UNIVERSITAT DE BARCELONA

Essays on Urban Economics

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PhD in Economics

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To my beloved family

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Contents

1 Introduction

In modern economies, cities are a crucial element for the achievement of economic prosperity and welfare (Glaeser, 2008), their productive advantages explaining why people and firms tend to cluster in them, despite their associated congestion costs. Indeed, employing the well-documented evidence on agglomeration gains, a thought experiment to determine the impact of an increase in the size of a city would show a rise in average firm productivity (as measured by total factor productivity, TFP) and workers earnings (Combes and Gobillon, 2015).[1]. Even when moving to slightly denser urban areas, individuals retain part of the wage premium gained in more dense cities (De la Roca and Puga, 2017). However, as people and firms concentrate, urban costs also become more evident (e.g. traffic congestion and the lack of affordable housing) (Duranton, 2015), as do growing wage inequalities (Baum-Snow and Pavan, 2013).

Similar sized cities, however, present huge disparities in various dimensions, including hourly wages and TFP (Moretti, 2012). Moreover, such differences are also evident within the same city. Regardless of whether a city is vibrant or lagging, big or small, they all have neighborhoods marked by a high concentration of social problems. The economic status of neighborhoods (measured in terms of per capita income) tends to rise or fall (Rosenthal and Ross, 2015), leaving spatial inequalities as a common characteristic of all cities. In the US, as income is, in general, correlated with ethnic composition, deteriorated areas tend to be characterized by high shares of black population, following migration from rural to urban areas in the post-war period (Cutler et al., 1999). Although recent figures show a decline in segregation, minority groups continue to live in the more deprived city neighborhoods (De la Roca et al., 2014). This is specifically the case in the US of new arrivals of Latino population, who tend to concentrate in neighborhoods with fewer public services and lower amenity levels (De la Roca et al., 2017). Similarly, after recent immigration waves, ethnic segregation has become a striking feature of European cities (Tammaru et al., 2016), emerging as a central concern for policy-makers and as a significant subject of analysis among urban economists. For exam-

[1]Using aggregate data and correcting for different endogeneity sources, the elasticity of wages or TFP with respect to employment or population density ranges from .03 to .06, whereas when using individual data, this elasticity is about .02.

ple, comparing France, Germany and the UK, Algan et al. (2010) show that first- and second-generation immigrants have difficulties in accessing the labor market, suffering higher rates of unemployment and earning lower salaries than natives.

Governments struggle to design urban development policies that can curb spatial inequalities and reduce income segregation and which, ultimately, could deconcentrate clusters of minority populations. A notable effort in this direction are the EU regional policies funded through the European Regional Development Fund (ERDF), in which a “strengthened urban dimension and fight for social inclusion (...)” to support “marginalized communities” is one of its priorities for 2014-2020. To achieve this goal, it is critical to understand which policies drive urban development and social integration. The aim of creating more income diverse neighborhoods has been pursued through two different income-mixing strategies, which can either focus on people or places (Boustan, 2011). Applied to people, such measures mean providing the low-income population with resources that can help them gain access to more affluent neighborhoods, or, alternatively, encouraging more affluent residents to move into deteriorated neighborhoods. Applied to places, such measures seek the renewal of deteriorated neighborhoods in the hope of attracting more affluent residents or, in some cases, in the hope of attracting them back. This latter strategy has been described in the literature as comprising place-based policies[2], among which, urban revitalization programs form an important part. Indeed, to date, several studies have analyzed the effect of urban renewal policies; however, the focus has been almost exclusively on house prices (Rossi-Hansberg et al., 2010; Ahlfeldt et al., 2016; Koster and Van Ommeren, 2017; González-Navarro and Quintana-Domeque, 2016).

The second chapter of this thesis, therefore, proposes analyzing the effects of an urban renewal policy from a different perspective. Specifically, it studies whether a major renewal program implemented in Catalonia (Spain) between 2004 and 2010 has been able to reverse the ethnic dynamic that leads to a tipping point being reached in neighborhoods when natives move out. Some of the region’s most deprived neighborhoods received large investments in their public spaces and public facilities with the aim of attracting natives and high income individuals and of reducing the concentration of poverty and immigration. From a theoretical perspective, it is understood that natives’ residential choices depend, among other factors, on the level of exogenous neighborhood amenities available to them (i.e. urban infrastructure).

A residential choice model with social interactions generally embeds multiple equilibria each with a different level of neighborhood diversity. The question we

[2]See Neumark and Simpson (2015) for a detailed literature review.

seek to address is whether, in the context of deprived neighborhoods with rapidly growing minority communities, a substantial increase in the level of exogenous amenities (a big push) can lead to a more mixed and stable neighborhood equilibrium over time. To assess this question empirically, difference-in-difference specifications combined with the Oaxaca-Blinder estimator approach is used.[3] The treatment group includes completed projects, while the control group comprises rejected projects and projects that were accepted towards the end of the program but which, due to a fall in public revenues, were never executed. The results suggest that the urban renewal projects had little (if any) effect on population dynamics, suggesting that substantial investment in deprived neighborhoods is insufficient to attract natives and/or high income households. Interestingly, the sole exception to this were the interventions made in Barcelona's historic districts, where the policy seems to have boosted ongoing processes of urban revival within the city's most deprived neighborhoods, furthering processes of gentrification.

To enhance the design of income-mixing policies, it is important to study the drivers of residential segregation. In this regard, the literature has identified three compelling factors (Glaeser, 2008; Boustan, 2011; O'Flaherty and Sethi, 2015). First, since ethnic background is highly correlated with income, income sorting naturally leads to ethnic segregation. Despite being the main factor underpinning black segregation, Bayer et al. (2004) show that income only accounts for around 10% of the observed segregation in the San Francisco Bay Area. Furthermore, Bayer et al. (2014) find that a critical mass of higher income blacks is needed for the formation of middle-income black communities; otherwise, they are more likely to reside in middle-income white neighborhoods. Second, discrimination in both the housing and lending markets can be considered collective actions taken by majority groups to avoid minorities. However, legal reforms have limited taste-based discrimination against minorities.[4] Although it has been reduced, statistical discrimination in the housing market remains. Ondrich et al. (2003) find that the behavior of real estate agents varies with the ethnicity of the buyer. Moreover, other forms of prejudice, such as neighborhood discrimination, defined as customer discrimination that depends on the ownership structure within buildings, are likely to emerge (Combes et al., 2018).[5] Third, segregation might result from social interactions in housing demand as natives seek to avoid neighborhoods with a high proportion of minority residents. The seminal paper by Schelling (1971) shows how decentralized

[3] See Kline (2011) for further details of the Oaxaca-Blinder estimator.

[4] The 1968 Fair Housing Act in the US is one such example; the European non-discrimination law is another (see the Handbook on European Non-Discrimination Law for further details).

[5] These authors consider two different ownership structures: on the one hand, where one landlord owns the entire building; and, on the other, where various landlords own flats in the building.

individual actions can result in neighborhood tipping and the consequent absence of integrated neighborhoods. More recently, Card et al. (2008, 2011) have developed a framework with multiplicity of equilibria that allows for tipping but also for mixed neighborhoods. However, among these three main factors, the most important seems to be the flight of whites from racially mixed neighborhoods (Boustan, 2011).

Most of the literature examining such social interactions at the city level concerns studies undertaken in the US. This raises the following question: Are social interactions an important driver of residential segregation in the case of European cities? The third chapter of this thesis conducts an in-depth analysis of the role played by social interactions on neighborhood residential choices, at this neighborhood level (census tract), by different ethnic groups in the case of Spain.

The empirical study is guided by a simple residential choice model, based on Glaeser (2008) and Banzhaf and Walsh (2013), which embeds both exogenous and endogenous amenities. In this model, exogenous amenities are the neighborhood's characteristics, including such elements as the level of urban infrastructure, while endogenous amenities are defined as the size of the neighborhood's minority community. These correspond, respectively, to the first and third main drivers of residential segregation discussed above. Specifically, we consider two types of model depending on native preferences toward minority ethnic groups, that is, their social interactions. In the first model, characterized by a single mixed equilibrium, we do not include any social interaction effects. In the second model, these social interactions are included so that whenever endogenous neighborhood amenities are important, it allows for multiple equilibria as in the one-side tipping point model developed by Card et al. (2008).

The predictions of both model types vary according to the specific context described by the city-wide immigration. Under a massive wave of immigration, both models predict that neighborhoods with a high initial minority share experience a larger increase in the share of foreign-born population. If social interactions matter for the natives' residential choice, then this reinforces residential segregation driven by income sorting. After the immigration boom and during a period of immigration freeze, the two model types differ in their predictions regarding neighborhood ethnic dynamics. In a model without social interactions, a slight decrease in a neighborhood's minority share is predicted, pointing to a reversal in their ethnic dynamics. If, however, social interactions are relevant, neighborhoods that tip will experience further losses of native population and evolve toward a full-immigrant enclave in the long-run. This asymmetric response in the natives' behavior would provide further evidence in favor of one-side tipping point models and indicate that once the tipping point is surpassed the neighborhood dynamic is unlikely to be reversed.

The empirical analysis focuses on the six largest cities in Spain, a suitable setting for this study given that we can identify two quite distinct sub-periods between 2001 and 2015 in the country's immigration. The first is the Spanish immigration boom (2001-2009). During these boom years, neighborhoods with high initial minority shares experienced a sharp increase in their immigrant shares, as predicted by both types of model. In the second sub-period (2010-2015), when economic growth was at a standstill, immigrant inflows stopped, and the number of immigrants nationwide fell slightly. Indeed, tracts with high immigrant shares in 2010, experienced a reduction in their minority shares in the years that followed. Here, therefore, we analyze the following questions. First, do the neighborhoods in Spanish cities have high and stable minority shares in equilibrium as predicted by the model without any social interaction effects? Second, are natives still leaving Spain's high minority neighborhoods even though the minority population is no longer increasing? Seeking answers to these two questions can help us disentangle the two main drivers of ethnic segregation. The evidence provided by this chapter enables us to conclude that the tipping-like behavior of natives seems to be relevant to explanations of residential segregation in the Spanish case.

Tipping is just one among several possible drivers that might explain spatial and temporal patterns in neighborhood economic status. Another factor that helps explain neighborhood disparities in economic status and residential segregation patterns is durable housing (Rosenthal, 2008; Brueckner and Rosenthal, 2009). The age profile of the housing stock contributes to explain why low- and high-income households concentrate across different neighborhoods. Newly built housing generally attracts high-income residents. After a period of time, as housing ages, high-income dwellers shift towards communities with newer housing, leading to systematic cycles in neighborhood economic status Brueckner (2011).

Profit-driven developers target their investments in growing areas characterized by dynamic local housing markets. As a result, cities have areas that concentrate most of the housing investments, while others demonstrate the effects of urban decay with old, deteriorated housing stocks. Developers are unwilling to invest in these latter places and high-income households tend to avoid them because of this lack of housing investments. An extreme case is the city of Detroit, which has a lively central business district surrounded by vacant houses, showing a suboptimal urban structure (Owens III et al., 2017). Revitalization efforts aim to enhance the functionality of the areas from the perspective of their public spaces and mobility, as well as to attract commercial activity and resident populations capable of rejuvenating the local economy. Nevertheless, these interventions are costly and, so, policymakers need to know whether they are effective at achieving their intended goals. In this sense, the presence of housing externalities are important from a policy perspective since they could potentially justify such interventions.

The fourth chapter in this thesis sheds light on the externalities derived from a housing policy implemented in Uruguay that provides subsidies to residential developers and which has a place-based structure. The government opted to employ a private housing solution that targeted underinvested areas. Under this policy, developers were able to choose the locations they wanted to invest in within the subsidized area. Furthermore, development projects undertaken on land lots containing abandoned buildings or disused factories benefited from laxer requirements. This study focuses on spillover effects on house prices and on criminal records.

From a theoretical perspective, there are at least two channels via which these subsidized private investments might affect house prices. First, there is a potential direct effect - i.e. as the policy increases the level of amenities in the neighborhood, house prices are likely to rise. Second, there is a potential indirect effect - i.e. as the new residents are likely to be from higher-income strata, the policy will probably change the neighborhood's income composition, thus potentially producing a further increase in house prices. In the case of crime, the empirical evidence shows that the presence of vacant and abandoned buildings not only depresses housing prices (Campbell et al., 2011) but also encourages criminal activities in the neighborhood (Spelman, 1993).

The empirical analysis focuses on new construction projects executed between 2011 and 2013 in the Uruguayan capital city (Montevideo), where the subsidy forms part of a place-based scheme. Spillovers are estimated taking advantage of the high concentration of projects undertaken close to the border separating the subsidized from the unsubsidized areas. While average prices were higher in the unsubsidized areas, the fact that pre-treatment housing prices remained smooth at the border indicates that there were no major cross-border differences in their respective levels of amenities. Two estimation strategies are used. The first is a difference-in-differences strategy that exploits house price changes in different narrowly defined bands before and after policy implementation. Clear evidence of spillovers is found by using this strategy. However, as developers could choose where to invest from among various locations along the subsidized border we also conducted an intention-to-treat analysis. Accordingly, the second strategy uses an investment exposure measure that captures the developers' investment location. Using a continuous but endogenous treatment measure, the spillover hypothesis is also confirmed. An examination of criminal records shows that the property crime rate seems to decrease at the border, but there is no evidence of a decrease in the non-property crime rate. Place-based subsidized housing appears to be revitalizing deteriorated neighborhoods, albeit at the cost of leaving behind less affluent areas not targeted by developers.

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2 Can Urban Renewal Policies Reverse Neighborhood Ethnic Dynamics?[§]

2.1 Introduction

There are large differences in income across cities (Moretti, 2012); yet, at the same time, all cities, be they vibrant or lagging, present evidence of substantial inequality across their neighborhoods. Indeed, it would appear that in some regions of the world income differences are greater within than they are across cities. For example, Rosenthal and Ross (2015) report that while the interquartile range in the distribution of city income in the US is about 25%, the corresponding figure for neighborhood income within these cities is as high as 55%. Moreover, evidence suggests that within city income inequality is increasing both in the US (Watson, 2009) and in Europe (Tammaru et al., 2016). Partly because income correlates with racial and ethnic backgrounds, and partly due to social interactions in relation to the demand for housing (Card et al., 2008), a salient feature of the urban landscape is residential segregation along lines of race and ethnicity (Cutler et al., 1999; Boustan, 2011).

Various reasons can be forwarded for seeking to reduce income and ethnic segregation at the city level. The first corresponds to reasons of local externalities. The socioeconomic characteristics of neighborhoods have been found to affect individual performance in both education (Chetty et al., 2016) and the labor market (Bayer et al., 2008; Chetty et al., 2016) and to impact both criminal behavior (Damm and Dustmann, 2014) and welfare use (Åslund and Fredriksson, 2009). Exploiting variation in racial segregation across US cities, Cutler et al. (1999) and Ananat (2011) further show that minorities perform worse in more segregated cities. Hence, residential segregation could contribute to amplify performance differences between income and ethnic groups. Second, multiple equilibria are a prominent feature of

[§]The paper in this chapter is coauthored with Elisabet Viladecans-Marsal and Jordi Jofre-Monseny.

neighborhood ethnic composition (Schelling, 1971; Card et al., 2008) and, thus, public policies aimed at deconcentrating poverty can potentially be welfare enhancing.

Policies aimed at deconcentrating poverty and minority population groups abound in OECD countries (Cheshire, 2009). These initiatives, often known as income-mixing policies, fall into two broad categories (Boustan, 2011). The first encompasses policies that encourage low income households to move into more affluent neighborhoods and include, for example, the Section 8 and the Moving to Opportunity programs in the US. The second category consists of policies that seek to improve deprived neighborhoods so as to attract higher income individuals. Major programs falling into this category include the HOPE VI and the Community Development Block Grant in the US and the URBAN projects in the EU[1]. While the reasons outlined above are used to justify measures aimed at reducing segregation, income-mixing policies are controversial for three reasons (Cheshire, 2009)[2]. First, neighborhood effects are much smaller than the raw correlations suggest (Topa and Zenou, 2015). Second, if income-mixing policies work, investing in deprived neighborhoods will increase housing prices and cause gentrification, suggesting that such policies might end up hurting (rather than helping) the low income residents of the targeted neighborhoods. Finally, while income-mixing policies are costly, the effectiveness of such initiatives is unclear. The goal of this paper, therefore, is to assess the effectiveness of place-based policies that invest in deprived neighborhoods in order to deconcentrate poverty and immigration. To this end, we evaluate the effects on population dynamics at the neighborhood level of a prominent place-based policy implemented in the Spanish region of Catalonia.

The Catalan Neighborhoods Act was passed in 2004 by the regional government with the objective of deconcentrating poverty and immigration through the improvement of public spaces and public use facilities in some of the region's most deprived neighborhoods. The policy was adopted in the middle of the Spanish immigration wave, when the share of immigrant population was rising rapidly, especially in low income urban neighborhoods (Fernández-Huertas Moraga et al., 2017). In the areas we study, immigration rose by more than 8.5 percentage points in the three-year period preceding the start of the policy. The intervention was implemented through annual calls for proposals, with an annual budget of 99 million EUR between 2004

[1]Sweden and Denmark, in a policy more explicitly related to ethnic enclaves, adopted refugee dispersal measures in the late 80s to reduce the concentration of immigrants in specific neighborhoods and urban areas (Edin et al., 2003; Damm and Dustmann, 2014). Around the same time, Singapore introduced ethnic quotas at the neighborhood level, limiting housing transactions that could further increase segregation (Wong, 2013).

[2]An additional cost of income-mixing policies might be the disruptive effects of mobility on teenagers (Gibbons et al., 2017; Chetty et al., 2016).

and 2010. However, note that due to the fall in public sector revenues and the project length (4 years), the degree of execution was low among those projects accepted in the 2008-2010 calls for funding. For this reason, we focus on the 39 interventions corresponding to the 2004-2007 calls, with an average investment of 3,065 EUR per inhabitant. The intervention areas present an average population of 13,000 inhabitants, reflecting one of the policy guidelines, namely that of concentrating large investments in specific locations.

The selection process consisted of two rounds. In the first round, a deprivation index was calculated for each application made, using 20 socioeconomic indicators of neighborhood characteristics. Neighborhoods scoring above a certain threshold were included in the second round, when the projects were ranked according to a final score determined by this deprivation index (with a weight of 40%) and an assessment of the projects' characteristics. Our pool of control neighborhoods comprises the rejected projects and the projects accepted in the 2008-2010 calls that were never executed. As the most deprived neighborhoods were treated first, the projects accepted at the initial calls differ significantly from the control neighborhoods. Moreover, these differences translate into differential pre-treatment trends in neighborhood population dynamics, making it inappropriate to apply standard differences-in-differences estimators. We therefore adopt the Oaxaca-Blinder estimator, as developed by Kline (2011) and as recently used in Busso et al. (2014) and Kline and Moretti (2014), to evaluate place-based policies. Interestingly, here, we have access to all the neighborhood indicators used by the policy-makers when selecting the treated, which reduces the risk that unobserved neighborhood characteristics confound our treatment estimates.

The results suggest that the urban renewal projects had no effects on the population dynamics of the intervention areas, indicating that substantial investment in public spaces and facilities is insufficient to attract native and/or high income households to deprived neighborhoods. The one notable exception are the urban renewal projects carried out in the historic districts of Barcelona, the one large metropolitan area in the region. Here, the policy attracted native and EU15 college graduates. Overall, the policy reduced the share of non-EU15 immigrants by more than 5 percentage points and increased the share of population with a college degree by 16 percentage points. Since the late 1990s, the Barcelona city center has experienced an urban revival process and our findings suggest that the Neighborhoods Act intensified this process in some of the city's most deprived historic districts.

To guide the empirical findings, we develop a residential choice model with two neighborhoods and two population groups: natives and (low-income) immigrants. Native and immigrant neighborhood valuations rise in line with increased urban renewal investment, and the two groups present idiosyncratic residential preferences with regard to neighborhoods. Additionally, native utility is decreasing with the (square of the) immigrant share in the neighborhood, giving rise to multiple equilibria in a one-sided tipping model to use the terminology of Card et al. (2008). In mixed equilibria, investment in the neighborhood increases native willingness to pay more than immigrants can pay and, so, the place-based policy reduces the immigrant share in the neighborhood. However, when the minority share lies beyond the tipping point, urban renewal investment might be insufficient to attract natives to the neighborhood and the policy will have no impact on the neighborhood's ethnic composition. Our results are consistent with this latter scenario in which the urban renewal policy is unable to reverse the tipping process.

Rossi-Hansberg et al. (2010), Ahlfeldt et al. (2016) and Koster and Van Ommeren (2017) estimate housing externalities arising from urban renewal policies. Rossi-Hansberg et al. (2010) study a policy implemented in Richmond (Virginia) while Ahlfeldt et al. (2016) examine interventions in Berlin following re-unification. Using housing prices for renovated and non-renovated dwellings, these two studies estimate housing externalities in terms of the extent to which the value of a property depends on the quality of the nearby housing stock. While both papers analyze policies aimed at renovating the private housing stock, Koster and Van Ommeren (2017) analyze a Dutch program that targeted the public housing stock. They estimate the housing externalities caused by the policy in terms of both prices and sales times. Our paper differs from these studies in two respects. First, instead of estimating housing externalities, we assess the effectiveness of urban renewal measures for reducing income and ethnic segregation in cities, the primary goals of these policies. Second, while the above papers study programs targeting the housing stock, we examine an intervention that improved public spaces and public use facilities.

Our paper is closely related to Baum-Snow and Marion (2009) and Diamond and McQuade (2018), who study the effects of affordable housing developments in the US financed by the Low Income Housing Tax Credit (LIHTC). Although this policy is not strictly an urban regeneration program, some of the investments target disadvantaged neighborhoods. When focusing on developments in the least attractive locations, Baum-Snow and Marion (2009) find that these interventions increase local housing prices, reflecting either *better* neighbors or the conversion into new housing units of vacant buildings or unsightly empty lots. Similarly, Diamond and McQuade (2018)'s findings indicate that developments in disadvantaged areas cause a rise in local housing prices as well as in the income and the non-minority share in the neighborhood[3]. The results in this latter paper suggest that building affordable

[3] In contrast, Baum-Snow and Marion (2009) and Diamond and McQuade (2018) find that LIHTC developments in more affluent neighborhoods decrease housing prices, reflecting a lower willingness to pay to live with lower income neighbors.

housing in disadvantaged areas can be a more effective tool for reducing income and ethnic segregation than simply improving the public spaces and facilities of these areas.

Our paper is also related to the literature (surveyed in Neumark and Simpson (2015)) that analyzes the effects of enterprise zones, focused mainly on the US and France[4]. Although enterprise zones are also place-based policies that target deprived neighborhoods, they differ from the urban renewal policy studied here in that they focus on tax incentives aimed at boosting local employment.

The paper is structured as follows. In section 2 we develop a theoretical framework to understand the potential effects of urban renewal policies on the composition of the population in a neighborhood. Section 3 describes the urban renewal policy studied here, while section 4 introduces the data and the variables used. In section 5 we explain the empirical approach adopted in the paper. The results are presented in section 6 and section 7 concludes.

2.2 Theoretical framework

In this section, we develop a residential choice model with two neighborhoods and two population groups (natives and immigrants). The model is a variant of those developed in Banzhaf and Walsh (2013) and in Glaeser (2008) to examine racial segregation. The model's key elements include idiosyncratic residential preferences for neighborhoods, social interactions in housing demand (endogenous amenities) as well as an exogenous amenity, whose value increases with neighborhood investments. We use the model to study the neighborhood population responses to an urban renewal policy.

2.2.1 Model set-up

There are two neighborhoods in the city. The size of neighborhoods 1 and 2 are fixed and given by S and $1-S$, respectively. In turn, there are two population groups, natives (N) and immigrants (I), respectively. Each individual consumes one unit of housing and overall city population is normalized to one, with P denoting the city-wide immigrant share. P_1 and P_2 are the immigrant shares in neighborhoods 1 and 2, with $P = SP_1 + (1-S)P_2$. We restrict our analysis to equilibria in which $P_1 \geq P_2$. We further assume that $S \leq P < 0.5$, to allow the possibility that neighborhood 1 becomes a ghetto ($P_1 = 1$).

[4]Studies of enterprise zones in the US include Hanson (2009), Neumark and Kolko (2010), Hanson and Rohlin (2013) and Busso et al. (2014); while studies of the French experience include Gobillon et al. (2012), Givord et al. (2013), Briant et al. (2015) and Mayer et al. (2015).

If residing in community 1, natives' utility is given by $U_1^N = Y^N - Q - \alpha(P_1) + \gamma^N G + a_n$, where Y^N is income and Q is the price of housing in the neighborhood. The term $\alpha(P_1)$ reflects that natives care about the ethnic composition of the neighborhood. We return to the nature of the function α below. Natives also value the exogenous amenity G where $\gamma^N > 0$. The amenity value is assumed to increase as a consequence of urban renewal investments. The only individual-specific utility component is a_n, which reflects the attachment of individual n to neighborhood 1, and it is uniformly distributed in the unit interval, i.e. $a_n \sim U(0,1)$. In neighborhood 2, the values of Q and G are normalized to zero, implying that natives' utility in neighborhood 2 is given by $U_2^N = Y^N - \alpha(P_2)$.

Similarly, immigrants' utilities in neighborhoods 1 and 2 are $U_1^I = Y^I - Q + \gamma^I G + a_i$ and $U_2^I = Y^I$, respectively, with $a_i \sim U(0,1)$. Note that unlike natives, immigrants' utility does not depend directly on the neighborhood ethnic composition.[5] This assumption is based on evidence that suggests racial segregation is (largely) driven by the desire of non-minority residents to avoid neighborhoods with a high minority share (Cutler et al., 1999; Card et al., 2008; Boustan, 2011). We further assume that the natives' income is higher than that of immigrants ($Y^N > Y^I$) and that $\gamma^N > \gamma^I \geq 0$, as higher income is associated with a higher willingness to pay for amenities (Kuminoff et al., 2013; Kahn and Walsh, 2015)[6].

To study the effects of investment on the population composition of the neighborhoods, we focus on the minority share in neighborhood 1, which fully determines the proportions of both groups in the two neighborhoods[7]. There are two types of equilibrium. In a mixed equilibrium, neighborhood 1 is inhabited by both natives and immigrants ($P_1 < 1$) while in a ghetto, neighborhood 1 only hosts immigrants ($P_1 = 1$).

Note that there are always immigrants in the two neighborhoods, implying that there is an immigrant who is indifferent between neighborhoods, with a willingness to pay for neighborhood 1 given by $Q(a_{i^*}) = \gamma^I G + a_{i^*}$. In a mixed equilibrium, there is also an indifferent native, whose willingness to pay to live in neighborhood 1 amounts to $Q(a_{n^*}) = -\alpha(P_1) + \alpha(P_2) + \gamma^N G + a_{n^*}$. In equilibrium, the two marginal residents' willingness to pay must be equal, i.e. $Q(a_{i^*}) = Q(a_{n^*})$. Solving this equation gives us the equilibrium immigrant share and housing price in

[5] Relaxing this assumption does not change the main predictions of the model. Specifically, assuming that immigrants like neighborhoods with higher immigrant shares lead to an immigrants' willingness to pay curve for neighborhood 1 in Figure 2.1(a) that is U-shaped. Therefore, the predictions of the effects of the policy on the neighborhood's minority share remains unchanged.

[6] Koster et al. (2016) provide empirical evidence regarding this assumption for the case of historic amenities in Dutch cities.

[7] Note that $P_2 = (P - S\,P_1)/(1-S)$, while the proportion of natives in neighborhoods 1 and 2 are $N_1 = 1 - P_1$ and $N_2 = 1 - P_2$, respectively.

neighborhood 1[8]:

$$P_1 = P + \frac{P(1-P)}{S}\left(\alpha(P_1) - \alpha(P_2) + (\gamma^I - \gamma^N)G\right) \quad (2.1)$$

and:

$$Q = 1 - S + (1-P)\left(-\alpha(P_1) + \alpha(P_2) + \gamma^N G\right) + P\gamma^I G \quad (2.2)$$

If we assume that α is a linear function with $\alpha > 0$, then there is a unique mixed equilibrium. This equilibrium will be stable if social interactions are moderate. Specifically, the equilibrium will be stable if $\alpha P(1-P) < S(1-S)$. Otherwise, the mixed equilibrium is unstable and the neighborhood will be either in $P^1 = 0$ or in $P^1 = 1$[9]. If the mixed equilibria is unstable, then the resulting outcome is a two-sided tipping model to the use Card et al. (2008)'s terminology. In a two-sided model of this type, neighborhoods to the left of the mixed equilibrium evolve towards $P^1 = 0$ while those that to the right of it evolve towards $P^1 = 1$. This type of equilibrium is inconsistent with the neighborhood dynamics documented in Card et al. (2008) and in Card et al. (2011). In fact, the evidence supports one-sided tipping models, which predict that mixed neighborhoods are dynamically stable for low minority shares but that they quickly evolve towards a full minority equilibrium ($P^1 = 1$) once they surpass the tipping point.

In order to obtain a one-sided tipping model, we assume that α is quadratic, i.e. $\alpha(P_i) = \alpha P_i^2$ with $\alpha > 0$.[10] If social interactions are sufficiently strong, i.e. α is sufficiently large, the model can present two mixed equilibria. Then, the first of these (with the lower minority share) will be stable while the second will be unstable, as illustrated in Figure 2.1(a)[11]. The solid line is the natives' willingness to pay curve. When the minority share is low, the willingness to pay increases with

[8] We have used the fact $a_{i^*} = 1 - S(P_1/P)$, $a_{n^*} = 1 - S((1-P_1)/(1-P))$ and $P_2 = (P - SP_1)/(1-S)$.

[9] Specifically, if α is a linear function it turns out that

$$P_1 = P + \left(\frac{(1-S)P(1-P)}{S(1-S) - \alpha P(1-P)}\right)\left(\gamma^I - \gamma^N\right)G$$

[10] We obtain the same qualitative results if instead we assume that natives prefer to live in neighborhoods with a minority share that is not too different from that of the city-average, i.e. $\alpha(P_i) = \alpha(P_i - P)^2$.

[11] Formally, note that expression 2.1 is a function that maps into itself, i.e. $P_1 = H(P_1)$, where mixed equilibria are the values of P_1 in which H intersects the 45 degree line. If H crosses the 45 degrees line from above (below), then $H' < 1$ ($H' > 1$) and the equilibrium is stable (unstable). If $\alpha(P_i)$ is quadratic, then H is an increasing and convex function of P_1. If α is sufficiently low, the model has one mixed (and stable) equilibrium. If α is sufficiently high, then two mixed equilibria arise with the first (second) equilibrium being stable (unstable). The stability condition, i.e. $H' < 1$, implies $S/(P(1-P)) > (2\alpha P_1 + 2\alpha P_2(S/(1-S)))$.

P_1 as preference heterogeneity dominates the social interaction effect. However, for higher minority shares, the social interaction becomes relatively more important and the curve slopes downwards. Since immigrants' willingness to pay curve (dashed line) slopes downwards due to preference heterogeneity, two mixed equilibria arise. Consider the low minority share equilibrium depicted in point A. If a shock decreases the minority share, the immigrants' willingness to pay curve exceeds that of natives and equilibrium is restored. Similarly, if there is a shock that increases the minority share, the natives' willingness to pay curve exceeds that of immigrants, subsequently reducing the minority share. Hence, point A equilibrium is stable. The opposite is true in the second equilibrium (point B), where any shock takes the minority share away from the initial equilibrium.

Models that feature social interactions in housing demand typically exhibit multiple equilibria. Here, note that point C in Figure 2.1(a) is also a stable equilibrium in which neighborhood 1 only hosts immigrants ($P_1 = 1$). In such an equilibrium, no native wants to enter the neighborhood, which is guaranteed by the following condition:

$$\frac{S}{P} - \alpha(1) + \alpha(P_2) + (\gamma^N - \gamma^I)G < 0 \tag{2.3}$$

The housing price is determined by the marginal immigrant. Specifically:

$$Q = 1 - \frac{S}{P} + \gamma^I G \tag{2.4}$$

Finally, point D in Figure 2.1(a) is the model's tipping point, which is the highest minority share that can sustain a stable equilibrium[12]. With α quadratic, the tipping point (${P^t}_1$) is:

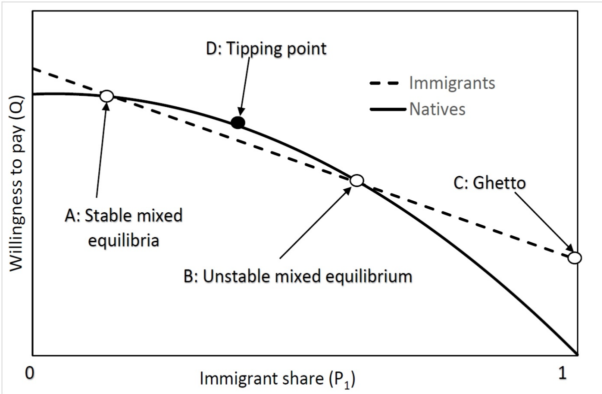
$${P^t}_1 = \frac{1}{1-2S}\left(\frac{S(1-S)^2}{2\alpha P(1-P)} - SP\right) \tag{2.5}$$

No stable neighborhood can exist between point D (the tipping point) and $P_1 = 1$, implying that neighborhoods to the left of the tipping point are transitioning towards the ghetto equilibrium.

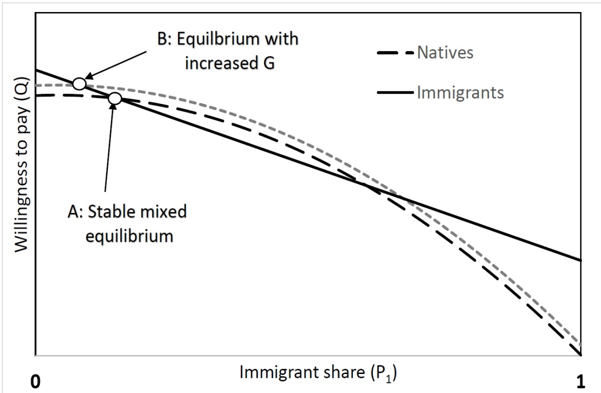
[12]Formally, the tipping point satisfies $H' = 1$.

Figure 2.1: Model illustrations

(a) Immigrant share equilibria

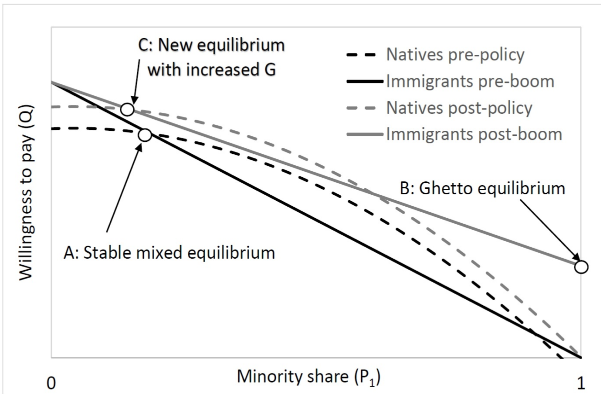


(b) The effect of the urban renewal policy on the minority share



The urban renewal policy is modelled as an increase in the neighborhood's amenity ($\uparrow G$). In this illustration, we assume that $\gamma^I = 0$, implying that increasing G does not affect the immigrants' willingness to pay. B is the post-intervention new minority share.

(c) Urban renewal policy and tipping reversal



Starting from A, there is an increase in the city-wide minority share P, shifting immigrants' willingness to pay upwards (gray solid line). As a result, B is the new ghetto equilibrium. The urban renewal policy is then implemented ($\uparrow G$) which shifts natives' willingness to pay upwards (gray dashed line). In this illustration, we assume $\gamma^I = 0$, implying that higher G does not shift immigrants' willingness to pay. Hence, the post intervention equilibrium is point C.

2.2.2 Impact of urban renewal policy on neighborhood ethnic composition

An urban renewal policy increases the neighborhood amenity level G; however, the effects of the policy will differ depending on the nature of the equilibrium considered. We first study the policy effects in a mixed (and stable equilibrium). Then, we study the impacts of investing in a ghetto. Finally, we discuss the role of the urban renewal policy as a "big push" policy that might reverse the dynamics of neighborhoods that are tipping.

In a mixed (and stable equilibrium), the effects of increasing G on the minority share and housing price of neighborhood 1 are given by:

$$\frac{dP_1}{dG} = \frac{\gamma^I - \gamma^N}{\frac{S}{P(1-P)} - \left(2\alpha P_1 + 2\alpha P_2 \frac{S}{1-S}\right)} < 0 \tag{2.6}$$

and

$$\frac{dQ}{dG} = \left((1-P)\gamma^N + P\gamma^I\right) - (1-P)\left(2\alpha P_1 + 2\alpha P_2 \frac{S}{1-S}\right)\frac{dP_1}{dG} > 0 \tag{2.7}$$

Expression 2.6 indicates that investments in the neighborhood decrease the neighborhood minority share. To see this, note that the numerator is a negative term as the natives' willingness to pay for G exceeds that of immigrants ($\gamma^I < \gamma^N$). In turn, the denominator is a positive term. In fact, this is the same condition that guarantees that the mixed equilibrium is stable (see footnote 11). In terms of the housing price, expression 2.7 shows that investing in the neighborhood increases it. The first part of the expression is the city-level (weighted) average willingness to pay for G, i.e. $(1-P)\gamma^N + P\gamma^I$. The second part indicates that higher G decreases the proportion of immigrants in the neighborhood, which further contributes to the increase in the price of housing. Hence, the housing price increase exceeds the willingness to pay for G as it also incorporates the value that natives attach to the shift in the neighborhood's ethnic composition.

Figure 2.1(b) illustrates the effects of such an intervention in the specific case where $\gamma^I = 0$[13]. Starting from a stable mixed equilibrium (point A), investing in the neighborhood increases the natives' willingness to pay to live in neighborhood 1, which shifts from the dashed to the dotted curve. In the new equilibrium (point B), the minority share is lower and the price of housing is higher. The arrival of native residents and the increase in housing prices in the intervened neighborhoods are

[13] If $\gamma^I = 0$, then increasing G does not affect the immigrants' willingness to pay.

actually among the policy's stated goals. This is also why income mixing policies are controversial. Investments in the neighborhood might eventually harm the low income residents of the treated neighborhoods. Note that higher amenities generate a price increase that exceeds the immigrants' willingness to pay. In our model, increasing G reduces immigrants' welfare as inframarginal immigrants in neighborhood 1 experience a housing price increase that exceeds the direct utility gain of higher amenities[14].

