Place-based subsidies and location decisions: The case of Uruguay

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Abstract

This paper uses differences-in-differences to analyse the impact of a program which grants place-based subsidies to residential construction (Ley de Vivienda de Interés Social) on the location of housing developments in the Uruguayan city of Montevideo using administrative municipal data over the period 2007-2014. The results reveal that the policy has a sizeable and statistically significant impact on the location of residential construction. The relative effect of the policy on treated areas is over 200% of the mean for square metres built and approximately 50% of the mean for the number of housing starts. Furthermore, the study shows that the policy increased the mean square metres of residential projects in treated zones. These results suggest that the program promoted a relocation process of residential construction towards the treated areas while simultaneously increasing the average size of housing projects in said areas.

Keywords: subsidies, location, housing market, Uruguay.

JEL codes: D04, H25, R31

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1 Introduction

The Social Interest Housing Promotion Law (LVIS, which stands for its Spanish name Ley de Promoción de la Vivienda de Interés Social) is one of the main low and middle-income housing public policies in Uruguay. Approximately 12,000 housing units have been subsidized by the LVIS regime since 2012, through tax credits to its builders and owners.

Public debate over this law in recent years has focused in determining whether the policy has improved the access of low and middle-income sectors to decent housing (Gilet and Rey, 2015). The fact that the price of subsidized units is not substantially different than those of similar non-subsidized units is a frequently criticized aspect of the LVIS. Critics point out that the final beneficiaries of the policy are not low and middle-income groups. Defenders of the law claim that the law bolstered employment and activity levels in the construction sector and that the policy’s effect on housing prices will occur in the long-run (Custodio, 2015).

Improving social integration while maximizing utilization of existing city infrastructure are also goals of the LVIS, according to the legislative text. However, this aspect of the policy has been somewhat neglected from public debate. Moreover, there are no academic studies that give evidence to support the claim that the law altered the previous location pattern of residential construction in Uruguayan cities.

LVIS subsidies are an example of place-based subsidies, since they give special treatment to geographic areas with certain characteristics so as to direct public and private resources towards them, often in order to mitigate spatial inequalities. A location-based policy can be thought of as a dual treatment, which creates regulatory asymmetries that give way to significantly different results between subsidized and non-subsidized areas (Kline and Moretti, 2013).

This paper analyses the impact of the LVIS on the location of residential housing construction in the city of Montevideo, Uruguay’s capital and most important city. In order to do so, the asymmetry between promoted and non-promoted areas is used to evaluate the market’s response to the introduction of tax credits. Suppose a situation where the policy increases the housing stock of treated neighborhoods but that said increase is composed exclusively of units that, in absence of the policy, would be built in non-treated areas. The aggregate housing

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1 The law’s complete text can be accessed here: https://parlamento.gub.uy/documentosyleyes/leyes/ley/18795

2 It is clear that this example assumes that housing demand is perfectly mobile. It is possible to imagine scenarios where there are moving costs between zones. Similarly, adjustment to subsidies could be processed through changes in the area of the housing units and not in the number of new units.
stock would remain constant, but on a neighbourhood level the results would be heterogeneous: the gains in treated areas would equal the losses of non-treated ones. This study quantifies the difference between the variations of the number of housing starts and of square metres built in treated and non-treated zones, as an approximation to the policy’s effect on residential construction location. However, it is not possible to determine the net effect of the LVIS on the aggregate housing stock in the city nor the program’s effect on the informal housing market.

Through the construction of an original dataset that allows to precisely georeference housing starts in Montevideo, this paper makes 3 relevant contributions. Firstly, it proposes a methodology to empirically evaluate the impact of the LVIS on residential construction. Secondly, it quantifies the size of the relocation between zones. I find that the law has a statistically significant and positive effect, that is over 200% of the mean for the total square metres built and of approximately 50% for the number of housing starts. Lastly, it shows that the policy altered the type of construction built in Montevideo, since housing projects in subsidized areas are larger (measured by square metres per project). These results indicate that the LVIS was effective in generating a relocation process of housing activity towards subsidized zones while increasing the average size of housing developments in said areas. This is, to the best of my knowledge, the first impact evaluation of the LVIS, so results are preliminary and require more robustness checks by future studies.

The paper is organised as follows. The section 2 summarizes the main aspects of the LVIS, describing the benefits that it grants and their access conditions. In 3, prior evidence found in existing literature is reviewed, with an emphasis on subsidies on housing supply. In 4, a brief conceptual model is outlined. The construction of the dataset is presented in 5, while the empirical strategy used for the impact evaluation is reviewed in 6. The results are presented in 7. Finally, in 8, the main findings are summarized.

2 The Social Interest Housing Promotion Law

In August of 2011, Uruguay’s Parliament enacted the LVIS. Social interest housing units (SIH) are dwellings that comply with specific requirements set by Uruguayan law, related to the dwellings’ square metres and their construction cost. The policy aims to i) increase the total stock of SIH units available for rent or for sale, ii) provide access to decent housing for low and middle income families, iii) maximize the use of existing infrastructure and iv) contribute to social cohesion.

The LVIS is a key component of a restructuring process in Uruguayan housing public policies that started in 2005. The pre-existing system consisted of pub-
lic construction programs of low-income housing -implemented by a diverse set of institutions- and public supply of mortgage funds for housing purchases. After the major banking crisis in 2002, which led to the bankruptcy of Uruguay’s largest mortgage bank (the state-owned Banco Hipotecario del Uruguay, BHU), the system collapsed (Casacuberta, 2006).

Reform started in 2005 which resulted in the creation of the National Housing Agency (Agencia Nacional de Vivienda, ANV) in 2007 and the recapitalization of the BHU. The new system assigned the management of public construction programs and risky mortgages to the ANV, while the BHU focused in supplying mortgage funds to middle and high-income sectors. After 2010, once the institutional structure was consolidated, the ANV sought to encourage private developers into building economic housing for low and middle-income groups.

The main incentive used to attract private investment has been the LVIS program. Basically, the policy grants a set of benefits to residential investment projects, which consist of tax credits on duties associated with the construction, sale and rent of SIH units. The policy can exonerate taxes that affect the construction costs (particularly Value Added Tax) but also those that affect real estate after its construction (namely income tax (IRPF), property tax (IP), inheritance tax (ITP) and corporate tax (IRAE)). The LVIS also seeks to impact on the demand side through the Warranty Fund for Mortgages (Fondo de Garantía de Créditos Hipotecarios, FGCH), which grants partial warranties for those who wish to acquire a SIH unit.

Source: Geographic Information System, Intendencia de Montevideo.

Figure 1: Geographic distribution of subsidized zones in Montevideo
Eligibility to receive tax benefits depends on i) the type of construction that is carried out (particularly, whether it is new construction or a reform of existing stock), ii) the project’s location, iii) the characteristics of each dwelling and, in some cases, iv) the selling prices of each SIH unit. Only urban construction can be subsidized, which excludes temporary residencies, particularly those that are built on holiday destinations. In Montevideo, the policy divides the urban area in 4 different zones; whilst all urban areas in the rest of the country constitute an homogeneous zone. The geographical distribution of subsidized zones in Montevideo is shown in figure 1. Specific criteria for promotion in each zone are summarized in table 1. Up to 2014, zones C01, C02 and C03 had no price ceilings, which were implemented partially in 2014. As a result, the final sale price of 25% of the total SIH units in residential projects that have more than 4 units cannot surpass the price ceilings established by the ANV.

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<th>New construction or reforms of existing stock</th>
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<td>Partial price control</td>
<td>Zones C01 and C02</td>
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Table 1: Zones and eligibility criteria of LVIS.

The LVIS does not subsidize construction in Montevideo’s coastline (zone C04) nor the urban periphery (zone C03). Zone C04 encompasses the most highly densely populated neighborhoods in the city, amounting to 24% of Montevideo’s total population, approximately (Intendencia de Montevideo, 2013). Zone C02 grants identical benefits than zone C01 for income derived of real-estate sales, but the treatment of rent income is slightly different. For all zones, technical requirements are established that must be fulfilled by the terrain, the project and the individual housing units.

