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Does Urbanization Cause Crime? Evidence from Rural-Urban Migration in South Africa

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Keywords:
Crime; Migration; South Africa; Urbanization

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Working Paper

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1 Introduction

Crime imposes large welfare costs on society (Johnston, Shields, and Suziedelyte 2018). In addition to diverting private and public resources away from productive activities, crime has been shown to negatively affect private investment (Lacoe, Bostic, and Acolin 2018), labor market earnings (Bindler and Ketel 2022), educational attainment (Brown and Velasquez 2017), birth outcome (Currie, Mueller-Smith, and Rossin-Slater 2022; Grossman and Khalil 2022), mental health (Alloush and Bloem 2022; Dustmann and Fasani 2016), and economic mobility (Sharkey and Torrats-Espinosa 2017). While it often ranks as a top concern for people in many countries, crime is mostly an issue in the developing world.[1] Indeed, of nearly half-a-million homicides committed globally in 2017, only 5% occurred in Europe while 36% occurred in Africa, and 37% in America (UNODC 2019).[2] A similar pattern appears for thefts and violence, which have increased in Africa over the last decade (Dijk, Nieuwbeerta, and Joudo Larsen 2021). Then, crime cost disproportionately weighs on developing countries and may ultimately represent a substantial share of their GDP (Global Peace Index 2020).

A long standing thought is that crime is connected to city size (Wirth 1938; Haynes 1973). In the US, Glaeser and Sacerdote (1999) and O'Flaherty and Sethi (2015) show that crime rates are higher in big cities than in either small cities or rural areas. On the contrary, for non-US high income countries, Ahlfeldt and Pietrostefani (2019) highlight a negative effect of population density on homicide rate. Causal evidence for developing countries remains, however, particularly scant. This is surprising given the strong urbanization growth experienced by these countries over the last 60 years (Moriconi-Ebrard, Heinrigs, and Trémolières 2020).

The present paper aims to start filling this gap by investigating the impact of urbanization on crime in South Africa. This question is of particular importance as South Africa stands as one of the most crime-prone country in the world (Global Peace Index 2020), and it has experienced a sustained growth of its urbanization rate since the 1990's and the end of the Apartheid regime (UN 2015).[3]

Our empirical analysis builds on an original dataset. We combine land use data with spatial population data to recover a yearly measure of urban population at the municipality level over the 2011-2018 period. For identification, we rely on the fact that urbanization in Sub-Saharan Africa has been partially driven by climate shocks through rural-urban migration (Barrios, Bertinelli,

[1] In half of countries surveyed in the 2017 World Value Survey, more than 10% of respondents report that one of their relatives has been a victim of crime over the last year.

[2] America's high rate is partly due to South American countries such as Brazil, Colombia, Mexico, or Venezuela, for instance.

[3] Estimates suggest that violence costs about 13% of its GDP annually (Global Peace Index 2020).

and Strobl 2006). More precisely, we use a shift-share instrumental variable where we combine the shifts (climate shocks at origins) with the shares (previous migration pattern to urban areas) as an instrument for urban population. We use this IV to estimate the effect of an increase in urban population on both pecuniary and non-pecuniary crime rates. We find that a one percent increase in urban population decreases pecuniary crime rate by 1.9 percent and has no effect on non-pecuniary crime rate. This estimate suggests the existence of a social multiplier effect, a well known phenomenon in the context of crime (Glaeser, Sacerdote, and Scheinkman 2003). In other words, not only are rural-urban migrants potentially less exposed than urban natives, but their arrival in city may also lower crime rate for the rest of the urban population.

We then investigate the potential transmission channels and we highlight two mechanisms: a compositional change and a network effect. Since urbanization is driven by internal migration from rural areas where people have, on average, a lower level of education, we conjecture that the negative effect of urbanization could be explained by the fact that newcomers are too poor to be targeted by criminals motivated by pecuniary gains. To test this hypothesis, we use an augmented shift-share instrumental variable allowing us to weight differently origin municipalities based on their characteristics at baseline. We show that when we over-weight the poorest migrants (those coming from geographically close and low educated rural areas) in migration flows, the magnitude of the negative effect is exacerbated, suggesting that these individuals are less exposed to pecuniary crimes than local residents. By contrast, as we over-weight the richest migrants (those coming from geographically distant and educated rural areas), the magnitude of the negative effect of urbanization on pecuniary crime decreases and ultimately becomes non-significant, suggesting that these migrants tend to be as targeted as local residents. Performing similar regressions on non-pecuniary crime, we do not find any significant effect which strengthens our interpretation of a pecuniary mechanism.

In addition, we also highlight the role of social networks which may decrease crime rate by improving social control or by easing labor market integration, for instance (Topa and Zenou 2015). Our test for this hypothesis is twofold. We first show that the negative effect of urban population density on pecuniary crime is driven by the arrival of migrants who already have a diaspora at destination, while the inflow of migrants who do not have a network at destination has no significant effect. Next, following Stuart and Taylor (2021), we show that the negative effect on pecuniary crime is stronger in cities where migrants' social connectedness is higher. Both results suggest that migrants' social network is at play.

We also investigate competitive mechanisms such as residential segregation, change in land use, and ethnic diversity. None of them appear to explain the negative effect of urbanization on pecuniary crimes.

We perform three types of robustness checks to assess the validity of our results. First, the identification assumption underlying our shift-share design is that the shifts (i.e climate shocks) are numerous and as good as random (Borusyak, Hull, and Jaravel 2022). We provide support for this assumption by transforming our baseline specification at destination into a specification looking at the effect of shifts on transformed outcomes across origins (Borusyak, Hull, and Jaravel 2022). We use this specification to check that future migration shocks are not correlated with pre-trends in outcomes and with pre-determined variables. We also test that our results are not driven by the lagged effects of past migration waves (Jaeger, Ruist, and Stuhler 2018). In addition, we check that our results are robust to controlling for climate shocks in the destination municipality. Second, another concern lies in the under-reporting of crime. In particular, we could worry that migrants have a lower reporting rate than natives which could, in turn, leads criminals to target migrants rather than natives. This would induce a non-random measurement error on the dependent variable which could potentially drive our main findings. We rely on simulations to investigate to which extent our results are sensitive to this problem. We quantify how large both migrants' under-reporting rate and migrants' victimization rate should be, relative to natives' ones, to explain our estimated effect. For instance, we find that if migrants' reporting rate was 10 percent lower than natives' one, they would have to be more exposed to crime by 8 percent to challenge our main result. We compare these sets of values with the 2016 and 2017 Victim of Crime Surveys which provide individual data on crime experience and reporting to the police. These data reveal that migrants are less exposed to pecuniary crimes than natives while being as likely to report them to the police. This rules out the possibility that under-reporting of migrants is driving our results. Relatedly, one could be concerned about congestion effects if police resources do not sufficiently increase following a rise in urban population. In that case, both migrants and natives would face increasing difficulties to report crime to the police which would generate under-reporting among both groups. To rule out this concern, we show that stronger urban population growth is not correlated with a lower number of police officers per capita. Third, as one could worry about the consequences of measurement error of the urban population variable, we perform simulations and show that our 2SLS estimates are not exposed to a division bias. Finally, we use the Victim of Crime Surveys to check if our main result remains valid using individual data and to investigate who benefits the most from the decrease of pecuniary crime. We show that urbanization decreases the probability of victimization and that this effect is mostly driven by low-educated black individuals. Next, we also find that urbanization increases trust in neighbors. These estimates confirm our main findings and provide suggestive evidence that the social capital effect may play through an increase in social control.

In sum, our paper is the first to provide causal evidence of the impact of urbanization on crime in a developing country and to shed light on the underlying mechanisms. To that extent, we contribute to three strands of literature. First, our paper pertains to the literature on the impact of city size on crime. Initial works have mostly explored cross sectional US data and show a positive correlation of population density on crime rates (Glaeser and Sacerdote 1999). Recent evidence using panel data challenge this finding, highlighting either a non-linear relationship (Rotolo and Tittle 2006) or a negative effect (Twinam 2017). Still, these studies focus on the US, and to the best of our knowledge, evidence on developing countries remains surprisingly limited. The few exceptions we are aware of include Gaviria and Pages (2002) who provide evidence that, in Latin America, the probability of being a victim of crime is substantially higher in larger cities; Demombynes and Ozler (2005) who find, based on cross sectional South African data, that population is positively correlated with crime; and Gollin, Kirchberger, and Lagakos (2021) who show that victimization rates are slightly higher in denser areas of Sub-Saharan Africa although the difference is only significant in few countries. These studies, however, do not tackle identification concerns about city population and crime, and their estimates could therefore be exposed to endogeneity bias. By implementing a credible identification strategy, our study is therefore the first to provide causal evidence of the impact of city size on crime in a developing country.

Then, a closely related literature is the one on the impact of migration on crime. Most studies focus on the role of international migrants in developed countries, such as the US (Chalfin 2014, 2015), Italy (Bianchi, Buonanno, and Pinotti 2012), or the UK (Bell, Fasani, and Machin 2013), and usually conclude to modest effects. Using Malaysian data, Ozden, Testaverde, and Wagner (2018) find a negative effect of immigration on property and violent crimes, explained by a change in the economic environment. To the best of our knowledge, Egger (2022) is the only one investigating the role of internal migration. Focusing on Brazil, she finds that internal migration increases homicide rate in destinations through labor market effects. Our study differs from hers on at least two points: (i) our level of analysis is the municipality and we focus on urban areas while she works at the micro region level (a group of municipalities) without distinction between urban and rural areas; and (ii) we focus on rural-urban migrants while she considers all internal migrants. We therefore add to this literature by documenting the role of urbanization driven by rural-urban migration in a developing country on both pecuniary and non-pecuniary crimes.

Finally, our paper also contributes to the broader literature on crime determinants in developing countries. This literature has put much emphasis on the role of income shocks on violent crime. For instance, Sekhri and Storeygard (2014) find that a negative rainfall shock increases dowry deaths in India. More recently, Dix-Carneiro, Soares, and Ulyssea (2018) study the effect of the 1990s trade liberalization in Brazil. They find that regions exposed to larger tariff reductions experienced a temporary increase in crime, and highlight the role of labor market as an important transmission channel. Similarly, Dell, Feigenberg, and Teshima (2019) find that the Mexican manufacturing job loss induced by Chinese competition increases cocaine trafficking and violence. We contribute to this literature by demonstrating the role of two other mechanisms working through urbanization, namely the change in population composition and the role of social network.

The rest of the paper is organized as follows. Section 2 discusses the conceptual framework. Section 3 presents the data and the descriptive statistics. Sections 4 and 5 expose the empirical analysis and the results, respectively. Finally, Section 6 ends the paper with a discussion and a conclusion.

2 Conceptual framework

To inform our empirical work, this section presents the theoretical mechanisms potentially at play. In his seminal contribution, Becker (1968) considers that individuals are rational and engage in criminal activities based on a cost-benefit analysis which ultimately depends on three factors, namely (i) proceeds from crime, (ii) labor market opportunities (i.e jobs and wages), and (iii) crime deterrence (i.e the probability of apprehension and punishment level). By changing population composition and social network density, and by generating general equilibrium effects, urbanization may affect the aforementioned factors. In what follows, we detail how.

Population composition An inflow of internal migrants can change population composition, especially along the economic dimension. This could induce both positive and negative effects on crime. On the one hand, if migrants are poorer than natives, their arrival might decrease the average booty for local criminals which would lower crime rate at destination.[4] On the other hand, one could also argue that if migrants are poorer than natives, their opportunity cost of crime is lower, leading them to have a higher propensity to become criminals. As shown by Mariani and Mercier (2021), this, however, is not systematically true. Building on a model of crime with endogenous migration decisions and career choices, they demonstrate that migrants do not necessarily commit more crime than natives, even if they are poorer. This may happen when higher aggregate productivity or better institutions at destination compensate legal workers for their lower level of human capital, because it encourages self-selection of honest individuals (i.e. individuals that decide not to enter the illegal market) into migration.

Social control An increase of urban population density might also affect crime through social control, although the effect is, again, a priori unknown. For instance, Jacobs (1961) suggests that an increase of population density can reduce crime through a monitoring effect (*eyes on the street*). Importantly, she argues that this monitoring effect is more likely to arise in neighborhoods characterized by mixed land use and the presence of public infrastructure so that individuals share common interests. By contrast, urbanization could also create residential instability fueled by the

[4]Obviously, if migrants are on average richer than natives, they might increase crime returns for criminals, which would foster crime rate at destination.

arrival of migrants and potentially by the departure of local residents to different neighborhoods. As suggested by the social disorganization theory (Sampson and Groves 1989; Shaw and McKay 1942), this residential instability, and the potential increase in ethnic diversity, could weaken informal social control, thereby resulting in community disorganization and more crimes.