The effects of the policy, however, may differ greatly if the minority share is very high in the first place. In fact, in a ghetto equilibrium, small increases in G have no impact on the minority share in the neighborhood as long as condition 2.3 continues to hold. This suggests that once a neighborhood minority share is high, investments in the neighborhood are unlikely to attract natives since the value that they attach to a high immigrant share offsets any utility gain from a higher G. Despite there being no change in the ethnic composition of the neighborhood, the fact that there is a marginal immigrant implies that higher G increases the price of housing. Specifically, inspecting 2.7 reveals that rasing G by one unit increases the price of housing by γ^I units, which is the immigrants' willingness to pay for G.

An important feature of urban renewal policies is that they are not intended to be marginal interventions. Rather, the purpose of such policies is to bring about substantial changes in targeted neighborhoods. Consider a neighborhood that is in a stable mixed equilibrium such as point A in Graph 2.1(c) where, again, we have assumed $\gamma^I = 0$. Suppose that there is an increase in the city-level minority share (P increases), which reflects the context in which the Neighborhoods act was passed. As a consequence, the immigrants' willingness to pay curve moves upwards (solid gray line) and, as a result, this curve (solid line) exceeds that of the natives for all values of P_1. This neighborhood has tipped, and starts to lose natives until it reaches point B where $P_1 = 1$. Card et al. (2008) have shown that unstable neighborhood dynamics are not rapid. Hence, urban renewal policies that are implemented when tipping has already started but has not yet been finalized can restore the neighborhood's stability. Specifically, investing in the neighborhood will shift the natives' willingness to pay curve upwards (gray dashed line). Hence, the policy might bring the neighborhood back to the left of the tipping point (equilibrium C in this example). This seems to be the rationale underpinning the Neighborhoods Act, as the policy's goal was to deconcentrate poverty and immigration in a context where the immigrant share in deprived areas was growing rapidly.

[14]Of course, this model overstates the negative welfare effects of the policy since homeowners in the neighborhood would be protected from the housing price increase.

2.3 The urban renewal policy: the Neighborhoods Act

The regional government of Catalonia introduced the Neighborhoods Act (*Lei de Barris*) in 2004, with the aim of revitalizing neighborhoods deserving of 'special attention'. By means of massive, geographically concentrated investments, the policy sought to improve public spaces and facilities in the targeted neighborhoods, with the specific policy goal of deconcentrating poverty and immigration and, ultimately, reducing income and ethnic segregation (Nello, 2009).

Between 2004 and 2010, there was an annual call for funding with an assigned yearly budget of 99 million EUR, to be distributed among the selected projects submitted by the local councils[15]. The length of the projects was fixed at 4 years. The funds were channeled as transfers from the regional government to the local councils. As a rule, the regional transfer could account for just 50% of the project, meaning that local governments had to cover the remaining 50%, possibly with transfers from other tiers of government. As discussed, the policy was clearly focused on investing in public spaces. As much as 80% of the funds was spent on public spaces and public use facilities, while an additional 10% was devoted to renovating the existing stock of apartment buildings. Finally, the remaining 10% was spent on social services aimed at improving the labor market performance of the neighborhoods' residents. By way of example, Appendix 4.8.1 shows the investments funded through the *Santa Caterina & Sant Pere* project, a 15 million EUR intervention carried out in Barcelona's city center.

Across the seven calls for funding, of the 450 applications received 143 were granted. However, owing to the fall in regional and local government revenues, the degree of execution is low among the projects accepted in the last calls. All projects from the 2004 to 2006 calls (46 in total) were completed while, from the 2007 call, only 17 out of the 24 accepted projects were completed. Thus a total of 63 projects were executed (corresponding to the 2004 to 2007 calls) and completed between 2008 and 2011. We exclude (what are mostly small) projects in municipalities with less than 10,000 inhabitants. We do so because census tracts in these municipalities (which is the finest geographical detail for which data are available) do not provide a realistic approximation to neighborhoods. We also exclude a few cases in which the municipality underwent a complete redrawing of its census tract borders in the period under study. Finally, we focus on 39 interventions with an average investment of 3,065 EUR per inhabitant. The intervention areas have an average population of 13,000 inhabitants, indicating that investments were quite localized.

[15] Each municipality could only be awarded one project per call (two in the case of Barcelona).

The selection process consisted of two rounds. In the first round, a deprivation index was calculated for each application. The index considers a large number of indicators measuring the following items: property value, characteristics of the housing stock (share of $\geq$ 4-storey apartment buildings without elevator, share of apartments without piped water or sewerage connection), high density, drastic population growth or decline, concentration of young and old people, non-EU immigration, proportion of welfare benefit users, unemployment rate, percentage of low-educated inhabitants, percentage of people at risk of social exclusion, deficit of public transportation, lack of parking space, lack of parks and green areas and a high vacancy rate in commercial property. The areas with a score above a certain threshold were considered in the second round.

In the second round, projects were ranked according to a final score determined by this deprivation index (with a weight of around 40%), the population size of the treated area together with more qualitative aspects of the project, including, the financial effort of the municipality, the type of project (historic district renovation or not), the involvement of the local community, and the adequacy of the project to policy goals. After ranking the projects on the basis of their final score, the budget limit of 99 million EUR implicitly defined a 'cut-off', which varied across calls depending on the proposed budget of the applicants at the top of the ranking.

The first column in Table 2.1 shows the average values of all indicators used in the deprivation index as well as the population size of the 39 projects analyzed here. As explained above, the projects were implemented in under-performing neighborhoods. Unemployment is about three percentage points higher than the regional average (12.8 vs. 10.2%), while the share of individuals (above 10 years) with no high-school diploma (or equivalent) is remarkably high (76% compared to the regional average of 40%). Treated neighborhoods also show a high presence of foreigners. In 2003, the share of non-EU15 immigrants was already high compared to the regional average (13.9 vs. 4.9%). Moreover, it was rising rapidly as the share of non-EU15 immigrants increased by 8.5 percentage points between 2001 and 2004. This dramatic increase is a consequence of the Spanish immigration wave, which meant Spain received almost 5 million immigrants between 1998 and 2008[16]. The increases in immigrant density experienced by the treated neighborhoods are particularly high as immigrants tended to concentrate in low-income urban areas (Fernández-Huertas Moraga et al., 2017)), that is, the locations specifically targeted by the Neighborhoods Act[17]. In fact, the massive immigration wave,

[16]The largest inflows of (mostly low-skilled) immigrants originated primarily from Ecuador, Morocco, Romania and Colombia. See Fernández-Huertas Moraga et al. (2017) and Jofre-Monseny et al. (2016) for a detailed description of the Spanish immigration wave.

[17]Public housing is quantitatively unimportant in Spain, hosting only 2 percent of households. In

coupled with its unequal impact across the neighborhoods in the region, was one of the main reasons why the Neighborhoods Act was introduced in 2004 (Nello, 2009).

The control group consists of 68 rejected projects and 35 projects accepted during the 2008, 2009 and 2010 calls that, due to the fall in public tax revenues, present a degree of budget execution below 20%. The second column in Table 2.1 shows the average values of all the quantitative indicators used in the program. The fourth column in the table reports differences between treatments and controls. Overall, the treatment group scores higher on most of the indicators, reflecting greater needs. For instance, unemployment is almost 4 percentage points higher in treated than in control neighborhoods. In terms of immigration, in 2003 the treated areas presented a non-EU15 immigrant share that was 7.0 percentage points higher than that of the control neighborhoods.

The Neighborhoods Act does not overlap in space and time with any other major urban interventions, which means that the estimated policy effects are not be confounded by concurrent urban policies. The EU URBAN projects are very similar in goals and nature with the interventions studied here. However, only two such projects have been implemented in Catalonia and neither coincide with the neighborhoods studied here. Likewise, Plan-E was a stimulus investment plan launched by the Spanish Government in the midst of the great recession (2008) and operationalized as transfers to local governments for investment. We analyzed all Plan-E projects and confirmed that none of the accepted or rejected neighborhoods considered herein receives investments of any significant quantitative importance in relation to the Neighborhoods Act investments[18].

2.4 Data and variables

We combine official data from the projects accepted and rejected under the Neighborhoods Act with data on population characteristics at the neighborhood level. In the case of the accepted projects, we have scores for all the indicators considered in the selection process (see Table 2.1), the amount invested, the timing of the execution, as well as the project boundaries and the exact location of all investments within these boundaries. One such project is illustrated in Appendix 4.8.2, where the solid line depicts the boundaries of the *Santa Caterina & Sant Pere* project, while the colored areas indicate the specific location of all investments. In the case

contrast to other European countries, public housing is not an important factor driving immigrants' locations decisions.

[18] Typically, Plan-E consists of many small projects scattered around the municipality.

Table 2.1: Sample balance: average of pre-treatment characteristics

	Treated (1)	Non-treated (2)	Non-treated reweighted (3)	Difference (1) - (2)	Difference (1) - (3)
Matching variables (used in the Oaxaca-Blinder estimator)					
Property value, relative to municipal average = 100, t-1	70.705	90.423	70.705	-19.718*	0.000
% of buildings in poor condition, 2001	5.192	3.372	5.192	1.820***	0.000
% of buildings without pipe water, 2001	0.654	0.717	0.654	-0.063	0.000
% of buildings without sewerage connection, 2001	0.634	1.201	0.634	-0.567***	0.000
% of >= 4 storey-buildings without elevator, 2001	65.507	66.038	65.507	-0.531	0.000
Density in 2003 (inhabitants per hectare, logged)	5.482	4.194	5.482	1.288***	0.000
Population growth rate (2003 - 2001, %)	9.978	7.038	9.978	2.940	0.000
% of population 0-16 years, 2001	15.135	14.307	15.135	0.827	0.000
% of population >65 years, 2001	17.211	20.349	17.211	-3.138***	0.000
% of non-EU15 immigrants in 2003	13.972	6.985	13.972	6.987***	0.000
% of welfare benefits users, t-1	1.804	1.440	1.804	0.363	0.000
% unemployment, 2001	12.784	8.953	12.784	3.831***	0.000
% of residents > 10 yrs, without a high-school diploma, 2001	75.653	73.867	75.653	1.786	0.000
% of people at risk of social exclusion, t-1	28.509	17.461	28.509	11.049	0.000
Public transportation: Presence, t-1	0.769	0.388	0.769	0.381***	0.000
Public transportation: Freq. >= 30', working hours, t-1	0.308	0.398	0.308	-0.090	0.000
Presence of public parking lots, t-1	0.359	0.194	0.359	0.165*	0.000
< 50 of buildings with private parking, t-1	0.897	0.612	0.897	0.286***	0.000
Lack of parks and green areas, t-1	58.057	46.081	58.057	11.976**	0.000
% vacant commercial property, 2001	29.207	26.271	29.207	2.936	0.000
Population in 2003 (logged)	8.663	6.593	8.663	2.069***	0.000
Variables not used in the matching procedure					
Δ log natives (2004 - 2001)	-0.023	0.002	-0.037	-0.025	0.014
Δ log non-EU15 immigrants (2004 - 2001)	1.090	0.840	1.033	0.250***	0.057
Δ log EU15 immigrants (2004 - 2001)	0.186	0.139	0.107	0.047	0.079
Δ share of natives (2004 - 2001)	-9.700	-4.580	-8.320	-5.120***	-1.380
Δ share of non-EU15 immigrants (2004 - 2001)	8.509	3.845	7.861	4.665***	0.648
Δ share of EU15 immigrants (2004 - 2001)	0.105	0.122	-0.032	-0.017	0.137
Number of observations	39	103			

Notes: Variables measured in 2001 drawn from the Census while variables measured in 2003 and 2004 are from population registers. Variables referred to t-1 are provided by local councils through the project's proposal.

of the rejected projects, we also know the scores for all the selection indicators, the budget proposal as well as the project boundaries.

As for our outcome variables, namely, population characteristics at the neighborhood level, we use yearly data from the municipal population register (*Padrón municipal de habitantes*) and data drawn from the 2001 and 2011 Population Censuses[19]. The population register is a yearly population count (with base date January 1st) containing information on an individual's age, gender and country of birth. The (decennial) population census contains further information, including, the educational level attained by the individual. Both the Census and the population register data are available at the census tract level (*Sección censal*), which is the most disaggregated geographical level existing in Spain. Unfortunately, some information for the Census is not disclosed in all tracts for reasons of confidentiality.

[19]See Foremny et al. (2017) for a detailed explanation of the workings of the municipal population register.

The intervention areas are typically made up of several census tracts. For the 142 treatment and control neighborhoods, the average number of tracts is 7.2, while the average census tract hosts 1,417 inhabitants. Although most project boundaries match the census tract borders, this is not always the case. For example, the dashed lines in Appendix 4.8.2 identify the census tracts in the *Santa Caterina & Sant Pere* project in Barcelona. Note that the project boundaries (solid line) generally follow the census tract borders. However, in the north-eastern corner, there is a census tract that is only partially included in the intervention area. In such instances, to compute the population of the intervened area, we resort to imputations. Specifically, we do so based on the share of developed land of each tract that belongs to the intervention area[20].

In the case of the outcome variables, we are interested in the long-run population dynamics at the neighborhood level. In terms of ethnic composition, we decompose the population into three groups: Natives, non-EU15 immigrants, and EU-15 immigrants. We divide the immigrants into these two groups as the policy goal is to reduce the concentration of non-EU15 immigrants in the treated neighborhoods (Nello, 2009)[21]. For each of these three groups, we examine two outcome variables by tracking changes that occurred between 2004 and 2013. The first of these is the difference between the logged population levels in 2003 and 2014 and, thus, it approximates the growth rate of each population group between these two years. The second is the change in the percentage of each group over the same time window. We also study the population dynamics of college graduates as this proxies high-income individuals. This information is only available from the Census and, thus, we study the changes occurring between 2001 and 2011. Specifically, we examine differences in the logged stock of college graduates as well as the changes in the percentage of this group in the neighborhood. Since the population by level of education is not disclosed for all tracts in the 2001 and 2011 censuses, in a few cases, the areas of intervention do not coincide exactly with the outcomes measuring changes in the population by ethnic and educational backgrounds.

2.5 Empirical approach

The main equation of interest is:

[20]To compute these shares, we use the SIOSE 2005 (Sistema de Información sobre Ocupación del Suelo de España).

[21]According to the 2001 Census, immigrants born in EU15 countries have a higher level of education than both non-EU15 migrants and natives. Therefore, individuals from EU15 countries can be seen as natives belonging to the upper tail of the income distribution.

$$\Delta Y_i = \alpha T_i + X_i^{'} \beta + \varepsilon_i \tag{2.8}$$

where ΔY_i is a measure of population change in neighborhood i, T_i is a treatment indicator while $X_i^{'}$ is a vector containing the neighborhood characteristics used by policy-makers to determine treatment. We focus here on long-differences. Specifically, we examine changes in native, EU15 and non-EU15 populations between 2004 and 2013 and changes in the population of college-graduates between 2001 and 2011.

As reported in Table 2.1, accepted projects between 2004 and 2007 differ from those that were rejected and those that were accepted in the 2008-2010 calls but that were never executed. In the bottom panel of Table 2.1, we test if treated and control neighborhoods also differ in pre-treatment trends. Specifically, we check if treatments and controls are balanced in terms of the outcomes of interest measured between 2001 and 2004. Prior to treatment, treated neighborhoods experienced larger increases in the share of non-EU15 immigrants. As a result, treated neighborhoods experienced more marked compositional changes, in which the share of natives (non-EU15 immigrants) decreased (increased) more in the intervention neighborhoods. This implies that the underlying assumption in the differences-in-differences setting, namely, the parallel trends assumption, does not hold in our application. As a result, we adopt the Oaxaca-Blinder approach developed in Kline (2011). The estimator procedure involves two steps. In the first step, the control units are used to estimate the following auxiliary regression:

$$\Delta Y_i = X_i^{'} \beta + \varepsilon_i \tag{2.9}$$

where the outcomes of interest, ΔY_i, is regressed on X_i, the vector containing all the indicators listed in the top panel of Table 2.1. In some specifications, we also include the lagged outcome measured during the pre-treatment period 2001-2004. In a second step, using the coefficients estimated in 2.9, namely $\hat{\beta}$, the average treatment effect is given by:

$$\widehat{ATT} = \hat{\mu} - \frac{1}{N_T} \sum^{N_T} \left(X_i^{'} \hat{\beta} \right) \tag{2.10}$$

The first term of this estimator ($\hat{\mu}$) is the unconditional mean of the treated units while the second term is the counterfactual mean (for the treated units) obtained when using the estimated coefficients in 2.9, while N_T is the number of treated neighborhoods. Note that this is the estimator of counterfactual means developed by Oaxaca (1973) and Blinder (1973). Writing the counterfactual mean for treated units in matrix form and replacing the coefficient estimates by the Ordinary Least

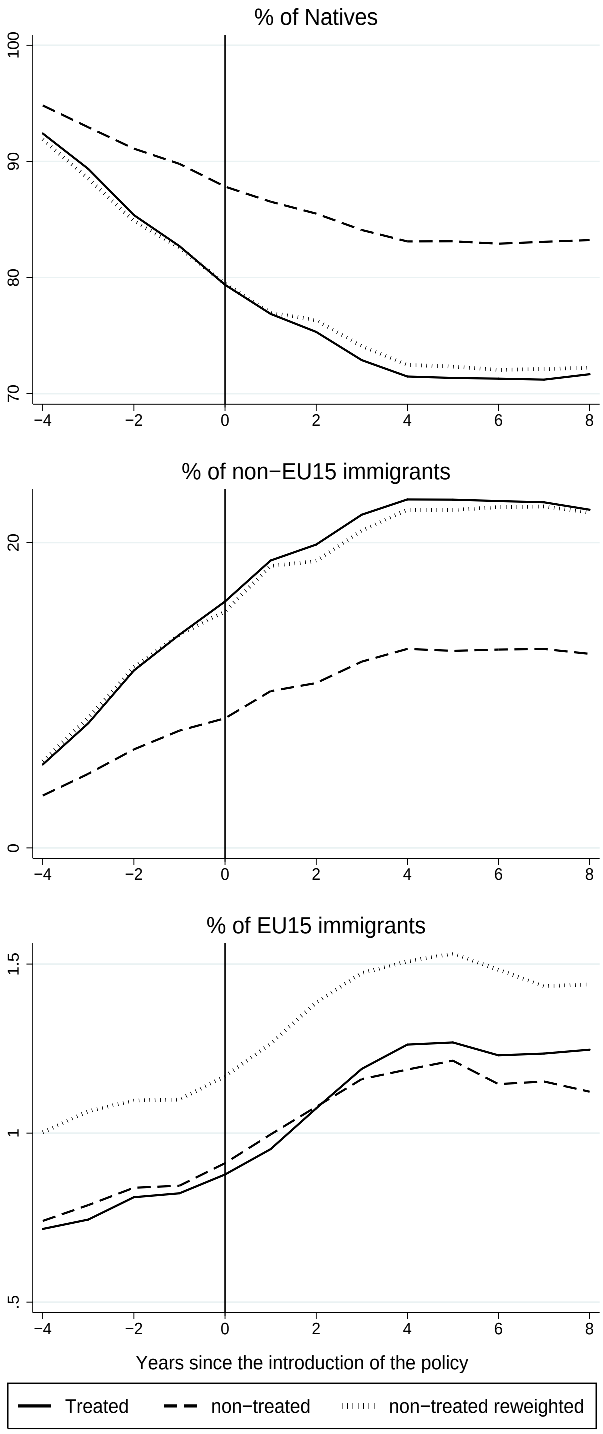
Squares (OLS) formula yields:

$$\frac{1}{N_T}D'X(X'WX)^{-1}X'WY = wY \tag{2.11}$$

where D is a vector weighting the treated observations while W is a matrix that only weights the control observations. The expression preceding Y in equation 2.11 turns out to be a vector of weights, denoted by w. Hence, the counterfactual mean for the treated units is a weighted average of the control outcomes. These weights have two important properties. First, they guarantee that the mean of each and every control variable included in X is exactly the same in treated and re-weighted control samples, as reflected in the third column of Table 2.1. Second, the w's can be interpreted as those of a propensity score re-weighting estimator in which the weights are proportional to the conditional odds of treatment. This estimator is particularly desirable in settings, such as ours, in which the number of controls exceeds that of treatments and the vector X contains a large number of variables (Kline, 2011). Moreover, in our application, the vector of controls X contains all neighborhood features that have been selected by policy-makers to determine treatment, reducing the risk that unobservable variables confound our estimates of interest.

The dotted lines in Figure 2.2 illustrate the weighted averages for the shares of natives, non-EU15 immigrants and EU-15 immigrants obtained from a specification in which the first step (specification 2.9) only includes the indicators listed in the top panel of Table 2.1. While the treated and the unweighted control samples show clearly diverging pre-treatment neighborhood dynamics, the treated and the re-weighted control samples show very similar pre-treatment trends. This is formally shown in the last columns of the bottom panel of Table 2.1.

Figure 2.2: Evolution of demographic outcome variables



2.6 Results

2.6.1 Baseline results

In Table 4.2, we present the baseline results for Equation 2.10 including different sets of controls. Each row corresponds to a different estimation and shows the effect of the urban renewal policy on a different outcome variable. Column 1 presents the results obtained when all variables shown in the top panel of Table 2.1 are used as controls. Note that these are all neighborhood characteristics used by policy-makers to select the treated neighborhoods. The estimates for all the outcome variables are small and statistically insignificant. This evidence suggests that, on average, the urban renewal policy had no effects on the population dynamics and educational composition of the intervention areas. The results for the shares of natives, non-EU15 and EU-15 immigrants are illustrated in Figure 2.2. Each dotted line represents the evolution of each variable for the reweighted control sample. Hence, the policy effect is the vertical difference between the treated (solid line) and the reweighted control sample. The graphs also indicate that the urban renewal policy had little (if any) effect on population dynamics. Similarly, the graphs show that the results do not hinge on the time window specified in the regression analyses.

As detailed in Table 2.1, the variables used for the treatment selection are drawn from three different sources: 1) the 2001 census; 2) the population registers; and, 3) data provided by the municipalities when completing the project application. The variables corresponding to sources 2) and 3) refer to different points in time depending on the specific call for funding in question. The variables built from annual population registers, including the population level and the share of non-EU15 immigrants in the neighborhood, can be measured prior to 2004 and we do so throughout the analysis. Specifically, we measure these variables in 2003, which correspond to the data used for treatment selection in the first (2004) call of the Neighborhoods Act. The third group of variables cannot be measured at different points in time and, so, we include controls that are not strictly pre-determined with respect to our outcomes. Thus, in column 2, we show the results obtained when we re-run the main analysis excluding these controls when estimating equation 2.10. Fortunately, the results do not change substantially when these variables are excluded. In column 3, we report the results obtained when, in addition to all the variables used in column 1, we add the lagged outcome measured between 2001 and 2004. Although the bottom panel in Table 2.1 suggests that treatment and the reweighted control groups show similar pre-trends, there might be efficiency gains by directly matching on pre-treatment trends in the outcome variable[22]. This is the

[22]Busso et al. (2014) also match on pre-treatment outcomes.

most complete and, therefore, our preferred specification.

Table 2.2: Impact of the urban renewal policy on ethnic and educational composition

	Oaxaca-Blinder (OB)			OLS
	(I)	(II)	(III)	(IV)
Ethnic composition, 2004-2013				
Δ log population	0.022	0.054	0.002	0.016
	(0.028)	(0.039)	(0.030)	(0.027)
Δ log natives	-0.006	0.017	-0.011	-0.010
	(0.030)	(0.040)	(0.030)	(0.026)
Δ log non-EU15 immigrants	0.087	0.116	0.082	0.080
	(0.081)	(0.081)	(0.080)	(0.068)
Δ log EU15 immigrants	0.042	0.084	0.078	0.039
	(0.109)	(0.108)	(0.107)	(0.089)
Δ % of natives	-1.398	-2.137	-0.871	-0.832
	(1.235)	(1.178)*	(1.257)	(1.018)
Δ % of non-EU15 immigrants	0.783	1.485	0.266	0.499
	(1.137)	(1.061)	(1.086)	(0.886)
Δ % of EU15 immigrants	0.307	0.224	0.269	0.077
	(0.210)	(0.209)	(0.212)	(0.132)
Educational composition, 2001-2011				
Δ log pop. with college degree	-0.161	-0.186	-.-	-0.086
	(0.180)	(0.185)		(0.171)
Δ % of pop- with college degree	-0.069	-0.288	-.-	-0.194
	(1.374)	(1.389)		(1.192)
Control variables				
2001 census indicators	Y	Y	Y	Y
2003 population registers indicators	Y	Y	Y	Y
Other indicators	Y	N	Y	Y
Lagged outcome, 2001-2004	N	N	Y	Y^a
Number of observations		142		
Number of treated observations		39		

Notes: Each entry represents an outcome variable. Control variables enter the first step in the Oaxaca-Blinder estimator (equation 2.9). 2001 census and 2003 population registers indicators as shown in Table 1. Other indicators are the remaining variables in the top panel of Table 2.1 used by policy-makers to determine treatment. *a*) The lagged outcome is not included in the regressions of college-graduates as we do not have this information. Robust standard errors in parenthesis. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

The results are similar to those above, pointing once again to the null effects of the urban renewal policy on neighborhood population dynamics. For the sake of completeness, column 4 reports the OLS estimation. It should be noted that these results are very similar to those obtained with the OB procedure.

2.6.2 Heterogeneous analyses

Although the baseline results indicate that, on average, there are no significant effects of the urban renewal policy on the population dynamics of the treated area, it could well be that these results are heterogeneous with respect to the characteristics of projects and neighborhoods. In Table 3 we focus on project heterogeneity while in Tables 4 and 5 we analyze the effects of interest by different neighborhood characteristics. The estimates reported throughout this section correspond to the more complete specification (column 3 in Table 4.2), which includes the 2001-2004 lagged outcome, and, as a result, pre-treatment trends between the treated and the reweighted control samples are exactly matched by construction[23]. Exploring these heterogeneous patterns can also shed light on the reasons why the policy was, on average, ineffective.

Effects by project size

One prediction of the model developed in Section 2 is that, if the minority share is relatively high, the policy will only be effective if the investment (G) is large enough to reverse the tipping point. In Table 2.3, we present the estimations after dividing the interventions between those with low and high levels of investment. We use three criteria to divide the projects by size: Total amount invested, per capita investment, and per area investment. In the case of total investment (columns 1 and 2), we consider projects above and below the median (14 million EUR). The results indicate that the impact of the urban renewal initiatives on neighborhood population dynamics are null even for the sub-sample of large projects in which the average investment is just above 16 million EUR. The same picture emerges when we consider investment per capita (above and below the median - 2,459 EUR) in columns 3 and 4, and when we consider investment per area (above and below the median - 0.46 million EUR per hectare) in columns 5 and 6. These results indicate that investment in urban renewal has no effects on population dynamics even in the case of those projects that concentrate the highest sums of money.

[23]Thus, the analogous graphs to Figure 2.2 are not reported.

Table 2.3: Impact of the urban renewal policy on ethnic and educational composition by level of investment.

	Total investment < the median	**Total investment > the median**	**Investment per capita < the median**	**Investment per capita > the median**	**Investment per He < the median**	**Investment per He > the median**
Ethnic composition, 2004-2013						
Δ log population	0.028	-0.022	0.026	-0.021	0.010	-0.005
	(0.038)	(0.041)	(0.036)	(0.040)	(0.035)	(0.042)
Δ log natives	0.025	-0.045	-0.005	-0.017	-0.026	0.003
	(0.041)	(0.039)	(0.033)	(0.044)	(0.032)	(0.046)
Δ log non-EU15 immigrants	0.073	0.091	0.075	0.090	0.088	0.077
	(0.101)	(0.106)	(0.115)	(0.087)	(0.115)	(0.088)
Δ log EU15 immigrants	0.095	0.061	0.045	0.109	0.043	0.111
	(0.131)	(0.147)	(0.141)	(0.131)	(0.142)	(0.132)
Δ % of natives	-0.152	-1.554	-2.035	0.235	-2.358	0.542
	(1.589)	(1.544)	(1.428)	(1.665)	(1.478)	(1.581)
Δ % of non-EU15 immigrants	-0.173	0.684	0.554	-0.008	0.836	-0.275
	(1.392)	(1.380)	(1.251)	(1.529)	(1.307)	(1.458)
Δ % of EU15 immigrants	0.079	0.451	0.496	0.054	0.495	0.055
	(0.141)	(0.373)	(0.380)	(0.154)	(0.380)	(0.158)
Educational composition, 2001-2011						
Δ log pop. with college degree	0.039	-0.351	-0.078	-0.239	-0.186	-0.137
	(0.236)	(0.244)	(0.253)	(0.232)	(0.248)	(0.239)
Δ % pop. with college degree	0.555	-0.662	0.618	-0.721	0.191	-0.315
	(1.434)	(2.100)	(2.046)	(1.613)	(2.062)	(1.614)
Number of observations	122	123	122	123	122	123
Number of treated observations	19	20	19	20	19	20

Notes: OB estimates, with each entry representing one outcome variable. Control variables include all variables in Table 2.1, as well as the 2001-2004 lagged outcome in the ethnic composition outcomes. Robust standard errors in parenthesis. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

Effects by neighborhood type

The results reported in Table 2.3 are consistent with the model's prediction when a neighborhood has surpassed the tipping point. Since neighborhoods are also heterogeneous with respect to the initial minority share, we next test if the policy has been effective in settings where the initial minority share was lower ($P<P^t$ in terms of the model). Since the exact location of the tipping point is empirically unknown, we split the sample according to the percentage of immigrants in the neighborhood in 2003[24]. The results are reported in 2.4. Columns 1 and 2 show the results for treated neighborhoods with a non-EU15 immigrant share below and above the median (12 percent), respectively. The results for the two sub-samples are qualitatively similar, corroborating the conclusion that the policy had no effects on neighborhood dynamics. Since the average non-EU15 immigrant share is 6.6 percent in the sample with low immigration, the results suggest that a high minority share is unlikely to account for the policy's total lack of impact.

In the model presented in section 2 we considered two population groups, natives and immigrants. The two key elements in that model are that the natives' income is higher than that of the immigrants and, at the same time, the natives prefer neighborhoods where the share of immigrants is low. Note, however, that preferences for neighbors might not be restricted to ethnicity. Bayer et al. (2007) have shown for the San Francisco Bay area that, besides race, households prefer college-educated and rich neighbors. Hence, the two population groups in the model could alternatively be interpreted as poor and rich, or neighbors with high vs. low education (Glaeser, 2008). In columns 3 and 4 of Table 2.4 we explore heterogenous patterns with respect to the level of education. Specifically, we consider treated units with a population share ($\geq$10 years of age) without a high-school diploma below and above the median (76.6%). The results also remain close to zero for the two sub-samples. Finally, in columns 5 and 6 we divided the sample considering treated units with a deprivation index below and above the median (44.4), respectively. This index is a function of all the deprivation indicators shown in the upper panel of Table 2.1 and, thus, the highest values indicate the greatest social needs. Again, the results indicate no policy effects for the two sub-samples. Note, however, that all the treated neighborhoods score above the deprivation index threshold set by policy-makers in order to exclude non-deprived neighborhoods.

[24] Card et al. (2008) estimate tipping points for US cities and find a large degree of heterogeneity across cities and over time. This finding suggests that tipping points are context specific and that they are not generalizable.

Table 2.4: Impact of the urban renewal policy on ethnic and educational composition by neighborhood characteristics

	Initial % of non-EU15 < the median	**Initial % of non-EU15 > the median**	**% of low educated residents > the median**	**% of low educated residents < the median**	**Treated with deprivation index < the median**	**Treated with deprivation index > the median**
Ethnic composition, 2004-2013						
Δ log population	0.030	-0.014	-0.038	0.044	0.038	-0.032
	(0.037)	(0.076)	(0.040)	(0.044)	(0.035)	(0.047)
Δ log natives	0.014	-0.046	-0.071	0.052	0.018	-0.039
	(0.039)	(0.069)	(0.044)	(0.044)	(0.035)	(0.047)
Δ log non-EU15 immigrants	0.030	0.117	0.144	0.017	0.080	0.085
	(0.101)	(0.151)	(0.108)	(0.108)	(0.098)	(0.106)
Δ log EU15 immigrants	0.036	0.118	-0.166	0.334	0.033	0.120
	(0.108)	(0.220)	(0.111)	(0.162)**	(0.096)	(0.167)
Δ % of natives	-1.536	-2.232	-2.022	0.341	-1.525	-0.249
	(1.436)	(1.935)	(1.815)	(1.524)	(1.384)	(1.852)
Δ % of non-EU15 immigrants	0.938	1.179	1.452	-0.982	0.779	-0.221
	(1.174)	(1.924)	(1.521)	(1.353)	(1.104)	(1.679)
Δ % of EU15 immigrants	0.093	0.497	0.009	0.543	0.171	0.363
	(0.141)	(0.457)	(0.137)	(0.390)	(0.134)	(0.376)
Educational composition, 2001-2011						
Δ log pop. with college degree	0.193	-0.277	-0.267	-0.049	0.140	-0.446
	(0.155)	(0.320)	(0.232)	(0.262)	(0.183)	(0.273)
Δ % of pop- with college degree	1.542	-0.966	-1.880	1.838	1.727	-1.775
	(1.436)	(2.530)	(1.368)	(2.226)	(1.372)	(2.173)
Number of observations	122	123	123	122	122	123
Number of treated observation	19	20	20	19	19	20

Notes: OB estimates, with each entry representing one outcome variable. Control variables include all variables in Table 2.1, as well as the 2001-2004 lagged outcome in the ethnic composition outcomes. Robust standard errors in parenthesis. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

In fact, most of the treated neighborhoods are low-income neighborhoods that share a history of bad reputation and stigma (Nello, 2009). Thus, the hypothesis that the ineffectiveness of the policy can be attributed to the unfavorable social mix in the treated neighborhoods does not strike us as implausible.

The characteristics and location of the treated neighborhoods may also affect the impact of the policy. In the model developed in Section 2, we only consider one exogenous amenity G, which reflects the quality of public spaces and facilities in the neighborhood. The policy guidelines distinguish between interventions in suburbs and historic districts; thus, it might be the case that people value more highly investments in public spaces in historic districts given their more central location and the (potential) historical-architectural value of the intervention areas. To explore this dimension of heterogeneity, in columns 1 and 2 in Table 2.5 we report the results for the impact of the urban renewal policy in suburbs (24 projects) and historic districts (15 projects). In order to adapt the matching strategy to this analysis, we further include as control variables, an indicator for suburbs and historic districts in columns 1 and 2, respectively.[25] Column 1 shows the estimates for the 24 suburbs, which indicate that there are no policy effects in these deprived areas. Column 2 presents the results for the urban renewal projects in the historic districts. Here, in contrast to all previous estimates, the results suggest that the urban renewal policy did have some impact on population dynamics. Specifically, for this subset of projects, interventions caused a 43% increase in the population of EU15 immigrants over the nine-year period between 2004 and 2013. Given the moderate size of this population group, this results in a modest .72 percentage point increase in the share of EU-15 immigrants in the treated historic districts.

Interventions in Barcelona's historic districts

Baum-Snow and Hartley (2016) and Couture and Handbury (2017) document that, between 2000 and 2010, city centers in US cities experienced a process of urban revival, with relative increases in income and in the share of college graduates. A similar gentrification process has occurred in Barcelona, the only large metropolitan area in the region. In the historic city center, *Ciutat Vella*, the share of college graduates has increased by more than 16 percentage points between 2001 and 2011, increasing from 13.7 to 30.4%[26]. Hence, one possible hypothesis to emerge is that

[25]Note that these adjustments were not necessary for the exercises conducted in Table 2.4. There, the share of non-EU15 immigrants, the population share without a college degree and all the variables entering the deprivation index are already included among the controls considered when estimating equation 2.9. Hence, for the sample splits in Table 2.4, treatments and controls are properly matched with respect to the variables used to split the samples.

[26]During the same period, the city-level share of college graduates also increased, but to a lesser

Table 2.5: Impact of the urban renewal policy on demographic variables and education outcomes: Suburbs versus historic districts

	Suburbs	**Historic districts**	**Historic districts, without Barcelona**	**Historic districts in Barcelona**	**Historic districts, Madrid placebo test**
Ethnic composition, 2004-2013					
Δ log population	-0.039	0.027	0.015	0.093	-0.060
	(0.047)	(0.049)	(0.052)	(0.073)	(0.051)
	[-0.160 ; 0.049]	[-0.137 ; 0.119]	[-0.151 ; 0.132]	[-1.026 ; 0.270]	
Δ log natives	-0.072	0.053	0.041	0.134	-0.065
	(0.049)	(0.047)	(0.052)	(0.079)*	(0.051)
	[-0.153 ; 0.007]	[-0.097 ; 0.150]	[-0.124 ; 0.144]	[-0.404 ; 0.313]	
Δ log non-EU15 immigrants	0.033	0.021	0.060	-0.252	-0.029
	(0.105)	(0.128)	(0.136)	(0.222)	(0.100)
	[-0.138 ; 0.223]	[-0.270 ; 0.235]	[-0.232 ; 0.340]	[-1.015 ; 0.280]	
Δ log EU15 immigrants	-0.142	0.427	0.330	0.702	-0.211
	(0.130)	(0.194)**	(0.201)	(0.301)**	(0.139)
	[-0.422 ; 0.085]	**[0.036 ; 0.702]**	[-0.092 ; 0.660]	[-0.182 ; 1.375]	
Δ % of natives	-2.063	1.924	1.756	3.379	-0.431
	(1.464)	(1.948)	(2.223)	(2.860)	(0.964)
	[-4.108 ; -0.385]	[-0.588 ; 4.156]	[-0.827 ; 4.564]	[-1.599 ; 11.262]	
Δ % of non-EU15 immigrants	0.948	-1.573	-0.705	-5.786	0.340
	(1.221)	(1.792)	(1.983)	(2.990)*	(2.067)
	[-.401 ; 2.747]	[-3.385 ; 0.775]	[-2.964 ; 1.790]	**[-9.932 ; -0.666]**	
Δ % of EU15 immigrants	0.003	0.722	0.193	2.523	-0.603
	(0.163)	(0.474)	(0.210)	(1.361)*	(0.587)
	[-0.350 ; 0.371]	**[0.062 ; 1.255]**	[-0.276 ; 0.582]	**[1.512 ; 3.744]**	
Educational composition, 2001-2011					
Δ log pop. with college degree	-0.330	-0.019	-0.241	0.978	-0.178
	(0.215)	(0.330)	(0.380)	(0.303)***	(0.183)
	[-0.687 ; -0.003]	[-0.549 ; 0.340]	[-0.976 ; 0.221]	[-1.135 ; 1.580]	
Δ % pop. with college degree	-1.692	2.249	-1.003	16.176	1.009
	(1.254)	(2.810)	(2.497)	(3.638)***	(3.765)
	[-3.863 ; 0.620]	[-1.980 ; 5.825]	[-5.477 ; 2.939]	**[4.604 ; 25.901]**	
Number of observations	127	118	115	106	128
Number of treated observations	24	15	12	3	3

Notes: OB estimates, with each entry representing one outcome variable. Control variables include i) all variables in Table 2.1, ii) the 2001-2004 lagged outcome in the ethnic composition outcomes, and iii) an indicator for suburbs (column 1), an indicator for historic district (columns 2 and 3), and two indicators for historic district and municipality of Barcelona (column 4). Analytical robust standard errors in parenthesis where ***, ** and * denote statistical significance at the 1, 5 and 10% level. In brackets, bootstrapped 95% confidence intervals based on 1,000 replications. In each column, each replica draws the corresponding number of treated observations (e.g. 3 for Barcelona's historic districts) and considers the rest as control units.

urban renewal projects in Barcelona's historic districts have had significant impacts. To test this, we split the sample between historic districts in Barcelona and those located elsewhere. In column 4 in Table 2.5, we report the results for the three interventions in Barcelona while in column 3 we show the results for the remaining 12 interventions[27]. Given that there are only three treated neighborhoods, statistical inference based on the analytical standard errors provided by Busso et al. (2014) might be misleading. To address this issue, and following Gobillon and Magnac (2016), we build parameter estimates with 95% confidence intervals based on the following bootstrap procedure. We draw three units from the entire population of treatments and controls and consider them as treated units, while the remaining

extent, rising from 21.2 to 32%.