In order to be subsidized, residential developments must contain between 2 and 100 housing units. The appraisal process starts with an application by developers to the ANV, something that can be done at any time of the year. Each investment proposal is assessed by ANV staff, who report to an advisory committee composed by representatives of the Ministry of Housing, Land Use Planning and Environment (Ministerio de Vivienda, Ordenamiento Territorial y Medio Am-

3These are linked, among other aspects, to the minimum and maximum square metres that each unit must possess, the maximum amount of units allowed for an individual project, the construction quality and the environmental sustainability of the building. A comprehensive description of the requirements can be accessed here: [http://anv.gub.uy/archivos/2014/06/ODI_RM.636.2014.pdf](http://anv.gub.uy/archivos/2014/06/ODI_RM.636.2014.pdf).
biente, MVOTMA) and the Ministry of Economics and Finance (Ministerio de Economía y Finanzas, MEF). The committee must recommend the approval or the rejection of each project, which is decided by the authorities of the MVOTMA. Throughout the construction phase, developers must abide to a set of deadlines scheduled in the investment project. Tolerance to limited delays is allowed, while schedule extensions must be approved by the advisory committee.

According to data from the ANV for December 2014, 305 investment projects have been approved totalling more than 12,000 SIH units. 72% of projects are currently in construction, while projects amounting to 747 SIH units have been completed. 94% of projects consist of new construction, 4% recycling and the remaining 2% are other types of reforms. Projects are highly concentrated in Montevideo, where 70% of the total SIH units are located, followed by Maldonado with 21% and 9% in the remaining 17 provinces of Uruguay. Furthermore, 70% of the SIH units of Montevideo are located in 9 central neighborhoods (ANV, 2015).

3 Previous research

Evidence from existing literature on place-based tax incentives is not conclusive (Mayer, Mayneris and Py, 2013; Wilder and Rubin, 1996). This section reviews two aspects of this type of subsidies: their direct effects on promoted zones and the relocation effects between promoted and non-promoted zones that they induce. For the specific case of residential housing markets, subsidies’ effects on the total housing stock on promoted areas, the spatial pattern of residential construction and possible externalities derived from it are presented. Discussion on the optimal geographical aggregation levels used for impact evaluation of place-based policies is also reviewed. Research examined in this section evaluates policies which, essentially, consist on tax credits that seek to attract investment and employment to regions that underperform economically (Lynch and Zax, 2011). The evidence that follows applies to the United States and European countries, since it was not possible to identify similar studies for Latin American countries.

Evidence on place-based subsidies’ effects on promoted regions is mixed. Most studies find no significant effect on employment levels, although findings for the USA vary between states (Neumark and Kolko, 2010; Lynch and Zax, 2011). Impact on activity levels, instead, appears to be positive. Nevertheless, causality is not clear since increased economic performance could be unrelated to the subsidies themselves, and may be attributed to the effect of subsidies as a signal of political prioritization of subsidized zones (Wilder and Rubin, 1996). Furthermore, it is not clear that the aforementioned effects are permanent, since promoted areas could revert to their previous state after the policy is over (Neumark and Simpson, 2014).
Empirical research for the United States finds that subsidies to low-income housing construction increase the total housing supply, although they seem to partially crowd-out private non-subsidized construction (Murray, 1983; Murray, 1999; Malpezzi and Vandell, 2002; Sinai and Waldfogel, 2005; Baum-Snow and Marion, 2009; Eriksen and Rosenthal, 2010). An important finding of this studies is that the amount of crowding-out varies according to the income levels of neighbourhoods, their urban characteristics and the final destination of the housing units -sale or rent- (Sinai and Waldfogel, 2005; Baum-Snow and Marion, 2009; Eriksen and Rosenthal, 2010). The final impact of place-based subsidies on the aggregate housing stock depends on the composition of the recipients of subsidized housing units. If the subsidized units are targeted to excluded consumers, the increased supply creates previous non-existent transactions since it does not compete with nor reduces demand for non-subsidized units. However, if recipients are not properly targeted, subsidized units could compete with and lower demand for non-subsidized units.

Subsidies seem to have an impact on the location of residential investments. Evaluating the Low Income Housing Tax Credit (LIHTC) for the USA, Baum-Snow and Marion (2009) show that zones which grant higher benefits receive higher levels of investment compared to other normally subsidized areas. Schwartz, Ellen, Voicu and Schill (2009) show that low-income subsidized housing projects have positive spillovers on the neighbourhoods where they are located. Schwartz et al. (2009) also point out the positive effects of building low-income housing in middle and high-income zones, since it could enable low-income families to access better quality public services than those that are available in their previous neighbourhoods.

Most empirical research on place-based subsidies focuses on the relative effects between zones rather than on the policy’s impact on the aggregate stock. As a stylized fact, place-based subsidies encourage relocation from non-promoted towards promoted areas. According to Busso, Gregory and Kline (2013), the American federal program Empowerment Zones was effective in raising employment and wages in subsidized regions. Since the cost of living did not increase in promoted zones, the authors conclude that the policy did not motivate workers’ migration towards them. However, the program’s impact on worker relocation could be higher when considering longer time spans. Hanson and Rohlin (2013), analysing Empowerment Zones impacts on non-promoted areas that are geographically close to promoted ones, find large, negative and statistically significant effects on the number of jobs and businesses; which could be associated with a relocation process of investment towards promoted zones.

\footnote{Externalities are associated to the fact that low-income families benefit from the rise in property values when subsidized projects are completed, since they reduce disamenities.}
Results are similar in European countries. Studies of the enterprise zones program in the United Kingdom (Neumark and Simpson, 2014) reveal that between 50% and 80% of the new investment on promoted zones consisted of relocating firms (firms already in activity in non-promoted zones that decided to move). Givord, Rathelot and Sillard (2013) find that higher activity and employment levels in Zones Franches Urbaines (ZFU) in France is mainly explained by a higher number of relocating firms rather than by the birth of new firms.

Findings depend on the chosen geographical aggregation level to capture the policies’ effects. An ideal aggregation level should be sufficiently small in order to ensure that the zone is homogeneous and sufficiently large in order to obtain a sizeable number of observations. Also, the set of geographical sub-units should also be large enough to cover all relocation decisions triggered by the policy. Suppose agents relocate towards promoted zones from regions that are not included in the control group. In that case, estimates would be biased and the program’s effect would be overestimated (Eriksen and Rosenthal, 2010; Sinai and Waldfogel, 2005).

Sinai and Waldfogel (2005) work with two aggregation levels: census places (roughly speaking, city-level) and metropolitan statistical areas (MSA). Baum-Snow and Marion (2009), instead, use census-tracts and 1 kilometre rings centred in census blocks. Eriksen and Rosenthal (2010) estimates are obtained through aggregation at MSA-level, county-level and 10-mile radius rings centred in the centre of the different counties. Neumark and Kolko (2010) compare results between subsidized and non-subsidized areas using rings of variable radius in order to capture possible spillovers. The convenience of each aggregation level seems to be critically associated with the objectives of the analysis. Nevertheless, the authors point out that estimating results using variable geographical aggregation levels is useful to study if policy spillovers exist.

The mixed evidence on place-based subsidies can be attributed to the diverse impact that tax incentives can have on firms’ location decisions. Wilder and Rubin (1996) point out that investors’ preferences are usually consistent and unrelated to fiscal treatment. Tax benefits are more relevant when all other factors are equal between two alternatives, operating more as a tie-breaker rather than a crucial factor for investors. Also, tax credits seem to have a larger impact on already established firms, particularly on large ones that are capital-intensive and which have an important tax burden.

The sensitivity of location to tax incentives can be evaluated indirectly through supply and demand price elasticities. Empirical estimates of housing demand price elasticities for the United States are usually between $-0.3$ and $-0.5$ for tenants and between $-0.5$ and $-0.99$ for home-owners, suggesting that housing
demand is relatively inelastic (Eriksen and Rosenthal, 2010). Results should be nuanced by the fact that price-elasticities tend to be sensitive to the specification of the chosen empirical model (Garabato y Ramada-Sarasola, 2011). Demand price elasticity for Uruguay has been estimated to be 0.372, lower than those found in advanced economies. Estimates of supply price-elasticities for the United States are consistently higher than 1, indicating that housing supply is strongly sensitive to variations in prices (Eriksen and Rosenthal, 2011).

The LVIS has not yet been studied nor evaluated. This is related to its relatively recent enactment and implementation (late 2011 and early 2012, respectively), which hinders economical analysis of the policy. Existing related studies for Uruguay are centred in studying the real estate market dynamics. Ferrer (2015), using administrative municipal data on housing starts for Montevideo, finds no significant relationship between construction activity and construction costs in Montevideo. Results change are heterogeneous between zones: whilst results for coastline neighbourhoods remain unaltered, construction costs are relevant for construction in non-coast neighbourhoods; which suggests the presence of segmentation in the residential market in Montevideo. Domínguez and Martínez (2014) identify that housing affordability has deteriorated despite the quantitative surplus of housing units -relative to the number of households- in Uruguay. Supply and demand structural mismatch in the Uruguayan housing market is also mentioned by Casacuberta and Gandelman (2006) as a result of imperfections and supply restrictions in the domestic mortgage market. Amarante and Caffera (2003) showed that neighbourhoods located on Montevideo’s coastline attracted investments to residential construction that was not justified by demographic trends, a fact that they link to the use of housing as a financial asset.