Social networks Relatedly, urbanization might also change social networks density, which could affect crime rate and potentially generate a multiplier effect. Here again, the mechanisms are manifold, playing either positively or negatively, and the net effect is a priori unknown. To begin with, as shown by Calvó-Armengol and Zenou (2004), social networks might help delinquents to acquire proper know-how on the crime business and then foster crime. This theory has been backed by several empirical studies demonstrating that individuals are more likely to become criminals when living in a crime-prone neighborhood (Billings, Deming, and Ross 2019; Damm and Dustmann 2014). More specifically related to our question, Dustmann and Landersø (2021) show that these spillover effects on peers are significantly stronger in neighborhoods characterized by a higher residential population density. Therefore, urbanization could foster crime rate by improving strategic complementarities between crime-prone individuals. By contrast, the impact of migration-led urbanization on network density could also dampen crime rate for at least two reasons (Stuart and Taylor 2021). First, benefiting from a network at destination could ease migrants' labor market integration through the communication of hiring opportunities (Hellerstein, Kutzbach, and Neumark 2014), thereby improving their probability to find a job and increasing their opportunity cost of crime.[5] Second, the arrival of migrants could reduce crime victimization at destination by raising network density and social capital at the community level, thereby enhancing social control (Buonanno, Montolio, and Vanin 2009; Dominguez and Montolio 2021).

General equilibrium Last but not least, the urban economics literature has highlighted various effects of population density that could also play on crime rate (Ahlfeldt and Pietrostefani 2019;

[5]Labor market effects induced by migration can also affect income or employment of other local residents. Yet, these effects are hard to sign as they could play either positively or negatively on both the number of victims and the number of criminals. What is more, the effect of migration on labor market outcomes in South Africa is itself unclear. For instance, Biavaschi et al. (2018) find that international migration has a negative impact on natives' income but not on employment rates while a report by the World Bank (2018) suggests that international migration fosters job creation in South Africa. In addition, these studies investigate the role of international migrants, and, to the best of our knowledge, there is no evidence on internal migration.

Duranton and Puga 2020).[6] First, larger cities offer both a larger pool of potential victims, which increases the expected returns of crimes, and a higher level of anonymity which lowers the probability of arrest (Glaeser and Sacerdote 1999).[7] In addition, wages tend to be higher in large cities than in small ones (Henderson, Nigmatulina, and Kriticos 2021). Finally, denser areas are often associated with higher land rents and higher commuting costs, thereby diminishing the opportunity cost of crime (Verdier and Zenou 2004). Taking these factors into account, Gaigne and Zenou (2015) develop a general equilibrium model of urban crime where city size exerts both positive and negative effects on crime rate through land, labor, and product markets. They demonstrate that crime rate ultimately increases with population because this latter raises proceeds from crime for criminals and reduces workers' disposable income due to higher urban costs.

3 Data and descriptive statistics

3.1 Data

South Africa is divided into nine provinces, 52 districts, and 234 municipalities. Our study is conducted at the municipality-year level over the 2011-2018 period. We restrict our sample to municipalities with urban populations exceeding 10,000 inhabitants in 2011 (according to the 2011 Census), leading us to a balanced sample of 182 municipalities.[8] This section describes how we build our dataset.

Urban population Measuring urban population density at a disaggregated (i.e municipal) level on an annual basis in developing countries is challenging due to the lack of data. Indeed, censuses are undertaken every decade, and surveys do not allow to reliably infer urbanization rates. We tackle this issue by relying on high-resolution spatial data. More precisely, we make use of three datasets and follow a two-step procedure. The first step consists in identifying urban areas in each municipality. We do so by combining municipal administrative boundaries with the

[6]Although existing studies mostly focus on developed countries with potentially limited external validity (Chauvin et al. 2017), there is also evidence on the consequences of urbanization in developing countries (Combes et al. 2020; Gollin, Kirchberger, and Lagakos 2021; Imbert et al. 2022).

[7]In the same vein, in the criminology literature, the routine activity theory (Cohen and Felson 1979) states that crime results from the physical convergence in space and time of potential criminals and targets. Then, by easing this convergence, population density can result in more crime.

[8]Following the 2016 election, the government undertook a redemarcation of municipalities which divided the country into 213 municipalities. In our empirical analysis, we keep consistent boundaries throughout the period and we work with the 2011 demarcation.

2010 Urban Extent Africa dataset (HarvestChoice 2015), a raster delineating urban areas in 2010 at a 1km resolution. Second, we compute the number of people living in the areas previously identified as urban using the Worldpop dataset which provides annual population data at a 100m resolution (WorldPop 2018). While other datasets exist to map population, a key advantage of Worldpop is its ability to compare data over time (Henderson, Nigmatulina, and Kriticos 2021). Since the boundaries of urban areas are fixed at baseline, one could worry that city growth could be underestimated if urban boundaries were too narrowly defined and if cities grow at the extensive margins (urban sprawl).[9] To check that our constructed variable of urban population correctly measures urban population as defined in the census, we take advantage of the 2011 Census and of the 2016 Community Survey (a large scale survey covering more than 3.3 million individuals) and we compute, for both waves, the number of individuals living in urban areas of each municipality.[10] Then we regress this urban population variable on our own measure of urban population. Results reported in Table 1 show an elasticity equal to one (col. 2), indicating a strong correlation between our urban population variable and the urban population measured in the censuses.

Table 1: Urban population, OLS Estimates

	(1) ln(Census Population)	(2) ln(Census Population)
ln(*population*)	0.789***	0.994***
	(0.049)	(0.112)
Municipality FEs		✓
Nb. Observations	364	364

Notes: Regressions performed on 182 municipalities and two years (2011 and 2016). The dependent variable is the logarithm of the urban population computed from the 2011 Census and the 2016 Community Survey. ln(*population*) is the logarithm of urban population computed from WorldPop and Urban Extent of Africa data. Standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration.

[9] This threat is partially alleviated by the fact that, in developing countries, migrants tend to settle in already built-area (Jedwab, Loungani, and Yezer 2021).

[10] A recurrent issue in census data is the undercount problem, that is the erroneous exclusion of people from the sample. This is particularly true for people living in informal dwellings or in gated communities that are difficult to enter for enumerators. To tackle this issue, the national department of statistics (Stats SA) conducted a post-enumeration survey aiming at quantifying the undercount relative to the true population and applied an adjustment factor to census data to correct it (Stats SA 2011).

Crime We rely on the South African Police Station (SAPS) database which provides yearly official statistics on total reported incidents over the 2011-2018 period at the police station level.[11] We compute two crime variables. First, we create a pecuniary crime category which includes: burglaries, common robberies, robberies with aggravated circumstances, vehicle thefts (which also includes thefts out or from motor vehicles), and shoplifting. Second, we also create a non-pecuniary crime category which covers: common assaults, aggravated assaults, and murders (which also includes attempted murders). Detailed definitions of each of these categories are provided in Table A1 in Appendix. We restrict the sample to police stations located in urban areas only and we aggregate data at the municipality level.[12] Last, in order to get a crime rate, we divide the number of crimes by the urban population computed above.

3.2 Descriptive statistics

Our data reveal that, on average, urban population increased by 15 percent over the 2011-2018 period. As shown in Figure 1a, this rise occurred in most municipalities and has been particularly strong (i.e above the median) in the South-West and in the North-East, namely in the provinces of Western Cape and Gauteng. By contrast, in the Free State province, many municipalities experienced a decline of their urban population.

Regarding violence, on average, South Africans experience 1,600 pecuniary crimes per 100,000 inhabitants, half of them being burglaries; and 1,200 non-pecuniary crimes per 100,000 inhabitants, 90 percent of them being assaults (Table 2). Interestingly, between 2011 and 2018, the evolution of pecuniary and non-pecuniary crime rates have followed different trends over time at the national level (Figure A1). On the one hand, pecuniary crime rate evolved according to an inverted U-shape, peaking in 2014 and landing at its initial level in 2018. Yet, this seeming stagnation actually hides a strong heterogeneity across municipalities. Indeed, as shown in Figure 1b, between 2011 and 2018, half of the municipalities experienced an average 21 percent rise in crime rate, while the rest of the sample experienced a decrease of the same magnitude. On the other hand, non-pecuniary crime rate steadily declined over time (Figure A1), and this decrease is quite homogeneous across municipalities (Figure 1c), although particularly strong in Free State and Limpopo provinces. These figures are pretty reassuring as they suggest that both crime variables exhibit substantial within variation, which we rely on in our empirical analysis.

[11] There are 1152 police stations in South Africa.

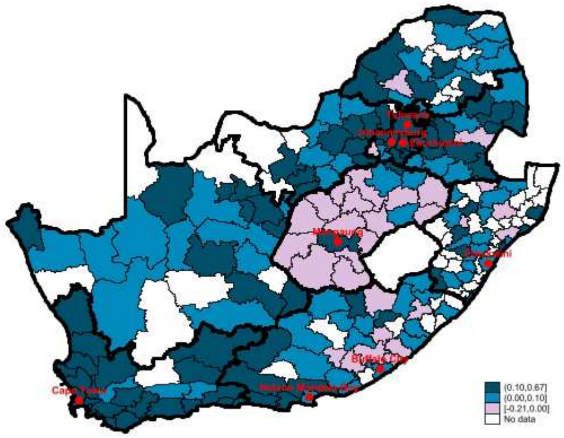
[12] 882 police stations out of 1152 are located in urban areas.

Table 2: Descriptive statistics

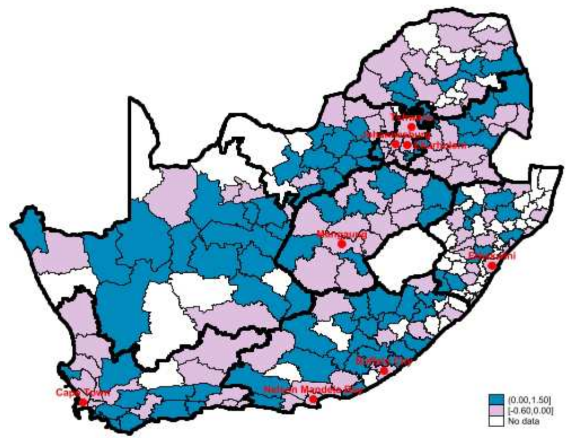
	Mean	Sd. Dev.	10^{th} per.	90^{th} per.
Population	232.48	641.44	19.85	399.98
Pecuniary crimes	1602.34	773.93	835.37	2499.65
Burglary	893.60	435.38	490.35	1397.51
Robbery	98.24	57.46	38.81	172.23
Aggravated robbery	197.07	181.85	57.93	349.53
Shoplifting	128.24	84.83	29.94	238.26
Vehicle theft	285.18	214.41	86.44	559.40
Non-pecuniary crimes	1214.01	752.34	532.40	1979.70
Assault	459.88	310.73	168.06	836.85
Aggravated assault	652.79	460.96	279.26	1116.55
Murder	101.34	136.11	36.30	164.63

Note: 10^{th} per. and 90^{th} per. are the values of the 10th and 90th percentiles, respectively. Population is expressed in thousand. Crimes are expressed per 100,000 inhabitants. Crime category definitions are available in Table A1. Source: Authors' elaboration.

(a) Urban population



(b) Pecuniary crimes



(c) Non-pecuniary crimes

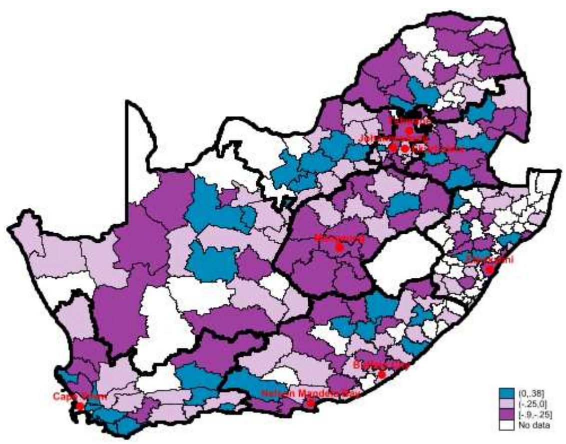


Figure 1: Log change computed between 2011 and 2018.

Notes: Province boundaries are drawn in bold black. Red dots represent metropolitan municipalities. Source: Authors' elaboration.

4 Empirical analysis

4.1 Model

The main assumption to be tested is whether an increase of urban population density affects crime rates. Our baseline model is therefore:

$$\ln(crime_{mt}) = \alpha + \beta \ln(population_{mt}) + \nu_m + \gamma_t + \varepsilon_{mt} \quad (1)$$

where the dependent variable, $\ln(crime_{mt})$, is the logarithm of either pecuniary or non-pecuniary crime rate in municipality m at year t; $\ln(population_{mt})$ is the number of inhabitants living in the urban area of municipality m at year t; ν_m and γ_t denote municipality and year fixed effects, respectively; and ε_{mt} is the error term.

