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Unexpected Guests: The Impact of Internal Displacement Inflows on Rental Prices in Colombian Host Cities

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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.

JEL: J1, O15, R23, R31

Keywords: Internal Displaced People, Migration, Crime, Rental Prices

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> *"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 (López et al., 2011)

Since the beginning of the Colombian conflict in the 60s, nearly 7 million individuals have been forcibly displaced from their homes.[1]. A feature that stands out for the Colombian case is that Internal Displaced People (IDP, from now on) typically originate from small, peripheral, rural areas, and mostly resettle in larger cities, particularly departmental capitals (Dueñas and Zuluaga, 2014). Consequently, Colombian IDP tends to be poor and many lack the skills necessary for work in urban areas (Ibañez, 2008). Moreover, as the intensity of the conflict varies over time and across the Colombian territory, the intensity and timing of this large rural-urban migration phenomenon also varies across host cities. For several host cities the size of IDP inflows is considerably large and and represents a sizable shock to local markets.

In this paper we examine the economic impact of these large inflows of IDP on rental prices. Estimating unbiased effects of forced migration inflows on host communities advances our understanding of how they interact with markets and provides a rational basis for policy. This is indeed an urgent policy issue as forced migration reached in 2015 its global highest level ever recorded (UNHCR). Moreover, the arrival of forced migrants generated push-back from local populations as evidenced by the European response to

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

the influx of Syrian, Afghan, and Iraqi refugees.

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-Garcia and Saah, 2010; Alix-Garcia et al., 2012).

We contribute to this research by analyzing, for the first time, the effect forced migrants have on the housing rental market. We examine the reaction of rental prices to the arrival of IDP in Colombian cities. Furthermore, we differentiate this effect according to the income level of the corresponding housing unit: low, middle or high income.

Two reasons motivate this research question. First, there is abundant anecdotal evidence on the severity and policy relevance of the issue as it is succinctly summarized by the quote at the beginning of this paper. Second, and despite the aforementioned relevance, there is no robust causal evidence to answer this research question.[2] Indeed, after reviewing the literature, we find that the only authors that deal with housing prices are Alix-Garcia et al. (2012) but their evidence is purely anecdotal.

The effects of forced migration inflows on housing prices are not obvious: IDP provide cheap unskilled labor, which is partly absorbed by the construction sector (lowering housing prices), but they also increase demand for housing units (which increases housing prices). Additionally, larger competition in the labor market for urban unskilled workers may depress wages for both IDP and non-IDP workers generating income effects in the housing

[2] Certainly, as pointed out by Ruiz and Vargas-Silva (2013, pp.782), "The evidence on the impact of forced migration [...] on the housing market remains strictly qualitative at this point".

market. In sum, IDP may affect both the demand and supply of rental units. Further, heterogenous 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).[3] Therefore, the demand pressure on the rental market should be particularly salient for low-income housing.[4] Additionally, and due to congestion in public good provision as well as to rising levels of crime or crime perceptions, large IDP inflows may be associated with deteriorating living conditions.

We leverage a novel dataset with high quality 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. Given its quality, high frequency, and temporal extension, our data provides 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. Using high-frequency (i.e., quarterly) data mitigates concerns of other time-varying factors that may take place at longer time intervals (e.g., annually) and may potentially confound our analysis. Moreover, the time period under our analysis provides time windows before, during, and after the peak of the

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

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

displacement crisis occurred in the early 2000s due to the intensification of the internal conflict in Colombia.

We identify an economically large heterogeneous impact of these forced migration shocks: as IDP inflows increase, real rental prices increase (decrease) for low (high) income units.[5] Our preferred specification suggests that a 10% increase in IDP inflows in a given city and quarter increases average rental prices by 0.11% and low-income rental prices by 0.26%. However, high-income rental prices decrease by 0.32%. [6].

In order to empirically establish the impact of IDP inflows on rental prices we follow two strategies. We first present OLS estimates in which we document a strong and robust conditional association between relative rental prices and IDP inflows lagged one quarter. These OLS point estimates are conditional on city and time fixed effects so they account for both time-invariant (such as geography) and city-invariant (such as macroeconomic shocks) characteristics that may potentially confound our analysis. They are also conditional on city-specific time trends. Our main result are also robust to the inclusion of covariates accounting for the conflict intensity, the economic activity and the quality of public good provision of the host city.

Even though our OLS estimates seem to be robust and suggest a long-lasting impact of IDP on rental prices, we acknowledge that these documented associations do not necessarily imply causality and could arise from omitted confounders. In particular, IDP may migrate based on unobservable characteristics of the cities or another unobservable. Our second and preferred

[5] Of important note is that we focus on real rental prices (i.e., we deflate rental prices by each city-specific CPI); thus overall inflation trends are accounted for in our analysis.

[6] Section IV.B 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))

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) either the geodesic or road distance between the host city and the municipality where the IDP outflow originates. We again find a robust impact of IDP inflows on rental prices. Moreover, as we discussed above, we find that the impact varies with income levels and appears to be particularly persistent in the case of low-income rental prices.

We provide empirical evidence for two possible mechanisms that might explain the differential impact of inflows on housing prices. We show that construction licenses for housing units for low-income households are inelastic to IDP inflows and that construction licenses for housing units for medium- and high-income households increase with 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 of the cities.

Using census data from 1993 and 2005, we provide additional evidence of IDP inflows pushing prices up because of an excess in demand. We identify and quantify large housing deficits driven by IDP shocks but only in highly urbanized municipalities which is consistent with the fact that the majority of IDP migrate to urban areas.

Second, we find that quarterly homicides react to IDP inflows. A 10% increase in IDP inflows increases the homicide rate by 0.67 homicides per 100000 inhabitant or by 5.6% with respect to the homicides rate 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.

The main contribution of this paper is to study for the first time the effects of IDP inflows on the housing market with a particular emphasis on rental prices. In addition to that, this is also the first paper to analyze the effect of IDP inflows on crime.

Regarding housing prices, the paper closest to ours is Saiz (2007) (see also (Saiz, 2003)), which finds that immigration inflows positively affect average or median housing rents. However, Saiz focuses on economic migrants, and forced migrants are considerably different. While economic migrants tend to take migration decision based on both push (i.e., conditions that induce them to leave their homes) and pull (i.e., characteristics of the destination that attract them) factors, IDP are mainly pushed by violence. Indeed, nearly 90% of IDP households in Colombia out-migrate after direct threat from armed actors. Economic migrants are also more likely to self-select based on skills and human capital while IDP tend to have low levels of formal education and lack the skills necessary for work in urban areas (Ibáñez, 2008). Economic migrants tend to be very heterogeneous in terms of socio-economic characteristics while IDP are mainly poor arriving from rural areas (Ibáñez, 2008).

Regarding crime, 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 our paper is organized as follows: section I provides the context of displacement in Colombia. Section II presents the data and section III the econometric model. Section IV presents our main OLS and IV results (which are very similar) and performs a battery of robustness checks. Second V highlights two potential channels (housing supply and excess demand, and crime). Section VI concludes.

