PLACE-BASED SUBSIDIES AND LOCATION DECISIONS: THE CASE OF URUGUAY

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ABSTRACT

This paper combines a spatial discontinuity design with differences-in-differences to evaluate the effects of a program which grants place-based subsidies to residential construction on the location of housing developments in Montevideo, the Uruguayan capital, using administrative municipal data over the period 2007-2015. The results reveal that the policy has a sizeable and statistically significant impact on the location of residential construction. Also, findings indicate that the policy increases the average size of residential projects. The policy's impact on the densification of the city, however, is heterogeneous. Finally, the evidence on spillovers on non-subsidized zones is mixed.

Keywords: *subsidies, location, housing market, Uruguay.*
JEL codes: D04, H25, R31

1. Introduction

I estimate the relocation effects of a program which grants place-based subsidies to residential construction. The program of interest is the Social Interest Housing Promotion Law (LVIS, which stands for its Spanish name *Ley de Promoción de la Vivienda de Interés Social*), which has subsidized more than 12,000 housing units since 2012, through tax credits to builders and owners. Analysing relocation effects seeks to shed light on whether the policy achieved two of its stated purposes: improving social integration and maximizing utilization of existing city infrastructure[1]. This aspect of the policy has been somewhat neglected from public debate, which has been focused in determining whether the policy has improved the access of low and middle-income sectors to decent housing. 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.

This paper also contributes to the literature on place-based subsidies. These consist of subsidies limited to specific geographic areas which usually seek to direct public and private resources towards them, often in order to mitigate spatial inequalities. Location-based policies are economically interesting because they can create regulatory asymmetries that give way to significantly different results between subsidized and non-subsidized areas (Kline and Moretti, 2013).

The empirical strategy exploits a spatial discontinuity generated by the treatment's border combined with a difference-in-differences methodology to recover the causal effect of the policy, using an original dataset that allows to precisely georeference all housing starts in

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

Montevideo for the period 2007-2015. This paper makes four relevant contributions to the existing knowledge of the LVIS program. The first estimates of the LVIS program's relative effect on treated zones indicate the existence of a positive, large and statistically significant effect, which is over 130% of the mean for the case of square metres and over 60% for the number of housing starts. Estimates are robust and their magnitude reflects an intense relocation process of residential construction from non-subsidized zones to subsidized ones. In addition, the policy seems to have increased the size of residential projects, measured by the average square metres per housing start. The effect on the densification of the city, however, is heterogeneous: while zones nearer to the treatment's border seem attract more high-rise buildings, zones that are farther away do not exhibit a similar pattern. Finally, the policy seems to have some positive spillovers on residential construction on nearby non-subsidized areas but no evidence of spillovers on commercial activity is found.

The paper is structured as follows. Section 2 presents the program's characteristics and benefits. Section 3 reviews the existing literature. Section 4 presents the model and the identification strategy, while section 5 presents the data and the construction of the treatment and control groups. Results are presented in section 6 and section 7 concludes.

2. The Social Interest Housing Promotion Law

In August of 2011, Uruguay's Parliament enacted the LVIS. The policy aims to *i)* increase the total stock of social interest housing (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 public 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 and 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. 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. SIH units are dwellings that comply with specific requirements set by Uruguayan law, related to the dwellings' square metres and their construction cost. 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.