In summary, according to the reviewed research place-based policies seem to have an impact on investments’ location. Nevertheless, their aggregate effects are not clear and their influence on firms is heterogeneous. Moreover, Uruguayan residential market shows symptoms of market imperfections and structural mismatch between supply and demand.

4 Conceptual model

Housing supply is composed by the stock of housing units available for sale or rent at a given moment. Supply’s level is determined by the number of housing starts and the extent of housing decay. The number of housing starts in period $t$ is a function of the change between $t$ and $t-1$ of construction costs and housing prices. In presence of place-based subsidies, total supply can be

\footnote{The price and cost levels determine the housing stock by equalling supply and demand. However, since housing starts are variations of the aggregate housing stock; their determinants}
subdivided between subsidized supply (LVIS market) and non-subsidized supply (NLVIS market).

Supply’s response to subsidies is linked to its sensitivity to price variations, namely supply’s price-elasticity. Two determinants of housing supply’s price-elasticity are specially relevant. Strict regulations that create market rigidities - such as maximum height dispositions - on the one hand, can affect supply’s ability to react to price stimuli. Secondly, because changes in supply have an impact on factor markets, price-elasticity of supply is closely tied to productive factors’ price-elasticity. An increase in housing supply increases demand for labor, land and loanable funds. As long as some of those are non-reproducible or inelastically supplied goods, housing supply will fail to increase or will be associated with higher increases in prices; lowering housing supply’s price-elasticity.

The case of two specific factors is of particular interest: land and firms’ capabilities. Land, on the one hand, is a non-reproducible resource (Davis and Heathcote, 2007). In highly densely populated urban areas, land supply can be highly inelastic and increases in supply will be associated with significant rises in construction costs. Therefore, inelasticity of land’s supply can greatly reduce subsidies’ impact on housing supply. Firm’s capabilities can be thought of as the set of resources and strategies that developers possess in order to carry out their activities successfully. In small markets - such as Uruguay’s housing market -, firms with low capabilities can be rather prevalent. Said firms could be unable to cope with increasing production levels despite reduction in costs, reducing price-elasticity of supply.

Housing demand is composed by the number of households searching to purchase or rent dwellings at a given moment. According to the spatial equilibrium model (Glaeser and Gottlieb, 2008), households will decide to locate in those places that maximize their utility levels considering income, prices and amenities levels in each location. Assuming that agents can only work in the location where they live and that migration between locations can occur at zero costs, the model predicts that utility levels across locations will be equal and constant.

Demand’s sensitivity to price changes (demand price-elasticity) is fundamentally linked with characteristics of the mortgage supply and preferences for specific locations. If mortgages are inelastically supplied, lower housing prices can increase the financial cost of housing purchases (Murray, 1983). This assumes that the rise in interest rates - a result of higher housing demand - is not enough to increase the total stock of mortgage funds up to the level that satisfies the new mortgage demand.

are related to variations in prices and costs (Mayer and Somerville, 2000).
Location preferences are related, among other factors, to the extent of residential segregation in the city. In presence of perfect segregation, residents in one location will not be willing to migrate towards others despite any price and income asymmetries between the two. In other words, demand will be perfectly inelastic (Glaeser and Gottlieb, 2008). Although perfect segregation is rare in practice, agents frequently discriminate between regions. A reasonable assumption is that preferences for location are higher in zones with higher comfort and amenity levels, and that they are practically zero in depressed regions. Demand price-elasticities will be different in the two markets, since consumers will be less inclined to accept price increases in regions that are not pleasant and vice versa.

4.1 Partial equilibrium with place-based subsidies

A preliminary theoretical analysis of the LVIS can be represented as panel (b) in figure 2. Suppose a tax credit of magnitude $\Delta C$. Since tax credits reduce construction costs, residential developers will be willing to build more housing units for all price levels. As a consequence, the supply curve shifts to the right. Since taxes are usually ad valorem, the new supply curve is not parallel to the original one since the total amount of subsidies is increasing in $Q$. This increase in supply leads to the lowering of housing prices from $p^*_1$ to $p^*_2$. In the new equilibrium, housing stock in the LVIS is unequivocally higher since it increased from $q^*_1$ to $q^*_2$.

![Figure 2: Partial equilibrium of the housing market in presence of place-based subsidies.](a) NLVIS market (b) LVIS market)

However, place-based policies restrict their benefits to some geographical regions. Therefore, the previous analysis is only valid for subsidized areas. LVIS effects on non-subsidized supply are presented in panel (a) of figure 2. Assuming

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6 Analogous reasonings can be performed, such as technological improvements that are exclusive of firms operating in some zones.
that developers can migrate between zones at zero costs (perfect mobility between markets), tax credits in the LVIS market increase relative costs of firms operating in non-LVIS markets. This cost asymmetry between LVIS and NLVIS markets incentives migration towards subsidized areas where, ceteris paribus, profits will be higher. This leads to a price increase from $p_1^*$ up to $p_2^*$. In the new equilibrium, the housing stock in the NLVIS market is reduced from $q_2^*$ to $q_1^*$.

The exact amount of the final gains or losses ($\Delta Q^*$) in each market depends critically on price-elasticities of supply and demand. If demand is perfectly inelastic (the demand curve is vertical) as in figure A.1 in the Annex, the housing stock does not change and subsidized developers crowds-out non-subsidized developers in the LVIS market. With perfectly elastic demand or perfectly inelastic supply, increases in supply do not crowd-out non-subsidized construction and the housing stock in the LVIS market will be larger in the new equilibrium.

Graphic analysis so far implicitly assumes that there is no demand mobility between LVIS and NLVIS markets, since the demand curve $V^D$ does not shift in figures 2 and A.1. This assumption is somewhat unrealistic, since agents will take price variations into account for their location decisions. If we lift this assumption, price reduction in panel (b) of figure 2 attracts consumers from the NLVIS market to the LVIS market, which shifts the demand curve rightwards as shown in panel (b) of figure 3. The policy’s impact on prices and quantities in the new equilibrium depend on the magnitudes of demand and supply shifts. Assuming that the shock on supply is more intense, variations in NLVIS and LVIS markets are of equal sign compared to the case in figure 2 but of lower magnitude. If the shock on demand is more intense, variations would be of opposite sign. It is observed that demand mobility nuances the effect of the cost reduction on price levels, increasing the program’s impact on housing stocks in both markets.

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7 However, it is possible to imagine situations where mobility costs between markets are so high that migration and demand shifts are avoided.

8 The subsidy is depicted as a lump sum subsidy for graphical simplicity. Results are analogous for ad valorem subsidies.
The program’s aggregate effect on the housing market (both LVIS and NLVIS markets) is unclear. It is theoretically possible that the program’s effects on each market cancel themselves out if $-\Delta Q^{NLVIS} = \Delta Q^{LVIS}$. In that case, the aggregate housing stock is unchanged and the policy only redistributes prices and quantities between subsidized and non-subsidized markets. Nevertheless, if the increase in the LVIS market is higher than the decrease in the NLVIS market ($\Delta Q^{LVIS} > \Delta Q^{NLVIS}$) the program has a positive impact on the aggregate housing stock.

The determinants of the difference between $\Delta Q^{NLVIS}$ and $\Delta Q^{LVIS}$ are associated with the presence of different price-elasticities of supply and demand between both markets. If there are regulatory asymmetries between subsidized and non-subsidized zones, and supply in the LVIS market is more inelastic because of higher urban or land use regulations; place-based subsidies can compensate higher costs associated with regulations and correct pre-existing distortions in the residential construction sector (Kline and Moretti, 2013). However, if initial differences in costs between zones are large and subsidies are small, the policy can fail to alter relative costs between regions.

If firms have low entrepreneurial capabilities, subsidies are prone to operate inframarginally (they will benefit firms that were already present in the market). If developers are unable to develop more projects despite of being subsidized and no new firms enter the market, suppliers will only relocate projects -that would have taken place in absence of the policy- to subsidized zones where costs are lower. Similarly, relocation towards subsidized zones can impact on LVIS land markets. Higher land demand, specially in densely populated zones, could lead to land price surges, which could eventually counter the reduction on construction costs.