Despite the inclusion of municipality and year fixed effects, which control for municipal time invariant factors and time varying common shocks, our model remains exposed to several threats to identification. First, it could suffer from an omitted variable bias if time varying factors correlated with both urbanization and crime occurrence exist. Second, our model is obviously exposed to a reverse causality issue as crime level may affect migrants' location choice. Since the effect of urbanization on crime is a priori unknown, we are unable to determine the sign of the bias. Last, since population appears also on the denominator of the dependent variable, measurement error on this variable would lead to a division bias (Farber 2015). Our identification strategy relies on a shift-share instrumental variable which we present below.[13]

4.2 A shift-share instrument

In this section, we describe the context of urbanization for the African continent and for South Africa in particular. Next, we present how we construct the instrumental variable.

Context During colonial times, natives Africans were often forbidden to live permanently in cities in eastern and southern Africa. As a result, in the 1950s, Sub-Saharan Africa was the least urbanized region of the world, with roughly 15 percent of its inhabitants living in urban centers, compared to figures above 30 percent in the rest of the developing world and around 60 percent in the developed countries (Barrios, Bertinelli, and Strobl 2006). The lift of colonial rules with decolonization triggered internal migration which then fueled urbanization. The catch-up has

[13] Another issue in crime studies lies in the under-reporting of crimes, which may induce a non-random measurement error on the dependent variable. We address this point in Section 5.4.

been dramatically fast, and urbanization currently reaches 41 percent in Sub-Saharan Africa.[14] Theoretically, rural-urban migration can be driven by both push and pull factors (see Selod and Shilpi (2021) for a review). As rural Africa is mostly dominated by rain-dependent agriculture, climate shocks have been identified as one determinant of urbanization (Barrios, Bertinelli, and Strobl 2006; Henderson, Storeygard, and Deichmann 2017).

Similarly, urbanization in South Africa has also been strongly impacted by political factors. In fact, although it was one of the first country to gain independence (in 1910), segregation laws continued to persist, especially after the adoption of the Apartheid regime in 1948, which severely restricted black population mobility. Large parts of the population were forced to live in homelands and townships leading to high population density in non-urban areas (Shilpi et al. 2018), and shortage of unskilled labor and increased wages in urban centers (Hofmeyr 1994). Restrictions were lifted in 1991, leading to migration flows which resulted in an increase of urbanization (Bakker, Parsons, and Rauch 2020).[15] As for the rest of the continent, climate shocks play an important role as a push factor. Indeed, agriculture remains one of the main activity in rural South Africa offering a livelihood to approximately six million people. Moreover, the sector remains highly dualistic with, on the one hand, a well-developed, capital-intensive, commercially integrated sector, run by white farmers; and, on the other hand, a subsistence sector, poorly irrigated and therefore heavily climate-dependent, run by black farmers (OECD 2006). Then, temperature shocks and negative excess rainfall foster out-migration, and the effect is particularly strong for black and low-income people (Mastrorillo et al. 2016).

Shift-share instrumental variable Since our objective is to identify the change in crime rates due to urban population increase, we build our instrument variable in spirit of the Bartik shift-share instrument in order to capture exogenous changes in internal migration flows to urban areas. More precisely, our instrumental variable, z_{mt}, combines previous migration pattern (the "shares") and climate shocks in the municipality of origin (the "shifts"), and is defined as:

$$z_{mt} = \sum_{o \neq m}^{O} \alpha_{om} \times T_{ot} \tag{2}$$

with α_{om} the share of migrants from origin municipality o who migrated to urban area of another municipality m,[16] and T_{ot} is the standardized annual deviation of temperature from long-term mean

[14] Although natural population increase in urban areas played a role in African city growth, Jedwab, Christiaensen, and Gindelsky (2017) estimate that roughly half of African urbanization has been driven by internal migration.

[15] Urbanization rate rose from 52 percent in 1990 to 67 percent in 2020 (World Bank data).

[16] The shares are computed from the 2011 Census, where we focus on individuals between 18 and 65 years old living in urban areas.

in municipality o at year t.[17]

Predicting urban population Our instrumental variable rests on the assumption that climate shocks at origin induce migration to urban areas. We rely on the 2011 Census to provide evidence that this is the case. We use information on the type of the current place of residence (urban or rural), the origin municipality, and the year of migration to construct a panel data of yearly out-migration flows over the 2009-2011 period. This allows us to regress the emigration rate on the temperature shock in municipality o at year t, and to consider separately emigration to urban areas and to rural areas:

$$\eta_{ot} = \beta_0 + \beta_1 T_{ot} + \nu_o + \gamma_t + \varepsilon_{ot} \tag{3}$$

where η_{ot} is the emigration rate in municipality o at year t, T_{ot} is the temperature shock, ν_o is the municipality fixed-effect, γ_t is a year fixed-effect, ε_{ot} is the error term. Results are reported in Table 3, showing that temperature shocks indeed induce out-migration (col.1), and that people tend to migrate to urban areas (col. 2) rather than to rural areas (col. 3).

[17] The mean and the standard deviation are computed over the 1900-2018 period. Temperature data are drawn from the UDel Air Temperature dataset (v5.01). A usual concern is the lack of precision of climate data, especially in Africa, due to insufficient weather stations. This threat is minimized in South Africa which is better endowed than other African countries (according to the South African Weather Service, South Africa has 243 automatic weather stations).

Table 3: Emigration rate (2009-2011), OLS Estimates

	(1) Total	(2) Urban	(3) Rural
T_{ot}	0.023**	0.023***	0.008
	(0.010)	(0.008)	(0.010)
Municipality FEs	✓	✓	✓
Year FEs	✓	✓	✓
Nb. Observations	603	603	603

Notes: Regressions performed on 201 municipalities and three years (2009-2011). The dependent variables are the logarithm of the share of individuals leaving municipality o at year t in col (1); the logarithm of the share of individuals leaving municipality o at year t and settling in urban areas in col. (2); the logarithm of the share of individuals leaving municipality o at year t and settling in rural areas in col. (3). T_{ot} is the temperature shock variable defined above. Standard errors in parentheses are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration.

Then, to check that our shift-share instrumental variable is a good predictor of urban population, we regress the urban population variable in municipality m at year t on z_{mt}, controlling for municipality (ν_m) and year (γ_t) fixed effects:

$$\ln(population_{mt}) = \alpha_0 + \alpha_1 z_{mt} + \nu_m + \gamma_t + \varepsilon_{mt} \tag{4}$$

Results reported in Table 4 show that the instrument is indeed correlated with the urban population variable. This is the first stage equation of the IV estimator we use in the rest of the paper. From column 2, a one standard deviation increase in the instrumental variable raises urban population by 0.7 percent. As a matter of comparison, the average annual growth rate of urban population is 1.3 percent (Figure A2).

Table 4: First stage, OLS Estimates

	(1) ln(*population*)	(2) ln(*population*)
z_{mt}	0.015***	0.007***
	(0.003)	(0.001)
Municipality FEs	✓	✓
Year FEs		✓
Nb. Observations	1456	1456

Notes: Regressions performed on 182 municipalities and eight years (2011-2018). ln(*population*) is the logarithm of urban population. z_{mt} is the shift-share instrumental variable. z_{mt} is standardized (its standard deviation equals one). Standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration.

Instrument validity The regressions above provide evidence that our instrument is indeed well correlated with our urban population variable. Then, the critical point lies in the exogeneity condition which has been the focus of recent works. Instrument validity can be achieved either if the shares (migration pattern at baseline) are exogenous (Goldsmith-Pinkham, Sorkin, and Swift 2020); or if the shifts (climate shocks at origin) are exogenous (Borusyak, Hull, and Jaravel 2022). In our setting, since individuals may chose their destination location based on crime prevalence and its expected evolution, the shares are likely to be endogenous. Then, the validity of our identification strategy relies on the fact that climate shocks at origin are exogenous to crime at destination. Specifically, following Borusyak, Hull, and Jaravel (2022), our instrument is consistent under two conditions: (i) shifts are quasi-randomly assigned across origins; and (ii) there are many uncorrelated shifts. Since temperature shocks are likely to be spatially correlated, we apply the equivalence result of Borusyak, Hull, and Jaravel (2022) to transform our specification. This allows us to perform validity tests while controlling for this spatial correlation across origin municipalities. Initially, our shift-share design transforms climate shocks at origin into shocks to urban population at destination via a matrix of migration patterns. The equivalence result of Borusyak, Hull, and Jaravel (2022) allows to invert this transformation and estimate, at the origin-level, the effect of climate shocks on crime rate in the typical destination (i.e on a weighted average of crime rate using migration patterns as weights). More formally, dependent variables are transformed such as: $\widetilde{y}_{ot} = \sum_m \alpha_{om} y_{mt}$, where α_{om} is the share of migrants from origin municipality o who migrated to municipality m (from the 2011 Census). Following Borusyak, Hull, and Jaravel (2022), we use this specification to discuss identification. First, we check that shocks over the 2011-2018 period are not correlated with pre-determined variables (Table B1) and with pre-trend in outcomes between 2008 and 2010 (Table B2). We also check that past migration does not influence current outcomes (a concern raised by Jaeger, Ruist, and Stuhler (2018)). We do so by checking that the sum of lagged shocks (2001-2010) does not explain later change in outcomes (Table B3). Next, we show that our main results are robust to controlling for spatial auto-correlation of temperature shocks (Table B4). In addition, in Section 5.4, we show that controlling for temperature shocks at destination does not affect our results. Last, as suggested by Borusyak, Hull, and Jaravel (2022), we also check that the Herfindahl index of origin contributions is small, so that the effect is not driven by a few origins (Table B5).[18]

5 Results

This section presents our main empirical results, namely the impact of urbanization on crime, and an investigation of underlying mechanisms.

5.1 Baseline results

Table 5 reports OLS estimates for both pecuniary and non-pecuniary crimes under alternative specifications where we successively add year and municipality fixed effects. Full specifications are reported in columns 3 and 6 for pecuniary and non-pecuniary crimes, respectively. Estimates suggest that population is negatively correlated with the former such that an increase of 1 percent in urban population is associated with a 0.6 percent decrease in pecuniary crime rate. On the contrary, the estimated coefficient for non-pecuniary crimes (col. 6) is much smaller in magnitude and non-significant, suggesting that urbanization does not affect non-pecuniary crimes.

[18]The Herfindahl index of origin contributions, denoted λ_m, is defined as: $\lambda_m = \sum_{o \neq m}^{O} \gamma_{mo}^2$ where γ_{mo} is the share of migrants in municipality m coming from municipality o.

Table 5: Baseline model, OLS Estimates

	(1) Pecuniary	(2) Pecuniary	(3) Pecuniary	(4) Non-Pecuniary	(5) Non-Pecuniary	(6) Non-Pecuniary
ln(*population*)	0.002	-0.449***	-0.641***	-0.227***	-1.057***	-0.179
	(0.026)	(0.129)	(0.166)	(0.028)	(0.119)	(0.209)
Municipality FEs		✓	✓		✓	✓
Year FEs	✓		✓	✓		✓
Nb. Observations	1456	1456	1456	1456	1456	1456

Notes: Regressions performed on 182 municipalities and eight years (2011-2018). The dependent variables are the logarithm of pecuniary crime rate (col. 1-3) and the logarithm of non-pecuniary crime rate (col. 4-6). ln(*population*) is the logarithm of urban population. Standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration.

We then turn to IV estimates, reported in Table 6, where urban population is now instrumented with the shift-share instrumental variable. Results are qualitatively unchanged, namely we find that urbanization negatively affects pecuniary crime rate while it has no effect on non-pecuniary crime rate. Yet, the magnitude of the estimated coefficient increases as we find that a one percent increase in population density lowers pecuniary crime rate by 1.9 percent.[19] This estimate is consistent with existing research that acknowledges the substantial social multiplier effects associated with the determinants of crime (Glaeser, Sacerdote, and Scheinkman 2003). For instance, Ozden, Testaverde, and Wagner (2018) find that the elasticity of violent crimes to international migration in Malaysia is -1.8. Estimating the model on each crime sub-category (Table C2), we find that this effect mainly operates through burglaries, shoplifting, and thefts of vehicles. We explore the potential mechanisms in the next sub-section.[20]

[19] In Table C1, we split the sample along population size at baseline (below and above the median) and conclusions remain the same.

[20] The difference between the magnitude of OLS and IV estimates is discussed in Section 6.

