Crime

In Table 7, we analyze the role of labor market conditions as determinants of crime rates. We use the 1990s trade reform to create an instrument to local labor market conditions and focus on the 1991-2000 period. Before discussing the IV results, we first estimate a series of OLS specifications projecting log-changes in local crime rates onto log-changes in different local labor market variables. In Columns 1 to 4, we regress log-changes in crime rates on log-changes in, respectively, earnings, employment, earnings and employment, and our labor market index (expected labor market earnings). In the OLS specifications from the first four columns, changes in all variables are negatively correlated with changes in crime, suggesting that declines in earnings and employment rates are associated with increases in crime rates. However, standard errors are very large, leading to wide confidence intervals and non-significant results.

Table 7: Log-Changes in Crime Rates and Labor Market Conditions (1991-2000)

Dep. Var. $\Delta_{91-00}\log(CR_r)$	OLS (1)	OLS (2)	OLS (3)	OLS (4)	2SLS (5)
$\Delta_{91-00}\log(w_r)$	-0.366		-0.300		
	(0.772)		(0.728)		
$\Delta_{91-00}\log(P_{e,r})$		-0.767	-0.719		
		(0.777)	(0.694)		
$\Delta_{91-00}\log(w_r \times P_{e,r})$				-0.460	-3.278***
				(0.642)	(1.178)
First Stage F-stat					94.9
Observations	411	411	411	411	411
R-squared	0.276	0.279	0.280	0.279	–

Notes: DATASUS and Decennial Census data. Standard errors (in parentheses) adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. Observations are weighted by population. Two-stage least square specification uses RTC_r as instrument. All specifications control for state fixed effects.
Significant at the *** 1 percent, ** 5 percent, * 10 percent level.

In Column 5, we present the results from our IV strategy. The first stage is similar to Column 5 in Table 6.[23] As Table 7 indicates, our first stage is very strong, with an F-statistic of around 95. The second stage result shows that improvements in labor market conditions (as reflected in increases in expected earnings) lead to reductions in crime rates, with an elasticity of -3.3. This effect is economically large. Moving a region from the 90th percentile to the 10th percentile of the distribution of regional tariff changes, leads to a change in RTC_r of -0.1. According to our first stage estimate from Table D.3 in the Appendix, this would lead to a reduction in the log of expected earnings of 0.12. Our second stage, from Column 5 in Table 7, indicates that this would be associated with an increase of 0.39 in the log of crime rates (an increase of 48 percent), an effect almost identical to the total effect from the same regional tariff change estimated from our reduced-form specification in Table 3. This further strengthens the argument that the response of crime to the reduction in tariffs worked exclusively through the labor

[23] Column 5 in Table 6 is not exactly our first stage simply because of different weights. In the previous section, when we analyze the local labor market effects of the trade shock, we weight observations by the variance of the dependent variable. In our IV approach, since we weight the homicide regression by population, we also weight the respective first stage by population. Nevertheless, results from our actual first stage, presented in Table D.3, are very close to those from Table 6.

market. To put this effect in perspective, consider that the difference between the 10th and 90th percentiles of the 1991 distribution of crime rates was of the order of 12 times (respectively, 2.5 and 30 per 100,000 inhabitants).

The change in point estimates between Columns 4 and 5 indicates that the OLS coefficient on labor market conditions is biased towards zero. Similar patterns have been documented before in the literature on labor markets and crime (e.g. Gould et al., 2002; Fougère et al., 2009). This would be expected if, for example, more dynamic areas with better labor market prospects attracted more young and unskilled immigrants, leading to increases in crime. Such variation would weaken the correlation between labor market conditions and crime and bias the estimated coefficient toward positive values.

Table 6 showed that the effect of regional tariff changes on local labor market conditions was dampened down substantially in the long term (1991-2010). Our first stage in this case, which would be similar to Column 6 in Table 6, is consequently much weaker, with an F-statistic of the instrument equal to only 2. Therefore, the long-term second stage does not carry much meaning. Still, in the Appendix (Tables D.7 and D.8), we reproduce all the results from Table 7 for the 1991-2010 period (both with earnings and hourly wages as measures of labor market income), with non-significant results.

Finally, much of the literature on labor markets and crime focuses on young or unskilled workers, given that these are the groups most prone to engage in crime. If we reproduce our entire estimation strategy restricting ourselves to young or unskilled workers, results remain very similar to those reported here. Appendix D contains all of our results restricting the sample to, and reconstructing our trade shocks based on, these groups of workers. The evidence suggests that the local impact of the trade-induced shocks was roughly homogeneous across different groups in the population (see Dix-Carneiro and Kovak, 2015a), which helps to explain this result.

5.5 Additional Controls

In Tables 8 and 9, we estimate our main reduced-form and IV specifications (Column 4 in Table 3 and Column 5 in Table 7, respectively) controlling for potential determinants of crime that may be correlated with the regional tariff changes. These controls correspond to changes in the share of unskilled workers (eighth grade or less) in the labor force, the share of population living in urban areas, the share of youth (from 18 to 30 years old) in the population, and inequality (variance of the log of household income per capita). The literature on labor markets and crime previously cited discusses the role that unskilled and young individuals play in crime. The age composition of the population is also a classic topic in the criminology literature, as discussed by Levitt (1999). Examples of assessments of urbanization and inequality as determinants of crime can be found in

Glaeser et al. (1996), Fajnzylber et al. (2002) and Bourguignon et al. (2003).

The variables are calculated directly from Census data at the micro-region level. Virtually all of them could be endogenous to the trade reform. Therefore, if regional tariff changes affect other dimensions of local economies primarily through their initial effects on the labor market, these variables do not strictly belong to the right-hand side of our estimating equation. Keeping this limitation in mind, we still think that the tables are informative as to whether the effects detected before indeed seem to be due to labor market conditions per se, or to other subsequent economic changes driven by the initial changes in labor market conditions.

