# Spillover effects from new housing supply*

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**Abstract**

I estimate spillovers from new housing supply on house prices. To estimate these effects, I use exogenous variation in supply induced by a housing subsidy implemented in middle-income neighborhoods in the city of Montevideo, Uruguay. I find evidence of externalities from the new supply on house prices, with prices increasing 12%. I explore two possible mechanisms of these externalities: income and crime rates. Although the evidence suggests a reduction in property crime rates, changes in the neighborhood income mix due to the supply expansion represent an important contributor to the external effects. These findings underline the role of amenities in the determination of local house prices.

**JEL Codes**: R23, R30, R58

**Keywords:** Housing supply, neighborhood change, housing prices, crime.

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# 1 Introduction

Evidence on the effect of supply regulations on house prices in cities or metropolitan areas is extensive. (Glaeser and Gyourko, 2009; Hilber and Vermeulen, 2014; Gyourko and Molloy, 2015; Glaeser and Gyourko, 2018). However, research has largely overlooked the potential local effects of increasing housing supply . Within a city, the spatial distribution of these new dwellings may have important implications for neighborhood change (Rosenthal and Ross, 2015). New supply may generate spillovers on the nearby existing housing stock. Whenever the newly built units replace vacant or deteriorated structures or brownfields, the quality of the neighborhood improves (Owens, Rossi-Hansberg and Sarte, 2010; Campbell, Giglio and Pathak, 2010). Also, the new affluent neighbors can be seen as an amenity (Diamond and McQuade, 2019) that potentially attracts other higher-income residents into these neighborhoods (Guerrieri, Hartley and Hurst, 2013). Under spatial equilibrium conditions, all these neighborhood improved amenities are capitalized into higher house prices. If these external effects are substantial, housing prices are predicted to increase locally, even if increased supply reduces prices at the city level. Thus, obtaining causal estimates of external effects to incorporate the full impact of new housing on neighbors and cities is important.

This paper estimates spillovers on house prices from new housing developments. Concretely, I study externalities on the price of existing properties located next to the new housing stock. The resulting estimate provides the local net effect of new supply on house prices. To understand what drives the price effects, I estimate the effect of residential investments on local household incomes, because the new supply may induce changes in the neighborhood composition (Brueckner and Rosenthal, 2009). Also, I assess the impact of new supply on local crime rates, because evidence shows a causal link between crime rates and house prices (Gibbons, 2004; Pope, 2008; Linden and Rockoff, 2008; Ihlanfeldt and Mayock, 2010).[1]

To identify the causal impact of housing supply, I use exogenous variation in the

[1]New housing usually replaces abandoned structures or vacant lots that often attract criminal activity (Spelman, 1993; Ellen, Lacoe and Sharygin, 2013; Cui and Walsh, 2015). Then, criminal activity can be reduced or displaced. However, as the number of higher-income residents increases, the reduction in crime rates can be attenuated, because income is positively related to property crime rates (Sampson, Raudenbush and Earls, 1997). Which of these effects dominates is an empirical question.

spatial distribution of residential development induced by a housing policy implemented in Montevideo, Uruguay, starting in 2011.[2] The policy consisted of a series of tax breaks to developers and private investors to promote the introduction of new housing stock into certain neighborhoods, with the program applying to new developments in a spatially defined middle-income area within the city. The borders of the subsidized (or targeted) area follow several city divisions provided by the main avenues and streets without following the boundaries of any administrative area.[3] The criteria used to select the targeted areas are not available in any official document. However, the policy 's design reveals the intention of excluding high-income neighborhoods in the city. Investments carried out under this program were sizable, totalling 1.5% of the country's GDP. No explicit rules were given regarding the socioeconomic characteristics of buyers or tenants of newly built units, and developers ended up building affordable housing for middle- and high-income households. Overall, the policy pushed new construction into the targeted area, especially to neighborhoods close to the spatial boundary.

I obtain my main estimates using a continuous difference-in-differences estimator that I combine with an instrumental variable strategy. The continuous treatment variable is based on the realized spatial pattern of new developments to reflect different intensities of exposure to the policy. For this purpose, I construct a treatment measure that captures the investment exposure of existing housing units to subsidized projects, by computing the weighted sum of all nearby investments using inverse distance decay weights. Because it can be endogenous, the investment-intensity variable is instrumented by a binary indicator that takes a value of one for units located in the treated area, thus exploiting the place-based nature of the subsidy. To estimate these effects, I build a dataset combining information on the individual projects with data on the universe of housing transactions, survey data on household incomes, and daily crime records.

I find evidence of substantial – but highly local – spillover effects of new residential development. Specifically, a one-standard-deviation increase in the intensity of exposure leads to a 12% increase in house prices. Price effects vanish after roughly

[2] The department of Montevideo has 1.3 million inhabitants, whereas its metropolitan area houses 1.9 million, making it the largest urban area.

[3] I use the terms subsidized, targeted, and treated area interchangeably.

200 meters from the border on the unsubsidized side. My reduced-form results are in line with the estimates obtained in Ahlfeldt et al. (2015) resulting from estimating a quantitative model, with residential externalities being highly local. Also, externalities are larger whenever the new housing is in locations with initially lower infrastructure (i.e., a higher share of buildings in poor condition and a lower quality of dwellings pre-policy). Regarding mechanisms, the results confirm an increase in household income per capita, representing an important driver of these externalities, whereas evidence suggests a reduction in property crime rates.

This paper contributes to the growing literature analyzing the effect of new housing supply on neighborhoods and housing markets. Ooi and Le (2013) and Zahirovich-Herbert and Gibler (2014) use a ring regression approach to analyze spillovers on local housing prices from new residential construction, which does little to mitigate endogeneity concerns due to developers' investment-location decisions. One of the contributions of this paper is to overcome this concern by taking advantage of the policy to generate an exogenous change to the spatial distribution of residential housing investments in the city. Based on simulated evidence from a neighborhood-choice model, Anenberg and Kung (2018) find increasing supply has negligible effects on rents. Brueckner and Rosenthal (2009) develop a model that predicts how residential development and redevelopment affect the income composition of neighborhoods. Here, I provide causal estimates by exploiting exogenous (spatial) variation in residential construction. To estimate whether supply-side factors can drive gentrification in urban cores, Boustan et al. (2019) analyze whether a higher density of condominiums attracts high-income residents. Instead, I focus on estimating the local spillover effects of new supply on the housing market.

This paper is related to the literature examining the spillovers from affordable housing developments in the US. Many studies have investigated the effect of the Low-Income Housing Tax Credit (LIHTC). Eriksen and Rosenthal (2010) analyze the crowding-out effects of the LIHTC. Baum-Snow and Marion (2009) and Diamond and McQuade (2019) find evidence of positive spillovers on house prices when the subsidized units are located in lower-income neighborhoods, but Diamond and McQuade (2019) also find negative externalities when units are located in higher-income neighborhoods. Freedman and Owens (2011) analyze the effects of LIHTC on crime at the county level and find a significant reduction in violent crime but no detectable effects on property crime. Schwartz et al. (2006) study spillovers from place-based subsidized housing in New York using a ring regression approach. One key difference in my paper relative to this literature is that the new developments studied here are not targeted at low-income households. Therefore, my results may provide a better understanding of the spillover effects from regular, private construction activity in the residential sector.

Finally, this paper draws on previous studies estimating externalities resulting from urban renewal interventions (Owens, Rossi-Hansberg and Sarte, 2010; Ahlfeldt, Maennig and Richter, 2017; Koster and Van Ommeren, 2019), which find mild to moderate evidence of housing externalities and very localized effects. From a methodological perspective, my paper relates to Turner, Haughwout and Klaauw (2014), who estimate external effects of land-use regulation on land values, exploiting the fact that land-use regulation varies across municipal borders. One advantage of using a within-city boundary is that local policies vary smoothly over space so that substantial differences in local public services and other unobservables between treated and control locations are unlikely.

# 2 Theoretical framework

In a city-wide demand-and-supply framework, increasing housing supply reduces prices, whereas housing shortages in fast-growing cities lead to higher house prices. These predictions are empirically supported by a well-developed literature on the effect of restricting supply on house prices at the city or metro-area level (Glaeser and Gyourko, 2018). However, spatially aggregated analyses may mask local demand effects induced by increasing housing supply across different housing segments. The filtering literature (Rosenthal and Ross, 2015), for example, would suggest that as new construction comes online in affected neighborhoods, some housing segments in other locations experience a negative demand shock (at least relative to trend), leading to lower prices than otherwise at the city level. These models provide a compelling explanation of why average prices drop after cities face new housing developments.

In a more disaggregated context, the effect of new housing on house prices can vary. Considering a demand-and-supply model again but focusing on a smaller geographical unit (e.g., a neighborhood), we find increasing housing supply leads to a set of predictions that depend on countervailing demand effects. Supply effects naturally push down prices in affected neighborhoods. On the other hand, some factors cause the demand curve to shift upward, potentially increasing prices. For example, locations with newly built houses increase high-income households ' demand for housing services (Brueckner and Rosenthal, 2009; Guerrieri, Hartley and Hurst, 2013; Bilal and Rossi-Hansberg, 2018). Additionally, the new units refresh the housing stock, reducing brownfield sites or replacing vacant structures. Therefore, increasing housing supply improves neighborhoods' quality, which may induce changes in their income composition. These demand effects drive house prices up, contributing to positive spillovers on the nearby housing stock. If demand factors more than offset supply effects, house prices will increase locally. Otherwise, a decrease in prices or an absence of an effect should be expected. Which effect ultimately prevails (if any) is an empirical question.