I. Context

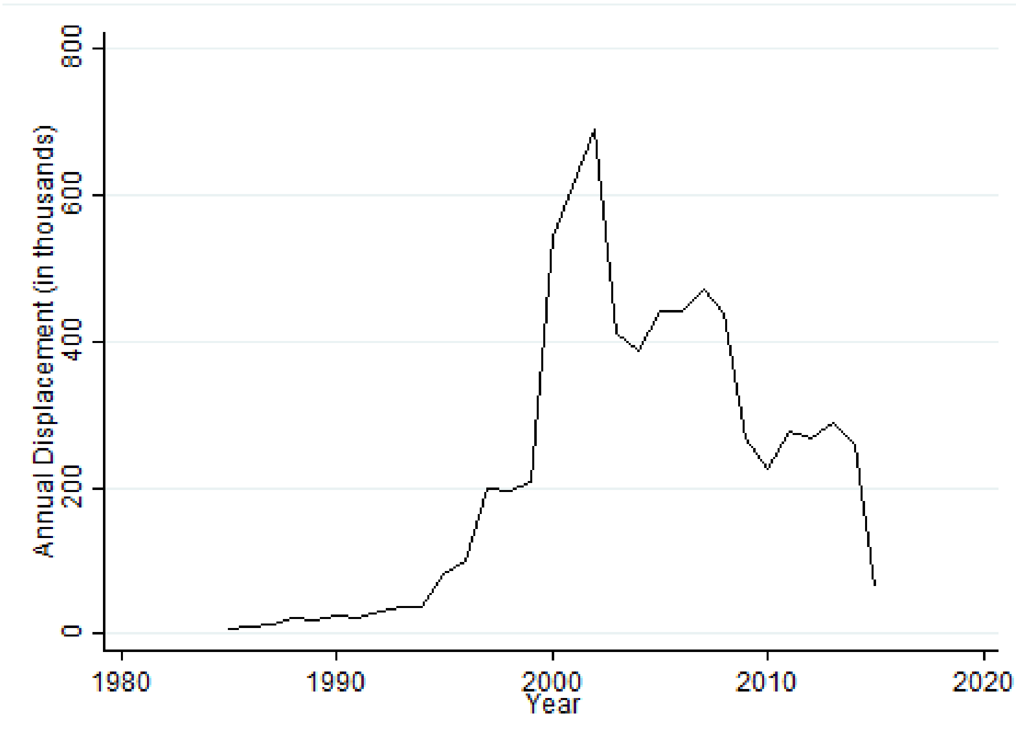
Colombia suffers from a long history of forced displacement as a result of political and drug-related violence. Left-wing guerrilla groups, like the FARC and ELN, emerged in rural areas of the country in the 1960s and persisted for years engaging in relatively low-scale violence against the Colombian government. However, with an increase in illicit crop cultivation and drug trafficking in the 1980s and 1990s, levels of violence began to increase, particularly as these rebel groups entered the drug trade, using the revenues to purchase arms and scale up their operations against the government. Particularly 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 much of their 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. In some cases, individuals were specifically targeted for their political activities, for their refusal to collaborate with a particular armed group, or or because they were seen as community leaders and were singled out to intimidate the community. In other cases, entire communities were displaced because of their perceived involvement or unwillingness to collaborate with a particular armed group, because the community was inconveniently located along an important trafficking route, or because of land disputes or the community's stance against local private industry or development projects that happen to be paying off an armed group for enforcement, among a variety of other reasons. Since the beginning of the conflict, the United Nations High Commissioner for Refugees (UNHCR) estimates over that over 6,640,000 individuals have been forcibly displaced from their homes, approximately 11 percent of the Colombian population (UNHCR, 2015).

Though displacement is violent and traumatic, evidence shows that once displaced, households typically do not travel far; more than half of displaced households reestablish themselves within the same department, an administrative division similar to a state in United States (Ibañez, 2008). Additionally, while IDP typically originate from small, peripheral, rural areas, Dueñas et al. (2014) find that they tend to resettle in larger cities, particularly departmental capitals.

Figure 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 especially 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.

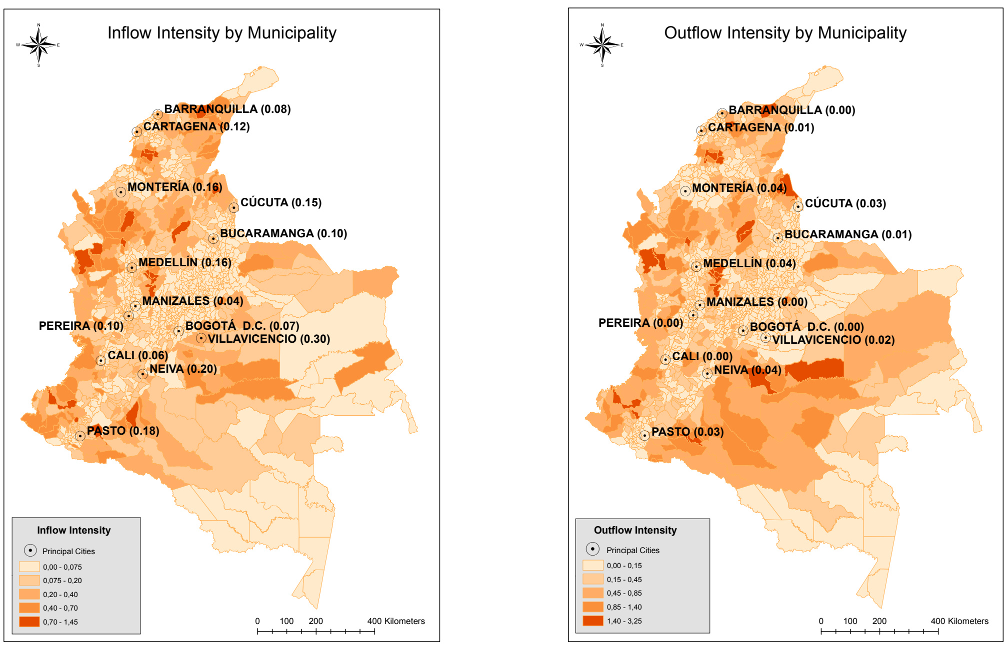
Figure 1. : Internal Displacement in Colombia



Just as the intensity of displacement has been uneven across time, it has varied across the different regions of Colombia. The two panels in figure 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 migrants (either inflows or outflows) over the 1999-2015 period to municipality total population in 1999. As the graphs show, the 13 largest cities are net receivers of IDP, and the magnitude of the IDP inflows shock 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, such as Antioquia, Cauca, Caquetá , Nariõ, Valle del Cauca, Norte de Santander, Arauca, Putumayo, and Meta.

Figure 2. : Inflows and Outflows Intensity

(a) Inflows (b) Outflows

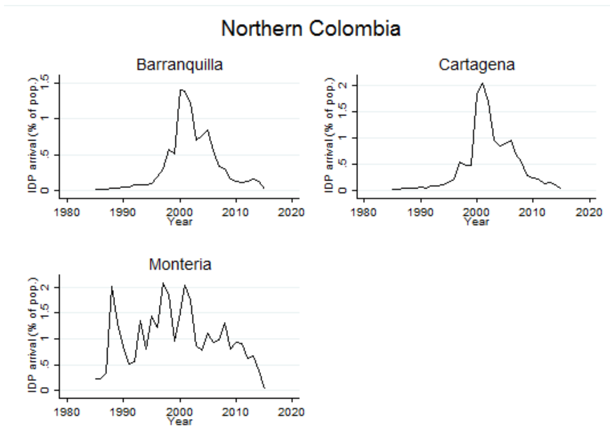


In 1999, the Colombian government created a victims' registry, allowing displaced individuals (including those displaced before 1999) to come before a government office, where their displacement status is validated and they can become eligible for receiving government assistance.[7] The information from the victims' registry allows us to graph the evolution of IDP inflows over time and across cities. We do this in Figure 3, where cities are grouped according to geographic region (i.e; Northern, Eastern, and Central Colombia). Coinciding with the intensification of the armed conflict in the early 2000s, the 13 cities experienced a spike in IDP inflows during the period 2000-2001. A group of cities, including Bogotá, Cali, and Neiva, faced a second peak at the end of the decade. Some cities, like Montería, followed a different path, with continual spikes and drops in IDP inflows over the period. As already stated, there is also substantial variation across cities in the intensity of IDP inflows. Here, the case of Villavicencio again stands out, which in peak years received an inflow of IDP equivalent to 3% of its original population.

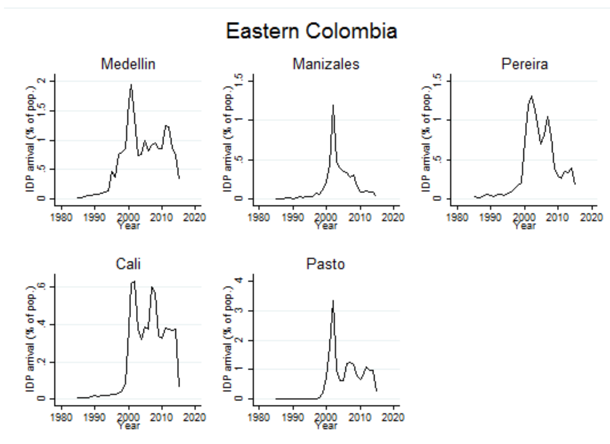
[7]There are three assistance categories for the forcibly displaced population: immediate assistance, including temporary housing and food aid, may be provided by the host municipality starting the moment the victim makes their claim until the RUV has made a decision on their case (up to two months); 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 (Ley 1448, 2011; Prada and Poveda, 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).