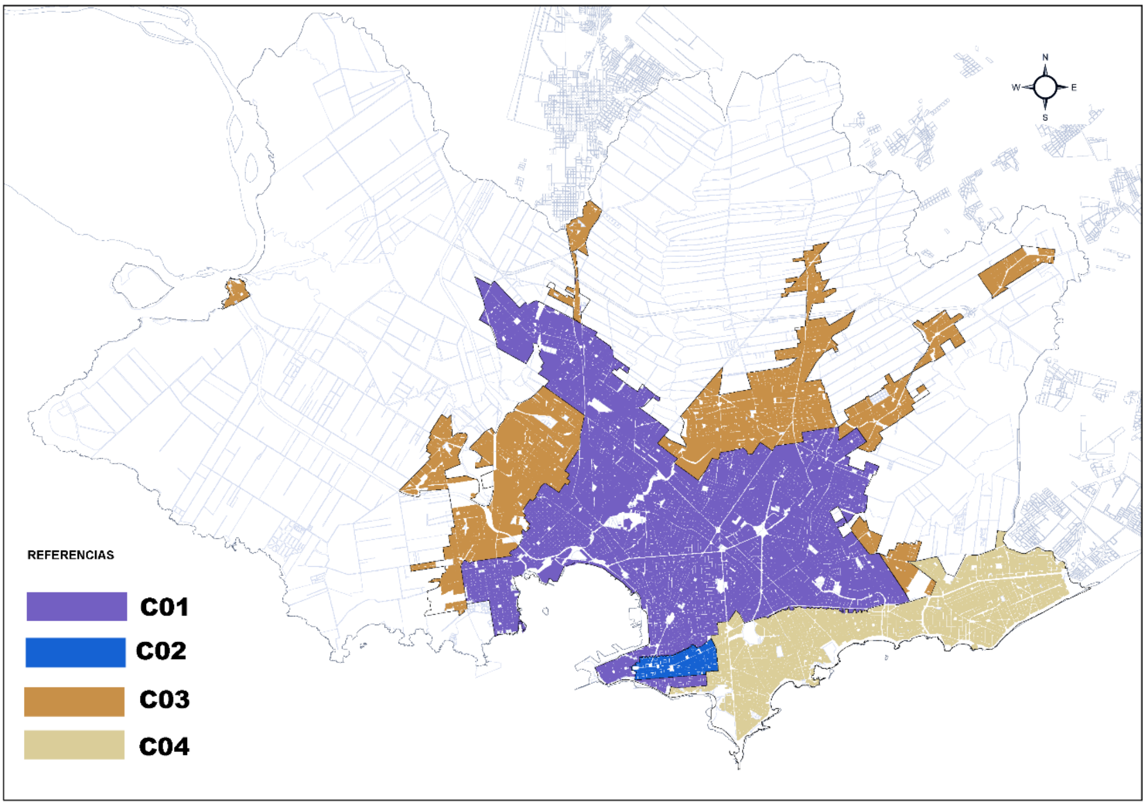


Figure 1. Subsidized and non-subsidized zones in Montevideo.

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.

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 neighbourhoods 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[2] that must be fulfilled by the terrain, the project and the individual housing units. Up to 2014, zones C01, C02 and C03 had

[2] These 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.

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.

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 Ambiente*, 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 July 2016, 441 investment projects have been approved amounting to more than 14,000 SIH units in the entire country. 212 (48%) projects have already been completed, while 172 (42%) are currently in construction. SIH construction is highly concentrated in Montevideo, where 70% of the total subsidized units are located, followed by Maldonado with 21% and 9% in the remaining 17 provinces of Uruguay. In Montevideo, completed projects have finished the construction of 2,398 units. 70% of the SIH units of Montevideo are located in 9 central neighbourhoods (ANV, 2015).

3. Related literature

Place-based policies are commonly used instruments to attract capital and labour so as to encourage development in underperforming areas. 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 place-based subsidies to residential construction has found that they 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). 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.

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. Billings (2009) finds no significant effect on firms' location decisions. Busso, Gregory and Kline (2013) find that 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. 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 (those 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.

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.

Schwartz, Ellen, Voicu and Schill (2009) show that low-income subsidized housing projects in New York have positive spillovers on the neighbourhoods where they are located. These relate to the fact that low-income families benefit from the rise in property values when subsidized projects are completed, since they reduce disamenities. Diamond and McQuade (2015) find similar results considering a larger number of US states.