[27]The three interventions in Barceona's historic districts correspond to *Santa Caterina i Sant Pau*, *Poble Sec* and *La bordeta*.

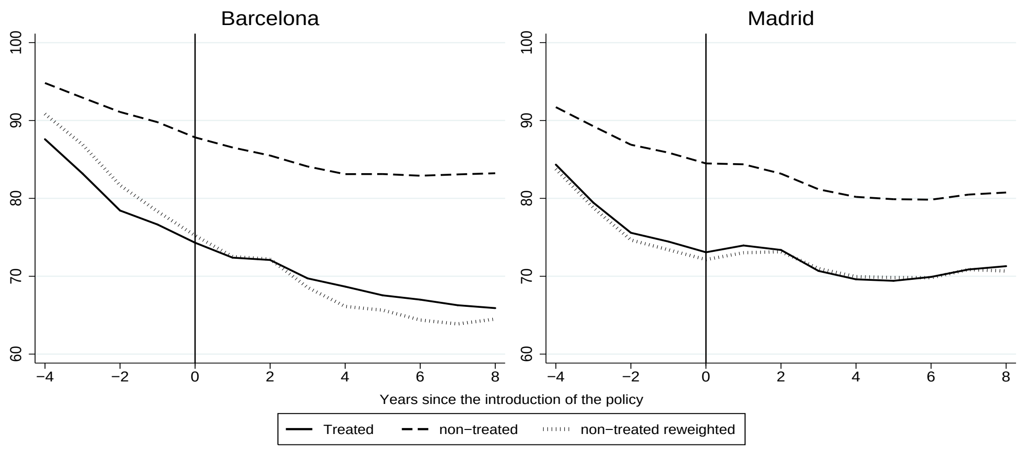
units correspond to the control group. We then estimate the treatment coefficients and replicate this exercise 1,000 times. The 95% confidence interval is obtained as the 2.5th and 97.5th percentiles of the empirical distribution of these estimates.

The results show that, when considering the treated areas located in historic districts, excluding those in the city of Barcelona, the urban renewal policies have no significant effects on population dynamics. This suggests that the results obtained in column 2 were entirely driven by the projects in Barcelona. The results in column 4 confirm that this is indeed the case. Taken at face value, the impact of the urban renewal projects in the historic districts of Barcelona are quite large. Over the period 2004 to 2013, the policy increased the native and EU15 immigrant populations by 13.4 and 70%, respectively. The point estimates, which are not statistically different from zero, also imply that overall population increased by 9.3%, while the count of non-EU15 immigrants fell by 25.2%. These different population growth rates meant considerable changes in the composition of these neighborhoods with the share of natives and EU15 immigrants increasing by 3.4 and 2.5 percentage points, respectively, and the percentage of non-EU15 immigrants falling by 5.8 percentage points. These changes in the ethnic composition of neighborhoods involved a sizable increase in the share of college graduates in the neighborhood. Specifically, between 2001 and 2011, the urban renewal policy increased the population of college graduates by 98%, increasing the proportion of college graduates (among individuals aged 16 or more) by 16.2 percentage points. All in all, this indicates that the urban renewal projects implemented in the city of Barcelona have intensified the ongoing gentrification of its city center in the deprived historic neighborhoods targeted under the policy.

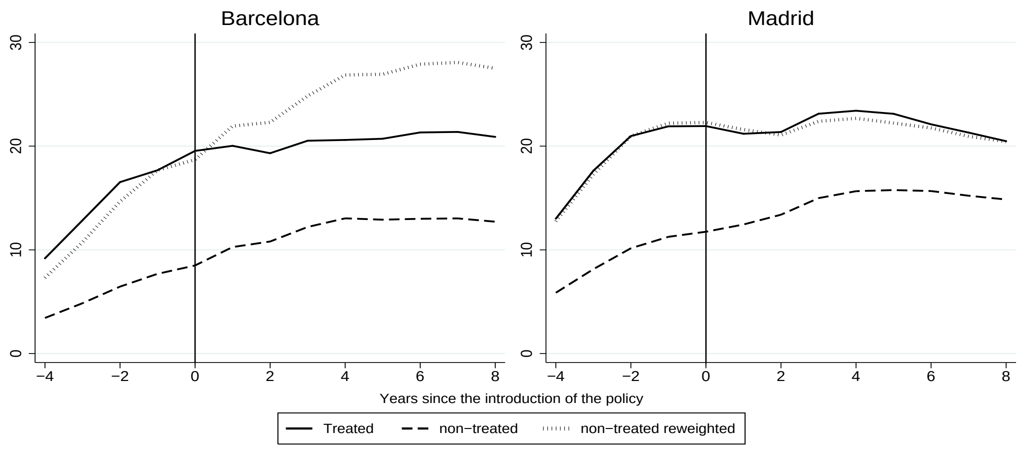
The effects recorded in Barcelona's three historic districts coupled with the policy's general ineffectiveness elsewhere is consistent with a tipping story in which the policy is unable to surpass the tipping point if the "big push" is of insufficient size. The share of non-EU15 immigrants is not especially low in the treated neighborhoods of Barcelona's historic districts. In fact, the 2003 (average) share of non-EU15 immigrants for these three projects is higher than the average recorded for all the treated neighborhoods (16.6 vs 14.0%). Hence, the low immigrant share is unlikely to explain why the policy was effective there. Although we are unable to test this hypothesis, the high historical-architectural value of these neighborhoods together with their central location might explain the effectiveness of the Neighborhoods Act in Barcelona's historic districts. In their study of enterprise zones, Briant et al. (2015) also document a high degree of heterogeneity with regards the effects of the policy and they show that differences in accessibility across the treated neighborhoods may account for these heterogeneous policy effects.

Figure 2.3: Evolution of demographic outcome variables. Historic districts in Barcelona vs historic districts in Madrid.

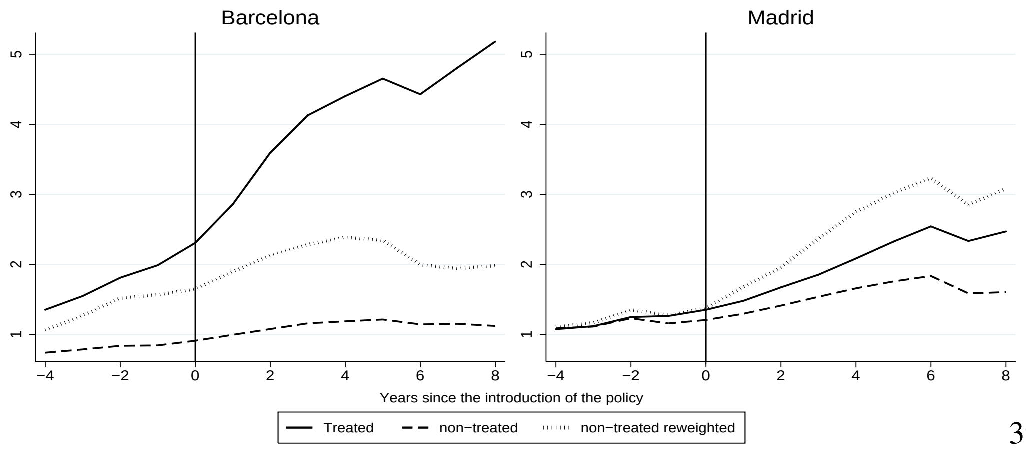
(a) % of natives



(b) % of non-EU15



(c) % of EU15



One valid concern is the possibility that Barcelona's historic districts might experience contemporaneous shocks in their population dynamics that may confound our estimates. To partly address these concerns, we conduct a placebo test using Madrid, given that its city center also underwent a process of urban revival in the period studied here. We select the three neighborhoods in Madrid's city center that are most similar to the three treated neighborhoods in Barcelona in terms of their probability of being treated[28]. The pre-treatment population dynamics of the three units selected in Madrid are reasonably similar to those of their Barcelona counterparts. Figure 2.3 shows the evolution in population of the three treated units in Barcelona's historic districts (left panels) and of the three placebo units in Madrid (right panel). In contrast to the results reported in columns 5 and 6 in Table 2.5, these figures correspond to estimates in which the lagged outcome is not included. Specifically, the specification of equation 2.9 corresponds to that of column 2 in Table 4.2. The figure shows similar pre-treatment patterns, especially for the population shares of natives and non-EU15 immigrants. Admittedly, the share of EU15 immigrants is growing in both cities albeit at a higher rate in Barcelona. Note, however, that the estimates reported in Table 2.5 correspond to a specification in which treatments and controls are exactly matched in terms of their pre-treatment outcomes, too. Reassuringly, the estimates reported in the last column of Table 2.5 for Madrid indicate no effects.

2.6.3 Results at the census tract level

In the analyses reported above, the geographical unit of analysis has been the intervention area as designated in the Neighborhoods Act grant applications. In many cases, however, there is substantial heterogeneity in the amounts invested within these areas. For example, under *Santa Caterina i Sant Pere* project (see map in Appendix 4.8.2), the works carried out in the *Pou de la Figuera* area (see top panel in Figure 4.8) concentrated the lion's share of funding (corresponding to the largest colored area on the map). Since we know the exact location of all the investments made, we are able to re-run the analysis at the tract level (i.e. the areas delineated by dashed lines on the map), which is the finest geographical detail for which our outcome variables are available. The estimations are presented in Table 2.6. For the

[28] To identify these three neighborhoods, we pool the sample of (121) controls and all the neighborhoods in (the municipality of) Madrid. With these data, we calculate the Oaxaca-Blinder weights as defined in Kline (2011), reflecting the probability of treatment. The specification used includes the variables considered in column 2 of Table 4.2, as well as an indicator for historic district. The probabilities of receiving treatment are plotted for all the neighborhoods of Madrid in Appendix C. The highest probabilities are those of *Embajadores*, *La Chopera* and *Palos de Moguer* which are the placebo treatments considered here.

baseline analysis (column 1), we have 412 treated and 317 non-treated tracts, reflecting the fact that the treated tracts (39) are larger in area than the controls (103). One limitation of undertaking this analysis is that some observations are lost when analyzing the educational composition of neighborhoods, given that for reasons of statistical confidentiality the population by level of education is not disclosed for all census tracts. Since tracts belonging to the same intervention area are not independent observations, we cluster the standard errors at this level of the intervention area. Moreover, as in some of the exercises we conduct the number of clusters is low, we supplement the results with p-values obtained with the clustered wild bootstrap procedure developed by Cameron et al. (2008)[29].

The first column shows the baseline estimates. These correspond closely to the results reported in the third column of Table 4.2, and indicate that the policy effects are, on average, null. When we re-run the analysis considering only very large and localized projects (column 2), the results indicate no effects, even when we focus specifically on the 30 tracts receiving the largest sums of funding (above 3.5 million euros at the tract level). As for the characteristics of the treated area, columns 3 and 4 report the estimates for interventions in the suburbs and the historic districts, respectively. Since the results obtained in Table 2.5 indicated that the effects found for historic districts were entirely driven by three interventions in Barcelona, in column 5 we report the results excluding Barcelona. Finally, column 6 shows the treatment effects estimated for the 42 census tracts belonging to the three projects carried out in historic districts in Barcelona. Overall, our results are broadly consistent with those obtained when working with the intervention areas. First, the policy has no impact on the population dynamics of the suburbs. Second, when focusing on interventions in historic districts, the policy is found to reduce the share of non-EU15 immigrants in the intervention tracts by almost 3 percentage points. Third, when excluding the three projects in the historic districts of Barcelona, the result becomes statistically insignificant. Fourth, according to the (point) estimates, the urban renewal policies implemented in the historic districts of Barcelona have had marked impacts on the population dynamics of these neighborhoods. In the period 2004 to 2013, the policy increased the overall, native and EU15 immigrant populations by 4, 8.9 and 50%, respectively, although the effect is only statistically significant for this last variable. As a result, the share of natives and EU15 immigrants increased by 4.4 (non-significant) and 1.8 percentage points, while the percentage of non-EU15 immigrants fell by 5.3 percentage points. As for changes in the educational composition, the policy increased the share of neighbors (with 16 years of age or more) with college education by 10.6 percentage points.

[29]See Cameron and Miller (2015) for a discussion of standard errors clustering.

Table 2.6: Impact of the urban renewal policy on demographic variables and education. Results at the tract level

	Baseline	Investment in the tract > 90th pctile 3.5M Euros	Suburbs	Historic districts	Historic districts, without Barcelona	Historic districts in Barcelona
Ethnic composition, 2004-2013						
Δ log population	0.021	-0.071	-0.003	0.049	0.047	0.040
	(0.026)	(0.037)	(0.029)	(0.043)	(0.047)	(0.060)
	[.731]	[.110]	[.950]	[.455]	[.521]	[.555]
Δ log natives	0.030	-0.038	-0.014	0.097	0.094	0.089
	(0.023)	(0.036)	(0.026)	(0.039)	(0.041)	(0.063)
	[.796]	[.449]	[.954]	[.178]	[.277]	[.234]
Δ log non-EU15 immigrants	0.052	-0.111	0.032	0.084	0.137	-0.206
	(0.079)	(0.111)	(0.089)	(0.124)	(0.131)	(0.184)
	[.611]	[.333]	[.756]	[.567]	[.421]	[.265]
Δ log EU15 immigrants	0.102	0.168	-0.056	0.313	0.239	0.502
	(0.097)	(0.159)	(0.085)	(0.180)	(0.163)	(0.273)*
	[.473]	[.269]	[.687]	[.172]	[.297]	[.054][c]
Δ % of natives	0.401	1.968	-0.911	3.223	2.943	4.383
	(1.139)	(1.733)	(1.350)	(1.594)	(1.761)	(2.896)
	[.842]	[.261]	[.511]	[.124]	[.202]	[.138]
Δ % of non-EU15 immigrants	-0.957	-2.106	0.050	-2.963	-2.537	-5.285
	(0.931)	(1.622)	(1.071)	(1.342)**	(1.496)	(2.181)**
	[.423]	[.202]	[.970]	[.046][b]	[.114]	[.026][b]
Δ % of EU15 immigrants	0.232	0.401	0.012	0.452	0.118	1.828
	(0.211)	(0.351)	(0.137)	(0.436)	(0.197)	(1.275)**
	[.347]	[.299]	[.940]	[.371]	[.655]	[.050][b]
Number of observations	729	347	572	474	432	359
Number of treated observations	412	30	255	157	115	42
Educational composition						
Δ log pop. with college degree	-0.068	-0.271	-0.303	0.063	-0.079	0.505
	(0.182)	(0.340)	(0.188)	(0.371)	(0.425)	(0.410)
	[.794]	[.437]	[.148]	[.876]	[.890]	[.281]
Δ % pop. with college degree	-0.043	-0.396	-1.219	1.693	-1.090	10.590
	(1.254)	(2.277)	(1.164)	(2.501)	(1.993)	(3.715)***
	[.982]	[.852]	[.277]	[.561]	[.627]	[.008][a]
Number of observations	609	311	474	423	381	330
Number of treated observations	412	30	255	157	115	42

Notes: OB estimates, with each entry representing one outcome variable. Control variables are all 2001 census variables listed in 4.2. Clustered standard errors at the proposed area of intervention in parenthesis where ***, ** and * denote statistical significance at the 1, 5 and 10% level. Alternatively, p-value in squared brackets from a clustered wild bootstrap procedure (with 1000 replications) developed by (Cameron et al., 2008) where [a], [b] and [c] denote statistical significance at the 1, 5 and 10% levels.

2.7 Summary and concluding remarks

This paper has analyzed the impact on the population dynamics at the neighborhood level of a prominent place-based policy (the Neighborhoods Act) implemented in some of the most deprived neighborhoods of the Spanish region of Catalonia in the period 2004-2010. The goal of the policy was, by investing heavily in public spaces and facilities, to attract natives and high income individuals and to reduce the concentration of poverty and immigration. The policy has had little (if any) impact on the population composition and dynamics of the treated neighborhoods. One notable exception, however, is that of the interventions that targeted the historic districts of the city of Barcelona. There, the policy significantly increased the native and EU-15 immigrant populations, reduced the non-EU15 immigrant share of the population and increased the share of college graduates. Given that the city center is experiencing an ongoing process of urban revival, the policy seems to have augmented this process into these deprived neighborhoods.

The results of this paper indicate that income-mixing policies dependent on investments in public spaces and facilities in neighborhoods in which poverty and immigration concentrate are generally not effective, even in the case of high-cost projects. This finding contrasts with the fact that, in the historic districts of Barcelona, the same policy has had a marked impact on the population dynamics of the treated neighborhoods. This outcome is in line with previous studies showing that the effects of place-based policies can be highly heterogeneous (Becker et al., 2013; Briant et al., 2015) and that successful policy experiences cannot always be generalized to other urban contexts.

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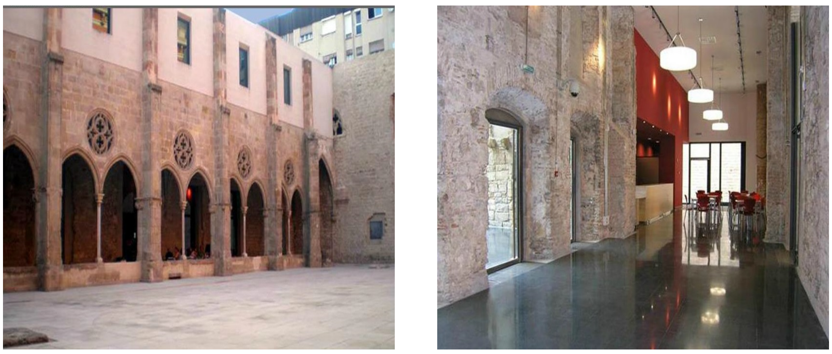
2.8 Appendix

2.8.1 Example of an urban renewal policy

Figure 2.4: Santa Caterina i Sant Pere, Barcelona, 15MER

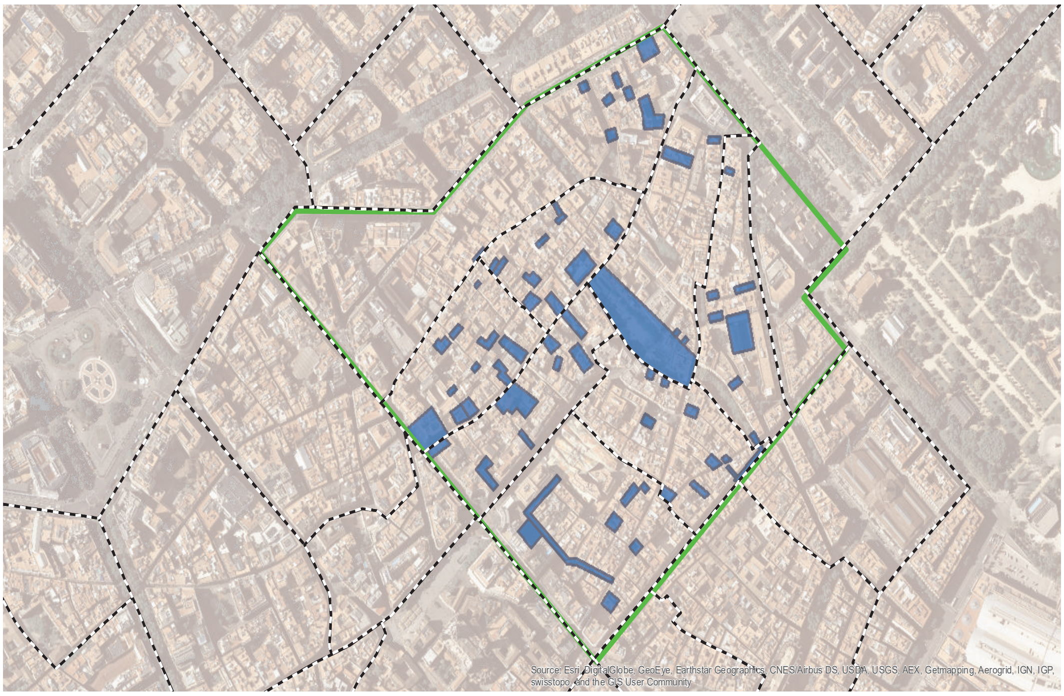
(a) Public spaces: *Pou de la Figuera*

(b) Public facilities: *Convent de Sant Agustí*



2.8.2 Consolidating intervention areas with census tracts: An example

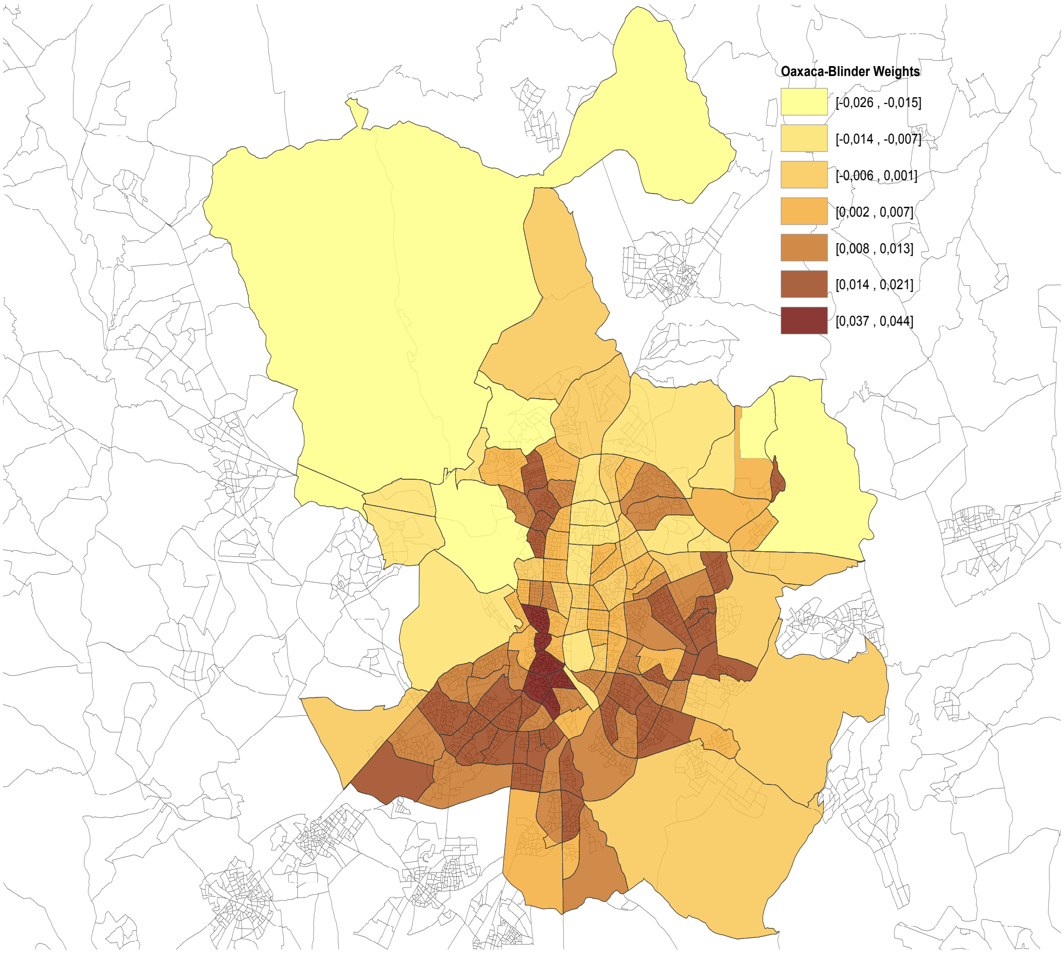
Figure 2.5: Santa Caterina i Sant Pere, Barcelona



Dashed lines indicate census tracts boundaries while the solid line delineates the intervention area. The colored areas indicate the exact location of all investments.

2.8.3 Placebo analysis for Madrid

Figure 2.6: Probability of treatment (Oaxaca-Blinder weights)



Neighborhoods (barrios) as defined by the Madrid's city council. Oaxaca-Blinder weights as defined in Kline (2011) reflect the probability of treatment. The specification used includes the variables considered in column 2 of Table 2.4, as well as an indicator for historic districts. The highest 3 values are *Embajadores*, *La Chopera* and *Palos de Moguer*.

3 Are high minority neighborhoods dynamically stable?[§]

3.1 Introduction

One striking fact about cities both in the US and in Europe is economic inequality across neighborhoods (Rosenthal and Ross, 2015; Tammaru et al., 2016). Another salient fact is the high degree of residential segregation based on racial or ethnic backgrounds (Cutler et al., 1999; Boustan, 2011), with the typical minority resident living in a neighborhood where the minority share is much larger than that of the city where she lives.

There are two broad causes that can explain the high degree of residential segregation of black and latino populations in the US or of immigrants in European cities. The first one is the correlation between income and ethnic or racial background (Bayer et al., 2004). The second one are social interactions in housing demand, whose study was pioneered by Schelling (1971)[1]. Card et al. (2008) introduced a model with social interactions that yields a one-sided tipping model. This model predicts the existence of tipping points, which are thresholds in the neighborhood minority share. Once a neighborhood surpasses its tipping point, the neighborhood becomes dynamically unstable as non-minorities will start leaving the neighborhood. Hence, neighborhoods with minority shares beyond the tipping point converge towards an all-minority equilibrium. One empirical prediction of one-sided tipping models is that there is a discontinuity in the non-minority population growth at the tipping point, where non-minority population growth should be lower to the right of the tipping point. Card et al. (2008) provide a methodology to identify tipping points and apply it to US metro areas. While they find evidence supporting the existence of tipping, their empirical test need not work in all applications. More specifically, their test of tipping can have limited power because it requires the tipping point to be exactly the same across all neighborhoods in the city. If the tipping point is, instead, heterogeneous across neighborhoods, tipping points might

[§]The paper in this chapter is coauthored with Jordi Jofre-Monseny.

[1]Relevant references here include Bayer et al. (2007), Boustan (2010) and Saiz and Wachter (2011).

be very hard to detect and identify with their methodology. One source of heterogeneity arises if, for instance, the non-minority population care differently about immigrants from different countries of origin, a situation that it is likely to arise in many contexts.

This paper provides a new empirical (and more powerful) test of one-sided tipping models (Card et al., 2008) that exploits an unusual set of events that took place in Spain in the period 2001-2015. In particular, we will examine population dynamics in the largest six Spanish cities (Barcelona, Madrid, Málaga, Sevilla, Valencia and Zaragoza) at the census tract level between 2001 and 2015. Caused by sustained economic growth, Spain experienced a massive immigrant inflow between the late nineties up until 2009. In fact, during these years Spain was the second highest recipient of immigrants in absolute terms (behind the US) and the highest relative to its population level. The recession, which ended in Spain in 2013, stopped these immigrant inflows and, in fact, the immigrant share slightly decline between 2010 and 2015.

In the immigrant boom period, 2001-2009, we document that new immigrants located in neighborhoods with an already high immigrant density in 2001. At the same time, these neighborhoods lost native population during these years. These facts, can be rationalized by two competing theories. The first one is tipping. Native residents leave or stop entering these neighborhoods because the high immigrant share is already beyond the tipping point. Alternatively, this pattern might simply be explained by the dynamics of income sorting. Since housing supply is rigid, the inflow of an immigrant mechanically reduces native population in the neighborhood, and immigrants did choose neighborhoods with a high immigrant share in 2001.

By contrast, these two competing theories yield different predictions for the immigrant freeze period, 2010-2015. If immigrant dense neighborhoods had tipped by 2010, these neighborhoods should be loosing native population between 2010-2015, despite the fact that the city is not currently receiving immigrant inflows during this period. Instead, if the observed pattern in 2001-2009 is simply dynamic sorting by income, we do not expect immigrant dense neighborhoods in 2010 to keep loosing native population between 2010 and 2015. The results clearly support the predictions of the tipping model, whereby native population keep leaving or avoiding immigrant dense neighborhoods despite small reductions in immigrant population.

To guide the empirical analysis, we set up a model of residential choice with two neighborhoods and two population groups (natives and immigrants in our case) as in Banzhaf and Walsh (2013) and Glaeser (2008). Neighborhoods differ in an exogenous amenity which creates income sorting as immigrants are poorer than natives. Both natives and immigrants have idiosyncratic preferences for neighborhoods. We consider two nested models. In the first one, we exclude social interactions. In the second one, we allow one group, the natives, to have preferences with respect to the neighborhood immigrant share, which constitutes an endogenous amenity in the model. These social interactions in housing demand yields multiple equilibria and a one-side tipping point as in Card et al. (2008). We use the model to study the effect of city-wide shocks in the immigrant share on neighborhood population dynamics. In an immigration boom period, the two models yield the same qualitative empirical predictions. Without social interactions, immigrants, which are the low income group, will outbid natives to live in low amenity neighborhoods, which are more affordable and also exhibit a higher initial immigrant share. With social interactions, neighborhoods with a high minority share will surpass the tipping point and will start loosing native population. Hence, both models predict that natives will leave immigrant dense neighborhoods. In the immigration freeze period, however, the two models generate different qualitative predictions. Without tipping points, if the city-wide proportion of immigrants falls, neighborhoods with a higher proportion of immigrants should experience an increase in native population. Instead, if neighborhoods were already tipping, natives should keep leaving or avoiding these neighborhoods until these become all-immigrant neighborhoods.

The paper that is more closely related to our is Card et al. (2008) which, to our knowledge, is the only paper that tests the empirical relevance of neighborhood tipping. The advantage of our empirical approach is that it does not require the tipping point to be exactly the same across all neighborhoods in the city. The theoretical framework that we develop indeed suggests that tipping points will be heterogeneous across neighborhoods. In fact, as we show in the paper, neighborhood population dynamics are consistent with tipping behavior, despite the fact that no obvious discontinuity in native population growth appears at a given immigrant minority share. Our study is also closely related to Fernández-Huertas Moraga et al. (2017) since they also study native population changes at the neighborhood level in response to immigration in Spain. They focus on the immigration boom period and estimate how immigrant inflows generate outflows of natives. Instead, we study more specifically the empirical predictions of tipping models by exploiting the fact that the immigration boom was followed by an immigration freeze period.

The paper is organized as follows. Section 2 provides a brief description of the immigration episodes in Spain in the period 2001-2015. Section 3 contains the theoretical framework developed in order to understand the effects of city-wide immigrant shocks on neighborhood population changes in a model with and without tipping points. Section 4 describes data sources, sample and the main outcome variables of the analysis. The empirical exercise is conducted in Section 5. First, we study neighborhood population dynamics in the immigration boom period, 2001-

2009 and, then, we conduct the analogous analysis for the immigrant freeze, 2010-2015. Section 6 concludes.

3.2 Immigration in Spain, 2001-2015

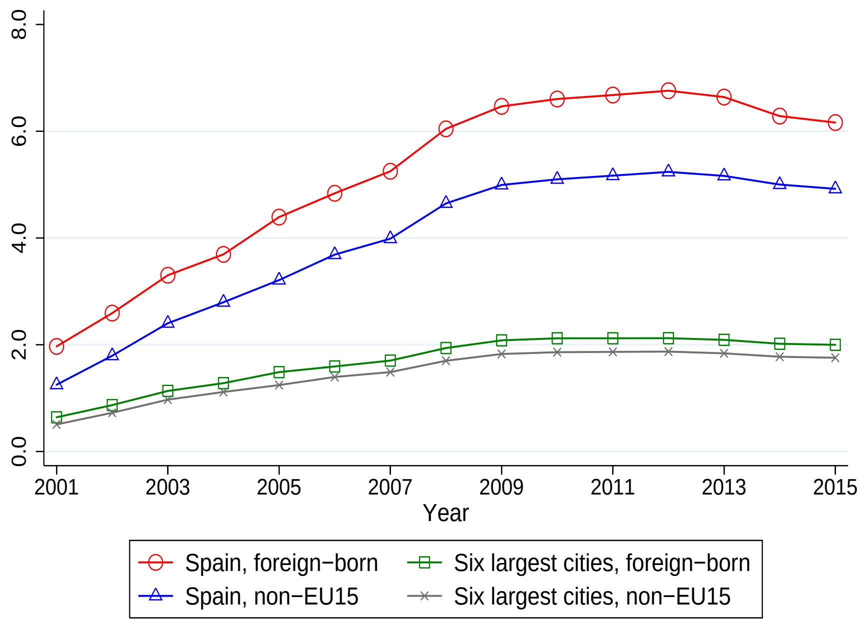
Starting in the late nineties, and coinciding with a period of sustained economic growth, Spain received a massive immigration wave (González and Ortega, 2011; Bertoli and Fernández-Huertas Moraga, 2013; Jofre-Monseny et al., 2016). In fact, during this period and according to the OECD International Migration Database, Spain was the second highest recipient of immigrants in absolute terms (behind the US) and the highest relative to its population level.

This massive immigration wave can be seen in Figure 3.1, with about 4.2 million of foreign-born individuals entering the country between 2001 and 2009. This immigration wave increased the share of foreign-born by 9 percentage points in this period, reaching 14% in 2009. We label this subperiod as the "immigration boom" period. If we focus on the 6 cities that constitute our object of study (Barcelona, Madrid, Málaga, Sevilla, Valencia and Zaragoza), the relative inflow is even larger, with the share of foreign born climbing to 17% in 2009. The list of countries sending large numbers of immigrants to Spain is large with flows from Ecuador, Morocco, Rumania and Colombia being the largest ones. The typical immigrant arriving to Spain in this period was relatively low-skilled. According to the 2011 Census, while 64 (31) percent of natives had, at least, secondary (tertiary) education, this percentage was 55(20) for non-EU immigrants. As for the housing market, immigrants are more likely to be renters than natives. According to the 2011 Census, while 83% of the people born in Spain lived in an "owner" household, this percentage was only 43% for people born abroad. Unlike other European countries, the stock of social housing is very low and, thus, the typical immigrant is a market renter.

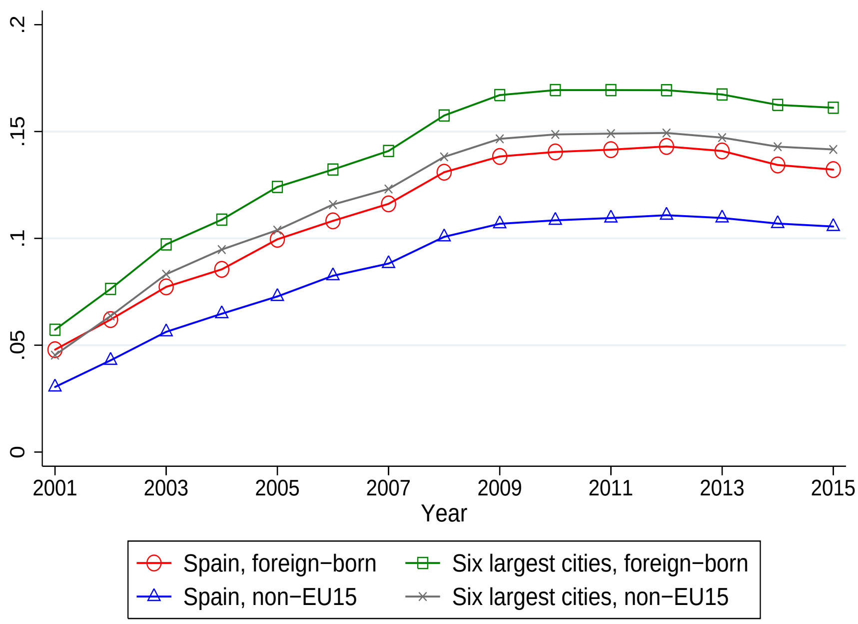
The Great recession hit Spain in the last quarter of 2008 and GDP growth was negative for the period 2008-2013. With some delay, this change in economic conditions stopped the immigration inflows. Focusing on the 6 largest cities, the immigrant population plateaus in 2010 and decreases from 2013 to 2015. We label the period 2010-2015 the "immigration freeze" although, in fact, the foreign-born population in Spain decreased from 6.6M in 2010 to 6.2M in 2015. As it will be clear in the next section, the "immigration freeze" episode that followed the "immigration boom" period will prove useful to assess if tipping is a pervasive phenomenon in neighborhood population dynamics.