Table 8: Regional Tariff Changes and Log-Changes in Crime Rates (1991–2000): Additional Controls

Dep. Var. $\Delta_{91-00} \log (CR_r)$	(1)	(2)	(3)	(4)	(5)	(6)
RTC_r	-3.838***	-3.907***	-3.072**	-4.095***	-3.794***	-3.166**
	(1.426)	(1.433)	(1.263)	(1.436)	(1.389)	(1.315)
$\Delta_{91-00} \log (\text{Share Unskilled}_r)$		0.157				-0.337
		(2.142)				(2.032)
$\Delta_{91-00} \log (\text{Share Urban}_r)$			-0.848***			-0.785***
			(0.262)			(0.265)
$\Delta_{91-00} \log (\text{Share Young}_r)$				1.514*		1.894**
				(0.842)		(0.790)
$\Delta_{91-00} \log (\text{Inequality}_r)$					-0.186	-0.575
					(0.391)	(0.366)
Observations	411	411	411	411	411	411
R-squared	0.346	0.346	0.358	0.354	0.347	0.369

Notes: Share Unskilled$_r$ is the share of region r individuals who are 18 or older and who have eighth grade complete or less; Share Urban$_r$ is the share of region r individuals living in urban areas; Share Young$_r$ is the share of region r individuals who are 18 to 30 years old; and $Inequality_r$ is the variance of log-household income per person. Standard errors (in parentheses) adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. Observations are weighted by population and all specifications control for state fixed effects.

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

In both tables, we present results including each control variable separately, one at a time, and all variables simultaneously. For purposes of comparison, the tables show our benchmark results in the first column. Both tables show that the reduced-form and IV results remain significant, typically with point estimates of similar magnitude, irrespectively of the set of controls included. It is worth mentioning that OLS regressions analogous to Column 4 in Table 7 including these additional controls still lead to non-significant coefficients (not shown, but available upon request) .

Table 9: Labor Market Conditions and Log-Changes in Crime Rates (1991-2000): Additional Controls

Dep. Var. $\Delta_{91-00}\log(CR_r)$	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta_{91-00}\log(w_r \times P_{e,r})$	-3.278***	-5.175**	-2.606**	-3.357***	-3.024***	-3.730**
	(1.178)	(2.111)	(1.027)	(1.167)	(1.035)	(1.655)
$\Delta_{91-00}\log(\text{Share Unskilled}_r)$		5.003				3.010
		(4.068)				(3.300)
$\Delta_{91-00}\log(\text{Share Urban}_r)$			-0.870***			-0.804***
			(0.280)			(0.291)
$\Delta_{91-00}\log(\text{Share Young}_r)$				0.543		2.236**
				(1.022)		(0.978)
$\Delta_{91-00}\log(\text{Inequality}_r)$					-1.273**	-1.966**
					(0.623)	(0.779)
First Stage F-stat	94.9	17.1	117.1	109.9	123.1	25.8
Observations	411	411	411	411	411	411

Notes: Share Unskilled$_r$ is the share of region r individuals who are 18 or older and who have eighth grade complete or less; Share Urban$_r$ is the share of region r individuals living in urban areas; Share Young$_r$ is the share of region r individuals who are 18 to 30 years old; and $Inequality_r$ is the variance of log-household income per person. Standard errors (in parentheses) adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. Observations are weighted by population. All specifications control for state fixed effects and instrument $\Delta_{91-00}\log(w_r \times P_{e,r})$ with RTC_r (two-stage least squares). Significant at the *** 1 percent, ** 5 percent, and * 10 percent level.

In the most complete specification from Tables 8 and 9, changes in the share of youth in the population are positively and significantly related to changes in crime rates, as expected, while changes in the share of unskilled workers appear as statistically non-significant. Changes in the share of the population living in urban areas, on the other hand, appear with a negative and statistically significant sign in both tables, while changes in inequality appear with a negative and statistically significant sign in the IV specification. These results go against the expected effect of urbanization and inequality on crime. Since we do not have instruments for these two variables, and they are potentially endogenous to the labor market effects of the trade reform, we do not attach much meaning to these point estimates. In any case, the results from Tables 8 and 9 suggest that the effects of the trade reform on crime estimated before are indeed working directly through the labor market.

5.6 Discussion

Both our reduced-form and IV results show that regions facing more negative shocks induced by trade liberalization experienced deteriorating labor market outcomes relative to the national mean which led to relative increases in crime. Our results trace out the responses of crime through time and map them clearly onto concurrent labor market changes.

In line with our Table 6 results, Dix-Carneiro and Kovak (2015b) documented that regions that were harder hit by the trade liberalization faced increasingly declining nominal earnings over time. However, they find no evidence that inter-regional migration responded to these trade-induced local shocks. The absence of substantial effects on migration in this and similar contexts from other developing countries has led some to question whether the documented labor market responses indeed represented real welfare losses (see, for example, Monte, 2015). According to this view, rather than reflecting mobility barriers, the absence of migration could be interpreted as indicating that prices of non-tradables (e.g. real estate) were also reduced in equilibrium so that regional real incomes were unaffected by the tariff changes. This would mean that regions experiencing relatively larger exposure to foreign competition and worse labor market performance would also have experienced relative reductions in the prices of non-tradables that would have compensated for the lower earnings. As a consequence, migration decisions would be unaffected by the relative change in tariffs. Dix-Carneiro and Kovak (2015b) argue that, even though they cannot observe the response of local prices to the regional change in tariffs, welfare must have been differentially affected by trade liberalization, since many regional real outcomes – such as employment rates, informality, and the duration of nonformal spells – did respond to the local shocks. We add local crime to the list of real outcomes that were affected by the trade liberalization, giving support to the argument that the costs and benefits of the reform were unevenly distributed across the country. This evidence speaks directly to the ongoing debate in the literature on adjustment costs from trade reforms (Dix-Carneiro, 2014; Autor et al., 2014; Utar, 2015).

It is also worthwhile to stress another important aspect of our approach. Most of the literature on labor markets and crime that uses some identification strategy investigates the relationship between either unemployment rates or earnings, separately, and crime (Grogger, 1998; Lin, 2008; Fougère et al., 2009). Conceptually, it is difficult to think of labor market shocks that would affect one of these dimensions but not the other. This is precisely what motivated our theoretical model, which framed individual choices in terms of expected labor market earnings. In our data, as made clear in Table 6, regional tariff changes affected both local earnings and employment. In fact, if we estimate our IV specification separately for earnings and employment rates, both results come out as negative and statistically significant (Appendix Table D.4 presents these results for the interested reader). However, both of these labor market variables can in principle affect crime, so these two specifications would not satisfy the exclusion restriction required by an IV estimator. This means that most of the specifications estimated in the previous literature are likely to be misspecified, since they consider the effects of wages and unemployment separately.[24] For this reason, we believe that our index of labor market conditions is a more theoretically consistent measure to assess the response of crime rates to changes in labor market conditions.