In my empirical analysis, I use housing policy as a supply shifter that induces an exogenous change in the spatial distribution of new residential construction. Therefore, finding a decrease in prices in treated neighborhoods should be interpreted as demand effects being negligible. Otherwise, we would conclude demand effects are important and possibly outweigh the supply effects. These amenity effects increase the demand for housing services by making the neighborhoods more appealing. To complement my analysis, I explore two potential demand drivers: changes in the income composition of affected neighborhoods and criminal activity.

# 3 Institutional setting: the LVS policy

In August 2011, the Uruguayan government introduced tax breaks for private investments in housing (Law Nbr. 18,975), the LVS policy hereafter. The program aimed to incentivize the construction sector and improve the housing stock for both sale and rent.The program allows up to 10% of all produced units to be commercial, but no tax benefits are given for these units. It does not impose requirements on buyers or tenants' characteristics, and then developers mainly produced housing for the middle-high income household segment.[4] There were two types of LVS projects: new construction and rehabilitation projects, with almost three-quarters being new builds. Rehabilitation projects involve upgrading deteriorated housing and increasing the total number of residential units, usually of semidetached houses. New construction projects comprise substantially larger budgets (around 11 times) compared to rehabilitation projects that generally involved low investments. In my empirical analysis, I focus on new construction projects.

Regardless of the type, the program required to produce at least two and up to a maximum of 100 new residential units per lot except for large-size lots or lots containing disused structures (e.g., factories or homes), where the upper limit did not apply. So, the LVS policy promoted the construction of flats and semi-detached houses. Also, LVS units had to adhere to the guidelines laid down in the National Housing Plan and other ministerial regulations on quality and size requirements.[5] On a quality scale from 'Very poor' to 'Excellent', around 95% of the LVS units were assessed as 'Excellent' by the Cadaster Agency. Figure B.1 in the Online Appendix shows the distribution of quality of the LVS units and the existing stock (non-LVS units).

The main fiscal advantage for developers and private investors was an exemption from paying any corporate tax (25%) on the sold LVS units. At the same time, rents from LVS units were partially exempted from personal income and corporate taxes over nine years.[6] The tax benefits only applied to projects undertaken in urban areas, excluding those in cities with a high proportion of second homes. Applications were submitted at any time of the year. Submissions were first evaluated by the National Housing Agency (ANV) and, later, by a committee composed of members from the Ministry of Economics and Finance and the Ministry of Housing responsible for implementing the policy.

A total of 494 new construction projects were performed between December 2011

[4]The average price in $m^2$ of an LVS unit in Montevideo is approximately 2,700 USD, while the average for the city is 1,896 USD.

[5]The National Housing Plan contained in Law Nbr. 13,728 of 1968 was replaced by Law Nbr. 19,581 that came into force in 2018. The new regulation requires that one-bedroom LVS units must have between $35m^2$ and $50m^2$. With each additional bedroom (up to a total of four), the lower and upper limit increased by $15m^2$ and $25m^2$, respectively. In addition, the number of single-bedroom units must be lower than 50% of the total number of produced units.

[6]Other fiscal benefits included exemptions from the wealth tax and the transfer tax if buying unsold units. There were also tax credits for value-added tax on national and imported inputs, which, given the structure of these taxes, it may reduce developers' liquidity constraints and opportunity costs.

and December 2018, involving 15.5K new housing units. The total amount invested is roughly 1.5% Uruguayan GDP. 70% of all projects were concentrated in the capital city. The empirical analysis is focused on 309 projects carried out in Montevideo (3.4M USD per project) with an average construction period of 21 months. For these projects, the tax exemptions per housing unit roughly represent 20% of the construction costs.

As observed in Figure 1, the LVS policy has a place-based structure for new construction with tax breaks only applying to projects located in the area labeled as $S$. This area represents just over half of Montevideo's total urbanized area, composed of central and peripheral neighborhoods. House prices vary widely across the area $S$ (see Figure B.4 in the Online Appendix). The dotted and dashed areas are the two unsubsidized parts of the city.[7] The area labeled as $U$ is the richest and densest area of the city. In contrast, the dotted areas (the suburbs) is the city's poorest areas, where most slums are located. Figure 1 also shows the spatial distribution of the LVS projects. There is a high concentration of projects on the southern border of area $S$. Three-quarters of the projects were performed within two kilometers and three-fifth within one kilometer of this border. Then, developers chose locations close to the unsubsidized (dashed) areas characterized by high house prices.

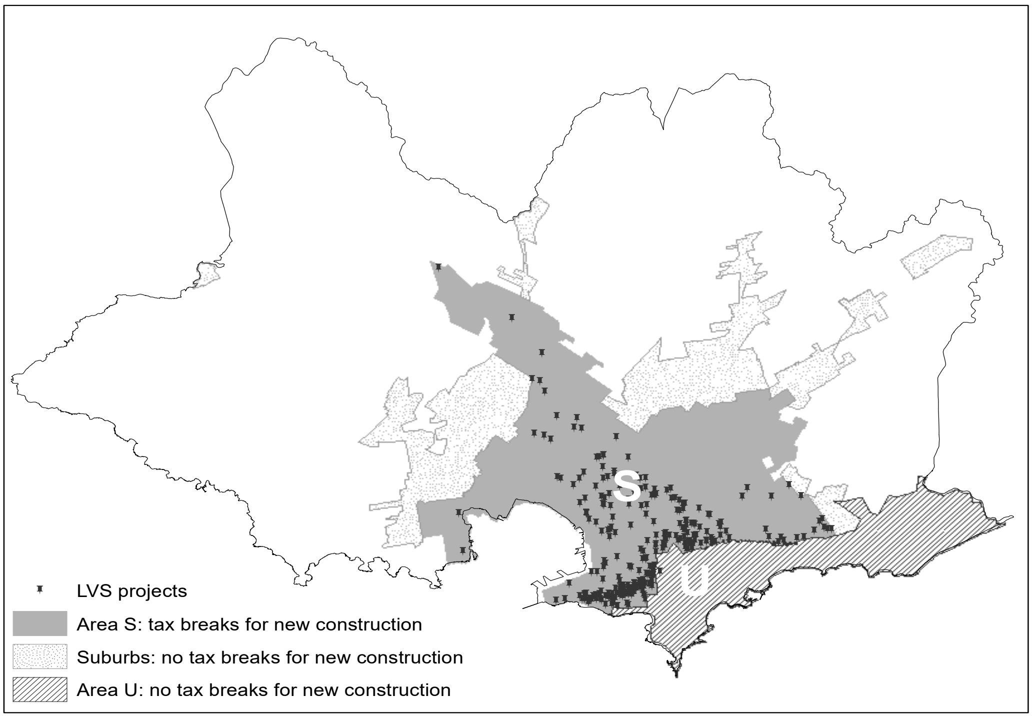
The treated area $S$ was defined by the Ministry of Housing, the Ministry of Economics and Finance, and the Local Government of Montevideo. There is no official document on the criteria used to define the treated area. Overall, it adhered to some natural city divisions provided by main avenues and streets without following any other administrative division's borders.[8] The program also seems to intentionally exclude the city's high-income areas where most construction activity occurs.

How did the LVS policy affect the spatial distribution of housing supply? Table 1 shows the m$^2$ of new residential buildings within one km of border $S-U$, where most of the LVS activity occurred. The first column shows that the policy boosted the construction of residential units in area $S$, which more than doubles the m$^2$ developed in the pre-LVS period. In contrast, the construction activity in area $U$ remains

[7]In both areas, there are tax breaks for rehabilitation projects. However, only 11 rehabilitation projects were performed in area $U$, while no such projects were in the dotted areas.

[8]Assignment of students to public schools is, overall, not residence-based. Also, the private sector accounts for a large share of the city's schooling supply. 44% and 50% of the primary and secondary schools in Montevideo are private, respectively (INEEd, 2014).

Figure 1: Place-based scheme for new construction projects in Montevideo (Uruguay)




*Notes:* The policy was introduced in August of 2011. The subsidy for new construction projects only applies in the gray area $S$. *Source:* National Housing Agency (ANV) & Local Government of Montevideo.

at the pre-LVS policy levels. The double difference (last column and last row) indicates a substantial increase (456,708 m$^2$) of new residential buildings, which gives a raw estimate of the effect of the policy on new construction in the LVS area ($S$). In Figure B.5 in the Online Appendix, I extend this analysis using lower geographical units (i.e., census tracts), and I show that there is a discontinuity in new residential construction at the S-U border. Also, Berrutti (2017) finds that the policy impacted the distribution of residential construction, the size of the housing being produced, and the density of new buildings close to the border. So then, the policy is likely to lead to quantity effects. Quality effects are also likely to be present as the existing housing stock in the LVS area close to the S-U border has a regular quality (on average), while the LVS units were assessed as having an excellent quality by the Cadaster Agency. Figure B.2 in the Online Appendix shows the distribution of the quality of the existing stock of housing in area $S$ and area $U$ within one kilometer of the border S-U. This naturally increases the average quality of housing in affected neighborhoods.

The evidence seems to supports that the policy led to more residential investments in the subsidized area relative to the unsubsidized area and increased the quality of the housing stock.

Table 1: $\mathrm{m}^2$ of new residential buildings within one km of border $S - U$.

| | Area S | Area U | Diff. (S-U): |
|---|---|---|---|
| (1) pre-LVS | 291,234 | 302,861 | -11,627 |
| (2) LVS period | 754,879 | 309,798 | 445,081 |
| Diff. (2)-(1): | 463,645 | 6,937 | 456,708 |

*Source:* Own calculations based on data from the National Cadaster Agency (DNC).

# 4 Data & variables

To undertake this research, I use data from multiple sources. I begin presenting the data on LVS projects used to construct the intensity of exposure to the policy later defined in Section 5. Subsequently, I introduce the data on the primary outcome variable (i.e., house prices) and the outcomes potentially linked to the underlying mechanisms of housing spillovers. Finally, I show descriptive statistics for the area used in the empirical analysis.