It is also possible that subsidies attract new firms to the market. According to press reports, foreign developers are investing in SIH (El País, 2015).
costs. In this scenario, the program’s effect on the LVIS market housing stock would be small and inefficient firms that cannot face higher land prices could be substituted for subsidized firms.

The existence of mechanisms for preferentially assigning subsidized supply is crucial for determining the program’s final effect on the aggregate housing stock. Substitutability between subsidized and non-subsidized housing units depends on the characteristics of each unit but also on the their access mechanisms. If subsidized housing units have to be assigned only to low-income families unable to afford current market prices, increases in LVIS supply won’t reduce NLVIS demand. If no assignment mechanisms are in place, competition for a “fixed” number of consumers will probably relocate consumers. Sinai and Waldfogel (2005) point out that place-based subsidies’ effects on the aggregate housing stock depends on whether the program affects agents that were already part of the housing market or if it affects previously excluded agents. In the latter case, the program creates new suppliers and new demandants simultaneously and the analysis in figure 2 is not valid. In the former case, however, the program’s effects are inframarginal because of competition between suppliers for a fixed number of consumers.

Finally, according to the spatial equilibrium model, a reduction in prices in one region increases the utility of their inhabitants which was equal to the utility of the rest of the economy. Hence, higher utility levels in the relatively cheaper zone would incentive migration towards it. Nevertheless, in presence of residential segregation demand will be relatively inelastic (Glaeser and Gottlieb, 2008). If significant differences between subsidized and non subsidized zones exist, consumers won’t relocate -as in figure 2- and the increase in the housing stock of LVIS zones will be lower.

5 Data

This paper combines data from multiple sources. The main dataset is composed by administrative data on approved construction starts collected by the government of Montevideo (Intendencia de Montevideo, IM) from 1997 up to November 2015.10 The database is available to the general public in the Open Data Catalogue of the Electronic Government and Information and Knowledge Society’s Agency (AGESIC, for its Spanish name Agencia de Gobierno Electrónico y Sociedad de la Información y del Conocimiento). In Uruguay, regulation and approval of construction activity is in charge of subnational governments. In Montevideo, information about the specific location, size (total square metres built),

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10 Data is updated monthly with new information on approved starts up to two months before the download date. More information can be accessed in the following URL [https://catalogodatos.gub.uy/dataset/permisos-de-construccion-aprobados](https://catalogodatos.gub.uy/dataset/permisos-de-construccion-aprobados)
purpose and type of every approved construction application is entered to the
dataset. Therefore, the data covers the entire universe of approved construction
starts in Montevideo. However, no data is available for construction activity that
takes place informally (without government approval). Through a listing of LVIS
subsidized projects made available by the ANV, all housing starts can be classified
as subsidized or non-subsidized.

Data is georreferenced using QGIS software through the parcel number (padrón
number) of each construction start\textsuperscript{11} using maps developed by the Geographical
Information System of the IM (SIG-IM) and the National Institute of Statistics
of Uruguay (INE, for its Spanish name Instituto Nacional de Estadística). As a
result, the precise location in the city of Montevideo of each construction start
in the IM dataset is identified, as well as information on the LVIS zone status,
neighbourhood, census section and census tract of each one. Since obtaining
approval is a prerequisite for starting construction activities in Uruguay, this
database contains information on all past and current projects, even those that
have not yet been completed.

Several caveats apply. Data is reported at a project level, so no information
on individual housing units can be obtained. Furthermore, INE publishes ag-
grerate series on the number of approved housing starts in Montevideo, which
are constructed using data reported by the IM. However, Ferrer (2015) identifies
discrepancies between series constructed with IM data and INE’s series, which
are also found in this study as shown in figure 4. Despite conversations with
IM statisticians, no conclusive response on the origin of the discrepancy was ob-
tained. Also, data on construction’s purpose can be inexact. Some LVIS subsi-
dized projects -clearly built with housing purposes- are listed with other purposes
in the IM dataset. According to IM statisticians, the listed purpose is defined by
administrative staff that receives construction applications and no standardized
protocol for purpose definition is in place. In that sense, projects with multi-
ple purposes\textsuperscript{12} could be either listed as “others” or assigned to non-residential
purposes. The extent of this classification error could be significant. Informa-
tion on LVIS subsidized housing starts can be used to approximate misreporting
in purposes, although misreporting patterns could be different in non subsidized
starts. Nevertheless, 143 LVIS subsidized housing starts (almost 30% of total
LVIS housing projects) appear to be listed with non-residential purposes.

\textsuperscript{11}The entire area of Uruguay is divided into parcels (padrones), which are the smallest geo-
graphical subdivisions of land in the country. In each department (subnational political units),
each land parcel possesses an unique identifying number (parcel number) that allows its precise
georreferenciation.

\textsuperscript{12}A typical case could be a high-rise residential building that has some commercial offices in
lower floors.
Because the focus of this study is new residential construction, only a specific set of construction starts is considered. Those consist of construction starts listed with housing purposes of a subset of construction types: new construction, reforms or extensions. Starts with commercial, industrial or other purposes and starts that are demolitions, parkings, storage, marquees, regularisations, foundations, awnings, incorporations to horizontal property and building site modifications are therefore deleted from the database.

<table>
<thead>
<tr>
<th>Parcels</th>
<th>Starts</th>
</tr>
</thead>
<tbody>
<tr>
<td>C01 zone</td>
<td>Mean: 598.6, Standard deviation: 5,198.6, Proportion: 57.0%</td>
</tr>
<tr>
<td>C02 zone</td>
<td>Mean: 327.6, Standard deviation: 504.7, Proportion: 2.37%</td>
</tr>
<tr>
<td>C03 zone</td>
<td>Mean: 2,024.6, Standard deviation: 28,863.5, Proportion: 15.88%</td>
</tr>
<tr>
<td>C04 zone</td>
<td>Mean: 573.7, Standard deviation: 4,012.6, Proportion: 17.65%</td>
</tr>
<tr>
<td>Rural zone</td>
<td>Mean: 30,884.8, Standard deviation: 161,292, Proportion: 7.1%</td>
</tr>
<tr>
<td>Total</td>
<td>Mean: 2,964.9, Standard deviation: 45,362.0, Proportion: 100%</td>
</tr>
</tbody>
</table>

Table 2: Total square metres of parcels and construction starts in Montevideo (1998-2014).

There are 12,819 observations on the final starts dataset. 33 parcel numbers are listed as subsidized by the ANV but do not appear in the SIG-IM’s parcel map nor the IM’s starts database. Therefore, said projects cannot be georreferenced and no information on their size is available. As shown in table 2, housing starts’ are geographically distributed in a notoriously different pattern than parcels. This indicates a strong concentration of construction activity, particularly on coastline zones (C04 zone) which possess only 17% of total parcels in Montevideo but attract more than 60% of total housing starts. An analysis of the average year of application of housing starts in each LVIS can show which areas of the city are attracting recent investments. In figure 5, the mean application year for housing
starts in each Centro Comunal Zonal (CCZ) -Montevideo’s political subdivisions- is presented. The figure suggests a possible shift in investments’ location pattern, since non-coastline central neighbourhoods begin to accumulate more housing starts in recent years.

![Maps showing housing starts distribution](image)

(a) 1998 - 2014  
(b) 1998 - 2003  
(c) 2004 - 2010  
(d) 2011 - 2014

Figure 5: Accumulated number of approved housing starts, by CCZ.

<table>
<thead>
<tr>
<th>Year</th>
<th>NLVIS</th>
<th>LVIS</th>
<th>Total</th>
<th>LVIS/Total</th>
<th>Number of starts</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>m²</td>
<td></td>
<td></td>
<td></td>
<td>NLVIS LVIS Total LVIS/Total</td>
</tr>
<tr>
<td>2011</td>
<td>321,587.2</td>
<td>92,806.9</td>
<td>414,394.1</td>
<td>22.4%</td>
<td>557 280 837 33.5%</td>
</tr>
<tr>
<td>2012</td>
<td>177,504.3</td>
<td>300,901.3</td>
<td>478,405.6</td>
<td>62.9%</td>
<td>442 286 728 39.3%</td>
</tr>
<tr>
<td>2013</td>
<td>145,415.6</td>
<td>187,649.4</td>
<td>333,065</td>
<td>56.3%</td>
<td>314 214 528 40.5%</td>
</tr>
<tr>
<td>2014</td>
<td>114,583.5</td>
<td>222,947.5</td>
<td>337531</td>
<td>66.1%</td>
<td>273 293 566 51.8%</td>
</tr>
</tbody>
</table>

Table 3: Total square metres built and number of approved starts, per year and LVIS zone.

