



Unexpected guests: The impact of internal displacement inflows on rental prices in Colombian host cities[☆]

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ABSTRACT

We study the causal impact of internally displaced people (IDP) inflows on rental prices in Colombian host cities. Following an instrumental variables approach we find that as IDP inflows increase, low-income rental prices increase and high-income rental prices decrease. We provide empirical evidence on two potential mechanisms for these findings: Excess demand for low-income housing puts upwards pressure on rental prices; increasing supply of high-income housing coupled with rising homicide rates put downwards pressure on rental prices.

“The impact of the displaced population [IDP] is huge. For example, regarding the issue of housing, many ‘invasions’ are carried out by the displaced population and we have to evict people; we try to find alternative solutions, but in truth there is not enough housing. There is not very much conflict generated over other services. The issue of housing is a disaster at the national and district levels for both the displaced and host populations.”

Mayor of Ciudad Bolívar, 2011 (Vidal-López et al., 2011)

Since the beginning of the Colombian conflict in the 1960s, 7 million individuals have been forcibly displaced from their homes.¹ Internally Displaced People (IDP, from now on) are expelled from small rural

areas in the periphery and resettle in large cities where they usually live in rented properties (Dueñas and Zuluaga, 2014). These inflows of IDP are large relative to population and represent important shocks to local markets. In this paper, we investigate the impact of these large IDP inflows on the housing markets of host cities: How do rental prices respond to these shocks? Are low-income rental units more affected? Is housing supply responsive to inflows? What factors mediate the relationship between rental prices and IDP arrivals?

Renting is quite common in developing countries (Rakodi, 1995).² However, research on developing countries housing markets has focused attention exclusively on homeownership. Similarly, research on immigration shocks and rental prices has focused exclusively on devel-

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¹ This estimate represents approximately 15 percent of the Colombian population (UNHCR, 2015).

² While estimates of the overall extent of rental tenure at the urban level are scarce for most of the developing world, the proportion of households that rent in Latin American countries lies roughly between 10 percent and 50 percent, being precisely Colombia the country with the highest rate of rental tenure (Vargas et al., 2017).

oping countries.³ We aim to fill these gaps by providing empirical evidence on the effects of forced immigration on the rental housing market of a developing country. Equally important, we disentangle several of the economic mechanisms behind these effects.

This research is particularly important because empirical findings on the functioning of the housing markets in rich countries cannot be easily extrapolated to developing countries. Not only the extent of the rental housing market depends on the level of development (Gilbert, 2016) but —because of rapid urbanization (Glaeser and Henderson, 2017)⁴ coupled with an inelastic housing supply— cities in the developing world suffer from substantial housing shortages (Fontenla and Gonzalez, 2009) and from a proliferation of informal housing and slums (Feler and Henderson, 2011). Because of increasing demand, land is expensive and the limited affordability of the poorest households⁵ —who are usually credit constrained— leave them with few options other than renting. Finally, demographic pressures common to the developing world (increasing life expectancy, reduction in household size and increasing divorce rates) are fuel that fire housing demand (Blanco et al., 2014). Colombian cities fit reasonably well with this description and provide an ideal setting to study the effects of massive (forced) immigration shocks on the urban housing market in the developing world.

The direction of these effects is unclear from a theoretical perspective: IDP are poor and they increase the demand for low-income rental units (increasing rental prices) but, at the same time, they provide cheap labor which is partially absorbed by the construction sector (decreasing rental prices). Additionally, competition in the labor market may depress wages for both IDP and non-IDP workers generating income effects in the housing market. In sum, IDP affect both the demand and supply of rental units. Further, heterogeneous effects from these demand and supply shocks are likely to emerge due to the segmentation of the market along income levels. For instance, serviced and affordable land on which to construct low-income housing units is scarce in the principal cities of Colombia which translates into large quantitative and qualitative housing deficits (World Bank, 2010).⁶ This suggests that the demand pressure on the rental market should be particularly salient for low-income housing.⁷ Additionally, because of rising levels of crime or crime perceptions and because of congestion in the provision of public goods, large IDP inflows may be associated with deteriorating living conditions which may translate into lower housing prices.⁸ Finally, when facing large immigration shocks, local governments may enact regulations or avoid the implementation of housing policies to deter low-income households to move in. Such policies (or intended policy inaction) may exacerbate the excess of demand.⁹

In this work, we use an ideal dataset with administrative panel

data on quarterly IDP flows across Colombian municipalities and match them with rental prices by income level for the 13 principal cities during the period 1999–2014 (these 13 cities received 65.64% of all IDP inflows). Given its quality, high frequency, and temporal extension, these data provide a meaningful source of variation in IDP inflows to identify their effects on rental prices. Indeed, unlike previous works that —due to the lack of IDP data at the host level— use different proxies for the intensity of IDP inflows, our paper exploits actual IDP figures at the city level. Further, as the intensity of the conflict varied over time and across the Colombian territory, our panel data analysis allows us to account for the fact that the intensity and timing of these large rural-urban migration flows also varied across host cities. Not less important, the time period under analysis provides time windows before, during, and after one of the most dramatic displacement crisis which occurred at the turn of the century.

To empirically establish the impact of IDP inflows on rental prices we first present OLS estimates that document a strong and robust positive conditional association between (average, low-income and middle-income) rental prices and IDP inflows lagged one quarter.

Even though our OLS estimates seem to be robust and suggest a long-lasting impact of IDP on rental prices and even if several papers assume that IDP inflows are exogenous (Tumen, 2016; Balkan and Tumen, 2016), we argue that these documented associations do not necessarily imply causality and could arise from omitted confounders. In particular, IDP may migrate to cities with low costs of living biasing OLS estimates towards zero. Deprecating the extent of this problem, part of the literature assumes that inflows of IDP are exogenous because they are driven by push factors (i.e., conditions that induce them to leave their homes). However, nothing guarantees that after being expelled IDP chose their destinations randomly. This is why our second and preferred strategy deals with the endogeneity of inflows following an instrumental variables approach where the instrument (to which we refer as “Receptivity”) captures the potential of a city to receive IDP. The instrument is the weighed sum of IDP outflows from all municipalities except the receiving host city, where the weights are (the inverse of) the road distance between the host city and the municipality where the IDP outflow originates.

Our instrumental variables approach suggests that the effect of IDP inflows on average rental prices is statistically indistinguishable from zero. However, this non-result masks important heterogeneous effects: rental prices increase for low-income units but they decrease for high-income units. Our preferred specification suggests that a 10% increase in IDP inflows in a given city and quarter increases low-income rental prices by 0.15% and decreases high-income rental prices by 0.39%.¹⁰ These magnitudes seem small but recall that they capture impacts within a quarter. If we focus on the inflows in the period 2004–2014, the reported elasticities imply an increase in low-income rental prices of 14% and a decrease in high-income rental prices of 37%, effects that are far from being negligible.

We provide empirical evidence for two possible mechanisms that might explain the differential impact of inflows. We show that, while the total number of construction licenses does not vary with IDP inflows, the share of construction licenses of low-income housing units are crowded-out by the share of construction licenses of non-low-income housing units. Using census data from 1993 to 2005, we produce additional evidence of IDP inflows pushing prices up because of an excess in demand. We also show that real wages in the construction sector are negatively affected by the arrival of IDP. These findings are consistent with the heterogeneous impact of IDP on rental prices and with the idea of IDP fueling the construction sector in the richest areas

³ For developing countries, since the work of Saiz (2003), the related literature has studied, among others, cases in the US (Saiz, 2003; Mussa et al., 2017), UK (Sá, 2014), Italy (Accetturo et al., 2014), Spain (Gonzalez and Ortega, 2013), and Canada (Akbari and Aydede, 2012).

⁴ Urban population in Africa and Latin America is today larger than in Europe and North America (Chauvin et al., 2017).

⁵ According to Blanco et al. (2014), the price of a new home in terms of the representative average wage (i.e., price-to-income ratio) in Latin America may be up to three times higher than in the United States.

⁶ Indeed, available land in Colombian cities is highly priced and mainly destined to high-income segments of the housing market.

⁷ This argument is echoed in Vidal-López et al. (2011) who, based on interviews to focus groups composed by IDP and non-IDP members of host communities, argue that rising rental prices due to IDP-driven demand shocks are particularly important in Suba and Ciudad Bolívar (i.e., two localities on the outskirts of Bogotá).

⁸ Accetturo et al. (2014) provides evidence on large influx of immigrants affecting natives' perceived local amenities and their propensity to move to other areas within a city.

⁹ Based on the case of Brazil, Feler and Henderson (2011) provide evidence on strategic withholding of public good provision to deter immigration of poor households.

¹⁰ Section 4.2 compares the magnitude of our estimates with the magnitude of the available estimates most similar to ours; those that quantify the effect of economic migrants on rental prices (Saiz (2007)).

of the cities.

Second, we find that quarterly homicides in host cities react to IDP inflows. A 10% increase in IDP inflows increases the homicide rate by 6.4% with respect to its mean. Crime is a negative externality which depresses housing prices (Besley and Hannes, 2012). We hypothesize that, in poorer areas, the boost in housing demand outweighs the impact of the externality.¹¹

This research speaks to several strands of the literature. First, a growing research in economics estimates the impacts of the arrival of forced migrants on host communities. Recent studies analyze effects on consumption (Kreibaum, 2016; Maystadt and Verwimp, 2014), children's health (Baez, 2011), wages (Calderón-Mejía and Ibáñez, 2015) and food prices (Alix-García and Saah, 2010; Alix-García et al., 2012). We contribute to this research by being the first paper that causally quantifies the effect forced migrants have on the housing rental market for varying levels of income.

Regarding rental housing, two papers are related to ours. Saiz (2007) (see also (Saiz, 2003)), finds that immigration inflows positively affect average or median housing rents. However, Saiz (2007) focuses on economic migrants who tend to be heterogeneous in terms of socio-economic characteristics while IDP are mainly poor arriving from rural areas (Ibáñez, 2008). An additional paper (Tumen, 2016) exploits regional variation in inflows of Syrian refugees within Turkey to study their impacts on prices of different goods, including rental prices. Tumen (2016) finds that rental prices increase, especially for high quality rentals. However, the author assumes that inflows are exogenous, cannot quantify the impact of continuous increments in inflows (because figures of refugees seem to be unavailable or because the set of treatment regions is small) and is silent about the channels of the reduced form effects.

Regarding crime, and notwithstanding the relevance of the topic, the literature is practically mute.¹² The paper most related to us is Varano et al. (2010) which studies the impact of migrants forcefully displaced by Hurricane Katrina on crime in Houston, San Antonio and Phoenix. With the caveat that they only have time series data and no control groups, they find that homicides increased in Houston and Phoenix, a result that echoes ours.

The remainder of the paper is organized as follows: section 1 provides the context of displacement and characterizes the rental housing market in Colombia. Section 2 presents the data and section 3 the econometric model. Section 4 presents our main OLS and IV results and section 5 performs a battery of robustness checks. Section 6 highlights two potential channels (housing supply and excess demand, and crime). Section 7 concludes.

1. Context

1.1. Forced displacement in Colombia

Colombia suffers from a long history of forced displacement as a result of political and drug-related violence. Left-wing guerrilla groups, like the FARC and ELN, emerged in rural areas of the country in the 1960s and persisted for years engaging in relatively low-scale violence against the Colombian government. Levels of violence began to increase in the 1980s and 1990s as these rebel groups entered the drug trade. During the mid-1990s and early-2000s, right wing paramilitary groups stepped in to fill the void created by the lack of State presence in many parts of the country, themselves relying on the narcotics trade for their

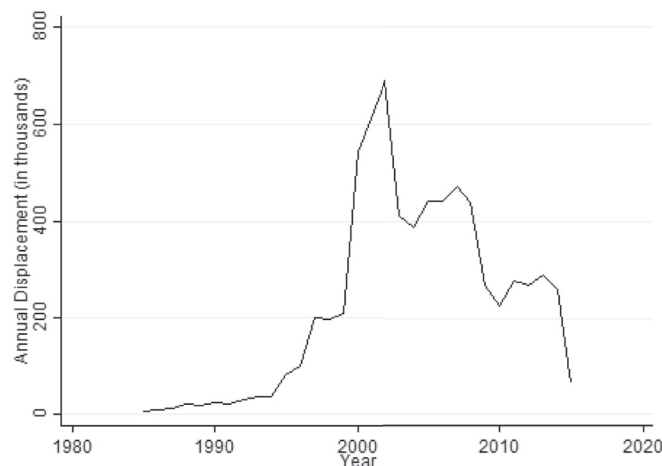


Fig. 1. Internal displacement in Colombia.

financing. Colombians in peripheral and rural areas were caught in the middle of a three-way war between paramilitary groups, guerrillas, and government forces.