Table 6: Baseline model, IV Estimates

	(1) Pecuniary	(2) Non-Pecuniary
ln(*population*)	-1.891***	-0.421
	(0.610)	(0.559)
Municipality FEs	✓	✓
Year FEs	✓	✓
Nb. Observations	1456	1456
KP F-test	20.45	20.45

Notes: Regressions performed on 182 municipalities and eight years (2011-2018). The dependent variables are the logarithm of pecuniary crime rate (col. 1) and the logarithm of non-pecuniary crime rate (col. 2). ln(*population*) is the logarithm of urban population. Standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration.

5.2 Transmission channels

In the previous section, we provided evidence that urban population has a negative effect on pecuniary crime rate and no effect on non-pecuniary crime rate. We now highlight two mechanisms at play, namely the change in population composition, and the role of social networks.

Population composition A first explanation lies in the change of population composition induced by rural-urban migration. If South African rural-urban migrants are too poor to represent a benefit for pecuniary-motivated criminals, they may not be targeted as potential victims and their arrival may, therefore, reduce crime rate at destination.[21] We use two features to test for

[21] The underlying assumption is that victimization is positively correlated with education. We check that this is the case using the 2016 Community Survey. We create a pecuniary crime victimization variable which takes the value one if the urban household experienced at least one of the following crimes over the last 12 months: home robbery, housebreaking, robbery, or theft of a motor vehicle; and zero otherwise. We also compute a murder victimization variable taking the value one if the household experienced a murder over the last 12 months and zero otherwise. Both variables are regressed on the education level of the household head and municipality fixed effects. Results are reported in Figure D1 and show that the probability of experiencing a pecuniary crime is positively correlated with the education level of the household head. By contrast, only the most educated households (post-graduate) are slightly less exposed to murders.

this hypothesis, namely educational and geographical distances between origin and destination municipalities. First, in line with the migration literature, we assume that migrants bear migration costs that positively depend on the distance between the origin and destination municipalities (Bakker, Parsons, and Rauch 2020). Then, everything else being constant, the farther away the origin municipality, the higher migrants' labor market earnings should be, and the more they will be exposed to criminal activity. Second, more educated migrants should get higher earnings from legitimate activity, and should therefore be more exposed to criminal activity. In sum, we expect the poorest migrants, that is those originating from geographically close and low educated areas, to be much less exposed to pecuniary crimes than the richest migrants, that is those originating from geographically distant and more educated areas.

We rely on Alesina, Harnoss, and Rapoport (2016) and Docquier et al. (2020) and expand our shift-share instrumental variable by adding two group weight variables to account for both the geographical (d_{om}) and the educational (e_{om}) distances between destination and origin municipalities. Our augmented shift-share instrument then writes as:

$$z_{mt}^{A} = \sum_{o=1}^{O} \alpha_{om} \times T_{ot} \times d_{om} \times e_{om} \tag{5}$$

where:

$$d_{om} = 2 \Big/ \left(1 + e^{-(\theta_1 * D^d_{om})}\right) \tag{6}$$

and

$$e_{om} = 2 \Big/ \left(1 + e^{-(\theta_2 * D^e_{om})}\right) \tag{7}$$

with θ_1 and θ_2 ranging between -10 and +10; D^d_{om} is the standardized geographical distance between destination and origin municipalities; and D^e_{om} is the standardized difference of high skill (secondary education and above) share between destination and origin municipalities (from the 2011 Census).[22] When θ_1 increases, for a given θ_2, the instrument overweights migrants from geographically distant municipalities. When θ_2 increases, for a given θ_1, it overweights migrants from educationally distant (poorly educated) origins.

One could be concerned that more educated rural areas do not necessarily send more educated migrants. To check that this is not the case, we investigate how population composition at destination changes in response to migration flows when we use different values of θ_2 (weight to educational distance). Using the 2011 Census and the 2016 Community Survey, we regress the share of individuals pertaining to different education level categories (ζ_{mt}) in the destination municipality on urban population instrumented by the augmented shift-share:

[22] When $D^e_{om} = 0$, origin and destination municipalities have the same share of high skilled people. Positive values of D^e_{om} mean that the origin municipality has a lower share of high skilled people than the destination municipality.

$$\zeta_{mt} = \alpha_0 + \alpha_1 \ln(population_{mt}) + \nu_m + \gamma_t + \varepsilon_{mt} \tag{8}$$

We show, in Table 7, that when we over-weight low-educated origin municipalities (θ_2 = 10), an increase of urban population lowers the average level of education at destination. On the contrary, average education levels are not affected by migration flows when we over-weight people from similarly educated origin areas (θ_2 = -10). This validates our augmented shift-share instrument.

Table 7: Test of the augmented shift-share, IV Estimates

	$\theta_1 = 0$, $\theta_2 = 10$			
	(1)	(2)	(3)	(4)
	No schooling	Primary	Secondary	Higher
ln(*population*)	0.067	0.144***	-0.352***	-0.088**
	(0.043)	(0.045)	(0.068)	(0.034)
KP F-test	23.14	23.14	23.14	23.14
Nb. Observations	364	364	364	364
	$\theta_1 = 0$, $\theta_2 = -10$			
	(1)	(2)	(3)	(4)
	No schooling	Primary	Secondary	Higher
ln(*population*)	-0.070	0.056	-0.156	-0.035
	(0.068)	(0.069)	(0.101)	(0.043)
KP F-test	17.69	17.69	17.69	17.69
Nb. Observations	364	364	364	364
Mean dep. var.	0.10	0.28	0.48	0.07
Municipality FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓

Notes: Regressions performed on 182 municipalities and two years (2011 and 2016). The dependent variables are the shares of individuals in each category in the destination municipality. ln(*population*) is the logarithm of urban population. θ_1 is the parameter associated to the geographical distance, and θ_2 is the parameter associated to the educational distance. Standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration.

We estimate our baseline model (Equation 1) using the augmented shift-share instrumental variable for each integers combination of θ_1 and θ_2 (leading to 442 regressions). Results for pecuniary crimes are reported in Figure 2. Each cell represents the estimated coefficient of interest obtained from a single regression with a particular combination of θ_1 (geographical distance varying vertically) and θ_2 (educational distance varying on the horizontal axis). Darker (lighter) cells correspond to a smaller (larger) effect of urban population on pecuniary crime rate, significant at the 5 percent level or below. Black cells represent coefficients with a p-value above 5 percent. Moving South on the graph implies over-weighting migrants from geographically close municipalities; while moving East implies over-weighting migrants from municipalities with a high share of low-skilled people. We see that as people come from geographically distant and highly educated rural areas (North-West on the graph), the magnitude of the negative effect of urbanization on pecuniary crime rate decreases and becomes non-significant. This finding supports our hypothesis that when migrants are similar to the native urban population, they are equally targeted by criminals, and therefore increasing urban population does not significantly change crime rate. By contrast, when migrants come from geographically close and less educated rural areas (South-East), the negative effect is magnified, suggesting that low earning migrants are less likely to be targeted by criminals. For completeness, some of the regressions are reported in Table E1, and we show that the IV remains a good instrument even for extreme values of θ_1 and θ_2.

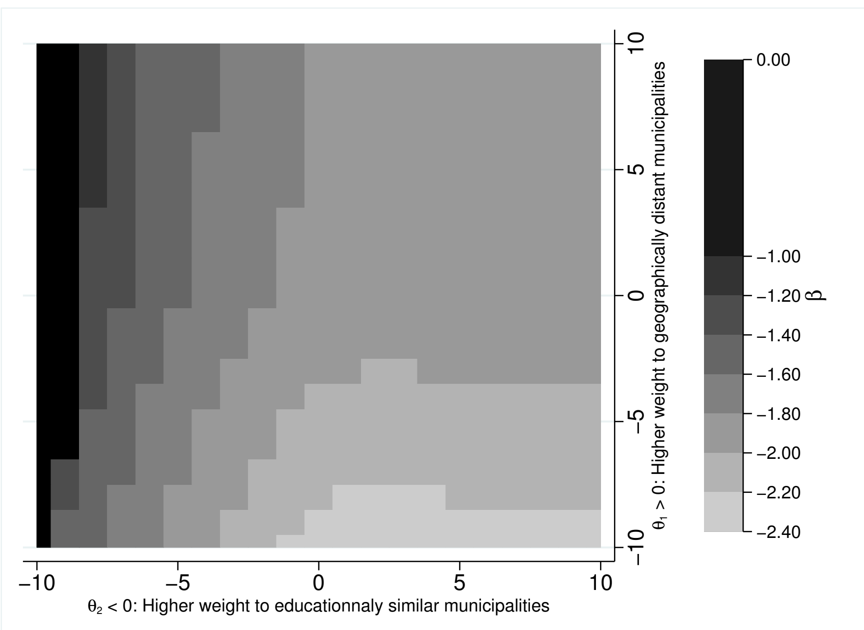


Figure 2: Pecuniary crimes, IV Estimates

Notes: Regressions performed on 182 municipalities and eight years (2011-2018). The dependent variable is the logarithm of pecuniary crime rate. Each cell represents the coefficient of ln(*population*) estimated by 2SLS with the augmented shift-share as an IV and for different values of θ_1 (on the y-axis) and θ_2 (on the x-axis). Source: Authors' elaboration.

Finally, we perform the same regressions for non-pecuniary crimes. We show that none of the estimates is significant (Table E2). Two comments are in order. First, this result strengthens our interpretation that the profile of migrants matters for pecuniary crimes through a crime return effect. Second, one could have been concerned that the results documented in Figure 2 reflect a shift from one crime category to another. More precisely, it could have been possible that the poorest migrants commit more violent pecuniary crimes such that a robbery would be classified as an assault, for instance. In that case, a stronger negative effect on pecuniary crimes could be counter-balanced by a positive effect on non-pecuniary crimes. This is not the case.

Social network A second explanation lies in social networks. As discussed in Section 2, they could lower crime rate by easing migrants' labor market integration or by improving social control. Our test for this mechanism is twofold. First, we investigate whether migration flows have a different effect on crime rates depending on whether migrants already have a diaspora at destination. A municipality is considered to have a diaspora in another city if at least one migrant living there in 2011 comes from this origin municipality (based on the 2011 Census data). Then, we make use of the 2016 Community Survey, which provides migration records over the 2012-2016 period, to compute yearly migration flows which we separate in two groups namely, migrants with a diaspora, and migrants without a diaspora. We estimate the effect of both migration flows on both crime variables. To ease estimates comparison, independent variables are standardized.[23] Results are reported in Table 8. A one standard deviation increase in the number of migrants having a diaspora at destination lowers pecuniary crime rate by 2.2 percent, while migrants without a diaspora at destination have no statistically significant effect (col. 3). One could argue that the lack of significant effect of migrants without diaspora could be driven by a lack of statistical power if this variable exhibits insufficient variation. To check that this is not the case, we follow Ioannidis, Stanley, and Doucouliagos (2017) and we derive the minimum detectable effect size at conventional power (80 percent) and statistical significance (5 percent) by multiplying standard errors by 2.8. From column 3, our regression is powered to detect any effect of migrants without diaspora greater than -1.4 percent (which is equal to 60 percent of the effect on pecuniary crime rate of migrants with a diaspora). Regarding non-pecuniary crimes (col. 6), the arrival of migrants does not have any significant effect, whether they have a diaspora at destination or not.

[23]Their standard deviation is equal to one.

Table 8: Social network, OLS Estimates

	(1) Pecuniary	(2) Pecuniary	(3) Pecuniary	(4) Non-Pecuniary	(5) Non-Pecuniary	(6) Non-Pecuniary
Migrants without diaspora		-0.004	-0.003		0.001	0.002
		(0.005)	(0.005)		(0.004)	(0.004)
Migrants with diaspora	-0.022**		-0.022**	-0.005		-0.006
	(0.009)		(0.008)	(0.007)		(0.007)
Municipality FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓
Nb. Observations	910	910	910	910	910	910

Notes: Regressions performed on 182 municipalities and five years (2012-2016). The dependent variables are the logarithm of pecuniary crime rate (col. 1-3) and the logarithm of non-pecuniary crime rate (col. 4-6). The independent variables are standardized. *Migrants without diaspora* is the number of migrants without a diaspora at destination; *Migrants with diaspora* is the number of migrants having a diaspora at destination. Standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration.

By design, the previous model cannot be estimated using the shift-share as an IV. Indeed, the shift-share exploits migration flows from origins that already have migrants at baseline (the shares). Then, as a second test, we follow Stuart and Taylor (2021) and we use a Herfindahl index to measure migrants' social connectedness. In practice, we compute, for each destination municipality m, the Herfindahl index as $HHI_m = \sum_{o \neq m}^{O} \gamma_{mo}^2$ where γ_{mo} is the share of migrants in municipality m coming from municipality o (from the 2011 Census). Then, we estimate the conditional effect of urban population on pecuniary and non-pecuniary crime rates by interacting the population variable with a dummy variable taking the value one if the municipality has a Herfindahl index above the median (strong social connectedness, i.e people come from a small number of origin municipalities), and zero otherwise. Results are reported in Table 9. We show that the negative effect of urbanization on pecuniary crime rate is stronger for municipalities hosting migrants from a small number of origins (high *HHI*) than for municipalities with a larger pool of origins. There is, however, no conditional effect for non-pecuniary crimes (col. 2).[24]

[24] We estimate the model by OLS with the continuous Herfindahl index variable instead of a dummy variable and we plot marginal effects of population on pecuniary and non-pecuniary crimes in Figures F1 and F2. Conclusions are qualitatively unchanged.

