I. Introduction
Does the arrival of migrants result in more crime? This question is popular in the public debate, especially with regard to international migration into developed countries. However, the existing literature finds no or only very small effects of international migration on crime (and only for specific groups of migrants; Spenkuch 2011; Bianchi, Buonanno, and Pinotti 2012; Bell, Machin, and Fasani 2013; Chalfin 2015; Özden, Testaverde, and Wagner 2018). The question is potentially more relevant in the case of internal migration: Internal migrants are much closer substitutes to residents in the destination labor markets than their international counterparts. They do not face language barriers or legal restrictions to their participation, and they can often move at much lower costs, as internal mobility is mostly unregulated. In contrast, international migrant composition in terms of skills and demographics and associated labor market outcomes are, to a large part, determined by immigration regulations (Lazear 2020). Consequently, an influx of internal migrants may result in unemployment of either residents or migrants. The labor market effects of migration have been a particular concern in the literature on rural-urban migration.
(Harris and Todaro 1970; Todaro 1971), and recent empirical evidence confirms that migration into urban areas does indeed give rise to unemployment and lower wages (Kleemans and Magruder 2018). In turn, these labor market effects may lead to higher levels of crime—an association that has also been independently confirmed in the context of the United States (Grogger and Willis 1998; Gould, Weinberg, and Mustard 2002) as well as Brazil (Dix-Carneiro, Soares, and Ulyssea 2018).

In this paper, I provide the first evidence for the effect of internal mobility on local crime rates, focusing on violent crimes (homicides) in Brazil. Crime rates in Brazil are among the highest in the world (UNODC 2013). High crime, especially homicides, comes at a high economic cost. Aside from public safety expenditures, violent crime is shown to have a negative impact on human capital (Monteiro and Rocha 2016) and health (Manacorda and Koppensteiner 2015). According to the 2014 Annual Brazilian Public Safety Report, violent crime alone cost Brazil an equivalent of 5.4% of the country’s gross domestic product (GDP; de Segurança Publica 2014). In addition, rates of internal mobility in Brazil are high, comparable to those in the United States, making it important to establish the relationship between migration and crime.1

I estimate the effect of internal migration on homicides in a panel of Brazilian microregiões (microregions) from 2005 to 2010 using the nationally representative Brazilian census survey of 2010. The advantage of these data is that they allow analysis at a fine geographic level and of annual dynamics. Assessing the impact of internal migration on crime at destinations is complicated by issues of reverse causality and omitted variable bias. To overcome this endogeneity, I apply an instrumental variable (IV) approach that has been previously used to study the effects of migration on labor market outcomes. The exogenous variation in the migration rate over time comes from local labor demand shocks in the manufacturing sector in migrants’ places of origin (Bound and Holzer 2000; Notowidigdo 2013; Monras 2015; Diamond 2016; Morten and Oliveira 2016).

I find an elasticity of 0.94% between the in-migration rate and the homicide rate. The elasticity can be interpreted as the lower bound of the effect on overall crime rates, because homicides are the most extreme act of crime and are highly correlated with other types of crime in Brazil (Dix-Carneiro, Soares, and Ulyssea 2018). Exploring the channels through which migration might increase crime, I find that low-educated individuals are the most likely to migrate in response to the local labor market shocks in their origins. Thus, labor market

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1 Approximately 20% of Brazilians had moved to another microregião, according to the census of 2010.
competition among low-educated workers is expected to rise at migrants’ destinations. However, those usually associated with high crime—young, uneducated men—are not more likely to migrate than their female or older counterparts, which suggests that it is not necessarily the migrants themselves who commit crimes at the destination. Yet I find that in-migration increases the share of young men who are unemployed, supporting the hypothesis of direct labor market competition between migrants and residents at destinations. A back-of-the-envelope calculation suggests that this channel explains around 86% of the total effect. Furthermore, I show that in locations where the informal sector is relatively small, migration increases homicide rates significantly, whereas the same is not true in locations with the opposite setting, pointing to the importance of the informal sector in absorbing labor. In more rigid markets, formal employment declines in response to in-migration, while it remains unaffected in locations with a large informal sector. Similarly, the main effect is found only in locations with high past crime. These variables have been shown to be significantly related to high crime, and it appears that in such locations, their effects become exacerbated as labor market competition increases through migration.

This paper is structured as follows. An overview of the literature on crime and migration in section II is followed by setting the context of crime in Brazil in section III. Next, I describe the data and define the variables used in the analysis (sec. IV). Section V explains the empirical methodology. Thereafter, results are presented and discussed in section VI. Section VII discusses possible mechanisms. Section VIII presents robustness checks and sensitivity tests, and I conclude in section IX.

II. Literature Review

In the economic model of crime (Becker 1968), both unemployment and low wages reduce the opportunity costs of crime. The empirical literature confirms this for property crime in the United States (Grogger and Willis 1998; Gould, Weinberg, and Mustard 2002). Kelly (2000) emphasizes that only inequality matters significantly for violent crimes in the United States. Evidence for developing countries is thinner, despite the high prevalence of crime and inequality in some regions, such as Latin America. Fajnzylber, Lederman, and Loayza (2002) present cross-country evidence for the positive effect of inequality on crime, including in developing countries. They also document that crime is countercyclical in occurrence and that past crime is a strong predictor of current crime levels. Demombynes and Özler (2005) find that in South Africa, higher inequality is related to higher levels of property crime. Evidence for Latin America comes mainly from Mexico, where homicide rates have increased.
severely in the past decade. Inequality significantly predicts drug-related homicides (Enamorado et al. 2016). For Brazil, there is evidence that inequality, urbanization, and unemployment are determinants of federal homicide rates (Sachsida et al. 2010; Sachsida 2013), but income, male population, drug use, firearm ownership, incarceration rate, and police effectiveness are also strongly correlated with crime (Cerqueira 2014a). A recent study by Dix-Carneiro, Soares, and Ulyssea (2018) is the first to identify the causal effect of local economic shocks on crime in a developing country by exploiting the trade liberalization in Brazil as an exogenous shock. Their findings confirm that worse employment rates increase crime in the medium run.

Migrant inflows arguably constitute a different kind of economic shock but one that can also impact the local labor market in the migrant destination. In the context of the United States, the literature has found that the arrival of internal migrants has no effect on local wages in the destination US cities but reduced the hours worked and induced out-migration among residents (Bous-tan, Fishback, and Kantor 2010). The only evidence on this topic in a developing country context comes from Kleemans and Magruder (2018). They study the effect of internal migration on local labor markets in Indonesia and find that in-migration increases unemployment and lowers wages for residents in the destination areas. Furthermore, they demonstrate how the impact differs for formal and informal sector workers. The formal sector is affected in terms of employment, whereas incomes are affected only in the informal sector. These findings point to a negative impact of internal migration on destination labor markets. This negative effect could further lead to an increase in crime rates.

The impact of migration on crime has been studied only in the context of international migration (across US counties: Spenkuch 2011; Chal fin 2014, 2015; for Italy: Bell, Machin, and Fasani 2013; for the United Kingdom: Bianchi, Buonanno, and Pinotti 2012; for Malaysia: Özden, Testaverde, and Wagner 2018). These studies conclude that immigration has no effect, or only a weak positive effect, on property crime, but all emphasize that there is heterogeneity in the group of immigrants. Immigrants with few labor market prospects—for example, unskilled Mexican immigrants in the United States or asylum seekers in the United Kingdom—increase property crime rates, whereas those immigrants with qualifications and legal access to the labor market—for example, Polish immigrants in the United Kingdom—have no effect on property crime or are even associated with lower crime rates relative to residents.

This paper therefore contributes to the literature by analyzing a possible determinant of crime that has not been considered so far: internal migration. The literature on migration and crime has focused so far only on international migration, even though internal migration occurs at much higher rates and
differs in its dynamics and the migrants’ characteristics. By focusing on Brazil, this paper enriches the literature on internal migration and its consequences in developing countries.