In this context, millions of civilians were forcibly displaced from their homes for a variety of reasons. Since the beginning of the conflict, the United Nations High Commissioner for Refugees (UNHCR) estimates that over 6,640,000 individuals have been forcibly displaced from their homes, approximately 15 percent of the Colombian population (UNHCR, 2015).

Though displacement is violent and traumatic, displaced households do not travel far from the expelling municipality. More than half of displaced households reestablish themselves within the same department, an administrative division similar to the state in the USA (Ibáñez, 2008). In Bogotá, the country's capital, 71% of IDP live in rental housing (Departamento Administrativo de Planeación Distrital, 2004). In contrast with other countries, there are no displacement camps in Colombia.

Fig. 1 shows the trends in forced displacement for Colombia since 1985. The figure shows a notable increase in the number of individuals displaced beginning in the mid-90s, as guerrilla groups ramped up their operations in earnest, and drug revenues fueled the rise of paramilitary groups. The 2000–2001 spike stands out, corresponding to the breakdown of failed peace negotiations with the FARC, and an important period of violent expansion for the AUC paramilitary group (see Acemoglu et al. (2013)). The dramatic decline in displacement after the AUC's demobilization in 2008 is noteworthy, as is the drop since the 2012 announcement of new peace talks between the FARC and the Colombian government.

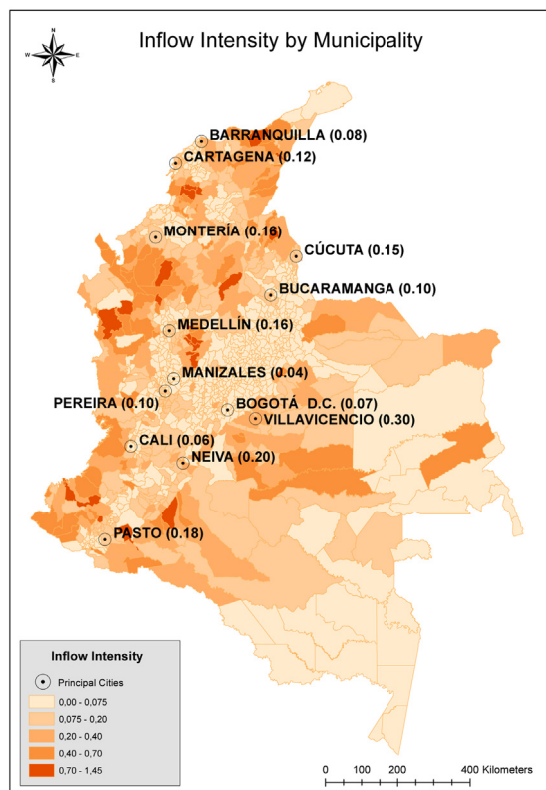
Just as the intensity of displacement has been uneven across time, it has varied across the different regions of Colombia. The two panels in Fig. 2 provide information on the intensity of inflows (left panel) and outflows (right panel) of IDP by municipality. The two intensity measures are calculated as the ratio of accumulated forced migrants (either inflows or outflows) over the 1999–2015 period to the population of the municipality in 1999. As the graphs show, the 13 largest cities are net receivers of IDP, and the magnitude of the IDP inflows in those cities is substantial, as in the case of Villavicencio where the accumulated stock of IDP received over 1999–2015 represents 30 percent of 1999 population. The graphs also demonstrate two commonly understood facts regarding the nature of internal displacement in Colombia: IDP are mainly expelled from rural areas and low population density municipalities, and there is a high intensity of IDP outflows in areas of Colombia where armed conflict has been more intense.

In 1999, the Colombian government created a victims' registry, allowing displaced individuals (including those displaced before 1999) to come before a government office, where their displacement status

¹¹ In another work (Depetris-Chauvin and Santos, 2018) we provide further evidence on the relationship between crime and displacement.

¹² An exception is Bell et al. (2013) who studies the effect of recent immigration waves into the UK; they find an effect on property crimes but not on other types of crimes. Similarly, Bianchi et al. (2012) find that non-forced migrants increase robberies but not other crimes.

(a) Inflows



(b) Outflows

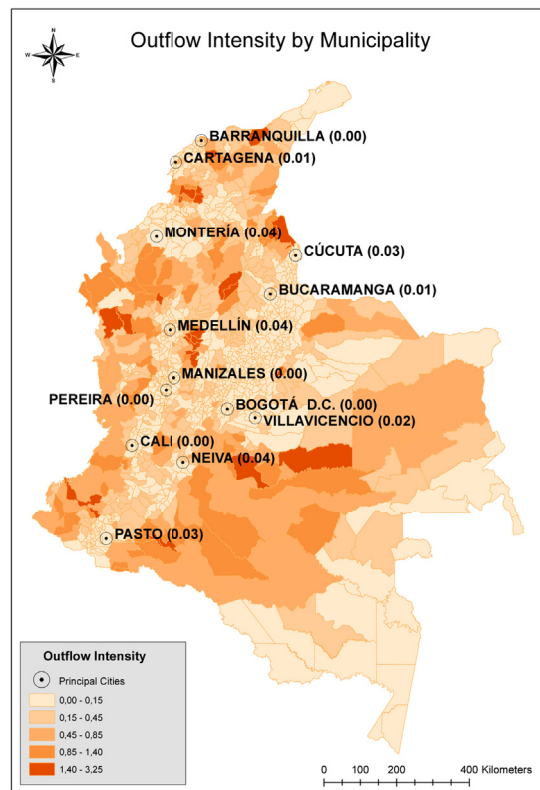


Fig. 2. Inflows and outflows intensity.

is validated and they can become eligible for receiving government assistance.¹³ The information from the victims' registry is one of the main data sources used in this research. Fig. B.1, in the online appendix shows that there are important differences in the intensity and timing of inflows in the 13 host cities. However, all cities experiment a spike in 2000.

1.2. The rental housing market in Colombia

Colombia has the highest share of rental housing in Latin America (Vargas et al., 2017) and this share has been increasing for the last three decades.^{14,15} Rental tenure in Colombia is mainly urban

¹³ There are three assistance categories for the forcibly displaced population: immediate assistance, including temporary housing and food aid, may be provided by the host municipality starting the moment the victim makes their claim until the RUV has made a decision on their case (up to two months); short- or medium-term emergency aid is provided by the RUV to displaced individuals whose cases are determined to meet certain urgency requirements, and allows monthly payments of up to 1.5 times Colombian minimum monthly wage; finally, transition assistance in the form of employment programs or access to food or housing assistance is provided on a case-by-case basis to displaced individuals whose cases are not determined to meet the emergency assistance requirements (Prada and Poveda Rodríguez, 2012). However, the reality of how this is implemented is far from ideal. For displaced individuals who registered between 2002 and 2004, only 50 percent had received any assistance at all, and for much of the period in our study, most qualifying individuals received an assistance package for only three months (Human Rights Watch, 2005).

¹⁴ See Arbeláez et al. (2011) for a detailed discussion on housing tenure and demand in Colombia.

¹⁵ According to official statistics, the proportion of households that are renters has grown from nearly 25 percent in 1993 to about 40 percent in 2014.

(World Bank, 2010), spatially segregated by socioeconomic strata (Blanco et al., 2014), and highly prevalent among poor households.¹⁶

The rapid urbanization process taking place in the largest Colombian cities has put pressure on the housing market which is characterized by a constrained capacity to meet a growing demand (World Bank, 2010). Even before the peak of the displacement crisis occurred in the early 2000s, Colombia already presented large housing deficits (i.e., insufficient houses to provide a home to every household).¹⁷ While these deficits exhibit a downward trend over the last years, it remains high and concentrated in the largest cities. In addition to that, 40% of the households with housing deficit are tenants (Blanco et al., 2014).

The scarcity of affordable land for social housing is a primary obstacle to expand the supply of low-income housing in Colombia (Blanco et al., 2014).¹⁸ Areas where this expansion is feasible are highly priced and only suitable for high-income households. Moreover, the lack of credit is an important constraint which limits the expansion of social housing (Arbeláez et al., 2011).¹⁹ In fact, private actors are almost absent in the construction of low-income housing. (World Bank, 2010). The limited specialized real estate intermediation is mainly concen-

¹⁶ The homeownership rate is below 50 percent for the households within the two lowest income quintiles.

¹⁷ By the year 1993, total housing deficit was 54 percent (Arbeláez et al., 2011).

¹⁸ The scarcity of available land in the main cities of Colombia has been discussed in several works (see for instance, Carrillo (2009) and World Bank (2010)).

¹⁹ Households in the lowest-income segments are virtually excluded from the mortgage market in Colombia (Blanco et al., 2014).

trated in the medium-high and high price segments of the market (Blanco et al., 2014).

From the perspective of housing rental supply, there is an additional factor that hinders the development of a formal rental market in Colombia: landlords face legal difficulties when they try to repossess their properties. The 2003 Colombian Law of Rentals protects the tenants at the expense of landlord's rights.²⁰ In this context, the supply of rental housing for low-income sectors is fundamentally of informal origin and quite inelastic in the short-run (Blanco et al., 2014).

2. Data

To conduct this research we use different data sources. Most of the variables used in the paper are summarized in Table 1. Appendix Table A.1 provides more details on the source and the construction of the variables. Our main dependent variables are average rental prices and rental prices by income level with cross-sectional variation at the city level ($N = 13$) and quarterly time frequency for the period 1999–2014 ($t = 64$). We use 13 cities because no data on rental prices is available for other cities. However, these cities are the 13 largest Colombian cities and the main recipients of IDP. During our period of analysis, these 13 cities received 65.64% of all IDP inflows.²¹ In some specifications, we deflate rental prices by the city-level CPI to obtain a measure of relative rental prices. We believe this is important because inflation varies considerably across Colombian cities, because IDP inflows can affect the price of the representative consumption bundle (see, for instance, Balkan and Tumen (2016)) and because we want to check that we are not confounding the effect on rental prices with changes in inflation rates. All of these variables come from the Colombian National Statistics Department (DANE).

For the purposes of subsidizing public utilities, the Colombian government classifies urban housing units into different strata with similar economic characteristics. This system classifies areas on a scale from 1 to 6, with 1 as the lowest and 6 as the highest income area. When computing rental price indices, the DANE classifies as low-income, middle-income, and high-income the rental units in stratas 1–2, 3–4, and 5–6, respectively, allowing us to examine the differential impact of IDP arrivals by income level.

Our main independent variable is the log of IDP inflows arriving to city c in quarter $t - 1$. We built these data using information from the Registro Unico de Victimas (RUV), a dataset of IDP inflows and outflows collected by the Colombian government registry for IDP (i.e.; Registro Nacional de Información -RNI-). The flows that are captured are those arising from the Colombian internal conflict as described in the previous section. From these data we will also use outflows in city c and quarter $t - 1$ as an armed conflict control in some specifications and outflows in all other municipalities to construct our main instrumental variable, described in the next paragraph. Table 1 shows that on average, the number of IDP inflows is 1159.8 per quarter (from $\exp(7.056)$).

The main instrumental variable is a distance-weighted sum of IDP outflows in all municipalities but city c , where the weights are the inverse of the road distance between expelling municipality m and city c . We label this variable “Receptivity Instrument” as it predicts the potential of a city to attract IDP. The road distance measure is novel in the Colombian context. To create it we relied on road maps from CIGOT-IGAC for the year of 2011, the first year for which we could find an almost complete geo-coded network. Some distant municipalities are not connected to the Colombian road network and this might

Table 1
Descriptive statistics.

