ISSN 2042-2695

CENTRE *for* ECONOMIC PERFORMANCE

CEP Discussion Paper No 1660

November 2019

Spillover Effects from New Housing Supply

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ESRC ECONOMIC & SOCIAL RESEARCH COUNCIL

Abstract
I estimate spillovers from new housing supply on house prices, crime rates, and household income. 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. The program incentivized residential development through tax breaks that led to sizable investments in certain neighborhoods. I exploit the spatial structure of the scheme to identify the externalities and find clear evidence of spillovers from new supply on house prices, with prices increasing between 12 and 17%. Property crime rates only decreased in the short term, while there is evidence of an increase in household income, suggesting that the neighborhood income mix responded to the supply expansion. Increasing supply appears to revitalize neighborhoods, but these effects also reduce housing affordability.

Key words: housing supply, house prices, neighborhood change, crime, difference-in-differences, housing policies
JEL Codes: R23; R30; R58

This paper was produced as part of the Centre's Urban Programme. The Centre for Economic Performance is financed by the Economic and Social Research Council.

I am grateful for the comments received from Elisabet Viladecans-Marsal, Jordi Jofre-Monseny, Laurent Gobillon, Miren Lafourcade, Henry Overman, Gilles Duranton, Felipe Carozzi, Gabriel Ahlfeldt, Camille Hémet, Jorge de la Roca, Javier Vázquez-Grenno, Amedeo Piolatto, Dirk Foremny, Guillaume Chapelle, Jan Stuhler, and participants at the l'X/UEA Summer School in Urban Economics (Paris), UEA (Copenhagen), IEB seminars, the Public Policies, Cities and Regions workshop (Lyon), the 13th meeting of the UEA (New York), and the CEP Annual Conference 2019.

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Published by
Centre for Economic Performance
London School of Economics and Political Science
Houghton Street
London WC2A 2AE

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1. Introduction

Increasing affordability problems in cities across the developed world have often been met with calls to increase the supply response and increase building output. For example, a large debate has emerged around the stringency of regulatory restrictions on new development, with several studies indicating that tight planning constraints in some US cities have led to increases in housing prices (Glaeser and Gyourko, 2009, 2018). Nevertheless, this enthusiasm for increased supply at the city level often overlooks the local effects of new housing stock. Within a city, the spatial distribution of these new dwellings may have important implications for neighborhood change. New supply may generate spillovers on the nearby existing housing stock. First, 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). Second, households moving into the new dwellings are seen as an amenity by their neighbors, provided they are of a higher (or at least similar) socioeconomic status (Diamond and McQuade, 2019). This channel may be reinforced if other higher-income residents are attracted to 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, then housing prices are predicted to increase locally even if increased supply reduces prices at the city level. Thus, to incorporate the full impact of new housing on our neighbors and cities, it is important to obtain causal estimates of external effects and analyze how they extend across space.

In this paper, I estimate spillovers resulting from new housing developments on three different outcomes. First, I study spillovers on the price of existing properties located next to the new housing stock. Second, I estimate the effects of new supply on local crime rates. Crime represents a neighborhood (dis)amenity, and there is substantial evidence of a causal link between crime rates and house prices (Gibbons, 2004; Pope, 2008; Linden and Rockoff, 2008; Ihlanfeldt and Mayock, 2010). New housing supply can reduce the number of vacant lots and abandoned structures that may be linked to criminal activities (Spelman, 1993; Ellen, Lacoe and Sharygin, 2013; Cui and Walsh, 2015). New units may also attract more affluent residents who are less likely to engage in criminal activities and more prone to invest in security measures (i.e., alarms, cameras, etc., Farrell et al. (2011)). On the other hand, the level of income is positively related to the number of property crimes (Sampson, Raudenbush and Earls, 1997). Ultimately, which effect prevails remains an empirical question, which I will answer below. Finally, I estimate the effect of residential investment on local household incomes, as the new supply may induce changes in the neighborhood composition (Brueckner and Rosenthal, 2009; Rosenthal and Ross, 2015).

In my empirical strategy, I make use of exogenous variation in the spatial distribution of residential development induced by a housing policy implemented in the city of Montevideo, Uruguay starting in 2011.[1] The program consisted of a series of tax breaks to developers and private investors aimed at promoting 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.[2] The borders of the subsidized (or targeted) area follow a number of city divisions provided by the main avenues and streets, without following the boundaries of any administrative area.[3] There is no official document describing the criteria used to select the subsidized area, but its design reveals the intention of excluding high-income areas of the city, where most of the private residential investments had taken place in the past.[4] Investments carried out under this program were sizable, totaling 1.5% of the country's GDP. There were no explicit rules regarding the socioeconomic characteristics of buyers or tenants of newly built units, and developers ended up producing housing affordable for middle-high/high-income households.

As will be shown below, the policy pushed new construction into the targeted area, especially to those neighborhoods located close to the spatial boundary. My analysis uses 98 of these large housing projects (approximately 3.3M USD per project) executed between 2011 and 2013 to study the spatial spillovers of new housing stock on house prices, crime rates and neighborhood composition. To estimate these effects, I construct a dataset combining information on the individual projects with data on the universe of housing transactions, daily crime records and survey data on household incomes.

Estimating spillovers is challenging because new developments may be located in areas with high expectations of future price growth. 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. My identifying assumption requires

[1] The Department of Montevideo has 1.3M inhabitants, while its metropolitan area houses 1.9M, making it the largest urban area in the country.

[2] Subsidized areas were set by the Ministry of Housing in conjunction with the Ministry of Economics and Finance and the Local Government of Montevideo. In these areas, tax breaks applied to housing projects, which implied the production of a number of new homes to sell or rent.

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

[4] It also excludes the suburbs and other areas where most of the slums are located, which are hardly targeted by developers.

that developers are not able to manipulate the borders of the subsidized areas in response to expected changes in prices or other neighborhood characteristics. In my analysis, I use estimates from a boundary discontinuity design (BDD) to show that while average housing prices are higher in subsidized areas, pretreatment prices are smooth at the policy boundary, suggesting that differences in housing prices did not dictate the definition of the treated area. This also indicates that there are no major cross-border differences in the level of initial local amenities. In addition, I report that no discontinuities in housing prices of existing units emerge after the law came into force, which is likely to be explained by external effects spilling into nontreated areas. These spillovers rule out the application of a BDD (as in Black (1999); Dell (2010)) to measure causal effects.

My main estimates are obtained using a difference-in-differences (DiD) strategy exploiting house price changes of unsubsidized sold units in different narrowly defined bands around the treatment border before and after the policy. To avoid contamination by the spillovers from new development, the control group comprises a set of properties in an unsubsidized area located over 400 meters from the border. Because not all of the area targeted by the policy experiences an increase in housing supply, this analysis yields intention-to-treat estimates. Alternatively, I can use the realized spatial pattern of new developments to estimate the effect of externalities using a continuous difference-in-differences estimator. For this purpose, I construct a measure that captures the investment exposure of each existing housing unit to subsidized projects by computing the weighted sum of all nearby investments using exponential distance decay weights. Since the investment intensity variable can be endogenous, I instrument this measure using a binary indicator that takes a value of one for units located in the subsidized area, thus exploiting the place-based nature of the subsidy.

I find evidence of substantial – but highly local – spillover effects of new residential development.[5] DiD estimates using bands around the policy area boundary show that house prices increase by approximately 12% close to the border in the treated locations. External effects vanish after roughly two hundred meters from the border on the unsubsidized side. Based on the continuous DiD strategy, I observe a 15-17% increase in house prices in areas with high intensity of subsidized investments. Estimates of price effects are heterogeneous with respect to neighborhood characteristics. In census tracts with a high share of renters, I find a consider-

[5]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.

ably larger impact of new developments on housing prices, which may be explained by a higher turnover allowing for a faster gentrification process. Additional estimates indicate a significant effect of new developments on property crime, showing a reduction ranging between 20% and 30%. My event-study graphs show that this reduction is temporary, suggesting that it is a result of the removal of vacant/disused structures that were replaced by the new developments. Finally, the results show an increase in household income per capita.

This paper contributes to the scarce literature analyzing the effect of new housing supply on neighborhoods and housing markets. Ooi and Le (2013) and Zahirovich-Herbert and Gibler (2014) analyze spillovers on local housing prices from new residential construction using a ring regression approach, which hardly mitigates endogeneity concerns due to developers' investment location decisions. Based on simulated evidence from a neighborhood choice model, Anenberg and Kung (2018) find that 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. Boustan et al. (2019) analyze whether a higher density of condominiums attracts high-income residents to estimate whether supply-side factors can drive gentrification in urban cores. Instead, I focus on estimating 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. Specifically, a large number of studies have investigated the effect of the Low Income Housing Tax Credit (LIHTC). Spillovers from developments built under this program are more likely to result from changes in neighborhood composition, rather than from externalities associated with the physical characteristics of dwellings. 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) 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, finding 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 of my paper relative to this literature is that the new developments studied here are not targeted to low-income households. Therefore, my results may be better suited to understanding 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, 2016; Koster and Van Ommeren, 2018), which find mild to moderate evidence of housing externalities and very localized effects.[6] 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.

The rest of this paper is organized as follows. In section 2, I describe the housing policy. In section 3, I develop a simple theoretical framework. In section 4, I describe the data. In section 5, I outline the empirical method used. In section 6, the results are presented, and finally, I conclude in section 7.

2. A subsidy for private residential housing investors

In August 2011, the Uruguayan government introduced tax breaks for private investments in housing (Law Nbr. 18,975).[7] The policy aimed at incentivizing the construction sector, as well as improving the stock of housing for both sale and rent. It does not impose requirements regarding the characteristics of buyers and tenants; subsequently, developers mainly produced housing that was just affordable for middle-high/high-income households.[8] Tax benefits apply to new construction and rehabilitation projects, with 73% of all projects being new builds.[9] New construction projects had substantially larger (on average, 11 times) budgets than rehabilitation projects, which overall had low budgets. In my empirical analysis, I only focus on new con-

[6]By one half every 1,000 feet in Owens, Rossi-Hansberg and Sarte (2010). Koster and Van Ommeren (2018) find mild evidence of spatial externalities. Gabriel M. Ahlfeldt, Wolfgang Maennig and Feliz J. Richter (2016) find no evidence in favor of the housing externality hypothesis, as house prices in the area surrounding the intervention do not exhibit a relative increase in price.

[7]Projects are allowed to include a small number of commercial units (up to 10% of the total produced units), but no tax benefits are given for these units.

[8]In Montevideo, the average price (in m^2) of a subsidized unit is approximately 2,700 USD, while the average for the city is 1,896 USD, and the average in the case of high-income neighborhoods is 2,591 USD. There were price caps for a portion of units of projects located in some areas of the city, but these price limits were not binding. Since concerns were expressed about the prices of subsidized units, in 2017, the government set lower price caps to improve affordability.

[9]Rehabilitation projects involve upgrading, in addition to increasing the total number of housing units in deteriorated properties, usually semidetached houses.

struction developed under this policy.

Projects were required to produce at least two and up to a maximum of 100 new residential units per land lot, thus subsidizing the construction of semidetached houses or flats. The upper limit requirement did not apply for projects undertaken on large vacant lots or lots containing disused factories or abandoned homes. There were also size requirements for each unit depending on the number of bedrooms.[10] Regarding quality, the new units had to adhere to the guidelines laid down in the National Housing Plan (Law Nbr. 13,728) and other ministerial regulations, which established requirements on the materials used. Tax breaks only applied to projects undertaken in urban areas, excluding those in cities with a high proportion of second homes.

The main fiscal advantage for developers and private investors was an exemption from paying any corporate tax (25%) on the sold units, while rents were partially exempted from personal income and corporate taxes over a nine-year period.[11] Applications were submitted at any time of the year. Submissions were evaluated by the National Housing Agency (*Agencia Nacional de Vivienda*, ANV) and, later, by a committee made up of members from the Ministry of Economics and Finance and the Ministry of Housing, both responsible for implementing the policy.

Regarding new construction projects, 494 were performed between December 2011 and December 2018, involving approximately 15.5K new housing units. The total amount invested for these subsidized projects was 990M USD, equivalent to approximately 1.5% of the 2011-2018 average GDP in USD. Despite being a national policy, 70% of these projects were concentrated in the city of Montevideo. My empirical analysis focuses on 98 subsidized new construction projects promoted and executed in Montevideo between 2011 and 2013 (approximately 3.3M USD per project), with an average construction period of 1.8 years. The tax exemptions per housing unit amounted to roughly 17K USD on average, which represents approximately 21% of the construction costs.[12] In Montevideo, the subsidy had a place-based structure for new construction. As observed in Figure 1, only in area S did the tax breaks apply to new construction

[10] $>$32 m^2 and $<$50 m^2 for one-bedroom units. With each additional bedroom (up to a total of four), the lower bound increased by 12 m^2 while the upper increased by 19 m^2. The number of single-bedroom units must be lower than 50% of the total number of produced units.

[11] Other fiscal benefits included exemptions from the wealth tax on land and improvements during construction, as well as on built and subsequently rented units over nine years. Additionally, private investors were exempted from paying the transfer tax (2% of the property appraisal value) if buying their unsold units. There were also tax credits for value-added tax on national and imported inputs, which, given the structure of these taxes, potentially reduced liquidity constraints and opportunity costs for developers.

[12] The National Statistical Office (*Instituto Nacional de Estadística*, INE) reports the cost of building one square meter according to house quality type, as defined in the National Housing Plan.

projects.