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 are heterogeneous between zones: whilst results for coastline neighbourhoods remain unaltered, construction costs are relevant for construction in non-coastal 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. The model

Suppose that all locations in a city can be ordered on a real line that varies between – 1 and 1. This city is composed of two regulatory areas: the subsidized area (*S*) and the non-subsidized one (*N*). Area S consists of all $i \in [-1,0]$ and area *N* consists of all $i \in [0,1]$. Assume that residential construction depends on time-invariant unobservables that are specific to each i (such as the level of amenities or proximity to urban centres), time-varying factors that are common to all $i \in [-1,1]$ (the existing macroeconomic conditions at time *t*), time-varying unobservables specific to each x and construction costs that are homogenous in i. Suppose that the LVIS subsidies affect construction costs, reducing them by *z*. Therefore, construction costs in area S will be *c – z*. The data generating process can be written as follows

$$y_{i,t} = \gamma_i + \theta_t + \beta(zLVIS_i * P_t) + u_{i,t}$$

where $zLVIS_i$ is a dummy variable that indicates if location *i* belongs to area *S*, and P_t is a dummy variable that indicates if the year *t* belongs to the post-treatment period. The interaction between those dummies will indicate the difference in construction costs between areas S and *N*. If $E(u_{i,t}) = 0$ and assuming only two time periods, then $\beta = \Delta y_S - \Delta y_N$, where $\Delta y_S = y_{i,1} - y_{i,0}$ for locations in subsidized areas and $\Delta y_S = y_{i,1} - y_{i,0}$ for locations in non-subsidized areas. This coefficient can be easily estimated using ordinary least squares as a fixed effects model, including both location-specific (γ_i) and year (θ_t) fixed effects. Using that specification, β is a difference-in-differences estimator that recovers the relative effect of the policy between areas *S* and *N*.

4.1. Identification

Estimates of β are only consistent if any differences in time-varying unobservables between the treated and the control group are random (Baum-Snow and Ferreira, 2015). More formally, the following identification condition must hold:

$$[E(u|zLVIS = 1, P = 1) - E(u|zLVIS = 1, P = 0)] - [E(u|zLVIS = 0, P = 1) - E(u|zLVIS = 0, P = 0)] = 0$$

The key identification assumption is that time-variant unobservables won't differ between locations that are located near the treatment's border. In other words, if the sample is restricted to those $i \in [-\epsilon, \epsilon]$, where ϵ is a distance from the border[3], the identification assumption holds. Exploiting spatial discontinuities for impact evaluation is common in the literature (Holmes, 1998; Billings, 2009; Turner, 2014). The basic intuition is that areas which

[3] It is clear that $\epsilon>0$ indicates non-treated locations and $\epsilon<0$ indicates treated locations.

are near to each other will usually be similar; a stylized fact in geography known as Tobler's law. Therefore, although treated and non-treated areas can be quite different, this difference will be minimal near the treatment's border. Evidence to support this assumption will be given through the comparison of trends between groups before the treatment is implemented (Baum-Snow and Ferreira, 2015). If non-treated units present similar pre-treatment trends than treated units, any deviation from previous trends is attributable to the program (since trends were equal before its enactment).

Nevertheless, the identification strategy could fail. For instance, 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 β downwards, since all activity that occurs before the policy's enactment won't be attributed to it (Blundell, Francesconi and van der Klaaw, 2011). Nevertheless, this is less likely when considering the level of residential construction, since investors won't start construction projects before the reception of subsidies.

A more serious concern is that, as a result of the implementation of the LVIS, unobservables could change because of migration into subsidized zones. 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 β estimates upwards. Nevertheless, if these changes occur because of the policy, then they are a part of the causal effect of the policy. In other words, although β will cease to measure the specific causal effect of the tax break in this scenario, it will continue measuring the policy's impact as a whole.

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 nonsubsidized ones located farther away. Since treatment is assumed to be a binary variable, estimates of β would be biased downwards since they would not recover treatment effects on non-subsidized 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). Estimates will be obtained using different magnitudes of ϵ to account for possible heterogeneities.

4.2. Geographical aggregation

Findings depend on the geographical aggregation level that defines a location. 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. Billings (2009) uses buffers of 0,8 kilometers, while Turner, Haughwout and van der Klaaw (2014) consider distances from 100 km to 0,1 km. The convenience of each aggregation level seems to be critically associated with the objectives of the analysis. Nevertheless, the authors point out that using variable geographical aggregation levels is useful to study if policy spillovers exist.