	Mean	Std. Dev.
<i>Main dependent variables</i>		
Average rental prices	4.528	0.184
Low income rental prices	4.526	0.195
Middle income rental prices	4.527	0.182
High income rental prices	4.530	0.162
Relative average rental prices	0.069	0.085
Relative low-income rental prices	0.068	0.080
Relative middle-income rental prices	0.069	0.089
Relative high-income rental prices	0.071	0.109
<i>Other dependent variables (channels)</i>		
Share of new housing licenses of social Interest	9.395	12.238
Share of new housing licenses of non-social Interest	89.670	13.655
Share of new non-residential licenses	0.695	4.678
Overall housing deficit	0.647	0.224
Quantitative housing deficit	0.101	0.072
Homicides rate	12.047	8.48
<i>Independent Variables</i>		
IDP inflows	7.065	1.086
Population	13.559	0.898
IDP outflows	4.961	1.115
Receptivity instrument	−1.705	0.477
Average road distance	13.238	0.234
Tax revenues	10.683	1.581
Public school teachers per student	−3.309	0.104
Fraction of unavailable land	0.322	0.193

All variables expressed in natural logarithms except the licenses variables (shares from 0 to 100), housing deficits (rates from 0 to 1), the homicides rate (per 100,000 inhabitants) and the fraction of unavailable land.

generate noise in the instrument. More precisely, 111 municipalities of the 1122 for which we have data on outflows are not connected to the road network. Outflows from these municipalities represent 10.2% of all outflows. However, as we will show, results are similar when using road or geodesic distances, the latter of which uses information on outflows from all municipalities. Furthermore, a regression of the probability of being connected to the road network against past inflows produces insignificant estimates, which reduces concerns about selection.

The main regressions in the text include city and time fixed effects, city time trends and interactions between time dummies and the average road distance to all other municipalities (our measure of remoteness), but no additional controls. Robustness to the inclusion of determinants of housing rents are shown in the appendix where we control for population, for outflows (from RUV) as a proxy for conflict in the city, for tax revenues from industry and commerce as a proxy for economic activity and for the number of public school teachers per students as a measure of amenities (both from CEDE, at Universidad de Los Andes). Notice that, although these controls are important determinants of housing rents, they might be endogenous and, for this reason, we do not include them in baseline specifications even if this choice can lead to larger standard errors.

An additional control which is worth mentioning is the fraction of total land that exhibits slopes above 15% (computed following Saiz (2010) and using data on terrain slope by Nunn and Puga (2012)). This is a measure of unavailable land for construction. We include this control interacted with time dummies in regressions of housing supply.

Finally, we use other dependent variables to investigate potential channels. To examine the supply side of the housing market we first examine the impact of IDP on the share of new housing licenses of social interest (VIS Licenses, a Spanish acronym for Vivienda de Interés Social), the share of new housing licenses of non-social interest and the

²⁰ An indicator of the difficulties in repossessing a property is the average time invested in the repossession process: eight months (Blanco et al., 2014).

²¹ These cities are not very different in observables from the 32 state capitals which make us conjecture that our results might be extrapolated to this larger set of cities.

share of new non-residential construction licenses. The VIS category was established by the Colombian government to assist the low-income segment of the population to acquire homes and includes subsidized and free housing provided to poor families.²²

To analyze other dimensions of the housing market, we exploit data on housing deficits from the censuses of 1993 and 2005. We follow DANE and define overall housing deficit as the sum of quantitative and qualitatively housing deficits. The former is defined as the number of houses that need to be replaced or constructed to provide a home to every household whereas the latter is defined as the number of houses that needs to be expanded or improved due to addressable deficiencies such as overcrowding, inadequate utilities, and minor building deficiencies.²³ The advantages of the housing deficit measures is that they are available for 978 municipalities and that they allow us to explore the long term impact of IDP inflows on the housing market. These measures are used as ratios where the denominator is the total number of houses.

Last, inspired by Besley and Hannes (2012), we calculate the homicide rate from Colombian Vital Statistics. A nice feature of the data on homicides is that —except for some missing values— it is available for the same sample of rental prices.

3. Econometric model

To estimate the impact of IDP on rental prices we exploit both temporal and cross-sectional variation in the intensity of displacement inflows at the city level for the period 1999–2014. We estimate the following equation:

$$\ln(P_{c,t}) = \alpha + \beta \ln(\text{Inflows}_{c,t-1}) + \eta' X_{c,t} + d_c + d_t + u_{c,t}, \quad (1)$$

where the subscripts c and t denote city and quarter, respectively. The variable P is the price of rentals. Again, in our analysis we use average rental prices, as well as rental prices by income level (i.e., low-, middle- and high-income). *Inflows* is our main independent variable and represents the total number of displaced people arriving to the host city c during the time period (quarter) $t - 1$. The log-log specification presented in equation (1) facilitates the interpretation of the point estimate $\hat{\beta}$ as a standard elasticity. d_c and d_t denote city and quarter fixed effects, respectively. This collection of fixed effects captures time-invariant city characteristics (the d_c) and quarter-specific conditions (the d_t) that may be related to the evolution of rental prices. $X_{m,t}$ is a vector of controls. In all of the tables we include city-level linear trends to control for city-specific trends which might be anticipated by IDP. We also control for interactions between remoteness and time dummies to control for aggregate shocks that differentially affect distant cities (for example, changes in the price of oil or gas). This is also an important control in our instrumental variables strategy (see below). In the robustness sections we use and *expanded set of controls* to include the following (potentially endogenous) determinants of rental prices: population and city-level proxies for conflict intensity (outflows), economic activity (tax revenues), and the quality of amenities (public teachers per student).

u is an error term clustered at the city-year level (208 clusters). We do that to take into account correlation of unobservables that affect prices within a city and also within a year because, for example, inflation adjustments are done typically once at the beginning of each cal-

endar year with differences across cities.²⁴ Finally, we weight all the regressions by population because the effect of IDP inflows is larger in bigger cities. The latter have difficulties absorbing IDP and have more constraints to expand the supply of housing, in particular the supply of low-income housing.

We focus on IDP inflows lagged one period for two reasons: First, analyses of the lag structure suggest that, along with contemporaneous inflows, inflows in $t-1$ are the most important determinant of housing rents. Second, using a lagged independent variable may reduce concerns of reverse causality between IDP inflows and prices.

Of course, this approach of lagging the inflow variable does not convincingly solve potential endogeneity problems. According to the 2004 survey of IDP (Centro de Estudios sobre Desarrollo Económico, 2004), 15.28% of displaced households leave their origin because of economic reasons. This means that push factors (violence in the origin) are more important than pull factors (economic conditions in the city of arrival). However, 15% is not a negligible number. Furthermore, it is hard to argue that, after being displaced by violence, IDP chose their destination randomly.

If inflows are correlated with time varying characteristics of the recipient city, OLS estimates might be biased and the bias could be towards zero. Say, for example, that forced migrants move to cities where the general cost of living is lower, then the OLS estimates would be biased downwards. Similarly, IDP inflows could be correlated with expectations of growth in rental prices which would also lead to attenuation bias in the OLS estimates if expected growth and actual growth are positively correlated. Additionally, we cannot dismiss potential bias from measurement error in our IDP measure. In this sense, attenuation bias could be particularly important given that we exploit a panel data where fixed effects at both the city and quarter level are included.²⁵ We acknowledge that other sources of bias may still persist and it is precisely for this reason that we follow an instrumental variables approach.

Our instrument, which we label *receptivity* $_{c,t}$, is constructed based on RUV data, and accounts for the intensity of IDP outflows generated in each Colombian municipality every quarter during the period 1998–2014. Our *receptivity* $_{c,t}$ measure is a road-distance-weighted average of the outflows in all municipalities except city c during quarter t . Formally:

$$\text{receptivity}_{c,t} = \sum_{m \in M \setminus \{c\}} \text{outflows}_{m,t} \times D_{m,c}^{-1}, \quad (2)$$

where $c \in C \subseteq M$ is a city in our sample of the 13 largest cities, which is a subset of the 1122 Colombian municipalities. $D_{m,c}^{-1}$ is the road distance between municipality m (origin of IDP) and city c (destination of IDP). The instrument suggests that the number of IDP arriving to city c in time t increases with the number of outflows in other localities, but decreases with the distance from any expelling locality to the city. Thus the log of receptivity in $t - 1$ is used as an instrument for the log of inflows in $t - 1$.

This instrument is based on three ideas. First, large migration outflows of IDP are mainly determined by violent events toward civilians in rural areas. Second, the timing and intensity of those violent events are arguably orthogonal to relevant characteristics of the host cities.

²² More precisely, Law 388, issued in 1997, defined VIS housing as units costing up to 135 monthly minimum legal wages (i.e., SMLV, its spanish acronym). These housing units are intended for those earning less than 4 SMLV.

²³ Indeed, qualitative deficit is related to substandard structures and inadequate access to basic services.

²⁴ We also computed heteroskedasticity corrected standard errors and clustered at the quarter level. The standard errors clustered at the city-year level are much larger than under the other alternative methods. This pattern holds for all the specifications presented in this paper. Correspondingly, clustering at the city-year level appears to be the most conservative approach for avoiding over-rejection of the null hypothesis concerning the statistical significance of the coefficient of interest.

²⁵ It can be shown that under the case where measurement error is serially uncorrelated, using a fixed effect model might increase the variance of the measurement error while it might reduce the variance of the signal thus worsening the original attenuation bias.

Table 2
IDP and average rental prices, OLS.

	(1)	(2)	(3)	(4)
IDP Inflows t-1	0.00932** (0.00468)	0.00686*** (0.00220)	0.00714*** (0.00208)	0.00882*** (0.00265)
Observations	832	832	832	832
City FE	Y	Y	Y	Y
Time FE	Y	Y	Y	Y
City-specific Linear Trend	N	Y	Y	Y
Time Dummies Interacted with Remoteness	N	N	Y	Y
Expanded Set of Controls	N	N	N	Y

Standard errors clustered at the city.year level in parentheses. All variables are expressed in natural logarithms. The expanded set of controls includes population, IDP outflows, tax revenues, and public school teachers per student (all in logs). All regressions are weighted by city population. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

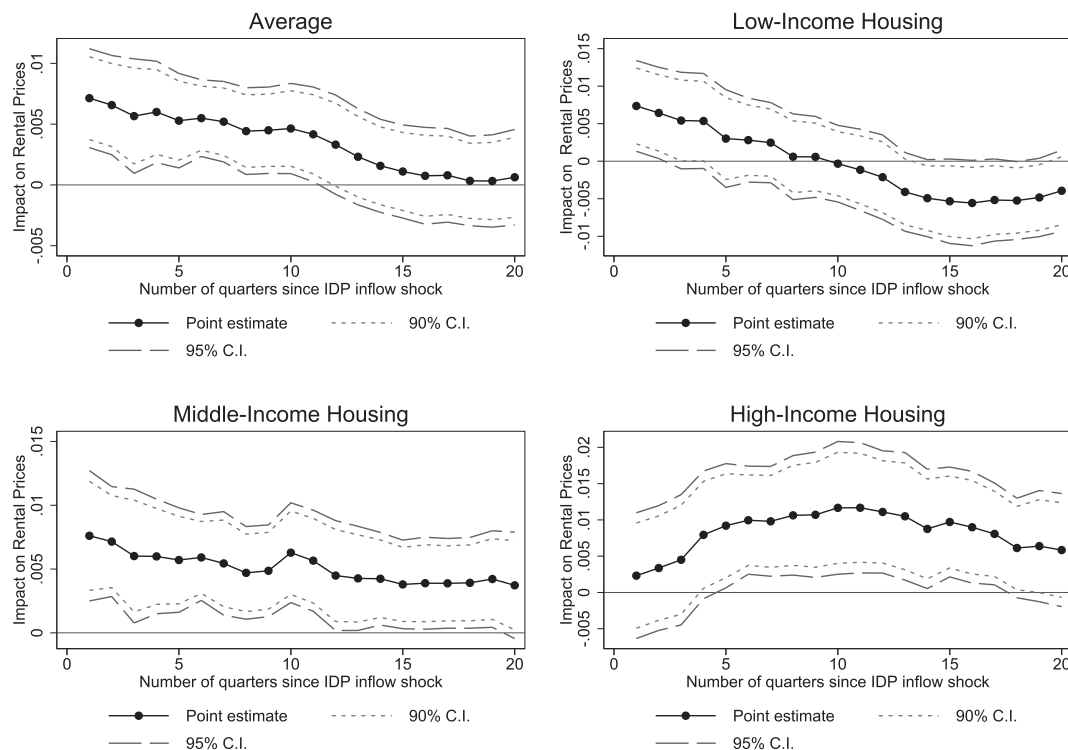


Fig. 3. IDP inflows and average rental prices over time (OLS).