Residential construction in LVIS subsidized zones has increased its importance in total residential construction in Montevideo, amounting to 66.1% of total square metres built in 2014. This rise is explained by an increase in individual projects’ size rather than by an increase in the number of projects in LVIS subsidized zones, since the proportion of starts located in promoted zones is lower.
than the proportion in total square metres. Figure 6 shows a noticeable change in the accumulated $m^2$ series by LVIS eligibility starting in 2012, with a possible anticipation effect in 2011. LVIS subsidized projects have been concentrated in central neighbourhoods in Montevideo which are near to coastline neighbourhoods. Nevertheless, neighbourhoods located farther away from coastline zones are increasingly attracting LVIS projects, as shown in figure 7b.

Figure 6: Accumulated $m^2$ and number of housing starts, by LVIS eligibility status.

(a) Number of LVIS projects, by neighbourhoods.  
(b) Mean starting year of LVIS projects, by neighbourhoods.

6 Empirical strategy

The goal of this study is to analyse the impact of the LVIS on the location of residential construction. The empirical strategy used directly estimates the
magnitude of the difference in variations on supply variables between subsidized and non-subsidized zones. As seen in section 4 this amounts to the difference between $\Delta Q_{LVIS}^*$ and $\Delta Q_{NLVIS}^*$ of figure 3. Although estimates of this difference will recover relocation effects of the policy, they will also capture higher (or lower) levels of activity caused by possible spillovers derived from the program’s incentives. Therefore, estimates will only approximate relocation effects.

Housing supply will be operationalized through two variables: $nperm_{s,t}$, the number of approved housing starts in geographical unit $s$ in period $t$ and $met_{s,t}$, the total square metres built in geographical unit $s$ in period $t$. The law’s impact on housing prices or quality is beyond the scope of this study, as is the program’s impact on the aggregate housing stock in Montevideo.

Ideally, the policy’s impact would be measurable through comparison between the same geographical units in different treatment states. In other words, the real values of $nperm$ and $met$ in subsidized units $s$ should be compared to their counterfactual: the values of $nperm$ and $met$ that would have been obtained in the same units $s$ if they hadn’t been promoted. The fundamental problem of impact evaluation is that units of analysis cannot be simultaneously treated and untreated, since the outcome in absence of the intervention is a theoretical abstraction that is never observed empirically. Hence, impact evaluation methodologies must create reasonable and convincing comparison groups that substitute the theoretical counterfactual (Khandker, Koolwal and Samal, 2010).

A crucial aspect for determining an appropriate comparison group is randomness in treatment assignment. Suppose all tax credits were granted randomly between all approved housing starts. In that case, creating a control group is
rather simple; since randomness will guarantee that no significant differences will exist between promoted and non-promoted projects. Therefore, any observed differences in outcomes between subsidized and non-subsidized projects can be attributed to the program’s effect. As seen in figure 8 and table 4, the case of the LVIS -as is the case in most public policies- is different since treatment was granted to specific parts of the city that are substantively different in observable variables than non-subsidized zones. This restricts methodological options for impact evaluations to quasi-experiments, which essentially consist on using natural situations that alter treatment status between otherwise similar units (Angrist and Pischke, 2009).

<table>
<thead>
<tr>
<th>CCZ NLVIS</th>
<th>CCZ LVIS</th>
</tr>
</thead>
<tbody>
<tr>
<td>Rental value</td>
<td>16.193,0</td>
</tr>
<tr>
<td>Percentage of poor households</td>
<td>0,95%</td>
</tr>
<tr>
<td>Mean income of households</td>
<td>81.766,2</td>
</tr>
<tr>
<td>Unemployment rate</td>
<td>1,72%</td>
</tr>
</tbody>
</table>

CCZ LVIS is composed by CCZ 1, 2 and 3 and CCZ NLVIS is composed by CCZ 5, 7 and 8.

Table 4: Socioeconomic indicators, by LVIS eligibility status(Household Survey Data, 2014).

Differences-in-differences (DiD) is used to select an appropriate control group. This methodology requires information on outcome variables in different time periods, in order to compare trends between groups before and after the treatment is implemented (Baum-Snow and Ferreira, 2015). DiD’s control groups are typically non-treated units that present similar pre-treatment trends than treated units. Assume that, previous to 2011, the evolution of nperm was similar between subsidized and non-subsidized zones but after 2011, evolution is different according to treatment status. Intuitively, any deviation from previous trends is attributable to the program (since trends were equal before its enactment). Therefore, under certain assumptions, variation between groups can be used to approximate the program’s impact.

The following model is based on Angrist and Pischke (2009). Housing supply variables on a geographical unit s in period t are assumed to depend on intrinsic characteristics of s (such as the level of amenities or proximity to urban centers), the existing macroeconomic conditions at time t and the amount of taxes to be paid (which is assumed equal between zones and, hence, only associated with its presence on a subsidized or non-subsidized zone). The data generating process can be written as follows:

\[
E(y_{s,t}|LVIS_s, I_t, A_s) = \theta + \lambda_t + \beta(LVIS_s \cdot I_t) + A_s \gamma
\]  

(1)
where $y$ is an outcome variable -$nperm$ or $met$-, $LVIS_s$ is a dummy variable that indicates if unit $s$ is eligible to receive LVIS tax credits or not and $I_t$ is a dummy variable that is only nonzero for post-treatment periods. $A_s$ is a vector of unobservable characteristics of $s$ that are time-invariant and $\gamma$ is a vector of coefficients that measure the sign and magnitude of the relationship between the dependent variable $y$ and unobservable factors. Finally, $\lambda_t$ is a time fixed effect. This parameter only depends on $t$ since it controls for changes in macroeconomic trends that affect all geographical units.

Assuming that only two time periods exist (0 is pre-treatment and 1 is post-treatment), substituting for $t = 1$ and $t = 0$ in (1) for each group (LVIS and NLVIS) and subtracting both results yields the following:

\[
\Delta Q^*_{LVIS} = E(y_{s,1}|LVIS = 1) - E(y_{s,0}|LVIS = 1) = \lambda_1 - \lambda_0 + \beta
\]
\[
\Delta Q^*_{NLVIS} = E(y_{s,1}|LVIS = 0) - E(y_{s,0}|LVIS = 0) = \lambda_1 - \lambda_0
\]

According to the model, variation of supply between periods only depends on macroeconomic conditions (common to all units) and on the presence of tax credits. Substracting $\Delta Q^*_{NLVIS}$ from $\Delta Q^*_{LVIS}$, the following is obtained:

\[
\beta = \Delta Q^*_{LVIS} - \Delta Q^*_{NLVIS}
\]  

(2)

Therefore, $\beta$ is the parameter of interest and it measures the relative effect of treatment between subsidized and non-subsidized zones which was analysed in figure 3. It is observed that the model assumes that causality is additive and constant, since

\[
E(y_{s,t}|LVIS = 1) = E(y_{s,t}|LVIS = 0) + \beta
\]

Results are sensitive to the chosen geographical aggregation level. Since data is georreferenced precisely to parcel location, it is possible to select any geographical aggregation level. There is a trade-off between reducing the size of the analysis unit $s$ for a better control on unobservables and increasing it in order to obtain more observations (housing starts) per unit.

Uruguay’s territory is divided in four geostatistical units: census departments, census sections, census tracts and census zones. Census departments match the political divisions of the country, while census sections are defined in reference with the judicial sections of Uruguay. Both levels comprise broad portions of the territory and, therefore, aggregate zones that are composed by units which are different on unobservables. Census tracts in urban Montevideo comprise a small set of blocks, while census zones consist of individual blocks. Census tracts conform
an intermediate level of geographical aggregation since they are formed by a num-
ber of blocks while maintaining a reduced geographical extension. While working
with census sections or higher levels of aggregation would imply losing heterogeneity,
working with census zones would gravely reduce the number of observations
per unit -possibly invalidating statistical inference-. Hence, geographical aggrega-
tion in this paper is done at a census tract level. This implies that, for instance,
\(nperm_{s,t}\) represents the number of approved housing starts in census tract \(s\) in
the year \(t\).