Since data allows to precisely locate all construction activity in Montevideo, it is possible to select any geographical aggregation level. There is a trade-off between reducing the size of each location for a better control on unobservables and increasing it in order to obtain more observations 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 number 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, throughout the paper location i refers to a specific census tract i in Montevideo.

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 June 2016[4]. 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), purpose and type of every approved construction application is entered to the dataset. Therefore, the

[4] 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.

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 effectively subsidized or non-subsidized.

Data is georreferenced using QGIS software through the parcel number (*padrón* number) of each construction start[5], 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 of each construction start in the 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 aggregate 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 it can be seen in figure A1 in the Annex. Despite conversations with IM statisticians, no conclusive response on the origin of the discrepancy was obtained.

Also, data on construction's purpose can be inexact. Some LVIS subsidized 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 multiple purposes[6] could be either listed as “others" or assigned to non-residential purposes. The extent of this classification error could be significant. Information on LVIS subsidized housing starts can be used to approximate misreporting in purposes, although misreporting patterns could be different in non subsidized starts. Nevertheless, 139 LVIS subsidized housing starts (almost 31% of total LVIS housing projects) appear to be listed with non-residential purposes.

Housing supply will be operationalized through two variables: $nperm_{i,t}$, the number of approved housing starts in census tract *i* in year *t* and $met_{i,t}$, the total square metres built in census tract *i* in year *t*. Also, 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, recyclings and 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.

[5] The entire area of Uruguay is divided into parcels (padrones), which are the smallest geographical subdivisions of land in the country. In each department (subnational political units), each land parcel possesses a unique identifying number (parcel number) that allows its precise georreferenciation.
[6] A typical case could be a high-rise residential building that has some commercial offices in lower floors.

Table 1. Total square metres of parcels and housing starts, by LVIS zone

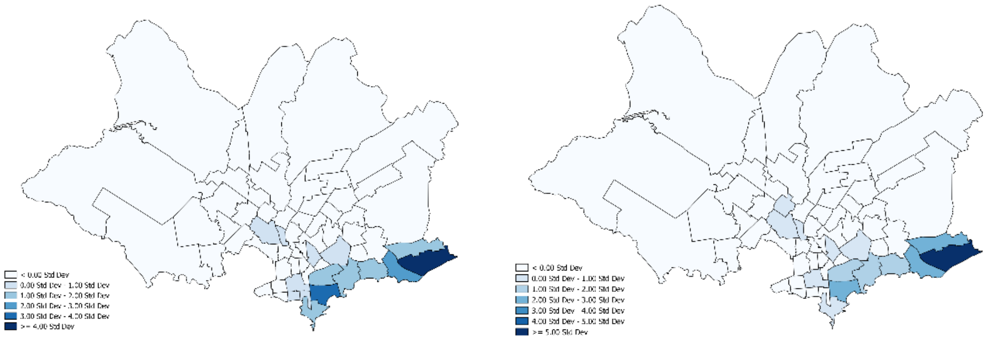
	Parcels			Housing starts		
Zone	**Mean**	**Standard deviation**	**Proportion**	**Mean**	**Standard deviation**	**Proportion**
C01	547,7	4.040,0	57,3%	380,3	1.856,6	29,1%
C02	327,2	502,2	2,4%	807,3	1.576,4	3,3%
C03	1.415,7	19.470,9	15,9%	679,6	1.396,6	1,5%
C04	554,1	3.709,1	17,8%	310,5	1.343,8	64,2%
Rural	23.494,6	91.857,4	6,6%	364,5	450,6	1,9%

After deletion and further cleaning, the final dataset is reduced to 12.893 housing starts since December 1997 up to December 2015. 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.

Table 2. Total square metres built and number of housing starts, per year and treatment status.