Figure 3. : IDP Inflows 1985-2015, by region

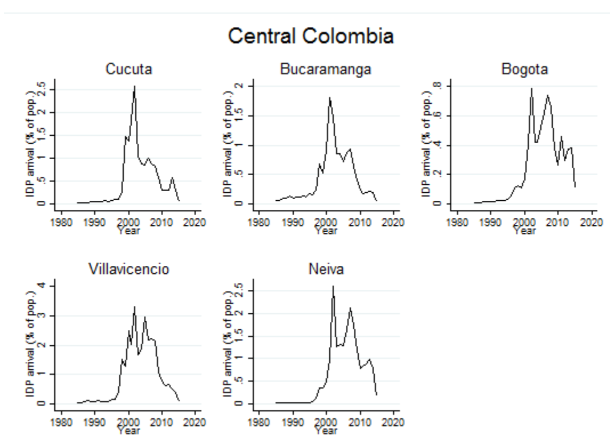
(a) Northern Colombia



(b) Eastern Colombia



(c) Central Colombia



II. Data

To conduct this research we use different data sources. All variables used in the paper are summarized in Table 1. Our main dependent variables are average real rental prices and real rental prices by income level with cross-sectional variation at the city level ($N = 13$) and quarterly time frequency for the period 1999-2014 ($t = 64$). The dependent variables are deflated using a city level CPI and used in logs in all specifications. All of these variables come from the Colombian National Statistics Department (DANE). We spent considerable effort trying to expand our set of cities, but complete data on other geographic areas is non-existent. We are thus restricted to work with a small N; yet besides being an improvement over the previous literature, this is where a disproportionate number of displaced people settle.

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 income area and 6 as the highest. 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. Descriptive statistics of our main dependent variables are presented in the first four columns of Table 1 where the expected positive gradient of rental prices against income is observed.

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 1170 per quarter (from $exp(13.56) = 1170.28$).

The main instrumental variable is a distance-weighed sum of IDP outflows in all municipalities but city c, where the weights are the inverse of the geodesic 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. We also show robustness to an analogous instrument which uses as weights the inverse of road distance. The latter 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 a complete geo-coded network. Some distant municipalities are not connected to the Colombian road network. To complete the road distance variable for these municipalities we first calculate the linear distance to the closest road and then add the actual road distance between this point and the host city.

All our regressions control for population (from DANE); some regressions control 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). All controls are included in logs.

Finally, to investigate potential channels we use three additional sets of dependent variables (all from DANE): i. The number of new constructions licenses to capture supply responses, ii. Measures of housing deficit (overall, quantitative and qualitative) and iii. The homicide rate from Colombian Vital Statistics. Variables in i. and ii. are described in greater detail in subsection V.A.

Table 1—: Descriptive Statistics

	mean	sd
Main Dependent Variables		
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
Other Dependent Variables (channels)		
New Construction Licenses (Social Interest)	1.957	1.482
New Construction Licenses (Excludes Social Interest)	4.572	0.966
Overall Housing Deficit	0.647	0.224
Quantitative Housing Deficit	0.101	0.072
Homicide Rate	12.047	8.48
Independent Variables		
IDP Inflows	7.065	1.086
Population	13.559	0.898
IDP Outflows	4.961	1.115
Receptivity Instrument	-1.223	0.482
Average Distance to All Other Municipalities	12.936	0.240
Tax Revenues	10.683	1.581
Public School Teachers per Student	-3.309	0.104

All variables expressed in natural logarithms. New construction licenses expressed as ln(1+licenses).

III. Econometric Model

In order 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, allowing us to study the potential impact of the unusually large movements of IDP taking place in early 2000s due to the intensification of the Colombian conflict. We thus estimate the following equation:

$$ln\left(P_{c,t}\right) = \alpha + \beta ln\left(Inflows_{c,t-1}\right) + \eta^{'} X_{c,t} + d_c + d_t + u_{c,t} \tag{1}$$

where the subscripts c and t denote city and quarter, respectively. The variable P is the relative price of rentals. Again, in our analysis we use average rental price, as well as rental prices by income level (i.e., low-, middle-, high-income). $Inflows$ is our main treatment 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 for β as a standard elasticity. $X_{m,t}$ is a vector of controls including the total population (in logs), city-level linear trends to control for city-specific trends which might be anticipated by IDP, and city-level proxies for conflict intensity, economic activity, and quality of amenities. 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. u is an error term clustered at the city $\times$ year level. 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 calendar year with differences across cities.[8] Finally, given the cross-sectional variation in city size, we weight all the regressions by city-year population.[9]

It is worth noting that we focus on IDP inflows lagged one period for two main reasons. First, the potential demand shock from a varying number of people arriving to a given location arguably takes some time to translate into price fluctuations. However is unreasonable to assume that it takes more than one quarter given evidence that 93% of IDP migrate directly to their destination (Ibáñez, 2008). Second, using a lagged independent variable may reduce concerns of reverse causality between IDP inflows and prices, which obviously provide valuable information about cost of living in a given city and thus may affect migration decisions. Of course, this approach of lagging the IDP inflow variable does not convincingly solve potential endogeneity problems since economic agents may anticipate the impact of future migration inflows and adjust prices or quantities demanded accordingly. Further, IDP inflows in $t-1$ may also be capturing the effect of expectations regarding future economic growth of the city. 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 setting wherein fixed effects at both the city and quarter

[8]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.

[9]Not weighting the regressions by city population leads to qualitatively similar results. Although the standard errors do not tend to be smaller when weighting our regressions (relative to the unweighted OLS case), the point estimates tend to be slightly larger in some cases. This would suggest that the impact of IDP inflows may be heterogenous across cities of different size (see Table A.3 in the appendix.)

level are included.[10] We acknowledge that other sources of bias may still persist and it is precisely for this reason that we also follow an instrumental variables approach.

As suggested above, estimating equation 1 by OLS may still lead to biased estimates of the impact of IDP inflows on rental prices, in part because IDP do not choose their destinations randomly. Indeed, location decisions might be explained by other, unobserved determinants of rental prices in destination cities. For instance, migration decisions may depend on other prices (such as wages), cost of living, amenities or quality of public good provision of a given city. Additionally, another potential source of bias is reverse causality: IDP might choose to migrate to cities with high rental prices because those cities tend to be richer and provide more employment opportunities. This, for instance, would bias the OLS regression upward, because it induces a positive correlation between IDP and rental prices. [11] In order to address these potential concerns, we follow an instrumental variables approach. Our instrument, which we refer to here as $receptivity_{c,t}$, 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 distance-weighed average of the outflows in all municipalities except city c during the quarter t. Formally:

$$receptivity_{c,t} = \sum_{m \in M \setminus \{c\}} outflows_{m,t} \times D_{m,c}^{-1} \tag{2}$$

[10]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.

[11]Of course, it could be also the case that reverse causality induces a negative correlation between IDP and rental prices: IDP Immigrants may be looking for cheaper places to live or areas where rents are increasing more slowly. This could bias the OLS estimates down.

where $c \in C \subseteq M$ is a city in our sample of the 13 largest cities (which are also municipalities), which is a subset of the 1100 Colombian municipalities. $D^{-1}_{m,c}$ is the geodesic distance between municipality m (origin of IDP) and city c (destination of IDP). The instrument thus suggest that the number of IDP arriving to city c in time t increases in the number of outflows in other localities, but decreases in the distance from any locality to the city. Thus (log of) $receptivity_{c,t-1}$ is used as an instrument for $ln\,(Inflows_{c,t-1})$.