## 4.1 LVS projects data

The official data on LVS projects comes from the National Housing Agency (ANV). It contains information about projects' location, including street address and the reference number in the land register (i.e., the parcel number) that I use to obtain their coordinates using GIS software.[9] Georeferenced projects are displayed in Figure 1. The information on LVS boundaries (in shp format) is publicly available at the Geographical Information System of Montevideo, powered by the Local Government. It allows computing the distance of each project (and any other parcel) to the borders of the LVS area.

The LVS project dataset also has information on approval date, the total number of housing units produced (including commercial units and lofts), the total budget and budget schedule, whether the project includes facilities and amenities (e.g.,

[9] I downloaded the shapefile of urban parcels produced by the Cadaster Agency. The data on LVS projects can be joined to the shapefile by the parcel number to get the geographical coordinates.

garages), and three categories for project size (large, medium and small). Budgets are reported in units of account indexed to inflation, which I then convert to USD using official data on exchange rates. This information will be used to construct my treatment measure.

## 4.2 House price data

The data on house prices comes from the National Registry Office (DGR) for 2004-2018. This dataset provides information on the price and built area (in square meters) of transacted housing units reported by notaries, who are in charge of registering housing sales in the DGR. The National Statistical Office (INE) uses this data to construct housing price indices and statistics on house prices per square meter by neighborhoods in Montevideo. Importantly, it includes the transaction date and parcel number that I used to get the location of transactions (i.e., latitude and longitude). Having the geographic coordinates is crucial as it allows computing the straight line distance from transacted units to LVS projects. As this paper is concerned with estimating spillovers, I identify and drop transactions of LVS units by merging the house price data with the data on LVS projects.

Almost 90% of all transactions are reported in USD. The remaining 10% is reported in local currency and units indexed to local inflation, which I convert to USD. Then, my primary outcome of interest is the logarithm of house prices in USD. Even after using logs, some values remain abnormally low because the currency was misreported (see Figure B.6 in the Online Appendix). In addition, there are extreme values at the top of the distribution related to large-size properties. To avoid ad-hoc cleaning rules, I exclude transactions with prices in the top/bottom one percentile by year and separately for houses and buildings. In the empirical section, I check whether results are sensitive to not dropping these observations and drop the top/bottom fifth percentile.

The house price data is combined with data on housing characteristics from the National Cadaster Agency (DNC), which generally updates its records whenever properties are reassessed.[10] The cadaster data includes parcel number (used to merge with the price data), constructed square meters, year of construction, quality

[10]This data is elaborated by the DNC and publicly available at the Open Data Catalogue of the Uruguayan Government (https://catalogodatos.gub.uy/). It is updated on a continuous basis.

and type of the building, number of floors, whether it is a single-family dwelling or an apartment, whether it has a garage, balcony or outdoor space, and other amenities. These housing characteristics will be used as control variables in a hedonic regression model.

## 4.3 Other outcomes

### 4.3.1 Household income data

I use data on household income from the National Household Survey (ECH), a yearly stratified random sample of households, from 2006 to 2019. Together with the Census, the ECH is the main source of socio-economic information about Uruguayan households at the national level, both being carried out by the National Statistical Office.

The ECH has information on monthly household disposable income with and without rental value and the number of household members. The rental value is the reported amount that renters pay for their house/flat. In the case of owner-occupied dwellings, the rental value is imputed. I construct two outcome variables, the per capita household income (adjusted by the consumer price index) with and without rental value. In addition, I use the rental value as a third outcome since it may represent a measure of local living costs. The ECH includes census tract identifiers that allows combining the income data with a tract-level version of the intensity of exposure to the LVS policy introduced in the next section.

### 4.3.2 Criminal register data

I got access to geo-coded daily crime incidents between 2006 and 2018 reported at the Police Department of Montevideo.[11] This database has the universe of all offenses, 1,331,357 in total. It also contains information about the geographical coordinates and type of crime according to the Uruguayan penal code of incidents. Theft and robbery are the two most frequent offenses, representing 53% and 13% of the recorded incidents, respectively. Both offences imply depriving a person of property, with robbery involving the use of violence.

[11]This database has been used by Ajzenman and Jaitman (2016) to study crime concentration and crime hotspots in Montevideo, among other Latin American cities. Munyo and Rossi (2015) use this data to study crime recidivism.

I focus on three outcomes, number of crimes, number of property crimes (sum of theft and robbery), and number of nonproperty crimes (the difference between total and property crimes). I aggregate these three variables by year and census tract and work with a tract-year panel. In Montevideo, tracts have around 1,300 inhabitants and an area of about a third-squared kilometer on average, being comparable to block groups in the US (see Figure B.9 in the Online Appendix for a visual example). For the three outcomes, below 1% of the tract-year units have zero offences. Then, I focus on the logarithm of tract-level yearly counts of total, property, and nonproperty crimes.[12]

## 4.4 Area of the analysis & descriptive statistics

The empirical analysis is focused on units (i.e., housing transactions, households, and census tracts) located within one kilometer of the southern border of the LVS area. Specifically, it comprises units located in the LVS area $S$ and those located in the unsubsidized area $U$ that falls within a one-kilometer buffer (see Figure B.7 in the Online Appendix). Even that the policy targeted a broader area, a large share of LVS projects took place close to the S-U border. Then, most of the identifying variation in the empirical analysis comes from locations nearby the border. Taking a broader area increases the number of less appealing sites for developers. In addition, the area $U$ is geographically constrained by the river. Thus, using a larger buffer results in an untreated area less comparable in length and size. Even though, in the empirical section, I check whether estimates change to using observations within two kilometers at both sides of the border.

Table 2 presents the average, minimum and maximum value of the outcome variables (in 2011) for units within one kilometer of S-U border. House prices are lower on the subsidized side compared to the unsubsidized side. Household income shows a similar pattern as house prices. Total crime and property crime rates are slightly lower in the LVS area, with nonproperty crime being slightly larger. In Latin American cities, property crime tends to be more prevalent among higher-income neighborhoods. There are also descriptive statistics of control variables from the 2011 Census. The LVS area has a higher share of rented dwellings, unemployment rate,

[12] Due to the small proportion of tract-year units with no offences, I add the value of one to each outcome before applying the log transformation.

and head of households with low education compared to tracts located in the untreated area. Both areas have a similar street quality index, which captures the level of basic urban infrastructure.[13] The LVS area also has a higher percentage of historical monuments as it comprises central city neighborhoods. Finally, density varies more widley in nontreated neighborhoods than in neighborhoods located in subsidized locations.

Table 2: Descriptive statistics.

| | LVS area ($S$) | | | Unsubsidized area ($U$) | | |
|---|---|---|---|---|---|---|
| | Mean | Min | Max | Mean | Min | Max |
| **Outcome variables (in 2011)** | | | | | | |
| Log of house prices | 10.934 | 7.713 | 13.653 | 11.399 | 8.294 | 13.592 |
| *Obs.* | | 2851 | | | 3052 | |
| Log of total crime | 4.495 | 2.079 | 6.232 | 4.389 | 1.609 | 6.290 |
| Log of property crime | 4.210 | 1.792 | 5.911 | 4.166 | 1.099 | 5.964 |
| Log of nonproperty crime | 3.071 | 0.693 | 5.468 | 2.791 | 1.099 | 5.017 |
| *Obs.* | | 169 | | | 85 | |
| Log of household income w/ rental value | 9.837 | 6.169 | 12.778 | 10.107 | 7.901 | 12.505 |
| Log of household income w/o rental value | 9.645 | 6.169 | 12.764 | 9.890 | 6.506 | 12.447 |
| Log of rental value | 9.054 | 4.546 | 10.561 | 9.400 | 6.139 | 11.290 |
| *Obs.* | | 3065 | | | 1844 | |
| **2011 census variables** | | | | | | |
| Log of density (inhabitants per km2) | 9.467 | 7.493 | 10.616 | 9.345 | 2.308 | 10.758 |
| % of vacant or uninhabitable dwellings | 3.438 | 0.509 | 11.899 | 2.665 | 0.000 | 8.041 |
| % of buildings in poor condition | 0.846 | 0.000 | 26.056 | 0.384 | 0.000 | 5.525 |
| % of rented dwellings | 37.983 | 11.036 | 63.753 | 28.643 | 0.000 | 51.421 |
| Unemployment rate | 5.877 | 2.723 | 11.840 | 5.018 | 3.050 | 11.111 |
| % of low-educated head of households | 8.718 | 2.157 | 45.377 | 5.887 | 1.569 | 40.000 |
| % of historical monuments | 3.441 | 0.000 | 72.746 | 0.578 | 0.000 | 10.590 |
| Street quality index [0,1] | 0.549 | 0.159 | 0.800 | 0.550 | 0.400 | 0.710 |
| *Obs.* | | 169 | | | 85 | |

*Notes:* Units within one kilometer of the border. Observation located at the border belong to the LVS area (area $S$).

# 5 Empirical strategy

To estimate housing spillovers, I measure the intensity of exposure to new housing developments carried out in the context of the LVS policy. The spatial variation in this measure of exposure is due to different developers' location decisions within the subsidized area. As a result, existing residential units get differently exposed to the policy. The empirical strategy compares changes in house prices across units with different intensities of exposure before-and-after the introduction of the LVS policy. To deal with endogenous project location, I exploit variation between sides of the

[13]This index is constructed as a weighted average of several binary indicators on public lighting, presence of trees, having paved streets in good condition, presence of sidewalk in good condition, presence of sidewalk with ramps for the disabled, information about the street name, presence of storm drains, and presence of dumps. For each indicator, weights are defined as one minus the average and then normalized to sum to one. The index is bounded between 0 and 1.