Figure 3.1: Evolution of immigration in Spain & the six largest cities.

a) Number of foreign-born



b) % of foreign-born



3.3 Theoretical Framework

To guide and interpret the empirical analysis we use a model of residential choice with two neighborhoods and two population groups (natives and immigrants). The model is a variant of those developed in Banzhaf and Walsh (2013) and in Glaeser (2008) and is described at length in González-Pampillón et al. (2016). Neighborhoods differ in an exogenous amenity as well as in the neighborhoods' immigrant share which, constitutes an endogenous amenity when natives care about the social composition of neighborhoods. We consider two nested models. In the first one, we do not consider social interactions and thus, the exogenous amenity is the only neighborhood characteristic considered. In the second one, we take social interactions into account. When natives' preferences to avoid high-immigrant neighborhoods are strong enough, the model allows for multiple equilibria as in the one-side tipping point model of Card et al. (2008). We first analyze the predictions of a city-wide immigration shock in the the two models: without and with social interactions. Then, we study the same predictions when city-wide immigration inflows stop.

3.3.1 Model set-up

The city, whose size is normalized to one, has two neighborhoods. The size of neighborhood 1 is S while that of neighborhood 2 is $1-S$. There are two population groups: Natives (denoted by N) and immigrants (denoted by I). While P denotes the city-wide proportion of foreign-born population, P_1 and P_2 represent the proportion of immigrants in neighborhoods 1 and 2, respectively. We consider equilibria in which $P_1 \geq P_2$, which implies that we only consider symmetric or asymmetric equilibria where community 1 has a higher minority share. We further assume that $S < P < .5$, which implies that there are more natives than immigrants in the city and that foreign-born population is large enough to fill-up community 1, allowing neighborhood 1 to be an all-immigrant community, i.e. $P_1 = 1$.

We make the assumption that each resident consumes one unit of housing. If residing in neighborhood 1, natives' utility is $U_1^N = Y^N - Q - \alpha(P_1) + \gamma^N G + a_n$, where Y^N represents the income level of natives and Q is the housing price in neighborhood 1. The next two elements reflect endogenous and exogenous neighborhood amenities. $\alpha(P_1)$ is a term that captures the impact the neighborhood immigrant share on the utility of natives, while G is a term that subsumes all relevant and exogenous neighborhood attributes. The parameter γ^N denotes the willingness to pay of natives for G. In turn, a_n is the only individual specific term which reflects neighborhood attachment of individual n to neighborhood 1 with respect to neighborhood 2. a_n is assumed to be uniformly distributed in the unit interval. If natives reside in neighborhood 2, they derive the following utility, $U_2^N = Y^N - \alpha(P_2)$, where Q, G and a_n in neighborhood 2 have been normalized to zero.

Immigrants derive the following utility when living in neighborhood 1, $U_1^I = Y^I - Q + \gamma^I G + a_i$ and $U_2^I = Y^I$ from living in neighborhood 2, given the normalizations on Q and G explained above. Since previous evidence suggests that residential segregation mainly emerge due to non-minorities efforts to avoid living in communities with high minority shares (Cutler et al., 1999; Card et al., 2008; Boustan, 2011), we make the assumption that immigrants do not value the social composition of neighborhoods. As it is empirically the case that $Y^N > Y^I$ we further assume that $\gamma^N > \gamma^I$ given that the willingness to pay for amenities is increasing with income (Kuminoff et al., 2013; Kahn and Walsh, 2015; Koster et al., 2016). Since we focus on equilibria in which $P_1 \geq P_2$, it follows that G cannot be positive. Finally, the term $a_i \sim U(0,1)$ reflects the individual specific attachment of immigrant i for neighborhood 1.

3.3.2 Equilibria

To study the equilibria of the model, we focus on the minority share in neighborhood 1, which fully determines the proportions of both groups in the two neighborhoods[2]. We first study the equilibrium without social interactions and then describe the equilibria with social interactions.

Without social interactions, $\alpha(\cdot) = 0$, there is a unique and mixed equilibrium. In that equilibrium, there is an immigrant who is indifferent between neighborhoods, with a willingness to pay for neighborhood 1 given by $Q(a_{i^*}) = \gamma^I G + a_{i^*}$. There is also an indifferent native, whose willingness to pay to live in neighborhood 1 amounts to $Q(a_{n^*}) = \gamma^N G + a_{n^*}$. In equilibrium, the two marginal residents' willingness to pay must be equal, i.e. $Q(a_{i^*}) = Q(a_{n^*})$. Solving this equation gives us the equilibrium immigrant share: [3]

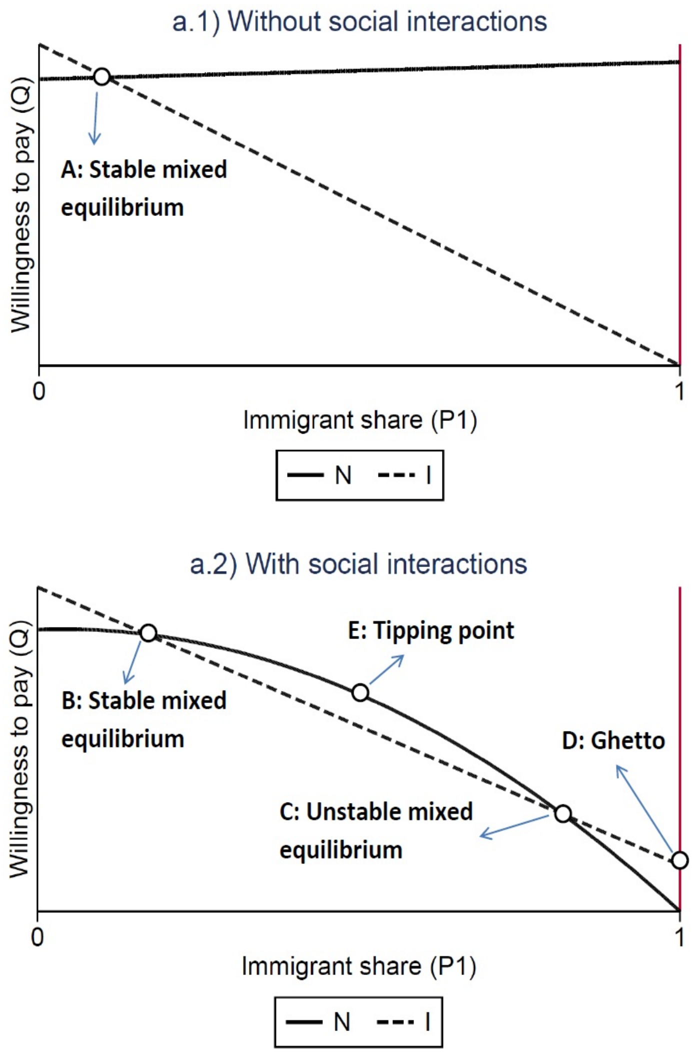
$$P_1^{No-Int} = P + \frac{P(1-P)}{S}(\gamma^I - \gamma^N)G \tag{3.1}$$

Panel a.1) in Figure 3.2 illustrates an example of the model's equilibrium without social interactions. The solid and the dashed lines represent the willingness to pay to live in community 1 for natives and immigrants, respectively. The equilibrium is asymmetric ($P_1 > P$) because neighborhood 1 is a less desirable place to live ($G < 0$) and natives' willingness to pay for G exceeds that of immigrants ($\gamma^I < \gamma^N$). In this

[2] Note that $P_2 = (P - S\,P_1)/(1-S)$, while the proportion of natives in neighborhoods 1 and 2 are $N_1 = 1 - P_1$ and $N_2 = 1 - P_2$, respectively.

[3] We have used the fact $a_{i^*} = 1 - S(P_1/P)$, $a_{n^*} = 1 - S((1-P_1)/(1-P))$ and $P_2 = (P - SP_1)/(1-S)$.

Figure 3.2: Model equilibrium, minority share in neighborhood 1



Notes: Each panel shows the equilibrium minority share in neighborhood 1.

case, residential segregation arises because of the correlation between income and immigrant status.

To study the model with social interactions, we rely on the parametric assumption that $\alpha(P_i) = \alpha P_i^2$. If social interactions are strong enough, this functional form produces a one-side tipping point model which has found empirical support by Card et al. (2008). The one sided tipping model shows two stable equilibria: The mixed ($P_1 < 1$) and ghetto ($P_1 = 1$) configurations which correspond to points B and D in panel a.2). In the mixed equilibrium ($P < 1$), there is one marginal native and one marginal immigrant with willingness to pay to live in neighborhood 1 of $Q(a_{i^*}) = \gamma^I G + a_{i^*}$ and $Q(a_{n^*}) = \alpha P_2^2 - \alpha P_1^2 + \gamma^N G + a_{n^*}$, respectively. In such an instance, the equilibrium minority in neighborhood 1 amounts to:

$$P_1^{Int} = P + \frac{P(1-P)}{S}\left[\alpha(P_1^2 - P_2^2) + (\gamma^I - \gamma^N)G\right] \tag{3.2}$$

An example of such equilibrium is point B in panel b.1) where the natives and the immigrants willingness to pay curves intersect first. The equilibrium is asymmetric as in the model without interactions ($P_1 > P$), the difference being that endogenous amenities $\alpha(P_1^2 - P_2^2)$ magnify the asymmetry. Point C, the all-immigrant neighborhood is the other stable equilibrium[4]. Finally, point D is the model's tipping point, P_1^{tip}, which is the highest minority share that can sustain a stable mixed equilibrium. No stable neighborhood can exist between the tipping point and $P_1 = 1$, implying that neighborhoods to the right of the tipping point must be transitioning towards an all-immigrant equilibrium. With α quadratic, the tipping point is

$$P_1^{tip} = \frac{1}{1-2S}\left(\frac{S(1-S)^2}{2\alpha P(1-P)} - SP\right) \tag{3.4}$$

Note that equation 3.4 suggests that tipping points might be heterogeneous. The exact location of the tipping point depends on the city-wide immigrant proportion (P), as well as on the size of the neighborhood, (S). In practice, the fact that natives might care differently about immigrants with different backgrounds implies that the tipping point in each neighborhood will depend on the exact mix of immigrants in each neighborhood.

3.3.3 Neighborhood population dynamics with a city-wide immigration inflow

Here, we analyze the predictions of a city-wide immigration shock ($P \uparrow$) with a model with and without social interactions. Panel a.1) in Figure 3.3 shows the effect of the shock in the model without social interactions. The immigrants' willingness to pay curve shifts outwards, increasing the neighborhood proportion of immigrants

[4] In an all-immigrant equilibrium, no native wants to enter neighborhood 1 when $P_1 = 1$. The condition that guarantees this is:

$$\frac{S}{P} - \alpha(1) + \alpha(P_2) + (\gamma^N - \gamma^I)G < 0 \tag{3.3}$$

It states that social interactions need to be strong enough. It turns out that this condition also ensures that there is multiple equilibria.

to point F. The question is if the increase in P_1 is larger or smaller than the city-wide increase in P. Note that without social interactions, the level of the exogenous amenity (G) is the only factor that determines the way in which the immigration inflow is allocated across neighborhoods. Formally, we take the derivative of the equilibrium immigrant share in neighborhood 1 (equation 3.1) with respect to P:

$$\frac{dP_1^{No-Int}}{dP} = 1 + \frac{(1-2P)(\gamma^I - \gamma^N)G}{S} > 1. \tag{3.5}$$

The second term is strictly positive implying that the expression is larger than one. Hence, an inflow of immigrants in the city does not increase the immigrant share by the same amount in the two neighborhoods. Instead, neighborhood 1, with $G < 0$ and with $P_1 > P_2$, will experience the larger increase in the immigrant share.

Next, we turn to the case in which the model features social interactions. Here, there are two possible instances. The first one corresponds to the illustration in panel a.2) in Figure 3.3. The increase in P is large enough to bring P_1 beyond the tipping point (P_1^{tip}), and so the immigrant inflow brings the neighborhood from being mixed to point H. If the increase in P is not large enough to bring neighborhood 1 beyond the tipping point, the increase in P_1 will also be larger than P for two different mechanisms. First, as revealed by expression 3.5, the less desirable the neighborhood is, the more immigrants it will attract as $\gamma^N > \gamma^I$. With social interactions, this effect is reinforced because the neighborhood becomes less attractive for natives as P_1 increases more than P_2[5].

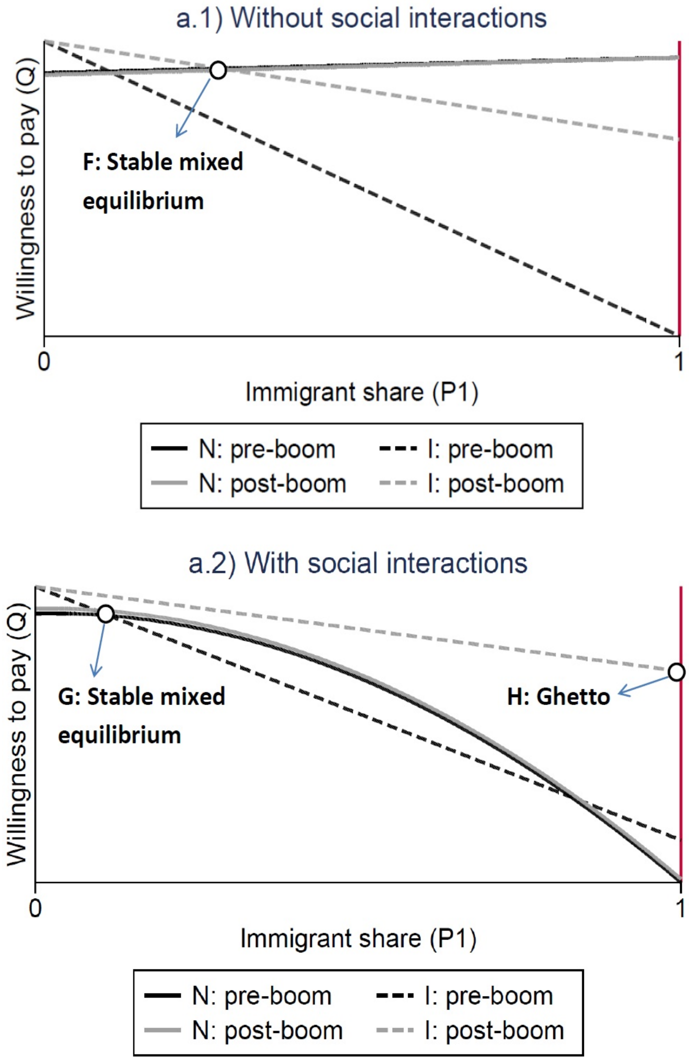
To summarize these predictions, after an increase in the city-wide immigrant share ($P \uparrow$), both models predict that the immigrant share will increase more in the neighborhood with a lower level of the exogenous amenity, which is the neighborhood with a higher immigrant share to start with. Hence, in periods with an increase in immigration at the city-level, the model with and without social interactions produce the same testable predictions.

[5]Formally:

$$\frac{dP_1^{Int}}{dP} = \frac{1 - \frac{2\alpha P_2 P(1-P)}{S(1-S)} + \frac{(1-2P)}{S}\left[\alpha(P_1^2 - P_2^2) + (\gamma^I - \gamma^N)G\right]}{1 - \frac{2\alpha P(1-P)}{S}\left[P_1 + P_2\frac{S}{1-S}\right]} > 1 \tag{3.6}$$

Note that, the term $\frac{(1-2P)}{S}\left[\alpha(P_1^2 - P_2^2) + (\gamma^I - \gamma^N)G\right]$ is positive. Hence, a sufficient condition for $\frac{dP_1}{dP} > 1$ is that $\frac{2\alpha P_2 P(1-P)}{S(1-S)} \leq \frac{2\alpha P(1-P)}{S}\left[P_1 + P_2\frac{S}{1-S}\right]$. After some math, this latter expression boils down to $2\alpha P_2 \leq 2\alpha P_1$. Then, $\frac{dP_1}{dP} > 1$ if $P_1 > P_2$ which also holds by assumption.

Figure 3.3: The effect of increasing the city-wide immigration share

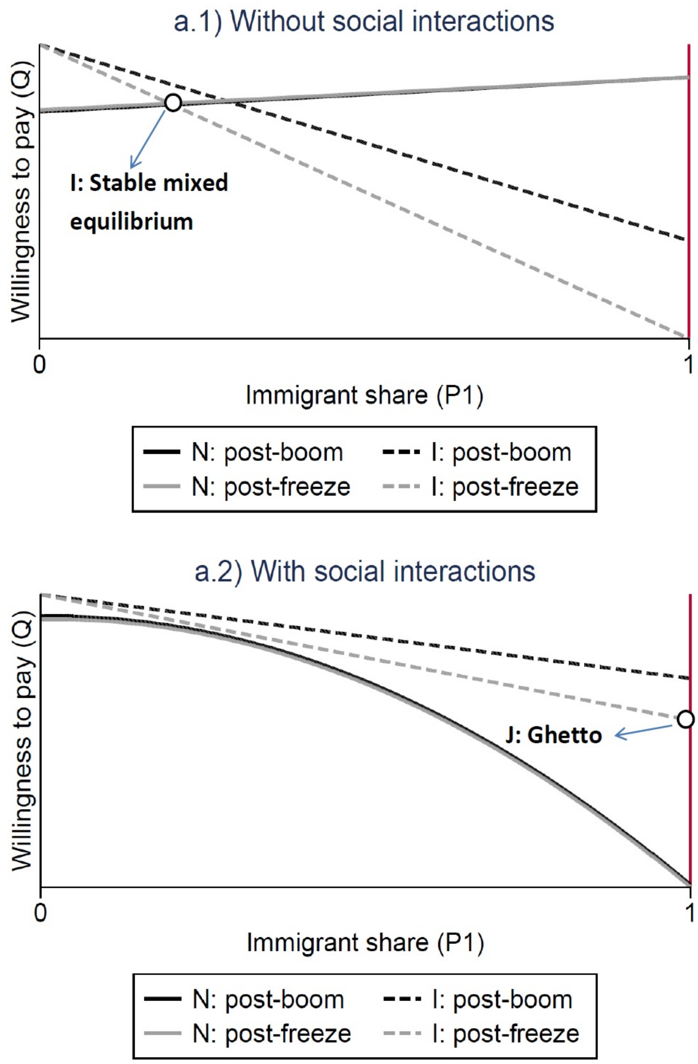


Notes: Each panel shows the minority share in equilibrium in neighborhood 1 before and after an **increase** in *P*.

3.3.4 Neighborhood population dynamics with an immigration freeze

We now turn to the predictions of the model with and without social interactions if a city, after experiencing an immigration boom, experiences a slight decrease in the city-wide level of immigration. Figure 3.4 illustrates the case for a model without and with social interactions. Panel a.1) illustrates the case with no social interactions. The fall in P reduces the immigrant share in neighborhood 1 to point I. In fact, P_1 decreases more than P given that city-wide immigration inflows and outflows have symmetric impacts on P_1. This symmetry also occurs in the model with social interactions if the initial increase in P_1 did not cause neighborhood 1 to surpass the tipping point P_1^{tip}. If, instead, the immigration boom period brought P_1 beyond the tipping point as shown in panel a.2), a city-wide immigration outflow would not stop neighborhood 1 from transitioning towards point J. Hence, studying neighborhood population dynamics in an immigration freeze period that followed an immigration boom can shed light on the prevalence of tipping points. Specifically, the tipping model generates empirical predictions that clearly differ from a model without social interactions.

Figure 3.4: The effect of decreasing the city-wide immigration share



Notes: Each panel shows the minority share in equilibrium in neighborhood 1 before and after a **decrease** in *P*.

3.4 Data and variables

3.4.1 Data sources

Our main data source is the Spanish Municipality Registry (*Padrón Municipal de Habitantes*) which contains population counts by country of birth at the census tract level, our geographical unit of analysis. The population counts refer to January 1 and are used to produce official population counts[6]. Since no proof of legal status is required, both regular and irregular immigrants are registered in the *Padrón*. On the one hand, registration enables residents to access public services such as primary health care or education. On the other hand, being registered in the *Padrón* is a proof of residence in nationality and residence permit applications. From 2005 onwards, undocumented immigrants must renew their registration every two years and are removed from the registry if they fail to do so. Hence, in the period we analyze the *Padrón* measures the native and immigrant populations accurately (Foremny et al., 2017). We focus on country of birth rather than on nationality to define immigrants as a significant fraction of immigrants obtain the nationality after a few years of residing in the country. This data source is supplemented with the 2001 and 2011 censuses, which provide us with additional information on the socio-economic and physical characteristics of census tracts.

3.4.2 Sample

We consider the six largest cities in Spain in terms of population: Barcelona, Madrid, Málaga, Sevilla, Valencia and Zaragoza. To study neighborhood population dynamics, we want to avoid abnormal neighborhood population growth. To exclude areas at (or close to) the city fringe, we use a metropolitan area definition based on contiguity of development and not on commuting patterns. Specifically, we use the AUDES definition of urban nucleous[7]. The six cities used in this analysis are shown in appendix 4.8.4.

We build a balanced panel of census tracts, which constitutes our definition of neighborhood, for the period 2001-2015. We explain how we deal with census tract redefinitions in Appendix 3.7.2. In the six cities that we consider, there are a total of 8,463 census tracts. Because of census tract redefinitions, and because we want to avoid newly developed areas, we drop: i) tracts with a yearly growth rate above 50%, below -50%, or with yearly population growth rate above 20% with the previous or following year experiencing a growth rate below -10%, ii) tracts with

[6]See Foremny et al. (2017) for a detailed description of the *Padrón Municipal de Habitantes*.

[7]The AUDES definitions for Núcleo urbano and Área urbana are build by the Grupo Alarcos, Universidad de Castilla-La Mancha, `https://alarcos.esi.uclm.es/per/fruiz/audes/`.

a growth rate in the number of dwellings above 50% between the 2001 and 2011 censuses, and iii) tracts with no dwellings in either 2001 or 2011. The final sample contains 7,133 tracts. The average size of each census tract is 37 hectares and 1,341 inhabitants.

3.4.3 Variables

To define immigrants, we focus on non-EU15 population, which includes immigrants born in non-European countries in addition to Romania, Bulgaria, Poland, Russia and Ukraine which are the main sending countries in Eastern Europe. Our definition of natives includes Spain as well as the main sending countries of the EU15 (France, Germany, Italy, Portugal and UK)[8]. As can be seen in Figure 3.1, non-EU15 immigration makes the lyon's share of the Spanish immigration wave.

Table 3.1 presents descriptive statistics for the neighborhoods in the sample. The first set of rows describes the demographic outcomes that we will examine in the empirical analysis. We will mostly examine changes in the percentage of immigrants and population growth at the neighborhood level. We will also decompose the population growth rate by the relative contribution of natives and immigrants as in Card et al. (2008), namely:

$$\frac{Pop_{t1} - Pop_{t0}}{Pop_{t0}} = \frac{Nat_{t1} - Nat_{t0}}{Pop_{t0}} + \frac{NonEU15_{t1} - NonEU15_{t0}}{Pop_{t0}} \tag{3.7}$$

where $t0$ and $t1$ will be 2001 and 2009 for the immigration boom period and 2010 and 2015 for the immigration freeze period.

Between 2001 and 2009, the average population growth rate is 5%, indicating most tracts gained population over this period. This 2001-2009 population growth is largely explained by the immigration boom. In fact, the contribution to growth of natives is, on average, slightly negative, although the data shows substantial variation. As expected, the population of non-EU15 immigrants increased dramatically between 2001 and 2009. The change in the number of immigrants (relative to the 2001 population level) is, on average, 11% in this period. As a results, the average increase in the share of immigrants is of almost 10 percentage points. In the second period, 2010-2015, the average population growth rate is negative, -4% , and the average tract loses both native and non-EU15 population. The average neighborhood experiences a 0.57 percentage point decline in the percentage of immigrants.The

[8] At the census tract level, the country of origin is only disclosed for Spain as well as the main sending countries listed above. That means that we cannot assign people from smaller countries (e.g. Georgia) into these categories as people born in Georgia are in the 'Rest of Europe' category.

last rows of Table 3.1 show relevant census tract characteristics drawn from the 2001 census.

Table 3.1: Descriptive statistics

Variable	Mean	Std. Dev.	Min	Max
Neighborhood population changes				
Population growth rate (2009 - 2001)	0.05	0.18	-0.43	2.12
Δ natives (2009 - 2001, relative to pop. in 2001)	-0.08	0.15	-0.50	1.56
Δ non-EU15 (2009 - 2001, relative to pop. in 2001)	0.11	0.09	-0.09	0.76
Δ share non-EU15 (2009 - 2001)	9.83	6.87	-5.60	48.19
% of non-EU15 immigrants, 2001	4.19	3.75	0.00	36.42
Population growth rate (2015 - 2010)	-0.04	0.08	-0.40	2.11
Δ natives (2015 - 2010, relative to pop. in 2010)	-0.03	0.07	-0.35	1.94
Δ non-EU15 (2015 - 2010, relative to pop. in 2010)	-0.01	0.03	-0.26	0.30
Δ share non-EU15 (2010 - 2015)	-0.57	2.32	-21.57	13.79
% of non-EU15 immigrants, 2010	14.23	8.84	0.40	60.60
Neighborhood characteristics, 2001 census				
Distance to the CBD	6.49	5.60	0.00	35.53
Distance to closest transportation network	0.87	1.59	0.01	18.04
Average dwelling size	80.87	18.00	39.38	174.88
% of rented households	15.64	12.92	0.00	95.91
% of building in poor condition	3.97	7.80	0.00	100.00
% of buildings with garage	41.69	39.79	0.00	100.00
% of residents >10 yrs, without a high-school diploma, 2001	57.60	17.03	12.85	96.88
Number of observations	7,133			

3.5 Neighborhood population dynamics

In this section, we analyze neighborhood populations dynamics for the two different subperiods. First, we examine the immigration boom (2001-2009), where city-wide immigration (P in the residential choice model above) increase from around 5% to almost 17%. In the second subperiod, the immigration freeze (2010-2015), the massive migration inflows stopped and in fact, we observe a one percentage point decrease in city-wide immigration share.

3.5.1 The immigration boom period

We start the analysis with panel a) in Figure 3.5, which shows the 2001-2009 changes in the immigrant share as a function of the immigrant share in 2001. Each dot plots bin averages where each bin contains the same number of neighborhoods. The fact that bins become less numerous as the immigrant share in 2001 increases reflects that there are fewer neighborhoods with very large immigrant shares. The solid line is a local linear regression fit using all (non-binned) data points, while dashed lines are 95% confidence bands of the regression fit. The graph clearly shows that neighborhoods with a higher minority share in 2001 experienced larger influxes of immigrants in the 2001-2009 period.

In panels b) and c) of Figure 3.5 we analyze the extent to which this pattern is driven by the behavior of natives, immigrants or both. In panel b) we show neighborhood population growth, together with the relative growth contributions of native and immigrant populations as defined in equation 3.7. Neighborhoods with a relatively high immigrant share in 2001 experienced substantial population growth, being about 10% for neighborhoods in which the immigrant share in 2001 was beyond 7 or 8%. This growth is driven by immigrant inflows, which clearly increase with the minority share in 2001 up until 10%. At this point, the immigrant contribution to population growth plateaus. Natives offset part of this effect, as their growth is decreasing with the immigrant share in 2001. Given that we focus on areas with nearly fixed housing supply, these positive rates of population growth are likely to be explained by a process of urban densification in neighborhoods with high immigrant shares (Standish et al., 2010; Krivo, 1995). This densification could be the result of immigrant families being larger, or by the fact that immigrants are more prone to share dwellings.

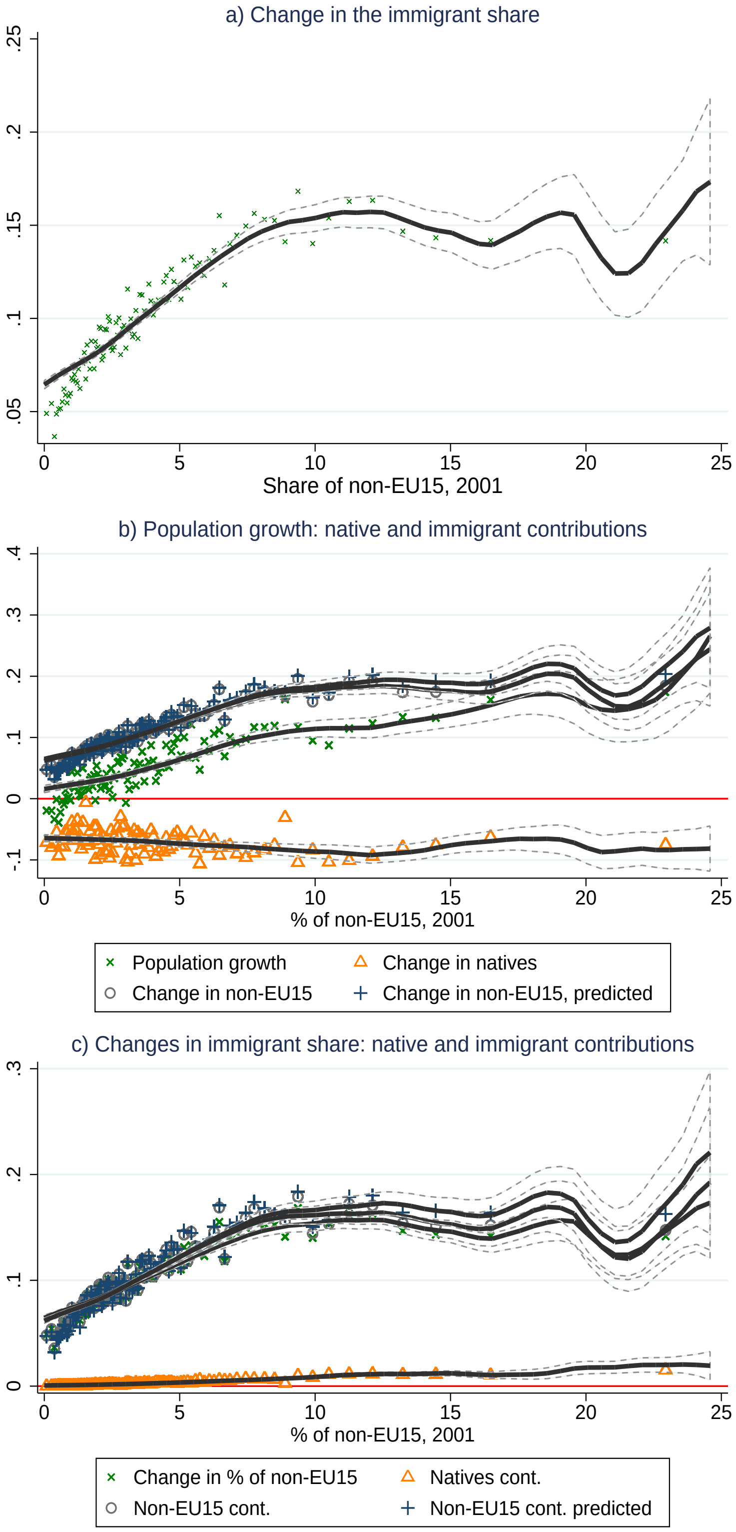
We can also decompose the changes in the immigrant share between 2001 and 2009 in terms of native and immigrant contributions. Specifically, changes in the immigrant share can be approximated as:

$$d\left(\frac{NonEU15}{NonEU15+Nat}\right) \simeq \frac{\partial\left(\frac{NonEU15}{NonEU15+Nat}\right)}{\partial NonEU15} dNonEU15 + \frac{\partial\left(\frac{NonEU15}{NonEU15+Nat}\right)}{\partial Nat} dNat \tag{3.8}$$

Panel c) in 3.5 shows the results of this exercise for 2001-2009 changes, where $dNat$ and $dNonEU15$ are 2001-2009 changes in population (in absolute numbers) and the partial derivatives of equation 3.8 are evaluated at 2001 levels. The results indicates that both natives and immigrants have contributed to the fact that the immigrant share increased more in neighborhoods with a high immigrant share in 2001. However, most of the effect is driven by the behavior of immigrants.

The literature that has studied new immigrants' location decisions has emphasized that new migrants tend to locate where earlier fellow-countrymen located both in the US (Altonji and Card, 1991; Saiz and Wachter, 2011) and in Spain (Gonzalez and Ortega, 2013; Jofre-Monseny et al., 2016; Fernández-Huertas Moraga et al., 2017).

Figure 3.5: Neighborhood population dynamics during the immigration boom (2001-2009)



Notes: Data points are bin averages while solid lines are local linear regression fits on non-binned data using an Epanechnikov kernel. Dashed lines are 95% confidence bands.

In panel b) and c) we report predicted shift-share immigrant inflows using the distributions of immigrants at the beginning of the period ($t0$) by country of birth (the share), and the nation-wide population increase between $t0$ and $t1$ by country of origin (the shift). Specifically, we build:

$$\widehat{NonEU15_{t1} - NonEU15_{t0}} = \left(\sum_c I_{c,t0}(1+g_c)\right) - NonEU15_{t0} \tag{3.9}$$

where $I_{c,t0}$ is the head count of immigrants born in (non-EU15) country c in each neighborhood at time $t0$, and g_c is the nation-wide population growth rate between $t0$ and $t1$ of individuals born in country c. In panels b) and c) we plot the predicted contributions of immigrants to population growth and to changes in immigrant shares, respectively. In both cases, the shift-share contributions match really well the observed contributions which indicates that new immigrant waves tend to increase the number of immigrants in each neighborhood by the same proportion[9].

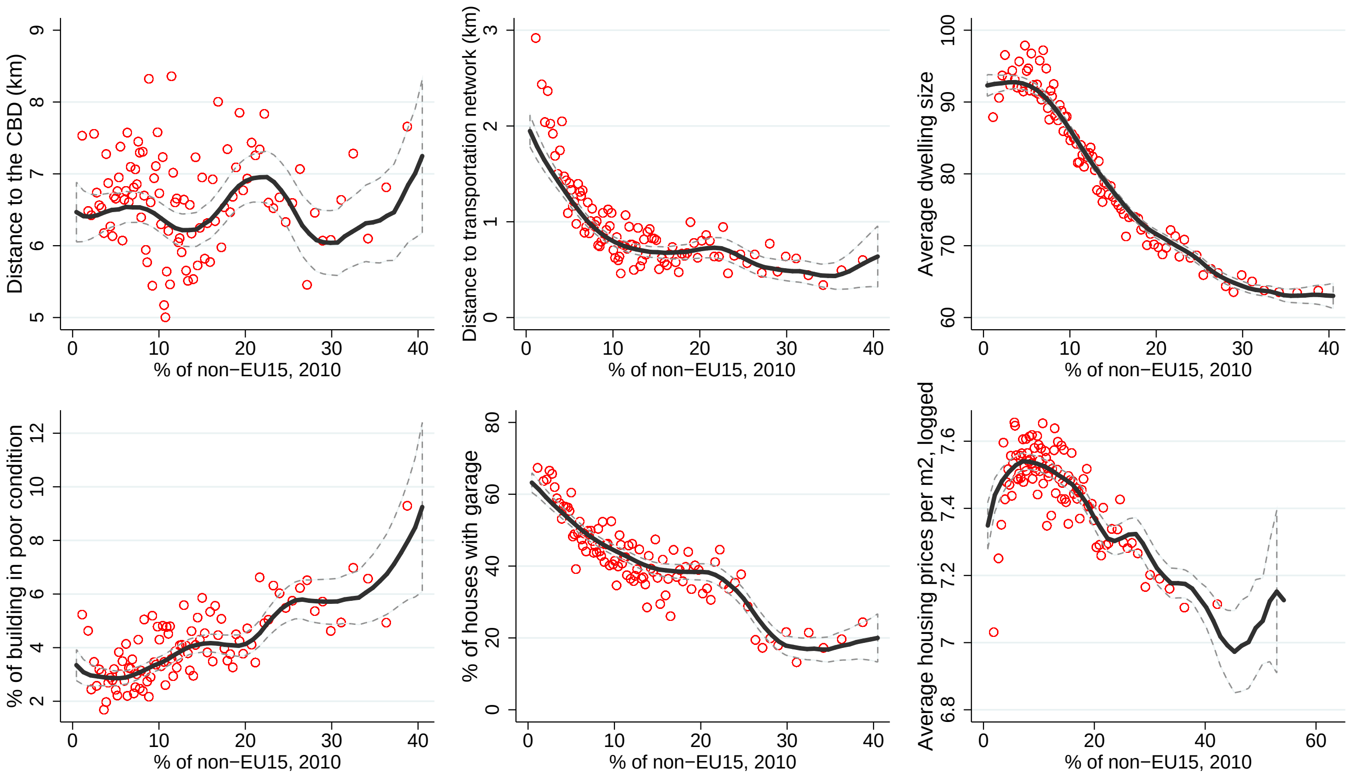
The neighborhood population dynamics of Figure 3.5 clearly indicate that neighborhoods with a higher proportion of immigrants in 2001 received larger inflows of immigrants in the 2001-2009 period, and experienced moderate native population losses. A natural question that follows is what characteristics do neighborhoods that received more immigrants have. To that end, in Figure 3.6 we plot the relationship of several neighborhood attributes in 2001 against the 2010 immigrant share in the neighborhood. In terms of our model, we think of this 2001 neighborhood characteristics as dimensions of the exogenous neighborhood amenity G. We first explore if immigrants settle close to the city center or close to the transportation network. While no clear pattern arises with respect to distance to the city center, immigrant density is higher in neighborhoods with better access to the public transportation network[10]. With respect to characteristics of the housing stock, neighborhoods with more immigrants in 2010 are neighborhoods with smaller dwellings, with more dwellings in poor conditions and with more dwellings without a garage. For the city of Barcelona, we also have house price data at the census tract level[11]. There, immigrant dense neighborhoods also happen to be in more affordable areas of the city.

[9]If this is the case, then the dissimilarity index of residential segregation will be unaltered when the proportion of immigrants in the city varies (Glaeser, 2008).

[10]This result is related to **?**. These authors find that public transportation plays a role in attracting the poor to city centers.