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. Third, the closer the proximity of a host city to a municipality experiencing IDP outflows in given point in time, the higher the probability of receiving a large IDP inflow for that host city.[12] We also conduct a robustness check on our IV results by rebuilding the instrument, but excluding IDP outflows from municipalities within 50 kilometers of the host city. These results are presented in subsection IV.C.

IV. Main Results

A. OLS Results

Impact on Average Rental Prices

Table 2 provides the first statistical test for the potential 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 relative

[12] 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.

rental price (i,e; relative to the CPI of the city). The specification in column 1 only includes d_c and d_t as controls. The former captures time-invariant characteristics of the city such as geographic conditions, whereas the latter captures city-invariant specific condition to the quarter such as international commodity prices or nation wide effect of macroeconomic policies. Particularly, it has been shown that international commodity price shocks impact conflict intensity in Colombia Dube and Vargas (2013); thus such shocks may also directly impact relative rental prices and IDP inflows. Consistent with a demand-side shock story, results in column 1 suggest that relative rental prices significantly and positively correlate 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 0.028 percent in relative 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 relative 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 is more than halved, albeit it remains positive and strongly statistically significant. Adding total population (in logs) of the city as a control in column 3 does not alter previous results. The magnitude of the statistical indicates that a one percent increase in IDP inflows is related to an increase of 0.009 percent in relative rental prices.

Is it possible that inflows are capturing the conflict intensity in the city? Are forced migrants arriving to more peaceful cities? The conflict intensity in the host city itself might deter IDP from migrating and at the same time

Table 2—: IDP and Rental Prices, OLS

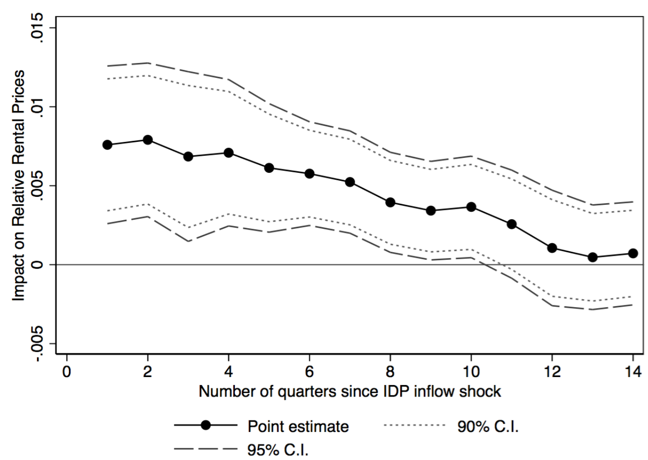
	(1)	(2)	(3)	(4)	(5)
IDP Inflows t-1	0.0284***	0.00854***	0.00864***	0.00783***	0.00759***
	(0.00496)	(0.00207)	(0.00210)	(0.00266)	(0.00255)
Population			0.163	0.129	0.0860
			(0.336)	(0.333)	(0.327)
IDP Outflows t-1				0.00107	0.00110
				(0.00148)	(0.00147)
Tax Revenues					0.00889*
					(0.00488)
Public School Teachers per Student					0.00566
					(0.0192)
Observations	832	832	832	832	832
City FE	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y
City-specific Linear Trend	N	Y	Y	Y	Y

Standard errors clustered at the city.year level in parenthesis. All variables are expressed in natural logarithms. All regressions are weighted by city population. *** p<0.01, ** p<0.05, * p<0.1

reduce housing and rental prices. To test for this we control for outflows at time t-1 in column 4 of Table 2. This variable is not significant and our parameter of interest changes remarkably little. Economic activity in the city might encourage in-migration and increase rental prices. Similarly, amenities available in the host city might increase in-migration and thus affect housing prices. The omission of these factors may introduce a bias in our estimates. To deal with that problem we directly control for related variables in column 5 of Table 2. These variables are tax revenues (proxying for unavailable city-level GDP) and public school teachers per student (one available measure of amenities). Although we acknowledge a potential endogeneity of these controls, their addition does not seem to affect previous results. Comparing the last two columns with the third one, we conclude that omission of these variables may lead to an small upward bias in our parameter of interest: our point estimate of interest decreases by about 10% but remains significant at less than the 1% level.

Are the effects of IDP inflows on prices short-lived? To answer that question we estimate equations like equation 1 replacing $ln\,(Inflows_{c,t-1})$ by $ln\,(Inflows_{c,t-\tau})$ for $\tau = 1, 2, ..., 20$ (i.e., we run 20 separate regressions). Figure 4 reports the coefficient on lagged inflows and corresponding confidence intervals. The first point in the solid line is just the parameter estimate of Column 3 of table 2 (the parameter estimate on $ln\,(Inflows_{c,t-1})$). The second point corresponds to the parameter estimate on $ln\,(Inflows_{c,t-2})$, and so on. The figure suggests that the impact of IDP flows on rental prices may be, indeed, long-lasting (up to 10 quarters).

Figure 4. : IDP Inflows and Average Rental Prices Over Time (OLS)



Impact on Rental Prices by Income Level

We now analyze whether the impact of IDP inflows on rental prices varies by income level, focusing on relative rental prices for low-, middle-, and high-income rentals. Unless otherwise stated, all specifications that follows have the same structure of column 5 in Table 2, our preferred specification. For comparison, column 1 of Table 3 replicates the results of that preferred specification when the dependent variable is average relative rental prices. Results in column 2 show that relative rental prices for low-income consumers significantly and positively correlate with IDP inflows. The magnitude is very similar to the one found in Table 2. We also find in column 3 that higher IDP inflow intensity is statistically associated with higher relative rental prices for middle-income consumers. We do not find, however, any statistically significant association between IDP inflows and relative rental prices for high-income consumers (column 4).

Table 3—: IDP and Rental Prices by Income Level, OLS

	Dependent Variable: Ln of Relative Rental Price			
	Average	Low Income	Middle Income	High Income
	(1)	(2)	(3)	(4)
IDP Inflows t-1	0.00759***	0.00810**	0.00787**	0.00160
	(0.00255)	(0.00320)	(0.00328)	(0.00445)
Observations	832	832	832	832

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 the full set of controls in column 5 of Table 2. All regressions are weighted by city population. *** p<0.01, ** p<0.05, * p<0.1

The magnitude of the estimated impact seems large. To put it in context, Villavicencio received almost twelve thousand IDP in 2002 which represented a 70 percent increase from 2001.[13] Taking the point estimate from column2 in Table 3 at face value would suggest that rental prices for low income consumers went up 0.567 percent above the overall CPI in Villavicencio

[13]Currently, more than 10 percent of Villavicencio's population are IDP.

during 2002 (CPI inflation in Villavicencio was 6.6% in 2002) due to that particular IDP inflow shock.

Table 4 provides a simple falsification test. We estimate specifications in which future levels of IDP inflows (one year forward or in $t+3$) replace our main explanatory variable (i.e; IDP inflows in $t-1$). We find that future levels of IDP inflows are not statistically related to any of the four relative rental prices. Reassuringly, the coefficient estimates are indeed near zero.

Table 4—: Falsification Test: Forward IDP Inflows and Rental Prices by Income Level, OLS

	Dependent Variable: Ln of Relative Rental Price			
	Average	Low Income	Middle Income	High Income
	(1)	(2)	(3)	(4)
One Year Forward IDP Inflows	0.00191	0.00260	0.00106	-0.000837
	(0.00219)	(0.00250)	(0.00273)	(0.00404)
Observations	831	831	831	831

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 the full set of controls in column 5 of Table 2. All regressions are weighted by city population. *** p<0.01, ** p<0.05, * p<0.1

B. *Instrumental Variable Results*

While the previous OLS results are consistent with a demand-side shock impact of IDP inflows on relative rental prices, the estimated coefficients might still be biased, mainly because IDP do not choose their destinations randomly. In this section we present instrumental variable estimates that address and correct for this bias.