LVS policy boundary over time – i.e. before-and-after the introduction of the policy – as an instrument for the intensity of LVS-related construction activity. I use the same strategy to explore the effects on household income and crime rates.

## 5.1 The measure of exposure: Intense

The intensity of exposure of an existing housing unit $i$ is a weighted sum of the total budget of LVS-projects ($J = 309$ in total), where the weighting scheme $\omega(d_{ij})$ is a decreasing function of distance $d_{ij}$ (in km) from unit $i$ to each project $j$:

$$\text{Intense}_i = \sum_{j=1}^{J} B_j \omega(d_{ij}) \tag{1}$$

$B_j$ is the total budget of project $j$ in USD, without including land values or price of previous properties. The ratio of the price of the previous property to the total budget (w/o land value) is almost 20% on average. The weighting function is defined as the inverse (Euclidean) distance: $\omega(d_{ij}) = 1/d_{ij}$. It puts more weight on nearby projects as in the Harris market potential (Harris, 1954). As a robustness check, I also show results using an exponential decay function: $\omega(d_{ij}) = e^{-\lambda d_{ij}}$.[14]. $\text{Intense}_i$ shows the level of LVS-related housing investments that each residential unit $i$ is exposed to, representing the continuous treatment variable in the empirical model later explained.

The variable Intense is right-skewed due to extreme values in the upper tail, and it only takes positive values. Then, I use this measure in logs instead of levels, which I standardized to ease interpretation. The top panel of Figure 2 shows the histogram of the standardized log of Intense for residential units located within one kilometer of the S-U border. The distribution of this measure seems close to a normal distribution. This measure presents outliers from transactions located only a few metres from LVS projects. In the empirical section, I test the sensitivity of results to dropping the 1st top/bottom percentile of the variable Intense. The bottom panel presents the investment intensity as a function of distance to the boundary. Distance is normalized to be zero at the border, with positive (negative) values denoting subsidized (unsubsidized) locations. Each evenly spaced bin represents the

[14]As in (Autor, Palmer and Pathak, 2014), I try different parameterizations of the decay function $\lambda$. For $\lambda \geq 6$, the different intensity measures get highly correlated. The correlation between an inverse distance and an exponential decay intensity measures with $\lambda = 9$ is .73.

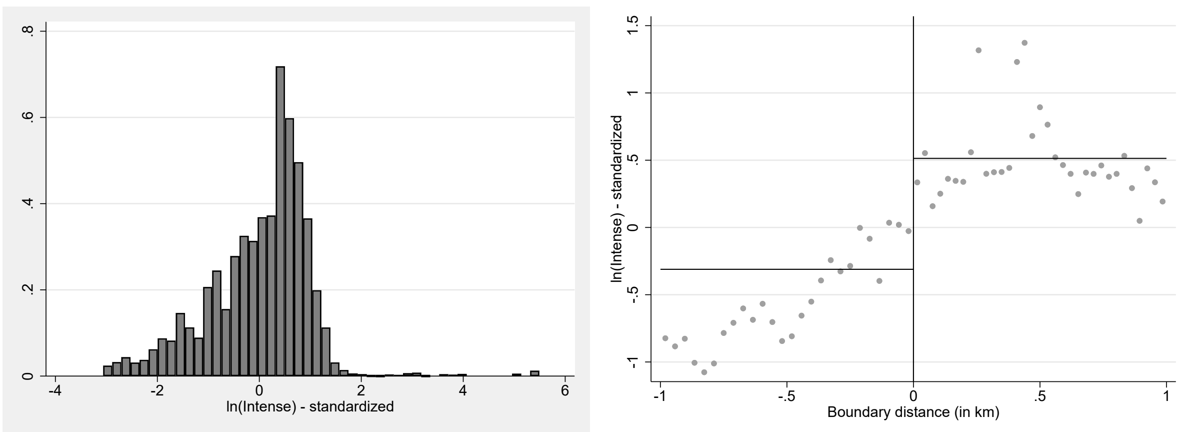
average degree of exposure, and the fitted line is the estimated conditional expectation function using a zero-order polynomial. The graph shows that the exposure to LVS developments increases when approaching the LVS area from left to right, suggesting a discontinuity in the boundary. The investment intensity also presents variation along the S-U border (as observed in Figure B.8 in the Online Appendix) that is used for estimating spillovers.

This continuous measure is preferred to a binary treatment as some LVS projects are spread while others are concentrated within the subsidized area. The spatial variation in projects' location naturally implies that some residential non-LVS units were highly exposed to the policy while others were slightly or not exposed. Projects also varied in the degree of investments and number of units built, adding another dimension of variation in the exposure. While this index enables variation in both projects' location and level of investment, these two dimensions complicate the implementation of a ring regression method as in Schwartz et al. (2006), Ooi and Le (2013) and, Zahirovich-Herbert and Gibler (2014) that requires defining (discrete) treated and control rings based on distance.

Figure 2: Measure of exposure to the LVS policy: Intense

(a) Histogram

(b) Exposure within 1km of the border




*Note*: the variable Intense is defined as in equation 1. In panel a and b, the standardized log of Intense is displayed for residential units located within one kilometer of the southern edge of the subsidized area. In panel b, bins are constructed using the data-driven procedure developed in Calonico, Cattaneo and Titiunik (2015, 2017).

## 5.2 The empirical model

I use a continuous-treatment difference-in-differences (DiD) model that compares changes in house prices of units in locations differently exposed to the LVS policy before-and-after its introduction. The main challenge is that developers' location decisions are non-random across space, even within small geographical areas and then, OLS estimates are likely to be biased. One way to deal with endogeneity induced by endogenous project placement is to use a third variable that explains the location of new developments without directly affecting house prices. I use the place-based structure of the LVS policy as an instrument for the intensity measure. Specifically, the instrument is a binary variable $Treat_i$ that takes the value of one(zero) for units located in the (un)treated side of the border. The tax breaks introduced by the LVS policy provide incentives for developers to build on the LVS area, so it correlates well to actual decisions captured by the intensity of exposure.

I instrument the log of $\text{Intense}_i$ and the log of $\text{Intense}_i \times post_t$ respectively with $Treat_i$ and $Treat_i \times post_t$, and I estimate the following equation by two-stage least squares:

$$ln(p_{ibt}) = \alpha ln(\text{Intense}_i) + \beta ln(\text{Intense}_i) \times post_t + X_i^{'}\theta + \delta_{bt} + u_{ibt} \quad (2)$$

$ln(p_{ibt})$ is the log-price of a non-LVS housing unit $i$ in border-segment $b$ and in year-month $t$. The definition of $b$ follows from the partition of the boundary into six segments based on census tract divisions.[15] Each border-segment is about two kilometers long and comprises neighborhoods with different socioeconomic characteristics. $\text{Intense}_i$ is constructed as in equation (1), and $post$ is a dummy that takes the value of one after 2011 (the year that the policy came into effect) onwards. $X_i$ is a vector of dwelling characteristics included in hedonic models that help explaining part of the variation in house prices. The specification includes border-by-year effects ($\delta_{bt}$) to account for possible unobserved time-varying trends. In that sense, the border-year effects allow the comparison of units comparable on unobserved dimensions and that are likely to be subject to similar shocks in housing segments in other locations (e.g. the suburbs). In addition, I explore adding census tract fixed effects

[15] Census tract edges are used as a reference to delimit the border-segments. So each segment $b$ comprises several census tracts from each side of the border. Figure B.9 in the Online Appendix shows an example of a border-segment together with census tracts divisions.

that further narrows the set of units being compared within each border-segment.

The parameter of interest is $\beta$, the causal effect of new supply due to the LVS policy on prices. In the context of a hedonic model of housing choice (Rosen, 1974; Bajari and Benkard, 2005), the estimated elasticity of prices to new housing is interpreted as the local marginal willingness to pay in areas exposed to the program.

The validity of my instrument relies on the border being exogenously determined by policymakers instead of developers interested in including places with high expected growth prices. As mentioned in section 3, there is no official document explaining the criteria used to set the LVS boundary. As observed in a heat map of house prices (Figure B.4 in the Online Appendix), the subsidized area excludes neighbourhoods with high prices. The fact that areas with high house prices were excluded may go against developers' preferences and tend to support the hypothesis that LVS borders were not manipulated.

The identifying variation of the instrumental variable approach comes from cross-border variation before-and-after the policy comes into effect. As a result, the identifying assumption is a typical parallel trend assumption as in a conventional difference-in-differences method. The presence of unobserved time-varying trends at different distances to the border is likely to be reflected as a violation of the common trend assumption. In other words, the existence of pre-trends is considered as evidence against strict exogeneity of the policy change (Freyaldenhoven, Hansen and Shapiro, 2019). I will show evidence of the absence of such pre-trends through event study graphs.

In the next section, I report the estimates of $\beta$. In addition, I report estimates from replacing Intense$_i$ by $Treat_i$ in equation (2) that involves directly comparing house prices inside and outside the LVS area before-and-after the changes in housing stock induced by the policy. This latter strategy provides intention-to-treat estimates since it relies only on variation across the treated boundary without incorporating the actual LVS building activity. The instrumental variable strategy previously explained is also used with other outcomes (i.e. crime activity and household income) to study the underlying housing externalities mechanisms.