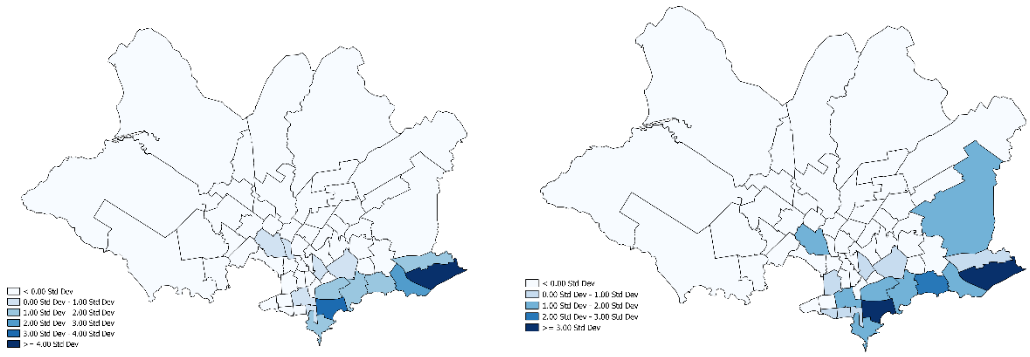
	Square metres				*Number of housing starts*			
Año	**NLVIS**	**ZLVIS**	**TOTAL**	**LVIS/Total**	**NLVIS**	**LVIS**	**TOTAL**	**LVIS/Total**
2011	321.587,2	90.275,01	411.862,2	21,9%	557	279	836	33,4%
2012	177.545,9	303.323,5	480.869,4	63,1%	442	285	727	39,2%
2013	145.679,8	187.649,4	333.329,2	56,3%	314	214	528	40,5%
2014	114.395,5	225.015,9	339.411,4	66,3%	271	293	564	52,0%
2015	87.268,3	181.825,5	269.093,8	67,6%	267	231	498	46,4%

Construction in subsidized areas has risen dramatically, albeit this increase has been focused on the intensive margin (larger buildings) rather than on the extensive margin (more individual buildings) as shown in table 2. Residential construction has historically been concentrated in coastal neighbourhoods. As seen on figure 2, this pattern has begun to change in recent years because of higher construction levels on neighbourhoods located in centric and non-coastal neighbourhoods; where most of the subsidized projects are located (figure A2).



(a) 1998 – 2015

(b) 1998 – 2003



(c) 2004 – 2011

(d) 2012 – 2015

Figure 2. Number of housing starts by neighbourhood.

5.1. Construction of treatment and control groups

In order to construct the treatment and control groups it is necessary to determine the magnitude of ϵ, the distance from the border for which the identification assumption holds. Since theory does not guide this choice, this decision is arbitrary. In this paper, two different magnitudes of ϵ, 500 and 1000 metres, will be used. Also, this study groups the four zones defined by the LVIS into two different treatment zones. C01 and C02 zones form the subsidized or LVIS zone, whilst C03 and C04 form the non-subsidized or NLVIS zone. Therefore, borders between C01 and C02 or between C03 and C04 are not relevant. Finally, since comparability is greatly reduced in the north-easternmost section of the border, this part is also excluded. The border is divided into two segments: the north border and the west border.

In order to select all census tracts that are located within 500 or 1000 metres from the border, buffer zones are constructed as seen on figure 3. A parcel belongs to the buffer zone if its geographical centroid belongs to the buffer zone. Since the buffer zones intersect some census tract, the following criteria is adopted: a census tract is located within a buffer zone if 50% or more of the parcels which compose it are located within said buffer zone. A similar situation occurs when considering if a specific census tract is located in a subsidized zone. In that case, it is considered treated if 50% or more of the parcels which compose it are located within the subsidized zone. These intersecting census tracts are less than 5% for the buffer zones and less than 8% for the treatment zones. Analysis considering only those census tracts that are fully located within both areas is carried out as a robustness check. As seen on figure 3, some sections of the northern border overlap with the western border. Census tracts located in these overlapping buffers are excluded from the analysis.