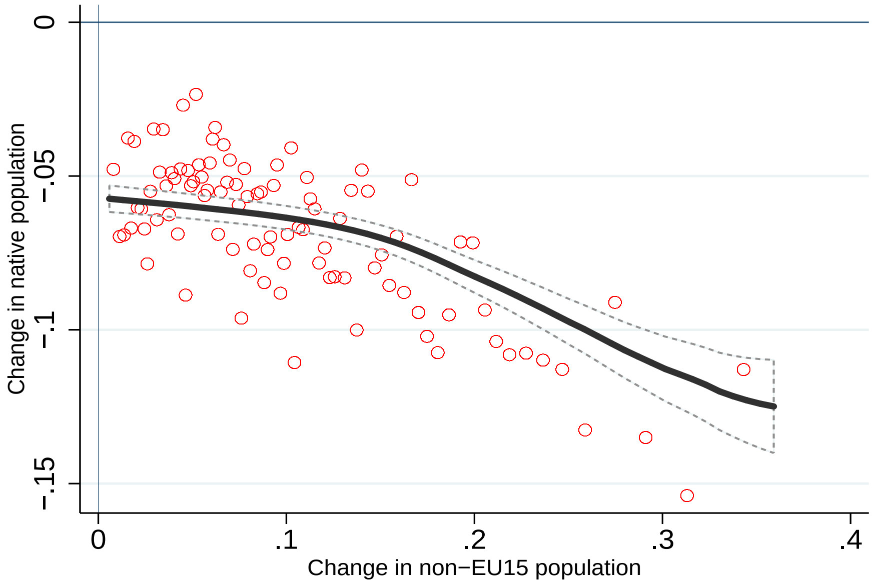
[11]We have individual-level data on all second-hand house transactions for the region of Catalonia for the period 2009-2016. The prices used in this analysis are neighborhood averages for the whole period. These data are drawn from individual declarations of a Stamp-duty tax, the *Impuesto sobre Transmisiones Patrimoniales*.

Figure 3.6: Where did immigrants settle during the immigration boom?



Notes: House-prices only available for the city of Barcelona (see text for details). Data points are bin averages while solid lines are local linear regression fits on non-binned data using an Epanechnikov kernel. Dashed lines are 95% confidence bands.

Figure 3.7: Native and non-EU15 neighborhood population changes during the immigration boom, 2001-2009.



Notes: Population changes expressed relative to the population level in 2001. Data points are bin averages while solid lines are local linear regression fits on non-binned data using an Epanechnikov kernel. Dashed lines are 95% confidence bands.

Overall, the analysis indicates that new immigrants in the 2001-2009 period located in neighborhoods with a high proportion of immigrants in 2001, which also tend to be neighborhoods with more affordable housings and where the housing stock is of lower quality. Note that these results are consistent with both the models with and without social interactions described in Section 3. With no social interactions, sorting by income still predicts immigrants will locate in areas with low amenities, low housing prices and with a higher proportion of immigrants to start with. With social interactions that cause tipping, the dynamics are qualitatively similar as neighborhoods with a high proportion of immigrants in 2001 should be transitioning towards an all-immigrant neighborhood.

To complete the analysis of the immigration boom period, and following Boustan (2010), Saiz and Wachter (2011) and Fernández-Huertas Moraga et al. (2017), in Figure 3.7 we plot neighborhood 2001-2009 changes in natives against non-EU15 population changes, where the tow variables are expressed relative to the population in 2001. The figure shows a negative correlation, with inflows of non-EU15 population being associated with native outflows, which is in line with the findings of Fernández-Huertas Moraga et al. (2017) for Spain during this same period. Again, though, this evidence is consistent with both a model with and without social interactions. Note that we are focusing on neighborhoods where new developments are limited and, thus, where the housing supply is rigid. In that context, we expect a mechanical correlation between immigrant inflows and native outflows even if no social interactions exist. Boustan (2010) argues that white flight occurs when one new black neighbor causes a white outflow which is larger than one. In our application, the densification of neighborhoods documented above would make this interpretation problematic.

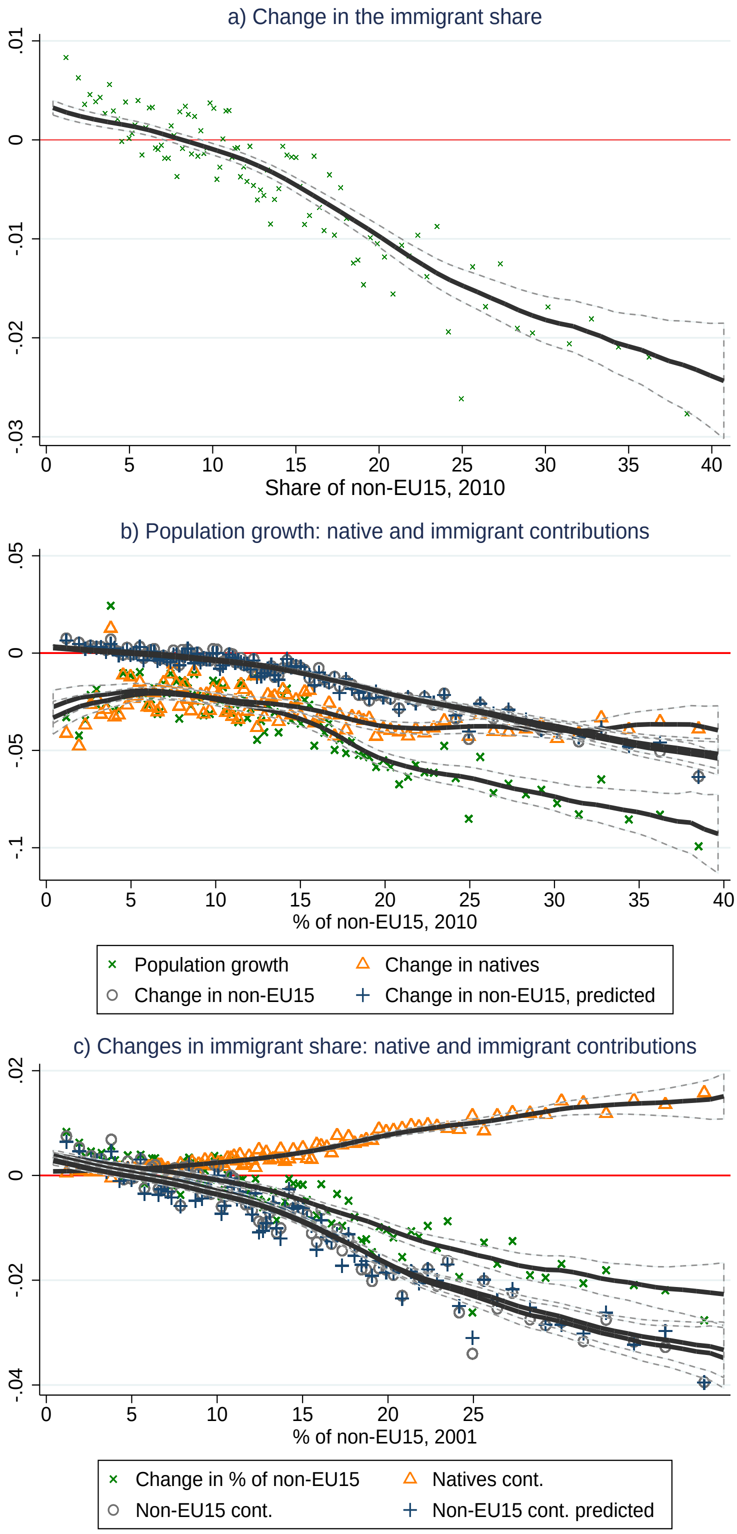
3.5.2 The immigration freeze period

Having analyzed neighborhood population dynamics in the immigration boom period, we now turn to the analysis of the immigration freeze, 2010-2015, where inflows of non-EU15 stopped and, in fact, the city-wide proportion of immigrants decreased by one percentage point over the period. Under the immigration freeze period that followed the immigration boom, the predictions of a residential choice model with and without social interactions differ as explained in Section 3. With social interactions that cause neighborhood tipping, neighborhoods that tipped during the immigration boom are expected to continue their transition towards an all-immigrant neighborhood. If tipping is not a pervasive phenomenon, we do not expect native populations to leave dense-immigrant neighborhoods.

Panel a) in Figure 3.8 plots changes in the immigrant share between 2010 and 2015 against the share of immigrants in the neighborhood in 2010. The slope of this graph is completely reversed with respect to the 2001-2009 period and clearly indicates that the minority share decreased more in neighborhoods with a high initial immigrant share. As discussed above, changes in immigrant density can be driven by both changes in native and immigrant populations.

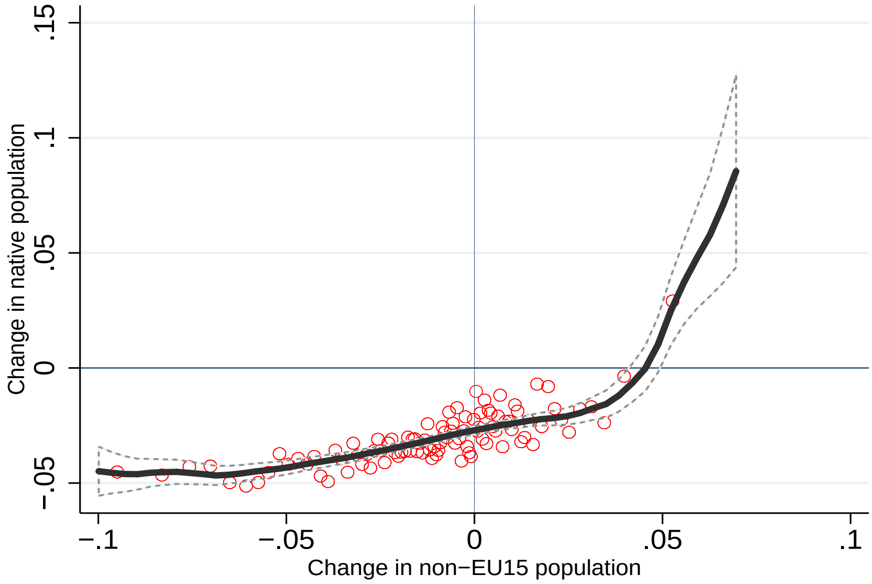
Panel b) shows the relative contribution of natives and immigrants to neighborhood population growth. Neighborhoods with a high minority share in 2010 lost more immigrant residents in the 2010-2015 period. This effect is mechanical and explained by the shift-share predictor described in equation 3.9. That is, neighborhoods with more immigrants in 2010 experienced larger immigrant population losses between 2010 and 2015, which mechanically causes the immigrant share to decrease more in neighborhoods with a high immigrant share in 2010. As for natives, there are also population losses in neighborhoods with a high immigrant density in 2010. Panel c) shows that these native population losses contributed to increase the immigrant share in these neighborhoods between 2010 and 2015. However, the outflows of immigrants dominate the outflows of natives resulting in a decreased immigrant share as can be seen in panel a).

Figure 3.8: Neighborhood population dynamics during the immigration freeze (2010-2015)



Notes: Data points are bin averages while solid lines are local linear regression fits on non-binned data using an Epanechnikov kernel. Dashed lines are 95% confidence bands.

Figure 3.9: Native and non-EU15 neighborhood population changes during the immigration freeze, 2010-2015



Notes: Population changes expressed relative to the population level in 2010. Data points are bin averages while solid lines are local linear regression fits on non-binned data using an Epanechnikov kernel. Dashed lines are 95% confidence bands.

In Figure 3.9 we plot changes in the number of native and non-EU15 population (relative to 2010 total population) for the immigration freeze period. While we found a negative slope in the immigration boom period, in the immigration freeze period this negative relationship is absent, and in fact, neighborhoods that lost more immigrant population also seem to experience somewhat larger losses in native population[12]. This asymmetry between the results found for the two subperiods is consistent with tipping behavior, and it is not consistent with a model without social interactions. In such a model, native population responses to immigrant inflows and outflows should be symmetric.

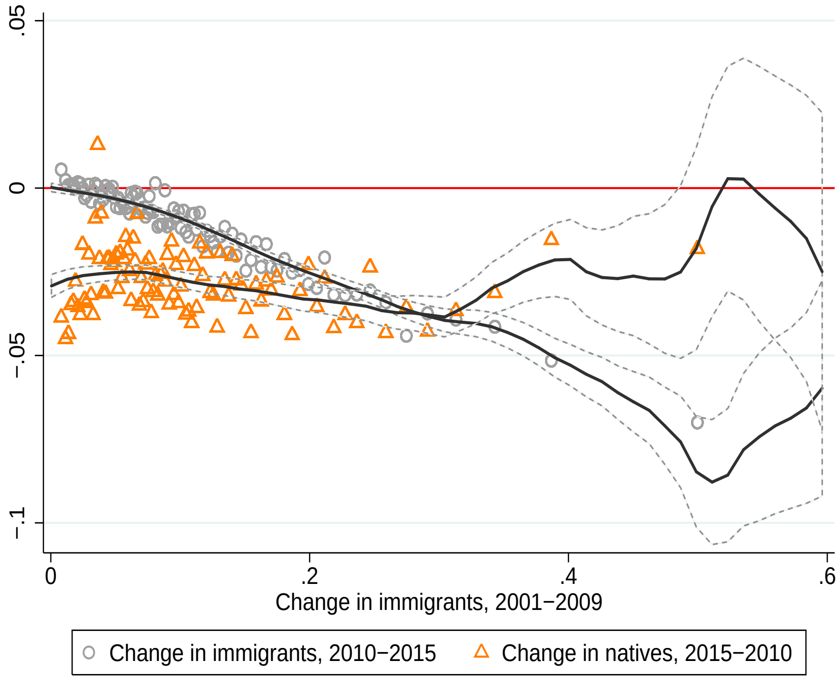
A related graph is presented in Figure 3.10, where we plot changes in non-EU15 population in the first subperiod (2009-2001) against changes in immigrant and native population in the second subperiod (2015-2010). All changes are measured relative to the population level in the base year. The graph shows that neighborhoods that received larger non-EU15 inflows in the first subperiod experienced larger losses in non-EU15 population in the second period. This correlation is completely driven by the shift-share behavior of the immigrant population at the neighborhood level. More interestingly, however, natives do not return to neighbor-

[12]This positive slope might be explained by new developments that attract both native and non-EU15 populations.

hoods that experienced large immigrant inflows in the 2001-2009 period, despite the fact that these neighborhoods lost immigrant population in current period. This is another piece of evidence that is more consistent with tipping than with income sorting dynamics.

Summarizing, we first provide evidence that during the immigration freeze period, the immigrant share decreased more in neighborhoods with higher immigrant shares in 2010. This dynamic behavior is driven by the shift-share evolution of immigrant population at the neighborhood level. As for natives, between 2010 and 2015, their population decreased more in neighborhoods where the immigrant share was high in 2010, and do not respond to contemporaneous outflows in immigrant population. Overall, we find that natives responses to immigrants inflows and outflows are asymmetric, a behavior that is consistent with neighborhood tipping. As suggested by Card et al. (2008), one distinct empirical prediction of tipping is that there should be a discontinuity in native population growth at the tipping point. Figure 3.11 reproduces the native contribution to population growth in the 2010-2015 period, that is, it zooms out the triangle figures in panel b) of Figure 3.8. The negative slope is consistent with tipping but note that the data does not show any obvious discontinuity.

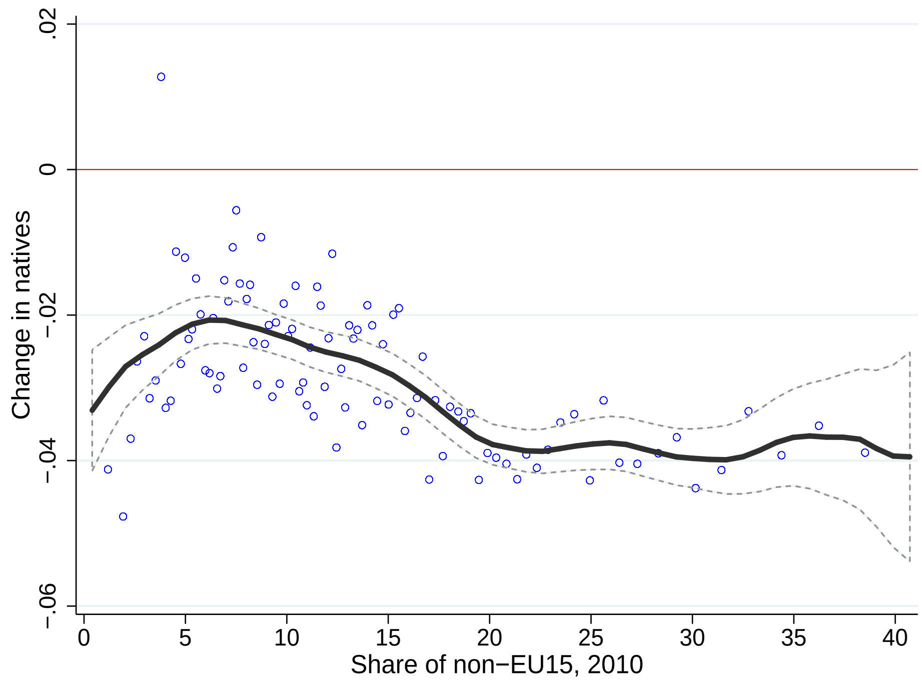
Figure 3.10: Population changes 2010-15 versus non-EU15 inflows 2001-2010



Notes: Population changes expressed relative to the population level in the base year. Data points are bin averages while solid lines are local linear regression fits on non-binned data using an Epanechnikov kernel. Dashed lines are 95% confidence bands.

Thus, while neighborhood population dynamics for the 2010-2015 period examined above support the existence of tipping, Figure 3.11 does not. One way to rationalize this seemingly conflicting evidence is that the tipping point is heterogeneous across neighborhoods and, thus, there is no exact immigrant share cut-off in 2010 which separates neighborhoods that are tipping from those that are not.

Figure 3.11: Neighborhood change in natives population during the immigration freeze (2010-2015)



Notes: Changes expressed relative to population in 2010. Data points are bin averages while solid lines are local linear regression fits on non-binned data using an Epanechnikov kernel. Dashed lines are 95% confidence bands.

3.6 Conclusion

In this paper we propose a new empirical approach to assess the empirical relevance of one-sided tipping models (Card et al., 2008). To that end, we exploit an unusual set of events that took place in Spain in the period 2001-2015. Spain experienced a massive immigrant inflow between the late nineties up until 2009. The recession stopped these immigrant inflows and the immigrant population slightly declined between 2010 and 2015.

The paper examines population changes at the census tract level in the largest six cities (Barcelona, Madrid, Málaga, Sevilla, Valencia and Zaragoza). In the immigrant boom period, 2001-2009, we find that new immigrants located in neighborhoods with an already high immigrant density in 2001. At the same time, these neighborhoods lost native population during these years. These facts, can be rationalized by two competing theories. The first one is tipping. Native residents leave or stop entering these neighborhoods because the high immigrant share is already beyond the tipping point. Alternatively, this pattern might simply be explained by the dynamics of income sorting. Since housing supply is rigid, the inflow of an immigrant mechanically reduces native population in the neighborhood, and immigrants did choose neighborhoods with a high immigrant share in 2001, which are neighborhoods with low quality housing stocks in which housing is more affordable.

By contrast, these two competing theories yield different predictions for the immigrant freeze period, 2010-2015. If immigrant dense neighborhoods had tipped by 2010, these neighborhoods should have lost native population between 2010-2015, despite the fact that cities were not receiving new immigrant inflows during the period. Instead, if the observed pattern in 2001-2010 was driven by income sorting, we do not expect immigrant dense neighborhoods in 2010 to keep loosing native population between 2010 and 2015. The results clearly support the predictions of the tipping model, whereby native population kept leaving or avoiding immigrant dense neighborhoods despite small population losses in these neighborhoods.

Neighborhood tipping implies that there is multiplicity of equilibria in the social composition of neighborhoods, implying that the decentralized equilibria need not be efficient and immigrants might be excessively segregated. In such an instance, income-mixing policies that try to reduce segregation might be desirable both on equity and efficiency grounds.

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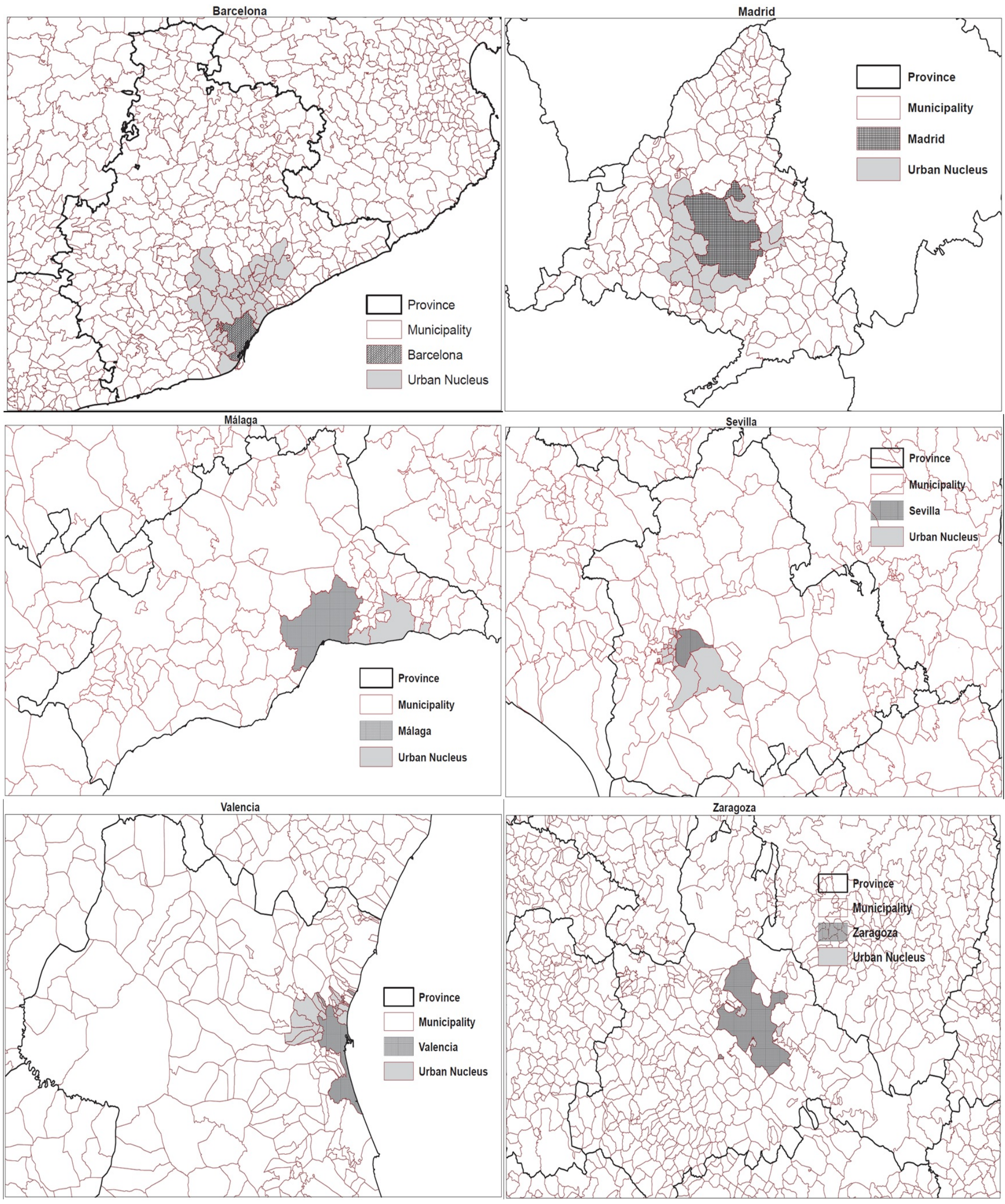
3.7 Appendix

3.7.1 Selected cities

Figure 3.12: Maps of municipalities considered in each city definition.

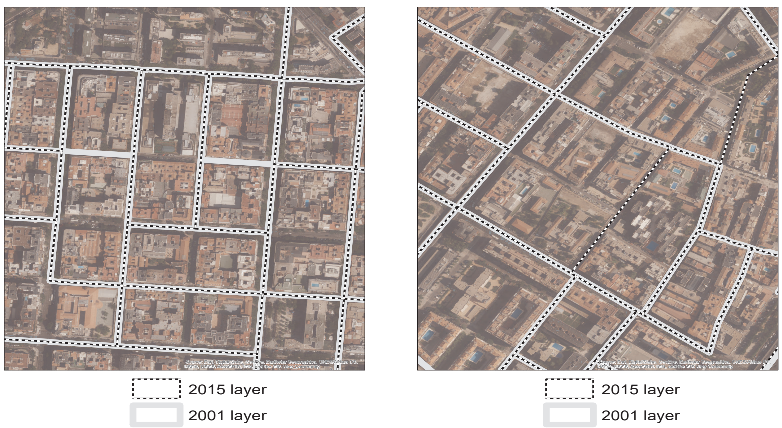


3.7.2 Sample appendix

To build a consolidated panel of tracts, we proceed with the following steps. First, we intersect each census tract layer from 2002 to 2015 with the 2001 layer, which is used as the base layer. Second, whenever a tract changed with respect to the base, we build weights as the portion of the tract surface that belongs to the reference layer divided by the tract's area. Then, the number of inhabitants in tracts that changed are reconstructed as weighted averages. This approach implicitly assumes population to be uniformly distributed across space. This assumption is actually not unrealistic in densely populated areas as it is the case in the city definition that we use. The following figure shows an example of census tract redefinitions. In the first case, two 2001 census tracts (solid line) were merged into a single tract in the 2015 layer (dashed line). To assign weights to these tracts, we divide the overlapped portion of the 2015 layer with the 2001 by the 2015 census tract area. The resulting weights are multiplied by the 2015 populations and then, summed up to get the 2015 population keeping the 2001 tract definition. The second case shows the opposite example. We observe a 2001 tract which is split into two tracts according to the 2015 layer. Weights equal one for those tracts. That is, we basically sum the 2015 population of both tracts to keep the 2001 census tract definition.

Figure 3.13: Example of census tracts redefinitions

Left panel: merged tracts; Right panel: split tracts



4 Spillover effects from a place-based housing subsidy

4.1 Introduction

In accordance with the concept of 'housing externalities', house prices depend not only on their own attributes but also on those of neighboring properties (Rossi-Hansberg et al., 2010). Thus, neighborhood amenities, such as the quality of the nearby housing stock, the number of green spaces in an area or even the quality of the neighboring streets, may influence both land and property values. Furthermore, externalities on property valuations may be reinforced by social interactions (see Koster and Rouwendal (2017) for a theoretical formulation).[1] This being the case, government subsidies to build new housing units should also be capitalized into the higher land and house prices of nearby, non-subsidized buildings through local spillovers. Interventions of this type are place-based policies that can potentially change residential patterns and developers' investment decisions across neighborhoods.[2] Therefore, if spillovers exist and are sufficiently large, the implementation of policies of this type would be justified. In other words, the subsidy could provide incentives to developers to internalize part of these spillovers in areas that lack housing or in which the housing stock has deteriorated.

In this paper, we estimate externalities derived from a subsidy paid to private investors to develop housing in Montevideo (Uruguay).[3] The total amount invested in the period 2011-2016, as a percentage of the average GDP for that same period, was 1.5%. In the same way as the Low-Income Housing Tax Credit (LIHTC) in the US (or the Scellier Tax Credit, STC, in France), these tax-benefits were introduced with the aim of increasing and improving the stock of housing. However, in the case of Uruguay there are no explicit rules regarding the socio-economic characteristics

[1] Ioannides (2003) provides evidence that social interactions matter when homeowners are determining their own property valuations while Patacchini and Venanzoni (2014) find that peer effects impact the demand for housing services.

[2] Such measures are welfare-enhancing in the presence of nonlinear externalities across space (Glaeser and Gottlieb, 2008; Neumark and Simpson, 2015)

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

that the buyers or tenants of the newly developed homes must present.[4] Without imposing any requirements as to the characteristics of the buyer or tenant, profit-driven developers focus their investments in those areas that offer the best prospects as regards future house prices. As such, this policy design could fuel urban renewal processes, and this factor needs to be capitalized into higher house prices. In this regard, local spillovers could partly explain why house prices in areas neighboring new construction investments increase more than do those in more distant locations.

From a theoretical perspective, we consider two channels via which external effects on nearby properties might emerge. First, the policy has a direct effect on nearby houses by improving the quality of the stock. This, in turn, leads to an increase in the overall amenity level in the neighborhood. Second, in the medium- and long-run, the policy is likely to result in a change in the neighborhood income composition, since subsidized housing units can only be afforded by high (middle-high) income households. As such, the policy should reinforce these external effects. The change in neighborhood income composition may affect crime patterns, as the new (higher-income) residents are less likely to engage in criminal activities and more likely to invest in security measures (i.e. alarms, cameras, etc., Farrell et al. (2011)).[5] Yet, it is more likely that this policy affects crime patterns by removing neighborhood disamenities, such as abandoned houses and disused factories, since most of this investment targeted such places.

Estimating spillovers is challenging because developers are likely to cherry-pick locations. One of the contributions of this paper is to overcome this challenge by taking advantage of the place-based nature of the subsidy for the construction of new housing, given that this subsidy applies to just one specific area of the city. However, there is a high concentration of projects close to the border that separates subsidized and unsubsidized areas, and while average prices are higher in the latter area, the fact that pre-treatment housing prices remain smooth at the border suggests there are no major cross-border differences in the level of amenities.

Using a difference-in-differences strategy, we exploit housing price changes in different narrowly defined bands before and after the policy. Specifically, we estimate externalities in two 400-meter wide bands at the border, one in the subsidized area and another in the unsubsidized area. The control group is constituted by a band in the unsubsidized area that lies 400 meters from the border and is, therefore,

[4] In contrast, housing units developed under the LIHTC in the US must be occupied by low-income households earning below a given threshold. Unlike in the US, here the only measure aimed at preventing sales prices rising too high is a price cap; however, this only applies to 25% of the total units developed under a specific project (and is not binding in most of the areas).

[5] Another strand of the literature argues, however, that the presence of more affluent residents in a neighborhood increases the number of potential targets in the area, thereby, increasing crime (Sampson et al., 1997)

unlikely to be affected by spillovers.[6] By implementing this strategy, we obtain an average external effect at the border. However, along the subsidized side of the border, developers have targeted their investments at certain specific locations as opposed to others. Therefore, this previous strategy leads to intention-to-treat estimates. We, then, construct a measure that captures the investment exposure of housing unit sold to subsidized projects, which we use as a continuous treatment variable. Since this investment intensity measure might be endogenous, we use an instrumental variable approach, exploiting once again the place-based structure of the subsidy.

The validity of this empirical strategy relies on the assumption that unobservable factors specific to locations do not vary across borders. In other words, estimates will be biased if the border was set in such a way that it includes places with better prospects or if factors known only to the policymakers were taken into consideration. We know this not to be the case for several reasons. First, the border of the subsidized area was set by the Ministry of Housing in conjunction with the Ministry of Economics and Finance and the Local Government of Montevideo. The border follows a number of natural city divisions provided by its main avenues and streets. It does not follow the borders of any other administrative division. Second, there are no physical barriers between the two sides of the border. Third, we provide evidence that housing prices do not vary discretely across borders, which means that developers have had a considerable impact on the definition of the boundaries of the subsidized area. Fourth, the fact that we use geographically detailed data allows us to focus on narrowly defined bands, all subject to the same shocks and all sharing common trends. Finally, we control for border fixed effects, housing characteristics, and census block characteristics, which should reduce bias due to unobservable factors.

We focus on 98 projects promoted and executed between 2011 and 2013 in Montevideo. These represent more than 60% of the total number of promoted projects (in that period) and the average tax exemption per housing unit was around 17 thousand USD (around 21% of the construction costs). We combine the projects' official data with house price data in USD for exact locations and with geocoded daily crime records, both for the period 2006-2016.

Results from the intention-to-treat analysis show that house prices increase by around 12% on the subsidized side of the border. These spillovers fall by around one half every 200 meters as we move towards the interior of the unsubsidized area. Moreover, there is no evidence of anticipation effects, which further validates the empirical strategy employed herein. Using the investment exposure measure as the

[6]As a robustness check, we estimate spillovers non-parametrically.

treatment variable, we obtain an elasticity of 0.035 with respect to house prices. Applying this approach, the spillover effects result in a 20% increase in house prices on the subsidized side of the border (6.3% annually). Similarly, it also leads to a 12% increase (3.7% annually) in house prices in the unsubsidized border zone. Results are heterogeneous with respect to certain neighborhood characteristics. In a census block with a high share of renters, we find an elasticity of .048 with respect to house prices. This might be attributed to the fact that in such neighborhoods, population inflows and outflows are likely to be more dynamic than in places with a high proportion of owner-occupied dwellings. As neighborhoods are renovated, they can be expected to receive an influx of more affluent residents leading potentially to a gentrification process or to densification with a higher income population. As for crime, when aggregating data at the street segment level, we find that property crime decreases by 60% (per 100 meters) in the subsidized border zone, while there is no effect on the non-property crime rate. To avoid contamination of these estimates by crime displacement effects, we aggregate crime data at the census block level. In this case, IV estimates point to a 15% reduction in property crime on the subsidized side of the border. Again, there is no effect on the non-property crime rate.

Most of the literature examining the spillovers from subsidized housing is based on the study of the (LIHTC) in the US (Baum-Snow and Marion, 2009; Freedman and Owens, 2011; Diamond and McQuade, 2018).[7] These papers focus on the effects of affordable housing on the housing market and other neighborhood outcomes. Baum-Snow and Marion (2009) take advantage of a discontinuity in the rule to award a census tract with a larger number of subsidized housing units. Then, using a regression discontinuity (RD) design, the authors find that subsidizing the housing supply increases local housing prices, reflecting either better neighbors or the conversion into new housing units of vacant buildings or unsightly empty lots. As the authors state, this identification strategy provides highly reliable causal estimates of LIHTC on low-income neighborhoods, but that these can hardly be extended to richer areas of the city. In the case we present, the border used to estimate spillovers is 12 km long, and includes neighborhoods of different socio-economic backgrounds. Freedman and Owens (2011) were the first to analyze the effects of LIHTC on crime at the county level, finding a significant reduction in violent crime

[7] Eriksen (2009) studies how the LIHTC affects developers' construction decisions in California while Eriksen and Rosenthal (2010) estimate the crowd-out effects of unsubsidized rental housing units under this policy. For developing countries, two studies examine the direct effects of urban policies (paving streets and land titling) on property prices. González-Navarro and Quintana-Domeque (2016) show that property prices in newly-paved streets increased by 17 percent according to appraisers, and by 28 percent when using homeowner valuations. Likewise, Galiani and Schargrodsky (2010) find that land rights in unregulated settlements lead to an increase in property values. However, neither of these studies tests the local spillover hypothesis.

but no detectable effects on property crime. In this paper, we use two lower levels of data aggregation: 1) street segment (80-meter length) and 2) census block (which includes around 1,100 inhabitants). More recently, Diamond and McQuade (2018) estimate housing spillovers of properties financed by the (LIHTC) on both house prices and crime, using highly disaggregated geographical units. Their identifying assumption relies on the fact that developers cannot choose exact locations within neighborhoods, and also there is uncertainty regarding when the project will receive funding.

In terms of its methodology, our paper is similar to Turner et al. (2014) and, to a lesser extent, to Boustan (2011). Turner et al. (2014) 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 border is that local policies are set by the same local government and, so, substantial differences are less likely to be found in local public services and other unobservables. For example, Boustan (2012) use city-suburban school district borders to estimate the effect of a school desegregation policy introduced during the 1970s in US on house prices.

Finally, this paper can be linked to Schwartz et al. (2006), who studies spillovers from place-based subsidized housing in New York, but using a ring regression approach. It also has obvious points in common with studies that estimate externalities but which derive more specifically from urban renewal interventions (Rossi-Hansberg et al., 2010; Ahlfeldt et al., 2016; Koster and Van Ommeren, 2017), and the lifting of rent control (Autor et al., 2016). These studies of the effects of urban renewal interventions find mild to moderate evidence of housing externalities, that is, spillovers of the intervention on the house values of nearby properties outside the targeted areas. Nevertheless, these externalities, when present, are extremely local and decay rapidly.[8] These findings suggest that externalities, though present, do not appear to be quantitatively very important, at least in developed countries. Finally, the positive effects on house prices and housing externalities increase with exposure to, and with the size of, the renewal intervention Autor et al. (2016).

Previous studies have also focused on spillover effects from affordable housing or public housing. The latter housing policies target low (middle-low) income households. Furthermore, investments are concentrated mainly in low-income areas, which received a disproportionately high number of affordable housing units (Baum-Snow and Marion, 2009). Whenever affordable housing is built in middle/middle-

[8] By one half every 1,000 feet in Rossi-Hansberg et al. (2010), and completely after 1.5 km in Koster and Van Ommeren (2017). Ahlfeldt et al. (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.

high income neighborhoods, spillovers have tended to be negative (Diamond and McQuade, 2018). However, this is not the case in Uruguay. We study a housing policy that produces housing units affordable to middle/middle-high income households and which targets middle-income neighborhoods.[9] Less is known about the spillover effects of housing policies of this type. The question we seek to address, therefore, is whether the fact of targeting the subsidy to housing for middle (middle-high) income households leads to higher spillovers which, in turn, helps overcome the coordination problem. Moreover, the existing literature has focused primarily on local spillover effects on house prices and has tended to pay less attention to effects on local crime rates. Finally, the identification strategy used in this paper enables us to avoid the fact that developers are likely to want to choose more profitable locations, which has been one of the main challenges when estimating housing externalities.

The paper is organized as follows. In section 2, we describe the housing subsidy. In section 3, we develop a simple theoretical framework. In sections 4 and 5, we explain how to measure spillovers and we outline the empirical method used. In section 6, we describe the data. In section 7 the results are presented and, finally, we conclude in section 8.

4.2 A subsidy for private housing investors

In August 2011, the Uruguayan government introduced tax benefits for private investments in housing (Law nbr. 18,975).[10] The policy aims at incentivizing the construction sector and improving the stock of housing both for sale and for rent throughout the country. Tax benefits apply to new builds and rehabilitation projects; however, 73% of all promoted projects are new builds.[11] Indeed, the budgets for new construction projects were on average 11 times greater than those for rehabilitation. For this reason, here, we focus on new construction projects subsidized under this law.

Projects are required to produce at least two and up to a maximum of 100 new residential housing units per land lot. However, this upper limit does not apply in the case of projects undertaken on large vacant lots or on parcels containing dis-

[9] The average price (in m^2) of a subsidized housing unit is 2,446 USD in 2014 while the average housing price (in m^2) in Montevideo is 1,401 USD in 2014. Since concerns were expressed about the prices of subsidized housing, the government made changes to the policy in 2016 to improve access to middle-low income households.

[10] Projects are permitted to include a small number of commercial units (up to 10%) and lofts (up to 20%), but no tax benefits are given for these units.