# 6 Results

## 6.1 Baseline estimates

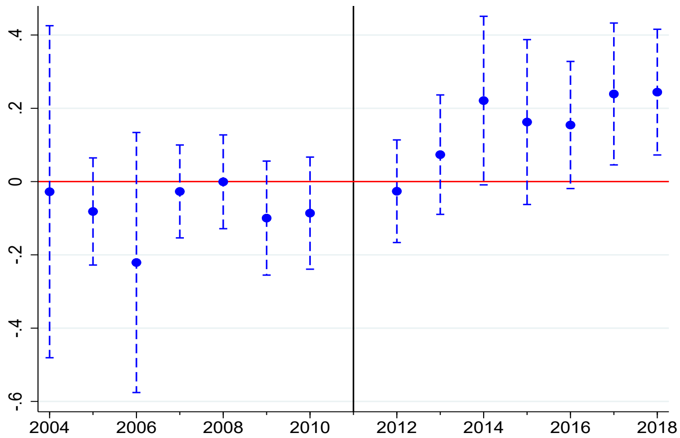
Before presenting the table with main results, I first focus on the dynamic effects of new developments on house prices obtained from replacing $ln(\text{Intense}_i) \times post_t$ by $\sum_{k=2004}^{2018} \rho^k ln(\text{Intense}_i)\mathbb{1}\{t = k\}$ in equation 2. Since $ln(\text{Intense}_i)$ is included in the regression, I omit $ln(\text{Intense}_i)\mathbb{1}\{t = 2011\}$. This means that these coefficients indicate the effect on prices relative to the year that the LVS policy was introduced. In addition, all regressions include housing characteristics as in traditional hedonic models.

The estimates of $\rho^k$ are reported in Figure 3. The graph shows that the differences in house prices across units differently exposed to the policy are stable and statistically indistinguishable from zero between 2004 and 2010. The p-value of a joint test for equality of coefficients $\rho^{2004}$ through $\rho^{2010}$ is .691. This is reassuring as it provides supporting evidence on the common trend assumption, validating the empirical strategy. It also rejects the hypothesis that developers reacted beforehand as there are no anticipation effects on the housing market locally.

Interestingly, it is also observed that there are no effects on prices one and two years after the introduction of the policy, which coincides with the construction of the first LVS projects. It is not until three years after the policy begins and when a large proportion of projects were completed (see panel a in Figure B.10 in the Online Appendix) that a break in the trend is observed. The 2017 and 2018 coefficients reveal an appreciable increase in prices, reporting an estimated elasticity of just over .2. This is due that the largest number of sales did not come until 2017, as observed in panel b in Figure B.10 in the Online Appendix that displays the accumulated sales of LVS units.

The DiD estimates of the effect of new developments on prices are reported in Table 3. Panel a provides the IV estimates - $\beta$ in equation 2 - and panel b provides the intention-to-treat estimates that result from replacing $ln(\text{Intense}_i)$ by $Treat_i$ in 2 and using OLS. Estimates reported in columns 2 and 3 are obtained from adding the 2011 census variables to the set of control variables $X$ and further including pre-trend prices at the tract level as a control. Results show a positive and significant

Figure 3: Event-study graph: house prices.




*Notes*: Dots represent the estimated parameters associated with the interaction between the variable Intense and year dummies, with 2011 being the omitted year. These are reduced form results estimated by OLS. Vertical dashed lines correspond to 95% confidence intervals constructed using clustered standard errors at the tract level.

effect of new housing developments on house prices across the board. The estimated magnitude diminishes across columns as more control variables are included. Focusing on column 3 of the IV estimates, the elasticity of house prices to the intensity of exposure to new developments is .169. Given that the variable Intense$_i$ has been standardized, the effect on house prices for a one-standard-deviation increase in the intensity of exposure is $.169 \times .718 \times 100 = 12.1\%$. The reported (Kleibergen-Paap) F-statistic associated with the first stage is relatively high.[16]

The intention-to-treat (ITT) results indicate that prices of dwellings located in the subsidized side experienced an 8.6% increase relative to properties in the unsubsidized area (column 3). In this case, the estimated effect is smaller as this strategy does not incorporate variation in actual building activity. The event study graph of the intention-to-treat estimates presented in Figure B.12 in the Online Appendix exhibits a similar pattern to that of IV yearly estimates.

As previously explained in section 4.2, the data on house price presents some transactions with prices abnormally low and some extremely high and then, I opted to drop the 1st top/bottom percentile. In Table A.1 in the Appendix, I check whether

[16] Lee et al. (2020) recently argued to adjust the critical value for $t$ from 1.96 to 3.43 if the threshold for $F$ of 10 is used. Using a critical value for $t$ of 3.43 leads to a non-significant point estimate (since the t-ratio is 3.13) for the entire post-treatment period (2012-2018). However, after splitting into two periods (2012-2014) and (2015-2018), estimates for the last period become significant under a critical value of 3.43.

results are sensitive to not dropping outliers at all (labeled as 'raw') and even dropping the 5th top/bottom percentile (labeled as 'trimmed: 5th top/bottom pc'). In neither case, the results change substantially. I also test the sensitivity of results to dropping the first top/bottom percentile of the variable Intense, and results are similar to the baseline IV estimates (see Table B.1 in the Online Appendix).

Table 3: IV and ITT estimates - House price effects of new developments.

| | (1) | (2) | (3) |
|---|---|---|---|
| **a) IV estimates** | | | |
| ln(Intense) × post | 0.225*** | 0.174*** | 0.169*** |
| | (0.070) | (0.057) | (0.054) |
| First-stage F-stat | 54.569 | 55.555 | 55.834 |
| **b) ITT estimates** | | | |
| Treat × post | 0.104*** | 0.089*** | 0.086*** |
| | (0.028) | (0.025) | (0.024) |
| Adj. $R^2$ | 0.492 | 0.506 | 0.509 |
| 2011 census controls | N | Y | Y |
| $\Delta$ house prices $_{\text{pre-policy}}$ | N | N | Y |
| Observations | | 70,422 | |
| Number of clusters | 251 | 251 | 251 |

*Notes:* All regressions include housing characteristics as described in section 4.2 and border-year fixed effects $\delta_{bt}$. Standard errors are clustered at the tract level. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

So far, the empirical results presented here are consistent with demand effects being relevant at lower geographical levels. Unless locally, increasing the stock of new housing triggers the demand for housing services and then driving house prices up. The estimated spillover effects on prices are 8.6% and 12.1% obtained from the ITT and IV strategy, respectively, with the largest set of control variables. Ooi and Le (2013) and Zahirovich-Herbert and Gibler (2014) find an approximately 2% increase in house prices due to new housing supply in Singapore and Baton Rouge (Louisiana), respectively. My findings are more comparable to Schwartz et al. (2006) who finds that subsidized housing investments lead to substantial spillovers, 9% increase in surrounding properties, in New York. The externalities that I find are slightly lower compared to Diamond and McQuade (2019) who finds that new affordable housing increases house prices of the nearby stock by 6.5% in low-income neighborhoods.

I perform a number of robustness checks that are presented in Table B.2 in the

Online Appendix. First, I add fixed effects at different geographic levels (columns 2 and 3) to check whether results could be capturing unobserved time-varying trends that may vary with the distance to the border.[17] Second, I extend the area of analysis from a one to a two-kilometer buffer zone around the southern LVS border (columns 4 to 6).With this extension the area of analysis comprises a larger part of the LVS area and the entire unsubsidized area $U$ that is bounded by the river. Figure B.15 in the Online Appendix shows the event study graph for this area of analysis. Third, I estimate externalities on house prices from using an intensity measure only based on projects that started between 2011 and 2014 (columns 7 to 9).[18] Fourth, I test the sensitivity of results to the use of an exponential weighting scheme instead of an inverse distance decaying function. In this case, the weighting function is defined as $\omega(d_{ij}) = e^{-\lambda d_{ij}}$, where $\lambda(> 0)$ is the parameter that governs the decaying rate of weights.[19] As in Autor, Palmer and Pathak (2014, 2017), estimates are presented for a range $\lambda$'s in Table B.3 in the Online Appendix. Finally, I construct a new exposure measure that considers both LVS and non-LVS housing units developed between

[17]Column 2 considers 'census section' fixed effects. Census sections are the next level of aggregation after census tracts, defined as a group of tracts. Then, this specification not only uses variation within border by year but also within census sections. Column 3 includes census tract fixed effects. This geographical unit is relatively small, as observed in the Online Appendix B.9, and then, transactions within tracts are highly comparable on unobserved dimensions. Figure B.14 in the Online Appendix shows the event study graph for this specification.

[18]The majority of LVS projects were ready between 2014-2015. However, LVS projects that started in 2017 and 2018 were not finished by 2018. Then, I construct the variable Intense only using 148 projects that started between 2011 and 2014, with around three quarters being completed by 2015.

[19]The exponential decay function also places larger weights on nearby LVS projects. A larger $\lambda$ implies a faster decline rate in the level of exposure, so in this case, this measure depends on the choice of this parameter. To guide the selection of the preferred estimate, I search for the value of $\lambda$ that maximizes the regression fit. First, I compute the sum of squared residuals that resulted from using intensity measures with different decaying rates (labeled as the 'in-sample' exercise). Second, I perform out-of-the sample predictions using random samples of half the actual size. The procedure involves five steps: 1) take a random sample of half the actual size; 2) estimate the model for a given $\lambda$; 3) perform an out-of-the sample prediction and compute the sum of the squared errors (SSE); 4) repeat steps 1 to 3 200 times; 5) perform steps 1 to 4 for another value of $\lambda$. For each $\lambda$, I compute the average SSE. Results from these two exercises are reported in Figure B.17 in the Online Appendix, and they both indicate that $\lambda = 6$ maximizes (minimizes) the regression fit (the sum of squared residuals). For $\lambda = 6$, the estimated effect on house prices is almost 15%.

2004 and 2018.[20] Estimates are reported in Table B.4 in the Online Appendix.[21]

Overall estimates are all statistically significant across different empirical exercises, with estimates ranging from 9% to almost 15%.