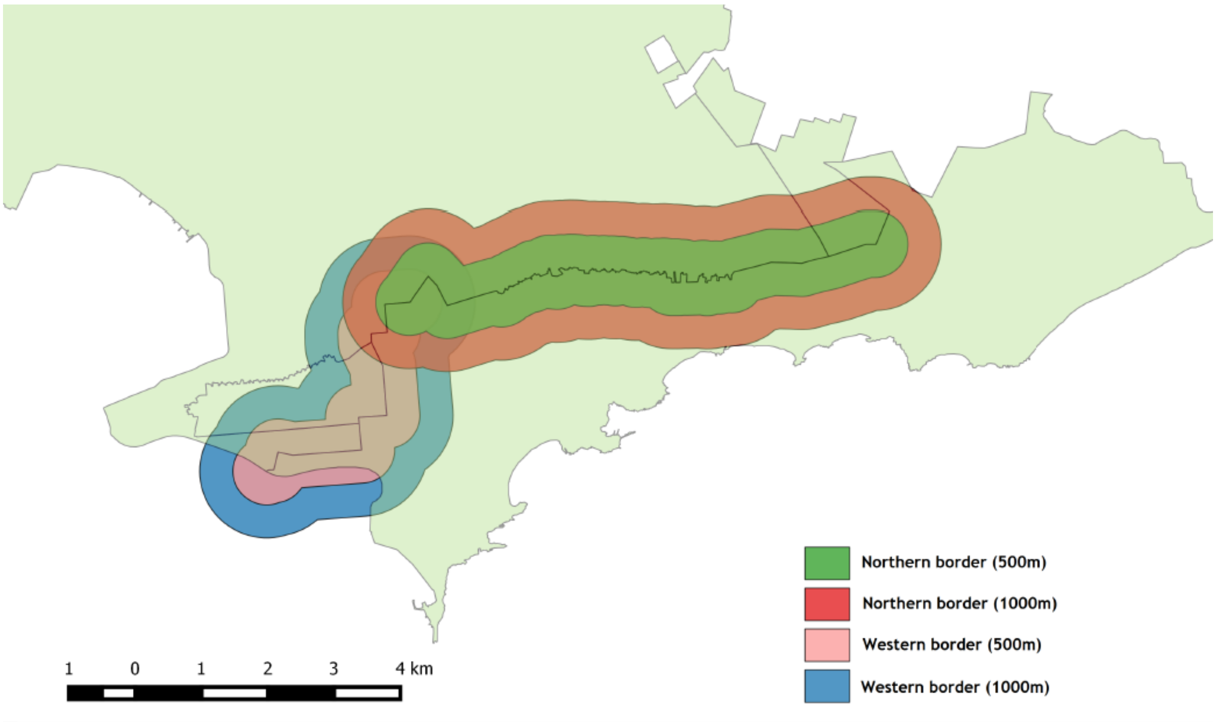


Figure 3. Buffer zones along the border.

Once each census tract is assigned to a treatment or control group, 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 2007-2015.

Although the key identification assumption will be formally tested later on, a preliminary evaluation of its adequacy can be carried out through graphic analysis. As can be seen in figure A3, the entire buffer zone does not show similar pre-treatment trends. These are only present in the western border (figure 4). Therefore, census tracts in the northern buffer zone are excluded from the analysis.

Table 3. Census information of census tracts per buffer zones and eligibility (2011).

	500 buffer zone		**1000 buffer zone**		**Hinterland buffer zone**	
	NLVIS	**LVIS**	**NLVIS**	**LVIS**	**NLVIS**	**LVIS**
% of dwellings that are houses	19.9%	20.0%	21.0%	18.6%	22.6%	17.5%
% of dwellings that are apartments	79.2%	78.4%	75.4%	79.7%	69.9%	80.8%
% of unoccupied dwellings	7.4%	8.4%	6.8%	9.2%	5.9%	9.9%
% of vacant dwellings	2.4%	3.0%	2.5%	3.4%	2.7%	3.6%
% of homeowner households	54.8%	47.5%	56.0%	44.9%	57.6%	42.9%
% of renter households	35.7%	42.4%	31.6%	44.5%	25.9%	46.2%
% of households that own a car	36.3%	25.5%	41.5%	23.7%	49.0%	22.2%
% of people with African ascendence	1.5%	2.9%	1.4%	2.4%	1.3%	2.0%
% of people that studied up to high school	31.7%	35.1%	30.8%	36.0%	29.4%	36.8%
% of people that studied up to university	49.4%	42.8%	49.4%	41.6%	49.4%	40.6%
% of unemployed people	3.5%	4.1%	3.5%	4.0%	3.5%	3.8%

% of inactive people	30.8%	30.6%	31.5%	32.5%	32.3%	34.0%

Figure 4. Annual square metres built (left) and number of housing starts (right), per year and eligibility status (western 1000 metres buffer zone).