[11] Rehabilitation projects involve upgrading and, also, increasing the total number of housing units in deteriorated properties (usually, semi-detached houses).

used factories or abandoned houses. The policy incentivizes the construction of semi-detached houses or flats and, so, detached houses are excluded. There is also a size restriction for each housing unit, determined by the number of bedrooms.[12] In terms of quality, the new units have to adhere to the guidelines laid down in the National Housing Plan (Law nbr. 13,728) and other ministerial regulations, which, among other characteristics, establish requirements regarding the materials to be used. Tax benefits only applied to projects completed in urban areas, with the exception of those with a high proportion of second homes.[13] The main fiscal advantage for developers and private investors is that projects promoted under this law are exempt from paying any corporate tax (25%) on the sold units, while rents are partially exempt from personal income and corporate taxes (for a period of nine years). Other fiscal benefits include exemptions from the wealth tax on land and improvements during construction, as well as, on built and subsequently rented units for a nine-year period.[14] Finally, the law establishes tax credits for value-added tax on national and imported inputs, potentially reducing liquidity constraints and/or opportunity costs for developers. Applications can be submitted at any time of the year. First, they are 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, the two institutions responsible for implementing the policy. It should be stressed that although the law came into force in August 2011, many of the rules and regulations governing its implementation were not finalized until October 2011, leaving developers with little opportunity to anticipate and cherry-pick locations. Additionally, this might, in part, explain why the first subsidized projects were approved by the end of 2011.

4.2.1 Policy numbers

According to the ANV, 345 new construction projects were undertaken between December 2011 and November 2016, involving 13,142 new housing units. The total amount invested for these subsidized projects was 742M USD, equivalent to around

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

[13] The Cadaster Agency of Uruguay (*Dirección Nacional de Catastro*) is responsible for classifying parcels as rural or urban depending on their use (urban or agricultural production). In addition, urban parcels have access to nearby public services and urban infrastructure. According to this definition, an urban area consists of a group of adjoining urban lots taking into account commuting patterns. The metropolitan area of Montevideo is the main urban area of the country with 1.9M inhabitants (around two thirds of the country's total population).

[14] In Uruguay, firms pay a wealth tax, which is rated at 1.5% of their net assets. Also, private investors are exempt from paying the transfer tax (2% of the property appraisal value) when buying unsold units.

1.5% of the 2011-2016 average GDP in USD. Despite being a nationwide policy, more than 60% of the projects were concentrated in Montevideo (the capital), attributable to the fact that around one half of the country's population reside in the capital and two thirds in the metropolitan area of Montevideo.[15] The empirical analysis focuses on 98 subsidized, new construction projects in Montevideo promoted and executed between 2011 and 2013.[16] These projects represent around 15% of the total of new construction permits issued between 2012-2013 in Montevideo and 40-50% of the total square meters developed in those years. Moreover, the tax exemptions per housing unit amount to around 17 thousand USD, on average, which represent around 21% of their construction costs.[17]

4.2.2 Place-based scheme for new construction

In Montevideo, the subsidy has a place-based structure for new constructions. As observed in Figure 4.1, only in area *S* do the tax benefits apply to new construction projects.[18]

Area *S* represents 52% of the total urbanized area of Montevideo, followed by the suburbs (29%) and area *U* (19%). It has a population density of 7,740 inhabitants per km^2 and is composed of both central and peripheral neighborhoods, constituting a highly heterogeneous area. This heterogeneity is also observed in income. Drawing on the 2011 National Household Survey (*Encuesta Continua de Hogares*, ECH), the coefficient of variation of real per capita household income is almost 30% in area *S* while it is around 25% for both area *U* and the suburbs. 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 real per capita income that doubles and triples that in area *S* and the suburbs, respectively. In contrast, the suburbs concentrate the highest share of slums and the lowest average income. A similar pattern is observed for house prices. Average housing prices per m^2 in area *U* more than double those in area *S* and are seven times higher than those in the suburbs.

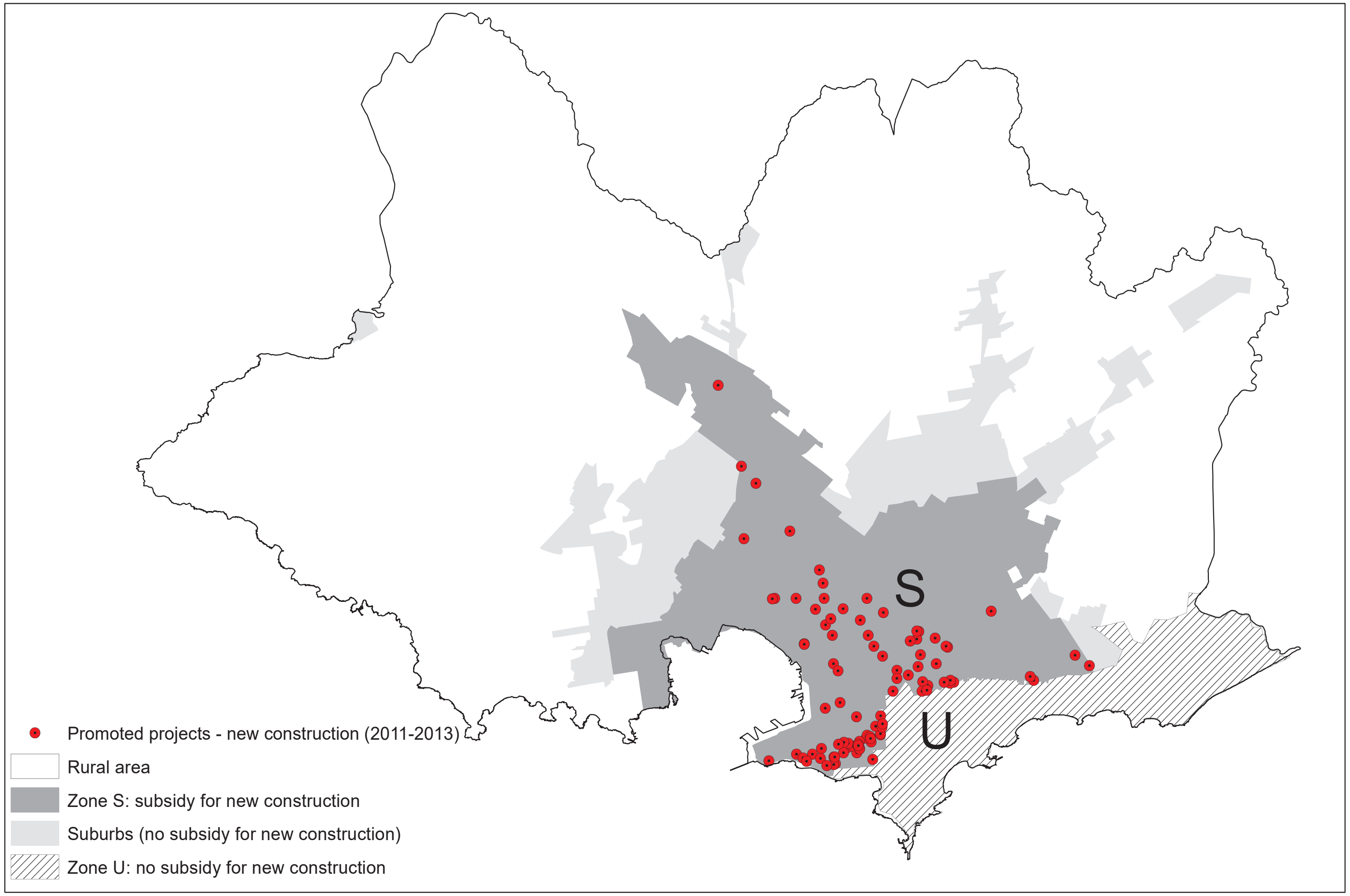
[15] According to the 2011 census, Uruguay has 3.3M inhabitants. The Department of Montevideo has 1.3M inhabitants while the metropolitan area of Montevideo has 1.9M inhabitants.

[16] The average length of time to complete a new construction project is 1.8 years.

[17] 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.

[18] Excluding rural areas, tax benefits for rehabilitation projects apply throughout the city. No rehabilitation projects were undertaken in the suburbs, while only five low-budget projects were carried out in area *U*.

Figure 4.1: Geographical distribution of new construction projects (2011-2013) in Montevideo (Uruguay)



As for their respective crime statistics, area U has the highest property crime rate but the lowest violent crime rate. Specifically, in 2011, the property crime rate per 100,000 inhabitants was 6,325, 6,067 and 3,962 for areas S, U, and the suburbs, respectively. The non-property crime rate per 100,000 inhabitants was 307, 138 and 368 for areas S, U, and the suburbs, respectively. Furthermore, the suburbs have the highest homicide rate, 20 per 100,000 inhabitants (equal to the average for Latin America and Caribbean countries); 5 in area U and 15 in area S.

The boundaries of area S were defined by the Ministry of Housing together with the Ministry of Economics and Finance and the Local Government of Montevideo. Overall, they adhere to a number of natural city divisions provided by its main avenues and streets. (Figure 4.8 in Appendix 4.8.1) and they do not follow the borders of any other administrative division.[19] One example is the upper border of area U with respect to area S which follows one of the largest avenues in Montevideo, that is, the main city access from the east. Moreover, it was clearly intended to include the city's central area in the 'new construction' area to meet the government's goals of revitalizing this area.[20]

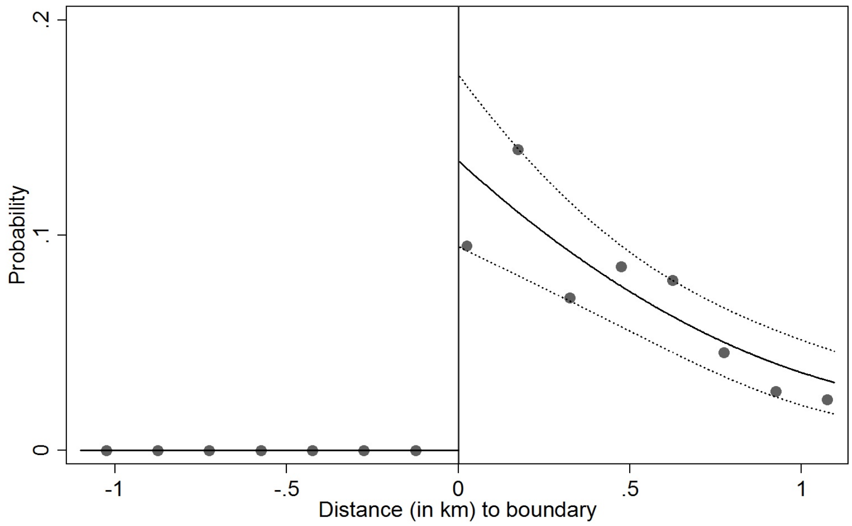
Figure 4.1 also shows the geographical distribution of new construction projects promoted and executed between 2011 and 2013 in Montevideo, and which are subject of our analysis. In particular, there is a high concentration of projects near the border of area S with area U. Figure 4.2 provides further evidence of this earlier observation, showing that the probability of a new construction project being promoted increases sharply in area S with increasing proximity to the border with area U. Table 4.1 shows an almost 13% statistically significant increase in the probability of a new construction project being promoted at the border. This result holds when using kernel-weighted local polynomial regressions with different polynomial orders (and following Calonico et al. (2014b,a) robust inference and bandwidth selection procedure), as well as, for linear regressions close to the border. In other words, the evidence points to large investments in subsidized, new construction projects in area S close to the border with area U.

[19] 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.

[20] The southeasterly part of area S is composed of the main central neighborhoods including the downtown area which extends to the coast.

Figure 4.2: Subsidized projects and house prices around the border (area U vs area S)

(a) Prob. of promoting a subsidized new construction project (2011-2013)



(b) House prices, pre-intervention period (1998-2010)

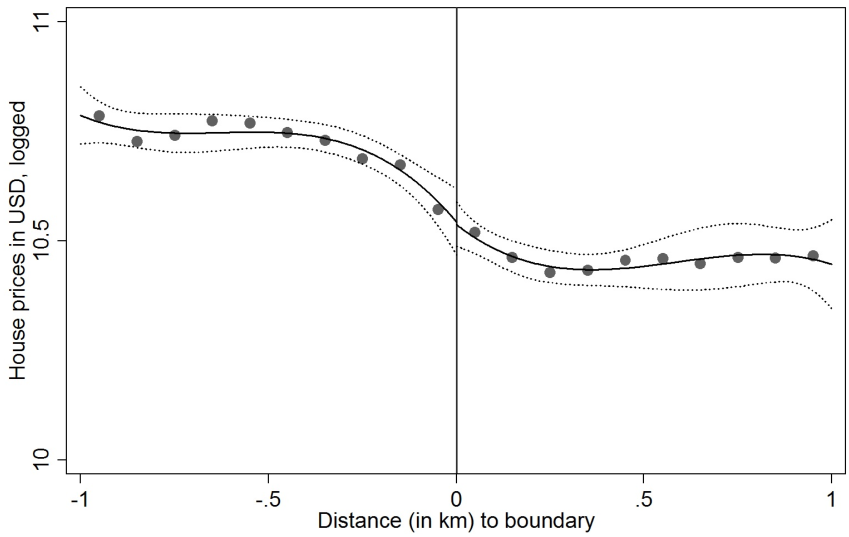


Table 4.1: Regression discontinuity estimates in the border (area S vs area U)

	Kernel-weighted local polynomial regressions				Regression close to the boundary	
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
(a) Probability of promoting a project (2011-2013)						
$\mathbb{1}\{\text{distance to border}>0\}$	0.129	0.131	0.133	0.134	0.102	0.122
	(0.019)***	(0.019)***	(0.020)***	(0.021)***	(0.027)***	(0.024)***
Observations	6,684	6,684	6,684	6,684	492	808
Number of clusters	564	653	722	799	106	157
Pol. order	1	2	3	4	1	1
Bandwidth (in km)	1.996	3.686	5.434	7.506	.250	.500
Kernel	triangular				uniform	
(b) House prices, pre-intervention period (2006-2010)						
$\mathbb{1}\{\text{distance to border}>0\}$	-0.020	-0.002	-0.025	-0.025	-0.024	-0.056
	(0.041)	(0.045)	(0.054)	(0.057)	(0.039)	(0.036)
Observations	75,514	75,514	75,514	75,514	8,331	14,016
Number of clusters	216	297	260	317	97	145
Bandwidth (in km)	.459	.844	.822	1.128	.250	.500
Pol. order	1	2	3	4	1	1
Kernel	triangular				uniform	

Notes: The variable distance is normalized to be zero at the border; positive values correspond to area S (the subsidized area) while negatives to area U (the unsubsidized area). $\mathbb{1}\{\text{distance to border}>0\}$ represents a dummy variable that takes the value of one for observation in area S. Columns (i) to (iv) are estimated using the roubst bias-corrected inference and bandwidth selection criteria developed in Calonico et al. (2014b,a). In column (v) and (vi) a linear regression model using observation close to the border is estimated. In panel (b), year-month fixed effects, border-year fixed effect and housing characteristics included as controls. Clustered standard errors at the neighborhood level. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

House prices near the border

The bottom panel in Figure 4.2 shows housing prices in the pre-policy period (2006-2010) as a function of the distance to the border of area S with area U. The distance is normalized to be zero at the border. Positive distance values are defined for area S; negative values are defined for area U. Each evenly spaced bin represents the average house price while the fitted line is estimated using a third-order polynomial, controlling for housing and census block characteristics. Dashed lines are (95%) confidence intervals. It can be seen that house prices are higher, on average, in area U (the richest part of the city) than they are in area S. While there is a substantial increase in the probability of promoting a subsidized, new construction project at the border (as we observe in panel a), the house price gradient remains smooth (panel b). House prices decrease as we move from area U to S without any discontinuities being observed.

In Table 4.1, we formally test for a discontinuity in the house price vector at the border. Using both kernel-weighted local polynomial (of different polynomial orders) and linear regressions close to the boundary, results show that there is no discontinuity in the house price gradient at the border of area S with area U.[21] This result also suggests that developers did not succeed in influencing the definition of the borders so as to include places with better prospects in area S. The result also indicates that the level of public services and amenities does not differ substantially across borders. Within-city border analysis has the potential advantage (with respect to cross-municipality border analysis) that public services and other amenities are not expected to vary considerably because they depend on the same local government. Finally, the lack of discontinuity at the border means we can reject the hypothesis that local taxes presenting geographical variation are potentially capitalized into house prices.[22]

How developer choose locations?

To analyze which factors predict the location of subsidized, new construction projects in area S, we construct a yearly panel (period 2011-2013) at the block level.[23] Table 4.2 reports the marginal effects of a probit model where the dependent variable takes a value of one if a subsidized, new construction project was promoted in block i. In the first column, % of low-educated heads of household are not included because they are likely to be highly correlated with house prices, while in the second column this covariate is added. In the last column, neighborhood dummies are included. The results in Table 4.2 indicate that the probability of promoting a subsidized project at t increases by 2% if block i belongs to a census block in which at least one subsidized high-investment project was developed at $t-1$. Other block characteristics, such as the street quality index, the share of buildings with more than six floors, and the fact of there being a slum within 300 meters, also help to predict the location of subsidized projects by developers. As for census block level variables, education and changes in house price between 2006 and 2010 also seem to be good predictors of the probability of promoting a subsidized new construction project. The fact that house prices matter for the location of subsidized projects (even within neighborhoods) also contributes to explaining the developers' behavior of seeking locations close to the border.

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

[22] In appendix 4.8.2, the property cadaster values, which are used as the tax base for the property tax in Uruguay, are plotted and, once again, no discontinuity is observed at the border.

[23] A block has an area of 80 by 80 meters.

Table 4.2: Determinants of the probability of promoting a subsidized new construction project (2011-2013). Probit model

	(i)	(ii)	(iii)
Control at census block level			
High-investment project promoted in $t-1$	0.021***	0.019***	0.032***
	(0.003)	(0.003)	(0.005)
Density (inhabitants per km^2, logged)	0.003*	0.002	0.001
	(0.002)	(0.002)	(0.003)
% of vacant & uninhabitable dwellings	-0.000	-0.000	0.000
	(0.000)	(0.000)	(0.001)
% of rented dwellings	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
% of historic heritage sites	0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)
Δ house prices$_{2010-2006}$ (in USD, logged)	0.004**	0.003*	0.007**
	(0.002)	(0.002)	(0.003)
% of head of household with low education level		-0.001***	-0.000
		(0.000)	(0.000)
Control at block level			
Street quality index [0,1]	0.012**	0.006	0.015
	(0.006)	(0.006)	(0.011)
Slum within 300 meters of the block	-0.007***	-0.004*	-0.005
	(0.002)	(0.002)	(0.004)
% of buildings with more than six floors	0.002***	0.001***	0.003***
	(0.000)	(0.000)	(0.001)
High height restrictions	-0.009	-0.008	-0.005
	(0.006)	(0.006)	(0.010)
Observations	14,518	14,518	8,300
Number of clusters	603	603	381
Neighborhood fixed effects	N	N	Y
Log Likelihood	-506.09	-498.64	-459.55

$P(\text{Promoting}_i = 1|X)$ - Dependent variable: 1 if a subsidized project was promoted between 2011 and 2013 at block i and 0 otherwise. Yearly panel (2011-2013) at the block level. Year dummies included as control. Marginal effects reported. Clustered standard errors at the census block level. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

4.3 Theoretical framework

To guide our empirical work, we develop a simple residential choice model which is based on Glaeser (2008), Banzhaf and Walsh (2013) and González-Pampillón et al. (2016). This framework explains how changing the quality of the housing stock by means of a subsidized housing policy that only affects a specific area of the city impacts housing prices in that area. According to this model, on introducing this policy, housing prices change as a result of two effects: 1) a direct effect attributable to the increasing the level of neighborhood amenities and; 2) an indirect effect that reflects the potential change in the neighborhood income composition. Since the newly subsidized housing units can only be afforded by high (middle-high) income households, this policy could lead more affluent households to move into the middle-income subsidized area. This effect could reinforce the external (direct) effects produced by better amenities.

In this framework, we have one city with two communities (1 and 2), and the stock of housing is denoted by S_1 and S_2, respectively. The size of the city is normalized to be one and then, $S_1 + S_2 = 1$. Under this assumption S_1 and S_2 represent the proportion of the housing stock in each community (or the size of the community in relative terms). We further assume that community 1 is larger than community 2 ($S_1 > S_2$). The quality of the housing stock is good (labeled as A) or bad (labeled as B; hence, $S_1 = S_1^A + S_1^B$ and $S_2 = S_2^A + S_2^B$. The quality of the housing stock is higher in community 2 than it is in community 1 ($S_2^A > S_1^A$).[24]

There are two groups in the city: high income households (denoted by H) and middle-income households (denoted by M).[25] P is the proportion of high income households in the city. P_1 and P_2 denotes the proportion of high income households in community 1 and 2, respectively.[26] we assume that the proportion of high income households is slightly larger than the stock of housing in community 2 and that $H < M$. Thus, implying that $S_2 \leq P < .5$. We restrict to equilibria such that: $P_2 > P_1$.

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 amenities and it is related to the fact that each group prefers to live with people of their own type. G^H represents exogenous amenities which depend on the quality of the housing stock in community 1 then, $G^H(\gamma_H, S_1^A, S_1^B)$. a_h is an individual specific term which 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, utility in community 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. We assume that $\gamma^H > \gamma^M$, which implies that H has a more developed taste for housing quality and also that they are more likely to be able to afford housing units of higher quality.[27] Finally, we assume that $Y^H > Y^M$.

[24]Within each community, we have that $S_1^A < S_1^B$ and $S_2^A > S_2^B$.

[25]Low-income households reside in the suburbs and are, therefore, excluded from the analysis. The model could also be extended to include three income groups or an income distribution could even be used. However, for the sake of simplicity we work with two income groups.

[26]Then, the proportion of high income households in the city is: $P = S_1P_1 + S_2P_2$

[27]In other words, higher income individuals have a greater willingness to pay for amenities (Brueckner and Rosenthal, 2009; Kuminoff et al., 2013; Kahn and Walsh, 2015)

4.3.1 Bid functions and model solution

By equalizing the utility functions of H and M across communities, we obtain the bid functions:

$$Q^H(a_{h^*}) = \alpha^H(P_1) - \alpha^H(P_2) + G^H + a_{h^*} \tag{4.1}$$

and

$$Q^M(a_{m^*}) = \alpha^M(P_2) - \alpha^M(P_1) + G^M + a_{m^*} \tag{4.2}$$

To solve the model, we equalize bid functions[28] and we also use the fact that, in equilibrium, the supply of housing units equals that of demand in both communities.[29] We obtain the solution for the share of high income households that resides in community 1 (P_1) and housing prices (Q):

$$P_1 = P + \frac{P(1-P)}{S_1}\left[\alpha^M(P_1) - \alpha^M(P_2) + \alpha^H(P_1) - \alpha^H(P_2) + G^H - G^M\right] \tag{4.3}$$

and

$$\begin{aligned} Q &= 1 - S_1 + PG^H + (1-P)G^M + P[\alpha^H(P_1) - \alpha^H(P_2)] \\ &+ (1-P)[\alpha^M(P_2) - \alpha^M(P_1)] \end{aligned} \tag{4.4}$$

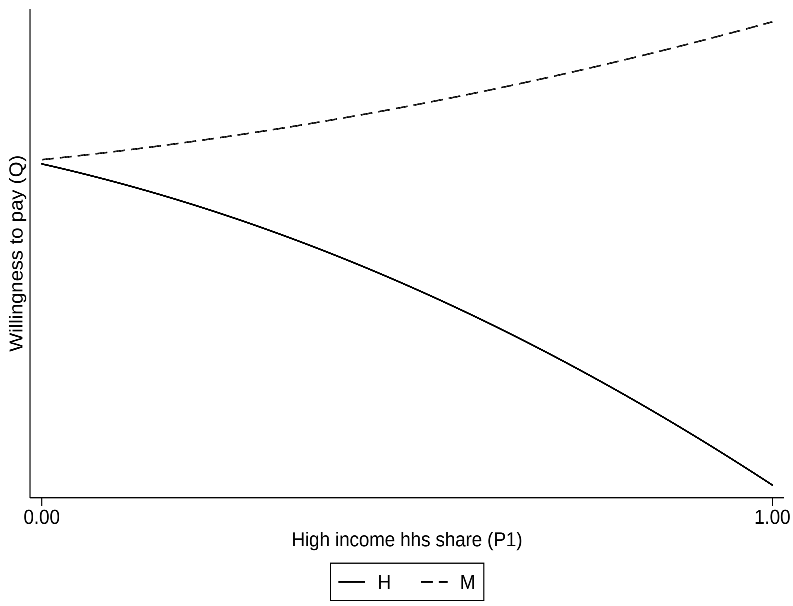
Figure 4.3 summarizes the model that plots bid functions of H and M as a function of the share of H in community 1 (P_1). The solid line represents the bid function of high income households (H), while the dash line is the bid function of middle income households (M). The model includes social interaction effects, i.e. that both groups have some preference to live with their peers. However, since these groups are not strongly polarized this effect does not play a major role and so only slightly affects the shape of bid functions. The top panel shows that there is no household belonging to H living in community 1, which we assume as having a lower quality housing stock. In order words, there is no marginal agent in the high income group willing to choose community 1.

[28]This leads to the following equation: $a_{h^*} = \alpha^M(P_2) - \alpha^M(P_1) + G^M + \alpha^H(P_1) - \alpha^H(P_2) + G^H + a_{m^*}$

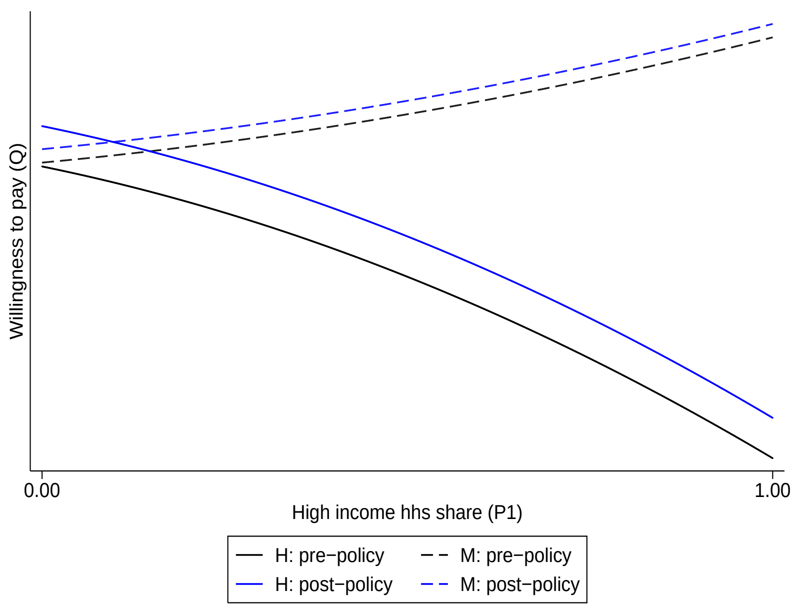
[29]For community 1, this implies that: $S_1 = (1-a_{m^*})(1-P) + (1-a_{h^*})P$. We also know that $(1-P_1)S_1 = (1-a_{m^*})(1-P)$. Using these three equations, we obtain a solution of P_1 that is a function of P_2. Since $P_2 = (P - S_1P1)/S_2$ after solving for P_1, we obtain P_2.

Figure 4.3: Model illustration

(a) Model equilibrium: share of high-income households in community 1



(b) The effect of increasing the quality of the housing stock in community 1



Notes: We assumed that $\alpha^M < \alpha^H$. Panel 4.3(a) shows the model's equilibrium. Under this setting the share of high-income households (P_1) in community 1 is zero. Panel 4.3(b) shows the effect of increasing the quality of the housing stock in community 1 on P_1. The policy leads to an increase of more affluent residents to community 1.

4.3.2 Increasing the quality of the stock

The next step is to analyze the effect of improving the quality of the housing stock in community 1. The government introduced a subsidized housing policy which only applies in community 1 and, as a result, the share of high quality housing units increases ($S_1^A \uparrow$). Therefore, this policy affects P_1 and Q since both depend on S_1^A. We first compute the derivative of P_1 with respect to S_1^A:

$$\frac{dP_1}{dS_1^A} = \frac{\frac{P(1-P)}{S_1}(\gamma^H - \gamma^M) - \frac{P(1-P)}{S_1^2}\left[\alpha^M(P_1) - \alpha^M(P_2) + \alpha^H(P_1) - \alpha^H(P_2) + G^H - G^M\right]}{1 - \frac{P(1-P)}{S_1}\left[\alpha^{M'}(P_1) + \alpha^{M'}(P_2)\frac{S_1}{S_2} + \alpha^{H'}(P_1) + \alpha^{H'}(P_2)\frac{S_1}{S_2}\right]} \tag{4.5}$$

We observe that increasing the quality of the housing stock increases the share of more affluent neighbors in community 1 only if the direct effect of improving amenities is greater for group H ($\gamma^H > \gamma^M$).[30] This latter fact holds by assumption and, therefore, $\frac{dP_1}{dS_1^A} > 0$. Hence, after the policy is introduced, the share of high income individuals increases. Second, in a mixed (and stable) equilibrium, the effects of increasing S_1^A on house prices is given by:

$$\begin{aligned}\frac{dQ}{dS_1^A} &= -1 + \underbrace{P\gamma^H + (1-P)\gamma^M}_{\textbf{Direct effect}} \\ &+ \underbrace{\frac{dP_1}{dS_1^A}\left[P\left(\alpha^{H'}(P_1) + \alpha^{H'}(P_2)\frac{P_1}{S_2}\right) - (1-P)\left(\alpha^{M'}(P_1) + \alpha^{M'}(P_2)\frac{P_1}{S_2}\right)\right]}_{\textbf{Indirect effect}}\end{aligned} \tag{4.6}$$

Spillovers can emerge via two channels. First, there is a direct effect of increasing the quality of the housing stock in community 1, which is always positive. Second, there is an indirect effect given by the fact that the neighborhood income composition changes since P_1 increases as S_1^A increases. This indirect effect could be either

[30] From equation 4.3, we can see that P_1 is a function that maps into itself: $P_1 = H(P_1)$. An equilibrium is stable if the following condition holds: $H' < 1$; since we only focus on stable equilibria this latter condition holds and then, the denominator is positive. Therefore, the derivative 4.5 is positive if the following condition holds: $\frac{P(1-P)}{S_1}(\gamma^H - \gamma^M) > \frac{P(1-P)}{S_1^2}\left[\alpha^M(P_1) - \alpha^M(P_2) + \alpha^H(P_1) - \alpha^H(P_2) + G^H - G^M\right]$. Assuming a quadratic functional form for social interactions (i.e. $\alpha(P) = \alpha P^2$) and defining $G^i = \gamma^i(F + S_1^A)$ with $F < 0$ (worse amenities in community 1), the latter expression is reduced to the following: $(\gamma^H - \gamma^M)(S_1^B - F) > (\alpha^M + \alpha^H)(P_1^2 - P_2^2)$. Since we assumed that $P_2 > P_1 > 0$ then, $0 > (\alpha^M + \alpha^H)(P_1^2 - P_2^2)$. Therefore, the condition holds if $\gamma^H > \gamma^M$.

positive or negative and depend on the value of the α parameter. For instance, assuming a quadratic functional form for social interactions, the indirect effect is positive if $\alpha^H > \bar{\alpha} = [\alpha^M(1-P)]/P$, meaning that the parameter governing social interactions of group H has to be above a critical value.[31] The overall effect, which is the sum of the direct and indirect effects, is observed in Figure 4.3. In the empirical analysis, we estimate the sum of both effects on house prices and we attempt to shed light on the sign of the indirect effect.

4.4 Empirical method

The empirical analysis focuses on new construction projects executed between 2011 and 2013 in the capital city (Montevideo)[32], where the subsidy forms part of a place-based scheme. Specifically, we exploit the fact that the policy introduced subsidies for newly built housing solely in one specific area of the city (area S in Figure 4.3) and that the border with respect to the unsubsidized area (U) was largely unmanipulated by developers (as discussed above in section 2). We further take advantage of the high concentration of projects implemented close to the border (around one half of the total). While average prices are higher in the unsubsidized areas, the fact that pre-treatment housing prices remain smooth at the border indicates that there are no major cross-border differences in their respective levels of amenities. Two strategies are used to estimate spillovers. The first is a standard difference-in-differences estimator that exploits house price changes in different narrowly defined bands before and after policy implementation. Since developers were able to choose where they wanted to invest among certain locations along the subsidized border, we consider this first approach as an intention-to-treat analysis. The second strategy uses an investment exposure measure that accounts for developers' investment locations. Using this measure, we apply a continuous-treatment difference-in-difference approach.

4.4.1 Intention-to-treat analysis

This strategy compares changes in house prices either side of the border (labeled as the "border band") with changes in house prices within the unsubsidized area U (labeled as the "control band") before and after subsidization to quantify these externalities. The border is almost 12 kilometers long, and included neighborhoods with different socio-economic characteristics. To control for heterogeneity along

[31] For low values of P (the share of H in the city) the condition over α^H is more stringent in order for the indirect effect to be positive.

[32] A total of 98 new construction projects.

the border, we include socio-economic characteristics from the 2011 census at the census block level. Furthermore, the boundary is split into six border-segments b (each around two kilometers long) based on census tract divisions and as such includes border-year effects. The main equation of interest is:

$$\begin{aligned} p_{i,b,t} &= \alpha^S B_i^S + \alpha^U B_i^U + \beta^S B_i^S \times post_{2011-2013} + \beta^U B_i^U \times post_{2011-2013} \\ &+ \gamma^S B_i^S \times post_{2014-2016} + \gamma^U B_i^U \times post_{2014-2016} \\ &+ X_i^{'} \theta + \delta_t + \delta_t \times \nu_b + \varepsilon_{i,b,t} \end{aligned} \tag{4.7}$$

$p_{i,b,t}$ is the logarithm of house prices (in USD) of a housing unit sold i along border-segment b and in year-month t, without considering the subsidized, new housing units sold. We also examine the number of crimes reported at the street segment level and census block level, distinguishing between property and non-property crimes. B_i^S and B_i^U are two binary indicators that take a value of one if the housing unit sold i is located within 400 meters of the border (the border band) in the subsidized area (S) and unsubsidized area (U), respectively, and zero if located in area U and between 400 and 800 meters of the border (the control band). $post_{2011-2013}$ and $post_{2014-2016}$ are indicators for two subperiods after the law came into force. Since we analyze projects that were executed between 2011 and 2013, we only report point estimates of the external effects for the subperiod 2014 and 2016 after their construction. However, we also present an event study analysis to test for external effects before, during, and after construction. X_i is a vector containing housing characteristics, as well as, socio-economic variables at the census block level. δ_t stands for year-month effects. $\delta_t \times \nu_b$ represents border-year effects to control for heterogeneity along the border. Finally, $\varepsilon_{i,b,t}$ is an idiosyncratic error term.

As for the set of controls, we include individual housing characteristics such as year of construction, quality of the building (according to the cadaster agency), number of floors, a dummy for a detached house, whether it has a garage, a balcony and other amenities. To control for cross-border heterogeneity, we include a set of socio-economic and urban infrastructure variables at the census block level, namely: 2011 density (inhabitants per km^2, logged), % of vacant and uninhabitable dwellings, % of buildings in poor condition, % of rented dwellings, unemployment rate, % of heads of household with low education level, % of historic buildings, % of buildings with more than six floors, and a street quality index.[33]

[33]This index is constructed as a weighted average of several binary indicators. The variables considered are dummy indicators of whether 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 dumps . For

This estimation strategy relies on two assumptions to obtain unbiased estimates. First, the control group, which is composed of observations within the (unsubsidized) area U is not affected by local spillovers. The empirical evidence indicates that external effects are quite local.[34] As a robustness check and to avoid ad-hoc control group definitions, we estimate externalities non-parametrically as explained in appendix 4.8.4. The second is the common trend assumption. To provide evidence that this identifying assumption holds, we interact $B_i^{j,k}$ with pre-intervention dummies and check whether this interaction is statistically different from zero in an event study graph.

4.4.2 Investment exposure measure

We construct a measure that captures the exposure of each housing unit sold to subsidized investments. This measure is computed as the weighted average of all project budgets using an exponential weighting decay factor. Formally,

$$\text{Intense}_i = \sum_{j=1}^{98} BT_j \times e^{-\lambda d_{ij}} \tag{4.8}$$

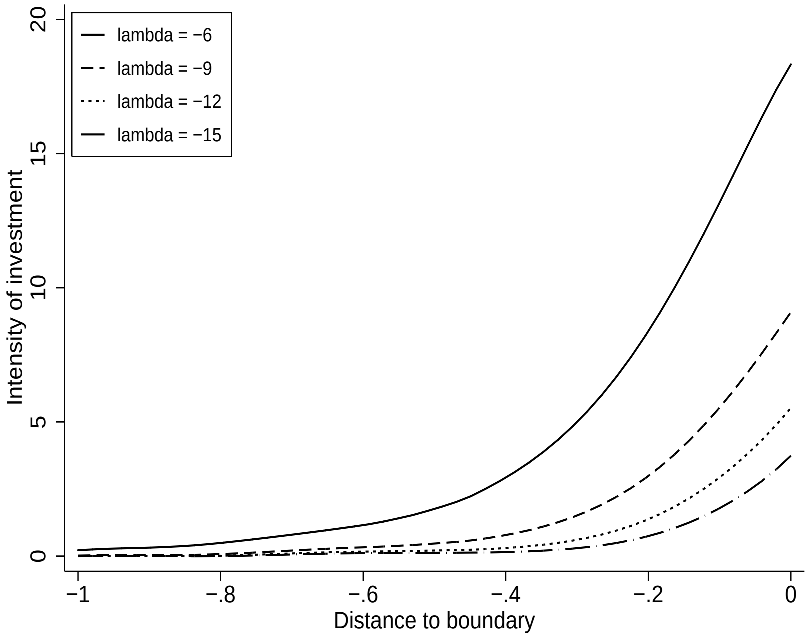
where BT_j is the budget of project j, d_{ij} is the distance (in km) from the sold housing unit i to project j. λ is the parameter that rules the decaying rate of weights. The top panel of Figure 4.4 plots the level of investment intensity (Intense_i) using different values of λ the decaying parameter. As λ increase, the level of exposure declines at faster rates with respect to distance. The bottom panel of Figure 4.4 shows the level of investment intensity along the border for $\lambda = -9$. We observe that there is no investment along some parts of the border of area S with respect to area U, while there is a high concentration of subsidized projects at other locations. As a consequence, external effects are likely to depend on the intensity of investments, which varies across space.

each indicator weights are constructed as one minus its average and they are normalized to sum one. The index is bounded between 0 and 1.