I also perform a non-parametric DiD to check for pre-trends at different distances from the border. Specifically, I estimate a nonparametric function of the distance to the border before-and-after the introduction of the policy using the semiparametric technics developed in Yatchew (1997); Yatchew and No (2001).[22] This method is similar to the ring regression approach used in previous literature, but instead of using rings surrounding projects, I focus on estimating house price effects as a function of the distance to the LVS border since the spatial concentration of LVS projects near the border complicates the definition of the rings. The results are presented in Figure A.1 in the Appendix. The distance is normalized to zero at the border. Negative values correspond to locations in the unsubsidized area ($S$) and positive values to locations in the LVS area. The solid line represents the nonparametric estimates, and the dashed lines are 95% confidence intervals. Panel a reports the estimates obtained from using the pre-policy period - i.e., I compare a three-year period around the beginning of the policy with the first three years in my sample -. Results from this exercise lead to a flat line and then, with magnitudes being close to zero and not statistically significant in any case. The absence of house price effects at any distance to the boundary provides evidence supporting the parallel trends assumption.

[20]This new measure varies across time, and it is defined as the cumulative sum of nearby new housing that a given transaction $i$ is exposed to. Formally, $ND_{it} = \sum_{k=2004}^{t} \sum_j H_{jk}\omega(d_{ij})$ where $H_{jk}$ is the cumulative sum of new housing units at year $t$ multiplied by a weighting function that depends on the distance from the existing housing $i$ to the new housing $j$. The change in $ND$ between $t-1$ and $t$ gives the number of new housing units developed at time $t$. Then, I regress the log of house prices on the log of exposure to new housing ($ND_{it}$) and the set of controls previously used. As before, I use the interaction of the variable $Treat_i$ with $post_t$ as an instrument for $ND_{it}$. In that sense, the policy acts as a housing supply shifter in the LVS area.

[21]Columns 1 to 3 consider an inverse distance decaying function (i.e., $\omega(d_{ij}) = 1/d_{ij}$). Columns 4 to 6 are based on new housing units within 250 meters (i.e., $\omega(d_{ij}) = \mathbb{1}\{d_{ij} \leq 250m\}$), while columns 7 to 9 within 500 meters (i.e., $\omega(d_{ij}) = \mathbb{1}\{d_{ij} \leq 500m\}$). Panel a presents the results of the first stage that shows the effect of the LVS policy on new housing developments ($ND_{it}$) for the different alternatives. Panel b presents the results for the IV estimates. The summary of this exercise is as follows: the LVS policy increases new residential construction in the LVS area. The induced changes in the spatial distribution of new construction impacted the prices of existing housing units. Once again, the evidence presented here supports the view that new construction leads to localized price effects.

[22]The estimated function is normalized to the average effect for the control band, defined as distances of more than 400 meters away from the border. The parametric part includes all the controls included as before (i.e. housing characteristics, border-year fixed effects, 2011 census variables, and tract-level pre-trends in house prices).

Panel b presents the estimates after the introduction of the policy. It shows positive house price effects that increase when approaching the border (coming from the non-treated area $U$), becoming statistically significant after 200 meters to the border and thereafter. Externalities are more pronounced within the LVS area and fall rapidly as one moves away from locations in the subsidized area. Estimates in the LVS area indicates an around 11% increase in house prices of the existing stock.

Finally, I carry out a placebo exercise that compares house prices across one simulated boundary (labeled as the 'placebo border') before and after the policy comes into force.[23] The idea of this exercise is to validate the empirical strategy used using an environment that is not or less exposed to the LVS policy. If unobservable time-varying trends drive the baseline estimates, we should also observe price effects as the resulting output of this placebo exercise. Alternatively, finding no effects on house prices provide supporting evidence on the identification strategy used. Results are presented in Table B.5 in the Online Appendix. Columns 1 to 3 use transactions within 500 meters, columns 4 to 6 within 1 kilometer, and columns 7 to 9 within 1.5 kilometers of the placebo border. Overall, estimates are close to zero across columns, being not statistically significant in any case. Figure B.21 in the Online Appendix shows the event study graph for each buffer zone. These results imply that housing located in neighborhoods less or not exposed to the policy does not experience house prices changes, thus validating the identifying strategy used.

## 6.2 Heterogeneity

Previous literature finds that spillovers on house prices vary with pre-socioeconomic and urban infrastructure conditions. Analyzing affordable housing, Baum-Snow and Marion (2009) and Diamond and McQuade (2019) show that house price effects are more prominent in lower-income and declining neighborhoods, and more strikingly, they observe negative price effects on higher-income and gentrifying neighborhoods.

[23]The 'placebo border' is constructed by shifting the original southern border of the LVS area (the $S-U$ border) such that it crosses the centroid of the LVS area. Figure B.20 in the Online Appendix shows the placebo border. Since LVS projects are mainly concentrated in the bottom part of the LVS area, housing units around the placebo border are not highly exposed to the policy or unless similarly exposed. It is expected to observe an absence of price effects when comparing transactions from above with transaction from below the placebo boundary. So, I define the binary variable $Placebo_i$ that takes the value of one for units located above the placebo border while zero if located below. I replace the variable $ln(\text{Intense}_i)$ by $Placebo_i$ in equation 2 and run a number of regressions using observations in buffer zones with different radius.

The LVS program induced the construction of above-market-rate housing in neighborhoods with different pre-policy characteristics. Adding LVS units to locations with better initial conditions may not result in more significant spillovers. Conversely, price effects may be larger in treated neighorhoods with initially worse conditions. Then, I explore potentially heterogeneous treatment effects by examining whether price effects are different along with a range of socioeconomic and urban infrastructure neighborhood characteristics from the 2011 census.

To do so, I interact the exposure measure (Intense) with different neighborhood's attributes one at a time and incorporate the interactions together with main effects into equation 2. Neighborhood characteristics ($k$) are measured through tract-level continuous variables denoted by $c_i^k$ that were discretized by using their median value (i.e., $\mathbb{1}\{c_i^k \geq p50\text{th}\}$). These regressions allow testing whether the coefficient $\beta$ vary with neighborhood pre-LVS characteristics. I consider a set of socioeconomic characteristics (e.g., % of renters, % of vacant dwellings, % of low-educated head of households, unemployment rate) that are related to the socioeconomic status of neighborhood as in previous research. Furthermore, I use variables linked to the urban infrastructure of neighborhoods (e.g., % of buildings in poor condition, average quality of dwellings, average street quality index, pre-trends in house prices).

Results are presented in Table 4. In each column, the specification with the highest number of control variables is used (column 3 in Table 3). In the first three columns of Table 4, I focus on heterogenous effects across socioeconomic characteristics of neighborhoods. A quick look across estimates suggests that price effects vary along with some of the tract-level dimensions, such as % of vacant dwellings or % of low-educated head of households. I formally test whether slopes are indeed different, but I do not reject the null of coefficients being equal in any case. This result is likely to be explained by the high correlation between the intensity of exposure and socioeconomic characteristics (see Figure B.22 in the Online Appendix). In the next four columns (5 to 8), I consider urban infrastructure variables. Results tend to indicate that spillovers are higher in neighborhoods with worse initial conditions. Only column 5 and 8 show slopes being statistically different. In column 5, we observe that price effects are larger in neighborhoods with a higher proportion of buildings in poor condition. Column 8 shows more significant externalities in locations with an increase in pre-policy prices lower than the median. To sum up, results in this section indicates that price effects are larger whenever above-market-rate housing is added to neighborhoods with lower pre-levels of infrastructure, a somehow intuitive result.

Table 4: Heterogeneous analyses. House price effects.

| $c_i^k, k =$ | Socioeconomic characteristics | | | | Urban infrastructure | | | |
|---|---|---|---|---|---|---|---|---|
| | % of renters (1) | % of vacant dwellings (2) | % of low-edu. head of hhs (3) | Unemp. rate (4) | % of buildings in poor condition (5) | Avg. quality of dwellings (6) | Street quality index (7) | Pre-trends house prices (8) |
| (1) $ln$(Intense) $\times \mathbb{1}\{c_i^k \geq p50\text{th}\} \times$ post | 0.171*** | 0.215** | 0.149** | 0.160** | 0.271*** | 0.146** | 0.173** | 0.123** |
| | (0.052) | (0.084) | (0.060) | (0.063) | (0.081) | (0.060) | (0.080) | (0.051) |
| (2) $ln$(Intense) $\times \mathbb{1}\{c_i^k < p50\text{th}\} \times$ post | 0.163* | 0.184 | 0.183*** | 0.102** | 0.145*** | 0.176*** | 0.133*** | 0.252*** |
| | (0.083) | (0.125) | (0.060) | (0.045) | (0.053) | (0.058) | (0.049) | (0.077) |
| First-stage F-stat | 12.693 | 6.845 | 17.503 | 15.029 | 17.853 | 24.492 | 10.350 | 16.663 |
| p-value: (1)=(2) | 0.892 | 0.844 | 0.508 | 0.238 | 0.026 | 0.542 | 0.578 | 0.023 |

*Notes:* Number of observations = 70,422, Number of clusters = 251. All regressions include housing characteristics as described in section 4.2 and border-year fixed effects $\delta_{bt}$. Standard errors are clustered at the tract level. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

# 7 Mechanisms

## 7.1 Effects on household income

Next, I explore the effect of increasing housing supply on the neighborhood income mix. I focus on three outcomes, disposable household income per capita with and without rental value and the rental value itself. The data comes from the National Household survey. This survey contains census tract identifiers that allow merging it with a tract-level measure of exposure to the LVS policy. Then, I test whether household income and rental values increased in highly exposed tracts.