III. Homicides in Brazil

The literature on migration and crime tends to focus on property crime because it can be seen as an economic activity with economic benefits and opportunity costs (Becker 1968). Furthermore, in most cases, empirical studies in developed countries do not find any significant effect of international immigration on violent crimes (Bianchi, Buonanno, and Pinotti 2012; Bell, Machin, and Fasani 2013). In those countries, violent crime is comparatively low compared with Brazil, which ranks as one of the most violent countries in the world in terms of homicides. Many Latin American countries have high homicide rates, driven by either civil war–like struggles (Colombia) or wars between the state and drug gangs (Mexico; Fernandes and de Sousa Nascimento 2007).

In Brazil, violence is not easily traced to any one factor. In some areas of the country, such as large metropolitan cities or the Amazon, illegal markets have emerged over the past few decades. The drug trade and trade of tropical woods are markets outside the legal order, and agents use violence to enforce their rules (Chimeli and Soares 2011). Violence between drug gangs in Rio de Janeiro regularly made global news and are often accompanied by crime unrelated to drugs in areas adjacent to drug trade areas in the city (Fernandes and de Sousa Nascimento 2007). Other sources of conflict are those concerning land, dating back to the colonization of Brazil, and such conflicts are often encountered in conflicts with the indigenous or landless population of Brazil (Hidalgo et al. 2010). All these points of conflict contribute to high homicide rates. Those who are mainly involved in committing homicides and those mostly victimized are young nonwhite men, reflecting the underlying socioeconomic issues of violence in Brazil (Reichenheim et al. 2011).

Additionally, many weapons found their way into the country during the dictatorship from the 1960s to the 1980s, and thus unreported firearm ownership is common. Small crimes such as street robbery can easily result in violent and often deadly crimes. In the words of Fernandes and de Sousa Nascimento (2007), “Brazil is a society with rates of firearm victimization that surpass some countries at war” (228). Despite the introduction of a disarmament law in 2003, crime does not appear to have reduced. The state of São Paulo was the only one successful in the reduction of death due to firearms (de Castro Cerqueira and Pinho de Mello 2012).

In most studies, crime rates are a combination of property crime, such as burglary and theft, and violent crime, with homicide as the most extreme case.
In contrast to the United States, for example, there is no reliable data on property and other crimes, aside from homicides, available for all of Brazil over the study period. Only the two most populous states of Brazil collect such data. Dix-Carneiro, Soares, and Ulyssea (2018), who investigate impacts of local employment shocks on homicide rates, show highly significant correlations of homicides and various other crimes for microregiões in the two most populous states of Brazil, for which such data are available. Homicide rates are therefore a strong indicator of general crime rates in Brazil, and any effect found on homicide rates can be seen as a lower bound estimate for the impact on overall crime.

IV. Data, Variables, and Definitions
The sample of analysis is an annual panel of destination microregiões spanning the period 2005–10. Microregiões are geographic and administrative agglomerations of municipalities sharing a labor market and economic activities, a bit larger than counties in the United States. The hypothesized effect of migration on crime is expected to be channeled through the labor market. Thus, local labor markets provide the best unit of observation. The data for the dependent, independent, and instrumental variables come from different sources and require aggregation in the case of migration rates.

A. Migration
The migration data come from the Brazilian Population Census Survey of 2010. Every 10 years, the Brazilian National Institute for Geography and Statistics (IBGE) conducts a nationally representative household survey along with the population census (Censo Demográfico 2010; IBGE 2012). This survey comprises approximately 20 million individuals in all microregiões of Brazil, covering 10% of the whole population. It contains information on household composition, living conditions, labor market, education, geographic location, and migration. I construct a panel of annual migration between microregiões from retrospective migration questions. Each individual is asked about their former microregião of residence and of work and the time since migration in years. For each year from 2001 to 2010, based on which year the individual stated as being the year of their move, I aggregate the in- and out-migration rates at the microregião level.2 The sample of migrants is restricted to working-age male

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2 I cannot identify the exact migration year of other migration spells aside from the most recent one to include in the analysis sample. For my analysis, this presents a form of measurement error of the endogenous variable, the in-migration flows. The instrumental variable takes care of this if we believe that the error is random. To test this, I draw on a question in the census survey, which asks where one lived in 2005 if not in the current location. I then check how many of those migrants in my sample who moved between 2005 and 2010 had also lived somewhere else in 2005 relative to their current location.
and female Brazilians, because the interest lies in labor market dynamics. For the aggregation from the individual to the microregião levels, I apply population survey weights. I restrict the definition to people who move at least 252 km. This is the median distance that migrants in the data move. I do this to avoid capturing people who just move to their neighboring town. The definition of migration between microregiões ensures that migrants move between local labor markets and not within them. This definition also reduces the concern of spatial correlation between the instrument measured at origins and the dependent variable observed at destinations. In the robustness checks (sec. VIII.A), I change the distance cutoff to test whether such potential spatial spillovers affect the results.

The in-migration rate $M_{m,t}$ is the number of migrants in microregião $m$ in year $t$ relative to the local population in the previous year per 100,000 inhabitants:

$$M_{m,t} = \frac{\text{Immigrants}_{m,t}}{\text{Population}_{m,t-1}} \times 100,000.$$

I use the rate of migration relative to population and not the absolute number of migrants, because I expect the effect of migration to be different if 10 migrants arrive in a microregião of 100 people compared with one with 100,000 people. It is also the same unit as the dependent variable, homicides per 100,000 inhabitants, which allows for an easier interpretation of the estimates.

B. Homicides

Homicide rates—that is, the number of homicides per 100,000 persons living in a microregião—come from the Brazilian System of Death Registration (SIM) maintained by the Brazilian Ministry of Health (Ministério da Saúde). Data were extracted from the Department of Public Health Information (DATASUS), which is regarded as the most reliable information source on location and the location from which they moved in their most recent migration spell. While the measure is imperfect, it provides an idea of the size of the problem. The share of such migrants is 12%. Comparing characteristics of these two groups, they look almost the same in terms of age, sex, education level, and ethnicity. Most importantly, multiple migrants are not more likely to be young, male, and low-skilled compared with one-time migrants. Further, I test whether migrants with multiple spells as defined above are more likely to move in response to the origin labor demand shock and whether they are more likely to move specific distances. They are marginally more likely to move in some of the years, probably as they are more mobile in general but not different in their likelihood to move specific distances. I therefore assume that it presents a random measurement error. Results are available on request.

The legal working age in Brazil is 16, and the retirement age for men is 65. All individuals in the sample are currently not in school.
homicides in Brazil (Cerqueira 2012). Homicides are those deaths registered with the codes X85 to Y09, according to the international coding of violent deaths in the Global Burden Disease 2004 Update by the World Health Organization (Murray, Cerqueira, and Kahn 2013).

Even though homicide is the most extreme type of crime and, as such, is more likely to be reported than other crimes, there are known issues of under- and nonreporting in Brazil (Cerqueira 2014b). However, at the level of the microregião, there are only 34 microregião-year observations with missing values.

C. Instrumental Variable
Data on employment and wages in the manufacturing sector at the microregião and national levels come from the RAIS (Annual Social Information Report), a national employment registry.4

D. Additional Variables
Data on municipal population size are obtained from Ipeadata.5 These are projections based on the 2000 and 2010 population censuses. Annual data from the nationally representative household survey (PNAD) is used to construct a panel of a subsample of 410 microregiões from 2005 until 2009 that is further used to complement the information on labor market outcomes.

E. Summary Statistics
In table 1, I present the summary statistics of the two main variables, homicide and in-migration rates in the 558 microregiões from 2005 to 2010. The number of observations is only 3,316 due to 34 microregião-year observations with missing homicide rates. The in-migration rate is, on average, 350 migrants per 100,000 inhabitants. An average of 19 out of 100,000 people in a microregião were murdered each year in the study period. This rate is very high in international comparison, where the average homicide rate was approximately 7 per 100,000 people in 2010 (UNODC 2011).