The treated area S was defined by the Ministry of Housing, together with the Ministry of Economics and Finance and the Local Government of Montevideo. Overall, it adhered to a number of natural city divisions provided by the main avenues and streets, and the area did not follow the borders of any other administrative division.[13] The law was intended to include the city's central neighborhoods in the targeted area, as revitalizing these neighborhoods was among the government's priorities, and to exclude the high-income areas of the city (those labeled as U). The subsidized area represents 52% of the total urbanized area of Montevideo, followed by the suburbs (29%) and the unsubsidized area (19%). It has a population density of 7,740 inhabitants per km^2, and it is composed of both central and peripheral neighborhoods, representing a highly heterogeneous area. This heterogeneity is also observed in income. Area U is the densest area (8,084 inhabitants per km^2), and it is also the richest part of the city, with an average household income double and triple that in area S and the suburbs, respectively. In contrast, the suburbs include the highest share of slums and have the lowest average income.

3. Theoretical framework

I develop a simple residential choice model based on Glaeser (2008), Banzhaf and Walsh (2013) and González-Pampillón, Jofre-Monseny and Viladecans-Marsal (2019) to explain how new housing supply in a given area of a given city impacts housing prices and the neighborhood income mix in that area. According to this model, on increasing the housing stock, housing prices change due to an increase in the level of neighborhood (exogenous) amenities (i.e., replacing vacant or disused structures by new dwellings) and because more affluent residents are attracted to these neighborhoods. As a countervailing effect, increasing the number of new dwellings exerts a downward pressure on prices.

The model considers one city with two communities (1 and 2), and the stock of housing in each community is denoted by S_1 and S_2, respectively. The size of the city is normalized to be one, and thus, $S_1 + S_2 = 1$. Under this assumption, S_1 and S_2 represent the proportions of the total housing stock in each community or the sizes of the community in relative terms.

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

I further assume that the stock of housing in community 1 is larger than that in community 2 ($S_1 > S_2$). The housing stock is categorized as new (N) or old (O); hence, $S_1 = S_1^N + S_1^O$ and $S_2 = S_2^N + S_2^O$. I also assume that the proportion of new dwellings in community 2 is larger than that in community 1 ($S_2^N > S_1^N$).[14]

There are two groups in the city: high-income households (denoted by H), and middle-income households (denoted by M).[15] P is the proportion of high-income households in the city. P_1 and P_2 denote the proportions of high-income households in communities 1 and 2, respectively. Then, the proportion of high-income households in the city can be defined as $P = S_1 P_1 + S_2 P_2$. I assume that the proportion of high-income households in the city is slightly larger than the stock of housing in community 2 and that the number of middle-income households is larger than the number of high-income households in the city ($H < M$). This implies that $S_2 \leq P < .5$. I restrict the model to equilibria such that $P_2 > P_1$, meaning that there is always a higher proportion of higher-income households living in the neighborhood with a newer housing stock.

The preferences of group H to live in community 1 are given by the following utility function: $U_1^H = Y^H - Q + \alpha^H(P_1) + G^H + a_h$, where Y^H is the average income of high-income households. Q denotes the price of housing in community 1 relative to community 2. $\alpha^H(P_1)$ represents the endogenous neighborhood amenities, and it is related to the fact that high-income households have a preference to live among households of their own type. G^H represents the level of neighborhood exogenous amenities which, among other factors, depends on the age of the housing stock in community 1, $G^H(\gamma_H, S_1^N, S_1^O)$. a_h is an individual specific term that reflects the attachment of individual h to community 1, and it is uniformly distributed in the unit interval, i.e., $a_h \sim U(0,1)$. Similarly, the preference of H to live in community 2 is represented by the following utility function: $U_2^H = Y^H + \alpha^H(P_2)$. In the case of group M, utilities in communities 1 and 2 are given by $U_1^M = Y^M - Q - \alpha^M(P_1) + G^M + a_m$ and $U_2^M = Y^M - \alpha^M(P_2)$, respectively. I assume that $\gamma^H > \gamma^M$, which implies that high-income individuals are more likely to be able to afford the new residential units and that they have a stronger preference for a younger housing stock.[16] Finally, I assume that $Y^H > Y^M$ and that $\alpha^M(P_k) = 0$, implying that the middle-income group is indifferent to living among their middle-income peers or

[14]Within each community, I assume that $S_1^N < S_1^O$, and that $S_2^N > S_2^O$.

[15]Low-income households reside mostly in the suburbs. The model can be extended to include three income groups. However, for simplicity, I work with two income groups.

[16]This assumption is in line with the evidence in Rosenthal (2008); Brueckner and Rosenthal (2009)

higher-income households.

By equalizing the utility functions of H and M across communities, I obtain the bid functions of each income group,

$$Q^H(a_{h^*}) = \alpha^H(P_1) - \alpha^H(P_2) + G^H + a_{h^*} \tag{1}$$

$$Q^M(a_{m^*}) = G^M + a_{m^*} \tag{2}$$

To solve the model, I equalize bid functions ($Q^H(a_{h^*}) = Q^M(a_{m^*})$), and I also use the fact that, in equilibrium, the supply of housing units equals that of demand in both communities. I obtain the solution for the share of high-income households that choose to reside in community 1 (P_1) and for housing prices (Q):[17]

$$P_1 = P + \frac{P(1-P)}{S_1}\left[\alpha^H(P_1) - \alpha^H(P_2) + G^H - G^M\right] \tag{3}$$

$$Q = 1 - S_1 + PG^H + (1-P)G^M + P[\alpha^H(P_1) - \alpha^H(P_2)] \tag{4}$$

Figure 2 summarizes the model and plots the previous four expressions. The bid functions of H and M (vertical axis) represent the willingness to pay for living in community 1 relative to community 2 for each income type, and they are plotted as a function of the share of income group H in community 1 (horizontal axis). The solid line represents the bid function of high-income households (H). It presents a mild curvature due to some preference of the high-income group to live among their own income type. The dashed line is the bid function of middle-income households (M), which has a downward slope as a regular demand function. The housing price Q and high-income share P_1 in equilibrium given in expressions 3 and 4, respectively, are graphically represented by the intersection of the bid functions. The top panel shows that there is a low share of high-income households (6%) living in community 1, mainly because of having an older housing stock compared to community 2.

[17]Equalizing bid functions leads to the following equation: $a_{h^*} = G^M + \alpha^H(P_1) - \alpha^H(P_2) + G^H + a_{m^*}$. For community 1, the market clearing condition in the housing market implies that $S_1 = (1-a_{m^*})(1-P)+(1-a_{h^*})P$. I also know that $(1-P_1)S_1 = (1-a_{m^*})(1-P)$. Using these three expressions, I obtain a solution for P_1 as a function of P_2. Since $P_2 = (P - S_1P_1)/S_2$, after solving for P_1, I also obtain a solution for P_2. After solving for P_1 and P_2, I derive the expression for housing prices Q.

3.1. The effect of new housing supply

I analyze the effect of increasing the number of new dwellings in community 1 on house prices and the neighborhood income mix. The government introduces a housing policy to incentivize new construction in community 1, and as a result, the share of new units increases ($S_1^N \uparrow$) in that community. This policy affects P_1 and Q since both depend on S_1^N. I first compute the derivative of P_1 with respect to S_1^N:

$$\frac{dP_1}{dS_1^N} = \frac{\frac{P(1-P)}{S_1}(\gamma^H - \gamma^M) - \frac{P(1-P)}{S_1^2}\left[\alpha^H(P_1) - \alpha^H(P_2) + G^H - G^M\right]}{1 - \frac{P(1-P)}{S_1}\left[\alpha^{H'}(P_1) + \alpha^{H'}(P_2)\frac{S_1}{S_2}\right]} \tag{5}$$

This derivative is positive only if the effect of improving the (exogenous) amenities is larger for the high-income group H than for the middle-income group ($\gamma^H > \gamma^M$), a condition that is satisfied by assumption.[18] In Figure 2, I analyze the effect of increasing the share of new houses in community 1, which is graphically represented as an upward shift of both bid functions in housing prices. The upward shift is larger for high-income dwellers, as they are more likely to buy the new homes. As a result, I observe that the share of high-income households in neighborhood 1 is larger in the new equilibrium, doubling the initial share (of 6%) to 12%.

The effect of increasing S_1^N on house prices is given by the following derivative:

$$\frac{dQ}{dS_1^N} = \underbrace{-1}_{\textbf{Supply effect}} + \underbrace{P\gamma^H + (1-P)\gamma^M}_{\textbf{Amenity effect}} + \underbrace{\frac{dP_1}{dS_1^N}\left[P\left(\alpha^{H'}(P_1) + \alpha^{H'}(P_2)\frac{P_1}{S_2}\right)\right]}_{\textbf{Change in the neighborhood income composition}} \tag{6}$$

There are three effects. The first is negative, and it is related to the number of new houses in community 1. The other two are positive. Local spillovers on house prices can emerge via two other channels. First, there is an amenity effect due to the increasing stock of new housing in community 1 (i.e., less deteriorated structures). The second channel is given by changes in the

[18] P_1 is a function that maps into itself: $P_1 = H(P_1)$. Since I only focus on stable equilibria ($H' < 1$), the denominator is positive. Hence, the derivative in 5 is positive if $\frac{P(1-P)}{S_1}(\gamma^H - \gamma^M) > \frac{P(1-P)}{S_1^2}\left[\alpha^H(P_1) - \alpha^H(P_2) + G^H - G^M\right]$. Assuming that $\alpha^k(P_l) = \alpha^k P_l^2$ (with $k = H, M$ and $l = 1, 2$) and defining $G^i = \gamma^i(F + S_1^N)$ with $F < 0$ (worse amenities in community 1), the latter expression is reduced to $(\gamma^H - \gamma^M)[S_1 - (F + S_1^N)] > \alpha^H(P_1^2 - P_2^2)$. By assumption $P_2 > P_1 > 0$, then, $0 > \alpha^H(P_1^2 - P_2^2)$. Therefore, if $\gamma^H > \gamma^M$, then $\frac{dP_1}{dS_1^N} > 0$.

neighborhood income composition since P_1 increases with S_1^N, as shown in expression 5. Given that $\alpha^{H'}(P_k) > 0$ (with $k = 1, 2$), it is straightforward to check that this second effect is also positive. The sum of these three effects is observed in Figure 2. Provided that the supply effect is not sizable, the model predicts that new housing supply increases house prices in community 1. Expression 6 suggests that the policy provides an exogenous increase in the stock of new units in community 1 that ends up affecting house prices. My reduced form estimates will capture the total effect, which is composed of the amenity effect, changes in the neighborhood income mix, and the supply effect. Within community 1, the distribution of H-type households will depend on the distance to the new amenities as in a model of housing externalities (Owens, Rossi-Hansberg and Sarte, 2010). Higher-income households are likely to concentrate close to newer units. Similarly, house prices are likely to experience larger increases in the surroundings of newly built homes than elsewhere. In the empirical analysis, I provide an estimate of the decaying rate of these externalities.

4. Data & variables

I use and combine data from different sources. First, I use official data from the National Housing Agency (*Agencia Nacional de Vivienda*) related to subsidized projects. These data contain information about project location, including street address and the reference number in the land register, approval date, 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., garages), and three categories for project size (large, medium and small).

Second, I use data on house prices from the National Registry Office (*Dirección General de Registros*, DGR) for the period 2006-2018. These data provide information on prices of housing transactions and built area (in square meters) reported by notaries who are in charge of registering this information in the DGR. It has been used by the National Statistical Office (*Instituto Nacional de Estadística*, INE) to compute housing prices indices. I focus on the logarithm of these transacted prices (in USD), being my main outcomes of interest. I exclude transactions with prices outside the bottom and top fifth percentiles by year and separately for houses and buildings to avoid outliers. I also drop transactions of subsidized sold units. This dataset is combined with data on housing characteristics from the National Cadaster Agency (*Dirección Nacional de Catastro*, DNC), which generally updates its records whenever the property is reassessed by the DNC. The data I use are based on properties mostly reassessed between 2008 and 2010.

Third, I use geo-coded daily crime incidents in Montevideo between 2006 and 2018 recorded by the National Police Department. This database has been used by Ajzenman and Jaitman (2016) to study crime concentration and crime hotspots in Montevideo and in other Latin American cities. Munyo and Rossi (2015) use this data to study crime recidivism. This database records 1,331,357 offenses, and it contains information about types of crime according to the Uruguayan penal code. In my analysis, I consider the total number of recorded crimes, and I also focus on the three most frequent offenses, which I classify as either property or nonproperty crimes. Under property crimes, I include thefts (53% of the total recorded incidents) and robberies (13% of the total recorded incidents). Both theft and robbery imply depriving a person of her/his property. However, robbery involves the use of violence. Nonproperty crimes include assault (4% of the total recorded incidents), defined as a physical attack upon another person. I aggregate this information at the census tract level, on which, on average, approximately 1,162 inhabitants reside, and it has an area of a half-square kilometer. Specifically, I focus on the logarithm of yearly counts of total crime, property crime and nonproperty crime.

Fourth, I use data from the National Household Survey (*Encuesta Continua de Hogares*, ECH), a yearly stratified random sample of households, from 2006 to 2018. I focus on the logarithm of the monthly disposable household income per capita with and without rental value as the outcome that measures the level of income of dwellers.[19] I combine household income data with a measure of the level of exposure to the policy (which I present in the next section) at the census tract level, as the survey presents census tract identifiers.

Finally, I use data from the 2011 census conducted by the INE, which contains socioeconomic information that allows constructing tract-level controls. A tract is the lowest level of aggregation at which the 2011 census is disclosed.