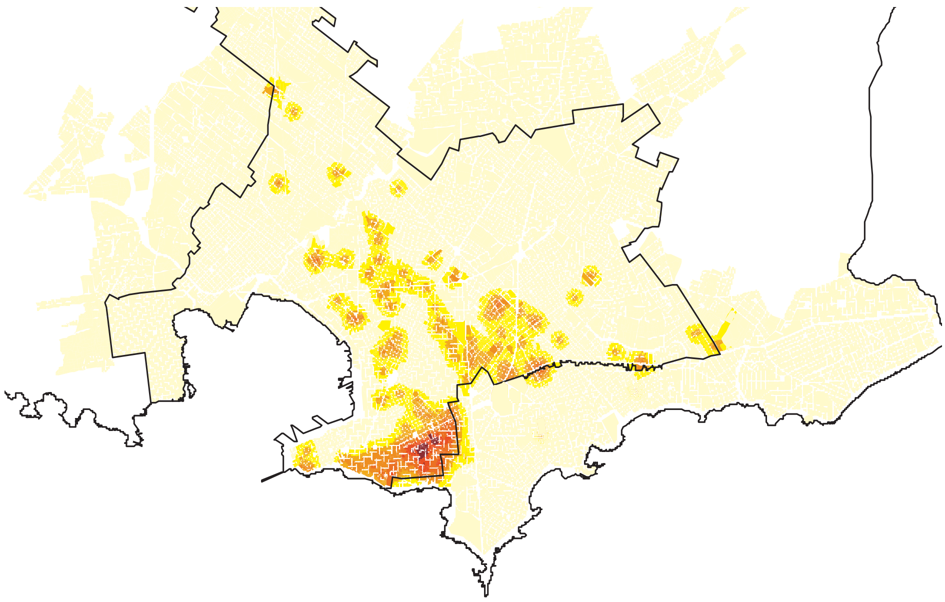
[34] See Turner et al. (2014) for a detailed discussion

Figure 4.4: The exposure to subsidized investments

(a) Investment intensity (Intense$_i$) measure



(b) Investment intensity map ($\lambda = -9$)



Notes: Intense$_i$ captures the level of investment intensity to which each housing unit sold i is exposed. It is built as a weighted average of all project budget with weights that follow an exponential decay scheme.

We use the variable Intense_i as a continuous-treatment variable. Replacing border band indicators in equation 4.7 with the investment exposure measure, we estimate the following continuous difference-in-difference estimator,

$$\begin{aligned} p_{i,b,t} &= \alpha ln(\text{Intense}_i) + \beta ln(\text{Intense}_i) \times post_{2011-2013} \\ &+ \gamma ln(\text{Intense}_i) \times post_{2014-2016} + X_i^{'}\theta + \delta_t + \delta_t \times \nu_b + \varepsilon_{i,b,t} \end{aligned} \tag{4.9}$$

where Intense_i is used as the intensity of the treatment. This continuous treatment variable is likely to be endogenous because developers would tend to choose the most profitable locations along the border. Hence, this measure is instrumented using a binary indicator that takes a value of one for the housing units sold that are located in the unsubsidized area U and zero if located in the subsidized area S, exploiting the place-based structure of the subsidy. We estimate equation 4.9 for several values of λ as a robustness check.

4.5 Data and variables

We combine data from different sources. First, we use official data from the National Housing Agency (*Agencia Nacional de Vivienda*) relating to subsidized, new construction projects. These data contain information about the exact geographical location of projects, approval date, total number of housing units produced (including commercial units and lofts), the budget of each project, budget schedule, whether the project includes facilities and amenities (i.e. garage), and project size (large, medium and small). Second, we use data on house prices from the National Registry Office (*Dirección General de Registros*, DGR) for the period 2006-2016.[35] These data contain transaction prices and built area (in square meters) of housing reported by notaries.[36] We exclude transactions with prices above the bottom and top first percentile by year. This dataset is combined with data on housing characteristics from the National Cadaster Agency (*Dirección Nacional de Catastro*, DNC).[37]

Third, we use geo-coded daily crime incidents in Montevideo between 2006 and 2016 recorded by the Police Department.[38] This database records 1,085,639 of-

[35]Transactions from December 2016 also include data from January and February 2017.

[36]Whenever a house is sold, a notary registers the transaction at the DGR. This information has been used by the National Statistical Office (*Instituto Nacional de Estadística*, INE) to compute the average house price by neighborhood and housing prices indices.

[37]These data are generally updated whenever the property is reassessed by the DNC. Between 2008 and 2010, the DNC performed several reassessments in Montevideo, which means most of the information is updated to that period.

[38]This database has been used by Ajzenman and Jaitman (2016) to study crime concentration

fenses and contains information about types of crime according to the Uruguayan penal code. We consider the three most frequent offenses and classify them as either property or non-property crimes. Under property crimes, we include thefts (55% of the total recorded incidents) and robberies (13% of the total recorded incidents). Both theft and robbery imply depriving a person of property; however, robbery involves the use of violence. Non-property crimes include assaults (4% of the total recorded incidents), defined as a physical attack upon another person. We aggregate this information at two different levels: 1) street segment, which is defined as one side of a block with a length of 80 meters, on average, and; 2) census block, which is a group of blocks where, on average, reside around 1,162 inhabitants and it has an area of half a square kilometer.

Table 4.3: Sample average for outcomes and control variables at border and control bands.

	Border		Control
	Subsidized	Unsubsidized	
Outcome variables in 2010			
House prices (in USD, logged)	10.821	11.137	11.281
Observations	1.067	1.424	847
Number of property crimes (at the street segment)	3.920	3.603	2.945
Number of non-property crimes (at the street segment)	0.124	0.085	0.098
Observations	971	882	857
Number of property crimes (at the census block)	4.898	5.074	4.846
Number of non-property crimes (at the census block)	1.678	1.538	1.126
Observations	71	67	31
2011 Census variables			
Density (inhabitants per km^2, logged)	9.491	9.292	9.434
% of buildings	60.986	62.969	64.905
% of occupied dwellings	89.542	90.361	89.710
% of vacant or uninhabitable dwellings	3.219	2.869	3.054
% of buildings in bad conditions	0.684	0.490	0.505
% of rented dwellings	38.434	31.512	29.702
Unemployment rate	5.979	5.282	5.023
% of head of household with low education level	9.101	6.749	6.331
% of historic heritage sites	1.181	1.254	0.036
Street quality index [0,1]	0.562	0.520	0.551
Observations	71	67	31

Finally, we use data from the 2011 census conducted by the INE, and which contains socio-economic information disclosed at the census block level. [39]

and crime hot-spots in Montevideo, as well as, in other Latin American cities. Borraz and Munyo (2014) use these data to study the effects of conditional cash transfer programs on crime and Munyo and Rossi (2015) to study crime recidivism.

[39]The census block is the lowest level of aggregation at which the 2011 census is disclosed.

Table 4.3 presents the average values of the outcome variables (in 2010), as well as the control variables at the border and the control bands. The control group is a slightly denser area with a higher share of buildings. As expected, the subsidized part of the border presents the highest share of rented dwellings, the highest unemployment rate, and the largest share of heads of household with low education level. Interestingly, it has the highest street quality index (.562) reflecting the fact that this central area is well served by several buses lines, the main means of public transport in the city.

4.6 Results

4.6.1 Spillovers on house prices

Intention-to-treat analysis

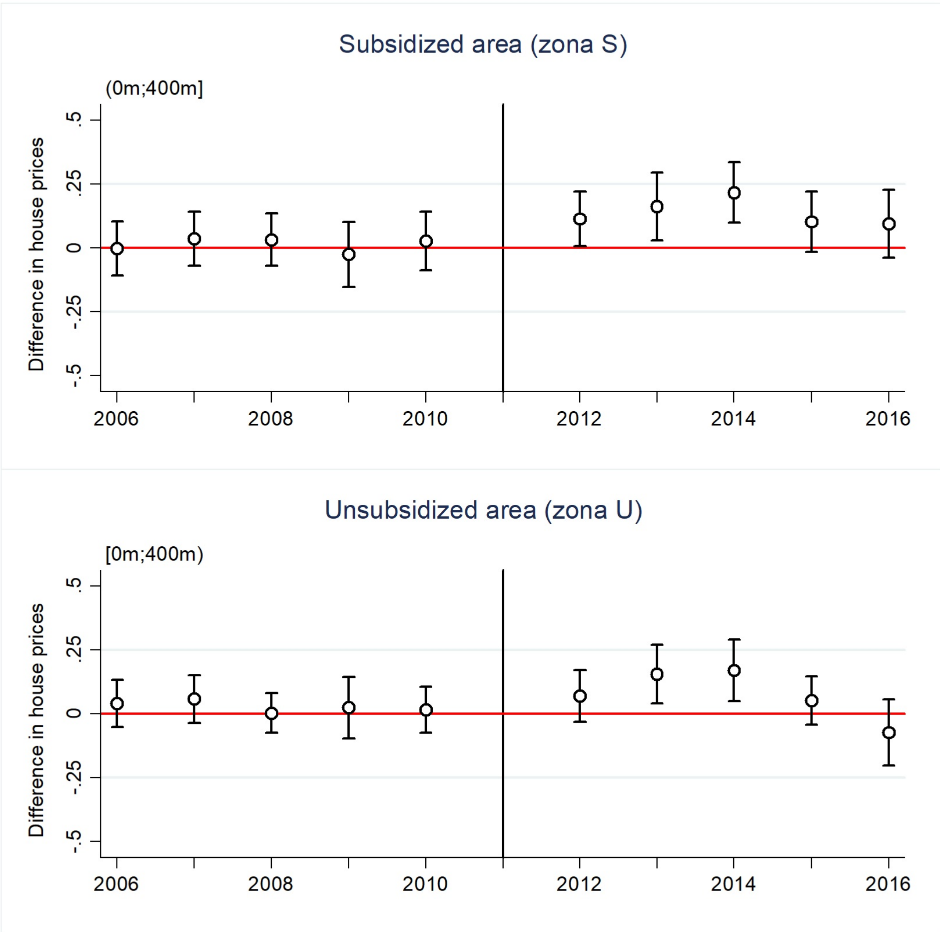
In Figure 4.5 and Table 4.4, we present the intention-to-treat results based on estimating equation 4.7.

In Figure 4.5, we estimate the external effects along the border by year, using the year the policy was introduced, 2011, as the base. The top panel shows that external effects increase along the subsidized part of the border one year after its introduction, reaching a peak in 2014 (an increase of almost 25%) and afterwards it decreases (by around 11%). As such, the evidence points to externalities both during construction (2012-2013) and once the subsidized projects were complete. This is consistent with the idea that agents tend to develop expectations as to how the local housing market will react once these place-based projects finish. The bottom panel indicates that these externalities also tend to increase on the unsubsidized side of the border following the introduction of the law. However, in this case the effect dissipates rapidly. In both panels, we observe that there are no spillovers prior to 2011, meaning that no anticipation effects are likely to have been present. The hypothesis that developers might react in order to affect the housing market before the policy was introduced can therefore be discarded. Furthermore, this provides supportive evidence that the common trends assumption, which is key to obtaining unbiased estimates of residential externalities, is likely to hold.

Table 4.4 shows the point estimates for the subperiod 2014-2016 - that is, after project completion, using different sets of controls and inference methods. The first and second rows present estimates of the external effects on the subsidized (S) and unsubsidized (U) borders, respectively. The control group is defined as a 400-meter wide band located 400 meters from the border within area U. In the first column, we include border-year effects and housing characteristics. We observe a

12% increase in house prices (3.7% annual rate) in the subsidized area, while there is no statistically significant effects in the unsubsidized area. Point estimates do not differ greatly after including pre-policy (2006-2010) house price changes at the block level (column 2) and also socio-economic variables from the 2011 census at the census block level (column 3).[40]

Figure 4.5: House prices results. Estimates allowing for effects before, during, and after adoption of the policy. Intention-to-treat analysis.



Notes: the dependent variable is house prices in USD, logged. The control group is composed by observations in the unsubsidized area (U) which are located at a distance between 400 and 800 meters from the border. The regression includes individual housing characteristics and controls at the census block level. Clustered standard errors at the census block level used to build 95% confidence intervals.

[40]Results are similar when only using condominium properties, with house prices increasing by around 11% on the subsidized side of the border.

Table 4.4: Spillovers from subsidized residential development projects on house prices. Intention-to-treat analysis.

				Spatial HAC inference[a]		
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Distance to the border (d_i)*:*						
Border band (B^S) **- subsidized area** (S)						
$\mathbb{1}\{0m < d_i \leq 400m\} \times \text{post}_{2014-2016}$	0.117***	0.116***	0.116***	0.117***	0.116***	0.116***
	(0.039)	(0.039)	(0.038)	(0.025)	(0.025)	(0.026)
Border band (B^U) **- unsubsidized area** (U)						
$\mathbb{1}\{0m \leq d_i < 400m\} \times \text{post}_{2014-2016}$	0.029	0.027	0.026	0.029	0.027	0.026
	(0.036)	(0.035)	(0.035)	(0.024)	(0.024)	(0.024)
Border-year FE	Y	Y	Y	Y	Y	Y
Housing characteristics	Y	Y	Y	Y	Y	Y
Δ house prices $_{2010-2006}$ (logged) at census block level	N	Y	Y	N	Y	Y
Controls at census block level	N	N	Y	N	N	Y
Observations	31,727	31,727	31,727	31,727	31,727	31,727
Number of clusters	153	153	153			
Adjusted R squared	0.416	0.416	0.426	0.416	0.416	0.426

Notes: the dependent variable is house prices in USD, logged. The control group is composed by observations in the unsubsidized area (U) which are located at a distance between 400 and 800 meters from the border. Individual housing characteristics include: constructed squared meters (and its square), a dummy for detached house, year of construction, quality of the building (according to the cadaster agency), number of floors, if the dwelling has parking, balcony and other amenities. Controls at census block level include: density in squared km (logged), % of vacant and uninhabitable houses, % of renters, % of unemployed, % of low-educated head of households, % of historic monuments and a street quality index (bounded between 0 and 1). In columns (i) to (iii), clustered standard errors at the census block level used. [a] in columns (iv) to (vi), Conley (1999) spatial HAC standard errors using linearly decay kernel weights *a la Bartlett* used; spatial correlation assumed to vanish above 500 meters; serial correlation assumed to vanish after one year. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

In the first three columns, clustered standard errors at the census tract level are used. The unit of analysis is geo-coded transaction prices and, therefore, sold units located along the census block edges could be correlated with other sold units located in neighboring tracts. Alternatively, in the last three columns of Table 4.4, we use the spatial HAC inference method proposed by Conley (1999) which adjusts for spatial and serial correlation and is robust to heteroscedasticity.[41] Results remain unchanged when using this inference procedure. Therefore, the first main result is that the average (border) spillovers seem to be present on the subsidized side of the border.

In Table 4.5, we present the results when estimating an augmented model, where the border indicator is now split into five dummies to test for a decaying pattern on externalities. As previously shown, most of the new construction projects were highly concentrated close to the border of the subsidized area S in relation to the unsubsidized area U. Housing externalities are expected to increase as we get closer to the border and to decay as we move through the interior of area S in the direction of the suburbs. This hypothesis is confirmed in Table 4.5, which shows this decaying

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

pattern on the subsidized side of the border. In our preferred specification (third column,) and on the subsidized side, we find that house prices increase around 12% in the first 200 meters, 11% in the second 200 meters and finally, 10% in the third 200 meters. We observe a slowly decaying rate in externalities because most of the investments made in the subsidized area close to the border. However, we observe that the external effect decreases from 12% to 7.5%, when we compare the first 200 meters of the subsidized area with the unsubsidized area. Thus, spillovers fall by around two fifth every 200 meters (slightly more than one half in 1,000 feet). Since our results are highly robust to the inclusion of socio-economic controls then, hereafter we present our results using the full-set of controls which is our preferred specification.

Table 4.5: Spillovers from subsidized residential development projects on house prices. Intention-to-treat analysis. Augmented empirical model.

	(i)	(ii)	(iii)
Distance to the border (d_i):			
Border band (B^S) - subsidized area (S)			
$\mathbb{1}\{0m < d_i \leq 200m\} \times \text{post}_{2014-2016}$	0.121***	0.121***	0.120***
	(0.045)	(0.045)	(0.043)
$\mathbb{1}\{200m < d_i \leq 400m\} \times \text{post}_{2014-2016}$	0.119***	0.118***	0.113***
	(0.044)	(0.044)	(0.043)
$\mathbb{1}\{400m < d_i \leq 600m\} \times \text{post}_{2014-2016}$	0.094	0.093	0.102*
	(0.062)	(0.061)	(0.060)
Border band (B^U) - unsubsidized area (U)			
$\mathbb{1}\{0m \leq d_i < 200m\} \times \text{post}_{2014-2016}$	0.075*	0.073*	0.074*
	(0.041)	(0.041)	(0.041)
$\mathbb{1}\{200m \leq d_i < 400m\} \times \text{post}_{2014-2016}$	-0.039	-0.040	-0.043
	(0.045)	(0.044)	(0.044)
Border-year FE	Y	Y	Y
Housing characteristics	Y	Y	Y
Δ house prices $_{2010-2006}$ (logged) at census block	N	Y	Y
Controls at census block level	N	N	Y
Observations	35,841	35,841	35,841
Number of clusters	173	173	173
Adjusted R squared	0.416	0.417	0.428

Notes: the dependent variable is house prices in USD, logged. The control group is composed by observations in the unsubsidized area (U) which are located at a distance between 400 and 800 meters from the border. Individual housing characteristics include: constructed squared meters (and its square), a dummy for detached house, year of construction, quality of the building (according to the cadaster agency), number of floors, if the dwelling has parking, balcony and other amenities. Controls at census tract level include: density in squared km (logged), % of vacant and uninhabitable houses, % of renters, % of unemployed, % of low-educated head of households, % of historic monuments and a street quality index (bounded between 0 and 1). Clustered standard errors at the census block level used. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

To estimate spillovers parametrically, this empirical strategy involves comparing a border with a control band that is somehow arbitrarily defined. We address this concern by estimating spillovers as a non-parametric function of the distance to the border before and after the introduction of the subsidy using the semi-parametric differencing approach developed by Yatchew (1997); Yatchew and No (2001). Results are presented in Figure 4.10 in Appendix 4.8.4, where the distance is normalized to zero at the border and negative values correspond to the unsubsidized area U and positive values to the subsidized area S. The figure shows that as we get closer to the border (coming from area U), spillovers increase becoming statistically significant 200 meters from the border. As in the parametric estimates, residential externalities are higher on the subsidized side of the border, showing almost a 6% increase in house prices, on average.

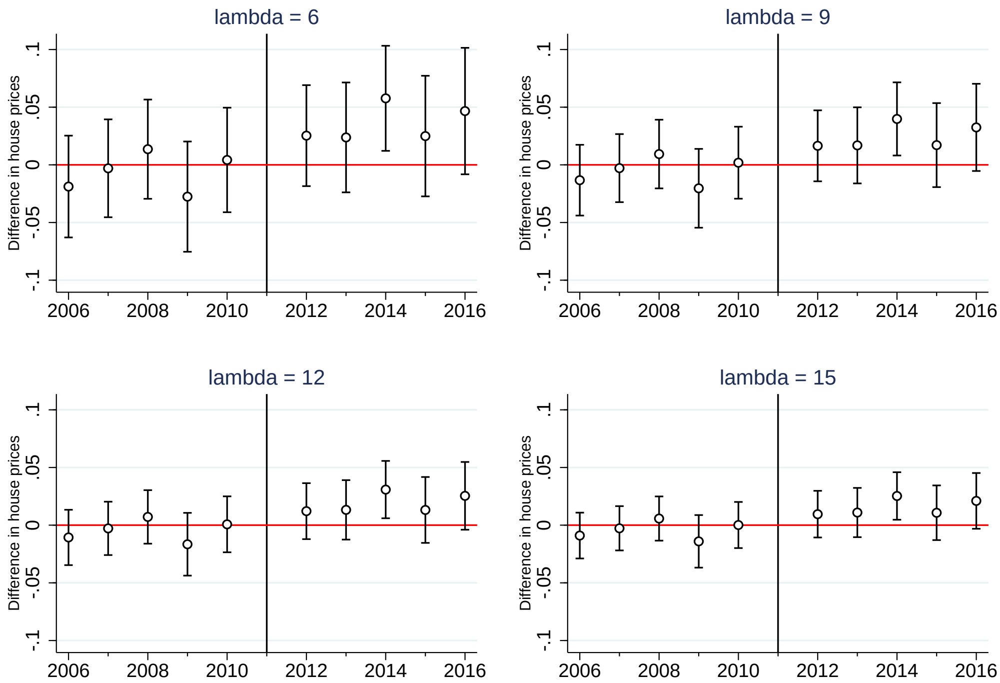
Continuous difference-in-difference

Externalities are likely to depend on the level of investment along the border. Developers might choose places with better housing market prospects as opposed to other less attractive locations, leading to a variation in the level of investment exposure along the border. Indeed, using the investment exposure measure built to capture the spatial heterogeneity in investment (Figure 4.4), we observe that developers focus their projects in zones close to the border location. As a choice variable, this continuous treatment measure is endogenous. To mitigate this source of endogeneity, we exploit the exogenous nature of the place-based structure of the subsidy. Then, our continuous treatment variable is instrumented by a binary indicator that takes a value of one for units located in the unsubsidized area U and zero otherwise. The exposure measure constructed depends on the decaying parameter λ.

Figure 4.6 shows the yearly estimates of the investment exposure measure on house prices for several λ values and for our preferred specification.[42] Overall, results are similar for different λ values. Note that precision increases while point estimates decrease for larger λ values. This is explained by the fact that as λ increases, the source of identification is provided by sold housing units that are located close to subsidized high-investment projects. As shown in Figure 4.6, before the subsidy came into force, the coefficient is not statistically significant and nearly zero. After 2011, the magnitude increases becoming significant for almost the last three years. As before, we observe external effects in the two years after the law is introduced, but now these effects are strongly present for the last subperiod (2014-2016). The graphical analysis supports the validity of our continuous difference-in-difference strategy.

[42] Similar to Autor et al. (2016) who construct a measure of rent-control exposure for each parcel, we build an investment exposure measure for four possible λ values (6, 9, 12, 15).

Figure 4.6: House prices results. Estimates allowing for effects before, during, and after adoption of the policy. IV estimates



Notes: the dependent variable is house prices in USD, logged. The main independent variable is the investment exposure ln(intense), which depend on the decaying parameter *lambda*. Instrument: binary indicator that takes the value of one for housing units in the unsubsidized area U, and zero otherwise. Regressions include individual housing characteristics and census block controls. Clustered standard errors at the census block level used to build 95% confidence intervals.

In Table 4.6, we present the results based on estimating equation 4.9, where border indicators B^j (with $j = S,U$) are replaced by the logarithm of the investment exposure variable (intense), and using different decaying parameter values λ. We estimate 4.9 using instrumental variables as explained above. We find positive and statistically significant effects for all the values of λ considered. As noted, this elasticity is reduced as we increase the parameter λ. Taking the average of the estimates for the different values of λ, we find that a one percent increase in the level of investment intensity led to an almost .035 percent increase in house prices. The instrument seems to be relevant since the (Kleibergen-Paap) F-statistic is well above ten, the usual benchmark.

To compare these findings with the results in Table 4.6, we compute the policy effect in the border as the average change in the investment exposure measure (from the control to the border band) multiplied by the estimated elasticity. We obtain a

Table 4.6: Measuring externalities from subsidized residential development projects on house prices. IV estimates

	λ			
	6	9	12	15
Investment exposure: intense; *Distance to the border:* d_i				
ln(intense) $\times$ $post_{2014-2016}$	0.049***	0.035***	0.028***	0.023***
	(0.016)	(0.011)	(0.009)	(0.008)
External effects in:				
Subsidized area (S): $d_i \in (0m;400m]$	20.682	19.798	19.665	19.732
Unsubsidized area (U): $d_i \in [0m;400m)$	11.856	11.427	11.475	11.637
Border-year FE	Y	Y	Y	Y
Housing characteristics	Y	Y	Y	Y
Δ house prices $_{2010-2006}$ (logged) at census block	Y	Y	Y	Y
Controls at census block level	Y	Y	Y	Y
Observations	31,727	31,727	31,727	31,727
Number of clusters	153	153	153	153
F-statistics	27.068	24.128	22.420	21.263

Notes: the dependent variable is house prices in USD, logged. The main independent variable is the investment exposure intense, which depend on the decaying parameter λ. Instrument: binary indicator that takes the value of one for housing units in the unsubsidized area U, and zero otherwise. Individual housing characteristics include constructed squared meters (and its square), a dummy for house, year of construction, quality of the building (according to the cadaster agency), number of floors, if the dwelling has parking, balcony and other amenities. Controls at census block level include: density in squared km (logged), % of vacant and uninhabitable houses, % of renters, % of unemployed, % of low-educated head of households, % of historic monuments, % of shops and % of buildings. Clustered standard errors at the census block level used. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

20% (6.3 annual rate) and 12% (3.7 annual rate) increase in house prices on the subsidized and unsubsidized sides of the border, respectively. As expected, these effects are greater than the 12% and 3% obtained in the intention-to-treat analysis for areas S and U, respectively, and are also relatively higher than the externalities reported in the previous literature for developed countries. Thus, the evidence presented indicates substantial spillovers as a result of these place-based subsidized investments. The empirical results are consistent with the simple theoretical framework previously developed, which predicts an increase in house prices as the quality of the housing stock in the neighborhood with the lower level of amenities increases.

To put these results into perspective, Rossi-Hansberg et al. (2010) report externalities in land prices of around 2 to 5% (annual rate) derived from revitalization programs in the US (Richmond, Virginia) and find that the effect is reduced by a half every 1,000 feet. Diamond and McQuade (2018) find that the LIHTC in US revitalizes low-income neighborhoods and increases housing prices by 6.5% after 10 years. Autor et al. (2016) find that residential properties at the 75th percentile of rent control exposure gained around 13 percent more in assessed value following decontrol than did properties at the 25th percentile of exposure in US. As such, the average external effects found herein are slightly larger than those reported in the previous literature when external effects are detected.[43] Furthermore, in line with the previous literature, these externalities are extremely local and decay rapidly.

The results obtained indicate that there are significant housing spillovers due to the construction of new subsidized housing units and that these externalities are mostly present along the subsidized border. One possibility is that these results are heterogeneous with respect to a neighborhood's characteristics. In the first column of Table 4.7, we analyze whether external effects are heterogeneous in terms of a street quality index bounded between 0 and 1 which measures the level and quality of urban infrastructure at the census block level. Specifically, we construct a dummy variable which takes a value of one if the street quality index is above the median. We then interact this with the level of investment intensity (using $\lambda = 9$) and with the *post* dummy variable for the subperiod 2014-2016. We find an elasticity of .049 with respect to house prices for census blocks with a street quality index above the median. This is higher than the .035 obtained in our baseline results. These results could be explained by the complementarities of these new investments with the existing urban infrastructure.

In the second column, we carry out the same analysis but using the % of renters. Again, we find an elasticity of .048 in census blocks with a high share of renters. This could be explained by higher tenant/rental turnover rates in these blocks, which implies a more dynamic change in neighborhood income-composition. Higher shares of renters could lead to faster neighborhood transitions in economic status.[44] Finally, we check for heterogeneous results depending on the % of vacant housing units and find an elasticity of .033 with respect to house prices. We also find significant effects in case of tracts with lower % of vacant houses; however, the estimate is lower. These findings suggest that there are heterogeneous effects regarding housing externalities, mainly, in census blocks with a high quality index and a high percentage of renters.[45]

[43] When studying an urban revitalization program in Berlin after reunification, Ahlfeldt et al. (2016) do not find evidence of significant spillover effects in house prices. (Koster and Van Ommeren, 2017) also do not find housing externalities when studying urban renewal interventions in neighborhoods in the Netherlands.

[44] Rosenthal (2008) finds evidence that higher homeownership rates contribute to reducing the probability of neighborhoods facing urban decline.

[45] In Table 4.11 in Appendix 4.8.4, we analyze heterogeneous effects but using the intention-to-treat approach. Results also point to heterogeneous effects. Similarly, we observe larger increases in house prices among census blocks with a higher street quality index and % of renters, and mainly, on the subsidized side of the border.

Table 4.7: Measuring externalities from subsidized residential development projects on house prices. IV estimates. Heterogeneous analyses (with $\lambda = 9$)

	$c_i =$		
	Street quality index	**% of renters**	**% of vacant or unhabitable houses**
	(i)	(ii)	(iii)
Investment exposure: intense; *Distance to the border:* d_i			
$ln(\text{intense}_{\lambda=9}) \times post_{2014-2016} \times \mathbb{1}\{c_i \geq p50\text{th}\}$	0.049***	0.048***	0.033**
	(0.014)	(0.018)	(0.014)
$ln(\text{intense}_{\lambda=9}) \times post_{2014-2016} \times \mathbb{1}\{c_i < p50\text{th}\}$	0.006	0.022	0.032**
	(0.018)	(0.024)	(0.014)
Border-year FE	Y	Y	Y
Housing characteristics	Y	Y	Y
Δ house prices $_{2010-2006}$ (logged) at census block	Y	Y	Y
Controls at census block level	Y	Y	Y
Observations	31727	31727	31727
Number of clusters	153	153	153
F-statistics	5.662	2.318	12.514

Notes: the dependent variable is house prices in USD, logged. The main independent variable is the investment exposure intense, with a decaying parameter $\lambda = 9$. Instrument: binary indicator that takes the value of one for housing units in the unsubsidized area U, and zero otherwise. Individual housing characteristics include constructed squared meters (and its square), a dummy for house, year of construction, quality of the building (according to the cadaster agency), number of floors, if the dwelling has parking, balcony and other amenities. Controls at census block level include: density in squared km (logged), % of vacant and uninhabitable houses, % of renters, % of unemployed, % of low-educated head of households, % of historic monuments, % of shops and % of buildings. Clustered standard errors at the census block level used. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

Alternative identification strategy

Our empirical strategy provides causal estimates of the effect of subsidized, place-based investments on house prices in the neighborhoods surrounding targeted locations. Here, we provide further evidence on house price spillovers by using an alternative identification strategy which involves using the policy as an exogenous shock on housing investments. In other words, the subsidy increases the number of housing starts in the subsidized area that ultimately affect house prices. We estimate the following regression,

$$p_{i,b,t} = \beta hs_{k,t} + X_i^{'}\theta + \delta_t + \delta_t \times \nu_b + \varepsilon_{i,b,t} \tag{4.10}$$

where hs are the number of square meters developed in housing starts aggregated at the census block level k and in year t. This variable is instrumented using a binary indicator that takes a value of one if the housing unit sold belongs to the unsubsidized area U and also by its interaction with a binary variable that takes a value of one indicating this occurred after the policy come into force.

Table 4.8: Measuring externalities from subsidized residential development projects on house prices. Alternative identification strategy.

a) First stage	hs = **Squared meters developed** IHS$(hs)|_{\theta=}$		
	.5	1	1.5
$\mathbb{1}\{$if i is in the unsubsidized area (U)$\}$	1.634***	0.906***	0.639***
	(0.587)	(0.320)	(0.224)
$\mathbb{1}\{$if i is in the unsubsidized area (U)$\} \times post_{2011-2016}$	-2.061***	-1.119***	-0.781***
	(0.582)	(0.319)	(0.224)
Observations	31,727	31,727	31,727
Number of clusters	153	153	153
Adjusted R squared	0.170	0.168	0.167
b) IV estimates	**House prices**		
	(i)	(ii)	(iii)
Squared meters developed (IHS transformation)	0.083***	0.155***	0.223***
	(0.028)	(0.052)	(0.076)
Observations	31,727	31,727	31,727
Number of clusters	153	153	153
F-statistics	6.998	6.959	6.930
Border-year FE	Y	Y	Y
Housing characteristics	Y	Y	Y
Δ house prices (logged) at census block level	N	Y	Y
Controls at census block level	N	N	Y

Notes: in **panel b**, the dependent variable is house prices in USD (logged) and the main independent variable is the squared meters developed in housing starts at the census block level transformed using the inverse hyperbolic sine transformation. Instrument: binary indicator that takes the value of one for housing units in the unsubsidized area U, and zero otherwise; and its interaction with the $post_{2011-2016}$ dummy variable. Individual housing characteristics include constructed squared meters (and its square), a dummy for house, year of construction, quality of the building (according to the cadaster agency), number of floors, if the dwelling has parking, balcony and other amenities. Controls at census block level include: density in squared km (logged), % of vacant and uninhabitable houses, % of renters, % of unemployed, % of low-educated head of households, % of historic monuments, % of shops and % of buildings. Clustered standard errors at the census block level used. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

Results are presented in Table 4.8. The first stage is shown in panel a. Since the variable hs has a non-negligible number of zeros (and for the sake of interpretation), we use an inverse hyperbolic sine transformation.[46] As expected, on average, there is a higher number of housing developments in the (richer) unsubsidized area U. Nevertheless, the policy seems to have decreased the number of square meters developed in area U. In other words, housing supply shifts upward in the subsidized area as a result of the law. In this setting, it is not possible to disentangle the relocation of housing investments from the unsubsidized to the subsidized area from the overall increase in housing supply. The subsidy could be creating incentives for current developers to relocate but also for new firms to invest and, as such, we

[46]Formally, IHS$(hs) = \frac{1}{\theta}\log[hs\theta + (hs^2\theta^2+1)^{1/2}]$

tend to be capturing a mixture of both effects.[47] The estimated coefficient change depends on the parameter θ of the IHS transformation. Taking the average of the estimates, we find that there is a reduction of 132% in the number of square meters developed in housing starts in the unsubsidized area. In the second stage, the average elasticity across estimates is .15, which is higher than that obtained using the investment exposure measure.

4.6.2 Estimates on crime

We can now examine the externalities that derive from subsidized new construction projects. One potential channel that could explain why these externalities emerge is that these subsidized new construction projects removed neighborhood disamenities. Most of these investments involved demolishing abandoned buildings or disused factories, which are typically seen as crime spots that citizens seek to avoid, and which can depress house prices (Campbell et al., 2010).[48] Therefore, one hypothesis is that these subsidized projects could have eliminated crime spots, thus affecting crime patterns in the areas surrounding the new investment zones.

A reduction in the number of offenses could also be behind these large external effects. Several studies find a causal link between crime and housing prices. For instance, Gibbons (2004) finds that domestic property crime has a significant impact on property prices in areas of London. Pope (2008); Linden and Rockoff (2008) find that house prices are sensitive to the presence of sex offenders in neighborhoods of some US cities. Ihlanfeldt and Mayock (2010) find that only certain types of crime, including robbery and aggravated assault, have an effect on house prices in Florida. To test whether these place-based investments affect crime patterns, we use the same empirical strategy as before but using the number of crimes as an outcome. Concretely, we estimate equation 4.9 using property and non-property crime as the dependent variable.[49]

Results at the street segment level

We aggregate geo-coded crime records at the street segment and year level.[50] Since some street segments recorded zero crime in some years, we use the number of

[47] It could also change the housing specifications of the newly-built units and, so, affect the number of square meters developed in the subsidized area.

[48] As mentioned earlier, the housing units developed under this policy could be afforded by middle and middle-high income households. As such they are less likely to attract individuals prone to engaging in criminal activity. So, this countervailing effect is not likely to be present in this context.

[49] Property crime includes theft and robbery while non-property crime includes assaults. All regressions are weighed by 2011 population at the census block level.

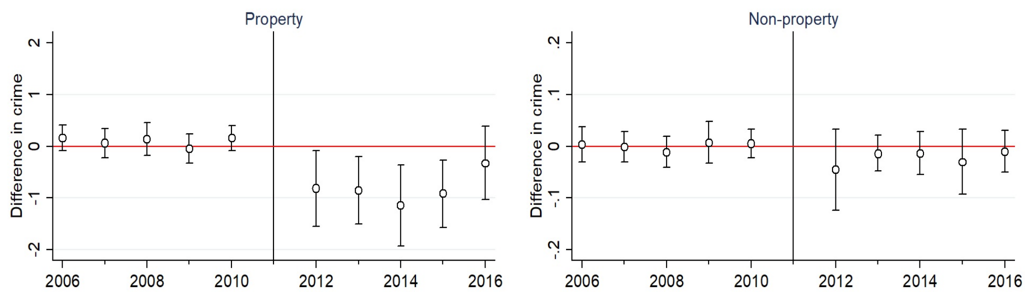
[50] As mentioned earlier, a street segment is one side of a block and, on average, is 80 meters long.

crimes (per 100 meters) as the dependent variable. Results are presented in Figure 4.7 (panel a) and Table 4.9.

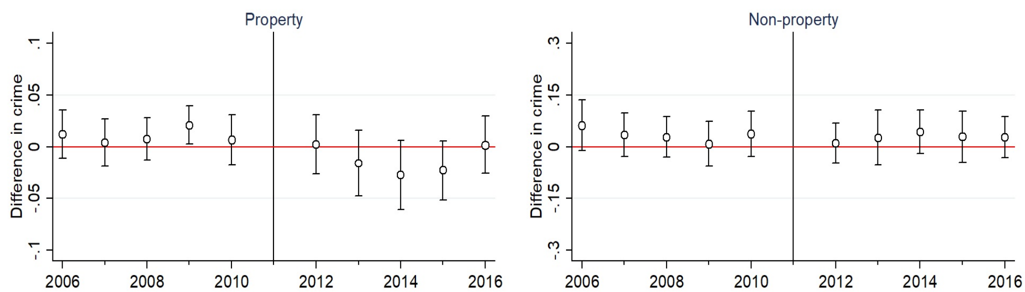
Figure 4.7 (panel a) shows estimates before and after the subsidy was introduced in 2011, using the year 2011 as the base category. The left-hand graph (in panel a) provides evidence of a decrease in property crime after 2011, while there was no effect before the introduction of the policy. As for non-property crime, the right-hand graph (in panel a) indicates that there was no substantial change in non-property crime patterns before and after 2011.