The standard deviations of the main variables presented in table 1 indicate a large variation across microregiões. Figures B2 and B3 show the distribution of

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4 The Annual Social Information Report (Relação Anual de Informações Sociais [RAIS]) is collected by the Ministry of Labor and Employment and comprises approximately 97% of Brazilian formal enterprises. Manufacturing sector is used as broad term for the processing industry. It is category “C” in the updated national code of economic activities (CNAE) by the Brazilian Institute for Geography and Statistics (IBGE), and it includes the two-digit level from 10 (“production of food products”) to 33 (“maintenance, reparation and installation of machinery and equipment”; IBGE 2012).

5 Ipeadata is an online data pool provided by Ipea (Instituto de Pesquisa Econômica Aplicada), a Brazilian public research institute, which collects the data from several ministries and other public sources.
The two main variables across the country pointing at some hot spots for crime and some major migration destinations, indicating some overlap. The variation within the greater regions of Brazil emphasizes the value of studying the relationship between internal migration flows and crime at the subregional level. The overlap in the maps is confirmed by a simple plotting of the correlation between homicide and in-migration rates in Figure B1. Higher in-migration rates are associated with higher homicide rates. To establish a causal relationship, however, an IV approach will be used.

V. Methodology
A. Empirical Strategy

This paper estimates the effect of internal migration on crime rates at the local level in a 6-year panel. I estimate a log-log fixed effect model of crime on in-migration:

$$\ln(H_{m,t}) = \beta_1 \ln(M_{m,t}) + \beta_2 \ln(\text{Pop}_{m,t}) + \theta_m + T_t + \gamma_t + \epsilon_{m,t}. \quad (2)$$

The homicide rate $H$ in microregião $m$ in year $t$ is a function of the rate of in-migration $M$ into microregião $m$. By including fixed effects $\theta_m$, any time-invariant unobservable characteristics of each locality are captured. The year dummy $T_t$ controls for year-specific events that affect all microregiões, such as the global economic crisis that hit Brazil in 2009/2010; $\gamma_t$ captures any state-specific trends in the dependent variable. In Brazil, public safety and the homicide reporting system fall under state legislation. For example, the state of São Paulo invested in policies that were successful in reducing homicide rates by nearly half over the study period (Peres et al. 2011; Justus et al. 2018). The trend thus controls for such time-varying policy differences between states; $\epsilon$ is an idiosyncratic error term.

Population is included to control for the natural population growth in each location. All regressions are also weighted by microregião population following the health literature, which shows that mortality realization is an estimator of the underlying mortality probability (Dix-Carneiro, Soares, and Ulyssea 2018). Other applications are seen, for example, in Bell, Machin, and Fasani (2013).

The estimated coefficient $\beta_1$ measures the percentage change in homicide rates associated with a 1% change in the in-migration rates. In contrast to many

| TABLE 1 |
|-----------------|-----|-----|
| **DESCRIPTIVE STATISTICS OF MAIN VARIABLES, DESTINATION MICROREGIÃO-YEAR OBSERVATIONS** |     |     |
| **N** | **Mean** | **SD** |
| Homicide rate | 3,316 | 18.5 | 14.5 |
| In-migration rate | 3,316 | 324.6 | 384.04 |
studies of the impact of international immigration, I assume here that the migration flows observed are normal labor market dynamics across space. I hence assume that it is common for a share of workers every year to move locations for new opportunities so that there is always a migration flow. The panel model of the migration flow thus measures deviations from that usual inflow to assess what happens when relatively more migrants arrive than usual. This approach is also based on the assumption that internal migrants are much closer substitutes to residents in the destination labor markets than their international counterparts. They do not face language barriers or legal restrictions to their participation, and they can often move at much lower costs as internal mobility is mostly unregulated. Hence, it is not the migrant stock that matters for local labor market dynamics but their inflow.

Two econometric issues arise in the context of estimating $\beta_1$. First, there might be unobservable factors that affect homicide rates and in-migration rates, for example, labor market institutions or police effectiveness. Assuming that such unobserved variables do not vary over the study period, the fixed effects model eliminates their effect. Second, there is the problem of reverse causality between crime and migration. The lower crime rates are, the more attractive a location might be for migrants. This would bias the estimate downward. Alternatively, the higher the crime rates are, the larger the illegal market will be, which could attract specific types of migrants. In this case, one would overestimate the impact. To identify the causal effect of immigration on crime, I apply an IV strategy.

B. Instrument for Migration Rates
The IV has to be a strong predictor of migration but independent of crime rates at the destination of migrants. It has to predict that migrants leave their origin and that they choose one destination over another (Card 2001). To estimate a fixed effects panel model, the instrument for this analysis also needs to predict variation in migration over time.

I adopt an instrumental variable strategy that has been used in the literature to study local labor market dynamics. The recent applications by Bound and Holzer (2000), Notowidigdo (2013), Monras (2015), Diamond (2016), and Morten and Oliveira (2016) showed that if a sector experiences a slump at the national level, then wages or employment will fall relatively more in locations where this sector usually employs a large share of workers. This crisis affects workers’ decision regarding whether to stay in or leave these locations. I

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6 I do not include additional time-varying control variables, such as unemployment rate, because they are likely to be endogenous.
employ such local labor demand shocks in the manufacturing sector to create exogenous variation in the migration rate.

The intuition behind the first stage is the following: Consider a microregião $m$ hosts immigrants from a specific origin $o$. If origin $o$ is affected by a local labor demand shock $S$, I expect this to change the rate of migrants arriving in microregião $m$ from origin $o$ in the following period. The sector-specific local labor demand shock in year $t$, $S_{o,t}$, is an interaction of the employment share in a specific sector in location $o$ in the prestudy year 2003, $e_{o,2003}$, and the national employment growth of that sector, $E^{S}_{t}$, in each year $t$ of the analysis:

$$S_{o,t} = e_{o,2003} \times E^{S}_{t}.$$  \hspace{1cm} (3)

If employment falls at the national level in sector $S$, demand for workers in this sector is lower, and people might lose their jobs. Employment or other economic shocks in one microregião cannot change this national trend. This is why I can assume the national employment growth to be exogenous. Instead of the national wage growth, Monras (2015) used a dummy indicating whether the year was before or after the financial crisis hit the United States. Bound and Holzer (2000) used hours worked in a specific sector. I will use national employment growth as Bartik (1991) did.

If the employment share of the sector in a specific microregião, $e_{o}$, is large, this location will be affected more strongly by the changes in national trends. The mobility of workers is likely to be affected by this variation. The out-migration of workers would change the employment share. I therefore use the employment share in 2003 preceding the study period. This provides variation across origins, and the national employment growth creates annual variation.

Many origins can be affected by such a shock, and destination microregiões receive migrants from all origins, not just one. Thus, the term $p_{o,m}$ is used to weigh each origin $o$ according to its importance as a migrant sender to each specific destination microregião $m$. The weight is based on migration patterns predating the study period, and it follows the literature exploiting the variation of historical migration flows between origin-destination pairs (Spenkuch 2011; Bianchi, Buonanno, and Pinotti 2012; Bell, Machin, and Fasani 2013; Jaitman and Machin 2013; Chalfin 2014, 2015; Özden, Testaverde, and Wagner 2018). The idea is that the destination choice of migrants follows certain patterns that were established in the past and evolved over time due to networks (Munshi 2003). A migrant from origin $A$ is more likely to go to destination $B$ than to $C$ if, in the past, more people moved from $A$ to $B$ than from $A$ to $C$.

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7 Each microregião is excluded when computing its local instrument to avoid that it could drive the national trend.
First, I aggregate all migrants that moved out of an origin microregião during the years 2001 to 2004. Next, I compute how many of these migrants moved to each specific destination microregião. Then, \( p_{o,m} \) is the share of the origin-destination-specific migrants over all migrants that left origin \( o \). One can think of it as the probability of migrants leaving origin \( o \) to move to destination \( m \). The probability that migrants who move in response to a local labor demand shock \( S \) will arrive in microregião \( m \) is therefore the sum over the shocks in all origins \( O \) weighted by the destination-specific migration probabilities of each origin \( p_{o,m} \).