The large response of homicide rates that we estimate – in contrast to close to zero coefficients for violent crime found in the previous literature – can have a couple of explanations. Our natural experiment represents a cleaner and stronger shock to labor market conditions than the instruments that have been used before. 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 allows us to estimate more precisely the response of crime to the labor market shock, while the second provides a setting where the response of crime to labor market conditions is likely to be stronger.

6 Final Remarks

There has recently been increased interest in the adjustment costs that follow trade shocks. Analyses of these costs have focused on the barriers to labor and capital reallocation across industries and regions and on the inefficiencies determined by the lack of complete arbitrage across labor markets. In this paper, we show that the limited labor mobility in response to trade shocks generates additional social costs that have been overlooked in the literature. We document that regions that were harder hit by the trade liberalization experienced increases in crime rates relative to the national average. These relative increases in crime were large and followed closely the timing of the local labor market responses to regional tariff changes. In the long term, as the local labor market responses progressively dissipated, so were the crime rate responses. This pattern highlights that losers in trade liberalization episodes face real adjustment costs that may well generate negative externalities to local economies.

By focusing on a developing country with high levels of violence, we document an economically large response of homicide rates to local labor market conditions, while the previous literature only detected significant effects on non-violent crimes. Our results show a much stronger link between labor markets and crime than that document by this literature. This suggests that the criminogenic effect of deteriorations in labor market conditions are likely to be more extreme and policy relevant in developing countries with poor labor market conditions and high levels of violence.

The evidence assembled in this paper constitutes an important input to the optimal

[24] Gould et al. (2002) is one of the few exceptions.

design of public policies and to decisions on the allocation of resources to public security. In addition, it highlights the relevance of educational and counter-cyclical policies, by improving labor market prospects in the long and short term respectively, as instruments to fight crime. Given the externalities associated with crime, and the link between the labor market and crime discussed here, the costs of economic downturns – or of low employability in general – go beyond those faced by the individuals who directly suffer from the worsened labor market opportunities. In such circumstances, there is a potential welfare enhancing role for government interventions that are successful in improving labor market outcomes.

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A Homicide Rates as a Proxy for Overall Criminal Activity

We investigate to what extent local homicide rates constitute a good proxy for local overall criminal activity. First, we examine data from Minas Gerais, one of the very few Brazilian states publishing crime data at the municipality level. Next, we corroborate this analysis using United States state-level data from the Uniform Crime Reporting Statistics and show that local homicide rates are a good barometer for local overall criminal activity in other contexts as well.

A.1 Evidence from a Large Brazilian State: Minas Gerais

Minas Gerais is the second most populous state in Brazil, with over 800 municipalities and 64 micro-regions. We use information on three types of crimes that are consistently recorded by the police of Minas Gerais and put together at the municipality by Fundação João Pinheiro: homicides, violent crimes against the person (excluding homicides), and violent property crimes. Apart from homicides, the other types of crime are defined as follows. Violent crimes against the person include assaults, attempted homicides, and rapes. Violent property crimes include robberies and armed robberies. We use these data to construct crime rates at the micro-region level within Minas Gerais between 2000 and 2010.

We start examining how the rates of different types of crime recorded by the police correlate with the homicide rates obtained from the health system data (DATASUS) used in our empirical analysis. Table A.1 shows the results in log-levels while Table A.2 shows correlations for log-changes. Homicide rates measured by the police and the health system are highly correlated, with a strongly significant correlation that ranges from 0.89 to 0.78 (moving from yearly to decennial data). Both measures of homicides are also strongly and significantly correlated with crimes against the person and property crimes. Except for the 10-year correlation between changes in homicides measured by the health system and crimes against the person in Panel C of Table A.2, all correlations remain large and strongly significant across time windows. The correlations in Panel C of Table A.2 should be interpreted with caution given the small number of observations used to generate them.

Table A.3 relates homicide rates (from the health system) to the rates of crimes against the person, property crime rates and homicide rates measured by the police. These regressions control for micro-region and year fixed effects, so we focus on how changes in our measure of criminal activity, relative to aggregate crime trends, relate to changes in other measures of crime within regions. The first three columns show results in line with those from Tables A.1 and A.2. Even after we account for micro-region fixed effects and common trends in crime, homicide rates measured by the health system are strongly correlated with homicides recorded by the police, crimes against the person, and property crimes. Moreover, these correlations are stronger when we restrict attention to longer time windows. Columns 4 to 7 include different combinations of crime rates recorded by the police of Minas Gerais in the right hand side. In general, the significant correlations from the first four columns survive the simultaneous inclusion of more than one variable at the same time. It is interesting to note that property crimes are significantly correlated with homicide rates from the health system, with a large coefficient, even after we control for homicide rates from the police records. This indicates that homicides from the health system are probably more correlated with other types of crime than homicides from police records.

Table A.1: Correlation Between Homicide Rates And Other Crime Measures: Micro-Regions of Minas Gerais, 2000–2010

Panel A: Yearly data

	$\log(CR_r)$	$\log(HomPol_r)$	$\log(Person_r)$	$\log(Property_r)$
$\log(CR_r)$	1.000			
$\log(HomPol_r)$	0.886***	1.000		
$\log(Person_r)$	0.665***	0.763***	1.000	
$\log(Property_r)$	0.744***	0.643***	0.604***	1.000
Observations			704	

Panel B: 5-year intervals (2000, 2005 and 2010)

	$\log(CR_r)$	$\log(HomPol_r)$	$\log(Person_r)$	$\log(Property_r)$
$\log(CR_r)$	1.000			
$\log(HomPol_r)$	0.836***	1.000		
$\log(Person_r)$	0.607***	0.750***	1.000	
$\log(Property_r)$	0.714***	0.629***	0.646***	1.000
Observations			192	

Panel C: 10-year intervals (2000 and 2010)

	$\log(CR_r)$	$\log(HomPol_r)$	$\log(Person_r)$	$\log(Property_r)$
$\log(CR_r)$	1.000			
$\log(HomPol_r)$	0.783***	1.000		
$\log(Person_r)$	0.507***	0.752***	1.000	
$\log(Property_r)$	0.706***	0.632***	0.617***	1.000
Observations			128	

Notes: Data from Fundação João Pinheiro. 64 micro-regions in the State of Minas Gerais (data on crimes against the person is missing for one micro-region in 2009). Observations are weighted by region-specific population. CR_r is the homicide rate measured by the health system (DATASUS), $HomPol_r$ is the homicide rate measured by the police, $Person_r$ is the rate of crimes against the person, and $Property_r$ is the rate of property crimes.
Significant at the *** 1 percent, ** 5 percent, and * 10 percent level.