Third, the closer the proximity of a host city to a municipality experiencing IDP outflows, the higher the probability of receiving a large IDP inflow for that host city. Indeed, using the 2004 survey of displacement (Centro de Estudios sobre Desarrollo Económico, 2004), 45.45% of displaced households chose their destination because it is close to the origin.²⁶

The exclusion restriction is that receptivity only affects rental prices through the channel of IDP inflows. One concern is that receptivity might be capturing the remoteness of a city. For this reason, all of our regressions control for interactions between time dummies and remoteness. Another concern is the spatial correlation of violence: if violence in nearby municipalities is correlated with violence in the host city our identification strategy might be invalid. To deal with this potential threat to identification, we conduct a series of robustness checks where we exclude outflows from nearby municipalities (see Section 5).

²⁶ According to Ibáñez (2008), more than 50 percent of internally displaced households migrate within the same state, and almost 20 percent do so within the same municipality.

4. Main results

4.1. OLS results

Table 2 provides the first statistical test for the impact of IDP on rental prices. We present OLS estimates of different specifications of equation (1), for which the dependent variable is the log of average rental prices. The specification in column 1 only includes d_c and d_t as controls. Consistent with a demand-side shock story, results in column 1 suggest that rental prices are positively and significantly correlated with IDP inflows lagged one period. The point estimate from our log-log specification indicates that a one percent increase in IDP inflows is related to an increase of 0.009 percent in average rental prices in a given quarter. Since IDP inflow shocks tend to occur in large magnitudes, the implied coefficient suggests that those shocks may result in a sizable effect on rental prices.

Nonetheless, the confounding influence of factors influencing both rental prices and IDP inflows within a city over time make these estimates unreliable. Indeed, when we include city-specific linear trends in column 2 of Table 2 our point estimate of interest decreases by 26%,

Table 3
IDP and rental prices by income level, OLS.

	(1) Average	(2) Low Income	(3) Middle Income	(4) High Income
IDP Inflows t-1	0.00714*** (0.00208)	0.00736** (0.00308)	0.00761*** (0.00261)	0.00232 (0.00442)
5 years Effect On Rental Prices	0.65395	0.67431	0.69682	0.21232
10 years Effect On Rental Prices	6.76044	6.97084	7.20359	2.19495
Standardized Coefficients	0.04995	0.04860	0.05393	0.01847
Observations	832	832	832	832

Standard errors clustered at the city.year level in parentheses. All variables are expressed in natural logarithms. All regressions include time and city fixed effects, city-specific linear trends and remoteness interacted with time dummies. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4
First-stage: IDP inflows and receptivity.

	(1)	(2)	(3)	(4)	(5)
Receptivity Instrument	0.901*** (0.205)	1.576*** (0.192)	1.539*** (0.221)	1.678*** (0.275)	1.130*** (0.222)
Observations	832	832	832	832	832
F-statistic	19.27	67.54	48.66	37.33	25.86
City FE	N	Y	Y	Y	Y
Time FE	N	Y	Y	Y	Y
City-specific Linear Trend	N	N	Y	Y	Y
Time Dummies Interacted with Remoteness	N	N	N	Y	Y
Expanded Set of Controls	N	N	N	N	Y

Standard errors clustered at the city.year level in parentheses. All variables are expressed in natural logarithms. The expanded set of controls includes population, IDP outflows, tax revenues, and public school teachers per student (all in logs). All regressions are weighted by city population. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

albeit it remains positive and highly significant. Column 3 adds trends by remoteness. This represents our baseline specification. The parameter estimate of interest remains positive, significant and of similar magnitude.

Finally, column 4 includes our expanded set of controls. These controls are important determinants of housing rents but are also potentially endogenous. Results are similar to those in column 3 but the point estimate increases by 23%.

Is the correlation between IDP inflows and average rental prices short-lived? To answer that question we estimate equations like equation (1) replacing $\ln(\text{Inflows}_{c,t-1})$ by $\ln(\text{Inflows}_{c,t-\tau})$ for $\tau = 1, 2, \dots, 20$ (i.e., we run 20 separate regressions). The top left panel of Fig. 3 reports the coefficient on lagged inflows and corresponding confidence intervals for average rental prices. The first point in the solid line is just the parameter estimate of Column 3 of Table 2 (the parameter estimate on $\ln(\text{Inflows}_{c,t-1})$). The second point corresponds to the parameter estimate on $\ln(\text{Inflows}_{c,t-2})$, and so on. The figure suggests that the impact of IDP inflows on rental prices may be long-lasting (up to 10 quarters for average rental prices).

We now analyze whether the impact of IDP inflows on rental prices varies by income level, focusing on rental prices for low-, middle-, and high-income rentals. The top right panel and the two bottom panels of Fig. 3 reveal that low-income and middle-income housing rentals increase with the arrival of IDP. On the other hand, inflows 6 to 14 quarters before t seem to have a positive impact on high income rental prices. This anticipates an important result that will be discussed below.

From these figures it is also clear that for all rental prices but high income, the estimate for inflows in $t-1$ is the most significant and quantitatively important estimate. This gives credence to our strategy of using inflows in time period $t-1$.

Table 3 estimates a regression analogous to that of Table 2, column 3 but focuses on rental prices by income level. For comparison, column 1 of Table 3 replicates the results of the baseline specification when the dependent variable is average rental prices (column 3, Table 2). Results in column 2 show that rental prices for low-income consumers significantly

and positively correlate with IDP inflows. The magnitude is very similar to that for average rentals. In column 3, we also find that IDP inflows are positively and significantly associated with middle-income rental prices. We do not find any statistically significant association between IDP inflows and high-income rental prices (column 4).

How large are these estimates? The bottom of Table 3 provides different ways of gauging the magnitudes of the estimates. We first present the effect implied by the rental prices elasticities and the percentage change in inflows in the last 5 and 10 years. For interpretation, note that these effects depend on the size of inflows in the 5 and 10 years previous to 2014 with the last 5 years being a period of relatively low inflows and the last 10 enclosing a sub-period of relatively high inflows (see Fig. 1). Focusing on the impact on low income rental prices, inflows in the last 5 years entail an increase in low income rental prices of 0.65% while inflows in the last 10 years entail an increase in low income rental prices of 6.76%.

The next to last row of Table 3 presents standardized coefficient, it reports the change in standard deviations of the dependent variable per standard deviation increase in the independent variable. A standard deviation increase in inflows increases low income rental prices by almost 0.05 standard deviations, a non-negligible effect.

Table A.2 provides a simple falsification test. We estimate specifications in which future levels of IDP inflows replace our main explanatory variable (i.e.; IDP inflows in $t-1$). Specifically, we include contemporaneous outflows and 12 forward lags. We expect no future lag to affect rental prices in t . Indeed, Table A.2 suggests that there is no consistent pattern between future inflows and actual prices, except what one could expect by chance.

To guarantee that the previous results are not an artifact of including several forward lags, in Table A.3 we present results with backward lags. Mirroring Table A.2, we include 12 lags and contemporaneous inflows. Strikingly, inflows in $t-1$ are positively and significantly correlated with average and low rental prices. Inflows in $t-1$ are also positively correlated with middle income rental prices (the estimate is marginally significant with a p -value of 0.14). This evidence, together

Table 5
IDP and rental prices by income level, IV.

	(1) Average	(2) Low Income	(3) Middle Income	(4) High Income
IDP Inflows t-1	−0.000646 (0.00594)	0.0147** (0.00670)	−0.00959 (0.00769)	−0.0391** (0.0180)
5 years Effect On Rental Prices	−0.059137	1.3503	−0.87795	−3.5834
10 years Effect On Rental Prices	−0.61135	13.9587	−9.07603	−37.0449
Standardized Coefficients	−0.00452	0.09731	−0.06795	−0.3117
Obs	832	832	832	832
First-stage F	37.33	37.33	37.33	37.33

Standard errors clustered at the city.year level in parenthesis. All variables are expressed in natural logarithms. All regressions include time and city fixed effects, city-specific linear trends and time dummies interacted with remoteness. All regressions are weighted by city population. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6
IDP and relative rental prices by income level, IV.

	(1) Average	(2) Low Income	(3) Middle Income	(4) High Income
IDP Inflows t-1	0.00971** (0.00477)	0.0251*** (0.00805)	0.000767 (0.00562)	−0.0288* (0.0151)
5 years Effect On Rental Prices	0.88904	2.2984	0.07022	−2.6353
10 years Effect On Rental Prices	9.19073	23.7609	0.72598	−27.2428
Standardized Coefficients	0.14712	0.40303	0.0111	−0.3406
Obs	832	832	832	832
First-stage F	37.33	37.33	37.33	37.33

Standard errors clustered at the city.year level in parenthesis. All variables are expressed in natural logarithms. All regressions include time and city fixed effects, city-specific linear trends and time dummies interacted with remoteness. All regressions are weighted by city population. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

with the graphs, gives support to using inflows in time period $t-1$. Note that the coefficients on contemporaneous inflows are almost identical (in magnitude and in statistical significance) to the coefficients on inflows in $t-1$ for all prices except high-income rentals. However, we are reluctant to use contemporaneous inflows because of reverse causality concerns.

4.2. Instrumental variables results

While the previous OLS results are consistent with a demand-side shock impact of IDP inflows on rental prices, the estimated coefficients might be biased. IDP do not choose their destinations randomly even if they are pushed by violence. In this subsection we present instrumental variables estimates that address and correct for this bias.

In Table 4 we explore the strength of the proposed IDP receptivity instrument, which was described in section 3 (see equation (2)).²⁷ Column 1 shows a positive and statistically significant unconditional relationship between IDP inflows and receptivity. Since both variables are logged, the point estimate can be interpreted as an elasticity which is very close to one. Our proposed instrument exhibits strong predictive power. In column 2 we add city and quarter fixed effects and find qualitatively similar results with an even smaller standard error for receptivity. In column 3, we add city-level linear trends. The implied first-stage F-statistic is 48.66 suggesting that, conditional on the aforementioned trends and both city and quarter fixed effects, receptivity is a strong instrument. Column 4 adds trends by remoteness. This specification

corresponds to the baseline specification. Adding trends by remoteness does not wash out the strong predictive power of our proposed instrument (the First-stage F is 37.44). Finally, the specification in column 5 adds our expanded set of controls (i.e., column 5 in Table 2). Results are similar.

In Table 5 we present IV estimates using our baseline specification to causally establish the impact of IDP inflows on rental prices for varying levels of income. From column 1 we observe that the effect of IDP inflows on average rentals is highly non-significant. This null finding hides important heterogeneous effects: column 2 reveals that low-income rentals increase with inflows while column 4 reveals that high-income rentals decrease with inflows. Both estimates are significant at the 5% level. There is no significant effect for middle-income rentals.

How large are these impacts? Again, the bottom of Table 5 presents different alternatives to gauge the magnitude of the effects. Standardized coefficients imply that one standard deviation increment in inflows increases low-income rentals by 0.1 standard deviations and decreases high-income rental by 0.3 standard deviations. The cumulative effect of inflows over the past 10 years is substantial: they increase low-income rental prices by 14% and decrease high-income rentals by 37%. In sum, the computed elasticities imply large effects of IDP inflows on rental prices.

How do these estimates compare with those found in previous research? The most comparable estimate comes from Saiz (2007) study on economic migrants. According to the author “Immigration inflows equal to 1% of a city’s population were associated with increases in average or median housing rents and prices of about 1%” (PP.346). We do not find an effect of inflows on average rental prices. Thus, the most comparable parameter is that for low-income rental prices (column 2 of Table 5). This parameter estimate is equal to 0.0147, significant at the 5% level. This estimate implies that an increase of forced immigration inflows equal to 1% of a city’s population is associated with

²⁷ We also experimented with other specifications for the reduced form relationship of IDP inflows and receptivity. We find that receptivity in $t-1$ is also a statistically significant predictor of IDP inflows in t . The point estimate is, however, four times smaller than for the case of receptivity in t . Adding receptivity in $t-1$ in the first stage does not quantitatively affect the IV results. No other lag of receptivity is statistically significant in the first stage.