Table 9: Social network, IV Estimates

	(1) Pecuniary	(2) Non-Pecuniary
ln($population$)	-0.925*	-0.153
	(0.500)	(0.451)
ln($population$) $\times$ HHI	-0.884**	-0.245
	(0.430)	(0.403)
Municipality FEs	✓	✓
Year FEs	✓	✓
Nb. Observations	1456	1456
KP F-test	15.46	15.46

Notes: Regressions performed on 182 municipalities and eight years (2011-2018). The dependent variables are the logarithm of pecuniary crime rate (col. 1) and the logarithm of non-pecuniary crime rate (col. 2). ln($population$) is the logarithm of urban population. HHI is a dummy variable taking the value one if the Herfindahl index is above the median and zero otherwise. Standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration.

In sum, we find that the negative effect of urbanization on pecuniary crime is driven by the arrival of migrants who already have a diaspora at destination, and the magnitude grows with migrants' social connectedness. Both results suggest that social networks play a role in decreasing pecuniary crime rate.

5.3 Alternative mechanisms

In this section, we test for alternative mechanisms that may potentially be at play.

Residential segregation As shown by Card, Mas, and Rothstein (2008) in the US context, a minority inflow might lead local residents to cluster in some neighborhoods. In that case, the decrease of pecuniary crime rate could be driven by the fact that the richest local residents move either to gated communities that are safer, or to other municipalities. To test this hypothesis, we rely on the 2016 Community Survey which provides information, over the 2012-2016 period, on respondents' previous place of residence and the year they move. This allows us to compute, for each municipality, the annual rate of individuals who changed their place of residence (whether they stayed in the same municipality or they moved to a different one). Then, we run our baseline model controlling for the annual share of local residents changing their place of residence. Results are reported in Table 10 below. None of the control variables added in the model significantly impacts the magnitude of the population effect with respect to our baseline specification. This suggests that neither migration within municipality nor migration to a different municipality explain the negative effect of urban population density on pecuniary crime.[25]

Table 10: Residential segregation, IV Estimates

	(1) Pecuniary	(2) Pecuniary	(3) Pecuniary	(4) Non-Pecuniary	(5) Non-Pecuniary	(6) Non-Pecuniary
ln(*population*)	-2.026***	-2.311***	-2.324***	-0.186	-0.222	-0.219
	(0.628)	(0.702)	(0.709)	(0.506)	(0.540)	(0.550)
Change residence (total)		-0.069***			-0.009	
		(0.023)			(0.017)	
Change residence (other city)			-0.076**			-0.007
			(0.033)			(0.024)
Change residence (same city)			-0.063**			-0.010
			(0.028)			(0.023)
Municipality FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓
KP F-test	25.51	26.64	26.11	25.51	26.64	26.11
Nb. Observations	910	910	910	910	910	910

Notes: Regressions performed on 182 municipalities and five years (2012-2016). The dependent variables are the logarithm of pecuniary crime rate (col. 1-3) and the logarithm of non-pecuniary crime rate (col. 4-6). ln(*population*) is the logarithm of urban population. *Change residence (total)* is the share of people changing their place of residence; *Change residence (other city)* is the share of people moving to another city; *Change residence (same city)* is the share of people changing their place of residence within the same municipality. Standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration.

City organization According to Jacobs (1961), urbanization may reduce crime through an increase of social control. As she suggested, this latter can be even more efficient in presence of mixed land use, that is when neighborhoods mix commercial and residential buildings. It is therefore possible that urbanization, by changing cities' organization, fosters social control and reduces crime rate. To test this hypothesis, we use the South African National Land Cover 2014 and 2018 Change Assessment dataset which provides information on land use type (i.e residential, smallholding, commercial, industrial) at a resolution of 30m. We then build a grid of 500m $\times$ 500m

[25] Similarly, using the 2011 Census and the 2016 Community survey, we also control for the share of people living in cluster houses (gated communities). Results are reported in Table G1 and our conclusions remain the same.

which allows us to create neighborhoods within each city. Following Alesina and Zhuravskaya (2011), we compute an index of segregation, applied to land use, at the municipality level for 2014 and 2018 as:

$$\text{Land use segregation index}_{mt} = \frac{1}{G_m - 1} \sum_{g=1}^{G_m} \sum_{j=1}^{J_m} \frac{a_m^j}{A_m} \frac{(\pi_{mt}^{jg} - \pi_{mt}^{g})^2}{\pi_{mt}^{g}} \tag{9}$$

where G_m is the total number of land use type groups, A_m is the surface area of municipality m, a_m^j is the surface area of neighborhood j in municipality m, and J_m is the total number of neighborhoods in municipality m. π_{mt}^{g} is the fraction of land use type g in municipality m at year t, and π_{mt}^{jg} is the fraction of land use type g in neighborhood j of municipality m at year t. Estimates are reported in Table 11. Controlling for land use segregation does not alter the estimated coefficient of interest.[26]

Table 11: Land use segregation, IV Estimates

	(1) Pecuniary	(2) Pecuniary	(3) Non-Pecuniary	(4) Non-Pecuniary
ln(*population*)	-2.967***	-2.945***	-0.132	-0.112
	(0.508)	(0.508)	(0.322)	(0.320)
Land use segregation index		0.066		0.060
		(0.162)		(0.141)
Municipality FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
KP F-test	26.39	27.49	26.39	27.49
Nb. Observations	364	364	364	364

Notes: Regressions performed on 182 municipalities and two years (2014 and 2018). The dependent variables are the logarithm of pecuniary crime rate (col. 1-2) and the logarithm of non-pecuniary crime rate (col. 3-4). ln(*population*) is the logarithm of urban population. Land use segregation index is computed at the municipal-year level for four occupation groups (residential, commercial, smallholding, industrial). Standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration.

[26]One could worry that the time period under consideration (i.e 2014 and 2018) is too short to observe an effect. It is worth underlying that, over this period, urbanization has indeed induced changes in land use materialized through an increase of residential and commercial units and a decrease of industrial units (Table G2).

Ethnic diversity The population composition mechanism highlighted above suggests that urbanization affects pecuniary crimes through crime returns. Yet, one could argue that urbanization not only changes population composition along the economic dimension but also along the ethnic one, which role has been widely investigated for conflicts (Novta 2016) and violent crimes (De Soysa and Noel 2020). Using the 2011 Census and the 2016 Community Survey, we compute polarization and fractionalization indices of linguistic diversity (Montalvo and Reynal-Querol 2005), which we add as control variables (Table 12).[27] The fractionalization index has a positive and significant impact on both pecuniary and non-pecuniary crime rates (col. 2 and 5). Interestingly, the magnitude of the coefficient associated to ln(*population*) increases for both crime categories and even becomes significant for non-pecuniary crime rate, although only at the 10 percent level.[28] In any case, although change in ethnic diversity could be one of the mechanism at play through urbanization, it does not explain the negative effect documented initially.

[27] We compute these indices based on linguistic diversity (from the most often spoken language at home) rather than ethnic diversity because there are 14 different languages in South Africa while only four population groups. Both indices are defined as follows: $Fractionalization = \sum_{i=1}^{N} \pi_i(1-\pi_i)$ and $Polarization = 4\sum_{i=1}^{N} \pi_i^2(1-\pi_i)$ where π_i is the proportion of people who speak language i and N is the number of different languages.

[28] The coefficient associated to ln(*population*) is, however, not significantly different between columns 1 and 2, and between columns 4 and 5.

Table 12: Ethnic diversity, IV Estimates

	(1) Pecuniary	(2) Pecuniary	(3) Pecuniary	(4) Non-Pecuniary	(5) Non-Pecuniary	(6) Non-Pecuniary
ln(*population*)	-1.937***	-2.981***	-2.972***	-0.524	-1.156*	-1.035
	(0.492)	(0.759)	(0.950)	(0.430)	(0.622)	(0.732)
Fractionalization		1.399***			0.847**	
		(0.498)			(0.409)	
Polarization			0.736*			0.364
			(0.373)			(0.277)
Municipality FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓
KP F-test	26.57	16.69	16.91	26.57	16.69	16.91
Nb. Observations	364	364	364	364	364	364

Notes: Regressions performed on 182 municipalities and two years (2011-2016). The dependent variables are the logarithm of pecuniary crime rate (col. 1-3) and the logarithm of non-pecuniary crime rate (col. 4-6). ln(*population*) is the logarithm of urban population. *Fractionalization* and *Polarization* indices are computed based on the language most often spoken in the household (14 groups). Standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration.

5.4 Robustness

Under-reporting A common issue in crime studies lies in the under-reporting of crimes. In our case, we could be concerned that migrants have a lower reporting rate than natives. This would induce a non-random measurement error of the dependent variable, which would thus not be tackled by our instrumental variable strategy. What is more, the problem could be exacerbated if, being aware of a differentiated reporting rate, criminals choose to target migrants rather than natives. The combination of both effects could potentially explain our main result (i.e an elasticity of -1.9).[29] To examine to what extent our baseline estimate is sensitive to this concern, we rely on simulations which we perform on the 182 municipalities over the 2011-2018 period. The idea is the following. Starting from the 2011 pecuniary crime rates in our dataset, we compute what would be the evolution of this variable if the change of population was only driven by migrants who could be both less likely to report crime to the police, and more exposed to crime than local residents. We therefore end up with a simulated pecuniary crime rate variable computed at the municipal-year level over the 2011-2018 period and which depends on two parameters, namely the relative under-reporting rate of migrants (denoted r), and the relative over-victimization of migrants with

[29] Migrants' under-reporting alone could at most lead to an elasticity of -1.

respect to natives (denoted s). Next, we use this variable as the dependent variable in our main model (Equation 1) which we estimate by 2SLS for different combinations of r and s.[30] This leads us to a set of estimated coefficients measuring the impact of urban population on the simulated pecuniary crime rate. Then, we are able to identify the different combinations of r and s for which the estimates become non-statistically different from -1.9 (the elasticity we estimated in the baseline regression). Further details about how we compute this variable, how we estimate the model, and how to interpret the results are provided in Appendix H.

Results are reported in Figure 3 where the relative reporting rate of migrants (r) is on the y-axis, and the relative over-victimization of migrants (s) is on the x-axis. Each cell represents the estimated coefficient of urban population on pecuniary crime rate computed for the corresponding values of r and s. The figure only displays the estimated coefficients which are statistically different from -1.9 at the 5 percent level. Then, it provides the combinations of r and s for which the estimated coefficient becomes non-statistically significant from our main result. For instance, if migrants had a reporting rate equal to 90 percent of that of the natives ($r = 0.9$), they would need to be 8 percent ($s = 0.8$) more exposed to pecuniary crimes than natives to get an estimated coefficient not statistically different from -1.9 (see Appendix H for details about the interpretation of s). On the contrary, if migrants had a reporting rate equal to 10 percent of that of the natives ($r = 0.1$), they would need to be more exposed to crime than natives by 4 percent ($s = 0.4$).

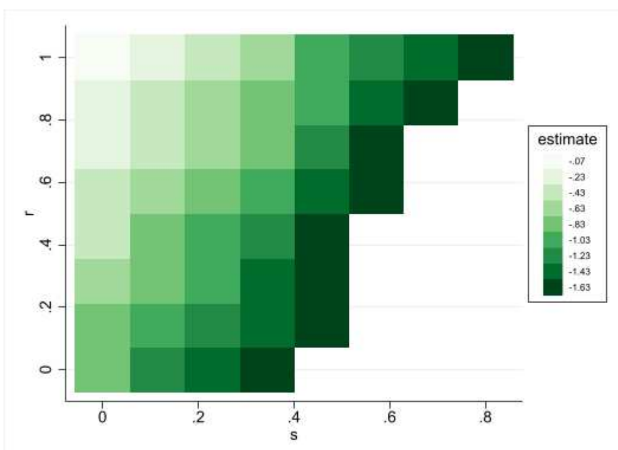


Figure 3: Simulated pecuniary crime variable, IV Estimates

Notes: Regressions performed on 182 municipalities and eight years (2011-2018). The dependent variable is the logarithm of the simulated pecuniary crime rate variable. Each cell represents the coefficient of ln(*population*) estimated by 2SLS with the shift-share as an IV. r is the relative reporting rate of migrants with respect to natives and s is the relative over-victimization of migrants with respect to natives. Source: Authors' elaboration.