In conclusion, Tables A.1, A.2 and A.3 indicate that local homicide rates measured by the health system (DATASUS) are indeed systematically correlated with local overall crime rates recorded by the police.

A.2 Evidence from the United States

We corroborate that homicide rates provide a good proxy for overall criminal activity in other contexts using United States state-level data from the Uniform Crime Reporting Statistics. The Uniform Crime Reports (UCR) are the official data source on crime in

Table A.2: Correlation Between Log-Changes in Homicide Rates and Other Crime Measures: Micro-Regions of Minas Gerais, 2000–2010

	Panel A: Yearly data			
	$\Delta_1 \log(CR_r)$	$\Delta_1 \log(HomPol_r)$	$\Delta_1 \log(Person_r)$	$\Delta_1 \log(Property_r)$
$\Delta_1 \log(CR_r)$	1.000			
$\Delta_1 \log(HomPol_r)$	0.591***	1.000		
$\Delta_1 \log(Person_r)$	0.149***	0.123**	1.000	
$\Delta_1 \log(Property_r)$	0.244***	0.170***	0.407***	1.000
Observations		640		
	Panel B: 5-year intervals (2000, 2005 and 2010)			
	$\Delta_5 \log(CR_r)$	$\Delta_5 \log(HomPol_r)$	$\Delta_5 \log(Person_r)$	$\Delta_5 \log(Property_r)$
$\Delta_5 \log(CR_r)$	1.000			
$\Delta_5 \log(HomPol_r)$	0.641***	1.000		
$\Delta_5 \log(Person_r)$	0.537***	0.438***	1.000	
$\Delta_5 \log(Property_r)$	0.524***	0.407***	0.827***	1.000
Observations		128		
	Panel C: 10-year intervals (2000 and 2010)			
	$\Delta_{10} \log(CR_r)$	$\Delta_{10} \log(HomPol_r)$	$\Delta_{10} \log(Person_r)$	$\Delta_{10} \log(Property_r)$
$\Delta_{10} \log(CR_r)$	1.000			
$\Delta_{10} \log(HomPol_r)$	0.342**	1.000		
$\Delta_{10} \log(Person_r)$	0.0799	0.264*	1.000	
$\Delta_{10} \log(Property_r)$	0.464***	0.0438	0.229	1.000
Observations		64		

Notes: Data from Fundação João Pinheiro. 64 micro-regions in the State of Minas Gerais (data on crimes against the person is missing for one micro-region in 2009). Observations are weighted by region-specific population. CR_r is the homicide rate measured by the health system (DATASUS), $HomPol_r$ is the homicide rate measured by the police, $Person_r$ is the rate of crimes against the person, and $Property_r$ is the rate of property crimes. Δ_s stands for $\Delta_{t,t+s}$.
Significant at the *** 1 percent, ** 5 percent, and * 10 percent level.

the United States, and are published by the Federal Bureau of Investigation (FBI).[25] UCR is a nationwide, cooperative statistical effort of nearly 18,000 city, university and college, county, state, tribal, and federal law enforcement agencies voluntarily reporting data on crimes brought to their attention. Crime statistics are compiled from UCR data and published annually by the FBI in the Crime in the United States series. We restrict our sample to the 1980–2010 period in order to have the same time horizon as our main analysis. We consider two broad categories defined by UCR: violent crimes and property

[25] Available at *http://www.ucrdatatool.gov/Search/Crime/State/StatebyState.cfm*. Last access in August 10th, 2015.

crimes. Violent crimes include murder, forcible rape, robbery, and aggravated assault. Property crimes include burglary, larceny-theft, and motor vehicle theft. We also use a third category, "total crimes," where we sum the occurrences of violent and property crimes. In order to avoid mechanical correlation between homicide rates and violent and total crimes, we exclude murders of the count of violent and total crimes.

Tables A.4 and A.5 show that levels and log-changes in homicides are strongly correlated with levels and log-changes in property crime and violent crime. Similarly to the results for Minas Gerais, these correlations become stronger when using longer time windows.

Table A.6 relates homicide rates to the rates of property crime and violent crime. These regressions control for state and year fixed effects, so we focus on how changes in our measure of criminal activity, relative to aggregate trends, relate to changes in other measures of crime within states. When we regress homicide rates on other types of crime separately, all conditional correlations are strong regardless of the time window under consideration. However, when we condition on total violent crime, homicides are no longer correlated with property crime when one considers longer time windows. Nevertheless, the analysis in this subsection and the previous one indicate that local homicide rates are a good proxy for local overall criminal activity. Moreover, this is not a finding specific to the Brazilian data, as we document the same patterns in the United States and in the Brazilian state of Minas Gerais.

Table A.3: Homicide Rates and Other Crime Rates: Micro-Regions of Minas Gerais, 2000–2010

Panel A: Yearly Data

Dep. Var.: $\log(CR_r)$	(1)	(2)	(3)	(4)	(5)
$\log(Person_r)$	0.375***			0.175*	0.0960
	(0.0976)			(0.0929)]	(0.0730)
$\log(Property_r)$		0.396***		0.381***	0.318***
		(0.102)		(0.0950)	(0.0842)
$\log(HomPol_r)$			0.738***		0.677***
			(0.0555)		(0.0564)
Observations	703	704	704	703	703
R^2 Within	0.380	0.412	0.532	0.428	0.582
R^2 Between	0.575	0.207	0.728	0.374	0.800

Panel B: 5-year intervals (2000, 2005 and 2010)

Dep. Var.: $\log(CR_r)$	(1)	(2)	(3)	(4)	(5)
$\log(Person_r)$	0.415***			0.218	0.0600
	(0.151)			(0.173)	(0.166)
$\log(Property_r)$		0.552***		0.501***	0.506***
		(0.137)		(0.152)	(0.152)
$\log(HomPol_r)$			0.781***		0.749***
			(0.121)		(0.103)
Observations	192	192	192	192	192
R^2 Within	0.422	0.471	0.530	0.478	0.597
R^2 Between	0.561	0.229	0.629	0.386	0.710