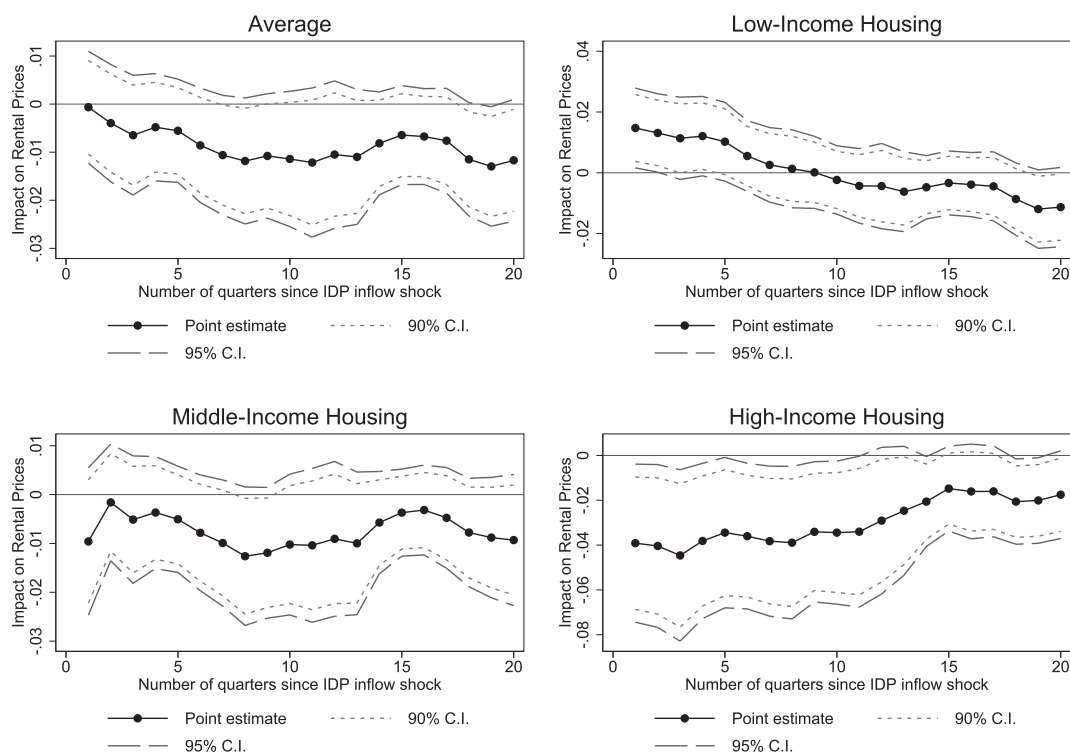


Fig. 4. Impact on rental prices over time, by income level (IV).

increases in low-income housing rents of 1.24%.²⁸ This is very similar to Saiz findings and it might be 24% larger because it focuses on the low-income segment of the housing market which receives a disproportionately large amount of IDP.

The IV estimate for low-income rental is almost two times the corresponding OLS estimate. This inflation in the IV coefficient is consistent with our presumption that IDP migrate to places where the cost of living is lower or with measurement error in the independent variable.

Now, although we focus on rental prices in levels, it is important to check what is the effect of IDP inflows on relative rental prices. To do this we divide rental prices by the city level CPI. This is important because IDP also affect the price of the representative consumption bundle. Indeed, appendix Table A.4 shows that the (IV) effect of IDP inflows on the city-level CPI is negative and significant, with an elasticity of -0.007 . This is consistent with research from other countries (see, for instance, Balkan and Tumen (2016)). Table 6 shows results using relative rental prices as the dependent variable. Results are similar to those using prices in levels: low-income (high-income) relative rental prices increase (decrease) with inflows. Importantly, the effect on the low-income segment of the market is much higher in comparison to the same estimate using prices in level (with relative prices the corresponding standardized coefficient is 0.4 vs. 0.1 with prices in levels). In contrast to results using price in levels, Table 6 shows a positive and significant impact of inflows on average relative rental prices.

How can we interpret the differential impact according to income? We will explain how these heterogeneous effects might arise in Section 6.1. But before performing robustness check and analyzing the potential channels underlying our reduce form results, we look at the

persistence of the IV effects on rental prices. To do so, we repeat in Fig. 4 the exercise carried in Fig. 3 although this time we present IV point estimates for the impact of different lags of IDP inflows on average, low-, middle-, and high-income rental prices. The key findings that emerges from Fig. 4 is that the effect of inflows on low-income rental prices lasts up to 5 quarters while the effect on high-income rental prices seems highly persistent (up to 11 quarters). Average and middle-income rental prices are not affected by any lag of IDP inflows.

5. Robustness checks

In this section we perform a series of robustness checks for our instrumental variables results.

In Table A.5 we present our main IV results using the receptivity instrument calculated using geodesic instead of road distances. This is an important robustness check because the earliest road network that we could obtain is from 2011 and, although there is no evidence for this, a concern of displacement affecting road construction might cast doubt on our results. In addition to that, we could not connect some municipalities to the road network. Results in Table A.5 alleviate these concerns. Qualitatively, results are equivalent to those in Table 5. The magnitude of the coefficient on low-income rentals using linear distances is almost unchanged. The coefficient on high-income rentals shrinks from -0.039 to -0.030 . In sum, we interpret this as evidence that our results are not dependent on the use of road distances.

Table A.6 presents our IV results when using our expanded set of controls. These are important variables which are related to inflows and also to housing rents. The outcome of adding these controls is that the previously significant parameter estimates remain equally significant but their magnitudes almost double. This might reflect the fact that these controls might be mediating part of the relationship between inflows and rental prices.

Table A.7 checks the sensitivity of our results when excluding outflows from municipalities within 25, 50, 75 and 100 km from the host city. The Table also reports the number of municipalities excluded and

²⁸ In our sample, 1% of average city population is 13,005 or 9.47 log points. An increase in our independent variable of 9.47 log points translates into an increase of $0.0147 \times 9.47 = 0.139$ log points in the dependent variable (which is the log of low income rental prices). The mean of this dependent variable is 4.53 log points. Thus, relative to its mean the increase in relative rental prices is $1.22\% (100 \times \exp(0.139) / \exp(4.53) = 1.24)$.

Table 7
Potential channels: IDP inflows and construction licenses.

	(1) OLS VIS	(2) OLS Non-VIS	(3) OLS Other	(4) IV VIS	(5) IV Non-VIS	(6) IV Other
IDP Inflows t-1	−4.461** (1.844)	3.760** (1.823)	0.490** (0.218)	−8.486** (3.279)	8.003** (3.485)	0.724* (0.400)
Obs	832	832	832	832	832	832
First-stage F	.	.	.	30.83	30.83	30.83

Standard errors clustered at the city,year level in parenthesis. Dependent variables are shares from 0 to 100. All right hand variables are expressed in natural logarithms. All regressions include time and city fixed effects, city-specific linear trends and time dummies interacted with remoteness and with land unavailability. All regressions are weighted by city population. VIS Licenses refer to social interest housing (see main text for details). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

the first-stage F in each case. Recall that there is a trade-off between relevance and endogeneity. Since IDP move to close cities, usually the department capital, the relevance of the first stage should decrease as we remove more municipalities (this is easily seen across panels in Table A.7); however, keeping close municipalities has the cost of generating results which might not be purged of endogeneity because of spatial correlation in unobservables.

Note that our results are robust to using the receptivity instrument excluding outflows within 25 and 50 km. We lose significance for low income rentals when excluding municipalities within 75 km but the size of the parameter remains invariant. However, this parameter estimate tends towards zero when excluding municipalities within 100 km. This is not surprising because excluding municipalities within 100 km renders our instrument weak: the first-stage F is 8.16, supporting the evidence that IDP move to close locations. However, the parameter estimate for high income rentals remains significant and similar in magnitude even when excluding municipalities within 100 km. The stability of the parameter estimates is reassuring and moderates concerns about spatial-induced endogeneity.

To further support our IV results we construct an enclave instrument following Morales (2018). Instead of weighting outflows from municipality m to city c with the road distance between m and c , we weight the outflows originating in municipality m by the fraction of immigrants in city c from municipality m using the census of 1993. Results are presented in Table A.8.

The idea of the relevance of this instrument is that IDP rely on networks created by previous immigrants. Notice that it is not clear which instrument does a better job in solving endogeneity: the enclave instrument is subject to the criticism that the factors that influenced the arrival of migrants to a municipality before 1993 (for example, a low unemployment rate) are likely to persist over time producing a mechanical correlation between contemporaneous IDP inflows and these persistent factors. On the other hand, the instrument using road distances is subject to the criticism of spatial correlation in unobservables. Nevertheless, the advantage of the road instrument is that it can be modified to lesser concerns of endogeneity: it is robust to excluding outflows from nearby municipalities. Such corrections cannot be done with the enclave instrument. This leads us to prefer our receptivity instrument. This is not to say that networks are not important. In the 2004 survey of IDP (Centro de Estudios sobre Desarrollo Económico, 2004), 78% of the displaced households report moving to the destination because they have families and friends while 45.45% report choosing their destination because it is close to the origin. These numbers mirror nicely the first-stage F-statistics: 93.91 for the enclave instrument (Table A.8) versus 37.33 for our receptivity (road distance) instrument (Table 5). However, we prefer the road distance instrument since we believe it does a better job in reducing concerns about endogeneity.

Results in Table A.8 are somehow similar to our IV results with some differences. The parameter estimate on low-income rentals is almost identical to the baseline and highly significant. The parameter estimate on high-income rentals is negative but lesser in magnitude than the

baseline and non-significant. The parameter estimate on average rental prices becomes positive and significant. We conclude that, our results for low-income rentals are robust to different instruments. However, again, we prefer the receptivity instrument. Indeed, an overidentification test when using both instruments rejects the null when the dependent variable is high-income rental but not when the dependent variables are low-income rentals, which is consistent with our idea that one instrument (we argue that the enclave instrument) might be endogenous.

Finally, Table A.9 presents unweighed results. The parameter estimate for low-income rentals becomes negative but highly non-significant. The parameter estimate for high-income rentals remains negative and significant but smaller in magnitude. How can we explain this? We believe there is a lot to be learnt from this finding. We hypothesize that the effect of IDP is precisely more important in the low-income housing market in large cities because in those cities it is harder to expand the supply of low income rental units. In addition to that, housing supply of high income rental units is less constrained across cities of different sizes. Indeed, in Table A.10, in the appendix, we show how quantitative housing deficits in urban areas (the municipalities seats) in 1993 are associated with population in that same year. In column (1), we show that population size is a strong predictor of quantitative housing deficits. We then divide all municipalities in Colombia by quintiles according to the distribution of population in 1993 and find that the higher the quintile, the larger the quantitative housing deficit of the municipality (column 2). This pattern becomes even stronger if we control for department level time invariant characteristics (column 3).

Additionally, we show in Table A.11 in the appendix that, for the sample of 13 host cities, population size is a strong predictor of IDP inflows from any given expelling municipality; even after controlling for the distance between destination and origin as well as for expelling municipality fixed effects. That is, we run the following regression:

$$\ln(IDP_{c,m}) = \alpha + \beta \ln(Population_c) + \eta' Distance_{c,m} + d_m + u_c, \quad (3)$$

where $IDP_{c,m}$ is the total number of IDP (between 1985 and 2010, in logs) who moved from expelling municipality m to host city c . $Distance_{c,m}$ is the distance between the two locations. d_m are expelling municipality fixed effects and u_c is a heteroskedasticity corrected error term. Note that the elasticity of population to IDP is not far from 1. To close, the evidence suggests that bigger cities have more problems absorbing IDP inflows. Since we do not have a large cross-section to interact inflows with population, we believe that our approach of weighing by population is sensible.

6. Potential channels

In this section, we explore two possible channels through which IDP arrivals might impact housing prices in Colombian cities.