Equation 1 can be estimated through the following regression:

\[
y_{s,t} = \alpha_s + \lambda_t + \beta (LVIS_s \cdot I_t) + \varepsilon_{s,t}
\]  

where \(\alpha_s = \theta + A_s \gamma\) is a census tract fixed effect that controls for time-invariant
unobservables and \(\varepsilon_s\) is the error, which satisfies \(E(\varepsilon_{s,t}|LVIS_s, I_t) = 0\). Estimation
of equation 3 is done through ordinary least squares (OLS). Since correlation
may exist between outcomes in neighbouring census tracts, standard errors are
clustered to neighbourhood level (Bertrand et al., 2004).

6.1 Challenges to identification

Previous estimates are only consistent if the following identification condition
holds:

\[
[E(\varepsilon|LVIS = 1, t = 1) - E(\varepsilon|LVIS = 1, t = 0)] - [E(\varepsilon|LVIS = 0, t = 1) - E(\varepsilon|LVIS = 0, t = 0)] = 0
\]

which implies that any difference in time-variant unobservables between the treat-
ment group \((LVIS = 1)\) and the control group \((LVIS = 0)\) must be random
(Baum-Snow y Ferreira, 2015). Time-invariant differences in unobservables are
controlled by the inclusion of census-tract fixed effects in regression 3 If cor-
rrelation between \(\varepsilon\) and the treatment exists (if census tracts’ unobservables are
modified as a result of the policy), estimates are not consistent.

The selection criteria for choosing which zones were to be subsidized is not
clear. Therefore, it is possible that promoted areas may have been established
based on unobservable factors in the dataset used in this study. Given the recent
economic expansion experienced by Uruguay and its positive impact on households
mean incomes, it is possible that developers could anticipate that middle-income
groups’ housing demand would increase. This demand would probably be di-
rected to depressed central neighbourhoods or to non-urbanized suburban areas
in metropolitan Montevideo. Hence, developers could have had incentives to press
for subsidies -which lower construction costs- in zones where higher activity lev-
els could be anticipated. This is even more troublesome considering that private
developers were involved in the LVIS design process (Chamber of Senators, 2010;
Gilet and Rey, 2015). Furthermore, figure 4 suggests that the law is enacted at a time of moderation in construction activity levels. The aforementioned factors could result in a higher $\beta$ than its real value, since estimation could attribute to the subsidies effects that are caused by economic recovery or unobservable factors.

On the other hand, if developers know that the program will exist in the future they will expect that subsidized zones will attract investments. In order to secure lower land prices or more privileged locations, they could anticipate to the policy by acting before its enactment. This could bias estimates of $\beta$ downwards, since all activity that occurs before the LVIS enactment is not attributed to it (Blundell, Francesconi and van der Klaaw, 2011).

If census-tracts’ composition changes as a result of the implementation of the LVIS, econometric estimates could be seriously invalidated. Two scenarios are particularly plausible. On the one hand, tax credits could attract more productive developers that are different from pre-existing ones. On the other hand, construction of new housing units could alter the socioeconomic characteristics of subsidized zones inhabitants and, therefore, the expected profitability of housing projects located in them. Both factors could bias $\beta$ estimates upwards. Furthermore, if the program induces changes in census tracts time-invariant unobservable predictors of outcomes ($A_s$), the key identification assumption wouldn’t be satisfied and estimates wouldn’t be consistent.

Spillovers between treated and non-treated areas could also bias estimates. If higher construction levels in subsidized zones also attract commercial activity, neighbouring non-subsidized zones could become more attractive relative to non-subsidized ones located farther away. Since treatment is assumed to be a binary variable, estimates of $\beta$ would be biased downwards since they would not recover the effects of the treatment in non-treated areas. Similarly, since treatment is assumed to be additive and constant, estimates are not able to recover possible heterogeneity of effects between zones (possibly associated with agglomeration economies).

### 6.2 Construction of treatment and control groups

Since trends between all treated and non-treated census tracts in Montevideo are strikingly different, as seen in figure 8, analysis should be carried out considering a narrower set of census tracts. Greater comparability is expected to be achieved when considering census tracts which are geographically near to the border between subsidized and non-subsidized zones. Hence, buffer zones are constructed around the border in order to search for higher levels of comparability between subsidized and non-subsidized zones. Nevertheless, construction of those buffer zones depends on two assumptions. Firstly, since zone C02 only differs
from zone C01 in the treatment of rent profits, both zones are considered to be identical and are therefore grouped as the treated zones. Secondly, since a small amount of housing starts are located in zone C03, said zone is grouped with zone C04 as the non-treated zone. Hence, the only relevant border for buffer zones is the one separating C01 zone from C04 zone. From now on, the former will be referred to as LVIS zone and the latter as NLVIS zone.

Analysis is carried out in buffer zones with a radius of 500 metres and 1 kilometer from the border, excluding the northwestern border, since comparability between treated and non-treated zones in that part of the city is severely reduced. Buffer zones are shown in figure 9 and they are divided into two groups: north and west.

Figure 9: 500 and 1000 metres buffer zones in the northern and western borders.

In order to build a panel of census tracts with information on supply variables, the total amount of square metres built and the number of approved housing starts per census tract per year are calculated as the sum of all housing starts belonging to parcels located on each census tract in that year. The resulting panel contains information for all census tracts located on the buffer zones between the period 2007-2014.

A descriptive analysis shown in table 5 suggests the existence of a relocation process of residential construction towards LVIS zones starting in 2012. Also, heterogeneity of results is found between buffer zones of 500 and 1000 metres.

The limits of LVIS and NLVIS zones do not necessarily match with census tracts limits. A small set of census tracts located in both treated and non-treated zones exist. It is considered that a census tract belongs to the zone in which its geometrical centroid is located.

---

13 The limits of LVIS and NLVIS zones do not necessarily match with census tracts limits. A small set of census tracts located in both treated and non-treated zones exist. It is considered that a census tract belongs to the zone in which its geometrical centroid is located.
Table 5: Means and variations of dependent variables.

<table>
<thead>
<tr>
<th>Variable</th>
<th>LVIS</th>
<th>NLVIS</th>
<th>ΔLVIS–NLVIS</th>
</tr>
</thead>
<tbody>
<tr>
<td>$met_{t=0,500m}$</td>
<td>248.1</td>
<td>377.5</td>
<td>-129.4</td>
</tr>
<tr>
<td>$met_{t=1,500m}$</td>
<td>1,035.1</td>
<td>298.8</td>
<td>736.3</td>
</tr>
<tr>
<td>$\Delta_{(t=1)-(t=0)}$</td>
<td>787</td>
<td>-78.7</td>
<td>865.7</td>
</tr>
<tr>
<td>$nperm_{t=0,1000m}$</td>
<td>183.6</td>
<td>381.8</td>
<td>-198.2</td>
</tr>
<tr>
<td>$nperm_{t=1,1000m}$</td>
<td>858.2</td>
<td>253.5</td>
<td>604.7</td>
</tr>
<tr>
<td>$\Delta_{(t=1)-(t=0)}$</td>
<td>674.6</td>
<td>128.3</td>
<td>802.9</td>
</tr>
<tr>
<td>$nperm_{t=0,1000m}$</td>
<td>0.647</td>
<td>0.830</td>
<td>-0.183</td>
</tr>
<tr>
<td>$nperm_{t=1,1000m}$</td>
<td>1.027</td>
<td>0.727</td>
<td>0.3</td>
</tr>
<tr>
<td>$\Delta_{(t=1)-(t=0)}$</td>
<td>0.38</td>
<td>-0.103</td>
<td>0.483</td>
</tr>
</tbody>
</table>

Although the key identification assumption will be formally tested in section 7.1, it can be preliminary evaluated through graphical analysis of the dependent variables in pre and post-treatment periods. Parallel trends are more evident when considering the number of housing starts rather than on the total square metres built. Also, Figure 10 shows that treated and non-treated zones located around the northern border present little levels of comparability. Hence, all census tracts located on northern buffer zones are left out of the analysis.

Figure 10: Annual number of approved housing starts and total square metres built per year in census tracts located on 1 kilometer northern buffer zone, per LVIS eligibility status.

Western buffer zones, as shown in figures 11 and 12, present higher levels of

\[\text{Since the LVIS enactment took place in late 2011 and the policy’s implementation started in early 2012, the treatment’s starting year is set to be 2012.}\]
comparability on both types of buffer zones (500 and 1000 metres). When metres and number of housing starts are divided per type of construction, no significant improvements in parallel trends are obtained.