[30] As in the baseline specification, ln(*population*) is instrumented by the shift-share instrument z_{mt}.

To check how these values compare to reality, we rely on the 2016/2017 and 2017/2018 Victim of Crime Surveys (VCS), which provide individual data on crime experience, crime reporting to the police, and migration background.[31] In Table 13, we estimate the impact of being a migrant on pecuniary crime experience (col. 1) and on pecuniary crime reporting to the police (col. 2).[32] Results show that being a migrant lowers the probability of experiencing a pecuniary crime by 0.014 percentage point (a 25% decrease relative to the mean). This finding is consistent with our previous estimates according to which South African migrants are, on average, poorer than urban natives, and are therefore less exposed to crime. In addition, being a migrant does not significantly affect the probability of reporting a pecuniary crime to the police. Both results are reassuring as they highlight that neither under-reporting nor over-victimization are a concern, suggesting that our main empirical results are unlikely to be driven by these effects.

Table 13: Crime reporting, OLS Estimates

	(1) Experience Pecuniary Crime	(2) Report Pecuniary Crime
Migrant	-0.014	-0.036
	[0.07]	[0.14]
Municipality FEs	✓	✓
Year FEs	✓	✓
Mean dep. var.	0.06	0.57
Nb. Observations	15076	861

Notes: The dependent variables are a dummy variable taking the value one if the individual experienced a pecuniary crime, and zero otherwise (col. 1); and a dummy variable taking the value one if the victim of a pecuniary crime reported it to the police, and zero otherwise (col. 2). Migrant is a dummy variable taking the value one if the individual lives in his current place for less than two years. p-values computed by wild bootstrap method (Cameron and Miller 2015; Roodman et al. 2019) for standard errors clustered at the metropolitan municipalities (8) level are reported in brackets. Source: Authors' elaboration based on VCS data.

[31] Although other waves of the Victim of Crime Surveys exist, those are the only ones enabling us to identify migrants from natives. We focus on individuals of South African citizenship and we define a migrant as someone who lived in his current place for less than two years.

[32] Since individuals are sampled from only eight metropolitan municipalities, we apply the wild bootstrap cluster method to correct for the small number of clusters (Cameron and Miller 2015; Roodman et al. 2019). The table directly reports p-values in parentheses.

Additionally, one could also be concerned that places with many more migrants get less attention from the police. This could be the case if the number of police officers does not sufficiently increase in response to urban population growth. Then, both natives and migrants could face longer time response from the police which could discourage them and leads to under-reporting of crime. To check whether this is the case, we investigate the impact of the urban population growth between 2011 and 2016 on the number of police officers per capita in 2016.[33] Results are reported in Table 14 and show that although bigger cities in 2011 tend to have more police officers per capita in 2016, urban population growth between 2011 and 2016 does not explain this latter.

Table 14: Police officers, OLS Estimates

	(1) Police officers pc	(2) Police officers pc	(3) Police officers pc
$\Delta \ln(population)_{2011-2016}$		0.789	0.275
		(0.877)	(0.839)
$\ln(population)_{2011}$	0.180***		0.177***
	(0.063)		(0.063)
Nb. Observations	182	182	182

Notes: Regressions performed on 182 municipalities. The dependent variable is the logarithm of the number of police officers per 100,000 inhabitants in municipality m in 2016. $\Delta \ln(population)_{2011-2016}$ is the difference of the logarithm of the urban population between 2011 and 2016. $\ln(population)_{2011}$ is the logarithm of the 2011 urban population. Standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration.

Local temperature Existing studies have shown that temperature may affect crime (Heilmann, Kahn, and Tang 2021; Ranson 2014). Then, we could worry that our instrumental variable could be correlated with temperature shocks at destination which would violate the exogeneity condition. To check that this is not the case, we estimate our baseline model controlling for temperature shocks at destination. Results are reported in Table 15 and our conclusions remain unchanged.

[33] Data on the number of police officers is only available for 2016.

Table 15: Robustness to temperature shocks at destination, IV Estimates

	(1) Pecuniary	(2) Non-Pecuniary
ln(*population*)	-1.963***	-0.407
	(0.622)	(0.570)
T_{mt}	0.003	-0.001
	(0.007)	(0.006)
Municipality FEs	✓	✓
Year FEs	✓	✓
Nb. Observations	1456	1456
KP F-test	20.00	20.00

Notes: Regressions performed on 182 municipalities and eight years (2011-2018). The dependent variables are the logarithm of pecuniary crime rate (col. 1) and the logarithm of non-pecuniary crime rate (col. 2). ln(*population*) is the logarithm of urban population. T_{mt} is the temperature shock in municipality m at year t. Standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration.

Measurement error and Division bias A potential concern is that population appears both on the right and left-hand side of our model, as our variable of interest is also the denominator of the dependent variable. Measurement error in this variable would therefore generate a downward bias in the OLS estimates, known as the division bias (Farber 2015). To check that our instrumental variable strategy tackles this issue we perform simulations. We generate a noisy population variable from our observed population and a random noise. We investigate to what extent an increase in measurement error affects the estimated coefficient. Results, using pecuniary crime rate as the dependent variable, are reported in Table 16. OLS estimates are indeed exposed to the division bias as an increase of measurement error on the population variable lowers the estimated coefficient. By contrast, the 2SLS estimates are not sensible to the measurement error, confirming that our estimates are not exposed to the division bias problem.

Table 16: Division Bias, Simulations

	(1)	(2)	(3)	(4)	(5)	(6)
		OLS			2SLS	
	τ=0.01	τ=0.05	τ=0.1	τ=0.01	τ=0.05	τ=0.1
$\ln(\tilde{pop})$	-0.67***	-0.87***	-0.96***	-1.89***	-1.88***	-1.76**
	(0.16)	(0.08)	(0.04)	(0.61)	(0.67)	(0.83)
Municipality FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓

Notes: The noisy population variable ($\tilde{pop}$) is generated from the observed population variable plus a random noise drawn from a normal distribution of mean 0 and of standard deviation $\tau \times population$. The dependent variable is the logarithm of pecuniary crime rate based on the noisy population parameter. Estimates based on 500 replications. *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration.

5.5 Individual data

Finally, we rely on the 2015-2018 Victim of Crime Surveys data to perform household-level analysis.[34] This allows us to check that our results remain valid using victimization data, and to investigate who benefits the most from the decrease of pecuniary crimes. Additionally, we partially uncover the mechanisms behind the role of social networks using a question on trust in neighbors. In practice, we estimate the following model by 2SLS using the shift-share variable as an instrument for urban population:

$$y_{imt} = \alpha + \beta \ln(population_{mt}) + \nu_m + \gamma_t + \varepsilon_{imt} \tag{10}$$

where y_{imt} is either a victimization variable taking the value one if the household i has experienced a pecuniary crime over the last year, and zero otherwise;[35] or a trust variable taking the value one if the household head answered positively to the following question: "Would you ask your next-door neighbour to watch your house for you if you were going away", and zero otherwise. ν_m and γ_t are municipality and year fixed-effects, respectively.

Results are reported in Table 17. We first estimate the impact of urbanization on victimization using the full sample. Column 1 confirms our main finding as a one percent increase in urban

[34]We can only use households living in metropolitan municipalities (i.e Buffalo, Cape town, Ekurhuleni, eThekwini, Johannesburg, Mangaung, Nelson Mandela Bay, and Tshwane). Other households are only located at the district level.

[35]The pecuniary variable takes the value one if the household has experienced any of the following crime over the last year: burglary, home robbery, and theft of motor vehicle; and zero otherwise.

population decreases the probability of victimization by 0.7 percentage point. Next, we investigate whether the effect materializes on a particular population group. We find that the effect is mostly driven by a decrease of victimization among black (col. 2) rather than non-black people (col. 3) where the estimate is not statistically significant. More precisely, low-educated black individuals, that is people with at most secondary level of education, are the ones who benefit the most from the dampening effect of urbanization on crime. Indeed, a one percent increase in urban population decreases their probability of victimization by 1.1 percentage point (col. 4). This is expected as rural individuals migrating following a climate shock are more likely to belong to this population group (Mastrorillo et al. 2016). On the contrary, urbanization has a small positive effect on victimization of high-educated black individuals. This suggests that the decrease in victimization on low-educated black individuals was not compensated by an increase in victimization among high-educated ones. Finally, we find that urbanization tends to increase trust in neighbors (col. 6-8), and, this effect is again driven by low-educated black individuals (col. 7). For these latter, a one percent increase in urban population increases trust in neighbors by 0.009 unit (i.e a 1.1 percent increase relative to the mean). Overall, these estimates convey the same message as our main results and provide suggestive evidence that the social network effect operates via enhanced social control.

Table 17: Robustness on VCS data, IV Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pecuniary crime					Trust		
	Full sample	Black only	Non-black	Low-ed black	High-ed black	Full sample	Low-ed black	High-ed black
ln(*population*)	-0.710	-0.772	-0.490	-1.065	0.051	0.183	0.947	0.047
	[0.04]	[0.04]	[0.17]	[0.07]	[0.08]	[0.00]	[0.00]	[0.00]
Municipality FEs	✓	✓	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓	✓	✓
Mean dep. var.	0.06	0.06	0.06	0.05	0.07	0.85	0.88	0.83
Nb. Observations	24799	18212	6587	12353	5859	24313	12137	5752
KP F-test	40.16	41.57	32.89	39.77	46.01	41.40	40.11	46.46

Notes: In columns 1 to 5, the dependent variable is a dummy variable taking the value one if the household experienced a pecuniary crime, and zero otherwise. In columns 6 to 8, the dependent variable is a dummy variable taking the value one if the household head answered positively to the question: "Would you ask your next-door neighbour to watch your house for you if you were going away". Non-black individuals include: coloured, indian/asian, and white. Low-educated individuals include people with no school to secondary completed. High-educated individuals include undergraduate and post-graduate individuals. p-values computed by wild bootstrap method (Cameron and Miller 2015; Roodman et al. 2019) for standard errors clustered at the metropolitan municipalities (8) level are reported in brackets. Source: Authors' elaboration.

6 Discussion and conclusions

This paper investigates the causal impact of urbanization on crime in South Africa. We do so by building an original dataset combining yearly information on crime and urban population density at the municipality level over the 2011-2018 period. We rely on the fact that urbanization is fueled by internal migrations that are themselves driven by income shocks, and we exploit climate shocks at origin for identification. We find that a one percent increase in urban population decreases pecuniary crime rate by 1.9 percent and has no effect on non-pecuniary crime. This coefficient suggests the existence of a social multiplier effect, a common phenomenon in the crime literature.
Our empirical analysis highlights two mechanisms at play. First, since migrants originating from rural areas differ from local residents in urban areas, their arrival in cities changes urban population characteristics at destination. In line with the Beckerian analysis, we show that this change in population composition has a direct effect on pecuniary crime rate through crime returns. Indeed, we find that over-weighting the poorest migrants in migration flows leads to a stronger negative effect of urbanization on pecuniary crime rate. By contrast, as we over-weight the richest migrants, the magnitude of the estimated effect declines. This result points to one explanation of the negative effect of population density on pecuniary crime, namely that urbanization is driven by migrants that are, on average, poorer than natives and are then less likely to be targeted by criminals for pecuniary crimes. There are, however, at least two alternative mechanisms that could challenge our interpretation. First, one could argue that the poorest migrants are more likely to be violent, such that when they commit robberies or burglaries, they would rather be classified as assaults, for instance. In that case, the decrease in pecuniary crime rate would be explained by a shift from the pecuniary crime category to the non-pecuniary one. The lack of significant effect of urbanization on non-pecuniary crime rate throughout the paper suggests, however, that this is not the case. Another concern lies in the change of place of residence of (some) local residents in response to migration flows. Controlling for the share of individuals that change their place of residence does not affect our coefficient of interest. This suggests that migration of natives to safer places either in the same municipality or to another one is not driving our main result.

Our analysis also highlights the role of social networks through two sets of regressions. We show that when migrants have a diaspora at destination, their arrival decreases pecuniary crime rate. On the contrary, an inflow of migrants without a diaspora has no effect on crime rate. Then, we show that the negative impact of urban population on pecuniary crime is strengthened in municipalities where migrants' social connectedness is higher. Both results suggest that the net effect of social networks on pecuniary crime rate is negative. The literature about the role of social networks on crime has mainly highlighted labor market and social control as potential transmission channels. While we do not rule out the role of labor market, our estimates suggest an increase of social control. Indeed, we show that the negative effect of urbanization on crime materializes through burglaries, thefts of vehicles, and shoplifting while robberies remain unaffected. These crimes are the most likely to be dampened by social control. On the contrary, if social network effects worked through the labor market channel, we would expect all pecuniary crime categories to be negatively affected. What is more, household level data points to an increase in trust in neighbors following an increase of urban population, which strengthens this conclusion. Last, we rule out changes in city organization and in ethnic diversity as potential factors.