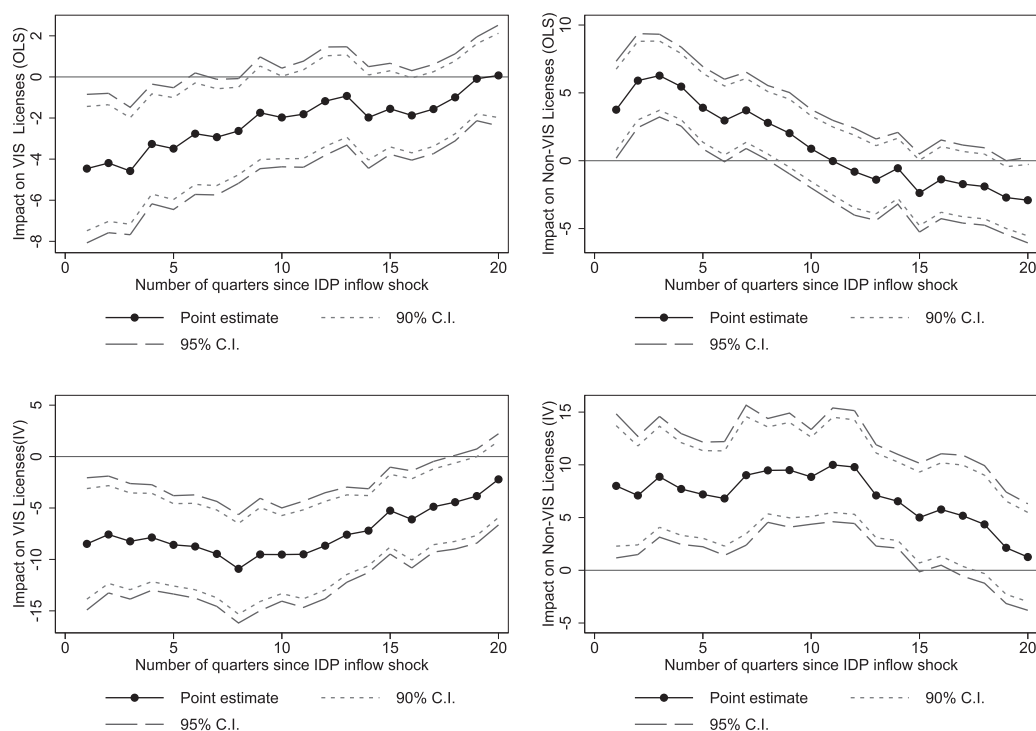


Fig. 5. Impact on construction licenses over time.

6.1. IDP, construction Sector's response, and housing deficit

We first study the empirical relationship between IDP and the response of the construction sector in the 13 largest Colombian cities. Our analysis uses shares of: new social interest housing construction licenses (VIS license, for its Spanish acronym), new non-social interest housing construction licenses (Non-vis) and new non-residential construction licenses (Other). We divide each of these categories by the sum of the three (times 100) to obtain the shares. To estimate the effect of IDP on the construction sector, we re-run equation (1) by OLS and IV, replacing housing prices with the share of each type of construction licenses. Furthermore, we follow Saiz (2010) and control for the share of unavailable land (fraction of land with slope greater than 15) in each recipient city interacted with time dummies. None of our results hinges on that control.

Before discussing the results, let us note that the denominator of the dependent variable (the total number of licenses) is invariant with respect to inflows, which further justifies the use of shares.

As Table 7 shows, the share of social interest housing negatively responds to the arrival of IDP. On the other hand, the shares of non-social interest and non-residential licenses increase with inflows. These results hold in both the OLS and IV estimates. It seems that non-social interest construction licenses crowd-out social interest construction licenses. A 1% increase in inflows decreases the share of social interest licenses by 0.08% and increases the share of non-social interest housing by 0.08%. These supply-side effects are consistent with the heterogeneous results for low-income and high-income rental prices.

Fig. 5 plots the coefficients on different OLS and IV regressions equivalent to those in Table 7 but replacing inflows in $t - 1$ by inflows in $t - 2$, $t - 3$, and so on until $t - 20$. For concreteness we focus on the two bottom figures, from left to right, the IV estimates for the share of licenses of social and non-social interest, respectively. Two features of these figures are worth highlighting. First, the impact of inflows on the share of licenses of social interest is negative, initially significant and monotonically increasing towards zero; the impact of inflows on

the share of licenses of non-social interest is positive, initially significant and (almost) monotonically decreasing towards zero. Second, the effects vanish at the same time, in quarter $t - 18$. Thus, inflows have a long-lasting impact on the housing market of up to 4 years.

The increase in the share of non-social interest construction licenses could be explained by the construction sector absorbing cheap non-skilled labor provided by IDP. Indeed, the survey of displaced people (Centro de Estudios sobre Desarrollo Económico, 2004) reveals that of the 2322 displaced household-heads who migrate to urban areas, one fifth are employed in the construction sector, a figure that ranks second just after retail (25%).

In relation to this, focused in the period 2001–2005, Calderón-Mejía and Ibáñez (2015) show that large IDP inflows induce a substantial reduction in wages for urban unskilled workers. We make use of the Calderón-Mejía and Ibáñez (2015) dataset²⁹ and regress log real hourly wages in the construction sector (at the individual level) against our baseline independent variables in the OLS and IV framework.³⁰ Column 1 and 2 of Table 8 show OLS and IV estimates (respectively) with our baseline independent variables. In columns 3 and 4 we add individual-level controls which are typical of the so-called Mincer equations: experience, experience squared, years of schooling and gender.

Notice that OLS estimates are negative but non-significant and much lesser in magnitude than the IV estimates. The OLS seem to be upwards biased as it is to be expected if IDP prefer to move to cities with better economic conditions. The IV are negative and significant at the 5% level. The estimate in column (2) implies that a 1% increase in IDP inflows in quarter $t - 1$ decreases the hourly wage of construction workers by 0.35%. The corresponding estimate when including individual-level controls is almost identical. We interpret these results as evidence of IDP inflows decreasing construction costs and through

²⁹ We thank the authors for sharing their data.

³⁰ We are careful to include just the CIU classification which represents construction of units for residential use, that is CIU rev 3 4521.

Table 8
Log hourly wages in the construction sector and IDP (2001QI–2005QIII).

	(1) OLS	(2) IV	(3) OLS	(4) IV
IDP Inflows t-1	−0.0508 (0.105)	−0.347** (0.166)	−0.0793 (0.0965)	−0.323** (0.145)
Observations	13,270	13,270	13,127	13,127
First Stage F	.	14.60	.	14.30
Individual-level controls	N	N	Y	Y
Observations	13,270	13,270	13,127	13,127

Standard errors clustered at the city.year level in parentheses. All variables are expressed in natural logarithms. All regressions include time and city fixed effects, city-specific linear trends and time dummies interacted with remoteness and with land unavailability. All regressions include individual-level sampling weights. Columns (3) and (4) include the following individual-level controls: experience, experience squared, years of schooling and gender. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 9
Potential channels: IDP inflows and housing deficit (OLS).

Dependent Variable:	Housing Deficit			
	Overall		Quantitative	
	(1)	(2)	(3)	(4)
Cumulative IDP Inflow t-1/Population t	0.137** (0.0597)	0.325*** (0.113)	0.171** (0.0843)	0.421*** (0.148)
Observations	1956	366	1956	366
Municipalities in Sample	All	Urbanized	All	Urbanized
Municipality FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Standard errors clustered at the municipality level in parenthesis. Housing deficit is computed as a share of total households in the municipality (based on 1993 and 2005 national census). The variable Cumulative IDP inflow/Population for 1993 (2005) is the cumulative sum of IDP inflows from 1985 (1997) to 1992 (2004) normalized by 1993 (2005) population. All regressions include time and municipality fixed effects and time dummies interacted with remoteness and land unavailability. Urbanized municipalities are those with an urbanization rate above 0.66 in 2005 (i.e., 75th percentile in the distribution of urbanization rates). All regressions are weighted by the number of households in the municipality. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

this channel potentially reducing the price of high income rental-units.

The decreasing share of social interest housing is not surprising given the constrained capacity of the low-income housing sector, particularly due to the scarcity of affordable land for VIS housing (see our discussion in the context section). The results in Table 7 also suggest that new IDP arrivals are not met with a government response providing free or subsidized housing units.³¹ Given that an influx of internally displaced individuals results in a population increase that should automatically generate increased demand for housing, and particularly for low-income rental units, the lack of a positive response in the supply of (social interest) housing appears at least partly responsible for the increase in low-income rental.³²

We next provide long-run evidence towards the existence of a large excess of demand in the low-income areas of host cities exacerbated by the arrival of IDP. We use housing deficits as defined in Section 2 and in appendix Table A.1. Although census data are only available for 1993 and 2005, we think that this time span is useful and informative about the longer term effects of IDP inflows. Furthermore, we now have

a larger cross-section to exploit within-municipality variation before and after the displacement crisis of the early 2000's. We estimate the following equation:

$$HD_{m,t} = \alpha + \beta \frac{\sum_{s=1}^T Inflows_{m,t-s}}{Population_{m,t}} + d_m + d_t + \gamma' X_{c,t} + u_{m,t}, \quad (4)$$

where the subscripts m and t denote municipality and year, respectively. We use data for two censuses: $t = 1993, 2005$. The dependent variable $HD_{m,t}$ is the housing deficit, measured as a rate (i.e., number of households with deficit divided by the total number of households in each municipality). We focus on two measures of deficit: overall and quantitative.³³ We do not find significant results when using qualitative housing deficit. Our treatment is a measure of IDP pressure which is computed as the cumulative sum of IDP inflows during the 8 years previous to the 1993 (2005) census divided by population in t . d_m and d_t denote municipality and year fixed effects, respectively. $X_{c,t}$ includes city-level linear trends and time interacted controls: average road distance and land unavailability. Finally, u is an error term clustered at the municipality level. Given the large heterogeneity in the number of households across municipalities, we weight all the regressions by the number of households in each municipality.

In Table 9 we present OLS results of the fixed effects model. In column (1) we focus in all municipalities and find a statistically significant and positive association between cumulative IDP inflows and overall

³¹ It is important to note that the participation of the private sector in housing construction for low-income segment is marginal (World Bank, 2010).

³² Case study evidence for Bogotá (Vidal-López et al., 2011) points in that direction; the following paragraph succinctly summarizes the idea: "Another point of complaint by local citizen is the lack of available housing. Due to the influx of large amounts of IDP housing has become scarce and rents has been increasing. This is particularly problematic for those living on minimum wages. They often blame the IDP for the rise in their living costs."

³³ Overall is the sum of quantitative and qualitative (see section 2).

Table 10
Potential channels: IDP inflows and housing deficit (IV).

Dependent Variable:	Housing Deficit			
	Overall		Quantitative	
	(1)	(2)	(3)	(4)
Cumulative IDP Inflow t-1/Population t	0.558* (0.295)	1.060 (0.650)	0.428** (0.168)	0.839** (0.412)
Observations	1956	366	1956	366
Municipalities in Sample	All	Urbanized	All	Urbanized
Municipality FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
First-stage F	19.01	6.3	19.01	6.3

Standard errors clustered at the municipality level in parenthesis. Housing deficit is computed as a ratio where the denominator is the total number of households in the municipality (based on 1993 and 2005 national census). The variable Cumulative IDP inflow/Population for 1993 (2005) is the cumulative sum of IDP inflows from 1985 (1997) to 1992 (2004) normalized by 1993 (2005) population. The instrumental variable is the cumulative sum (over the same period as the endogenous IDP inflows) of the receptivity measure. All regressions include time and municipality fixed effects and time dummies interacted with remoteness and land unavailability. Urbanized municipalities are those with an urbanization rate above 0.66 in 2005 (i.e., 75th percentile in the distribution of urbanization rates). All regressions are weighted by the number of households in the municipality. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 11
Potential channels: IDP inflows and homicides.

	(1)	(2)	(3)	(4)
	OLS	OLS	IV	IV
IDP Inflows t-1	4.730*** (1.201)	4.616*** (1.109)	7.748*** (1.978)	7.585*** (1.986)
Observations	823	823	823	823
Expanded Set of Controls	N	Y	N	Y
First-stage F	.	.	37.70	43.36

Standard errors clustered at the city,year level in parenthesis. Dependent variables are rates per 100,000 inhabitants. All right hand variables are expressed in natural logarithms. All regressions include time and city fixed effects, city-specific linear trends and time dummies interacted with remoteness. The expanded set of controls includes population, tax revenues, and public school teachers per student. All regressions are weighted by city population. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

housing deficit. This association remains strongly statistically significant and more than doubles when we restrict our sample to highly urbanized municipalities, in column 2.³⁴ This larger impact for highly urbanized municipalities is consistent with the fact that IDP tend to migrate to urban areas. In column (3) and (4) we find the same pattern when we focus on quantitative housing deficit. Nonetheless, the results in Table 9 are likely to be downward biased for two main reasons: time-varying omitted variables and measurement error.³⁵

In Table 10 we instrument cumulative IDP inflows with our receptivity instrument that now uses the cumulative outflows over the corresponding period. We now find a larger effects of IDP inflows on both overall and quantitative housing deficit. We also find the same pattern as in Table 9. That is, the causal impact of IDP inflows on housing deficits is substantially larger in highly urbanized areas. The estimated effects are economically large: an increase of IDP equivalent to 10 percent of the population in the host city would require an increase of 4.3 percent in the stock of houses in order to provide a home to every household in the municipality (see column 3 in Table 10). Do notice

that we lose conventional statistical significance when the dependent variable is the overall housing deficit and when the sample is restricted to highly urbanized municipalities.