Figure 4 reports the yearly estimates relative to 2011 and for the three considered outcomes. Panel a shows that the estimated coefficients are close to zero and not statistically significant before 2011. Two years after the introduction of the policy, estimates are still close to zero. After 2014, a break in the trend is observed, with most of the estimates being above zero and becoming statistically significant.[24] Part of the effects is attributed to the new dwellers of the LVS units. Panel b presents the event study graph for household income without rental value. Overall, a similar

[24]The National Statistics Office reported that the household income from the 2016 National Household Survey was subject to some corrections after identifying missing observations. For that year, effects on household income (with and without rental value) are close to zero and not statistically significant. On the other hand, estimates are positive and statistically significant for rental values. Then, I do not report the effect on that specific year as estimates are still likely to be affected by the problem mentioned above.

pattern is observed, but in this case, the effects are smaller. Panel c shows sizeable effects on rental values from 2015 onwards. These results indicate that effects are partly driven by an increase in the rental value in areas with high exposure to the policy.

Table 5 reports the IV and ITT estimates for two subperiod, 2012-2015 and 2016-2019. Column 1 presents the results obtained from using the log of the disposable household income with rental value as for the dependent variable. Income initially increases by 6% in neighborhood exposed to the LVS policy. The effect becomes four percentage point larger after 2015 onwards. As observed in previous graphs, the estimated effect is smaller when using the household without rental value as the dependent variable (column 2). The last column (3) provides evidence that the increase in total disposable income is partly explained by the rise in rental values in affected neighborhoods, as it displays larger estimated coefficients. Across columns, effects are larger in the longer run.

In short, this result confirms the arrival of more affluent residents to treated neighborhoods, another factor that pushes housing demand upwards.

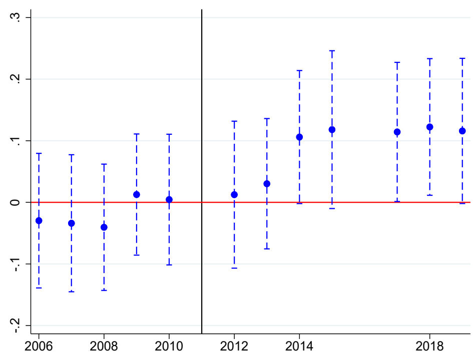
Table 5: Effects on household income and rental value.

| | Household income PC | | |
|---|---|---|---|
| | w/ rental value | w/o rental value | Rental value |
| | (1) | (2) | (3) |
| **a) Continuous exposure measure** | | | |
| $ln$(Intense) × $\text{post}_{2012-2015}$ | 0.080*** | 0.068** | 0.170*** |
| | (0.027) | (0.028) | (0.036) |
| $ln$(Intense) × $\text{post}_{2016-2019}$ | 0.133*** | 0.098*** | 0.281*** |
| | (0.032) | (0.031) | (0.051) |
| F-statistics | 27.361 | 27.326 | 26.567 |
| **b) Binary exposure measure** | | | |
| Treat × $\text{post}_{2012-2015}$ | 0.044*** | 0.037** | 0.094*** |
| | (0.014) | (0.015) | (0.017) |
| Treat × $\text{post}_{2016-2019}$ | 0.077*** | 0.056*** | 0.164*** |
| | (0.017) | (0.018) | (0.021) |
| Adj. $R^2$ | 0.187 | 0.164 | 0.317 |
| *Observations* | 61,064 | 61,016 | 42,350 |
| Nbr. of clusters | 236 | 236 | 236 |

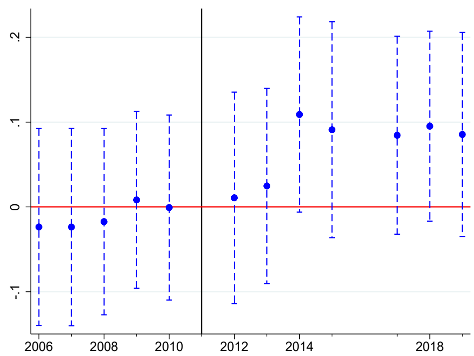
*Notes:* the dependent variable used are: the log of the disposable household income per capita with rental value (column 1); without rental value (column 2) and; the log of the rental value (column 3). All regressions include year-month dummies, border-year fixed effects $\delta_{bt}$, and 2011 census controls. Clustered standard errors at the tract level are used. *** significant at the 1% level. ** significant at the 5% level. * significant at the 10% level.

Figure 4: Effects on household income and rental value.

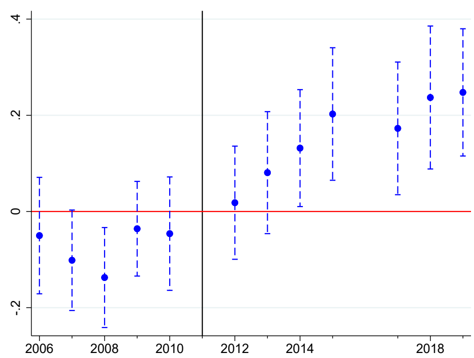
(a) Household income PC w/ rental value




(b) Household income PC w/o rental value




(c) Rental value




*Notes*: the dependent variable used are: the log of the disposable household income per capita with rental value (panel a); without rental value (panel b) and; the log of the rental value (panel c). All regressions include year-month dummies, border-year fixed effects $\delta_{bt}$, and 2011 census controls. Vertical segments correspond to 95% confidence intervals constructed using clustered standard errors at the tract level.

## 7.2 Crime effects

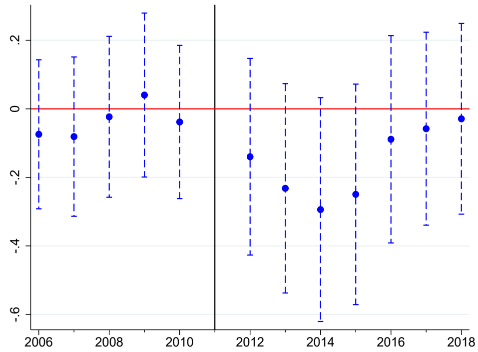
The level of local criminal activity represents one disamenity that is likely to affect residential externalities. Crime rates may be reduced as unoccupied homes or vacant lots are replaced with new housing, representing one driver of the house price effects. However, there may also be mediating effects that operate in the opposite direction. As new residents moved into their new homes, treated neighborhoods get denser, possibly offsetting the early reductions in criminal activity. In fact, I observe a high correlation between offenses and density in my dataset (see Figure B.23 in the Online Appendix). These two effects are likely to operate at different time scales. The first should be observed right after housing projects start, while the second is expected to show up by the time new residents move in. Since most of the projects involved replacing abandoned buildings or disused factories with new housing, the LVS policy offers an ideal setting to test these countervailing effects.

I test whether LVS residential investments affect crime patterns using the ITT and IV strategy used before. As explained in Section 4, the analysis employs a yearly panel of census tracts from 2006 to 2018. Three different outcomes are used, total crime, property crime (defined as theft and robbery), and non-property crime (defined as the difference between total and property crime). More specifically, for each outcome, I work with the log of the yearly tract-level counts.

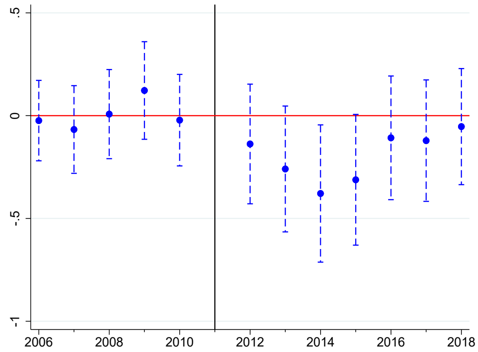
I begin presenting the evidence through event study graphs to check for pre-trends as well as the dynamic effects on crime. Figure 5 shows the yearly estimates (relative to 2011) obtained from interacting ln(Intense) with year dummies. For the three crime outcomes, it is reassuring that tracts with higher exposure to LVS projects do not seem to have been on differential paths before the introduction of the policy. The top panel suggests a reduction in total crime in the years following the introduction of the policy. Crime rates seem to revert to pre-policy levels after 2015, coinciding with the period of high sale volume of LVS units. The evidence reveals a similar pattern for property crime (mid-panel), but in this case, the reduction is larger and statistically significant for a couple of years. These seem to be short-term effects as estimated coefficients are close to zero after 2015. The bottom panel suggests a mild impact on nonproperty crime.

Figure 5: Event-study graph: criminal activity.

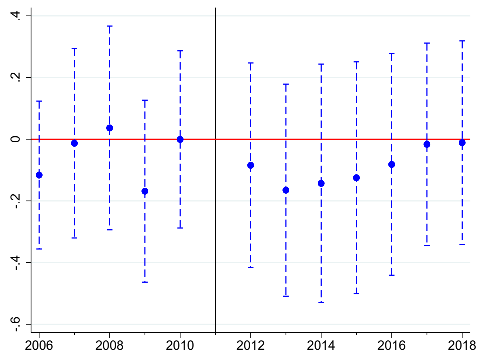
(a) All types




(b) Property crime




(c) Non-property crime




*Notes*: The dependent variable used is the log of the yearly count of reported crimes at the tract level. All regressions include year dummies, border-year fixed effects $\delta_{bt}$, 2011 census controls, initial crime level, and weighted by the 2011 tract-level population from the census. Vertical segments correspond to 95% confidence intervals constructed using clustered standard errors at the tract level.