Table 1 presents the average values of the outcome variables (in 2011) used and of the 2011 census variables for the subsidized and unsubsidized sides of the border. On the subsidized side, house prices are lower than on the neighboring unsubsidized side, even much lower than house prices in more distant locations in the nontreated part. Total crime and property crime rates are slightly lower in the targeted area, while nonproperty crime is slightly larger, a stylized fact in Latin American cities, as property crime is more often committed in higher-income neighbor-

[19]The rental value is the reported amount that renters pay for their house/flat. In case of owner-occupied, the rental market value is inputted.

hoods. Household income shows a similar pattern as house prices. Regarding socioeconomic variables from the 2011 census, the subsidized part has a larger share of rented dwellings, a higher unemployment rate, and a larger percentage of low-educated head of households compared with tracts located on the unsubsidized side of the border. It also presents a higher street quality index, partly explained by central areas having a number of bus lines, the main means of public transportation in the city, which is one of the variables included in the index. Finally, nontreated neighborhoods are slightly denser and characterized by a larger share of buildings, thus having a more inelastic housing supply.

5. Empirical approach

The aim of this paper is to estimate the local effects that residential developments have on housing price of neighboring dwellings, as well as on crime records and household income. I use two empirical strategies to estimate spillovers on house prices. First, I compare changes in house prices on either side of the border with changes in house prices within an unsubsidized area far from the border before and after the introduction of the housing policy. Specifically, I consider a standard difference-in-differences method that exploits house price changes in different narrowly defined bands before and after policy implementation. Then, I estimate the following equation:

$$
\begin{aligned}
ln(p_{ibt}) &= \alpha^S sb_i + \alpha^U ub_i + \beta^S sb_i \times post + \beta^U ub_i \times post \\
&+ X_i\theta + \delta_t + \nu_b + \delta_t \times \nu_b + \epsilon_{ibt} \qquad (7)
\end{aligned}
$$

where $ln(p_{ibt})$ is the log-price of sold housing i in year-month t. The border is almost twelve kilometers long, comprising neighborhoods with different socioeconomic characteristics. Then, I partition the boundary into six border segments b (each approximately two kilometers long) based on census tract divisions. Since I focus on external effects, I do not consider the transaction of units subsidized by the policy. sb_i and ub_i are two binary indicators that take a value of one if the sold unit i is located within 400 meters of the border in the subsidized (S) or unsubsidized (U) area, respectively, and zero if it is located in a 400-meter wide band in the nontreated area 400 meters from the border (labeled as the control band). $post$ is a dummy that takes a value of one for the period 2012-2018.

β^S and β^U capture spillovers on house prices around the border and estimate the average effect for the period 2012-2018. Furthermore, they capture the total effect derived in equation 6. I also include a vector containing dwelling characteristics (X_i') usually included in hedonic models that explain some of the variation in house prices, such as constructed square meters (and its square), a set of dummies for year of construction, dummies for quality of the building (as assessed by the cadaster agency), number of floors, a dummy for detached and semidetached houses, whether there is a garage, balcony or outdoor space, other amenities, and if there is a slum within 300 meters. In the most controlled specifications, I also include a set of socioeconomic and urban infrastructure variables from the 2011 census to control for neighborhood characteristics[20] and the change in tract-level housing prices pre-policy, as these variables may influence both prices and developers' investment locations. I also include year-month dummies (δ_t) and a set of border dummies (ν_b), which implies that the coefficients of interest (β^S and β^U) are estimated using within border-segment variation. Finally, as in Boustan (2012), I also include the interaction between border and year dummies ($\delta_t \times \nu_b$), thus allowing for a common border-segment trend since the economic status of neighborhoods along the border may rise and fall. Using narrowly defined bands together with border-year dummies enables localized unobserved shocks, as well as underlying local factors that may drive developers' investment decisions, to be accounted for.

To obtain unbiased estimates, the control band should not be contaminated by spillovers. In this sense, using tight bands ensures that units are exposed to the same unobserved factors, but at the expense of control units being potentially contaminated by externalities. Then, to avoid estimates being sensitive to the definition of the band width, I provide nonparametric estimates of spillovers.[21] As in every difference-in-differences strategy, the common trend assumption is required to obtain unbiased causal effects. Through an event-study graph, I provide evidence that this assumption is likely to hold in my setting.

[20]I consider density of inhabitants per square km (in log), % of vacant and uninhabitable houses, % of renters, unemployment rate, % of low-educated head of households, % of historical monuments, and a street quality index. This index is constructed as a weighted average of several binary indicators such as if the street has public lighting, if it has trees, if it is paved and in good condition, if the sidewalk is in good condition, if the sidewalk has ramps for the disabled, if there is information about the street name, if it has storm drains and if there are dumps. For each indicator, weights are constructed as one minus the average, and they are normalized to sum to one. The index is bounded between 0 and 1.

[21]Technical details are explained in Appendix A

5.1. Investment exposure to subsidized residential investments

The second strategy uses an investment exposure measure that accounts for developers' investment locations along the subsidized border. In particular, I observe that developers focus their investments at some locations along the subsidized side of the border. This implies that some unsubsidized sold units were more exposed to subsidized investments than other dwellings located at more distant locations. I construct a measure that captures the exposure of each unsubsidized sold unit i to the subsidized investments. This variable is computed as the sum of the budgets of all projects (98 in total) multiplied by an exponential decay factor. Similar to Autor, Palmer and Pathak (2016), I adopted an exponential decay function that places larger weights on nearby subsidized projects. This measure is formally defined as follows:

$$\text{Intense}_i = \sum_{j=1}^{98} BT_j \times e^{-\lambda d_{ij}} \tag{8}$$

where BT_j is the budget of project j located at a distance d_{ij} (in km) from the sold unit i, and $\lambda(< 0)$ is the parameter that governs the decaying rate of weights. Then, Intense_i measures the level of investment that each unit i is exposed to. As the unit is built, this variable enables measurement of the exposure to scattered, as well as to more concentrated, housing investments. As λ increases, the level of exposure declines at a faster rate. Figure 3 shows the level of investment intensity for the city and along the border for $\lambda = -9$, where a darker color indicates larger levels of subsidized investments. I observe that there are high investment levels along some parts of the subsidized side of the border of area S with respect to area U, while in some locations, there are no investments. Figure 3 plots the log of the investment intensity as a function of distance to the boundary, where negative and positive distance values represent locations in the nontreated and treated areas, respectively. The exposure to these housing developments increases when approaching the targeted area, while exhibiting a wide range of variation in the level of intensity that I use to identify the external effects.

I apply a continuous-treatment difference-in-differences approach that uses Intense_i as a continuous-treatment variable. Specifically, I replace the border band indicators in equation 7 by the investment exposure measure. Then, I estimate the following equation:

$$
\begin{aligned}
p_{ibt} &= \alpha ln(\text{Intense}_i) + \beta ln(\text{Intense}_i) \times post \\
&+ X_i'\theta + \delta_t + \nu_b + \delta_t \times \nu_b + \epsilon_{ibt}
\end{aligned} \tag{9}
$$

where $ln(\text{Intense}_i)$ is the log of the intensity of the treatment for a given value of the parameter λ. I explore the sensitivity of the results to different values of λ. As this continuous-treatment variable is likely to be endogenous due to developers choosing those locations with higher expected growth in prices, I use the policy, and more specifically its place-based structure, as an instrument for the intensity measure. I define the instrument as a binary variable that takes a value of one for sold units located in the treated area and a value of zero for sold units located in the nontreated area. The coefficient β captures the spillovers effects, and it provides estimates of the derivative in equation 6. I also include border-year effects to control for local unobserved confounding factors. This empirical method deals with endogenous location decisions of developers that are likely to be influenced by future growth of house prices and rental prices. I also use this latter strategy to estimate the effect of these new developments on criminal activity, being a more direct measure of neighborhood amenities, and on household income to study the role of income sorting on housing externalities.

5.2. Threats to identification

My empirical analysis relies on the border being exogenously determined by policymakers while not being affected by developers' interests of including places with high expected growth in house prices. If that were the case and developers had managed to manipulate the definition of the boundary of the targeted area, then I would observe discontinuities in the house price gradient at the border before the introduction of the policy. In other words, house prices as a function of the distance to the border would not be smooth at the boundary, thus implying neighborhoods with better quality/amenities on the subsidized side of the border. The top panel in Figure 4 shows house prices in the pre-policy period (2006-2010) as a function of the distance to the border of treated area S with respect to the nontreated area U. The distance is normalized to be zero at the border. Positive distance values are defined for area S, while negative values are defined for area U. Each evenly spaced bin represents the average house price computed using the data-driven procedure developed in Calonico, Cattaneo and Titiunik (2015, 2017), and the fitted line is estimated using a third-order polynomial controlling for housing characteristics.

In Figure 4, I observe that house prices are higher, on average, in the nontreated area than in the treated area, being the richest part of the city. I also observe that the price gradient remains smooth at the border, with house prices showing a decreasing pattern when moving away from the unsubsidized area. In Table 2, I formally test for a discontinuity in the house price vector at the border. Using a kernel-weighted local polynomial (of different orders), the results verify what I previously observed in the graph, that is, the absence of a discontinuity in the house price gradient at the border.[22]

In the post-policy period (2011-2018), the bottom panel of Figure 4 suggests that there is a slight discontinuity at the border. However, the formal test (in panel b of Table 2) indicates that there is no jump at the border in this period, which may be explained by externalities going beyond the boundary. Figure 4 suggests that the policy changed the slope of the price gradient, which is what my reduced form analysis would be estimating. The fact that discrete jumps are not found confirms that developers did not succeed in influencing the definition of the border of the targeted area.

The other concern of my empirical strategy is whether the policy acts as a shifter of residential developments in the treated area. Panel a) in Figure 5 shows the probability of new residential developments in the pre-policy period (2006-2010) as a function of the distance to the border of the subsidized area, where the distance is normalized to be zero at the border. Each evenly spaced bin represents the share of new residential developments at the tract level, and the fitted line is estimated using a third-order polynomial.[23] Before the housing policy, the probability of new housing developments is larger in the unsubsidized area (with an average of almost 40%) than in the subsidized areas (with an average of almost 15%). The figure changes substantially after the introduction of the policy (bottom panel). The probability of residential developments in the nontreated area experienced a downward shift, and the average decreased to approximately 30%. Conversely, the targeted area experienced an upward shift in the probability of residential developments, with the average increasing to approximately 25%. This

[22] I also checked for discontinuities 100, 200, 300 and 400 meters from the border in area U, but there is no evidence of discontinuities.

[23] Again, I used the data-driven procedure developed in Calonico, Cattaneo and Titiunik (2015, 2017) to construct bins.

shift is mainly observed close to the border. Therefore, the policy incentivized new residential construction in the treated area, which may be partly due to a crowding-out effects from private residential investments in the unsubsidized area. Berrutti (2017) finds that the policy has an impact on the distribution of residential construction, as well as on the size of housing produced, and on densification close to the border. Thus, the policy increases residential investments in the subsidized area.

6. Results

6.1. Estimates of spillovers on house prices

In Table 3 and Figure 6, I present the results from estimating equation 7 that captures the effects on 400-meter wide bands around the border, using sold units located in the unsubsidized area and at least 400 meters from the border as the comparison band. Figure 6 reports the spillover estimates along the border resulting from replacing the post dummy in equation 7 by a set of year dummies using 2011 (the year the policy was introduced) as the base year, which is omitted, and controlling for housing characteristics. The top panel indicates that external effects on house prices increase along the treated area close to the border after the introduction of the policy. The evidence points to local impacts both during construction (2012-2013) and once the subsidized housing was built. Observing spillovers during construction is likely to be associated with initial amenity improvements due to the removal of abandoned buildings and brownfield sites. It may also be related to the idea that agents tend to develop expectations regarding how local housing markets will react once these projects end. The bottom panel indicates the presence of externalities on the nontreated side of the border following the introduction of the program. In this case, there is a less evident pattern during the period, and for some years, the effects dissipate. In both panels, I observe that there are no spillover effects before 2011, which provides supportive evidence that the common trends assumption is likely to hold. This further validates the use of this empirical strategy, while it discards the hypothesis that developers react beforehand, thus affecting local housing markets.

Table 3 reports the average impact on house prices between 2012 and 2018 for different sets of controls. The first and second rows present the estimates of the spillovers along the subsidized and unsubsidized borders, respectively, using 400-meter wide bands. In the first column, I include border-year effects and housing characteristics, as in Figure 6. The results indicate an almost 13% increase in house prices on the treated side, while the prices increase by 6.6% on the nontreated side of the border. The point estimates decrease to approximately 12% in the subsidized band and 6% in the unsubsidized band after controlling for socioeconomic variables from the 2011 census (column 2). Similar estimates are obtained after also including pre-policy house price changes at the tract level (column 3). In the first three columns, I use clustered standard errors at the tract level. Alternatively, in the last three columns, I use the spatial HAC inference method proposed by Conley (1999) which adjusts for spatial and serial correlation and is robust to heteroscedasticity. [24] This allows me to deal with the correlation between nearby sold units located in different tracts, specifically for those units situated close to the tract edges. By applying this inference method, the standard errors decrease substantially (approximately 30-40%), and then, I opt to only report the clustered standard errors as they are more conservative.

In Table 4, I present the results from using 200-meter wide bands, which enables me to test for decaying patterns on externalities parametrically. Specifically, I consider three 200-meter wide bands on the treated side and two on the nontreated side, using the same reference group as in the previous table. Table 4 shows a decaying pattern on spillovers when moving away from the border. On the subsidized side, externalities are mainly present in the first and second 200 meters, while decreasing in the next 200 meters. Focusing on the third column, the specification with the highest set of controls, the point estimates indicate that house prices increase around 12% in the first 200 meters, almost 15% in the next 200 meters, and approximately 9% in the third 200 meters. The slight and slow decay rate observed in estimated spillovers in the targeted area is explained by the high concentration of housing investments around the border. On the unsubsidized side, house prices increase almost 9% in the first 200 meters, but then, the external effect disappears in the next 200 meters. The fast decaying rate in the nontreated part leads to the conclusion that externalities are highly local.