The transmission channels mentioned above help us to better understand the difference of magnitude between the OLS estimate where we find an elasticity of -0.6 and the IV estimate exhibiting an elasticity of -1.9. First, as explained in Section 4, the OLS estimate could be biased (potentially upward), and correcting for endogeneity could lead to a stronger negative effect. Second, the IV estimate should be interpreted as a LATE, that is, we only exploit the change in urbanization driven by internal migrations in response to climate shocks. As shown by Mastrorillo et al. (2016), temperature shocks are likely to induce migration among low-income people. One of our results is that low-income individuals are less exposed to pecuniary crimes, which may therefore explain why the IV estimate is more negative than the OLS one. Last, by design, the shift-share instrument only exploits migration flows of individuals already having a network at destination, and, as we demonstrate, this helps to decrease crime rate.

Two caveats merit attention. Firstly, it's important to note that our main estimate represents the net impact at the city level. We show, however, that the reduction in pecuniary crime benefits certain population groups, such as low-educated black individuals, more than others. Then, despite an overall decline in pecuniary crime rate, some individuals remain as exposed as before. Secondly, the pecuniary crime category is an aggregation of several crime categories which are not equally influenced by urbanization. Caution should therefore be taken when interpreting these results as indicative of broader crime trends or applying them to policy decisions without considering the specificities of each crime category.

To conclude, cities in developing countries are often considered as difficult places to live in. Indeed, strong and unplanned urbanization growth is commonly associated with social instability, threats to infrastructure, potential shortages of vital resources (e.g water), pollution, and crime. Current forecasts suggest that urbanization growth trend will last for several years. What we show here is that, contrary to popular beliefs, internal migration waves do not always increase crime rate at destination, especially if migrants can count on their network. Policy makers could therefore focus on strengthening community networks and providing support systems for migrants, to facilitate their integration and positively influence urban development and safety.

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Does Urbanization Cause Crime? Evidence from Rural-Urban Migration in South Africa

Online Appendix

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2. Appendix B: Shift-share validity tests
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4. Appendix D: Crime victimization by education level
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Appendix A Descriptive statistics

Table A1: Crime category definitions

Type of crime	Definition
Burglary	Crime committed by a person who unlawfully and intentionally breaks into a building or similar structure, with the intention to commit a crime on the premises.
Robbery	Unlawful and intentional forceful removal and appropriation of movable tangible property belonging to another.
Aggravated robbery	Unlawful and intentional forceful removal and appropriation in aggravating circumstances of movable tangible property belonging to another. Robbery cases are included in this category if any weapon, not restricted to a firearm, was used to commit the crime.
Shoplifting	Stealing an article for sale from a self-service shop during trading hours.
Vehicle theft	Stealing of a motor vehicle or motorcycle belonging to another person.
Theft out of or from motor vehicle	Unlawful and intentional removal of parts, accessories or equipment, that form part of a motor vehicle, or articles in or on the vehicle.
Assault	Unlawful and intentional : a) direct and indirect application of force to the body of another person; or b) threat of application of immediate personal violence to another, in circumstances in which the threatened person is prevailed upon to believe that the person who is threatening him has the intention and power to carry out his threat.
Aggravated assault	Unlawful and intentional direct or indirect application of force to the body of another person with the intention of causing grievous bodily harm to that person.
Murder	Unlawful and intentional killing of another human being.
Attempted murder	Commission of an unlawful act with the intention of killing another human being but which does not result in the death of that human being.

Source: Authors' elaboration on South African Police Service.

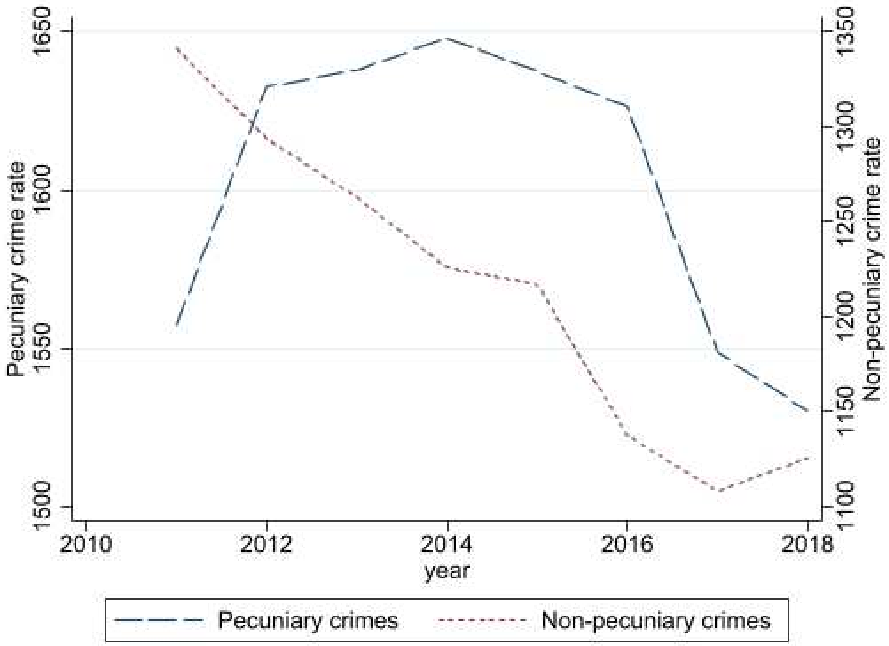


Figure A1: Pecuniary and Non-pecuniary crime rates over the 2011-2018 period.

Note: Crime rates are expressed per 100,000 inhabitants. Source: Authors' elaboration.

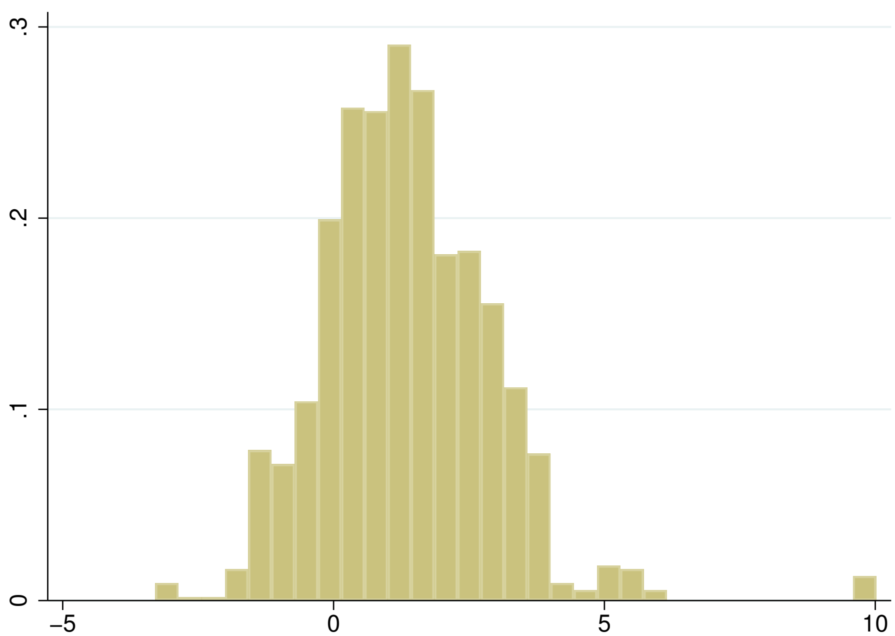


Figure A2: Annual urban population growth rate (in percent).

Appendix B Shift-share validity tests

Table B1: Pre-determined variables, OLS Estimates

	(1) Education	(2) Black	(3) Colored	(4) Asian	(5) White	(6) Employed	(7) High Income
$T_{2011-2018}$	0.002*	0.009	-0.006	-0.004*	0.001	-0.001	-0.002
	(0.001)	(0.007)	(0.008)	(0.002)	(0.001)	(0.001)	(0.001)
Mean dep. var.	0.30	0.65	0.14	0.03	0.13	0.62	0.41
Nb. Observations	201	201	201	201	201	201	201

Notes: This table reports coefficient estimates for the origin based regression following Borusyak, Hull, and Jaravel (2022). Regressions performed on 201 municipalities. Dependent variables are computed from the 2011 Census. Education is the share of low-educated (primary school) people; Black, Colored, Asian, White are the shares of each ethnic group; Employed is the share of employed individuals; High Income is the share of individuals with a monthly income above 19,201 ZAR (South African Rand). $T_{2011-2018}$ is the sum of temperature shocks in municipality o over the 2011-2018 period. Standard errors in parentheses are clustered to account for spatial auto-correlation (150km). *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration.

Table B2: Parallel trend assumption, OLS Estimates

	(1) Δ Pecuniary$_{2003-2009}$	(2) Δ Non-Pecuniary$_{2003-2009}$
$T_{2011-2018}$	0.004	-0.006
	(0.003)	(0.005)
Mean dep. var.	-0.23	-0.41
Nb. Observations	201	201

Notes: This table reports coefficient estimates for the origin based regression following Borusyak, Hull, and Jaravel (2022). Regressions performed on 201 municipalities. ΔPecuniary$_{2003-2009}$ is the difference between the 2009 and 2003 values of the logarithm of pecuniary crime rate. ΔNon-Pecuniary$_{2003-2009}$ is the difference between the 2009 and 2003 values of the logarithm of non-pecuniary crime rate. $T_{2011-2018}$ is the sum of temperature shocks in municipality o over the 2011-2018 period. Standard errors in parentheses are clustered to account for spatial auto-correlation (150km). *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration.

Table B3: Lagged shock, OLS Estimates

	(1) Δ Pecuniary$_{2011-2018}$	(2) Δ Non-Pecuniary$_{2011-2018}$
$T_{2000-2010}$	-0.001	-0.005
	(0.003)	(0.003)
Mean dep. var.	-0.13	-0.20
Nb. Observations	201	201

Notes: This table reports coefficient estimates for the origin based regression following Borusyak, Hull, and Jaravel (2022). Regressions performed on 201 municipalities. ΔPecuniary$_{2011-2018}$ is the difference between the 2018 and 2011 values of the logarithm of pecuniary crime rate. ΔNon Pecuniary$_{2011-2018}$ is the difference between the 2018 and 2011 values of the logarithm of non-pecuniary crime rate. $T_{2000-2010}$ is the sum of temperature shocks in municipality o over the 2000-2010 period. Standard errors in parentheses are clustered to account for spatial auto-correlation (150km). *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration.

Table B4: Origin based specification, IV Estimates

	(1) Pecuniary	(2) Non-Pecuniary
ln(*population*)	-1.826***	-0.627
	(0.658)	(0.633)
Municipality FEs	✓	✓
Year FEs	✓	✓
Nb. Observations	1608	1608
KP F-test	26.51	26.51

Notes: This table reports coefficient estimates for the origin based regression following Borusyak, Hull, and Jaravel (2022). Regressions performed on 201 municipalities and eight years (2011-2018). The dependent variables are the logarithm of pecuniary crime rate (col. 1) and the logarithm of non-pecuniary crime rate (col. 2). ln(*population*) is the logarithm of urban population. Standard errors in parentheses are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration.

Table B5: Descriptive statistics of migration patterns

	Mean	Sd. Dev.	10^{th} per.	90^{th} per.
Herfindahl index of origin contributions	0.11	0.07	0.04	0.18

Note: 10^{th} per. and 90^{th} per. are the values of the 10th and 90th percentiles, respectively. Source: Authors' elaboration.

Appendix C Additional IV estimates

Table C1: Sub-sample by initial population size, IV Estimates

	(1)	(2)	(3)	(4)
	Below median		Above median	
	Pecuniary	Non-Pecuniary	Pecuniary	Non-Pecuniary
ln(*population*)	-1.982***	-0.763*	-2.178***	-0.558
	(0.708)	(0.430)	(0.667)	(0.515)
Municipality FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
Nb. Observations	760	760	696	696
KP F-test	10.14	10.14	20.68	20.68

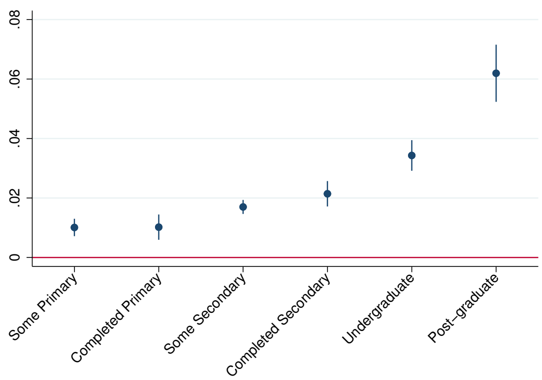
Notes: Regressions performed on 95 municipalities (col. 1 & 2) and 87 municipalities (col. 3 & 4) and eight years (2011-2018). The dependent variables are the logarithm of pecuniary crime rate (col. 1 & 3) and the logarithm of non-pecuniary crime rate (col. 2 & 4). ln(*population*) is the logarithm of urban population. Standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration.