In Table 5 we explore the strength of the proposed IDP receptivity instrument, which was described in section III (see equation 2).[14] Column

[14]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

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. The implied first-stage F-statistic when we add city-level linear trends in column 3 is 78.56 suggesting that, conditional on the aforementioned trends and both city and quarter fixed effects, receptivity is indeed a strong instrument. Adding population in column 4 does not wash out the strong predictive power of our proposed instrument. Finally, the specification in column 5 adds the full set of controls of our preferred specification (i.e., column 5 in Table 2). The results are very similar.

In Table 6 we present IV estimates using the specification in column 5 of Table 3 to causally establish the impact of IDP inflows on rental prices in the host cities for varying levels of income. From Column 1 we observe that the effect of IDP inflows on average relative rental price is statistically significant and almost 40 percent larger than in the OLS estimates. For the case of relative rental price of low-income tenants the elasticity is even larger, suggesting that a 1 percent increase in IDP inflows translates into a 0.03 percent increase in relative prices for the low-income segment of the rental market. How large are these impacts? Consider for example the estimated impact for low-income rentals (i.e., $\hat{\beta} = 0.026$) and the standard deviations of both inflows (std. dev. = 1.09) and low-income rental prices (std. dev. =

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.

Table 5—: First-Stage: IDP Inflows and Receptivity

	(1)	(2)	(3)	(4)	(5)
Receptivity Instrument	0.902***	1.630***	1.716***	1.730***	1.492***
	(0.204)	(0.176)	(0.207)	(0.208)	(0.188)
Population				-8.256	-12.69*
				(8.097)	(7.260)
IDP Outflows t-1					0.160***
					(0.0284)
Tax Revenues					0.119
					(0.122)
Public School Teachers per Student					-0.344
					(0.302)
Observations	832	832	832	832	832
F-statistic	19.56	43.59	78.65	85.22	74.59
City FE	N	Y	Y	Y	Y
Time FE	N	Y	Y	Y	Y
City-specific Linear Trend	N	N	Y	Y	Y

Standard errors clustered at the city.year level in parenthesis. All variables are expressed in natural logarithms. All regressions are weighted by city population. *** p<0.01, ** p<0.05, * p<0.1

0.08); this leads to a large standardized beta of 0.35. In words, one standard deviation increase in IDP inflows result in an increase of low-income rentals equivalent to more than one-third of its standard deviation.

An alternative way of conveying the magnitude of the estimated impact is to compare our results with the main result in Saiz (2007). According to the author "Immigration inflows equal to 1% of a citys population were associated with increases in average or median housing rents and prices of about 1%" (PP.346). To compare our results we need to use rental prices in levels, as Saiz does. Appendix Table A.4 presents IV regressions but with rental prices in levels (not relative to the city's CPI). Unfortunately, in this specification we find no effect for average rental prices. We do however find that rental prices increase for low income tenants while rental prices decrease for high income tenants. Thus, the most comparable parameter is that for low income rental prices (column 2 of Table A.4). This parameter estimate is equal to 0.0121, significant at the 10% level. In our sample, 1% of average city population is 13005 or 9.47 log points. An increase in our independent variable of 9.47 log points translates into an increase of $0.0121x9.47 = 0.114$ log points in the dependent variable (which is the log of low income absolute 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% ($100xexp(0.114)/exp(4.53) = 1.22$). Hence an increase of forced immigration inflows equal to 1% of a city's population is associated with increases in low income average housing rents of 1.22%. This is very similar to Saiz findings and it might be 22% large because it focuses on the low income segment of the housing market. Similar calculations for the high income segment of the market indicate that an increase of forced immigration inflows equal to 1% of a city's population entails a decrease in high income average housing rates of 1.66%.

Coming back to Table 6, we do not find any effect for IDP inflows on relative rental prices for the middle-income segment (column 3). We do find, however, that IDP inflows negatively impact relative rental prices for high income tenants: a 10 percent increase in IDP inflows leads to a 0.3-percent decrease in rental prices.[15]

One possible explanation for this result is that cheap labor provided by IDP fuels expansions in the construction sector. We test for the possibility of this supply-side channel in the subsection V.A.

[15]We find qualitatively similar results when we focus on absolute rental prices (i.e., without deflating rental price indeces by the CPI). Therefore, our main results do not seem to be explained by a general changes in overall prices.

Alternatively, large IDP inflows could be perceived as a negative amenity by wealthy residents, thus pushing high income rental prices down. Anecdotal evidence suggests that large inflows of IDP have been associated with perceptions of crime and other social problems such as the expansion of slums. This is not surprising given the lack of opportunities of displaced people, their lack of urban-market skills and their composition (more young males compared to the non-displaced). We test for the possibility of a crime channel in subsection V.B.

Finally, when IDP inflows are as large as in the case of Colombia's principal cities one cannot discard congestion externalities. For example, as transportation systems become congested and sidewalks crowded with street vendors, higher strata residents suffer from these externalities without receiving the housing demand shock implied by the arrival of low-income forced migrants. Although of significant interest, we do not have data to explore impacts on congestion externalities.

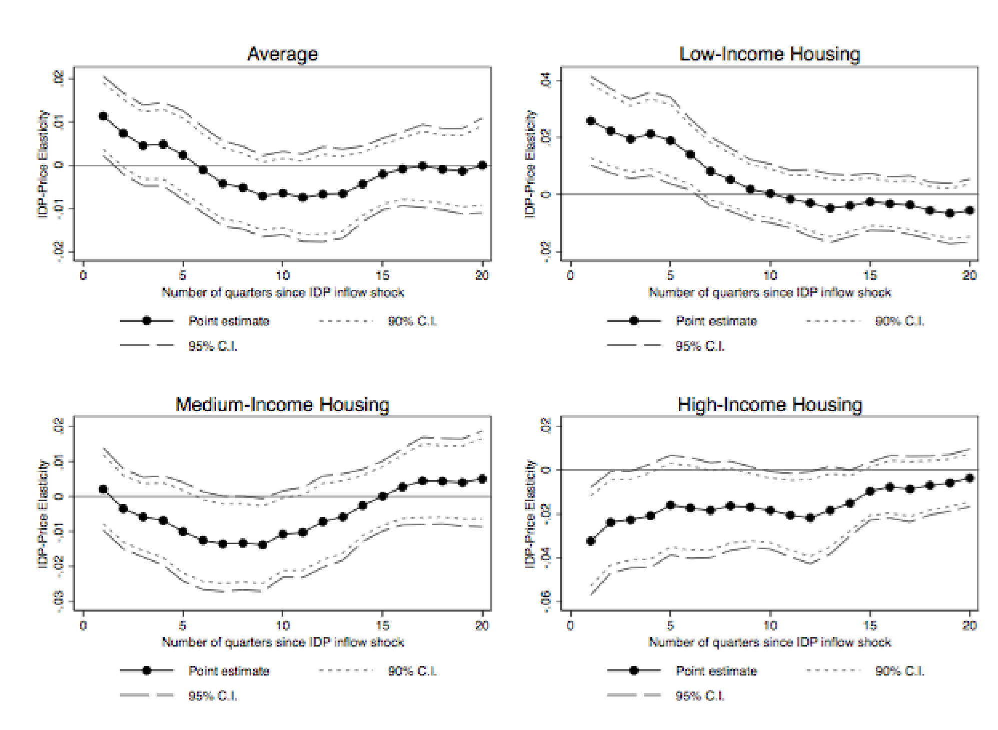
Table 6—: IDP and Rental Prices by Income Level, IV

	Dependent Variable: Ln of Relative Rental Price			
	Average	Low Income	Middle Income	High Income
	(1)	(2)	(3)	(4)
IDP Inflows t-1	0.0114**	0.0259***	0.00206	-0.0324**
	(0.00466)	(0.00791)	(0.00598)	(0.0125)
Obs	832	832	832	832
First-stage F	27.92	27.92	27.92	27.92

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 the full set of controls in column 5 of Table 2. All regressions are weighted by city population. *** p<0.01, ** p<0.05, * p<0.1

Before performing robustness check and analysing the potential channels underlying our reduce form results we look at how persistent these documented effects on rental prices may be. To do so, we repeat the exercise carried for figure 4 although this time we present IV point estimates for the impact of different lags of IDP inflows on average, low-, medium-, and high-income rental prices. Three key findings emerge from Figure 5: (1) the impact of IDP inflows on average and medium-income rental prices appears to be small and short-lived, (2) the negative effect of IDP inflows on high-income rental prices dissipates after 3 quarters, (3) while the impact on low-income rentals appears to be long-lasting (it persists up to 7 quarters).