Panel C: 10-year intervals (2000 and 2010)

Dep. Var.: $\log(CR_r)$	(1)	(2)	(3)	(4)	(5)
$\log(Person_r)$	0.157			-0.0691	-0.261
	(0.261)			(0.251)	(0.221)
$\log(Property_r)$		0.662***		0.673***	0.684***
		(0.153)		(0.169)	(0.163)
$\log(HomPol_r)$			0.756***		0.786***
			(0.225)		(0.234)
Observations	128	128	128	128	128
R^2 Within	0.446	0.562	0.506	0.563	0.627
R^2 Between	0.430	0.219	0.433	0.187	0.458

Notes: Data from Fundação João Pinheiro. 64 micro-regions in the State of Minas Gerais (data on crimes against the person is missing for one micro-region in 2009). Robust standard errors in parentheses (clustered at the micro-region levels). All regressions control for micro-regions and year fixed effects. CR_r is the homicide rate measured by the health system (DATASUS), $HomPol_r$ is the homicide rate measured by the police, $Person_r$ is the rate of crimes against the person, and $Property_r$ is the rate of property crimes.
Significant at *** 1 percent, ** 5 percent, and * 10 percent.

Table A.4: Correlation Between Homicide Rates And Other Crime Measures: American States, 1980–2010

	Panel A: Yearly data			
	$\log(Homicide_r)$	$\log(Property_r)$	$\log(Violent_r)$	$\log(Total_r)$
$\log(Homicide_r)$	1.000			
$\log(Property_r)$	0.574***	1.000		
$\log(Violent_r)$	0.753***	0.604***	1.000	
$\log(Total_r)$	0.631***	0.992***	0.699***	1.000
Observations		1,581		

	Panel B: 5-year intervals (1980, 1985, 1990, 1995, 2000, 2005 and 2010)			
	$\log(Homicide_r)$	$\log(Property_r)$	$\log(Violent_r)$	$\log(Total_r)$
$\log(Homicide_r)$	1.000			
$\log(Property_r)$	0.590***	1.000		
$\log(Violent_r)$	0.749***	0.611***	1.000	
$\log(Total_r)$	0.642***	0.992***	0.701***	1.000
Observations		357		

	Panel C: 10-year intervals (1980, 1990, 2000 and 2010)			
	$\log(Homicide_r)$	$\log(Property_r)$	$\log(Violent_r)$	$\log(Total_r)$
$\log(Homicide_r)$	1.000			
$\log(Property_r)$	0.628***	1.000		
$\log(Violent_r)$	0.754***	0.626***	1.000	
$\log(Total_r)$	0.673***	0.993***	0.708***	1.000
Observations		204		

Notes: Uniform Crime Reporting Statistics (Federal Bureau of Investigation) – 1980 to 2010. Data for 51 U.S. states. Observations are weighted by state population. $Homicide_r$ is the homicide rate, $Property_r$ is the rate of property crimes, $Violent_r$ is the rate of violent crimes and $Total_r$ is the total crime rate. The calculation of $Violent_r$ and $Total_r$ excludes homicides to avoid mechanical correlations.
Significant at the *** 1 percent, ** 5 percent, and * 10 percent level.

Table A.5: Correlation Between Log-Changes in Homicide Rates And Other Crime Measures:American States, 1980–2010

Panel A: Yearly data				
	$\Delta_1 \log(Homicide_r)$	$\Delta_1 \log(Property_r)$	$\Delta_1 \log(Violent_r)$	$\Delta_1 \log(Total_r)$
$\Delta_1 \log(Homicide_r)$	1.000			
$\Delta_1 \log(Property_r)$	0.330***	1.000		
$\Delta_1 \log(Violent_r)$	0.319***	0.556***	1.000	
$\Delta_1 \log(Total_r)$	0.341***	0.656***	0.991***	1.000
Observations	1,530			

Panel B: 5-year intervals (1980, 1985, 1990, 1995, 2000, 2005 and 2010)				
	$\Delta_5 \log(Homicide_r)$	$\Delta_5 \log(Property_r)$	$\Delta_5 \log(Violent_r)$	$\Delta_5 \log(Total_r)$
$\Delta_5 \log(Homicide_r)$	1.000			
$\Delta_5 \log(Property_r)$	0.637***	1.000		
$\Delta_5 \log(Violent_r)$	0.689***	0.752***	1.000	
$\Delta_5 \log(Total_r)$	0.703***	0.813***	0.995***	1.000
Observations	306			

Panel C: 10-year intervals (1980, 1990, 2000, 2010)				
	$\Delta_{10} \log(Homicide_r)$	$\Delta_{10} \log(Property_r)$	$\Delta_{10} \log(Violent_r)$	$\Delta_{10} \log(Total_r)$
$\Delta_{10} \log(Homicide_r)$	1.000			
$\Delta_{10} \log(Property_r)$	0.527***	1.000		
$\Delta_{10} \log(Violent_r)$	0.551***	0.746***	1.000	
$\Delta_{10} \log(Total_r)$	0.567***	0.812***	0.994***	1.000
Observations	153			

Notes: Uniform Crime Reporting Statistics (Federal Bureau of Investigation) – 1980 to 2010. Data for 51 U.S. states. Observations are weighted by state population. $Homicide_r$ is the homicide rate, $Property_r$ is the rate of property crimes, $Violent_r$ is the rate of violent crimes and $Total_r$ is the total crime rate. The calculation of $Violent_r$ and $Total_r$ excludes homicides to avoid mechanical correlations. Δ_s stands for $\Delta_{t,t+s}$.
Significant at the *** 1 percent, ** 5 percent, and * 10 percent level.