6.2. IDP and crime

We now study crime as a potential underlying mechanism through which IDP arrivals might impact rental housing prices. A priori, there are reasons to believe that crime increases with IDP inflows. We know that displaced people have more young men when compared to the general population; they are poor and lack economic opportunities; in sum, they might be more vulnerable to engage in criminal activities. Alternatively, clashes with the local population and coexistence problems might result in increased crimes. Furthermore, deteriorating economic conditions can become fertile ground for crime and it has been already documented that IDP inflows increase unemployment rates for the non-displaced (Calderón-Mejía and Ibáñez, 2015).

Anecdotal and case study evidence suggest that IDP inflows do increase crime in Colombia (see online appendix for this evidence). Beyond acting as a potential channel explaining our results and given the state of the literature, analyzing the effects of forced migration on criminality is an important endeavor.³⁶

Before proceeding with the results, it is important to note that with our data it is impossible to establish who is the victim and who is the victimizer; crime might increase because displaced people engage

³⁴ We define highly urbanized municipalities as those with an urbanization rate above 0.66 in 2005 (i.e., 75th percentile in the distribution of urbanization rates).

³⁵ A potential omitted variable bias could arise if IDP migrate to locations where housing deficit is expected to be less problematic (e.g., due to, for instance, a developing construction sector which might provide job opportunities). When it comes to measurement error, attenuation bias could be specially worrisome in a fixed effect model if measurement error is just serially uncorrelated noise.

³⁶ Besley and Hannes (2012) show that violent crime reduces housing prices.

in criminal activity, because usual residents engage in criminal activity, or both. With this caveat in mind, Table 11 explores the relationship between IDP arrivals and homicides. The dependent variable is the homicide rate per 100,000 inhabitants. Column 1 presents the OLS results where the right hand variables are those in Table 5 (i.e., our baseline specification). We find a positive and highly statistically significant association between lagged inflows and homicides: a 10% increase in IDP inflows increases the homicide rate by 0.47. Column 3 presents the corresponding instrumental variables results. The IV parameter estimate is 1.6 times the OLS parameter estimate. This is consistent with displaced people moving to cities where criminality or violence is lower. Column (2) and (4) add population and the economic activity (tax revenues) and amenities (teachers per students) controls. We do not control for outflows since this is a particularly bad control when the dependent variable is the homicides rate: both outflows and homicides are a measure of violence.³⁷ With these additional controls the OLS parameter estimate (column 2) is 4.6 and the IV parameter estimate (column 4) is 64% larger and equal to 7.6; both estimates are significant at much less than the 1% level (t-statistics are greater than 4). The IV estimate implies that a 10% increase in IDP inflows increases the homicide rate by 0.76 homicides per 100,000 inhabitants. To have an idea of the magnitude of this estimate recall that the mean and variance of the homicide rate are 12.0 and 8.5, respectively (see 1). Thus the IV estimate implies that a 10% increase in IDP inflows increases the homicide rate by 6.3% with respect to its mean.

Table A.13 presents a simple falsification test where we replace the homicide rate by the number of natural deaths per 100,000 inhabitants. If health is not severely affected by the arrival of IDP and if the homicides result is not driven by some peculiarity of the vital statistics data, we should expect no relationship between natural deaths and IDP inflows. Reassuringly, the relationship between IDP inflows and natural deaths is never significant.

An important concern that threatens the validity of our instrument is that outflows might be produced by violence in close proximity to the receiving city. In other words, our instrument could be capturing the spatial correlation of violence and this is more concerning when the dependent variable is the homicides rate instead of rental prices. In order to lessen this concern, appendix Table A.14 redefines the instrument as the weighed sum of outflows originating in municipalities at least 25, 50, 75 or 100 km away from the city of reception. Again, there is a trade-off when doing this since the majority of displaced people move to cities close to the expelling municipalities. Nevertheless, Table A.14 shows that the parameter estimate of inflows is practically unchanged when the instrument excludes outflows from close municipalities, even when we exclude municipalities within 100 km of the host cities. The results of this exercise

allow us to be more confident in giving a causal interpretation to our estimates.

The results in this section suggest that IDP arrivals are accompanied by a notable deterioration in security conditions in Colombia's largest cities, likely putting downward pressure on housing prices. We expect that as displaced individuals and families arrive to Colombia's cities, very few are able to afford housing in the high-income market segment, imperceptibly impacting demand for high-income housing rentals. With few IDP demanding high-income rental units, we find plausible that the association of IDP arrivals with noticeably higher levels of crime could help to explain the decrease in high-income rental prices.

7. Conclusions

We leverage a novel dataset with high quality administrative panel data on quarterly IDP flows across Colombian municipalities to identify the causal impact of IDP inflows on rental prices by income levels for the 13 largest Colombian cities during the period 1999–2014. Given its quality, high frequency, and temporal extension, our data provides a meaningful source of variation in IDP flows to identify the main effect of interest. Indeed, unlike previous works which used different proxies for the intensity of IDP inflows due to the lack of IDP data at the host level, our paper exploits actual IDP figures at the municipality level.

Following an instrumental variables approach to address the potential endogeneity of the location choices of IDP, we find that IDP inflows seem to be unrelated with average rental prices. However, this non-result masks important heterogeneous effects. IDP inflows increase low-income rental prices but decrease high-income rental prices.

We provide evidence on two potential mechanisms underlying these heterogeneous results. First, IDP inflows differentially impact the construction sector in Colombian cities. As IDP arrive to large cities, the share of new construction licenses of social interest are crowded out by construction licenses of non-social interest. We argue that this finding is consistent with IDP fueling the construction sector in rich areas (lowering rental prices) as well as exerting a sizeable demand shock in low-income areas. In the latter, an excess of demand is not followed by an increasing supply of rental units. Consistent with these findings, we exploit census data from 1993 to 2005 to identify and quantify large housing deficits driven by IDP shocks but only in highly urbanized municipalities. Second, we provide the first empirical evidence on IDP inflows increasing criminality as captured by homicides. This increase in criminality might exert downwards pressure on high-income rentals. The effect of IDP inflows on criminality and on poverty is part of our ongoing research agenda.

³⁷ This is not to say that our main conclusions do not hold when controlling for outflows but the parameter estimate on inflows become lesser in magnitude and the statistical significance is reduced. If we control for outflows in a specification otherwise identical to that of column 2 we obtain a parameter estimate of 2.3 (s.e. = 1.2). If we control for outflows in a specification otherwise identical to that of column 4 we obtain a parameter estimate of 5.0 (s.e. = 2.9).

Appendix. Additional Tables

Table A.1
Data Description.

Variable	Definition	Source
Rental Prices	Consumer Index Price (CPI) for housing item (in logs). It captures the prices of housing rentals and imputed owner occupied dwelling rentals in both formal and informal housing markets. These measures control for changes in housing quality. We use both the average rental prices and the rental prices by income level (low, middle and high income). Time Period: 1999-I to 2014-IV.	<i>Establishments Surveys from National Census of 2005, DANE.</i>
Consumer Price Index	Index for measuring the average percentage variation of the prices of a set of final goods and services demanded by consumers in the 13 host cities. Time Period: 1999-I to 2014-IV.	<i>Establishments Surveys from National Census of 2005, DANE.</i>
Real Rental Prices	Rental prices deflated by the Consumer Price Index (CPI) Time Period: 1999-I to 2014-IV.	<i>Establishments Surveys from National Census of 2005, DANE.</i>
Share of New Construction Licenses	Share of new construction licenses in the host cities (in logs). We use the share of new social interest housing (VIS, for its Spanish acronym; this includes subsidized and free housing provided to needy families) licenses, the share of new non-social housing (Non-VIS), and the share of new non-residential construction licenses. Time Period: 1999-I to 2014-IV.	<i>Contractor License Boards (Curadurías urbanas) and Planning Offices Surveys, DANE.</i>
Quantitative Housing Deficit	Number of houses that need to be replaced or constructed to provide a home to every household. Time Period: 1993 and 2005.	<i>National Census of 1993 and 2005, DANE.</i>
Qualitative Housing Deficit	Number of houses that need to be expanded or improved due to (addressable) deficiencies such as overcrowding, inadequate utilities, and (not major) building deficiencies. Time Period: 1993 and 2005.	<i>National Census of 1993 and 2005, DANE.</i>
Overall Housing Deficit	Sum of quantitative and qualitative housing deficits. Time Period: 1993 and 2005.	<i>National Census of 1993 and 2005, DANE.</i>
Homicide Rate	Total homicides per 100,000 inhabitants in the host cities. Time Period: 1999-I to 2014-IV.	<i>Vital Statistics, DANE.</i>
Natural Deaths Rate	Total natural deaths per 100,000 inhabitants in host cities. Time Period: 1999-I to 2014-IV.	<i>Vital Statistics, DANE.</i>
IDP Inflows	Inflows of internally displaced persons (IDPs) received by host cities. Measured quarterly from 1997-I to 2014-IV.	<i>Victims' Register (Registro Único de Víctimas (RUV)).</i>
Receptivity Instrument	Weighted sum of the IDP outflows from all Colombian municipalities (except the host city), where the weights are (the inverse of) either the road or geodesic distances between the expelling municipalities and the host city. Time Period: 1999-I to 2014-IV.	<i>Outflows from Victims' Register (Registro Único de Víctimas (RUV)) and road distances from SIGOT-IGAC.</i>
Enclave Instrument	Weighted sum of the IDP outflows from all Colombian municipalities (except the host city), where the weights are share of migrants of the expelling municipalities who lived in the host city in 1993. Time Period: 1999-I to 2014-IV.	<i>Outflows from Victims' Register (Registro Único de Víctimas (RUV)) and the share of migrants from National Census of 1993, DANE.</i>
IDP Outflows	Outflows of internally displaced persons (IDPs) expelled by Colombian municipalities (in logs). Time Period: 1999-I to 2014-IV.	<i>Victims' Register (Registro Único de Víctimas (RUV)).</i>
Population	Total municipal population (in logs). Time Period: 1999 to 2014.	<i>Population Projections based on National Census of 1993 and 2005, DANE.</i>
Tax Revenues	Municipal tax revenues from industry and commerce (in logs). Time Period: 1999 to 2014.	<i>Municipal Panel, CEDE - Universidad de los Andes.</i>
Public School Teachers per Student	Ratio of public school teachers to students (in logs). Time Period: 1999 to 2014.	<i>Municipal Panel, CEDE - Universidad de los Andes.</i>
Average Road Distance	Average road distance from the host city to all other Colombian municipalities (in logs).	<i>SIGOT-IGAC.</i>
Fraction of Unavailable Land	Fraction of municipality's total land that exhibits slopes above 15%.	<i>Author's calculation based on Nunn and Puga (2012).</i>

Table A.2

Falsification Test: Forward IDP Inflows and Rental Prices by Income Level, OLS.

	(1) Average	(2) Low Income	(3) Middle Income	(4) High Income
IDP Inflows in t	0.00367** (0.00174)	0.00244 (0.00319)	0.00462** (0.00200)	0.00278 (0.00454)
IDP Inflows in t+1	0.00181 (0.00131)	0.00149 (0.00203)	0.00218 (0.00181)	−0.00213 (0.00419)
IDP Inflows in t+2	0.00144 (0.00140)	0.00110 (0.00226)	0.00223 (0.00168)	−0.00437 (0.00410)
IDP Inflows in t+3	0.00116 (0.00171)	0.00280 (0.00262)	0.000606 (0.00188)	−0.000286 (0.00396)
IDP Inflows in t+4	0.000775 (0.00181)	0.00147 (0.00262)	−0.000000170 (0.00199)	0.00242 (0.00407)
IDP Inflows in t+5	0.000892 (0.00151)	0.00239 (0.00259)	0.000611 (0.00169)	−0.00534 (0.00432)
IDP Inflows in t+6	−0.00109 (0.00163)	0.00152 (0.00242)	−0.00292 (0.00193)	0.00140 (0.00365)

(continued on next page)

Table A.2 (continued)

	(1) Average	(2) Low Income	(3) Middle Income	(4) High Income
IDP Inflows in t+7	0.00169 (0.00144)	0.00514** (0.00234)	−0.000752 (0.00206)	0.00266 (0.00411)
IDP Inflows in t+8	−0.000781 (0.00179)	−0.00144 (0.00278)	0.000101 (0.00228)	−0.00124 (0.00399)
IDP Inflows in t+9	−0.000650 (0.00151)	0.00114 (0.00217)	−0.00206 (0.00196)	−0.00146 (0.00381)
IDP Inflows in t+10	−0.00164 (0.00222)	0.000433 (0.00269)	−0.00392 (0.00275)	0.00227 (0.00371)
IDP Inflows in t+11	−0.000143 (0.00162)	−0.00126 (0.00264)	0.000170 (0.00213)	0.00258 (0.00317)
IDP Inflows in t+12	0.000124 (0.00216)	−0.00141 (0.00305)	−0.000428 (0.00247)	0.0109** (0.00546)
Observations	716	716	716	716
Joint significance of future inflows (p-value)	0.627	0.501	0.729	0.331

Standard errors clustered at the city.year level in parenthesis. All variables are expressed in natural logarithms. All regressions include time and city fixed effects, city-specific linear trends and time dummies interacted with remoteness. All regressions are weighted by city population. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A.3

Lagged IDP Inflows and Rental Prices by Income Level, OLS.