Figure 5. : Impact on Rental Prices Over Time, By Income Level (2SLS)



C. Robustness Checks

In this section we perform a series of robustness checks for our instrumental variables results on rental prices. We conduct checks by controlling for time dummies that may vary according to a city's remoteness, by excluding IDP outflows from municipalities within 50 km of the host city from our receptivity instrument, and by replacing linear distances between cities and municipalities in our instrument with road distances. In general, our results hold up well. In fact, if anything, the inclusion of this set of control improves the precision of our estimates.

In Table A.1 we add interaction terms between the quarter dummies (i.e. time fixed effect) and a measure of remoteness based on the (log) average distance from the host city to all other Colombian municipalities to our main specification. This robustness check should mitigate concerns that our proposed instrument was capturing -for example- varying transportation costs which depend on gas prices (absorbed by the first term of the interaction - i,e. quarter dummies) and average distance required to reach city c (partially accounted for by our remoteness measure, the second term in the interaction). We believe this is indeed a stringent test, which absorbs a substantial amount of the cross-sectional variation in our instrument. Nonetheless, the results presented in Table A.1 suggest that our proposed instrument was not picking up the potential effect of a time-varying nation wide shock amplified by the remoteness of the host city.

In the next robustness check, we make a slight modification to our original instrument. Here we exclude IDP outflows from municipalities within 50 km of the host city in the computation of the receptivity instrument (equation 2). In doing so, we hope to remove concerns that regional violence and IDP arrivals from nearby municipalities are related to host city housing prices through a confounding relationship with some other variable reflecting local or regional trends. For example, increased cultivation of illicit crops or drug trafficking activity is related to forced displacement, while generating large revenues that could potentially increase demand for housing in the nearest large city, increasing prices. The trade-off here is that displaced people usually move to proximate cities. Therefore, excluding potential IDP inflows from close municipalities may reduce the strength of the instrument.

Table A.3 explores how our results change if we do not weight our regressions by city population. Qualitatively the results are very similar but quantitatively some differences emerge: the parameter estimates are smaller and less significant (now they are significant at the 10% level).

Table A.4 redefines the dependent variable: instead of relative rental prices we use absolute rental prices. The effect on average rental prices dissipates. However this non-result masks important heterogeneous effects: a 1% increase in lagged inflows increases low income rental prices by 0.12% and reduces high income rental prices by 0.05%.

Table A.2 presents the results for this exercise. We note that the estimate for the effect on high-income rentals remains significant at the $p<0.05$ level, and is larger in absolute terms than the estimate in Table 6. The estimate for the effect on low-income rentals is almost identical to the estimate in Table 6 and highly significant.

As a final robustness check, we replace the geodesic distance weights used in equation 2 to build our receptivity instrument with road distances. We believe this helps to account for the difficult geography in some regions of Colombia - though a displaced family may have a large potential host city within a short linear distance of their municipality of origin, the road that actually takes them there could be significantly longer, reducing the probability that they end up settling there. To give one example, the linear distance between Cali and Neiva is 143 kilometers but the road distance is more than twice that (345 kms). These results are shown in Table A.5, and are qualitatively similar to the results for our original instrument. The point estimate for low-income rental prices remains almost identical that the corresponding estimate in Table 6; it also remains statistically significant at the 1% level. The point estimate for high-income rental prices is also very similar to that using geodesic distances.[16]

V. Potential Channels

In this section, we explore two possible channels through which IDP arrivals might work to impact relative housing prices in Colombian cities. First, we explore the supply-side response to IDP arrivals by regressing new housing construction licenses on IDP inflows; we also show that IDP inflows are associated with excess demand as measured by housing deficits. Second, we examine the relationship between IDP arrivals and crime.

A. IDP, Construction Sector's Response, and Housing Deficit

We next study the empirical relationship between IDP and the response of the construction sector in the 13 largest Colombian cities. Our analysis uses new housing construction licenses, divided into social interest housing (VIS Licenses, for its Spanish acronym; this includes subsidized and free

[16]Table A.6 in appendix shows the first-stage results for the alternative instrument (it replicates Table 5). Table A.7 in appendix also shows how estimates in Table A.5 change when trends by remoteness are included in the regressions.

housing provided to needy families), and normal housing (Non-VIS). The data again comes from DANE and is also quarterly. The VIS category was established by the Colombian government to assist low-income segments of the population to acquire homes.[17] To estimate the effect of IDP on the supply-side response of housing construction, we re-run equation 1 by OLS and 2SLS, replacing housing prices with the number of new construction licenses issued in the quarter.[18]

Table 7—: Potential Channels: IDP Inflows and Construction Licenses

	(1) OLS Non-VIS	(2) OLS VIS	(3) IV Non-VIs	(4) IV VIS
IDP Inflows t-1	0.113**	0.0405	0.220*	0.138
	(0.0512)	(0.124)	(0.122)	(0.290)
Population	-0.564	-2.512	1.668	-0.478
	(7.588)	(18.60)	(8.309)	(18.82)
Obs	830	830	830	830
First-stage F	.	.	27.88	27.88

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 the full set of controls in column 5 of Table 2. 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

As table 7 shows, construction licenses for housing of non-social interest increase when IDP inflows increase (columns (1) and (3)). Indeed, construc-

[17]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.

[18]A small but non-negligible number of city-quarter observations saw zero newly issued construction licenses. To avoid losing these observations when taking the natural logarithm, we use ln(1+Licenses) as the dependent variable.

tion licenses increase by 0.2% after an increase of 1% in IDP inflows. This could be explained by the construction sector absorving cheap non-skilled labor provided by IDP.[19] If we interpret Non-VIS housing as representing the high income segment of the market, this expansion in supply could in part explain decreasing rental prices for high income tenants.

A different picture emerges when we look at construction licenses for social interest housing. In that case, we find no evidence that the construction sector responds to IDP inflows by increasing the supply of social interest housing in the next quarter (columns (2) and (4)).[20] This is not a surprising result since Colombian housing sector is characterized by a constrained capacity to meet a growing demand for housing solutions (World Bank, 2010). Well before the peak of the displacement crisis occurred in early 2000s Colombia already presented a large housing deficit.[21] Moreover, large cities is where the housing deficit is concentrated and faces important constraints of serviced and affordable land for VIS housing.[22] Areas where the expansion of the housing market is feasible are highly priced thus only suitable for high-income segments. Further the lack of credit is an important constraint which limits the expansion of social housing (Arbeláez et al., 2011).

The results in table 7 also suggest that new IDP arrivals are not met with a government response providing free or subsidized housing units.[23] Given

[19]Focused in the period 2001-2005, Calderón-Mejía and Ibáñez (2015) show that large IDP inflows induced a substantial reduction in wages for urban unskilled workers.

[20]To consider the possibility that new construction may take time to respond to IDP inflows, we also ran these regressions using higher lags of IDP. The estimates are not significant at any time within a year of the IDP shock. Interestingly, there is some evidence that new construction may actually decline around two years after the shock.

[21]By the year 1993, total housing deficit was 53.7 percent (Arbeláez et al., 2011).

[22]The scarcity of available land in the main Colombian cities on which to build social interest housing has been discussed in several works (see for instance, Carrillo (2009) and World Bank (2010)).

[23]It is important to note that the participation of the private sector in housing con-

that an influx of internally displaced individuals results in a population increase that should automatically generate increased demand for housing, and particularly for cheaper rental units, the lack of response in the supply of (social interest) housing to the demand shock appears at least partly responsible for the price increase for low income tenants.[24]

We next provide another piece of evidence towards the existence of a large excess of demand for housing in low-income segments due to the large influx of IDP. To do so, we exploit data on housing deficits from the censuses of 1993 and 2005. We compute different measures of housing deficits for over 1000 municipalities. 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 (not major) building deficiencies.[25] The main disadvantage of using census data is its periodicity. Thus, we are only able to quantify the impact of IDP from long changes in inflows (inflow changes between 2005 and 1993). On the other hand, using census data also has the advantage of allowing us to distinguish the impact of IDP on the quantitative deficit (i.e., coverage) from its impact on qualitative deficit (quality) as well as the differential effect on urban and rural areas. On top

struction for low-income segment is marginal (World Bank, 2010).