Table A.6: Homicide Rates and Other Crime Rates: American States, 1980–2010

Panel A: Yearly Data

Dep. Var.: log($Homicide_r$)	(1)	(2)	(3)	(4)
log($Property_r$)	0.591***			0.229**
	(0.129)			(0.0950)
log($Violent_r$)		0.555***		0.421***
		(0.0934)		(0.0901)
log($Total_r$)			0.627***	
			(0.127)	
Observations	1,581	1,581	1,581	1,581
R^2 Within	0.707	0.726	0.713	0.731
R^2 Between	0.381	0.763	0.488	0.744

Panel B: 5-year intervals (1980, 1985, 1990, 1995, 2000, 2005 and 2010)

Dep. Var.: log($Homicide_r$)	(1)	(2)	(3)	(4)
log($Property_r$)	0.512***			0.141
	(0.124)			(0.118)
log($Violent_r$)		0.540***		0.457***
		(0.0928)		(0.110)
log($Total_r$)			0.552***	
			(0.125)	
Observations	357	357	357	357
R^2 Within	0.753	0.776	0.758	0.778
R^2 Between	0.366	0.764	0.475	0.755

Panel C: 10-year intervals (1980, 1990, 2000 and 2010)

Dep. Var.: log($Homicide_r$)	(1)	(2)	(3)	(4)
log($Property_r$)	0.437***			0.0014
	(0.150)			(0.130)
log($Violent_r$)		0.526***		0.525***
		(0.104)		(0.110)
log($Total_r$)			0.482***	
			(0.151)	
Observations	204	204	204	204
R^2 Within	0.803	0.831	0.808	0.831
R^2 Between	0.389	0.767	0.494	0.767

Notes: Uniform Crime Reporting Statistics (Federal Bureau of Investigation) – 1980 to 2010. Standard errors in parentheses, adjusted for 51 state clusters. Observations are weighted by state population. $Homicide_r$ is the homicide rate, $Property_r$ is the rate of property crimes, $Violent_r$ is the rate of violent crimes and $Total_r$ is the total crime rate. The computation of $Violent_r$ and $Total_r$ excludes homicides to avoid mechanical correlations.
Significant at the *** 1 percent, ** 5 percent, and * 10 percent level.

B Tariff Changes after 1995

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

The data on tariffs used in the paper are from Kume et al. (2003). These data have been extensively used by previous papers in the literature on trade and labor markets in Brazil.[26] However, these data only cover the period 1987-1998. In order to show how post-liberalization tariff changes relate to changes induced by the trade reform, we use data from UNCTAD TRAINS, which cover the entire period from 1990 to 2010. Armed with these data, we compute regional tariff changes using sectoral tariff changes between 1990 and 1995 ($RTC_{r,90-95}$), 1990 and 2000 ($RTC_{r,90-00}$) and 1990 and 2010 ($RTC_{r,90-10}$). Table B.1 shows that regional tariff changes over longer horizons, $RTC_{r,90-00}$ and $RTC_{r,90-10}$, are almost perfectly correlated with $RTC_{r,90-95}$ (elasticities are all larger than 0.8 and R-squared's are all larger than 0.92). This implies that changes in tariffs between 1990 and 1995 can indeed be considered as permanent without substantially affecting any of our qualitative or quantitative results.

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

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

Notes: Regional Tariff Changes (RTC_r) over different horizons computed from UNCTAD TRAINS data. $RTC_{r,90-95}$ uses changes in sectoral tariffs between 1990 and 1995; $RTC_{r,90-00}$ uses changes in sectoral tariffs between 1990 and 2000; and $RTC_{r,90-10}$ uses changes in sectoral tariffs between 1990 and 2010. UNCTAD TRAINS tariffs at the product level were aggregated into 44 industries compatible with the 1991 Brazilian Demographic Census. Aggregation was performed using simple averages. These industry-level tariffs were then used in the calculation of RTC_r. Standard errors in parentheses.
Significant at the *** 1 percent, ** 5 percent, and * 10 percent level.

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

C Measuring Employment Rates Consistently over Time

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

In 1991 we define $Emp_{irt} = 1$ if the individual answers yes to "Have you worked in all or part of the previous 12 months?" and zero otherwise. For 2000 and 2010, we define $Emp_{irt} = 1$ if: (a) the individual worked for pay in the reference week; or (b) the individual had a job during the reference week, but for some reason did not work that week; or (c) the individual helped (without pay) a household member in her job or was an intern or apprentice; or (d) the individual helped (without pay) a household member engaged in agricultural activities; or (e) the individual worked in agricultural activities to supply food to household members; and $Emp_{irt} = 0$ otherwise. The answer "yes" to the 1991 question embeds all of the cases above.

D Additional Results

D.1 Regional Tariff Changes and Local Labor Market Outcomes

Tables D.1 and D.2 investigate the effect of regional tariff changes on local labor market outcomes when we restrict attention to outcomes of unskilled and young workers, or when we analyze outcomes using hourly wages instead of total labor market earnings as a measure for w_r. RTC_r is computed using employment shares λ_{ri} conditional on the relevant group (all workers, unskilled workers or young workers). Although this is not strictly consistent with the theoretical framework of Kovak (2013) that we adopt in this paper (see Dix-Carneiro and Kovak, 2015a), this procedure more closely follows the construction of Bartik shocks in Gould et al. (2002). Results are robust to using unconditional employment shares in all specifications.

Table D.1: Heterogeneous Effects Using *Total Earnings* as w_r

Dep. Var.:	$\Delta \log(w_r)$		$\Delta \log(P_{e,r})$		$\Delta \log(w_r \times P_{e,r})$	
	Young Workers (18–30 yrs old)					
	(1)	(2)	(3)	(4)	(5)	(6)
	1991-2000	1991-2010	1991-2000	1991-2010	1991-2000	1991-2010
RTC_r	0.506***	0.697**	0.773***	0.0422	1.267***	0.791**
	(0.123)	(0.267)	(0.107)	(0.179)	(0.163)	(0.336)
Observations	411	411	411	411	411	411
R-squared	0.725	0.687	0.482	0.659	0.682	0.687
	Unskilled Workers (8th grade or less)					
	(1)	(2)	(3)	(4)	(5)	(6)
	1991-2000	1991-2010	1991-2000	1991-2010	1991-2000	1991-2010
RTC_r	0.494***	0.592	0.712***	0.0519	1.179***	0.590
	(0.140)	(0.358)	(0.0795)	(0.116)	(0.162)	(0.392)
Observations	411	411	411	411	411	411
R-squared	0.672	0.632	0.490	0.584	0.679	0.696

Notes: Decennial Census data. Standard errors (in parentheses) adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. Observations are weighted by the inverse of the squared standard error of the estimated change in the dependent variable. All specifications control for state fixed effects. RTC_r is computed using employment shares λ_{ri} conditional on the relevant group (unskilled workers or young workers).
*** Significant at the 1 percent, ** 5 percent, * 10 percent level.