Table C2: Crime categories, IV Estimates

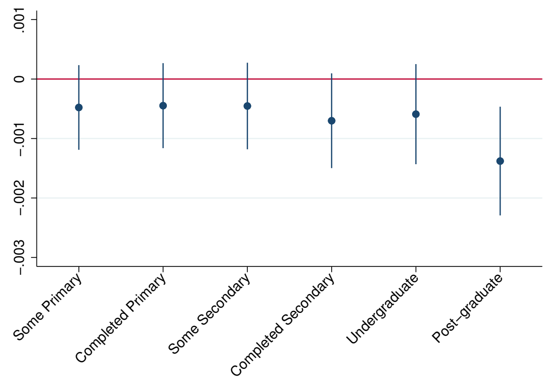
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Burglary	Robbery	Aggravated Robbery	Shoplifting	Theft of vehicles	Assault	Aggravated Assault	Murder
ln(*population*)	-2.615***	-0.874	-0.825	-2.808**	-3.279***	0.017	-0.698	1.466*
	(0.629)	(1.156)	(1.222)	(1.353)	(0.981)	(0.730)	(0.631)	(0.885)
Municipality FEs	✓	✓	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓	✓	✓
Nb. Observations	1456	1456	1456	1456	1456	1456	1456	1456
KP F-test	20.45	20.45	20.45	20.45	20.45	20.45	20.45	20.45

Notes: Regressions performed on 182 municipalities and eight years (2011-2018). The dependent variables are the logarithm of the crime rate of each crime category. ln(*population*) is the logarithm of urban population. Standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration.

Appendix D Crime victimization by education level



(a) Pecuniary crimes



(b) Murders

Figure D1: Effect of education on victimization, OLS Estimates.

Note: Estimates drawn from a regression where a dummy variable taking the value one if the household experienced at least one pecuniary crime (Fig. D1a) or one murder (Fig. D1b), and zero otherwise is regressed on the education level of the household head (reference category is "No schooling") and municipality fixed effects. Regressions performed on 601,864 obs. Standard errors are clustered at the municipality level. 95 percent confidence intervals are reported. Source: Authors' elaboration on the 2016 Community Survey.

Appendix E Augmented shift-share estimates

Table E1: Pecuniary crimes, Augmented shift-share, IV Estimates

θ_2 ↓	$\theta_1 \rightarrow$	-10	-5	5	10
-10	ln(*population*)	-1.313*	-1.095	-0.815	-0.783
		(0.771)	(0.729)	(0.632)	(0.643)
	KP F-test	15.30	17.02	18.68	18.33
-5	ln(*population*)	-1.892***	-1.733***	-1.532***	-1.516***
		(0.644)	(0.577)	(0.485)	(0.484)
	KP F-test	13.23	13.78	16.27	15.80
5	ln(*population*)	-2.281***	-2.079***	-1.837***	-1.821***
		(0.823)	(0.718)	(0.587)	(0.585)
	KP F-test	16.48	17.55	22.38	21.27
10	ln(*population*)	-2.264***	-2.065***	-1.826***	-1.810***
		(0.812)	(0.709)	(0.582)	(0.579)
	KP F-test	16.28	17.32	22.06	20.98

Notes: Regressions performed on 182 municipalities and eight years (2011-2018). The dependent variables is the logarithm of pecuniary crime rate. ln(*population*) is the logarithm of urban population. θ_1 is the parameter associated to the geographical distance, and θ_2 is the parameter associated to the educational distance. Standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration.

Table E2: Non-pecuniary crimes, Augmented shift-share, IV Estimates

θ_2 $\downarrow$	$\theta_1 \rightarrow$	-10	-5	5	10
	ln(*population*)	-0.149	-0.156	-0.044	-0.059
-10		(0.652)	(0.589)	(0.488)	(0.489)
	KP F-test	15.30	17.02	18.68	18.33
	ln(*population*)	-0.653	-0.553	-0.226	-0.240
-5		(0.568)	(0.517)	(0.456)	(0.453)
	KP F-test	13.23	13.78	16.27	15.80
	ln(*population*)	-1.055	-0.804	-0.275	-0.284
5		(0.713)	(0.632)	(0.545)	(0.541)
	KP F-test	16.48	17.55	22.38	21.27
	ln(*population*)	-1.037	-0.794	-0.274	-0.282
10		(0.703)	(0.625)	(0.540)	(0.536)
	KP F-test	16.28	17.32	22.06	20.98

Notes: Regressions performed on 182 municipalities and eight years (2011-2018). The dependent variables is the logarithm of non-pecuniary crime rate. ln(*population*) is the logarithm of urban population. θ_1 is the parameter associated to the geographical distance, and θ_2 is the parameter associated to the educational distance. Standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration.

Appendix F Additional estimates on social capital

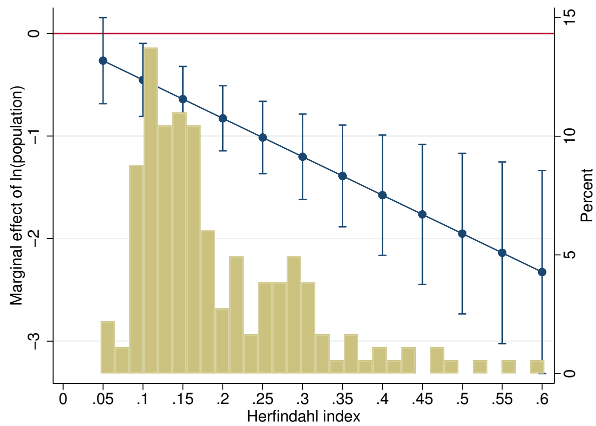


Figure F1: Marginal effects for pecuniary crimes, OLS estimates

Notes: Regression performed on 182 municipalities and eight years (2011-2018). Dots represent the marginal effect of ln(*population*) on the logarithm of pecuniary crime rate for different values of the Herfindahl index. 95 percent confidence intervals are reported for standard errors clustered at the municipality level. Source: Authors' elaboration.

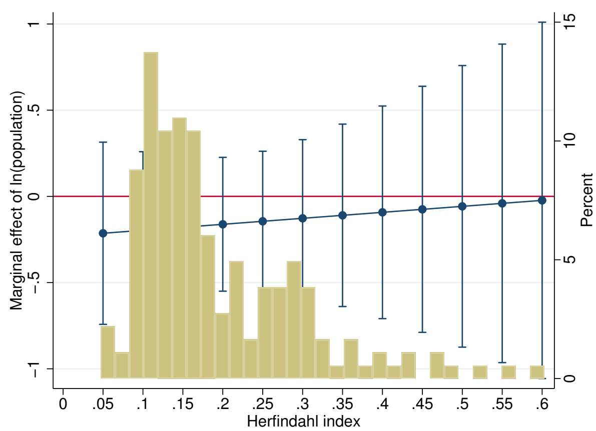


Figure F2: Marginal effects for non-pecuniary crimes, OLS estimates

Notes: Regression performed on 182 municipalities and eight years (2011-2018). Dots represent the marginal effect of ln(*population*) on the logarithm of non-pecuniary crime rate for different values of the Herfindahl index. 95 percent confidence intervals are reported for standard errors clustered at the municipality level. Source: Authors' elaboration.

Appendix G Robustness checks

Table G1: Cluster houses, IV Estimates

	(1) Pecuniary	(2) Pecuniary	(3) Non-Pecuniary	(4) Non-Pecuniary
ln(*population*)	-1.937***	-2.001***	-0.524	-0.571
	(0.492)	(0.496)	(0.430)	(0.439)
Cluster house		-0.006***		-0.004***
		(0.002)		(0.001)
Municipality FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
KP F-test	26.57	26.00	26.57	26.00
Nb. Observations	364	364	364	364

Notes: Regressions performed on 182 municipalities and two years (2011 and 2016). The dependent variables are the logarithm of pecuniary crime rate (col. 1-2) and the logarithm of non-pecuniary crime rate (col. 3-4). ln(*population*) is the logarithm of urban population. Cluster house is the share of households living in a cluster house (or gated community). Standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration.

Table G2: Land use, IV Estimates

	(1) Residential	(2) Smallholdings	(3) Commercial	(4) Industrial
ln(*population*)	61.528***	14.716	45.433***	-55.054***
	(18.055)	(11.239)	(9.994)	(14.224)
Municipality FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
Nb. Observations	364	364	364	364
KP F stat	26.39	26.39	26.39	26.39

Notes: Regressions performed on 182 municipalities and two years (2014 and 2018). The dependent variables are the number of pixels where land use is identified as Residential (col. 1), Smallholdings (col. 2), Commercial (col. 3), and Industrial (col. 4) in the 2018 South African National Land-Cover dataset. ln(*population*) is the logarithm of urban population. Standard errors in parentheses are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration.

Appendix H Simulations of under-reporting

In this section, we explain how we perform simulations presented in Section 5.4.

Simulated crime variable

This variable is defined at the municipality (182) year (8) level and it is computed as follows. We consider that the population of a municipality is made of two types of individuals, namely local residents (natives) and migrants. We make three assumptions on population:

1. Population at baseline (in 2011) is made of natives only;
2. Any positive change of population from 2012 onward is driven by migration only;[36]
3. Migrants never assimilate.

Then, the simulated pecuniary crime rate variable is the weighted average of reported pecuniary crimes for natives and for migrants, and it writes as follows:

$$crime^{sim}_{mt} = \frac{\left(Natives_m \times c^N_{mt} + Migrants_{mt} \times c^M_{mt}\right)}{Population_{mt}}$$

where $Natives_m$ is the number of individuals living in municipality m in 2011; $Migrants_{mt}$ is the stock of migrants living in municipality m at year t; $Population_{mt}$ is the sum of $Natives_m$ and $Migrants_{mt}$; c^N_{mt} is the number of reported pecuniary crimes per capita for natives; and c^M_{mt} is the number of reported pecuniary crimes per capita for migrants.

Let us define c^N_{mt}. We assume that any within variation can only be driven by a change in preference of criminals who would prefer to target migrants rather than natives. This shift depends on a parameter, s, bounded between zero and one, and on the share of migrants in the total population of the municipality. In practice, c^N_{mt} is equal to:

$$c^N_{mt} = c^N_{m2011} \times \left(1 - s \times \frac{Migrants_{mt}}{Population_{mt}}\right)$$

where c^N_{m2011} is the number of reported crimes per capita observed in our dataset in 2011.

Let us define c^M_{mt}. We assume that (i) migrants are initially equally targeted than natives; (ii) overtime, crime might shift from natives to migrants; (iii) migrants might have a lower reporting rate than natives. Then, the number of reported crimes a migrant is exposed to in municipality m at time t is equal to:

$$c^M_{mt} = c^N_{m2011} \times \left(1 + s \times \frac{Migrants_{mt}}{Population_{mt}}\right) \times r \tag{11}$$

[36] The number of natives remain therefore fixed in each municipality.

where r denotes the relative reporting rate to the police of migrants with respect to the natives, and it is bounded between zero and one.

Empirical model

We estimate the same model as in Equation 1 using the simulated crime variable, $crime_{mt}^{sim}$, as the dependent variable, and the shift-share as an IV for urban population. In practice, we estimate the following model for different values of r and s.

$$\ln(crime_{mt}^{sim}) = \alpha + \beta \ln(population_{mt}) + \nu_m + \gamma_t + \varepsilon_{mt} \tag{12}$$

where the dependent variable, $\ln(crime_{mt}^{sim})$, is the logarithm of the simulated pecuniary crime rate in municipality m at year t computed above; $population_{mt}$ is the number of inhabitants living in the urban area of municipality m at year t; ν_m and γ_t denote municipality and year fixed effects, respectively; and ε_{mt} is the error term.

Interpretation

Figure 3 plots the estimated coefficient that are significantly different from -1.9 (the estimated elasticity from our baseline specification). What is of interest is the combination of r and s for which the estimated coefficient becomes not significantly different from -1.9. For instance, when $r = 0.9$, s should be roughly equal to 0.8. Although s does not have a straightforward meaning, it can be interpreted through its effect on the migrants' over-victimization rate c_{mt}^{M}/c_{mt}^{N}. Assuming a value of $Migrants_{mt}/Population_{mt}$ equal to 0.1 (the median value in our sample in 2018), $s = 0.8$ means that migrants are more exposed to crime than natives by 8 percent.