[24]Case study evidence for Bogotá (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."

[25]Indeed, qualitative deficit is related to substandard structures and inadequate access to basic services.

of that, we now have a larger cross-section to exploit within-municipality variation before and after the displacement crisis occurred in early 2000s. We estimate the following equation:

$$HD_{m,t} = \alpha + \beta \frac{\sum_{s=1}^{T} Inflows_{m,t-s}}{Population_{m,t}} + \eta' X_{m,t} + d_m + d_t + u_{m,t} \qquad (3)$$

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 in three measures of deficit: overall, quantitative, and qualitative. Our treatment is a measure of IDP pressure which is computed as the cumulative sum of IDP inflows during the 8 (11) years previous to the 1993 (2005) census divided by population in t. For the census of 1993 we sum inflows from 1985 to 1992 (1985 is the first year with IDP data) whereas for the census of 2005 we sum from 1993 to 2004. The vector $X_{m,t}$ includes basic controls: population (in logs) and the measure of remoteness (based on the average distance from each municipality to all other municipalities) interacted with the year dummies. d_m and d_t denote municipality and year fixed effects, respectively. 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 8 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 housing deficit. This association remains strongly statistically significant and more than doubles when we restrict our sample to highly urbanized municipalities, in column

2.[26] 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 8 are likely to be downward biased for two main reasons: time-varying omitted variables and measurement error.[27]

Table 8—: 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	0.101**	0.252**	0.0990**	0.291***
	(0.0455)	(0.107)	(0.0489)	(0.102)
Observations	2048	368	2048	368
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 (1994) to 1992 (2004) normalized by 1993 (2005) population. Additional controls are population (in logs) and the interaction between year dummies and the average distance from the municipality to all other Colombian municipalities. 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

In Table 9 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 8. That is, the causal impact of IDP inflows on housing deficits is substantially

[26] 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).

[27] 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

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 7 percent in the stock of houses in order to provide a home to every household in the municipality (see column 3 in Table 9).

Table 9—: 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	0.901***	1.846***	0.702***	1.500***
	(0.294)	(0.678)	(0.268)	(0.532)
Observations	2048	368	2048	368
Municipalities in Sample	All	Urbanized	All	Urbanized
Municipality FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
First-stage F	33.17	10.63	33.17	10.63

Standard errors clustered at the municipality level in parenthesis. Housing deficit is computed as a share of total households in 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 (1993) to 1992 (2004) normalized by 1993 (2005) population. Additional controls are population (in logs) and the interaction between year dummies and the average distance from the municipality to all other Colombian municipalities. The instrumental variable is the cumulative sum (over the same period as the endogenous IDP inflows) of the receptivity measure (in logs). 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

We follow two additional exercises in Table A.8. First, in columns (1) and (2) we show that, regardless whether we focus in all municipalities or just the highly urbanized, large IDP inflows are not systematically associated with larger qualitatively deficits. Therefore, this highlights that IDP inflows mainly exerts a pressure in the quantity, rather than the quality, of housing. Second, in column (3) and (4) we show that even within highly urbanized municipalities the pressure is exclusively concentrated in their urban areas (i.e., municipalities' seat). Indeed, among the highly urbanized municipalities we find no statistically significant association between IDP inflows and housing deficits outside of the municipality seats. Again, this is consistent with the fact that IDP tend to migrate to urban areas like municipality seats or departmental capitals (Dueñas and Zuluaga, 2014).

B. IDP and Crime

Research on the effect of immigration on crime in the recipient population is scant and research on the effect of forced migration on crime is even scarcer. For immigration, existent research suggests a small positive effect on property crimes (see Bianchi et al. (2012)). For forced migration, the available evidence is from the effects of Katrina on cities not directly affected by the hurricane but receiving those displaced by it: Houston, San Antonio and Phoenix (Varano et al. (2010)). The authors find that homicides increase in Houston and Phoenix but not in San Antonio. However, it is hard to give a causal interpretation to these results since the authors only have time series data and no control cities.

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 we have 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. Case study evidence for the city of Pasto, the state capital of Nariño, suggests that security conditions deteriorate in places with settlements of displaced population (Bohada, 2010). In San Cristobal -a locality in eastern Bogotá composed mainly of poor (estrato 1 and 2) households- the NGO CHODES reports that "behind the violence in the locality, there is a serious problem of coexistence with the displaced people who arrived in recent years." (Valenzuela, 2014).

In this subsection we analyze the impact forced migration has on homicides in host cities. Following Besley and Hannes (2012) we can expect that increases in violent crime will reduce housing prices. This constitutes a possible channel through which rental prices fall with the arrival of IDP. In particular, this could explain the fall in rental prices for the richer segment of the population: In Bogotá perception surveys reveal that one of the main concerns of strata 5 and 6 residents regarding IDP is increasing insecurity. (Departamento Administrativo de Planeación Distrital (2004)). Beyond acting as a potential channel explaining our results, and given the state of the literature, analyzing the effects of forced migration on violent crime is also a very important endeavor on its own right.

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 in criminal activity, because usual residents engage in criminal activity, or both. With this caveat in mind, Table 10 explores the relationship between IDP arrivals and homicides. The dependent variable is the homicide rate per 100000 inhabitants. Column 1 presents the OLS results when the right hand variables are those in column 3 of Table 2 (ie: We do not include the potentially endogenous controls:

outflows, tax revenues and public school teachers per student). We find a positive an highly statistically significant association between lagged inflows and homicides: a 10% increase in IDP inflows increases the homicide rate by 0.35. Column 3 present the corresponding instrumental variables result. The IV parameter estimate is 1.7 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 trends by remoteness 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 [28]. With these additional controls the OLS parameter estimate (column 2) is 4.3 and the IV parameter estimate (column 4) is 55% larger and equal to 6.7; 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 0.67 homicides per 100000 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 5.6% with respect to the mean.

Table 11 presents a simple falsification test where we replace the homicide rate by the number of natural deaths per 100000 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

[28] 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 2.1 (s.e.=1.1). If we control for outflows in a specification otherwise identical to that of column 4 we obtain a parameter estimate of 3.9 (s.e.=1.9).

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. In order to lessen this concern, appendix table A.9 redefines the instrument as the weighed sum of outflows originating in municipalities at least 50KM 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.9 shows that the parameter estimates of inflows are practically unchanged when the instrument excludes municipalities within 50 km of the host cities.

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 in 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 very few new buyers entering the high-income market, we find plausible that the association of IDP arrivals with noticeably higher levels of crime could help to explain the the decrease in high-income rental prices.

Table 10—: Potential Channels: IDP Inflows and Homicides

	(1) OLS	(2) OLS	(3) IV	(4) IV
IDP Inflows t-1	3.699***	4.332***	5.902***	6.309***
	(0.967)	(1.032)	(1.636)	(1.569)
Population	-234.2**	-21.04	-208.1**	37.64
	(90.63)	(87.67)	(92.60)	(99.01)
Tax Revenues		3.553		3.383
		(2.193)		(2.205)
Public School Teachers per Student		-3.608		-2.527
		(6.015)		(5.909)
Observations	820	820	820	820
First-stage F	.	.	31.45	30.07
Trends by Remoteness	N	Y	N	Y

Standard errors clustered at the city.year level in parenthesis. Dependent variables are rates per 100000 inhabitants. All right hand variables are expressed in natural logarithms. All regressions include time and city fixed effects and city-specific linear trends. All regressions are weighted by city population. *** p<0.01, ** p<0.05, * p<0.1

Table 11—: Falsification test: IDP Inflows and Natural Deaths.