The first stage of the two-stage least squares (2SLS) IV estimation can thus be written as follows:

\[
\ln (M)_{m,t} = b_1 \sum_{o=1}^{O} (p_{o,m} \times S_{o,t-1}) + b_2 S_{m,t-1} + b_3 \ln (\text{Pop})_{m,t} + \theta_m + T_t + \gamma_t t + u_{m,t}. \tag{4}
\]

The in-migration rate in microregião \( m \) in year \( t \), \( \ln (M)_{m,t} \), depends on the sum of weighted lagged local labor demand shocks in the origins of migrants, \( S_{o,t-1} \).

Finally, the first and second stages also include a control of the lagged local labor demand shock in the destination microregiões, \( S_{m,t-1} \), to capture the potential correlation of these shocks across origin and destination microregiões. This is to ensure that the demand shock is related only to crime through the migration rate, as required by the exclusion restriction.

In the second stage, homicide rates are regressed on the predicted in-migration rate, \( \hat{M} \):

\[
\ln (H)_{m,t} = \beta_1 \ln (M)_{m,t} + \beta_2 \ln (\text{Pop})_{m,t} + \beta_3 S_{m,t-1} + \theta_m + T_t + \gamma_t t + \epsilon_{m,t}. \tag{5}
\]

With a valid instrument, the estimation of equation (5) gives a consistent estimate of \( \beta \), the impact of internal migration on crime.

Identification comes from the assumption that the exposure of each microregião to origin-specific shocks weighted by past in-migration shares is unrelated to any changes in the outcome (homicides), aside from those carried through actual migration. Following Goldsmith-Pinkham, Sorkin, and Swift (2020), we have three underlying assumptions. First, the initial migration shares are uncorrelated with changes in other correlates that could affect changes in the outcome (no omitted variable bias). Second, changes in the outcome prior to the study period are independent of the initial shares (parallel trends). Third, all instruments (i.e., all shares) are exogenous if used as individual instruments. I follow the proposed
tests by Goldsmith-Pinkham, Sorkin, and Swift (2020) and report results of those tests in section VIII.B. The plausibility of all assumptions can be confirmed.

In addition to these tests, I adjust national growth to exclude the microregião where I measure the local labor demand shock so that one location alone cannot drive the shock. Further, I include local labor demand shocks at the destination to control for correlations between sectoral shocks across the country. I address remaining endogeneity concerns in section VIII.A, where I show that the effect is not driven by spatial spillovers between closely located areas but only through migration between locations.

VI. Results
A. Reduced-Form and First-Stage Results

Table 2 shows the reduced form and first stage of the 2SLS estimation. The instrument—the sum of weighted labor demand shocks in migrants’ origins—is a significant predictor of crime at destinations (col. 1) controlling for local population, local labor demand shocks, microregião, and year fixed effects. In column 2, state-year fixed effects are added to control for time-varying state-level characteristics, such as spending on public safety including policing. Turning to the first stage in columns 3 and 4, it is confirmed that origin demand shocks weighted by past migration patterns predict a decrease in in-migration at destinations.

A control of the lagged local labor demand shock in the destination microregiões is included to capture the potential correlation of these shocks across

| TABLE 2 |
| --- |
| REDUCED FORM AND FIRST STAGE OF THE TWO-STAGE LEAST SQUARES REGRESSION |
|  |
| Reduced Form | First Stage |
| Log(Homicide Rate) | Log(In-Migration Rate) |
| (1) | (2) | (3) | (4) |
| Weighted origin labor demand shock in \( t - 1 \) | -.000*** | -.000*** | -.000*** | -.000*** |
| (0.000) | (0.000) | (0.000) | (0.000) |
| Local labor demand shock in \( t - 1 \) | -.208 | .057 | -.101 | -.273** |
| (0.207) | (0.124) | (0.106) | (0.112) |
| Log(population) | .425 | -.206 | -.923*** | -.624*** |
| (0.332) | (0.273) | (0.206) | (0.215) |
| Microregião fixed effects | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes |
| State-year fixed effects | No | Yes | No | Yes |
| Observations | 3,300 | 3,300 | 3,300 | 3,300 |
| \( R^2 \) | .886 | .916 | 1 | 1 |
| Kleibergen-Paap F-test | 18 | 25 |

Note. Standard errors are clustered at the microregião level, and all estimations are weighted by the microregião population.

** Significance at the 5% level.

*** Significance at the 1% level.
origin and destination microregiões. If they were correlated, their inclusion in the regression should change the size of the coefficient of the in-migration rate. It seems that the destination labor demand variation is, however, insignificant unless we include state-year fixed effects in the first stage.

The Kleibergen-Paap F-test for weak identification is in both cases well above the rule-of-thumb value of 10, which confirms the relevance of the instrument for the first stage. Column 4 of table 2 displays a negative coefficient for origin demand shocks. It should be noted that the construction of the instrumental variable allows migrants to move to all destinations available, giving more weight to those that they are most likely to move to due to past migration trends. The coefficient of the IV in column 4 of table 2 should thus be read as follows: A positive labor demand shock in an origin location predicts relatively less in-migration to locations where relatively more migrants moved to in the past. In contrast, the local labor demand shock at the destination does not control for this sorting of migrants and can thus not be interpreted as a pull instrument. The coefficient should be interpreted such that, on average, while a positive local labor demand shock is significantly associated with less in-migration, this might simply reflect that, on average, in the study period, those locations that experienced positive labor demand shocks were not systematically the major migrant destinations in the past.

The negative coefficient on the instrument means further that if, at the national level, employment in the manufacturing sector grew faster than in the previous year, fewer people will migrate out from microregiões with a relatively larger share of the manufacturing sector. The opposite applies for slower employment growth.

For this interpretation, it is important to understand how the local demand shocks affect local economies and, consequently, out-migration from the origins. Table 3 shows the coefficients of a fixed-effects regression of local GDP, formal wages, and employment, as well as manufacturing wages in origin microregiões on the first lag of the instrument, the manufacturing sector labor demand shock. The estimates show a negative and weakly significant relationship for the study period in the origin microregiões for GDP and employment, but none for formal sector wages. If national employment in the manufacturing sector grows faster than in the previous year, local economies of locations with high manufacturing employment overall show a weak reduction in GDP and employment but an increase in manufacturing wages. Thus, the instrument really captures sector-specific localized shocks. These variations in local economic conditions affect migrants’ moving decisions, as Monras (2015), among others, has shown.

Other studies have shown that out-migration can be less likely in response to negative shocks compared with positive local shocks due to the presence of
migration costs and credit constraints (e.g., Notowidigdo 2013). Morten and Oliveira (2016) find for Brazil that “37% of the total incidence of a [local] shock falls on residents [if migration is costly], compared to 1% in a model where migration is costless.” Guriev and Vakulenko (2015) show that internal out-migration is restricted by migration costs, credit constraints, and poverty. The out-migration rate from less-developed locations in Russia increases with an increase in income and declines in credit constraints in these areas. Hirvonen (2016) finds that Tanzanians exposed to negative income shocks are less likely to migrate at the individual and household levels.

In the context of this paper, the majority of migrants are low skilled. Therefore, I expect the majority of workers to be in the lower quantiles of the income distribution so that migration costs matter and migration flows depend on income levels. Based on table 3, the local labor demand shock works in two directions: it increases incomes for manufacturing workers, lifting their credit constraints to migrate, and it reduces overall GDP and employment, which in turn could limit workers’ mobility. It is not clear which effect would dominate in combination with the sorting of migrants across locations. As a result, the first-stage result shows a negative relationship between local labor demand shocks at the origin and in-migration at the destination.