Figure 11: Annual number of approved housing starts and total square metres built per year in census tracts located on 500 meter western buffer zone, per LVIS eligibility status.

Figure 12: Annual number of approved housing starts and total square metres built per year in census tracts located on 1000 meter western buffer zone, per LVIS eligibility status.

7 Results

7.1 Main model

Table 6 presents the result of four different specifications of the fixed effects model presented in equation 3. Estimates seem to support the hypothesis that the policy encouraged a relocation process from NLVIS to LVIS zones. The effect,
measured by $\beta$, is positive and statistically significant in both variables (square metres or number of housing starts).

<table>
<thead>
<tr>
<th></th>
<th>(1) $met_{500m}$</th>
<th>(2) $met_{1000m}$</th>
<th>(3) $nperm_{500m}$</th>
<th>(4) $nperm_{1000m}$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Z</td>
<td>1,135***</td>
<td>1,079***</td>
<td>0.451**</td>
<td>0.713***</td>
</tr>
<tr>
<td></td>
<td>(343.5)</td>
<td>(269.5)</td>
<td>(0.154)</td>
<td>(0.188)</td>
</tr>
<tr>
<td>Constant</td>
<td>169.3</td>
<td>127.5</td>
<td>0.937***</td>
<td>0.894***</td>
</tr>
<tr>
<td></td>
<td>(106.3)</td>
<td>(100.0)</td>
<td>(0.128)</td>
<td>(0.0950)</td>
</tr>
<tr>
<td>Observations</td>
<td>632</td>
<td>1.128</td>
<td>632</td>
<td>1.128</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.058</td>
<td>0.050</td>
<td>0.022</td>
<td>0.028</td>
</tr>
<tr>
<td>Number of census tracts</td>
<td>79</td>
<td>141</td>
<td>79</td>
<td>141</td>
</tr>
<tr>
<td>Mean of dependent variable</td>
<td>606.13</td>
<td>513.75</td>
<td>0.928</td>
<td>0.846</td>
</tr>
<tr>
<td>Fixed effects</td>
<td>Census tract and year</td>
<td>Census tract and year</td>
<td>Census tract and year</td>
<td>Census tract and year</td>
</tr>
</tbody>
</table>

Clusterized robust standard errors in parentheses.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 6: Estimates of model (3) for 500m and 1km western buffer zones.

The program’s effect is sizeable in relative terms. The effect amounts to more than twice the mean square metres in the respective buffer zones and more than half the mean of the number of housing starts per census tract. According to the estimates, the LVIS has altered the equilibrium of the residential housing market in Montevideo. Nevertheless, the estimated coefficient does not recover the net effect of the program on the LVIS zone ($\Delta Q^*_{LVIS}$), but rather the difference between said effect and the effect of the program on NLVIS zones ($\Delta Q^*_{LVIS} - \Delta Q^*_{NLVIS}$). Assuming that the LVIS caused a reduction on housing supply in non-treated zones ($\Delta Q^*_{NLVIS} < 0$), the estimated $\beta$ in Equation 6 is higher than the net effect on LVIS zones. These estimates provide no elements in order to discriminate between pure relocation and new activity on promoted areas.
7.1.1 Enhanced model

By interacting the treatment variable with year dummies, it is possible to obtain a more flexible model that allows to formally test the identification assumption (Pischke, 2005). Furthermore, it allows to detect possible anticipation effects. This model can be estimated through the following regression:

\[
y_{s,t} = \gamma_s + \lambda_t + \sum_{j=-m}^{q} \beta_j \cdot D_{s,t}(t = k + j) + X_{s,t} \cdot \delta + \varepsilon_{s,t} \tag{4}
\]

where \( D_{s,t}(t = k + j) \) is an interaction variable between a year dummy \(-m\) years before the treatment (leads) and \(q\) years after it (lags) are included- and a dummy variable that is 1 if the census tract \(s\) is located on a promoted zone and 0 otherwise. \(k\) is the year when treatment switches. \(\beta_j\) is the coefficient associated with the \(j\)-th interaction term. If the identification assumption is valid, then \(\beta_j = 0 \ \forall j < 0\). All interactions between location and pre-treatment years shouldn’t have a statistically significant effect on outcome variables.

As shown in table 7, the null hypothesis that validates the identification strategy is not rejected; which indicates that estimates are robust. Nevertheless, the coefficient associated with the 2011 lead is statistically significant, which suggests the existence of a possible anticipation effect\textsuperscript{15}. This could imply that \(\beta\) could be underestimated for \(nperm\) in the model presented in table 6.

\textsuperscript{15}This is consistent with figures 11 and 12.
<table>
<thead>
<tr>
<th>Year</th>
<th>$\text{met}_{500m}$</th>
<th>$\text{met}_{1000m}$</th>
<th>$\text{nperm}_{500m}$</th>
<th>$\text{nperm}_{1000m}$</th>
</tr>
</thead>
<tbody>
<tr>
<td>2008#zLV</td>
<td>-189.4 (307.1)</td>
<td>-347.8 (222.5)</td>
<td>0.273 (0.322)</td>
<td>-0.0560 (0.0949)</td>
</tr>
<tr>
<td>2009#zLV</td>
<td>190.6 (196.5)</td>
<td>-3.228 (114.0)</td>
<td>0.0851 (0.165)</td>
<td>0.169 (0.249)</td>
</tr>
<tr>
<td>2010#zLV</td>
<td>-48.41 (291.2)</td>
<td>-215.2 (268.0)</td>
<td>0.146 (0.318)</td>
<td>-0 (0.211)</td>
</tr>
<tr>
<td>2011#zLV</td>
<td>-729.5 (482.0)</td>
<td>-617.5 (356.9)</td>
<td>0.527 (0.313)</td>
<td>0.719** (0.274)</td>
</tr>
<tr>
<td>2012#zLV</td>
<td>912.4** (328.3)</td>
<td>710.8** (283.1)</td>
<td>0.686* (0.310)</td>
<td>0.859** (0.317)</td>
</tr>
<tr>
<td>2013#zLV</td>
<td>630.1* (302.2)</td>
<td>811.7** (279.0)</td>
<td>0.511** (0.198)</td>
<td>0.841*** (0.197)</td>
</tr>
<tr>
<td>2014#zLV</td>
<td>1,396* (765.5)</td>
<td>1,004* (540.6)</td>
<td>0.773** (0.319)</td>
<td>0.940** (0.369)</td>
</tr>
<tr>
<td>Constant</td>
<td>169.3 (104.4)</td>
<td>127.5 (101.1)</td>
<td>0.937*** (0.126)</td>
<td>0.894*** (0.0894)</td>
</tr>
</tbody>
</table>

Observations: 632

Fixed effects: Census tract and year

Clusterized robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 7: Estimates of the enhanced model in 4 for 500m and 1km western buffer zones.

### 7.2 Heterogeneous effects by construction type

Previous estimates include four different types of construction. However, the program’s effect solely on new construction outcome variables is of interest since the LVIS seems to be more oriented to residential developers that build new housing projects rather than reform existing stock. Therefore, regression 3 is estimated but restricting the sample to housing starts that declare to be new construction. According to estimates presented in table 8, the program’s impact on new construction is of lower absolute value than estimates presented in table 6 but the relative magnitude of the new estimates are much higher. The LVIS relative effect on square metres is over twice the mean in the respective buffer zone and over half the mean for the number of housing starts.
<table>
<thead>
<tr>
<th></th>
<th>(1) met(_{500m})</th>
<th>(2) met(_{1000m})</th>
<th>(3) nperm(_{500m})</th>
<th>(4) nperm(_{1000m})</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Z</strong></td>
<td>1,117***</td>
<td>1,044***</td>
<td>0.363**</td>
<td>0.509***</td>
</tr>
<tr>
<td></td>
<td>(335.0)</td>
<td>(251.6)</td>
<td>(0.115)</td>
<td>(0.0952)</td>
</tr>
<tr>
<td><strong>Constant</strong></td>
<td>94.91</td>
<td>68.79</td>
<td>0.430***</td>
<td>0.411***</td>
</tr>
<tr>
<td></td>
<td>(106.7)</td>
<td>(96.16)</td>
<td>(0.0542)</td>
<td>(0.0588)</td>
</tr>
<tr>
<td><strong>Observations</strong></td>
<td>632</td>
<td>1.128</td>
<td>632</td>
<td>1.128</td>
</tr>
<tr>
<td><strong>R(^2)</strong></td>
<td>0.056</td>
<td>0.047</td>
<td>0.031</td>
<td>0.035</td>
</tr>
<tr>
<td><strong>Number</strong></td>
<td>79</td>
<td>141</td>
<td>79</td>
<td>141</td>
</tr>
<tr>
<td><strong>Mean of</strong></td>
<td>574.08</td>
<td>490.70</td>
<td>0.508</td>
<td>0.478</td>
</tr>
<tr>
<td><strong>dependent</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>variable</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Fixed effects</strong></td>
<td>Census tract and year</td>
<td>Census tract and year</td>
<td>Census tract and year</td>
<td>Census tract and year</td>
</tr>
</tbody>
</table>

Clustered robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 8: Estimates of model 6 for 500m and 1km western buffer zones, sample restricted to new construction housing starts.