[24] I use the code developed by Hsiang (2010). Specifically, I estimate the Conley spatial HAC standard errors using kernel linear decay weights *a la Bartlett*, assuming that the spatial correlation vanishes after more than 500 meters and that the serial correlation vanishes after one year.

I further explore this decaying pattern by estimating spillovers nonparametrically, which has the advantage of avoiding arbitrary definition of the width of the bands. Specifically, I estimate spillovers as a nonparametric function of the distance to the border, before and after the introduction of the tax breaks, using the semiparametric differencing approach developed by Yatchew (1997); Yatchew and No (2001). The results are presented in Figure A.10 in Appendix A. The distance is normalized to zero at the border, and negative values correspond to the unsubsidized area and positive values to the subsidized area. The figure shows that when approaching the border (coming from the nontreated area), spillovers increase, becoming statistically significant within 200 meters to the border. Similar to the parametric estimates, externalities are higher on the treated side of the border, while they decay when going into the nontreated area. Both the parametric and nonparametric estimates capture spillovers along the border; as suggested in the theoretical framework, these externalities stem from improvements in local amenities and changes in the income composition of neighborhoods.

Developers chose specific locations along the border, which leads to variation in the level of exposure to residential developments. The exposure measure I built captures the spatial variation in the level of the housing investments under this policy. This treatment measure is endogenous, as it reflects developers' decisions on location. To mitigate this source of endogeneity, I exploit the exogenous nature of the place-based structure of the subsidy. Specifically, I instrument the continuous treatment variable by a binary indicator that takes a value of one for units located in the targeted area and a value of zero for units in the nontargeted area. Figure 7 shows the spillover estimates on house prices by year relative to 2011, the base year. This intensity measure depends on the decaying parameter λ. I report the results for four different λ values[25], controlling for housing characteristics. The figure shows that before the introduction of the policy, the estimated coefficients are nearly zero and not statistically significant, which provides evidence supporting the common trend assumption and the validity of this empirical strategy. After its introduction, the point estimates increase, with spillovers being mainly present after 2013. The results are not sensitive to the choice of the value of λ. Note that for larger λ values, the precision increases, while the point estimates decrease, as expected.

In Table 5, I present the results based on estimating equation 9 for $\lambda = 9$, considering three

[25] Similar to Autor, Palmer and Pathak (2016), who construct a measure of rent-control exposure for each parcel, my investment exposure measure for each sold unit is constructed considering four different λ values (6, 9, 12, 15).

different specifications. Since the dependent variable is on the log scale, as is the investment intensity variable, the effects reported in panel a) represent the elasticity of house price with respect to the subsidized housing investments. In the first column, I control for housing characteristics and border-year dummies as in the event-study graphs. I find positive and statistically significant effects, with an estimated elasticity of .032. In the second column, I include 2011 census controls, and the elasticity reduces to .028, while it decreases slightly when also adding pre-trend prices at the tract level as a control in the third column. Using the estimated elasticity in the third column, I compute the average effect within 400 meters of the border, obtaining a 15% increase in house prices on the treated side and a 9% increase on the nontreated side. These effects are larger than the 12% and 6% obtained when using the 400-meter border band indicators since the previous strategy leads to intention-to-treat estimates. Across specifications, the instrument seems relevant, as the (Kleibergen-Paap) F-statistic is well above ten, the usual benchmark. In panel b), I use a binary version of the intensity measure that takes a value of one whenever the intensity of the treatment is above the median and a value of zero otherwise. I find that the house price increases in areas with high investment intensity, with estimates ranging from 17% to 19%.

The empirical results presented here are consistent with amenities being the main determinant of house prices at the local level. In the context of the theoretical framework previously developed, increasing the stock of new housing in the mid-income neighborhood does not offset the positive effects that improved amenities and changes in the neighborhood income composition have on house prices. Ooi and Le (2013) and Zahirovich-Herbert and Gibler (2014) find an approximately 2% increase in house prices due to new housing supply. Compared to Diamond and McQuade (2019), the externalities that I find here are higher but more localized.[26]

Next, I explore whether these externalities are heterogeneous to some neighborhood's characteristics from the 2011 census. In the first column of Table 6, I analyze whether external effects are heterogeneous to a street quality index (bounded between 0 and 1) at the tract level. This index measures the presence of basic urban infrastructure (e.g., public lighting, storm drains, street names, bus stops, and ramps for the disabled), as well as its quality (e.g., paved streets

[26] Diamond and McQuade (2019) find a 6.5% increase in house prices for the case of low-income neighborhoods, and spillovers disappear after 1,500 meters (approximately 1 mile). I find that the increase of 12-17% in house prices and spillovers vanishes after approximately 200 meters.

and streets in good condition, sidewalks in good condition, and if there are dumps). I find a 26% increase in house prices in the case of tracts with a street quality index above the median that are highly exposed to subsidized investments, thus being higher than the baseline estimates. This result may be partly explained by the complementarities of these developments with the existing urban infrastructure. Central neighborhoods received significant investments, and since they have many bus lines and public transportation, which is positively correlated with the index, this could also explain the remarkable effects. In the second column, I use the share of renters as another neighborhood dimension. I find that spillovers are higher among tracts with a high share of renters that are highly exposed to the policy, observing a 21% increase in house prices. Higher tenant/rental turnover rates may explain the results in these tracts, thus reflecting a more dynamic change in neighborhood income composition. Higher shares of renters could lead to faster neighborhood transitions in economic status (Rosenthal, 2008). In the last column, I check for heterogeneous results depending on the proportion of head of households with low-education levels. I only observe slightly higher spillovers for tracts with lower shares, but the results are not statistically significant.

The intensity measure used in previous estimates only makes use of the residential construction developed under this policy. In Table 7, I consider both subsidized and unsubsidized new residential supply developed between 2011 and 2013. In the first three columns, I consider the tract-level sum of the square meters of the new developments. In the last three columns, I use a binary variable that equals one in the case of tracts where new housing was constructed. The variable measuring the tract-level subsidized developments is instrumented using the policy spatial scheme as before. Estimates in the first three columns show an elasticity of .02, being similar to previous estimates, while I find an elasticity of zero for the case of unsubsidized new developments. The results are similar across specifications. In the last three columns, I consider the binary tract-level measure and find a 17-18% increase in house prices in tracts with subsidized housing projects, while there is no evidence of spillovers from new unsubsidized housing. These results serve as a placebo exercise, as they proves that externalities are caused by the subsidized investments, not by unsubsidized residential investments.

6.2. *Effects on crime*

Most of the subsidized investments involved demolishing abandoned buildings or disused factories, which usually attract criminal activity and depress house prices (Campbell, Giglio and Pathak, 2010). Removing neighborhood disamenities may reduce local crime, which may also affect residential externalities. As a countervailing effect, the housing units developed under this policy were affordable for middle-high and high-income households, who face a higher probability of being the target of property offenses. Then, I empirically test whether these residential investments affect crime patterns using the continuous difference-in-differences estimator that uses the investment exposure measure for $\lambda = 9$ as the treatment variable. Concretely, I estimate equation 9 for three different outcomes, total crime, property crime (defined as theft and robbery), and nonproperty crime (defined as assaults). For these three outcomes, I use the log of the yearly counts at the tract level.

Figure 8 presents the yearly estimates for total crime, property crime, and nonproperty crime. Regarding total crime (top panel), I observe a reduction in the following years after the introduction of the policy, but after 2015, the crime rates seem to revert to pre-policy levels. For property crime (mid-panel), the evidence shows a similar pattern, with the reduction being statistically significant for a couple of years in this case. The graph also indicates a temporary reduction, as point estimates go back to zero after 2015. This finding suggests that the initial reduction due to the removal of crime hubs may be offset by the fact that neighborhoods started being populated by higher-income dwellers. The bottom panel shows no effect on nonproperty crime. For the three outcomes, I observe that in the pre-policy period, the estimated coefficients are not statistically significant, which enhances the robustness of these results.

In Table 8, I present the effect for the period 2012-2018. For each outcome, I consider two specifications. The first includes border-year dummies and 2011 census controls, and the second adds initial crime levels for the year 2006 to control for mean reversion in crime levels. The first two columns of panel a) indicate that there is no statistically significant reduction in total crime. The next two columns also show that there is no effect on property crime, while the last two columns reveal no effect for nonproperty crime rates. In panel b), I consider the binary treatment variable that takes a value of one for tracts with intensity higher than the median. The results show approximately 14% and 19% reductions in total crime and property crime, respectively, using the specification that also includes the initial crime levels. However, the point estimates are not statistically significant. For nonproperty crime, there is a nonnegligible reduction of

8.3%, but again, this result is not statistically significant. In the case of the total and property crime rates, the absence of an effect may be explained by an influx of more affluent residents. I explore the effect of housing supply on the neighborhood income mix in the next section.

In Table 9, I turn to analyzing property crime according to who is the victim of the crime. Specifically, I consider the logarithm of the number of offenses committed on houses and persons, while the remaining group, labeled as 'other', includes offenses committed on businesses, vehicles, and not defined.[27] Again, I use the investment intensity for $\lambda = 9$ as the treatment variable. I observe a reduction in property crime committed on houses and persons statistically significant at the 10% level. Additionally, I find that there is no effect in the case of property crime committed on businesses and vehicles and classified as not defined. I cannot disentangle whether the reduction in crime committed on houses/persons is due to the removal of crime hubs or more investments in safety measures taken by new dwellers. Given that the effects are mainly observed the year after the introduction of the policy, the reduction in crime is more likely to be explained by the removal of crime spots.

6.3. Effects on socioeconomic outcomes

From a theoretical perspective, the new residential construction leads to an increase in the share of high-income dwellers in the middle-income neighborhood that received these housing investments. Thus, I test whether this prediction holds by focusing on the effect of the policy on household income. Specifically, I focus on the disposable household income reported in the National Household Survey, which is drawn from a stratified random sample carried out every year. This survey contains census tract identifiers that allow me to merge this information with the level of exposure to the new housing investments at the tract level. Then, I test whether household income increased in highly exposed tracts using the continuous difference-in-differences estimator. The intensity measure is instrumented by a binary variable that takes a value of one for tracts in the treated area.

In Figure 9, I present the estimates by year relative to the year 2011. I first consider the disposable household income (per capita) with rental value (top panel). I observe that before

[27] I also considered offenses committed on businesses and vehicles separately, but I did not find significant effects.

the introduction of the policy, the estimated coefficients are close to zero and not statistically significant. Two years after the introduction of the policy, the estimates are still close to zero, while after 2013, I observe a break in the trend, with most point estimates being above zero and statistically significant.[28] The mid-panel presents the event-study graph that results from using the disposable household income (per capita) without rental value. Overall, I observe a similar pattern, but in this case, the effects are smaller. In the bottom panel, I turn to analyzing the effect on the rental value. The results show that the effect on disposable household income is partly driven by a substantial increase in the rental value in areas with high investment exposure. This also confirms that the willingness to pay in areas that face new supply increased.

In Table 10, I present the estimates for the period 2012-2018. In panel a), I regress the disposable household income (in log) with rental value (first column), without rental value (second column), and with log of rental value (third column) on the log of the intensity measure for the decay parameter $\lambda = 9$. For each panel and in each column, I report the F-statistics from the first stage that uses the policy border as the instrument for the investment intensity measure. In each regression, I control for border-year dummies and used 2011 census controls at the tract level. The first column shows an elasticity of .011, which is statistically significant at the 5% level, while there is no effect when considering the disposable household income without rental value (second column). The third column shows that the policy affects rental values, with an estimated elasticity of .031. In panel b), I use a binary exposure measure that takes a value of one for intensities above the median. The results indicate almost 10% and 6% increases in household income with and without rental value, respectively, but with only the first case being statistically significant at the 5% level. The third column shows a remarkable 26% in rental values. These findings confirm the predictions of the theoretical framework, as I observe an increase in the income of dwellers in the treated area.

7. Conclusions

This paper estimates spillover effects from new residential developments on house prices, crime records, and household income. The supply of new housing was incentivized through tax

[28] The National Statistics Office reported that the household income from the 2016 National Household Survey was subject to some corrections after realizing the presence of missing observations. This may be correlated with the absence of an effect for that year.

breaks applied in a spatially defined middle-income area in the city of Montevideo that resulted in major investments in the housing sector. I find evidence of a substantial increase in the price of the housing surrounding the subsidized investments, with residential externalities being highly local. The house prices increase by 12-17%, and this effect tends to vanish after almost 200 meters. The remarkable effects contrast with previous evidence on spillovers from new residential developments in general and from affordable housing but are in line with estimates from quantitative spatial models, such as those in Ahlfeldt et al. (2015).[29] Moreover, in areas with a high share of renters, externalities have a marked presence, suggesting that the spillovers can be highly heterogeneous and may be dependent on initial neighborhood characteristics. This has also been suggested in the literature (Baum-Snow and Marion, 2009; Eriksen and Rosenthal, 2010; Diamond and McQuade, 2019). Additionally, I analyze the effect of new housing supply on crime rates. I find that the property crime rate decreases, but this effect is not long lasting, as rates converge to pre-policy levels. Nonproperty crime remains unchanged - this casts doubt on crime being one of the main drivers of these spillovers. Finally, there is evidence that the income mix of neighborhoods that received large housing investments changed.

The results of this paper enhance the role of amenities in the determination of house prices locally. The findings also indicate that the new housing supply contributed to the revitalization of some middle-income areas of the city. In this sense, such policies may be justified based on externalities. Nevertheless, as the willingness to pay to live in these areas increases, the affordability of housing in these neighborhoods tends to be reduced. It is worth noting that the provision of affordable housing was also one of the aims of this legislation, yet little has been achieved in this regard because of 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 housing affordable, two first-order problems that seem to be addressed by two different policies.