	(1) OLS	(2) OLS	(3) IV	(4) IV
IDP Inflows t-1	1.408	0.156	3.857	4.269
	(1.253)	(1.120)	(4.236)	(4.046)
Population	326.8	-22.95	355.9	97.20
	(214.6)	(331.8)	(234.1)	(369.8)
Tax Revenues		-5.970*		-6.314*
		(3.459)		(3.548)
Public School Teachers per Student		-22.44		-20.14
		(16.06)		(15.68)
Observations	828	828	828	828
First-stage F	.	.	31.15	29.92
Trends by Remoteness	N	Y	N	Y

Standard errors clustered at the city.year level in parenthesis. Dependent variables are rates per 100000 inhabitants. All right hand variables are expressed in natural logarithms. All regressions include time and city fixed effects and city-specific linear trends. All regressions are weighted by city population. *** p<0.01, ** p<0.05, * p<0.1

VI. 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. Using high-frequency (i.e., quarterly) data mitigates concerns of other time-varying factors that may take place at longer time intervals (e.g., annualy) and may potentially confound our analysis. Moreover, the time period under analysis provides time windows before, during, and after the peak of the displacement crisis occurred in early 2000's due to the intensification of the Colombian internal conflict across different regions. Following an instrumental variable approach to address the potential endogeneity of the location choices of IDP, we identify a strong causal and economically large impact of IDP inflows on relative rental prices. On average, higher IDP inflows increase rental prices but the impact varies with income levels: rental price increase (decrease) for low (high) income units. Moreover, we provide evidence on two potential mechanisms underlying these heterogeneous results. First, IDP inflows differentially impact the construction sector in Colombian cities. We find that construction licenses for housing units of social interest are inelastic to IDP inflows. On the contrary, construction licenses for housing units of non-social interest increase with the arrival of IDP. 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 the rental market for low-income areas; an excess of demand not followed by an increasing supply of rental units (increasing rental prices). Reassuringly, we exploit census data from 1993 and 2005 to identify and quantify large housing deficits driven by IDP shocks but only in highly urbanized municipalities. Second, we find that higher IDP inflows are associated to increasing crime which may arguably put a downward pressure on rental prices in high-income rentals.

Appendix

Additional Tables

Table A.1—: Robustness Check: IDP and Rental Prices by Income Level, IV when controlling for trends by remoteness

	(1) Average	(2) Low Income	(3) Middle Income	(4) High Income
IDP Inflows t-1	0.0175***	0.0350***	0.00798*	-0.0330**
	(0.00493)	(0.00957)	(0.00474)	(0.0140)
Obs	832	832	832	832
First-stage F	25.31	25.31	25.31	25.31

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, the full set of controls in column 5 of Table 2, and time dummies interacted by the average distance from the host city to all other Colombian municipalities. All regressions are weighted by city population. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A.2—: Robustness Check: Instrument excluding municipalities within 50 km of host city

	(1) Average	(2) Low Income	(3) Middle Income	(4) High Income
IDP Inflows t-1	0.00722	0.0332**	-0.00967	-0.0732**
	(0.00983)	(0.0147)	(0.0138)	(0.0305)
Obs	832	832	832	832
First-stage F	18.67	18.67	18.67	18.67

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 the full set of controls in column 5 of Table 2. All regressions are weighted by city population. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A.3—: Robustness Check: Unweighted Results

	(1) Average	(2) Low Income	(3) Middle Income	(4) High Income
IDP Inflows t-1	0.00878*	0.0139*	0.00421	-0.0205*
	(0.00529)	(0.00723)	(0.00650)	(0.0109)
Obs	832	832	832	832
First-stage F	51.35	51.35	51.35	51.35

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 the full set of controls in column 5 of Table 2. *** p<0.01, ** p<0.05, * p<0.1

Table A.4—: Robustness Check: Impact on Absolute Prices

	(1) Average	(2) Low Income	(3) Middle Income	(4) High Income
IDP Inflows t-1	-0.00238	0.0121*	-0.0117	-0.0461***
	(0.00545)	(0.00689)	(0.00750)	(0.0149)
Obs	832	832	832	832
First-stage F	27.92	27.92	27.92	27.92

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 the full set of controls in column 5 of Table 2. *** p<0.01, ** p<0.05, * p<0.1

Table A.5—: Robustness Check: Instrument with weighting by road distances

	(1) Average	(2) Low Income	(3) Middle Income	(4) High Income
IDP Inflows t-1	0.00823	0.0268***	-0.00443	-0.0399**
	(0.00602)	(0.00972)	(0.00784)	(0.0170)
Obs	832	832	832	832
First-stage F	24.58	24.58	24.58	24.58

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 the full set of controls in column 5 of Table 2. All regressions are weighted by city population. *** p<0.01, ** p<0.05, * p<0.1

Table A.6—: Robustness Check: First-Stage for Alternative Instrument

	(1)	(2)	(3)	(4)	(5)
Road Instrument	0.858***	1.550***	1.533***	1.561***	1.318***
	(0.103)	(0.189)	(0.222)	(0.225)	(0.206)
Population				-9.949	-14.72*
				(9.557)	(8.434)
Outflows t-1					0.180***
					(0.0317)
Tax Revenues					0.143
					(0.130)
Public School Teachers per Student					-0.381
					(0.306)
Observations	832	832	832	832	832
R^2	0.843	0.901	0.953	0.953	0.958
City FE	N	Y	Y	Y	Y
Time FE	N	Y	Y	Y	Y
City-specific Linear Trend	N	N	Y	Y	Y

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, as well as a city-specific linear trend, and are weighted by city population. *** p<0.01, ** p<0.05, * p<0.1

Table A.7—: Robustness Check: Alternative Instrument when controlling for trends by remoteness

	(1) Average	(2) Low Income	(3) Middle Income	(4) High Income
IDP Inflows t-1	0.0172***	0.0417***	0.00336	-0.0463**
	(0.00658)	(0.0125)	(0.00676)	(0.0204)
Obs	832	832	832	832
First-stage F	24.37	24.37	24.37	24.37

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 the full set of controls in column 5 of Table 2. All regressions are weighted by city population. *** p<0.01, ** p<0.05, * p<0.1

Table A.8—: Potential Channels: IDP Inflows and Housing Deficit (IV)

Dependent Variable:	Housing Deficit			
	Qualitative		Quantitative	
			in Mun. Seat	Outside Mun. Seat
	(1)	(2)	(3)	(4)
Cumulative IDP Inflow t-1 /Population t	0.198	0.346	1.767***	0.00875
	(0.182)	(0.322)	(0.607)	(0.139)
Observations	2048	368	368	362
Municipalities in Sample	All	Urbanized	Urbanized	Urbanized
Municipality FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
First-stage F	33.17	10.63	9.167	13.25

Standard errors clustered at the municipality level in parenthesis. Housing deficit is computed as a share of total households in 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 (1994) to 1992 (2004) normalized by 1993 (2005) population. Additional controls are population (in logs) and the interaction between year dummies and the average distance from a municipality to all other Colombian municipalities. The instrumental variable is the cumulative sum (over the same period as the endogenous IDP inflows) of the receptivity measure (in logs). 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.9—: Potential Channels: IDP Inflows and Homicides. Instrument excluding municipalities within 50 km of host city

	(1) OLS	(2) OLS	(3) IV	(4) IV
IDP Inflows t-1	3.699***	4.332***	6.439***	6.013***
	(0.967)	(1.032)	(2.375)	(2.067)
Population	-234.2**	-21.04	-201.7**	28.85
	(90.63)	(87.67)	(100.4)	(115.5)
Tax Revenues		3.553		3.409
		(2.193)		(2.217)
Public School Teachers per Student		-3.608		-2.689
		(6.015)		(6.104)
Observations	820	820	820	820
First-stage F	.	.	28.55	32.01
Trends by Remoteness	N	Y	N	Y

Standard errors clustered at the city.year level in parenthesis. Dependent variables are rates per 100000 inhabitants. All right hand variables are expressed in natural logarithms. All regressions include time and city fixed effects and city-specific linear trends. All regressions are weighted by city population. *** p<0.01, ** p<0.05, * p<0.1

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