As it can be seen on table 3, greater levels of comparability on observables (for the pre-treatment period) are achieved when considering locations that are nearer to each other. The hinterland buffer zone consists of all census tracts which belong to the 1000 metres buffer zone but not to the 500 metres buffer zone. The lower levels of comparability in this buffer zone support the identification strategy since similarity between locations decays as the distance from the border increases. Subsidized census tracts were less affluent, more densely populated and more inhabited by households who rent their dwellings than non-subsidized census tracts in 2011. Despite these differences, pre-treatment trends are similar in both 500 and 1000 metres buffer zones. Table 4 shows that after the policy's implementation, the location of residential construction shifted dramatically towards subsidized zones.

Table 4. Means and variations of dependent variables

		LVIS	**NLVIS**	$\Delta_{(LVIS-NLVIS)}$
met	500, pre	658.6	624.0	34.6
	500, post	1313.2	241.3	1071.8
	$\Delta_{(post-pre)}$	654.6	-382.7	**1037.3**
	1000, pre	401.3	663.2	-261.9
	1000, post	1244.3	206.0	1038.3
	$\Delta_{(post-pre)}$	843.0	-457.2	**1300.2**
$nperm$	500, pre	1.288	1.494	-0.206
	500, post	1.590	1.147	0.443
	$\Delta_{(post-pre)}$	0.302	-0.347	**0.649**
	1000, pre	0.829	1.862	-1.033
	1000, post	1.121	1.052	0.069
	$\Delta_{(post-pre)}$	0.292	-0.810	**1.102**

6. Results

Table 5 presents the results of the estimation for both variables on three intervals. All models include time and census-tract fixed effects and all standard errors are clustered to a neighbourhood level. Coefficients are always positive, statistically significant and large relative to the mean of the dependent variable. Therefore, estimates seem to support the hypothesis that the LVIS promoted a relocation process towards subsidized zones. The policy's impact is well over 100% of the mean for the square metres and varies between 50% and 100% for the number of housing starts. These estimates do not recover the net effect of the policy on subsidized areas, but rather the difference between said effects and the net effect on NLVIS zones.

Table 5. Estimates for 500 and 1000 metres western buffer zones

	(1) met_{500m}	(2) met_{1000m}	(3) met_{hint}	(4) $nperm_{500m}$	(5) $nperm_{1000m}$	(6) $nperm_{hint}$
Z	1037.3**	1300.2***	1557.8**	.64906***	1.1023***	1.7505***
	(252.522)	(306.214)	(515.009)	(0.133)	(0.274)	(0.250)
Constant	60.019	48.7	4.8519	1.1137**	.46201	-.37829
	(274.703)	(167.353)	(152.938)	(0.311)	(0.310)	(0.213)
Mean of dependent variable	748.91	668.15	589.26	1.39	1.14	0.90
Observations	378	765	387	378	765	387
R-squared	0.059	0.051	0.056	0.030	0.049	0.128
Number of census tracts	42	85	43	42	85	43

Clusterized robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The policy's impact is larger for the hinterland zone, and this difference is statistically significant at any standard significance level. Using different control groups to test for spillovers is done following Neumark and Kolko (2010). The intuition is that if positive spillovers exist, residential construction will increase not only within treated zones but also in immediately neighbouring zones. In that case, the policy's impact would be lower when comparing two locations near to each other and higher when considering locations that are farther apart. A possible interpretation of these results is that subsidized construction attracts non-subsidized construction on nearby locations. The fact that the change in the coefficient is larger for the number of housing starts relative to the square metres may point out that spillovers consist of higher construction levels of small buildings rather than larger projects. These interpretations must be nuanced, since treated and non-treated areas on the hinterland zone exhibit lower levels of comparability (see table 3 and figure A4).