[29] Ooi and Le (2013) and Zahirovich-Herbert and Gibler (2014) find an approximate 2% increase in local house prices due to new housing supply. Diamond and McQuade (2019) find a 6.5% increase in the prices of dwellings located nearby LIHTC developments in low-income neighborhoods.

Tables

TABLE 1

SUMMARY STATISTICS OF OUTCOME AND CONTROL VARIABLES

	Subsidized side			Unsubsidized side					
	Within (0 m,400 m]			Within [0 m,400 m)			Within [400 m,800 m)		
	Mean	Min	Max	Mean	Min	Max	Mean	Min	Max
Outcome variables (in 2011)									
House prices (in log)	11.002	8.855	12.707	11.278	9.069	12.766	11.462	9.306	12.924
Observations		1,121			1,453			918	
All type of crimes (in log)	4.352	2.944	6.209	4.630	3.258	6.290	4.364	3.584	5.793
Observations		71			69			33	
Property crime (in log)	4.086	2.639	5.892	4.394	2.996	5.964	4.143	3.401	5.576
Observations		71			69			33	
Nonproperty crime (in log)	1.093	0.000	3.045	.961	0.000	3.761	.764	0.000	2.485
Observations		71			69			33	
Household income PC (in log) w/ rental value	9.892	7.943	11.765	10.072	7.617	12.323	10.172	8.197	12.505
Observations		815			1,029			535	
Household income PC (in log) w/o rental value	9.604	7.359	11.667	9.781	7.162	12.292	9.865	6.506	12.447
Observations		815			1,029			535	
Rental value (in log)	9.049	4.546	10.309	9.265	6.139	10.525	9.409	7.313	11.247
Observations		815			1,029			535	
2011 census variables									
Density (inhabitants per km2, in log)	9.491	8.190	10.616	9.257	2.308	10.758	9.372	8.212	10.167
% of occupied dwellings	89.542	66.922	96.278	90.420	75.798	100.000	90.022	73.857	95.161
% of vacant or uninhabitable dwellings	3.219	0.509	7.023	2.861	0.000	8.434	2.949	0.538	8.041
% of buildings in bad condition	0.684	0.000	6.452	0.478	0.000	3.698	0.481	0.000	5.525
% of rented dwellings	38.434	12.894	58.398	31.084	0.000	51.421	28.552	9.859	44.697
Unemployment rate	5.979	2.723	8.619	5.254	2.827	11.111	5.002	3.168	8.309
% of low-educated head of households	9.101	2.157	38.729	6.668	1.990	40.000	6.284	2.174	13.622
Street quality index [0,1]	0.562	0.278	0.728	0.518	0.261	0.686	0.549	0.433	0.710
Observations		71			69			33	

TABLE 2

TESTING FOR DISCONTINUITIES IN THE HOUSE PRICE GRADIENT AT THE BORDER. REGRESSION DISCONTINUITY ESTIMATES.

	(i)	(ii)	(iii)	(iv)
a) House prices (in log), pre-intervention period (2006-2010)				
Polynomial order	1	2	3	4
$\mathbb{1}$[Boundary distance $>$ 0]	0.004	0.020	0.022	0.021
	(0.033)	(0.042)	(0.047)	(0.049)
Robust p-value $^{(a)}$	.837	.599	.611	.701
Robust 95% CI $^{(a)}$	[.084,-.068]	[.115,-.066]	[.122,-.072]	[.118,-.080]
Observations	25,838	25,838	25,838	25,838
Effective nbr. of obs.	4,497 ǀ 4,397	4,558 ǀ 4,524	5,572 ǀ 5,683	6,486 ǀ 6,855
Nbr. of clusters	176	162	160	175
Bandwidth (in km)	.286	.294	.386	.498
b) House prices (in log), post-intervention period (2011-2018)				
Polynomial order	1	2	3	4
$\mathbb{1}$[Boundary distance $>$ 0]	-0.026	-0.010	-0.030	-0.055
	(0.042)	(0.051)	(0.058)	(0.064)
Robust p-value $^{(a)}$	.723	.827	.537	.337
Robust 95% CI $^{(a)}$	[.074,-.107]	[.094,-.118]	[.080,-.153]	[.065,-.191]
Observations	34,823	34,823	34,823	34,823
Effective nbr. of obs.	5,741 ǀ 4,825	5,866 ǀ 5,030	7,113 ǀ 6,089	8,019 ǀ 7,172
Nbr. of clusters	161	144	156	174
Bandwidth (in km)	.238	.250	.318	.380

Notes: Columns (i) to (iv) are estimated using the robust bias-corrected procedure and the optimal (covariate-adjusted and cluster-robust) bandwidth selection criteria developed in Calonico, Cattaneo and Titiunik (2014, 2017); Calonico et al. (2018). All estimates are computed using a triangular kernel function and one common MSE-optimal bandwidth. Covariates include year-month dummies, border-year dummies, and dwelling characteristics. $^{(a)}$ Robust p-values and robust 95% CIs are constructed using clustered-robust standard errors at the tract level. Conventional clustered standard errors are in parenthesis, where ***, **, and * denote statistical significance at the 1, 5, and 10% levels.

TABLE 3

SPILLOVER ESTIMATES ON HOUSE PRICES. DIFFERENCE-IN-DIFFERENCES STRATEGY USING 400-METER WIDE BANDS.

				Spatial HAC inference[a]		
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Distance to the border (d_i):						
Subsidized border band (sb)						
$\mathbb{1}\{0m < d_i \leq 400m\} \times$ post	0.128***	0.116***	0.115***	0.128***	0.117***	0.115***
	(0.032)	(0.030)	(0.030)	(0.013)	(0.014)	(0.014)
Unsubsidized border band (ub)						
$\mathbb{1}\{0m \leq d_i < 400m\} \times$ post	0.066***	0.060***	0.059***	0.066***	0.060***	0.060***
	(0.022)	(0.022)	(0.022)	(0.013)	(0.014)	(0.014)
Housing characteristics	Y	Y	Y	Y	Y	Y
Border-year dummies	Y	Y	Y	Y	Y	Y
2011 census controls	N	Y	Y	N	Y	Y
Δ house prices $_{\text{pre-policy}}$ at the tract level	N	N	Y	N	N	Y
Observations	37,741	37,741	37,741	37,741	37,741	37,741
Number of clusters	162	162	162			
Adjusted R squared	0.497	0.507	0.507	0.495	0.507	0.507

Notes: The dependent variable is the log of house prices. The comparison group is composed of sold units located in the unsubsidized area (U) that are between 400 and 800 meters from the border. All regressions include year-month dummies. In columns (i) to (iii), clustered standard errors at the tract level are used. [a] In columns (iv) to (vi), the spatial HAC standard errors developed in Conley (1999); Conley and Molinari (2007) are used; the linear decay kernel weights (a la Bartlett) used are assumed to vanish above 500 meters, and serial correlation is assumed to vanish after one year. *** significant at the 1% level. ** significant at the 5% level. * significant at the 10% level.

TABLE 4

SPILLOVER ESTIMATES ON HOUSE PRICES. DIFFERENCE-IN-DIFFERENCES STRATEGY USING 200-METER WIDE BANDS.

	(i)	(ii)	(iii)
Distance to the border (d_i):			
Subsidized border band (sb)			
$\mathbb{1}\{0m < d_i \leq 200m\} \times$ post	0.129***	0.122***	0.121***
	(0.042)	(0.041)	(0.041)
$\mathbb{1}\{200m < d_i \leq 400m\} \times$ post	0.177***	0.150***	0.147***
	(0.039)	(0.035)	(0.034)
$\mathbb{1}\{400m < d_i \leq 600m\} \times$ post	0.093***	0.089***	0.088***
	(0.035)	(0.033)	(0.033)
Unsubsidized border band (ub)			
$\mathbb{1}\{0m \leq d_i < 200m\} \times$ post	0.097***	0.089***	0.088***
	(0.028)	(0.026)	(0.026)
$\mathbb{1}\{200m \leq d_i < 400m\} \times$ post	0.019	0.014	0.013
	(0.028)	(0.028)	(0.027)
Housing characteristics	Y	Y	Y
Border-year dummies	Y	Y	Y
2011 census controls	N	Y	Y
Δ house prices $_{\text{pre-policy}}$ at the tract level	N	N	Y
Observations	42,173	42,173	42,173
Number of clusters	182	182	182
Adjusted R squared	0.496	0.508	0.509

Notes: The dependent variable is the log of house prices. The comparison group is composed of sold units located in the unsubsidized area (U) that are between 400 and 800 meters from the border. All regressions include year-month dummies. Clustered standard errors at the tract level are used. *** significant at the 1% level. ** significant at the 5% level. * significant at the 10% level.

TABLE 5

SPILLOVER ESTIMATES ON HOUSE PRICES. CONTINUOUS DIFFERENCE-IN-DIFFERENCES ESTIMATOR.

	(i)	(ii)	(iii)
a) Continuous exposure measure			
ln(Intense) $\times$ post	0.032***	0.028***	0.027***
	(0.010)	(0.009)	(0.009)
F-statistics	37.120	39.627	40.473
b) Binary exposure measure			
$\mathbb{1}[ln(\text{Intense}) > p50\text{th}] \times$ post	0.187**	0.175**	0.172**
	(0.082)	(0.073)	(0.073)
F-statistics	20.369	23.289	23.372
Housing characteristics	Y	Y	Y
Border-year dummies	Y	Y	Y
2011 census controls	N	Y	Y
Δ house prices $_{\text{pre-policy}}$ at the tract level	N	N	Y
Observations	37,741	37,741	37,741
Nbr. of clusters	162	162	162

Notes: The dependent variable is the log of house prices. The main independent variable is the investment exposure measure *Intense*, which depends on the decaying parameter λ. These estimates are produced using an exposure measure with $\lambda = 9$. *Intense* is instrumented by a binary variable that takes a value of one for sold units in the subsidized area and zero for sold unit in the unsubsidized area. All regressions include year-month dummies. Clustered standard errors at the tract level are used. *** significant at the 1% level. ** significant at the 5% level. * significant at the 10% level.

TABLE 6

SPILLOVER ESTIMATES ON HOUSE PRICES. CONTINUOUS DIFFERENCE-IN-DIFFERENCES ESTIMATOR, HETEROGENEOUS ANALYSES.

	$c_i =$		
	Street quality index	**% of renters**	**% of head of HH with low education level**
	(i)	(ii)	(iii)
$\mathbb{1}[ln(\text{Intense}) > p50\text{th}] \times \mathbb{1}\{c_i \geq p50\text{th}\} \times \text{post}$	0.263***	0.205**	0.084
	(0.099)	(0.094)	(0.094)
$\mathbb{1}[ln(\text{Intense}) > p50\text{th}] \times \mathbb{1}\{c_i < p50\text{th}\} \times \text{post}$	0.063	0.124	0.112
	(0.090)	(0.209)	(0.072)
Observations	37,741	37,741	37,741
Nbr. of clusters	162	162	162
F-statistics	7.641	2.152	7.709

Notes: The dependent variable is the log of house prices. The main independent variable is the investment exposure measure *Intense*, which depends on the decaying parameter λ. These estimates are produced using an exposure measure with $\lambda = 9$. *Intense* is instrumented by a binary variable that takes a value of one for sold units in the subsidized area and zero for sold units located in the unsubsidized area. All regressions include year-month dummies, border and border-year dummies, 2011 census controls, and change in house prices (pre-policy period, 2006-2010) at the tract level. Clustered standard errors at the tract level are used. *** significant at the 1% level. ** significant at the 5% level. * significant at the 10% level.

TABLE 7

SPILLOVER ESTIMATES ON HOUSE PRICES. CONTINUOUS DIFFERENCE-IN-DIFFERENCES ESTIMATOR. SUBSIDIZED VS UNSUBSIDIZED DEVELOPMENTS.

New residential developments	in m2 $^{(a)}$			binary $^{(b)}$		
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Subsidized x post	0.023***	0.023***	0.022***	0.184***	0.173***	0.171***
	(0.008)	(0.008)	(0.008)	(0.061)	(0.057)	(0.057)
Unsubsidized x post	0.002	0.003	0.003	0.009	0.011	0.011
	(0.004)	(0.004)	(0.004)	(0.032)	(0.030)	(0.029)
F-statistics	28.990	19.171	18.983	26.066	17.967	17.853
Housing characteristics	Y	Y	Y	Y	Y	Y
Border-year FE	Y	Y	Y	Y	Y	Y
2011 census controls	N	Y	Y	N	Y	Y
Δ house prices $_{\text{pre-policy}}$ at the tract level	N	N	Y	N	N	Y
Observations	37,741	37,741	37,741	37,741	37,741	37,741
Nbr. of clusters	162	162	162	162	162	162

Notes: The dependent variable is the log of house prices. $^{(a)}$ In the first three columns, the main independent variable is the sum of the area of new residential developments (in log) in a given tract. $^{(b)}$ In the last three columns, I use a binary version that takes a value of one whenever there were residential developments in the tract. I consider new residential developments ready to sell between 2013 and 2016. The sum of subsidized residential developments is instrumented by a binary measure that takes a value of one for tracts in the subsidized area and zero for tracts in the unsubsidized area. All regressions include year-month dummies. Clustered standard errors at the tract level are used. *** significant at the 1% level. ** significant at the 5% level. * significant at the 10% level.

TABLE 8

EFFECTS ON CRIME RECORDS. CONTINUOUS DIFFERENCE-IN-DIFFERENCES ESTIMATOR.