Table D.2: Effects Using *Hourly Wages* as w_r

Dep. Var.:	$\Delta\log(w_r)$		$\Delta\log(P_{e,r})$		$\Delta\log(w_r \times P_{e,r})$	
	All Workers					
	(1)	(2)	(3)	(4)	(5)	(6)
	1991-2000	1991-2010	1991-2000	1991-2010	1991-2000	1991-2010
RTC_r	0.702***	0.438	0.648***	0.0147	1.324***	0.435
	(0.126)	(0.273)	(0.0713)	(0.102)	(0.137)	(0.306)
Observations	411	411	411	411	411	411
R-squared	0.701	0.650	0.495	0.635	0.716	0.665
	Young Workers (18–30 yrs old)					
	(1)	(2)	(3)	(4)	(5)	(6)
	1991-2000	1991-2010	1991-2000	1991-2010	1991-2000	1991-2010
RTC_r	0.613***	0.470*	0.773***	0.0422	1.374***	0.565*
	(0.127)	(0.273)	(0.107)	(0.179)	(0.166)	(0.326)
Observations	411	411	411	411	411	411
R-squared	0.710	0.646	0.482	0.659	0.690	0.670
	Unskilled Workers (8th grade or less)					
	(1)	(2)	(3)	(4)	(5)	(6)
	1991-2000	1991-2010	1991-2000	1991-2010	1991-2000	1991-2010
RTC_r	0.673***	0.385	0.712***	0.0519	1.359***	0.398
	(0.151)	(0.339)	(0.0795)	(0.116)	(0.170)	(0.368)
Observations	411	411	411	411	411	411
R-squared	0.676	0.588	0.490	0.584	0.693	0.678

Notes: Decennial Census data. Standard errors (in parentheses) adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. Observations are weighted by the inverse of the squared standard error of the estimated change in the dependent variable. All specifications control for state fixed effects. RTC_r is computed using employment shares λ_{ri} conditional on the relevant group (all workers, unskilled workers or young workers).
*** Significant at the 1 percent, ** 5 percent, * 10 percent level.

D.2 Crime Rates and Local Labor Market Outcomes

Table D.3 displays the first stages of the two-stage least squares specifications shown in column 5 of Table 7 (column 1 – Medium Term) and in column 5 of the "All Workers" Panel of Table D.7 (column 2 – Long Term). These specifications are identical to those in columns 5 and 6 of Table 6, with the difference that observations are weighted by population (as are the second stages).

Table D.3: Regional Tariff Changes and Labor Market Conditions: First-Stage Regressions

	(1)	(2)
Dep. Var.: $\Delta \log(w_r \times P_{e,r})$	1991-2000	1991-2010
RTC_r	1.171***	0.409
	(0.120)	(0.280)
Observations	411	411
R-squared	0.739	0.720

Notes: Decennial Census data. Standard errors (in parentheses) adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. All specifications control for state fixed effects. Observations are weighted by population, as in the main specification shown in Table 7.
Significant at the *** 1 percent, ** 5 percent, and * 10 percent level.

Columns 1 and 2 of Table D.4 show specifications similar to column 5 of Table 7, but we separately instrument total labor market earnings and employment rates for illustration purposes.

Table D.4: Earnings/Employment Rates separately instrumented

Dep. Var.: $\Delta_{91-00} \log(CR_r)$	2SLS (1)	2SLS (2)
$\Delta_{91-00} \log(w_r)$	-7.082***	
	(2.731)	
$\Delta_{91-00} \log(P_{e,r})$		-5.799**
		(2.288)
First Stage F-Stat	18.5	107.8
Observations	411	411

Notes: DATASUS and Decennial Census data. Standard errors (in parentheses) adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. Observations are weighted by population. Two-stage least square specifications use RTC_r as instrument. All specifications control for state fixed effects.
*** Significant at the 1 percent, ** 5 percent, * 10 percent level.

Finally, Tables D.5 to D.8 investigate the effect of local labor market conditions on crime rates when we restrict attention to unskilled and young workers, or when we analyze outcomes using hourly wages instead of total labor market earnings as a measure for w_r. RTC_r is computed using employment shares λ_{ri} conditional on the relevant group (all workers, unskilled workers or young workers). Although this is not strictly consistent with the theoretical framework of Kovak (2013) that we adopt in this paper (see Dix-Carneiro and Kovak (2015a)), this procedure more closely follows the construction of Bartik shocks in Gould et al. (2002). Results are robust to using unconditional employment shares in all specifications.

Table D.5: Medium-Term Effects Using *Total Earnings* as w_r: 1991–2000

Dep. Var.: $\Delta_{91-00}\log(CR_r)$	OLS (1)	OLS (2)	OLS (3)	OLS (4)	2SLS (5)
	Unskilled Workers (8th grade or less)				
$\Delta_{91-00}\log(w_r)$	-0.0620		-0.0231		
	(0.574)		(0.556)		
$\Delta_{91-00}\log(P_{e,r})$		-0.641	-0.639		
		(0.665)	(0.634)		
$\Delta_{91-00}\log(w_r \times P_{e,r})$				-0.256	-3.381**
				(0.516)	(1.315)
First Stage F-stat					59.5
Observations	411	411	411	411	411
R-squared	0.275	0.278	0.278	0.276	–
	Young Workers (18–30 yrs old)				
$\Delta_{91-00}\log(w_r)$	-0.386		-0.340		
	(0.922)		(0.865)		
$\Delta_{91-00}\log(P_{e,r})$		-0.388	-0.341		
		(0.660)	(0.570)		
$\Delta_{91-00}\log(w_r \times P_{e,r})$				-0.340	-2.880***
				(0.647)	(0.943)
First Stage F-stat					88.2
Observations	411	411	411	411	411
R-squared	0.276	0.276	0.278	0.278	–

Notes: DATASUS and Decennial Census data. Standard errors (in parentheses) adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. Observations are weighted by population. All specifications control for state fixed effects. Two-stage least square specifications use RTC_r as instrument, which is computed using employment shares λ_{ri} conditional on the relevant group (unskilled workers or young workers).
*** Significant at the 1 percent, ** 5 percent, * 10 percent level.