B. Second-Stage Results
The results of the second stage are presented in table 4. The first two columns show the results of a simple fixed-effects estimation that does not account for

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**TABLE 3**

|                        | Log(GDP) | Log(Monthly Wages) | Log(Official Jobs) | Manufacturing Wages |
|------------------------|----------|--------------------|--------------------|---------------------|
| Manufacturing demand shock | -0.119*  | -0.001             | -0.052*            | 0.234**             |
| Controls               | Yes      | Yes                | Yes                | Yes                 |
| Microregião fixed effects | Yes     | Yes                | Yes                | Yes                 |
| Year fixed effects     | Yes      | Yes                | Yes                | Yes                 |
| State-year fixed effects | Yes     | Yes                | Yes                | Yes                 |
| Observations           | 3,300    | 3,288              | 3,288              | 3,288               |
| $R^2$                  | 1        | 1                  | 1                  | 1                   |

**Note.** Standard errors are clustered at the microregião level, and all estimations are weighted by the microregião population. Controls include population.

* Significance at the 10% level.
** Significance at the 5% level.

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b Cited from the abstract of Morten and Oliveira (2016).
the endogeneity of migration and crime. In this regression, there is no significant relationship between migration and crime and only a small weak effect when I control for state trends. Once I instrument for the in-migration rate in columns 3 and 4, there is a significant positive impact of the in-migration rate on crime, with an elasticity of approximately 0.94% in my preferred specification in column 4. As expected, reverse causality leads to an underestimation of the relationship, such that the IV-2SLS result is larger than the ordinary least squares result. The inclusion of state trends in column 4 reduces the effect, as it controls for state-specific policies unrelated to migration that influenced crime rates during the study period.

I find that, on average, a 1% increase in the in-migration rate into micro-regiões in Brazil in the 2005–10 period is associated with a significant increase in homicide rates of 0.94%. The average migration rate over the study period is 325 per 100,000 inhabitants. The migration inflows might seem small when compared with studies concerned with international migration. They are, however, not directly comparable. Internal migrants face much fewer barriers than international immigrants in the labor market in terms of legal restrictions, language barriers, or discrimination. Furthermore, the instrument used in this study reflects regular labor market dynamics instead of major economic shocks, which are often exploited in the international migration literature. Therefore,
the relatively smaller migration rates do not constitute a problem to answering the research question of this paper. If the migration rate increases by 10% to 357, which is likely given a standard deviation of 384, then homicide rates are expected to increase to 20. This corresponds to, on average, six more homicides per year in a microregião. Additionally, the effect on homicides is considered a lower bound estimate for the effect on other crimes, such as theft and robbery.

VII. Discussion of Possible Channels

A. Channels at the Individual Level

The results suggest that an increase in migration into destination microregiões is associated with a small but significant rise in homicide rates in these locations. The literature on migration and crime suggests different channels to explain this result. Migrants could be criminals and thus increase the crime rates upon their arrival. Another explanation in the context of international migration is that hate crimes against immigrants increase with higher immigration. These channels cannot be tested with the data, because incarceration or victimization data do not record previous residences if people moved within Brazil. Furthermore, for internal migration, hate crimes against migrants are less likely. The literature also suggests that immigrants with restricted access to the labor market might be more prone to commit crime due to lower opportunity costs. Internal migrants, however, do not face legal or language barriers to participation in the destination labor market.

Those who are statistically most likely to be involved in crime in Brazil are young, unskilled men (Reichenheim et al. 2011). I thus look further into the characteristics of workers who migrate in response to the sector-specific demand shocks. I run a probit regression at the individual level predicting the probability of a migrant being either low-educated, female, and young or a young, low-skilled man on the labor demand shock. The marginal effects in table 5 show that the migrant workers who move in response to a local manufacturing labor demand shock in origin microregiões are significantly more likely to be low educated or to be aged above 25 years. They are not significantly more likely to be male. Consequently, migrants in response to a local labor demand shock are significantly less likely to be young, male, and low-skilled workers compared with other demographics. This does not imply that this demographic does not move in response to the instrument but that they are not more likely to be compliers than less crime-prone groups.

The characteristics of migrants in response to local labor market shocks change somewhat across distances of migrants moved (see table A1). Young men are significantly more likely to migrate short distances (up to 100 km),
whereas longer distances reveal the same patterns as mentioned above. The effect of migration on crime at different distance cutoffs (see fig. 1) does not show any significant difference between shorter and longer distances so that this selection of migrants does not seem to be the driver of the results. While the results are not driven by the migration of a crime-prone population, they can partly be explained by the fact that relatively more low-skilled workers move in response to a manufacturing demand shock. These workers add to the unskilled workforce at destinations, increasing the competition for low-skilled jobs and, thereby, the crime rate.

B. Labor Market Dynamics: Impact of Migration on Labor Market Outcomes

The hypothesis of this paper is that internal migration affects crime levels through labor market dynamics. Studies investigating the impact of internal migration on local labor markets found negative effects of migration on wages and employment of residents at destinations (Boustan, Fishback, and Kantor 2010; Kleemans and Magruder 2018). This could lead to higher rates of crime committed by residents who lose their job or receive lower wages. Kleemans and Magruder (2018) document that in a developing country with a large informal sector, the impact of rural to urban migration is different for formal and informal sector workers.

To explore this heterogeneity in the effect, I use the subsample of 410 *micro-regiões* from the annual nationally representative household survey PNAD

| Probability of Being | Low-Skilled | Female | Young (16–25) | Young, Male, Low-Skilled | N |
|----------------------|------------|--------|---------------|-------------------------|---|
| 2009                 | .000***    | .000   | −.000         | −.000                   | 178,296 |
| 2008                 | .000***    | −.000  | −.000*        | −.000***                | 135,486 |
| 2007                 | .000***    | −.000  | −.000***      | −.000*                  | 138,280 |
| 2006                 | .000***    | −.000  | −.000***      | −.000**                 | 123,029 |
| 2005                 | .000***    | .000   | −.000**       | −.000*                  | 105,273 |

Note. Marginal effects from separate probit estimations of the probability of a migrant to be either low educated, female, young or young/male/unskilled in response to the local labor demand shock in a migrant’s origin in $t-1$ by year of migration. Standard errors are robust. All regressions include dummies for the state of origin.

* Significance at the 10% level.

** Significance at the 5% level.

*** Significance at the 1% level.

TABLE 5
MIGRANTS’ CHARACTERISTIC IN RESPONSE TO LOCAL LABOR DEMAND SHOCKS AT ORIGIN, BY YEAR

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MIGRANTS’ CHARACTERISTIC IN RESPONSE TO LOCAL LABOR DEMAND SHOCKS AT ORIGIN, BY YEAR
combined with the census data for 2010. Results for the impact of in-
migration on homicide rates as well as on various labor market indicators
are presented in table 6. The subsample shows a positive and significant effect
of migration on crime in column 1 a bit larger than that of the full sample.
Furthermore, the predictive power of the instrument remains above the crit-
ical value. Thus, there is no concern that the subsample is systematically differ-
ent from the main sample. In terms of labor market outcomes, column 2 shows
no effect on overall unemployment, but column 3 shows that in-migration in-
creases the formal employment share and wages (formal and informal; col. 4).
These results are the opposite to what Kleemans and Magruder (2018) found.
One explanation could be that Indonesian rural-urban migrants in their sam-
ple were mostly high skilled and thus competed for formal sector jobs. The
Brazilian internal migrants in the sample at hand are mostly low skilled so that
it seems plausible that relatively more native workers find formal jobs or gain
income, even if the differentiation between native and migrant is not possible
to make in the data. In the next section, I will investigate whether these effects
differ by labor market structure.