	All type		**Property crime**		**Nonproperty crime**	
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
a) Continuous exposure measure						
ln(Intense) $\times$ post	-0.031	-0.020	-0.042*	-0.027	-0.014	-0.012
	(0.025)	(0.021)	(0.025)	(0.021)	(0.024)	(0.024)
F-statistics	80.040	80.546	80.040	80.586	80.040	80.201
b) Binary exposure measure						
$\mathbb{1}[ln(\text{Intense}) > p50\text{th}] \times$ post	-0.216	-0.138	-0.290	-0.188	-0.094	-0.083
	(0.173)	(0.147)	(0.178)	(0.149)	(0.164)	(0.164)
F-statistics	52.202	52.485	52.202	52.443	52.202	52.792
Border-year FE	Y	Y	Y	Y	Y	Y
2011 census controls	Y	Y	Y	Y	Y	Y
Initial crime level	N	Y	N	Y	N	Y
Observations	2,249	2,249	2,249	2,249	2,249	2,249
Nbr. of clusters	173	173	173	173	173	173

Notes: The dependent variable is the log of the count of reported crimes at the tract level. Property crime includes theft and robbery, while nonproperty crime includes assault. The main independent variable is the investment exposure measure *Intense*, which depends on the decaying parameter λ. These estimates are produced using an exposure measure with $\lambda = 9$. *Intense* is instrumented by a binary variable that takes a value of one if the tract is located in the subsidized area and zero if the tract located in the unsubsidized area. All regressions include year dummies. Clustered standard errors at the tract level are used. *** significant at the 1% level. ** significant at the 5% level. * significant at the 10% level.

TABLE 9

EFFECTS ON PROPERTY CRIME RECORDS BY TYPE. CONTINUOUS DIFFERENCE-IN-DIFFERENCES ESTIMATOR.

	Property crime committed on			
	houses/persons		other	
	(i)	(ii)	(iii)	(iv)
$\mathbb{1}[ln(\text{Intense}) > p50\text{th}] \times$ post	-0.344*	-0.296*	-0.259	-0.109
	(0.179)	(0.159)	(0.189)	(0.158)
F-statistics	52.202	52.151	52.202	52.855
Border-year FE	Y	Y	Y	Y
Controls at the tract level	Y	Y	Y	Y
Initial crime level	N	Y	N	Y
Observations	2,249	2,249	2,249	2,249
Nbr. of clusters	173	173	173	173

Notes: The dependent variable is the log of the count of reported crimes at the tract level. Property crime includes theft and robbery, while nonproperty crime includes assault. The main independent variable is the investment exposure measure *Intense*, which depends on the decaying parameter λ. These estimates are produced using an exposure measure with $\lambda = 9$. *Intense* is instrumented by a binary variable that takes a value of one if the tract is located in the subsidized area and zero if the tract located in the unsubsidized area. All regressions include year dummies, border and border-year dummies, 2011 census controls, and the initial level of crime. Clustered standard errors at the tract level are used. *** significant at the 1% level. ** significant at the 5% level. * significant at the 10% level.

TABLE 10

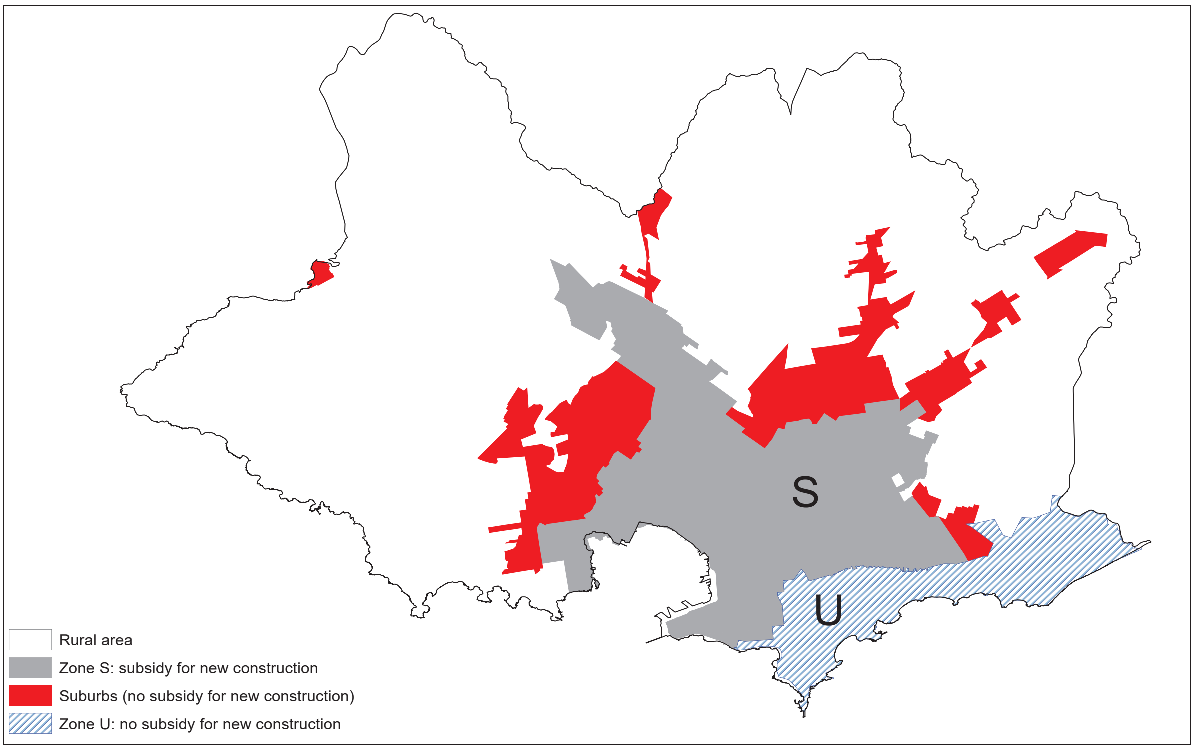
EFFECTS ON SOCIOECONOMIC OUTCOMES. CONTINUOUS DIFFERENCE-IN-DIFFERENCES ESTIMATOR.

	Household income PC		
	w/ rental value	w/o rental value	Rental value
	(i)	(ii)	(iii)
a) Continuous exposure measure			
ln(Intense) $\times$ post	0.011**	0.007	0.031***
	(0.005)	(0.005)	(0.006)
F-statistics	46.149	46.149	46.149
b) Binary exposure measure			
$\mathbb{1}[ln(\text{Intense}) > p50\text{th}] \times$ post	0.098**	0.058	0.262***
	(0.041)	(0.040)	(0.056)
F-statistics	33.754	33.754	33.754
Observations	42,610	42,610	42,610
Nbr. of clusters	236	236	236

Notes: In the first column, the dependent variable is the log of the disposable household income per capita with rental value, while in the second column, the rental value is not included. In the case of owned houses, the hypothetical self-reported rental value is assigned. The main independent variable is the investment exposure measure *Intense*, which depends on the decaying parameter λ. These estimates are produced using an exposure measure with $\lambda = 9$. *Intense* is instrumented by a binary variable that takes a value of one if the tract is located in the subsidized area and zero if the tract located in the unsubsidized area. All regressions include year-month dummies, border and border-year dummies, 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.

Figures

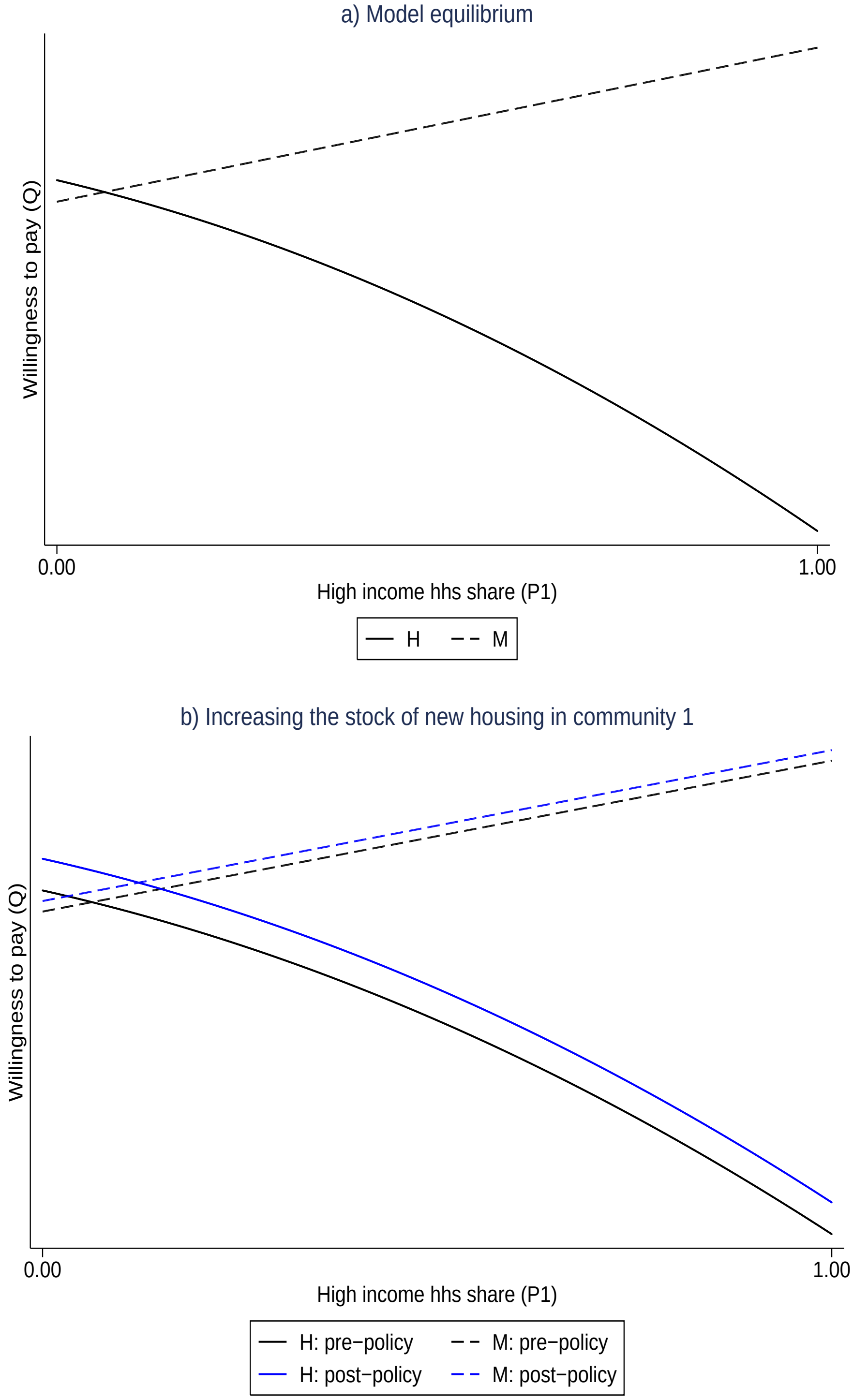
FIGURE 1
PLACE-BASED SCHEME FOR NEW CONSTRUCTION PROJECTS IN MONTEVIDEO (URUGUAY)



Notes: The policy was introduced in August of 2011. The subsidy for new construction projects only applies in the gray area S.

FIGURE 2

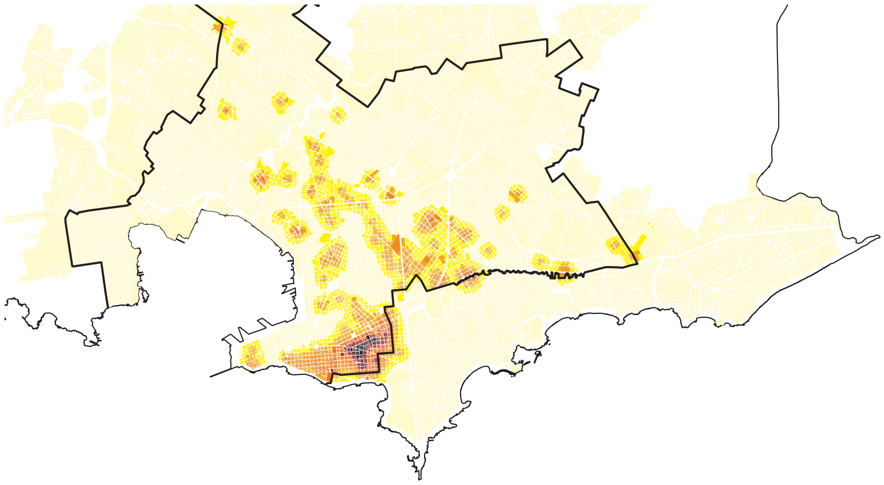
MODEL ILLUSTRATION



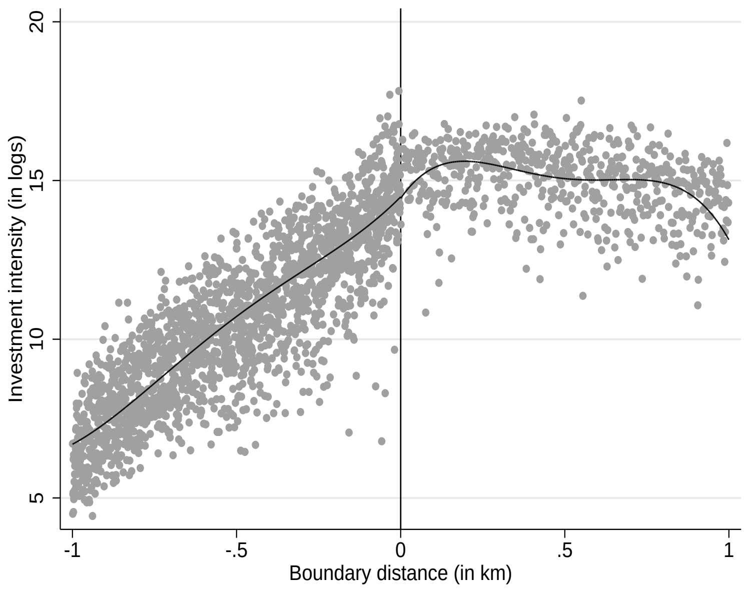
Notes: Panel 2 shows the model's equilibrium, which is determined by the intersection of the bid functions - solid line for high-income households and dashed line for middle-income households. Panel 2 shows the effect of increasing the stock of new housing in community 1 on P_1 and Q. The policy leads to an upward shift in both bid functions. At the new equilibrium, both the share of more affluent residents and the house prices in community 1 increase.