	(1) Average	(2) Low Income	(3) Middle Income	(4) High Income
IDP Inflows in t	0.00339* (0.00200)	0.00403* (0.00238)	0.00388 (0.00256)	−0.00767 (0.00641)
IDP Inflows t-1	0.00358* (0.00204)	0.00450* (0.00242)	0.00338 (0.00226)	0.00149 (0.00314)
IDP Inflows in t-2	0.00154 (0.00147)	0.00176 (0.00190)	0.00161 (0.00166)	−0.00114 (0.00321)
IDP Inflows in t-3	−0.000816 (0.00214)	−0.000241 (0.00205)	−0.000869 (0.00283)	−0.00273 (0.00333)
IDP Inflows in t-4	0.000286 (0.00166)	0.00199 (0.00224)	−0.00121 (0.00176)	0.00473 (0.00375)
IDP Inflows in t-5	0.000366 (0.00197)	0.00127 (0.00223)	−0.000291 (0.00215)	0.000701 (0.00379)
IDP Inflows in t-6	−0.000609 (0.00137)	0.000107 (0.00160)	−0.00147 (0.00158)	0.00201 (0.00319)
IDP Inflows in t-7	0.000492 (0.00186)	0.00248 (0.00168)	−0.000562 (0.00271)	−0.000919 (0.00319)
IDP Inflows in t-8	0.00120 (0.00164)	0.000570 (0.00193)	0.00151 (0.00202)	0.00233 (0.00354)
IDP Inflows in t-9	0.00105 (0.00146)	−0.000461 (0.00213)	0.00192 (0.00180)	0.000704 (0.00261)
IDP Inflows in t-10	0.00219* (0.00132)	−0.000752 (0.00170)	0.00399** (0.00169)	0.00441 (0.00344)
IDP Inflows in t-11	0.00143 (0.00146)	−0.000565 (0.00209)	0.00285* (0.00161)	0.00337 (0.00293)
IDP Inflows in t-12	−0.000547 (0.00188)	−0.00352 (0.00267)	0.00135 (0.00230)	0.00381 (0.00463)
Observations	780	780	780	780
Joint significance of past inflows (p-value)	0.931	0.375	0.836	0.438

Standard errors clustered at the city.year level in parenthesis. All variables are expressed in natural logarithms. All regressions include time and city fixed effects, city-specific linear trends and time dummies interacted with remoteness. All regressions are weighted by city population. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A.4

IDP and city-level CPI.

	(1) OLS	(2) IV
IDP Inflows t-1	0.000556 (0.00122)	−0.00717** (0.00324)
Obs	832	832
First-stage F		37.33

Standard errors clustered at the city.year level in parenthesis. All variables are expressed in natural logarithms. All regressions include time and city fixed effects, city-specific linear trends and time dummies interacted with remoteness. All regressions are weighted by city population. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A.5

IDP and rental prices by income level, instrumental variables results with receptivity using linear distances.

	(1) Average	(2) Low Income	(3) Middle Income	(4) High Income
IDP Inflows t-1	0.00385 (0.00449)	0.0157*** (0.00553)	−0.00268 (0.00587)	−0.0293** (0.0141)
Obs	832	832	832	832
First-stage F	61.03	61.03	61.03	61.03

Standard errors clustered at the city.year level in parenthesis. All variables are expressed in natural logarithms. All regressions include time and city fixed effects, city-specific linear trends and time dummies interacted with remoteness. All regressions are weighted by city population. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A.6

Robustness Check: IDP and rental prices by income level, instrumental variables results with additional controls.

	(1) Average	(2) Low Income	(3) Middle Income	(4) High Income
IDP Inflows t-1	−0.00101 (0.00784)	0.0240** (0.0101)	−0.0148 (0.0100)	−0.0672** (0.0265)
Obs	832	832	832	832
First-stage F	25.86	25.86	25.86	25.86

Standard errors clustered at the city.year level in parenthesis. All variables are expressed in natural logarithms. All regressions include time and city fixed effects, city-specific linear trends and time dummies interacted with remoteness. Regressions also include the following controls: population, IDP outflows, tax revenues and the number of public school teachers per student (all in logs) All regressions are weighted by city population. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A.7

Robustness Check: IDP and rental prices. Instrument excluding nearby municipalities

	(1) Average	(2) Low Income	(3) Middle Income	(4) High Income
<i>Instrument excluding municipalities within 25 km</i>				
IDP Inflows t-1	−0.000965 (0.00626)	0.0161** (0.00708)	−0.0110 (0.00826)	−0.0455** (0.0195)
Excluded Municipalities (average)	3.615	3.615	3.615	3.615
First-stage F	24.60	24.60	24.60	24.60
<i>Instrument excluding municipalities within 50 km</i>				
IDP Inflows t-1	−0.00385 (0.00755)	0.0157** (0.00777)	−0.0156 (0.0101)	−0.0516** (0.0226)
Excluded Municipalities (average)	11.692	11.692	11.692	11.692
First-stage F	18.45	18.45	18.45	18.45
<i>Instrument excluding municipalities within 75 km</i>				
IDP Inflows t-1	−0.00731 (0.00906)	0.0131 (0.00890)	−0.0194 (0.0121)	−0.0608** (0.0260)
Excluded Municipalities (average)	23.615	23.615	23.615	23.615
First-stage F	13.52	13.52	13.52	13.52
<i>Instrument excluding municipalities within 100 km</i>				
IDP Inflows t-1	−0.0137 (0.0116)	0.000511 (0.0123)	−0.0237 (0.0148)	−0.0602** (0.0285)
Excluded Municipalities (average)	36.538	36.538	36.538	36.538
First-stage F	8.164	8.164	8.164	8.164

Standard errors clustered at the city.year level in parenthesis. All variables are expressed in natural logarithms. All regressions include time and city fixed effects, city-specific linear trends and time dummies interacted with remoteness. All regressions are weighted by city population. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A.8

IDP and Rental Prices by Income Level, IV: Using enclave instrument.

	(1) Average	(2) Low Income	(3) Middle Income	(4) High Income
IDP Inflows t-1	0.00816** (0.00316)	0.0133*** (0.00466)	0.00480 (0.00372)	−0.00713 (0.00863)
Obs	832	832	832	832
First-stage F	93.91	93.91	93.91	93.91

Standard errors clustered at the city.year level in parenthesis. All variables are expressed in natural logarithms. All regressions include time and city fixed effects, city-specific linear trends and time dummies interacted with remoteness. All regressions are weighted by city population. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A.9

Robustness Check: Unweighted Results.

	(1) Average	(2) Low Income	(3) Middle Income	(4) High Income
IDP Inflows t-1	−0.00522 (0.00507)	−0.00115 (0.00601)	−0.00956* (0.00572)	−0.0193* (0.0108)
Obs	832	832	832	832
First-stage F	31.15	31.15	31.15	31.15

Standard errors clustered at the city.year level in parenthesis. All variables are expressed in natural logarithms. All regressions include time and city fixed effects, city-specific linear trends and time dummies interacted with remoteness. All regressions are weighted by city population. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A.10

Population and Housing Deficits.

Dependent Variable:	Quantitative Housing Deficit in Municipality Seat		
	(1)	(2)	(3)
Population in 1993 (in logs)	0.00924*** (0.00258)		
Quantile 2 of 1993 Population		−0.00121 (0.00805)	0.00864(0.00749)
Quantile 3 of 1993 Population		0.0111 (0.00793)	0.0295*** (0.00768)
Quantile 4 of 1993 Population		0.0115 (0.00771)	0.0400*** (0.00791)
Quantile 5 of 1993 Population		0.0300*** (0.00778)	0.0602*** (0.00766)
Observations	1029	1029	1029
Department FE	N	N	Y

Robust standard errors in parenthesis. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A.11

IDP Flows, Population and Distances.

Dependent Variable:	IDP Arriving to Host City from Municipality (1985–2010, in logs)			
	(1)	(2)	(3)	(4)
1993 Population in Host City (in logs)	0.809*** (0.0115)	0.789*** (0.0114)	0.834*** (0.0111)	0.807*** (0.0112)
Distance Between Locations (in kms, by routes)	−0.00247*** (0.0000681)	−0.00333*** (0.0000547)		
Geodesic Distance Between Locations (in kms)			−0.00318*** (0.0000830)	−0.00418*** (0.0000662)
Observations	13,143	13,143	13,804	13,804
Expelling Municipality FE	N	Y	N	Y

Standard errors clustered at the expelling municipality level in parenthesis. Each observation for the dependent variable is 1 + the total number of IDP (in logs) arrived to one of the 13 Colombian largest cities from an expelling municipality between 1985 and 2010. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A.12
Potential Channels: IDP Inflows and Housing Deficit (IV).

Dependent Variable:	Housing Deficit			
	Qualitative		Quantitative	
	(1)	(2)	in Mun. Seat (3)	Outside Mun. Seat (4)
Cumulative IDP Inflow t-1/Population t	0.131 (0.204)	0.221 (0.332)	0.830* (0.432)	0.0373 (0.283)
Observations	1956	366	366	360
Municipalities in Sample	All	Urbanized	Urbanized	Urbanized
Municipality FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
First-stage F	19.01	6.295	6.295	6.515

Standard errors clustered at the municipality level in parenthesis. Housing deficit is computed as a ratio where the denominator is the total number of households in the municipality (based on 1993 and 2005 national census). The variable Cumulative IDP inflow/Population for 1993 (2005) is the cumulative sum of IDP inflows from 1985 (1997) to 1992 (2004) normalized by 1993 (2005) population. The instrumental variable is the cumulative sum (over the same period as the endogenous IDP inflows) of the receptivity measure. All regressions include time and city fixed effects, city-specific linear trends and time dummies interacted with remoteness. Urbanized municipalities are those with an urbanization rate above 0.66 in 2005 (i.e., 75th percentile in the distribution of urbanization rates). All regressions are weighted by the number of households in the municipality. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.13
Falsification test: IDP Inflows and Natural Deaths.

	(1)OLS	(2)OLS	(3)IV	(4)IV
IDP Inflows t-1	0.297 (1.240)	0.00986 (1.198)	3.636 (5.092)	3.454 (5.027)
Observations	828	828	828	828
Expanded Set of Controls	N	Y	N	Y
First-stage F	.	.	37.16	42.98

Standard errors clustered at the city/year level in parenthesis. Dependent variables are rates per 100,000 inhabitants. All right hand variables are expressed in natural logarithms. All regressions include time and city fixed effects, city-specific linear trends and time dummies interacted with remoteness. The expanded set of controls includes population, tax revenues, and public school teachers per student. All regressions are weighted by city population. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.14
Potential Channels: IDP Inflows and Homicides. Instrument excluding close municipalities.

	(1) Threshold: 25km	(2) Threshold: 50km	(3) Threshold: 75km	(4) Threshold: 100km
IDP Inflows t-1	7.791*** (2.057)	7.977*** (2.130)	7.807*** (2.210)	6.979** (2.853)
Excluded Municipalities (average)	3.615	11.692	23.615	36.538
First-stage F	24.6	18.45	13.52	8.164

Standard errors clustered at the city/year level in parenthesis. Dependent variables are rates per 100,000 inhabitants. All right hand variables are expressed in natural logarithms. All regressions include time and city fixed effects, city-specific linear trends and time dummies interacted with remoteness. All regressions are weighted by city population. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix B. Supplementary data

Supplementary data related to this article can be found at <https://doi.org/10.1016/j.jdeveco.2018.05.006>.

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