Last, column 5 suggests that there is a small significant increase in young,
unemployed men—those most prone to crime in Brazil. In the previous sec-
tion, I showed that this population group is not more likely to be among

Figure 1. Results for different distance cutoffs in the migration definition.
migrants, but here I find that they are negatively affected by the labor market pressures induced by migration.9

A study by Britto, Pinotti, and Sampaio (2020) investigates the relationship between unemployment and crime in Brazil over a similar period at the individual level. The authors find that young men are significantly more likely than older workers to commit crimes after being laid off, and they suggest that this is related to liquidity constraints. Such young workers are often also not eligible for unemployment insurance as they lack job tenure. I use their estimate for the probability of male workers below age 25 to commit crime when becoming unemployed, which is roughly 0.33, to compute the effect of in-migration on crime explained by this demographic. In the estimation presented in table 6, I find an effect of 0.023 of in-migration on the share of young, unemployed, male workers. While this seems small, it corresponds to a 3.3% increase in the share of this demographic. Therefore, an increase in 1% of in-migration through this channel corresponds to a 1.09% increase in crime incidence. Considering that the effect of unemployment on homicides is around 74% of the size of the displacement effect on economic crimes in Britto, Pinotti, and Sampaio (2020), I can derive that the young, male unemployment channel is associated with an elasticity of 0.81 for homicides and thus explains

9 Using only low-educated young and unemployed men yields a very small and weakly significant negative effect (−0.002).

**TABLE 6**

|                  | Log(Homicide Rate) | Share of Unemployed | Share of Formally Employed | Log(Hourly Wage) | Share of Young, Unemployed Men |
|------------------|--------------------|---------------------|---------------------------|-----------------|-------------------------------|
|                  | (1)                | (2)                 | (3)                       | (4)             | (5)                           |
| Log(in-migration rate) | 1.260***          | −.145               | .600***                   | 2.658***        | .023*                         |
| Controls         | Yes                | Yes                 | Yes                       | Yes             | Yes                           |
| Microregião fixed effects | Yes             | Yes                 | Yes                       | Yes             | Yes                           |
| Year fixed effects | Yes               | Yes                 | Yes                       | Yes             | Yes                           |
| Year-state fixed effects | Yes             | Yes                 | Yes                       | Yes             | Yes                           |
| N                | 1,859              | 1,859               | 1,859                     | 1,830           | 1,859                         |
| Kleibergen-Paap F-test | 22.2           | 22.2                | 22.2                      | 23.3            | 22.2                          |

Note. Standard errors are clustered at the microregião level. Each regression is weighted by microregião population. Controls are the local labor demand shock in t − 1 and population. The sample includes all microregions for which the nationally representative household survey (PNAD) data were available.

* Significance at the 10% level.

*** Significance at the 1% level.
around 86% of my overall effect of in-migration on crime. These back-of-the-envelope calculations suggest that unemployment among the most crime-prone demographic due to in-migration is a main driver of the effect of in-migration on homicides.

C. Labor Market Structure: Informal and Criminal Sector

The economic model of crime by Becker (1968) argues that individuals are more likely to participate in criminal activities if the opportunity costs and deterrence are very low. Lower deterrence implies a lower probability to getting caught and being punished. Hence, the costs of participating in crime are lower. This depends on sentences in response to crime and on the probability of being caught based on policing measures. Fajnzylber, Lederman, and Loayza (2002) documented in a cross-country panel comparison that past crime is a significant predictor of higher current crime. If criminal markets are very large and long established, then this is often associated with less successful policing activities and low deterrence. It is therefore to be expected that the impact of internal migration on crime is greater in areas where there is a large and long-established criminal sector.

Another factor is whether there are enough outside options to the criminal market to increase the opportunity costs of crime. If in-migrants indeed increase the competition in the labor market, this reduces the opportunities to find a job, and hence crime becomes an option. In line with the traditional view by Fields (1975), large shares of informal work contracts in Brazil are due to the strict labor market regulations that make firing difficult and expensive, require payment of minimum wages, contribute to social protection and low working hours, and relax the role of trade unions (Barros and Corseuil 2001; Bosch, Goni, and Maloney 2007). This incentivizes employers to hire informally. Most recent evidence suggests that formally registered firms hire approximately 40%–50% of their workforce informally, depending on firm size. The wage differential between formal and informal workers is zero within firms, conditional on

10 The elasticity of 1.09 is larger than that of the effect of in-migration on homicide rates (0.94) in the corresponding sample in table 7. The explanation is that Britto, Pinotti, and Sampaio (2020) look at various crimes, not just homicides, while my estimate considers only homicide rates and is thus the lower bound estimate of overall crime rates. Britto, Pinotti, and Sampaio (2020) find that economic crimes have an elasticity of 43% to job displacement and homicides 33%. If I assume homicides work like economic crimes, they explain around 74% of the economic effect size so that we could expect the elasticity of homicides due to migration-induced larger unemployment among young men to be 0.81, explaining almost 86% of the total effect.
individual characteristics (Ulyssea 2018). Thus, the traditional view of informality as way to keep labor costs low seems valid in the Brazilian context.

If the informal sector is very large, immigrants as well as natives will find many opportunities in this sector, whereas the formal sector will be less easily accessible. The results of Kleemans and Magruder (2018) reflect this. Informal wages are affected by migration, but not informal employment, while formal employment is directly affected by the increase in labor supply. If the informal sector is small, low-skilled workers will be confronted with a very rigid formal labor market and higher chances of staying unemployed. This reduces the opportunity costs for crime for these workers.

These two hypotheses are tested in the data. In table 7, I present the results of the main regression for specific subsamples. In those microregiões where the homicide rate was below the median in the past (in 2000), the effect is insignificant (see col. 1). The second column shows the impact of migration inflows on crime in microregiões where the homicide rate has been above the median in the past. The effect is 1, close to the main result.

In the third and fourth columns, I conduct the same exercise with locations where the informal sector is below or above the median size based on 2004 data.11 As suggested, in microregiões where the informal sector is relatively small, the arrival of more workers is associated with a significantly higher homicide rate. The elasticity is 0.97. Columns 5–8 test whether the hypothesis of rigid labor markets holds. I find that formal employment is directly affected in areas with small informal labor markets (col. 5) and unaffected in markets with many informal outside options (col. 6). Expressed differently, in rigid labor markets, informal employment declines in response to in-migration relative to formal employment. This pressure is also reflected in an increase in wages in these markets (col. 7), while they do not react in areas with a large informal sector (col. 8). These results suggest that the prevalence of crime in combination with low availability of outside options to the restrictive formal labor market is associated with a stronger impact of in-migration on local crime rates.

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11 I use the formal sector employment registry (RAIS) data to get a proxy of formal sector size. These data provide the number of jobs formally registered in each microregião. I divide this number by the population size of a given microregião as proxy for formal sector size. Then I divide the sample into those microregiões with a formal sector size above the median in 2004, preceding the study period. Alternatively, to defining the subsample based on 2004 data, I allow it to change annually over the study period, or I use the PNAD subsample, which includes the exact formal employment share. The results are the same and are available on request.
### Table 7: Instrumental Variable–Two-Stage Least Squares (2SLS) Regressions, Subsamples

|                      | Log(Homicide Rate) | Share of Formally Employed | Log(Monthly Wages) |
|----------------------|--------------------|-----------------------------|--------------------|
|                      | Homicide Rate in 2000 | Informality in 2004 | Informality in 2004 | Informality in 2004 |
|                      | (1) Low | (2) High | (3) Low | (4) High | (5) Low | (6) High | (7) Low | (8) High |
| Log(in-migration)   | -0.049 | 1.009*** | .973*** | .254 | .706*** | -.032 | 2.927*** | -0.451 |
| (287)               | (226)   | (.211)   | (.274) |   | (.240) | (.112) | (.858) | (.321) |
| Controls            | Yes     | Yes     | Yes    | Yes | Yes    | Yes   | Yes    | Yes    |
| Microregião fixed effects | Yes     | Yes     | Yes    | Yes | Yes    | Yes   | Yes    | Yes    |
| Year fixed effects  | Yes     | Yes     | Yes    | Yes | Yes    | Yes   | Yes    | Yes    |
| State-year fixed effects | Yes     | Yes     | Yes    | Yes | Yes    | Yes   | Yes    | Yes    |
| Observations        | 1,672   | 1,586   | 1,614  | 1,650 | 1,032  | 793   | 1,016  | 779    |
| Kleibergen-Paap F-test | 10.01  | 24.457  | 28.278 | 20.027 | 23.174 | 11.74 | 23.723 | 11.296 |

**Note.** Standard errors are clustered at the microregião level. All estimates are 2SLS estimates, weighted by the microregião population. Controls are the local labor demand shock in $t - 1$ and population. Subsamples are divided into those above and below the median homicide rate in 2000 and the median share of workers in the informal sector in 2004 in destination microregiões, respectively. Columns 5–8 use only the subsample of observations for which labor market outcomes are available in all years coming from the nationally representative household survey (PNAD) survey.