FIGURE 3
EXPOSURE TO SUBSIDIZED INVESTMENTS

(A) INVESTMENT INTENSITY MAP



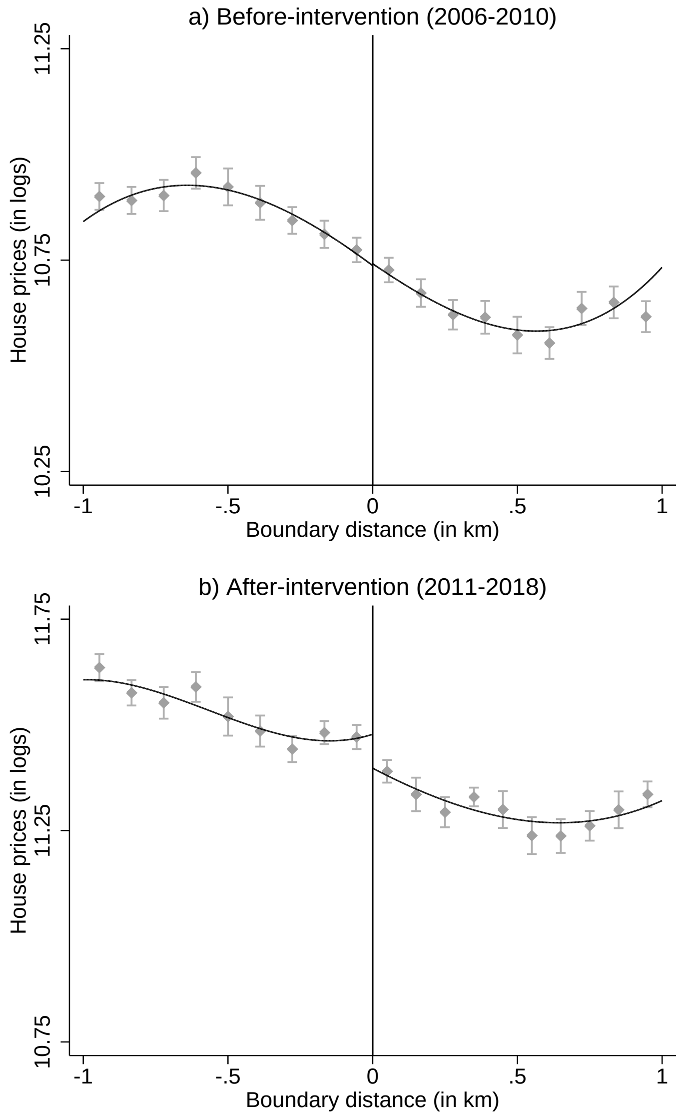
(B) LOG OF THE INVESTMENT INTENSITY AS A FUNCTION OF DISTANCE TO THE BORDER



Notes: This measure captures the level of investment intensity to which each sold unit i is exposed. It is built as the weighted sum of all project budgets using weights that follow an exponential decay scheme. Here, the decay rate $\lambda = 9$. In panel a), higher levels of investment intensity are represented with darker tones. In panel b), the solid lines are local linear fits to the plotted bins for the unsubsidized area (at the left of the border) and the subsidized area (at the right of the border), obtained using an Epanechnikov kernel function.

FIGURE 4

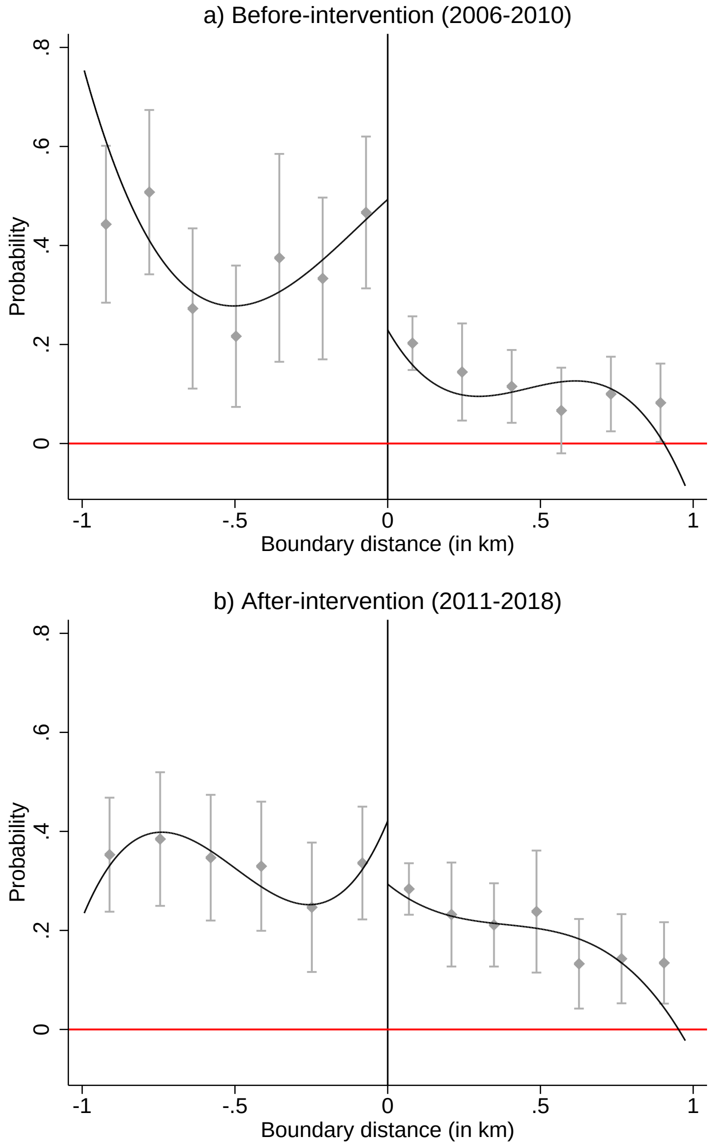
HOUSE PRICES AS A FUNCTION OF DISTANCE TO THE BORDER



Notes: The distance is normalized to be zero at the border; positive values correspond to the subsidized area S, while negatives correspond to the unsubsidized area U. Each evenly spaced bin represents the average house price (in log). Bins are constructed using the (IMSE-optimal) data-driven bandwidth selection criteria developed in Calonico, Cattaneo and Titiunik (2015, 2017), which allows confidence intervals for each bin. The solid line is estimated using a third-order polynomial and a triangular kernel function.

FIGURE 5

RESIDENTIAL DEVELOPMENTS AROUND THE BORDER

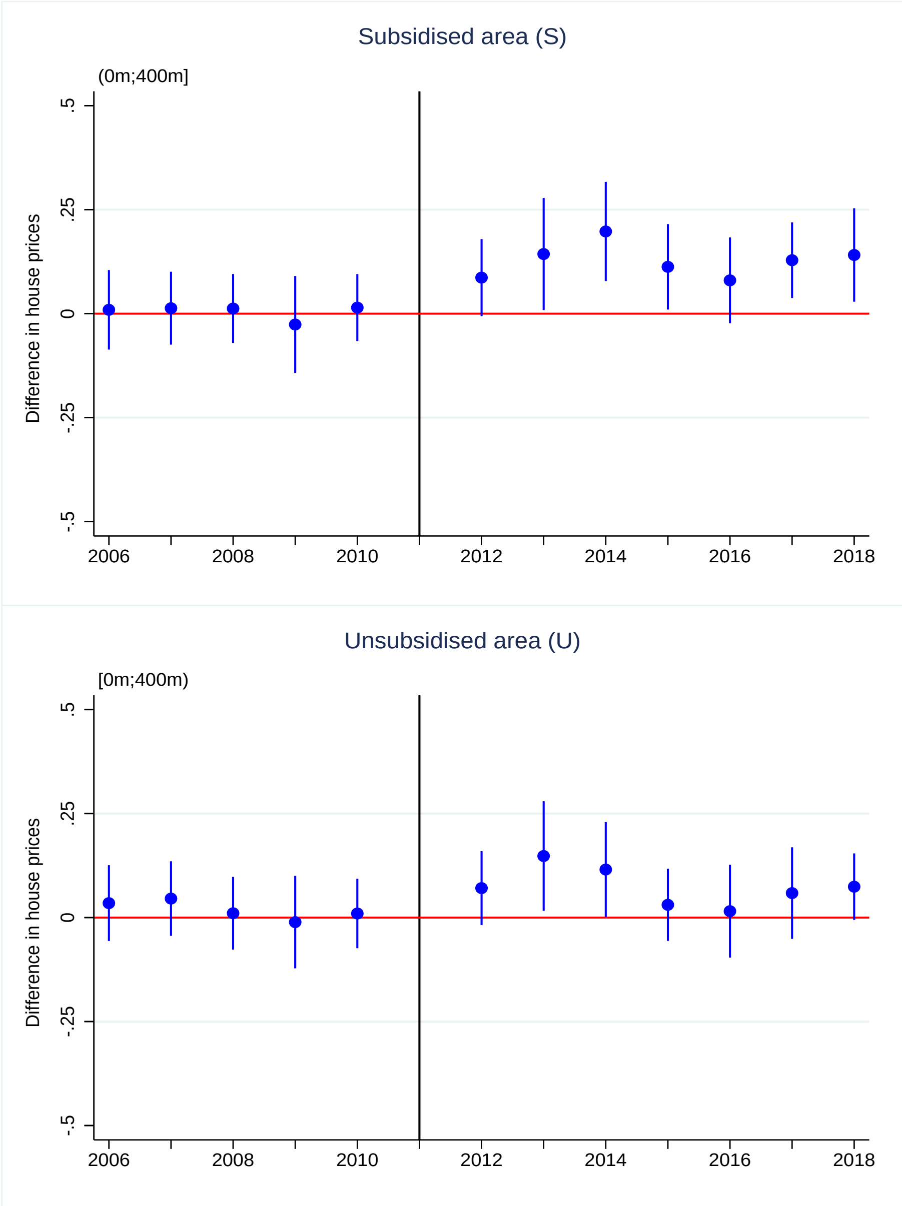


Notes: The distance is normalized to be zero at the border; positive values correspond to the subsidized area S, while negatives correspond to the unsubsidized area U. Each evenly spaced bin represents the share of tracts with residential developments. Bins are constructed using the (IMSE-optimal) data-driven bandwidth selection criteria developed in Calonico, Cattaneo and Titiunik (2015, 2017), which allows confidence intervals for each bin. The fitted line is estimated using a fourth-order polynomial and a triangular kernel function.

FIGURE 6

EVENT STUDY OF SPILLOVER ESTIMATES ON HOUSE PRICES

* DIFFERENCE-IN-DIFFERENCES STRATEGY USING 400-METER WIDE BANDS

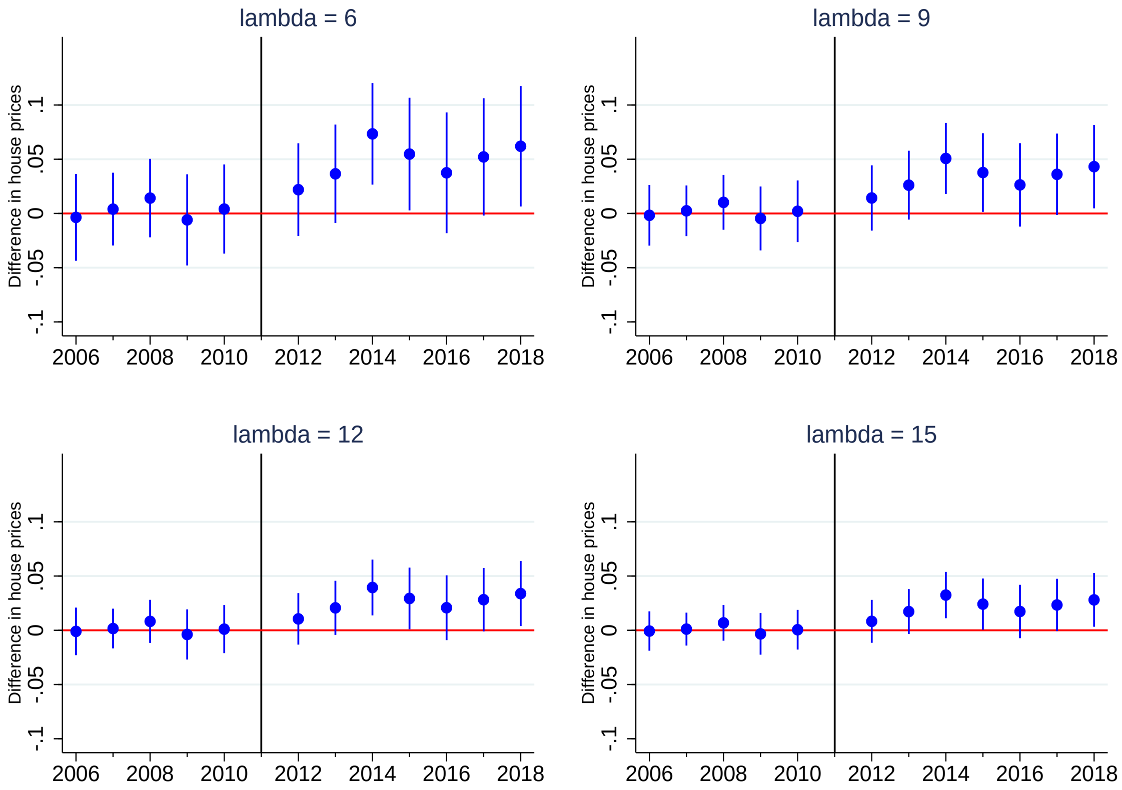


Notes: The dependent variable is the log of house prices. The comparison group is composed of sold units located in the unsubsidized area (U) that are between 400 and 800 meters from the border. All regressions include year-month dummies, border and border-year dummies, and housing characteristics. Clustered standard errors at the tract level are used to build 95% confidence intervals.