### 7.3 Scale effect

![Graph](image1.png)

(a) 500 metres western buffer       (b) 1 kilometre western buffer

Figure 13: Average square metres built per project.
According to the previous estimates, the LVIS was effective in incrementing the level of construction activity on the LVIS zones. However, it also of interest to analyse if the program was able to alter the characteristics of the new projects built on LVIS zones. A priori, it is possible to think that tax incentives will be more attractive to large investors, so it is highly likely that the subsidies will be somewhat oriented to the promotion of large scale construction projects.

To empirically test that hypothesis, I build an average square metres per project variable \( \text{avmet}_{s,t} \), which is defined as the quotient between \( \text{met}_{s,t} \) and \( n_{perm}_{s,t} \). This new variable is an approximation to the mean size of residential buildings. Its time trend is presented on figure 13, where it is possible to observe similar trends on pre-treatment years except for some outliers in 2001 and 2011. Results of the test of the identification assumption of 4 for the variable \( \text{avmet} \) validate the model and are presented in table A1.

<table>
<thead>
<tr>
<th></th>
<th>(1) ( \text{avmet}_{500m} )</th>
<th>(2) ( \text{avmet}_{1000m} )</th>
</tr>
</thead>
<tbody>
<tr>
<td>Z</td>
<td>540.2***</td>
<td>542.4***</td>
</tr>
<tr>
<td></td>
<td>(161.4)</td>
<td>(97.40)</td>
</tr>
<tr>
<td>Constante</td>
<td>102.2*</td>
<td>71.90*</td>
</tr>
<tr>
<td></td>
<td>(55.77)</td>
<td>(40.86)</td>
</tr>
<tr>
<td>Observations</td>
<td>632</td>
<td>1,128</td>
</tr>
<tr>
<td>( R^2 )</td>
<td>0.053</td>
<td>0.041</td>
</tr>
<tr>
<td>Number of census tracts</td>
<td>79</td>
<td>141</td>
</tr>
<tr>
<td>Mean of dependent variable</td>
<td>343.33</td>
<td>292.86</td>
</tr>
</tbody>
</table>

Fixed effects: Census tract and year Census tract and year
Clusterized robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 9: Estimates of model 6 for 500m and 1km western buffer zones.

According to table 9, the policy had a sizeable and significant effect on the average size of residential projects on LVIS zones. The mean square metres per project increased more than 100%. Results suggest that in addition to the program’s quantitative impact (more housing units are built on LVIS zones relative to NLVIS zones), the policy had a qualitative impact since new housing units tend to belong to larger housing projects.
Considering that estimates coefficients for \( met \) are larger relative to the mean-than for \( nperm \) in \( e \), the impact seems to be stronger on the intensive margin rather than on the extensive one. In other words, the program seems to have encouraged residential developers to build larger projects rather than building more individual projects of the same size as before. This could be associated with the fact that tax credits can be more appealing to developers that had already decided to invest in residential construction (and, after the tax credit, are motivated to increase their projects’ size), while the policy’s impact on the decision to start a new project seems to be lower.

8 Final remarks

This paper makes three relevant contributions to existing knowledge of the LVIS program. Firstly, a methodology for impact evaluation is proposed. Since no previous literature that evaluates this policy exists, this paper seeks to encourage further research. Secondly, the program’s relative impact is quantified precisely. A large and statistically significant effect is found, that amounts to more than 200% of the mean for square metres built and over 50% for the number of housing starts on LVIS zones relative to non-promoted areas. Finally, I find that the policy altered the characteristics of new residential projects on LVIS zones since the average size of new construction on subsidized areas seems to have increased. Estimates are robust and suggest that the LVIS encouraged a relocation process of residential construction towards subsidized areas, quantitatively and qualitatively.

Nevertheless, results are preliminary and require further robustness checks. The data seems to have some problems and it does not cover the informal construction sector in Montevideo. This is a relevant issue, since formal residential construction in Montevideo is only part of all residential construction activity (Ferrer, 2015).

Also, since estimates are obtained for geographical areas that have a relatively modest extension the external validity of the model is unclear. Therefore, to extrapolate the findings of this study to the entire city of Montevideo can be problematic\(^\text{16}\).

This paper does not measure the effect of the program on the total housing stock in Montevideo. Therefore, a pending issue is to measure precisely what happened with \( \Delta Q_{LVIS}^* \) and \( \Delta Q_{NLVIS}^* \) individually, in order to calculate which proportion of the relative effect is relocation from NLVIS areas and which propor-

\(^{16}\)Caution is advised, considering that the Montevidean housing market is highly segmented and divergent trends between treated and non-treated zones can be found in other parts of the city as seen in section 5.
tion is a net addition to the aggregate housing stock. Determining this magnitude is crucial in order to design hypothesis on the program’s impact on housing prices in Montevideo. Similarly, possible spillover effects of the program on non-treated areas are not studied in this paper and are an interesting topic for further research, although differences in estimated $\beta$ between ring-levels could be indicating the presence of positive spillovers on non-subsidized zones. Furthermore, the existence of price ceilings on LVIS housing units in the rest of Uruguay (and their recent implementation in Montevideo) can be useful to explore the effects of price regulation in housing.

Results suggest that the LVIS changed the geographical center of residential construction in Montevideo. While the effect of the policy on the socio-economic status of subsidized zones is not analysed in this paper, it remains a key dimension of the program’s evaluation. Higher activity levels on depressed neighbourhoods could attract higher income families to them, which could have possible spillover effects if higher income groups interact with lower income ones or, in absence of interaction, if higher income groups press for improvements on local public goods that can be enjoyed by lower income groups as well (Katzman, 2001). Also, higher demand for land in depressed neighbourhoods can benefit low-income landowners. The process could also have negative consequences. If demand for land raises housing prices on depressed neighbourhoods, low-income families could face severe problems and may even be forced to relocate on cheaper areas of the city (Harvey, 2008). Also, LVIS construction has been concentrated in central neighbourhoods that are mainly middle-class. Since these zones are not the most impoverished ones in Montevideo, the final distributive effect is far from clear.
References


A Annex

Figure A.1: Crowd-out effect with a perfectly inelastic demand.

Figure A.2: Annual number of approved housing starts and total square metres built, by LVIS eligibility status. 1km western buffer zone.
<table>
<thead>
<tr>
<th>Year</th>
<th>$avmet_{500m}$</th>
<th>$avmet_{1000m}$</th>
</tr>
</thead>
<tbody>
<tr>
<td>2008#zLV</td>
<td>-73.48 (138.9)</td>
<td>-55.43 (94.17)</td>
</tr>
<tr>
<td>2009#zLV</td>
<td>231.5* (108.4)</td>
<td>3.765 (123.4)</td>
</tr>
<tr>
<td>2010#zLV</td>
<td>220.5 (213.4)</td>
<td>72.83 (149.8)</td>
</tr>
<tr>
<td>2011#zLV</td>
<td>-718.4 (411.6)</td>
<td>-593.3* (292.1)</td>
</tr>
<tr>
<td>2012#zLV</td>
<td>514.7*** (160.9)</td>
<td>503.8*** (99.85)</td>
</tr>
<tr>
<td>2013#zLV</td>
<td>189.4* (101.8)</td>
<td>406.6*** (130.2)</td>
</tr>
<tr>
<td>2014#zLV</td>
<td>712.7 (454.1)</td>
<td>373.7* (197.7)</td>
</tr>
<tr>
<td>Constant</td>
<td>102.2* (54.86)</td>
<td>71.90* (39.62)</td>
</tr>
</tbody>
</table>

Observations: 632 1,128

$R^2$: 0.077 0.053

Number of census tracts: 79 141

Fixed effects: Census tract and year

Clustered robust standard errors in parentheses.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A1: Identification assumption test for variable $avmet$. 

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