Table D.6: Medium-Term Effects Using *Hourly Wages* as w_r: 1991–2000

Dep. Var.: $\Delta_{91-00}\log(CR_r)$	OLS (1)	OLS (2)	OLS (3)	OLS (4)	2SLS (5)
			All Workers		
$\Delta_{91-00}\log(P_{e,r})$		-0.767	-0.708		
		(0.777)	(0.683)		
$\Delta_{91-00}\log(w_r \times P_{e,r})$				-0.445	-2.898***
				(0.619)	(1.073)
First Stage F-stat					120.4
Observations	411	411	411	411	411
R-squared	0.276	0.279	0.280	0.279	–
			Unskilled Workers (8th grade or less)		
$\Delta_{91-00}\log(w_r)$	-0.0638		-0.00629		
	(0.577)		(0.552)		
$\Delta_{91-00}\log(P_{e,r})$		-0.641	-0.640		
		(0.665)	(0.617)		
$\Delta_{91-00}\log(w_r \times P_{e,r})$				-0.233	-2.861**
				(0.500)	(1.137)
First Stage F-stat					76.2
Observations	411	411	411	411	411
R-squared	0.275	0.278	0.278	0.276	–
			Young Workers (18–30 yrs old)		
$\Delta_{91-00}\log(w_r)$	-0.456		-0.417		
	(0.860)		(0.810)		
$\Delta_{91-00}\log(P_{e,r})$		-0.388	-0.332		
		(0.660)	(0.575)		
$\Delta_{91-00}\log(w_r \times P_{e,r})$				-0.377	-2.647***
				(0.633)	(0.897)
First Stage F-stat					109.0
Observations	411	411	411	411	411
R-squared	0.277	0.276	0.279	0.279	–

Notes: DATASUS and Decennial Census data. Standard errors (in parentheses) adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. Observations are weighted by population. All specifications control for state fixed effects. Two-stage least square specifications use RTC_r as instrument, which is computed using employment shares λ_{ri} conditional on the relevant group (all workers, unskilled workers or young workers).
*** Significant at the 1 percent, ** 5 percent, * 10 percent level.

Table D.7: Long-Term Effects Using *Total Earnings* as w_r: 1991–2010

Dep. Var.: $\Delta_{91-10}\log(CR_r)$	OLS (1)	OLS (2)	OLS (3)	OLS (4)	2SLS (5)
			All Workers		
$\Delta_{91-10}\log(w_r)$	0.964		0.959		
	(0.692)		(0.699)		
$\Delta_{91-10}\log(P_{e,r})$		-0.338	-0.295		
		(0.629)	(0.632)		
$\Delta_{91-10}\log(w_r \times P_{e,r})$				0.646	-3.236
				(0.609)	(6.612)
First Stage F-stat					2.1
Observations	411	411	411	411	411
R-squared	0.647	0.640	0.648	0.644	–
			Unskilled Workers (8th grade or less)		
$\Delta_{91-10}\log(w_r)$	1.001		0.985		
	(0.635)		(0.639)		
$\Delta_{91-10}\log(P_{e,r})$		-0.487	-0.345		
		(0.582)	(0.590)		
$\Delta_{91-10}\log(w_r \times P_{e,r})$				0.691	-6.264
				(0.550)	(16.96)
First Stage F-stat					0.38
Observations	411	411	411	411	411
R-squared	0.651	0.640	0.652	0.646	–
			Young Workers (18–30 yrs old)		
$\Delta_{91-10}\log(w_r)$	1.007		1.008		
	(0.651)		(0.656)		
$\Delta_{91-10}\log(P_{e,r})$		-0.0298	0.0204		
		(0.353)	(0.366)		
$\Delta_{91-10}\log(w_r \times P_{e,r})$				0.522	-2.457
				(0.451)	(4.683)
First Stage F-stat					3.4
Observations	411	411	411	411	411
R-squared	0.649	0.639	0.649	0.644	–

Notes: DATASUS and Decennial Census data. Standard errors (in parentheses) adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. Observations are weighted by population. All specifications control for state fixed effects. Two-stage least square specifications use RTC_r as instrument, which is computed using employment shares λ_{ri} conditional on the relevant group (all workers, unskilled workers or young workers).
*** Significant at the 1 percent, ** 5 percent, * 10 percent level.

Table D.8: Long-Term Effects Using *Hourly Wages* as w_r: 1991–2010

Dep. Var.: $\Delta_{91-10}\log(CR_r)$	OLS (1)	OLS (2)	OLS (3)	OLS (4)	2SLS (5)
			All Workers		
$\Delta_{91-10}\log(w_r)$	0.875		0.863		
	(0.690)		(0.700)		
$\Delta_{91-10}\log(P_{e,r})$		-0.338	-0.178		
		(0.629)	(0.641)		
$\Delta_{91-10}\log(w_r \times P_{e,r})$				0.607	-9.899
				(0.597)	(29.52)
First Stage F-stat					0.2
Observations	411	411	411	411	411
R-squared	0.645	0.640	0.646	0.643	–
			Unskilled Workers (8th grade or less)		
$\Delta_{91-10}\log(w_r)$	0.917		0.894		
	(0.680)		(0.684)		
$\Delta_{91-10}\log(P_{e,r})$		-0.487	-0.232		
		(0.582)	(0.575)		
$\Delta_{91-10}\log(w_r \times P_{e,r})$				0.652	24.87
				(0.579)	(135.1)
First Stage F-stat					0.03
Observations	411	411	411	411	411
R-squared	0.648	0.640	0.649	0.645	–
			Young Workers (18–30 yrs old)		
$\Delta_{91-10}\log(w_r)$	0.897		0.913		
	(0.636)		(0.643)		
$\Delta_{91-10}\log(P_{e,r})$		-0.0298	0.103		
		(0.353)	(0.363)		
$\Delta_{91-10}\log(w_r \times P_{e,r})$				0.502	-6.302
				(0.416)	(14.73)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
First Stage F-stat					0.55
Observations	411	411	411	411	411
R-squared	0.647	0.639	0.647	0.643	–

Notes: DATASUS and Decennial Census data. Standard errors (in parentheses) adjusted for 91 meso-region clusters. Unit of analysis r is a micro-region. Observations are weighted by population. All specifications control for state fixed effects. Two-stage least square specifications use RTC_r as instrument, which is computed using employment shares λ_{ri} conditional on the relevant group (all workers, unskilled workers or young workers).
*** Significant at the 1 percent, ** 5 percent, * 10 percent level.