*** Significance at the 1% level.
VIII. Robustness and Sensitivity of Results
A. Spillover Effects

One potential threat to identification is posed by the possibility that manufacturing sector labor demand shocks at origins predict not only out-migration but also other economic activities that may spill over to the migrant destinations.

For example, a negative shock in one origin might cause fewer workers to leave this location and, at the same time, might cause changes in prices of goods produced and their trade. The firms affected will lower the prices of their goods in order to be able to compete in the national market. Their goods will become cheaper relative to the goods produced in another microregião. Thus, internal trade from the microregião hit by the shock to an unaffected location should increase. This would put pressure on the firms in that destination, and it could imply negative consequences for the local labor market there. Such negative spillovers could, in turn, lead to more crime in destinations. Such spillovers would thus work against the estimated effect.

Ideally, one would use interregional trade data to test for such spillovers. As these are not available, I use an alternative approach. Similar to Kleemans and Magruder (2018), I test how my results change if I use different distance cutoffs to define the migrant sample. Until now, I included all migrants who moved at least the median distance of 252 km when I computed the in-migration rates. Now, I include only migrants who have moved at least a specific distance, starting with 100 km and moving up in steps of 100 to 1,600 km. The estimated coefficients of in-migration on crime from each of these separate regressions are plotted in figure 1.

Trade and labor market spillovers from the local labor demand shock at origins to the destinations are much more likely under a scenario when distances are shorter. If my main results were driven by inter-microregião trade and labor demand spillovers, I would expect the effect to be strongest for shorter distances and decline with longer distances. However, as shown in figure 1, the effect is robust to changes in the distance cutoff. Figure 2 plots the corresponding statistics of the Kleibergen-Paap F-test for weak identification. All results are above the critical value of 10. Thus, the main results should not be affected by such spillovers.

Another way to test for spillover effects is to include the instrumental variable as a control, but using distance instead of migrant shares as weights in its construction. In this way, I control for other unobserved patterns of how the shocks in origins might influence destinations, for example, through trade of goods. Results can be found in table A3. The main effect becomes larger (1.69), and the distance-weighted shocks are significant, while the first stage remains valid. The larger effect points at a downward bias from some spillover...
effects, thus working against my findings. However, testing whether coefficients of in-migration differ between this model and the main model shows no significant difference.

B. Plausibility of Exogenous Shares

As suggested by Goldsmith-Pinkham, Sorkin, and Swift (2020), I show that the initial origin-specific migration shares used to construct the instrumental variable are plausibly exogenous to changes in the outcome variable versus through migration itself.

First, I test whether initial migration shares are correlated to other local characteristics, which could influence changes in homicide rates through an omitted variable bias. Instead of showing these correlations for all shares, I choose the shares from origin locations with the highest Rotemberg weights, that is, those with the strongest influence in the IV. Figure 3 displays the $p$-value of the correlation between each of the weights and one local characteristic out of GDP, wages and employment. Only two of the origins show weakly significant correlations—one at the 10% level and one at the 5% level—with GDP.

Second, homicide rates in the prestudy period are regressed on the same origin-specific migration shares as a test for parallel pretrends. Here, the assumption is that homicides were on a trend in each location that is independent of the
Figure 3. Correlation of local economic conditions and origin-specific migration shares.
location-origin-specific migration shares and thus of the instrument. Figure 4 confirms this independence for each of the five shares.

C. Other Sensitivity Tests

The computation of the weights that are interacted with the instrument (term $p_{o,m}$ in eq. [4]) is based on migration rates from 2001 to 2004. Hence, one concern could be that this short period yields migration patterns specific to these years but not to the study period and that these migration rates are measured with error due to recall bias. As a robustness check, the weights are computed using the full period for which migration data are available, that is, from 2001 to 2010, and using the study period from 2005 to 2010. The results of these estimations are presented in table 8, in columns 1 and 2, respectively. Neither estimate differs from the original estimate of 0.9.

Column 3 of table 8 presents the results of a further sensitivity test using regional employment growth in the manufacturing sector instead of national growth. The effect remains the same. Last, in column 4, the origin-specific shares with largest Rotemberg weights (see sec. VIII.B) were excluded from the construction of the instrument, again without impacting the main result.

Further sensitivity tests were conducted regarding the level of clustering, tests for instrument redundancy, under- and overidentification, as well as misspecification comparing estimates with and without controls and applying a limited information maximum likelihood estimator. Results are presented in

![Figure 4. Coefficient of preperiod trend in homicide rates and origin-specific migration shares.](image)
tables A2 and A4. The coefficient of interest does not change. Only in the case when instead of weighted shocks I treat each origin-specific migration share as an individual instrument the effect is a bit smaller, but I cannot reject the over-identification test confirming that the shares can be considered exogenous. Clustering at state level weakens the test of instrument redundancy, while the test statistic of the Kleibergen-Paap F-test for underidentification is above the rule-of-thumb value of 10. The inclusion of controls compared with not including any controls does not yield statistically different results.

IX. Conclusion
This paper provides the first evidence for the effect of internal migration on crime rates, using data from Brazil. To overcome the endogeneity of migration, local labor demand shocks in the manufacturing sector are used as an instrument.

The results indicate a significant and positive effect of internal migration flows on homicides at migrants’ destinations. A 10% increase in in-migrants relative to the local population leads, on average, to an increase of 9.4% in the homicide rate in a Brazilian microregião in the period from 2005 to 2010. A limitation of the analysis is that data availability dictates a focus on homicides. Given that this is the most extreme form of crime, one may expect that the effects are even larger for overall crime rates, given that violent crimes and other crimes are highly correlated in Brazil.

|                  | Altering Periods for Weights | Other Instrument | Drop Origins | Microregião fixed effects | Year fixed effects | State-year fixed effects | Observations | Kleibergen-Paap F-test |
|------------------|-----------------------------|------------------|--------------|---------------------------|-------------------|-------------------------|--------------|------------------------|
|                  | 2001–10                     | 2005–10          |              |                           |                   |                         |              |                        |
| Log(in-migration) | .923***                     | .911***          | .962***      |                           |                   |                         |              |                        |
|                   | (.217)                      | (.220)           | (.211)       |                           |                   |                         |              |                        |
| Controls          | Yes                         | Yes              | Yes          |                           |                   |                         |              |                        |
| Microregião fixed effects | Yes                  | Yes              | Yes          |                           |                   |                         |              |                        |
| Year fixed effects | Yes                         | Yes              | Yes          |                           |                   |                         |              |                        |
| State-year fixed effects | Yes                 | Yes              | Yes          |                           |                   |                         |              |                        |
| Observations      | 3,300                       | 3,300            | 3,288        |                           |                   |                         |              |                        |
| Kleibergen-Paap F-test | 26.6                      | 27.9             | 24.2         |                           |                   |                         |              |                        |

Note. Standard errors are clustered at the microregião level. Each regression controls for the log of local population and is weighted by microregião population. Destination shock indicates that the regression includes the local labor demand shock variable at destination. Columns 1 and 2 present the results of the main estimation changing the period used to compute the migration probability weights used to construct the instrument. In col. 1, the full period of data available was used, 2001–10; in col. 2, only the most recent years, 2005–10, were used. Column 3 presents the result of the main estimation, but using regional instead of national employment growth to construct the instrument. Column 4 excludes the five origin-specific migration shares with largest Rotemberg weights from the construction of the instrumental variable.