Table 6 reports the IV and ITT estimates.[25] Columns 1 and 2 in panel a indicate

[25] I interact the exposure measure (Intense) with $\text{post}_{2012-2015}$ and $\text{post}_{2016-2018}$ a pair of binary variables. For each outcome, two specifications were considered. The first includes border-year fixed effects and 2011 census controls, and the second also adds initial crime levels to control for mean reversion.

an estimated elasticity of intensity of exposure to crime rates of around -.21 between 2012 and 2015 (first row), but which is not statistically significant. The estimated elasticity boils down to zero within 2016-2018 (second row). The effects on property crime rates seem larger. Using the estimate in column 4 first row (-.268), a one standard deviation increase in the exposure (.702) leads to an almost 19% decrease in property crime between 2012 and 2015. Effects are non-negligible in the case of non-property crime, but estimates are not statistically significant. Similar results are found for ITT estimates in panel b (i.e., sizeable initial crime reductions followed by estimates that are much closer to zero by the end of the period). I test the sensitivity of the results in Table B.6 in the Online Appendix.[26]

The evidence suggests an initial reduction in crime rates in neighborhoods with higher exposure to the LVS policy associated with the removal of crime hubs. But these effects dissipated after 2015, coinciding with the arrival of more affluent residents to the affected neighborhoods.[27] Then, it may be that the early effects on crime were counterbalanced by the increase in the number of higher-income dwellers who drive up the expected economic benefits of committing a crime.[28] Nevertheless, results seem suggestive considering the size of confidence intervals that indicates that estimates lack accuracy.

[26]In the first column, I replace 2011 census variables with census tract fixed effects. In the second column, neighborhood-year effects are used instead of border-year effects. There are 62 neighborhoods in Montevideo, and the police use this geographical definition to target patrolling areas. Then, the neighborhood-year (NE-year) effects may allow accounting for changes in police deployment and patrolling. Results are similar except that estimates are much larger in the regression that uses NE-year effects instead of border-year effects. Figure B.24 in the Online Appendix shows the event study graph when adding NE-year effects. Finally, in Table B.7, I analyze whether the effect on property crime rates is driven by crimes committed against persons outdoors or in their property rather than in businesses, cars or public services (e.g., public transport). In the short run, I observe a larger reduction (22-25%) in property crime committed against persons outdoors or within their property than in total property crime (19-20% reduction). The opposite is found in property crimes committed in businesses, vehicles, and public infrastructure. The estimates for this category (columns 3 and 4) show an initial decrease in crime rates of 6-9%.

[27]Autor, Palmer and Pathak (2017) find a reduction in crime rates that ranges from 7-15% due to gentrification induced by rent deregulations. They also find a similar pattern on crime activity when analyzing the dynamic effects of rent deregulation.

[28]In Figure B.25 and B.26 in the Online Appendix, I present the relationship between changes in house prices and property crime rates for two periods, 2011-2014 and 2011-2018. Interestingly, in the first period, a negative relationship between house prices and crime is observed, while for the second period, the linear fit shows an upward sloping relationship. This visual evidence is in line with the reduced form estimates. Changes in the income mix of affected neighborhoods seem to mediate the relationship between prices and crime through the second period.

Table 6: IV and ITT estimates. Effects on crime records.

| | All type | | Property crime | | Non-property crime | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **a) IV estimates** | | | | | | |
| ln(Intense) × $\text{post}_{2012-2015}$ | -0.203 | -0.215 | -0.279 | -0.268* | -0.071 | -0.113 |
| | (0.189) | (0.145) | (0.188) | (0.145) | (0.186) | (0.156) |
| ln(Intense) × $\text{post}_{2016-2018}$ | -0.033 | -0.045 | -0.101 | -0.091 | 0.025 | -0.018 |
| | (0.176) | (0.135) | (0.173) | (0.136) | (0.173) | (0.146) |
| F-statistics | 28.590 | 28.382 | 28.590 | 28.495 | 28.590 | 28.309 |
| **b) ITT estimates** | | | | | | |
| Treat × $\text{post}_{2012-2015}$ | -0.117 | -0.118 | -0.157 | -0.146* | -0.044 | -0.061 |
| | (0.099) | (0.075) | (0.097) | (0.075) | (0.099) | (0.083) |
| Treat × $\text{post}_{2016-2018}$ | -0.021 | -0.022 | -0.057 | -0.046 | 0.008 | -0.009 |
| | (0.094) | (0.073) | (0.092) | (0.073) | (0.092) | (0.078) |
| Adj. $R^2$ | 0.221 | 0.643 | 0.213 | 0.635 | 0.268 | 0.576 |
| 2011 census controls | Y | Y | Y | Y | Y | Y |
| Initial crime level | N | Y | N | Y | N | Y |

*Notes:* Number of observations = 2,834, Number of clusters = 218. The dependent variable used is the log of the yearly count of reported crimes at the tract level. All regressions include year dummies, border-year fixed effects $\delta_{bt}$, and are weighted by the 2011 tract-level population from the census. Standard errors are clustered at the tract level. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

# 8 Conclusions

This paper estimates the local effects of residential developments on house prices. The increase in housing supply comes from tax breaks applied in a spatially defined middle-income area within Montevideo, Uruguay, resulting in significant investments in the housing sector. I find evidence of a substantial increase in housing prices surrounding the subsidized investments, with residential externalities being highly local. Specifically, house prices increase by 12%, and externalities tend to vanish after 200 meters. These findings are in contrast to previous evidence on spillovers from market-rate housing (Ooi and Le, 2013; Zahirovich-Herbert and Gibler, 2014) and affordable housing in low-income neighborhoods (Diamond and McQuade, 2019), but are in line with estimates from quantitative spatial models such as in Ahlfeldt et al. (2015). Also, the estimated spillovers vary with some initial neighborhood characteristics related to the quality of the existing stock.

I analyzed two potential drivers of these externalities were: the neighborhood income composition and criminal activity. Evidence shows household income increases in neighborhoods exposed to the policy, primarily when a high volume of LVS units is sold. Moreover, the new housing supply initially seems to decrease property crime rates, but the estimated effects lack precision. Altogether, the results of this paper enhance the role of (endogenous) amenities in the determination of local house prices. However, note other channels may exist, such as a quality increase in the housing provided (Ahlfeldt, Maennig and Richter, 2017; Koster and Van Ommeren, 2019).

Finally, the findings also indicate the new housing supply contributed to revitalizing some middle-income areas of the city. In this sense, such policies can be justified on that basis. Note the provision of affordable housing was also one of the aims of this legislation. However, little has been achieved, due to the absence of any rules targeting new housing developments to more vulnerable households. As such, these findings highlight the apparent trade-off between inducing rapid urban revitalization and making neighborhoods more affordable.

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# A Appendix

## Robustness checks

Table A.1: Raw vs trimmed price data. IV estimates.

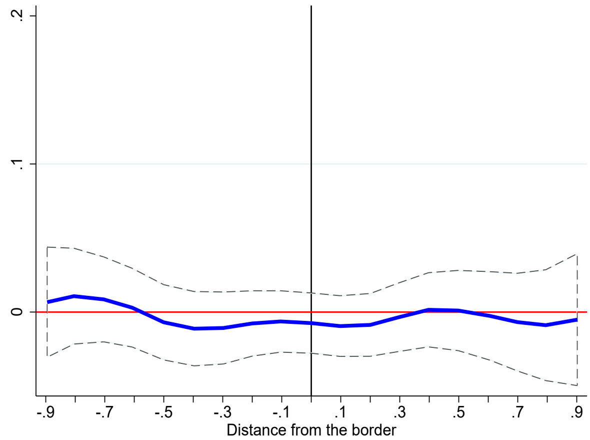
| | (1) | (2) | (3) |
|---|---|---|---|
| **a) Raw** | | | |
| ln(Intense) × post | 0.189** | 0.141** | 0.135** |
| | (0.078) | (0.065) | (0.062) |
| First-stage F-stat | 54.327 | 55.139 | 55.408 |
| Observations | | 71,093 | |
| **b) Trimmed: 1st top/bottom pc** | | | |
| ln(Intense) × post | 0.225*** | 0.174*** | 0.169*** |
| | (0.070) | (0.057) | (0.054) |
| First-stage F-stat | 54.569 | 55.555 | 55.834 |
| Observations | | 70,422 | |
| **c) Trimmed: 5th top/bottom pc** | | | |
| ln(Intense) × post | 0.182*** | 0.137*** | 0.138*** |
| | (0.060) | (0.049) | (0.049) |
| First-stage F-stat | 55.246 | 56.408 | 56.429 |
| Observations | | 68,507 | |
| 2011 census controls | N | Y | Y |
| $\Delta$ house prices $_{\text{pre-policy}}$, tract level | N | N | Y |
| Number of clusters | 251 | 251 | 251 |

*Notes:* All regressions include housing characteristics as described in section 4.2 and border-year fixed effects $\delta_{bt}$. Standard errors are clustered at the tract level. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

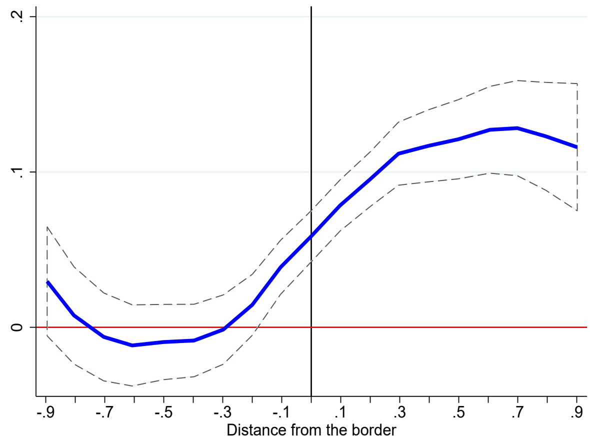
**Non-parametric DiD**

Figure A.1: House price effects. Nonparametric estimates.

**(a)** Pre-intervention




**(b)** Post-intervention




*Notes*: The solid line represents the estimated price effects obtained as a nonparametric function of the distance to the border using the approach developed by Yatchew (1997); Yatchew and No (2001). Dashed lines represents 95% confidence intervals constructed using bootstrap procedure with 500 replications.