Figure 4.7: Crime results. IV estimates, allowing for effects before, during, and after adoption of the policy.

a) Street segment level



b) Census block level



Notes: In panel a) the dependent variable is the number of crime (per 100 meters) at the street segment level while n panel b) is the number of crimes (logged) at the census block level. Property crime includes theft and robbery while non-property crime includes assaults. The investment exposure measure (ln(intense) with $\lambda = 9$) is used as a continuous treatment variable. Regression includes year effects, border-year effects, controls at street segment and census block level. Regressions are weighted by the 2011 population (at the census block level). Clustered standard errors at the census block level used. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

Table 4.9: Measuring the effects of subsidized residential development projects on crime. IV estimates.

	Street segment		Census block	
	Property crime	Non-property crime	Property crime	Non-property crime
	(i)	(ii)	(iii)	(iv)
Panel a) IV estimates (*intense*)				
Investment exposure intense				
$ln(\text{intense}) \times post_{2014-2016}$	-0.893**	-0.019	-0.026**	-0.001
	(0.398)	(0.018)	(0.012)	(0.024)
Observations	29,546	29,546	1,793	1,793
Number of clusters	162	162	163	163
F-statistics	14.500	14.535	21.876	21.529
Panel b) IV estimates (*building permits*)				
$\text{IHS}(hs\|_{\theta=1})$	-2.502*	-0.059	-0.040**	-0.001
	(1.507)	(0.055)	(0.019)	(0.040)
Observations	29,546	29,546	1,793	1,793
Number of clusters	162	162	163	163
F-statistics	4.978	4.979	9.066	8.931
Border-year FE	Y	Y	Y	Y
2006 crime $^{(*)}$	Y	Y	Y	Y
Controls at census block level	Y	Y	Y	Y
Controls at street segment level	Y	Y	N	N

Notes: in columns (i) and (ii), the dependent variable is the number of crimes (per 100 meters) at the street segment level and in columns (iii) and (iv), it is the number of crimes (logged) at the census block level. $^{(*)}$ in the first two columns, total number of crime (per 100 meters) at the street segment is used while in the last two columns total number of crime (logged) at the census block level is used. In panel a) the investment exposure measure ($ln(\text{intense})$ with $\lambda = 9$) is used as a continuous treatment variable while in panel b) the squared meters developed is used. In each case, the continuous treatment measure is instrumented by a binary indicator that takes the value of one for housing units in the unsubsidized area U, and zero otherwise. Controls at census block level include: density in squared km (logged), % of vacant and uninhabitable houses, % of renters, % of unemployed, % of low-educated head of households, % of historic monuments, % of shops and % of buildings, if there is a police station within 300 meters and if there is a slum within 300 meters. Controls at street segment level include: street lenght (in meters, logged) and, if the segment is part of an avenue. Regressions are weighted by the 2011 population at the census block. All regressions are weighted by 2011 population at the census block level. Clustered standard errors at the census block level used. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

Point estimates are presented in the first two columns of Table 4.9. In panel a, we present the IV estimates using the investment exposure measure (with $\lambda = 9$) as the main explanatory variable. Results (in first column) show that a one percent increase in investment exposure led to a .009 reduction in the number of property crimes. A change in investment exposure from the control band to the subsidized border led to a reduction to just four property crimes . The average number of property crimes (per 100 meters) is 6.7, implying a 60% reduction in property crime on the subsidized side of the border, where developers located most of their investments. As for non-property crime (second column of Table 4.9), there were no statistically significant effects.

One possible mechanism underpinning these results is that the subsidized, new constructions removed crime spots. In panel b of Table 4.9, we present the results from using the alternative identification strategy. That is, using the number of square meters of housing developed aggregated at the census block level. In this case, we observe that a one percent increase in the area developed results in a decrease of almost .03 property crimes. Although, slightly higher than in the previous case, the effect is only statistically significant at the 10% level. Finally, once again there is no effect in the case of non-property crime.

Results at the census block level

Crime could have been displaced from the streets lying close to areas receiving subsidized investments to neighboring streets. To address this concern, and by way of an alternative, we aggregate crime data at the census block level. On average, one census block is made up of 25 street segments. Since census block limits are determined by streets - where most of the crime incidents occur - we use a buffer. This means crimes recorded 50 meters from the census block boundaries are included as offences at the border. Here, we use the number of property and non-property crimes logged.

In Figure 4.7 (bottom panel), we can see that property crimes started to fall the year after the introduction of the policy. The graph suggests that this was only a short-term effect, because as population density increased this seems to have counterbalanced the effect on crime reduction. A longer time-span is clearly needed to corroborate this hypothesis.

In the last two columns of Table 4.9, results using crime data aggregated at the census block level are presented. Focusing on panel a, results indicate that a one-percent increase in investment reduces property crime by 0.026%. Using this elasticity, the policy effect at the border implies almost a 15% reduction in property crime. As for non-property crime, no effects are observed. Similar results are found when using the alternative identification strategy (panel b). In this case, we observe a reduction of .04% in property crimes for a one percent increase in the area of housing developed. This finding is in line with our result at the street segment level. That is, a higher degree of exposure to investment leads to a larger reduction in property crime.

In appendix 4.8.5, we present results for the intention-to-treat analysis. We find that in the subsidized border there is a reduction of almost four property crimes when using crime data at the street segment level. When using data at the census block level, we find an almost 10% reduction in property crimes. Results are similar in the intention-to-treat and the continuous-treatment difference-in-difference analyses. This could indicate that property crime has fallen because the overall policy goal has been to replace abandoned homes with new housing, regardless of the level of investment.

4.6.3 Estimates on socio-economic outcomes

In Table 4.10, we analyze the effect of the policy on three socio-economic outcomes: household income, population, and population with college education.[51] To do so, we use data from the National Household Survey from 2006 to 2016. We aggregate these data at the neighborhood level, the smallest geographical unit at which the survey is still representative.[52] In this case, the main dependent variable intense represents the average level of investment intensity at the neighborhood level. In each case, the continuous treatment measure is instrumented by a binary indicator that takes the value of one for neighborhoods in the unsubsidized area U, and zero otherwise.

Household income is seen to increase in neighborhoods where investment was high, showing, on average, an elasticity of .02. Moreover, the increase in population attributable to the policy shows an elasticity of .021. In the case of population with college education, although positive, the estimates are not statistically significant. The findings suggest a mild increase in both population and household income in neighborhoods with high exposure to subsidized investments. Given that among these high investment areas house prices increase around 6% annually, the effects on housing prices through this indirect channel tend to be moderate unless in the short run.

[51] We use monthly disposable household income.

[52] Montevideo has a total of 62 neighborhoods which are defined by the INE. Each neighborhood includes between 20 and 30 census blocks. We focus our study on neighborhoods that are close to the border.

Table 4.10: Measuring externalities from subsidized residential development projects on socio-demographic outcomes. IV estimates

	λ			
	6	9	12	15
Investment exposure: intense				
a) Household income				
$ln(\text{intense}) \times post_{2014-2016}$	0.025*	0.019*	0.016*	0.015*
	(0.015)	(0.011)	(0.009)	(0.008)
b) Population				
$ln(\text{intense}) \times post_{2014-2016}$	0.028*	0.021**	0.018**	0.017**
	(0.014)	(0.011)	(0.009)	(0.008)
c) Population with college				
$ln(\text{intense}) \times post_{2014-2016}$	0.018	0.014	0.012	0.011
	(0.049)	(0.037)	(0.031)	(0.028)
Observations	341	341	341	341
F-statistics	34.294	33.653	35.360	37.279
Border-year FE	Y	Y	Y	Y
Controls from the 2011 census	Y	Y	Y	Y

Notes: we use data from the National Household Survey (2006-2016), which is aggregated at the neighborhood level. Outcome variables: (real) disposable household income (logged) in **panel a**, total population (logged) in **panel b**, and total population with more than 24 years old and with college (logged). The main independent variable is the investment exposure $ln(\text{intense})$, with a decaying parameter λ. 2011 census controls: density in squared km (logged), % of vacant and uninhabitable houses, % of renters, % of unemployed, % of low-educated head of households and % of historic monuments. Clustered standard errors at the census block level used. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

4.7 Conclusions

This paper has estimated the spillover effects derived from a place-based housing subsidy in Montevideo (Uruguay), introduced by legislation passed in 2011. This subsidy has resulted in major investment in the housing sector, with a total of 742M USD invested between 2011 and 2016 (1.5% of the five-year GDP average), and the development of around 13K new housing units. The goal of this policy was to attract private residential investment in middle-income areas presenting a deteriorating quality in its housing. In these neighborhoods, both the private and public sectors had not invested for a considerable time and, in some instances, population loss had been experienced. We find evidence of substantial spillovers in zones located near new subsidized housing units. House prices increase by 12% in the border band, while the continuous treatment measure estimates a 20% increase. Indeed, when applying the latter strategy, house prices increase by 12% in the unsubsidized border zone. Moreover, in areas with a high share of renters, externalities have a marked presence, suggesting that the effects of place-based interventions can be highly heterogeneous and may be dependent on initial neighborhood characteristics, as has been suggested in the literature (Baum-Snow and Marion, 2009; Eriksen and Rosenthal, 2010; Diamond and McQuade, 2018). Finally, the property crime rate seems to decrease, whereas that of non-property crime remains unchanged.

The results of this paper indicate that this housing policy seems to have contributed to the revitalization of some middle-income areas of the city. However, 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 the development of new housing units for more vulnerable households. As such, these findings highlight the apparent trade-off between creating affordable housing and inducing rapid urban revitalization, two first-order problems that seem to be addressed by two different housing policies.

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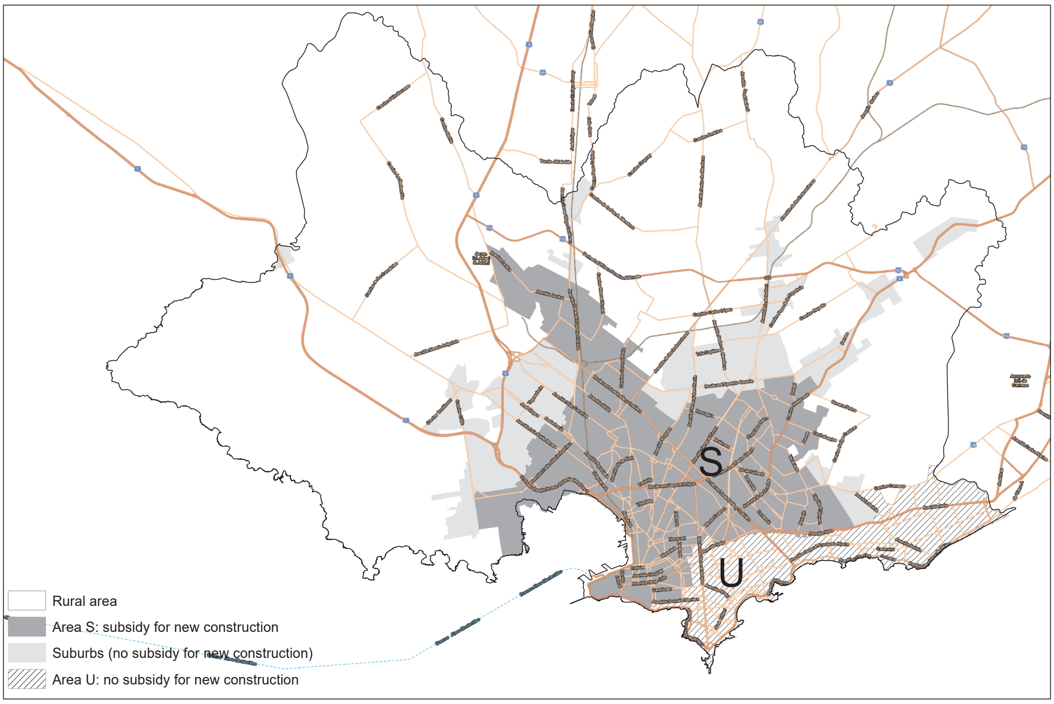
4.8 Appendix

4.8.1 Policy borders and main streets

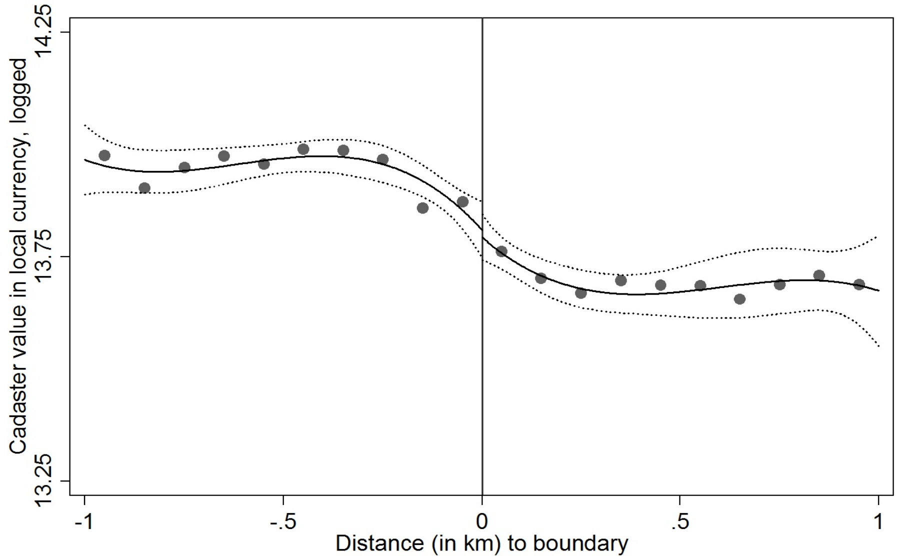
Figure 4.8: Boundary and main streets



Notes: This map includes the main streets and avenues of the city. An important avenue of the city was used to set the border of area S with respect to area U.

4.8.2 Cadaster value at the border

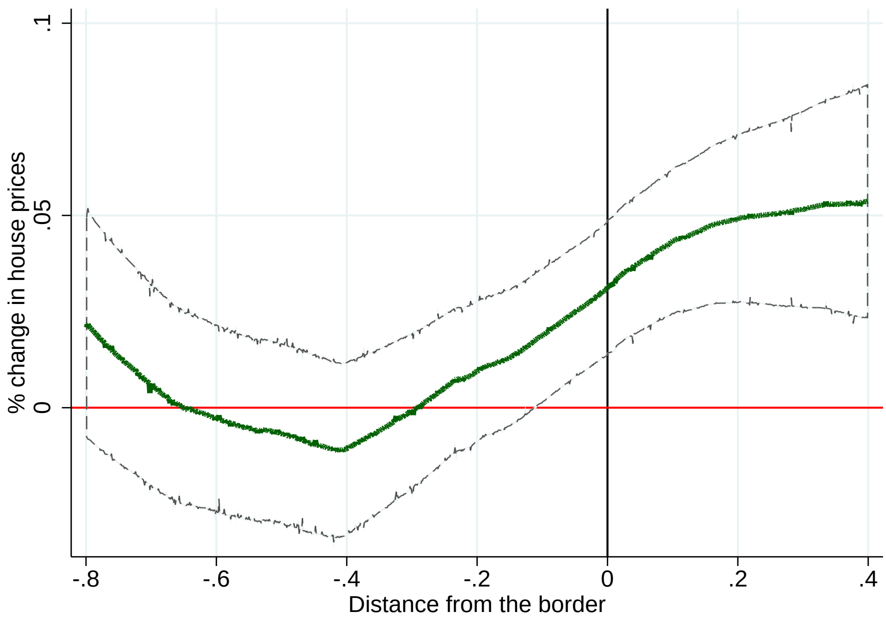
Figure 4.9: Cadaster value in local currency (logged) - area S vs area U



Notes: distance (in km) is normalized to be zero at the border and positive values correspond to area S while negatives to area U. Each evenly spaced bin represents the average cadaster value (in local currency, logged). The cadaster value of a property is set by the Cadaster Agency after its assessment. The cadaster values used here correspond to reassessment performed between 2008-2010. The fitted line is estimated using a 3rd order polynomial. Regressions include year-month fixed effects, border-year fixed effect and housing characteristics. Clustered standard errors at the census block level used to construct confidence intervals.

4.8.3 Spillovers on house prices: non-parametric estimates

Figure 4.10: Spillovers on house prices. Non-parametric estimates as function of the distance to the border of S with respect to U.



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

We estimate the following hedonic semi-parametric prices equation by using the differencing approach developed by Yatchew (1997); Yatchew and No (2001):

$$p_{i,b,t} = f_t(d_{i,b}) + X_i^{'}\theta + \delta_t + \delta_t \times \nu_b + \varepsilon_{i,b,t} \tag{4.11}$$

$p_{i,b,t}$ is the logarithm of house prices (in USD) of a housing unit sold i at border-segment b and year-month t, without considering subsidized new housing units sold; X_i contains housing characteristics, as well as, socio-economic variables at the census block level. Housing characteristics includes: constructed squared meters (and its square), a dummy for house, year of construction, quality of the building (according to the cadaster agency), number of floors, if the dwelling has parking, balcony and other amenities. Census block covariates include: density in squared km (logged), % of vacant and uninhabitable houses, % of renters, % of unemployed, % of low-educated head of households, % of historic monuments, % of shops and % of buildings; δ_t denotes year effects and $\delta_t \times \nu_b$ border-year fixed effects; $f_t(d_{i,b})$ is a nonparametric term which depends on the distance of each land lot to the border of area S with respect to area U, and it enables to measure how local spillovers evolve as moving away from the border before and after the intervention; ε is an idiosyncratic error term. The parametric and nonparametric components are assumed to be additively separable. The differencing approach of Yatchew (1997) is followed in order to get an estimate of the nonparametric part. The function $f_t(d_{i,b})$ is estimated for the pre-policy period and after the policy is implemented. Then, we get $\widehat{f}_{\text{pre}}(d_{i,b})$ and $\widehat{f}_{\text{post}}(d_{i,b})$. The green line in Figure 4.10 shows the difference between $\widehat{f}_{\text{post}}(d_{i,b})$ and $\widehat{f}_{\text{pre}}(d_{i,b})$, and it is normalized using the average estimated difference for the control band (between 400 and 800 meters away from the border located in area U).

4.8.4 Heterogeneous analyses: intended-to-treat estimates

Table 4.11: Spillovers from subsidized residential development projects on house prices. Heterogeneous analysis

	$c_i =$		
	Street quality index	**% of renters**	**% of vacant or unhabitable houses**
	(i)	(ii)	(iii)
Distance to the border (d_i)			
Subsidized area (zona S)			
$\mathbb{1}\{0m < d_i \leq 400m\} \times post_{2014-2016} \times \mathbb{1}\{c_i \geq p50\text{th}\}$	0.178***	0.220***	0.133***
	(0.044)	(0.071)	(0.045)
$\mathbb{1}\{0m < d_i \leq 400m\} \times post_{2014-2016} \times \mathbb{1}\{c_i < p50\text{th}\}$	0.025	0.100*	0.095*
	(0.058)	(0.060)	(0.056)
Unsubsidized area (zona U)			
$\mathbb{1}\{0m < d_i \leq 400m\} \times post_{2014-2016} \times \mathbb{1}\{c_i \geq p50\text{th}\}$	0.049	0.140*	0.032
	(0.055)	(0.071)	(0.045)
$\mathbb{1}\{0m < d_i \leq 400m\} \times post_{2014-2016} \times \mathbb{1}\{c_i < p50\text{th}\}$	-0.015	-0.002	0.021
	(0.044)	(0.042)	(0.054)
Border-year FE	Y	Y	Y
Housing characteristics	Y	Y	Y
Δ house prices $_{2010-2006}$ (logged) at census block	Y	Y	Y
Controls at census block level	Y	Y	Y
Observations	31,727	31,727	31,727
Number of clusters	153	153	153
Adjusted R squared	0.427	0.428	0.427

Notes: the dependent variable is house prices in USD, logged. The control group is composed by observations in the unsubsidized area (U) which are located at a distance between 400 and 800 meters from the border. Individual housing characteristics include: constructed squared meters (and its square), a dummy for detached house, year of construction, quality of the building (according to the cadaster agency), number of floors, if the dwelling has parking, balcony and other amenities. Controls at census tract level include: density in squared km (logged), % of vacant and uninhabitable houses, % of renters, % of unemployed, % of low-educated head of households, % of historic monuments and a street quality index (bounded between 0 and 1). Clustered standard errors at the census tract level used. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

4.8.5 Effects on crime: intended-to-treat analysis

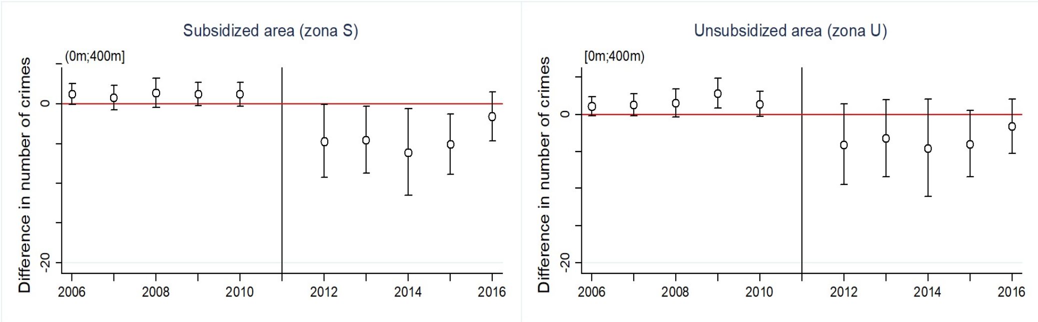
Table 4.12: Measuring the effects of subsidized residential development projects on crime. Intended-to-treat analysis.

	Street segment		**Census block**	
	Property crime	Non-property crime	Property crime	Non-property crime
	(i)	(ii)	(iii)	(iv)
Distance to the border (di):				
Subsidized area (zona S)				
$\mathbb{1}\{0m < di \leq 400m\} \times post_{2014-2016}$	-3.788**	-0.094	-0.097**	-0.027
	(1.791)	(0.087)	(0.041)	(0.100)
Unsubsidized area (zona U)				
$\mathbb{1}\{0m < di \leq 400m\} \times post_{2014-2016}$	-3.469	-0.130	-0.077**	-0.048
	(2.166)	(0.097)	(0.036)	(0.100)
Observations	29546	29546	1793	1793
Number of clusters	162	162	163	163
Adjusted R squared	0.216	0.066	0.840	0.458

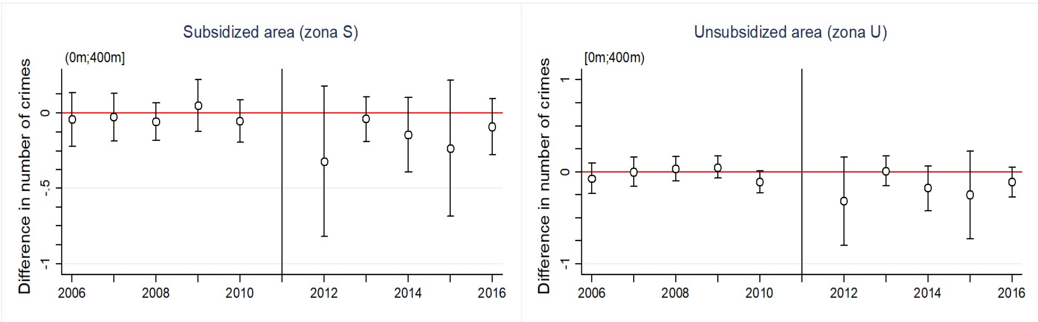
Notes: in columns (i) and (ii), the dependent variable is the number of crimes (per 100 meters) at the street segment level and in columns (iii) and (iv), it is the number of crimes (logged) at the census block level. The control group is composed by observations in the unsubsidized area (area U) which are located at a distance between 400 and 800 meters from the border. $^{(*)}$ in the first two columns, total number of crime (per 100 meters) at the street segment is used while in the last two columns total number of crime (logged) at the census block level is used. Controls at census block level include: density in squared km (logged), % of vacant and uninhabitable houses, % of renters, % of unemployed, % of low-educated head of households, % of historic monuments, % of shops and % of buildings, new residential housing developed (in km2) at t, if there is a police station within 300 meters and if there is a slum within 300 meters. Controls at street segment level include: street lenght (in meters, logged) and, if the segment is part of an avenue. Regressions are weighted by the 2011 population at the census block. All regressions are weighted by 2011 population at the census block level. Clustered standard errors at the census block level used. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

Figure 4.11: Crime results at street segment level. Estimates allowing for effects before, during, and after adoption of the policy.

Property crime



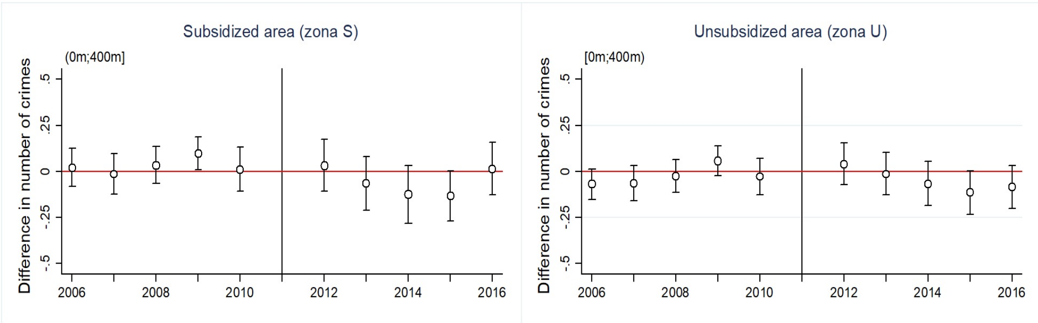
Non-property crime



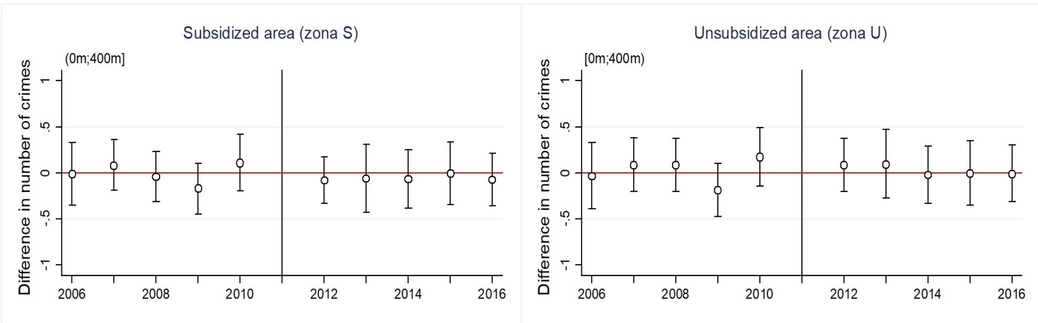
Notes: The dependent variable is the number of crime (per 100 meters) at the street segment level. Property crime includes theft and robbery while non-property crime includes assaults. The control group is composed by observations in the unsubsidized area (area U) which are located at a distance between 400 and 800 meters from the border. Regression includes year effects, border-year effects, controls at street segment and census block level. Controls at census block level include: density in squared km (logged), % of vacant and unhabitable houses, % of renters, % of unemployed, % of low-educated head of households, % of historic monuments, % of shops and % of buildings, if there is a police station within 300 meters and if there is a slum within 300 meters. Controls at street segment level include: street lenght (in meters, logged) and, if the segment is part of an avenue. Regressions are weighted by the 2011 population (at the census block level). Clustered standard errors at the census block level used. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

Figure 4.12: Crime results at census block level. Estimates allowing for effects before, during, and after adoption of the policy.

Property crime



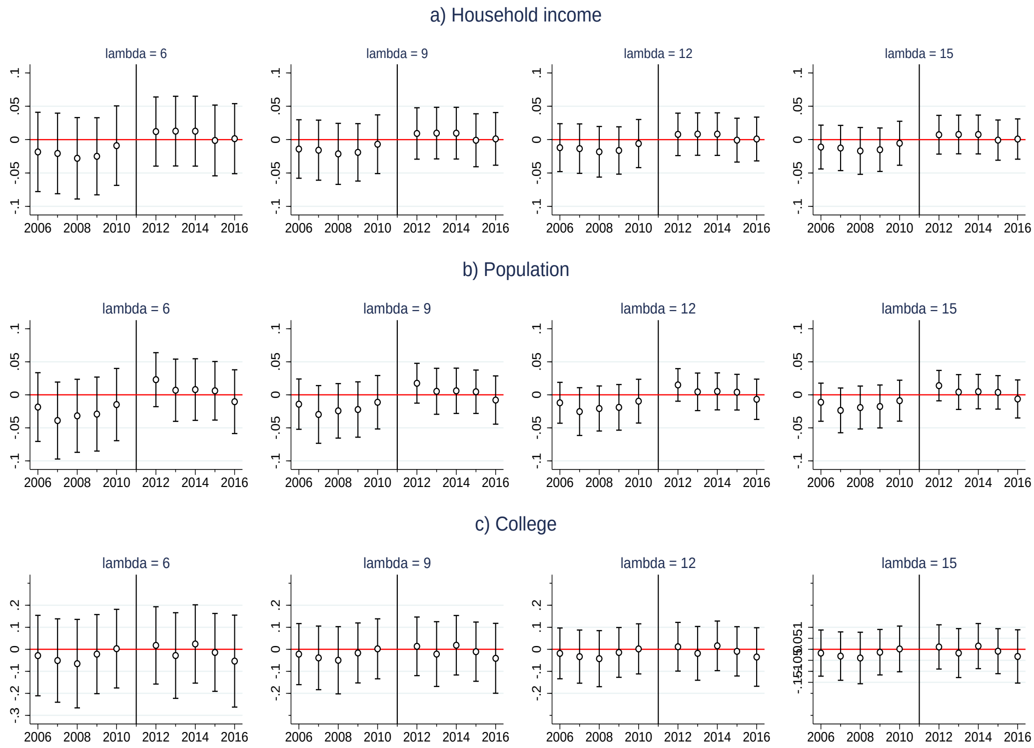
Non-property crime



Notes: The dependent variable is the number of crimes (logged) at the census block level. Property crime includes theft and robbery while non-property crime includes assaults. The control group is composed by observations in the unsubsidized area (area U) which are located at a distance between 400 and 800 meters from the border. Regression includes year effects, border-year effects, controls at the census block level. Controls at census block level include: density in squared km (logged), % of vacant and unhabitable houses, % of renters, % of unemployed, % of low-educated head of households, % of historic monuments, % of shops and % of buildings, if there is a police station within 300 meters and if there is a slum within 300 meters. Regressions are weighted by the 2011 population (at the census block level). Clustered standard errors at the census block level used. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

4.8.6 Effects on socio-economic outcomes

Figure 4.13: Socio-economic outcomes at the neighborhood level. Estimates allowing for effects before, during, and after adoption of the policy.



Notes: we use data from the National Household Survey (2006-2016), which is aggregated at the neighborhood level. Outcome variables: (real) disposable household income (logged) in **panel a**, total population (logged) in **panel b**, and total population with more than 24 years old and with college (logged). The main independent variable is the investment exposure ln(intense), with a decaying parameter λ. Instrument: binary indicator that takes the value of one for housing units in the unsubsidized area U, and zero otherwise. In each case, the continuous treatment measure is instrumented by a binary indicator that takes the value of one for neighborhoods in the unsubsidized area U, and zero otherwise. 2011 census controls: density in squared km (logged), % of vacant and uninhabitable houses, % of renters, % of unemployed, % of low-educated head of households and % of historic monuments. Robust standard errors used to build 95% confidence intervals

5 Conclusions

We live in an urban world and this is not a characteristic that looks set to change. The share of urban population increased from 33% in 1960 to 54% in 2016, and is predicted to reach 66% in 2050 (United Nations data). Yet, as the world becomes more urbanized, new challenges emerge and, with them, urban costs rise. Scholars have published solid theoretical and empirical research pointing to the benefits of living, working, and producing in the city; however, much less is known about the costs of urban living and the policies that might be useful in mitigating them. This PhD thesis sheds light on policies that attempt to deal with two of the most noteworthy problems: namely, urban inequality and urban residential segregation.

The second chapter of this Thesis assesses the impact on neighborhood population dynamics of a major urban renewal policy, specifically a place-based program implemented in the most deprived neighborhoods of Catalonia in the period 2004-2010. This policy sought to deconcentrate poverty and the immigrant population in the more highly deteriorated neighborhoods of the region and those with high shares of foreign-born population. Overall, the results indicate that the policy did not revert the population dynamics of the treated neighborhoods, despite the significant investments made.

However, interventions in the historic districts of the city of Barcelona seem to have been an exception to the more general outcome. Here, the evidence shows that native and EU15 immigrant populations, in addition to numbers of college graduates, increased among the treated neighborhoods in Barcelona. This finding suggests that people's willingness to pay for investments in public spaces and facilities in such neighborhoods is not zero. It should, however, be stressed that in the late 2000s, these same neighborhoods were evidencing a process of gentrification. As such, it might be the case that the urban renewal policy was in fact fueling this ongoing process in these specific areas. Alternatively, it could be that the effectiveness of income-mixing policies actually depends on the urban context. In the case of Barcelona, there could be complementarities between public investment and the historical amenities in the city's central areas. Therefore, the willingness to pay for urban renewal could be higher in central locations with a high historical-architectural value compared to that in other targeted locations.

In the case of the rest of the neighborhoods in Catalonia that experienced the policy intervention, the new amenities generated proved insufficient to attract native population. Overall, these targeted neighborhoods had high immigrant shares and seemed to be evolving towards highly segregated neighborhoods. In these communities, the policies failed to reverse their ethnic dynamics, the new amenities being insufficient to offset the native disutility of moving into neighborhoods with a high immigrant share. Therefore, in order to increase the probability of success, the evidence reported here suggests that a more targeted approach is needed, that is, a policy that concentrates investments into just a few neighborhoods (i.e., the most deprived) is likely to be more efficient than a dispersed investment approach.

According to the previous literature, there are two broad drivers of ethnic residential segregation. First, since ethnic background is highly correlated with income, income segregation naturally leads to ethnic segregation. Second, there might be social interactions in housing demand as natives appear to avoid neighborhoods with a high minority share. The third chapter of this Thesis provides a new empirical approach by employing one-sided tipping models in the population composition of neighborhoods, and at the same time exploits an extraordinary set of events in Spain. After a massive immigrant inflow in years 2001 to 2009, economic recession stemmed these inflows and the immigrant share fell slightly between 2010 and 2015. One prediction made by the tipping model is that neighborhoods with a high minority share should continue to lose native population, despite the fact that no new immigrants are settling in the city. To test this prediction, this study examines neighborhood population dynamics in Spain's six largest cities at the census tract level between 2001 and 2015. The results support the existence of tipping points. In the period of immigration boom (2001-2009), neighborhoods with high minority shares in 2001 received larger inflows of immigrants and experienced outflows in native population. In the period of immigration freeze (2010-2015), the neighborhoods that had received large immigrant influxes in the previous period continued to lose native population, despite the fact that these neighborhoods were actually experiencing slight losses of immigrant population.

The determinants of residential segregation could have direct implications for the optimal design of income-mixing policies. If segregation is caused primarily by economic factors, people-based policies that directly target the disadvantaged, irrespective of where they live, would be more effective than place-based initiatives. Targeting deteriorated areas rather than directly targeting the less affluent is unlikely to have efficient outcomes. The main reason for this is that the high levels of spatial concentration are the result of optimal residential choices taken by different ethnic groups which happen to have different levels of income. In contrast, if ethnic clusters are mainly caused by native flight, then the high level of residential segregation may not be desirable from a social point of view. In this instance, a place-based policy could be welfare enhancing, provided it is able to push segregated neighborhoods to another equilibrium characterized by more ethnically diverse neighborhoods. In this sense, a well-designed, place-based policy that concentrates substantial investments and which, as a result, substantially boosts neighborhood amenity levels could attract natives to move into the renewed areas.

The fourth chapter of this Thesis estimates the spillover effects on housing prices and criminal records of a place-based housing subsidy in the city of Montevideo (Uruguay). The main aim of this policy, introduced in 2011, was to increase and improve the stock of housing by providing incentives to the construction sector via subsidies. The empirical analysis indicates evidence of positive spillovers on both house prices and on property crime records.

The design of this housing policy in Montevideo is interesting for a number of reasons. First, it was designed to meet various goals, one of which was to produce housing units that could be afforded by low/middle-low income households. Yet, the policy lacked appropriate rules to ensure the units built targeted this income group. Indeed, the units eventually built could only be afforded by middle-high (/high) income households. As such, this raises questions regarding the apparent trade-off between producing affordable housing and inducing rapid urban revitalization. Second, most of the subsidized investments made between 2011 and 2013 were executed close to the border with the unsubsidized area. This is explained by another feature of the policy related to the combination of the type of tax benefits and the place-based structure of the subsidy, which naturally led to more projects being implemented in the more profitable areas. Thus, with this specific design, the policy seems to have extended the rich, unsubsidized area beyond the border, strengthening residential segregation in the process. Finally, the effect on house prices seems to operate via two channels. The first is linked to the direct effect that improving amenities has on local housing prices. The second can be attributed to changes in a neighborhood's income composition. The findings suggest a mild increase in both the population and the household income in neighborhoods with high exposure to subsidized investments. However, the effects on housing prices via this indirect channel tend to be moderate, at least in the short run. In the medium and long run, this mechanism is expected to play a more important role as the neighborhood income status changes. The new, higher income residents should increase the demand for (higher-price) services leading to gentrifying process that will revitalize these areas, albeit at the cost of further residential segregation. This gentrification process is likely to be more dynamic in neighborhoods with a higher share of renters, for which the results indicate a higher impact on housing prices due to the subsidized investments.

To sum up, this Thesis has taken a broad focus on spatial inequalities, paying particular attention to policies that seek to mitigate residential segregation, deconcentrate poverty, and revitalize deteriorated areas within cities. Three main lessons can be drawn from this research. First, for an optimal policy design, the causes of residential segregation need to be carefully studied. Otherwise, policymakers run the risk of implementing ineffective measures that do not have the intended results. Second, even when the root of the problem has been appropriately identified, designing policy strategies in accordance with that identification is no guarantee of isolating the intended goals. Third, place-based housing subsidies that target income-diverse areas but which do not impose requirements on buyers and tenants may increase residential segregation and potentially induce gentrification in high investment areas. Thus, a significant conclusion here is that there is room for policies that can improve social welfare. However, to achieve the goals set, it is crucial that the policy design takes into consideration the factors that cause neighborhood status to fall and that it takes careful account of the urban context.

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