*** Significance at the 1% level.
I show that the estimated effect applies to low-skilled migrants. Use of the local labor demand shock in the manufacturing sector as an instrument strongly predicts the migration of this group of workers. There is no indication that these migrants are more likely to commit crime based on their demographics, but there are no data available to tell us more about who commits more crime, migrants or residents.

The hypothesis of this paper was that internal migration has an impact on destination labor markets and, through these, affects crime. While in-migration does not affect overall unemployment at destinations, I find evidence that it increases unemployment among young men—those most prone to crime in Brazil (Britto, Pinotti, and Sampaio 2020)—and that this effect could explain around 86% of the total effect. Further, I find that in-migration increases the formal sector employment share in destinations. I therefore investigated heterogeneous effects with respect to labor market structures. The effect is significant and even larger in the subsample of microregiões with a small informal sector and a larger criminal sector. I suggest that a larger informal sector acts as a buffer that absorbs low-skilled workers in destination labor markets when labor supply increases due to the arrival of migrants. In contrast, in locations with fewer informal work opportunities, crime increases. Indeed, I confirm that employment and wages react only to in-migration in areas with rigid labor markets. The effect is also larger and significant if the destination has a historically larger criminal sector. The criminal inertia indicates that deterrence measures are unsuccessful in these locations and that “crime” is an economically important activity or “industry” (Becker 1968, 170).

This study is consistent with the findings of Kleemans and Magruder (2018), who showed that the impact of immigration on local labor markets in developing countries with high informality is different from that of previous model predictions and from empirical analyses that did not include the informal sector (Card and Lemieux 2001; Borjas 2003). Yet my results differ to theirs, which could be related to the skill profile of internal migrants presenting thus another aspect to take into consideration in future studies. In many developing economies, such as Brazil, it is hence important to account for the role of the informal sector.
### TABLE A1
MIGRANTS’ CHARACTERISTICS IN RESPONSE TO LOCAL LABOR DEMAND SHOCKS AT ORIGIN IN 2008, BY MAXIMUM DISTANCE MOVED

| Distance Moved | Low-Skilled | Female | Young (16–25) | Young, Male, and Low-Skilled | N   |
|----------------|-------------|--------|---------------|-----------------------------|-----|
| ≤100 km        | .000***     | -.000** | .000          | .000*                       | 33,821 |
|                | (.000)      | (.000) | (.000)        | (.000)                      |     |
| ≤500 km        | .000***     | -.000  | -.000         | -.000                       | 60,064 |
|                | (.000)      | (.000) | (.000)        | (.000)                      |     |
| ≤1,000 km      | .000***     | .000   | -.000**       | -.000**                     | 76,231 |
|                | (.000)      | (.000) | (.000)        | (.000)                      |     |
| ≤1,500 km      | .000***     | .000   | -.000**       | -.000**                     | 85,204 |
|                | (.000)      | (.000) | (.000)        | (.000)                      |     |

**Note.** Marginal effects from separate probit estimations of the probability of a migrant to be either low educated, female, young, or young/male/unskilled in response to the local labor demand shock in a migrant’s origin in t−1 by year of migration and distance cutoff moved. Standard errors are robust. All regressions include dummies for the state of origin.

* Significance at the 10% level.
** Significance at the 5% level.
*** Significance at the 1% level.

### TABLE A2
TWO-STAGE LEAST SQUARES (2SLS) ESTIMATIONS OF HOMICIDE RATES ON IN-MIGRATION RATES IN 2005–10, SECOND-STAGE RESULTS

| Log(Homicide Rate) | 2SLS | LIML (Individual Shares as Instruments) |
|--------------------|------|-------------------------------------|
|                    | Microregião | State | LIML | (1) | (2) | (3) | (4) |
| Log(in-migration)  | .949*** | .949*** | .949*** | .40*** |
|                    | (.211)  | (.286)   | (.211)  | (.22)  |
| Controls           | Yes    | Yes      | Yes    | Yes    |
| Microregião fixed effects | Yes  | Yes      | Yes    | Yes    |
| Year fixed effects | Yes    | Yes      | Yes    | Yes    |
| State-year fixed effects | Yes  | Yes      | Yes    | Yes    |
| Observations       | 3,300  | 3,300    | 3,300  | 3,300  |
| Kleibergen-Paap F-test | 24.9 | 17.0     | 24.88  |       |
| AR test for instrument redundancy (p-value) | .000 | .000  |
| Underidentification test (p-value) | .007 | .132 |
| Overidentification test (p-value) |       |         | 1.000  |       |

**Note.** All estimations are weighted by the microregião population. Controls are the local labor demand shock in t−1 and population. In col. 1, standard errors are clustered at microregião level and in col. 2 at state level. Columns 3 and 4 present results from a limited information maximum likelihood (LIML) estimation using the interaction of migration shares and manufacturing shocks at origins as instrumental variables (IVs; col. 3), the other using only the individual migration shares as IVs (col. 4), as suggested by Goldsmith-Pinkham, Sorkin, and Swift (2020). In this case, I can test for overidentification to confirm that all shares are exogenous, confirmed by the p-value in the last row. AR = Anderson-Rubin.

*** Significance at the 1% level.
### TABLE A3
**TWO-STAGE LEAST SQUARES (2SLS) ESTIMATIONS OF IMPACT OF IN-MIGRATION ON HOMICIDE RATES CONTROLLING FOR DISTANCE-WEIGHTED INSTRUMENTAL VARIABLES**

|                          | Log(Homicide Rate) |
|--------------------------|--------------------|
| Log(in-migration)        | 1.690***           |
|                          | (.347)             |
| Distance-weighted IV     | .000***            |
|                          | (.000)             |
| Controls                 | Yes                |
| Microregião fixed effects| Yes                |
| Year fixed effects       | Yes                |
| State-year fixed effects | Yes                |
| Observations             | 3,300              |
| Kleibergen-Paap F-test   | 27.91              |
| p-value                  | .68                |

**Note.** The estimation is weighted by the microregião population and is a 2SLS estimate. Controls are the local labor demand shock in \( t-1 \) and population. Standard errors are clustered at the microregião level. The \( p \)-value comes from a test of difference to the main estimate with bootstrapped standard errors. *** Significance at the 1% level.

### TABLE A4
**ROBUSTNESS CHECK: MISSPECIFICATION AND OVERIDENTIFICATION**

| Estimator                                         | No Controls | With Controls | Difference | Overidentification Test |
|---------------------------------------------------|-------------|---------------|------------|-------------------------|
| Two-stage least squares                           | 1.04        | .94           | [.23]      |                         |
|                                                   | [.43]       | [.37]         |            |                         |
| Modified bias-corrected two-stage least squares   | .19         | .21           | [.34]      |                         |
|                                                   | [.08]       | [.09]         |            |                         |
| Limited information maximum likelihood            | .35         | .40           | 1332.64    |                         |
|                                                   | (.00)       | (.22)         |            |                         |

**Note.** Shown are a variety of estimates of the elasticity of homicide rates. Each row presents a different estimation method. In the first column, no controls were included, and they were added in col. 2. Column 3 presents the \( p \)-value of the test of equality between estimates from the models with and without controls. Last, in col. 4, we present the overidentification test statistic and corresponding \( p \)-value. All estimations are weighted by population, and controls are the local labor demand shock in \( t-1 \) and population. The first row estimate is the main model as applied in the paper. The second row (MBTLSLS) presents results of the estimator of Anatolyev (2013) and Kolesár et al. (2015).
Appendix B
Additional Figures

Figure B1. Correlation of homicide and in-migration rates, 2005–10.
Figure B2. Map of average homicide rates, 2005–10.
Figure B3. Map of in-migration rates, 2005–10.

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