FIGURE 7

EVENT STUDY OF SPILLOVER ESTIMATES ON HOUSE PRICES

* CONTINUOUS DIFFERENCE-IN-DIFFERENCES ESTIMATOR

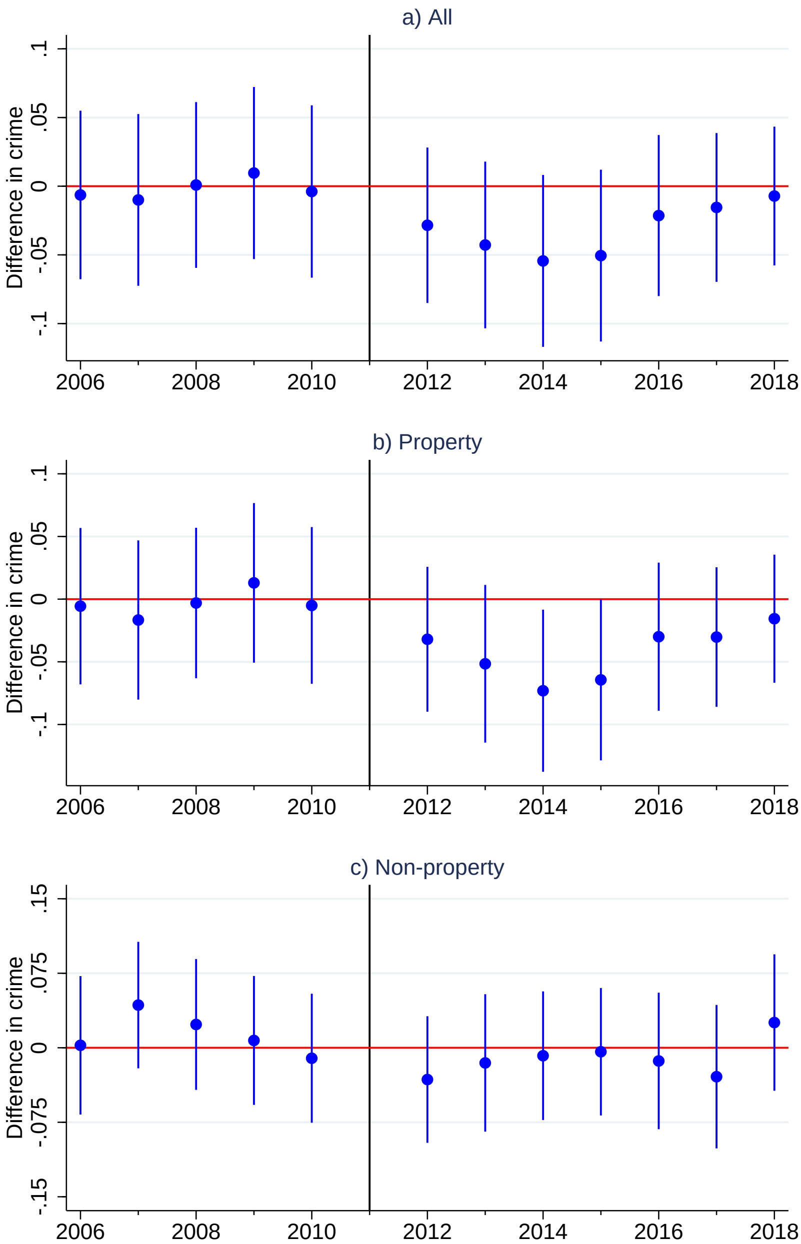


Notes: The dependent variable is the log of house prices. The main independent variable is the investment exposure measure *Intense*, which depends on the decaying parameter λ. These estimates are produced using an exposure measure with $\lambda = 9$. *Intense* is instrumented by a binary variable that takes a value of one for sold units in the subsidized area and zero for sold units in the unsubsidized area. All regressions include year-month dummies, border and border-year dummies, and housing characteristics. Clustered standard errors at the tract level are used to build 95% confidence intervals.

FIGURE 8

EVENT STUDY OF THE EFFECTS ON CRIME RECORDS

* CONTINUOUS DIFFERENCE-IN-DIFFERENCES ESTIMATOR

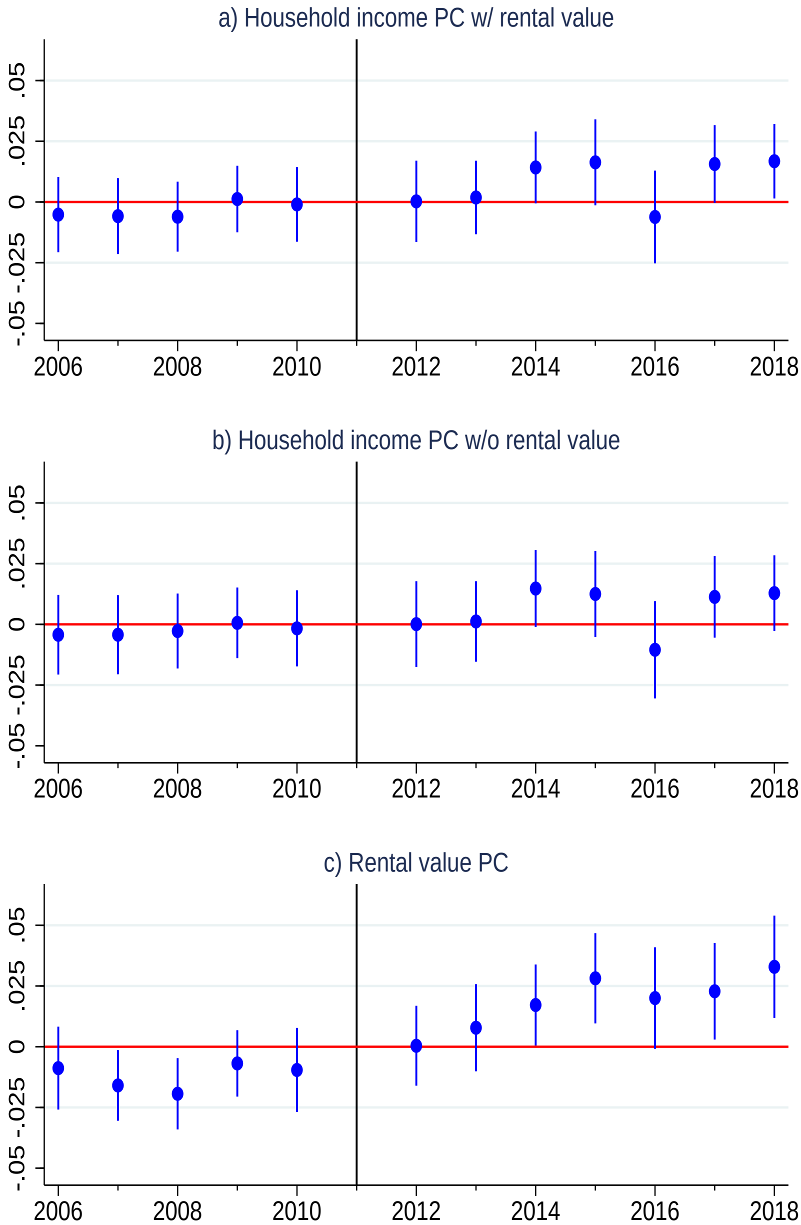


Notes: The dependent variable is the log of the yearly count of reported crimes at the tract level. Property crime includes theft and robbery, while nonproperty crime includes assault. The main independent variable is the investment exposure measure *Intense*, which depends on the decaying parameter λ. These estimates are produced using an exposure measure with $\lambda = 9$. *Intense* is instrumented by a binary variable that takes a value of one if the tract is located in the subsidized area and zero if the tract is located in the unsubsidized area. All regressions include year dummies, border and border-year dummies, and 2011 census controls. Clustered standard errors at the tract level are used to build 95% confidence intervals.

FIGURE 9

EVENT STUDY OF THE EFFECTS ON SOCIOECONOMIC VARIABLES

* CONTINUOUS DIFFERENCE-IN-DIFFERENCES ESTIMATOR



Notes: In panel a), the dependent variable is the log of the disposable household income per capita with rental value, while in panel b), the rental value is not included. Panel c) considers the log of the rental value. In the case of owned houses, the hypothetical self-reported rental value is assigned. The main independent variable is the investment exposure measure *Intense*, which depends on the decaying parameter λ. These estimates are produced using an exposure measure with $\lambda = 9$. *Intense* is instrumented by a binary variable that takes a value of one if the tract is located in the subsidized area and zero if the tract is located in the unsubsidized area. All regressions include year-month dummies, border and border-year dummies, and 2011 census controls. Clustered standard errors at the tract level are used to build 95% confidence intervals.

Appendices

A. Nonparametric estimates

I estimate the following hedonic semiparametric price equation by using the differencing approach developed by Yatchew (1997); Yatchew and No (2001):

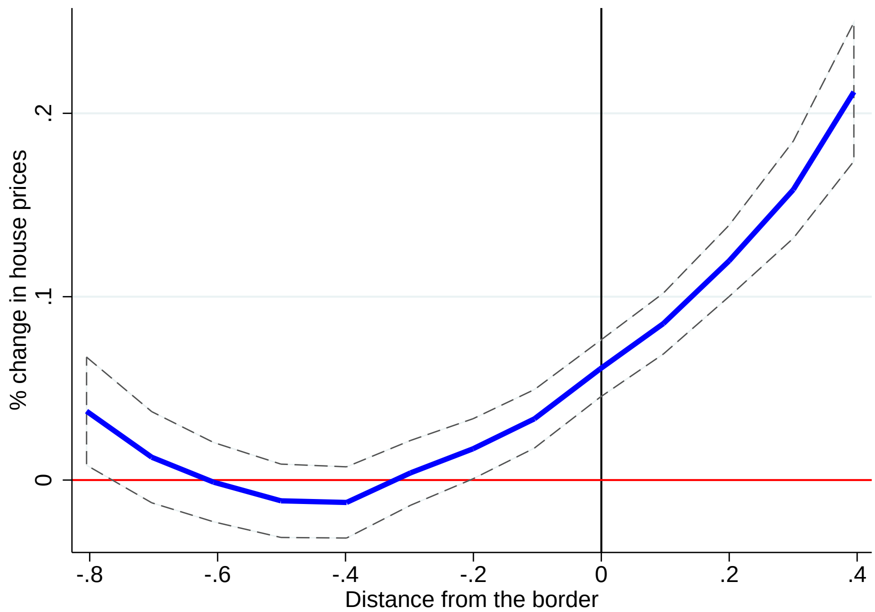
$$p_{i,b,t} = f_t(d_{i,b}) + X_i^{'}\theta + \delta_t + \delta_t \times \nu_b + \epsilon_{i,b,t} \tag{A.1}$$

$p_{i,b,t}$ is the logarithm of house prices of sold unit i at border-segment b in year-month t. I exclude sold units that were subsidized by the policy. $f_t(d_{i,b})$ is a nonparametric term that depends on the distance of each land lot to the border of the subsidized area with respect to the unsubsidized area, and it enables measurement of how local spillovers evolve when moving away from the border before and after the intervention. X_i contains housing characteristics, as well as socioeconomic variables at the tract level. Housing characteristics include constructed square meters (and its square), a set of dummies for year of construction, dummies for the quality of the building (as assessed by the cadaster agency), number of floors, a dummy for detached and semidetached houses, whether there is a garage, balcony or outdoor space, other amenities, and if there is a slum within 300 meters. As the tract level covariate, I consider the density of inhabitants per square km (in log), % of vacant and uninhabitable houses, % of renters, the unemployment rate, % of low-educated head of households, % of historic monuments, and a street quality index. δ_t denotes year effects and $\delta_t \times \nu_b$ border-year fixed effects.

The parametric and nonparametric components are assumed to be additively separable. The differencing approach of Yatchew (1997) is followed in order to obtain an estimate of the non-parametric part. The function $f_t(d_{i,b})$ is estimated separately for the pre-policy period and for the policy following the introduction of the policy. Then, I obtain $\widehat{f}_{\text{pre}}(d_{i,b})$ and $\widehat{f}_{\text{post}}(d_{i,b})$. The blue line in Figure A.10 shows the difference between $\widehat{f}_{\text{post}}(d_{i,b})$ and $\widehat{f}_{\text{pre}}(d_{i,b})$, which is normalized using the average estimated difference for the control band (defined as units located between 400 and 800 meters from the border in the unsubsidized area).

FIGURE A.10

NONPARAMETRIC SPILLOVER ESTIMATES ON HOUSE PRICES. INTENTION-TO-TREAT STRATEGY



Notes: The green line is the difference before and after the policy as a nonparametric function of the distance to the border. Estimates are obtained using a semiparametric approach developed by Yatchew (1997); Yatchew and No (2001). The parametric part includes year-month fixed effects, border-year fixed effects, housing characteristics and census block socioeconomic variables. Confidence intervals are constructed using the bootstrap standard error with 500 replications. The results are normalized to the average change in house prices in a control band defined as transacted units between -800 and -400 meters.

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