6.1. 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, this allows to detect possible anticipation effects. The model can be estimated through the following regression:

$$y_{i,t} = \gamma_i + \theta_t + \sum_{j=-m}^{q} \beta_j * D_{i,t}(t = k + j) + \varepsilon_{i,t}$$

where $D_{i,t}(t = k + j)$ is an interaction variable between a year dummy (m years before the treatment (leads) and q years after it (lags) are included) and the dummy variable $zLVIS_i$. k is the year when treatment switches, and β_j is the coefficient associated with the *j-th* interaction term. If the identification assumption is valid, then $\beta_j = 0 \, \forall \, j < 0$. Intuitively, this test implies that the levels of the dependent variable did not differ systematically between treated and control groups when the treatment didn't exist.

Table 6. Estimates for 500 and 1000 metres western buffer zones

	(1) met_{500m}	(2) met_{1000m}	(3) met_{hint}	(4) $nperm_{500m}$	(5) $nperm_{1000m}$	(6) $nperm_{hint}$
zLV#2008	352.12	96.801	-164.93	.57176	.019089	-.69892
	(191.073)	(94.926)	(284.141)	(0.533)	(0.259)	(1.477)
zLV#2009	228.43	58.519	-55.634	.14353	.27833	.62097
	(345.392)	(163.593)	(49.217)	(0.371)	(0.555)	(0.905)
zLV#2010	192.23	-117.29	-304.83	.061176	-.44828**	-1.0968
	(420.949)	(462.393)	(537.672)	(0.459)	(0.175)	(1.005)
zLV#2011	-162.86	-300.93	-373.8	1.2282**	1.2395***	1.3737*
	(473.671)	(290.581)	(307.099)	(0.356)	(0.363)	(0.664)
zLV#2012	1034.2***	1265.1***	1430.2*	.95294	1.4323*	2.1505*
	(112.432)	(332.665)	(649.203)	(0.512)	(0.666)	(1.119)
zLV#2013	1114.7*	1140.7**	1292.8*	.63294	1.0431*	1.6425
	(447.140)	(381.951)	(578.020)	(0.325)	(0.555)	(0.937)
zLV#2014	1639.2**	1619.4***	1696.2**	1.1553*	1.4883**	2.1183**
	(364.932)	(427.518)	(631.091)	(0.435)	(0.602)	(0.763)
zLV#2015	848.92*	965.48*	1092.6	1.4588**	1.3165***	1.25**
	(330.881)	(429.353)	(628.573)	(0.377)	(0.299)	(0.525)
Constant	189.79	165.24	141.27	1.2857***	1.1412***	1***
	(118.213)	(130.773)	(208.603)	(0.207)	(0.160)	(0.271)
Observations	378	765	387	378	765	387
R-squared	0.064	0.054	0.058	0.049	0.074	0.198
Number of census tracts	42	85	43	42	85	43

Clusterized robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The identification assumption, as shown in table 6, holds for almost all models. For the number of housing starts, some anticipation effects can be found; especially for the 1000 metres buffer. For the metres models, the identification assumption holds independently of which buffer zone is used. Also, the difference between the square metres coefficients for the 500 metres and the hinterland zones is always statistically significant except for the year 2014. This also occurs, except for the year 2015, for the number of housing starts coefficients. Therefore, the enhanced model also suggests the possibility of positive spillovers given that impacts are heterogeneous.

6.2. Scale effects

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 $avmet_{i,t}$, which is defined as the quotient between $met_{i,t}$ and $nperm_{i,t}$. This new variable is an approximation to the mean size of residential buildings. Its time trend is presented on figure 5, where it is possible to observe similar trends on pre-treatment years. The formal test for the identification assumption for this variable is shown in table A1.

Figure 5. Annual average square metres per project built on western 500 (left) and 1000 (right) metres buffer zones.

As seen on table 7, the policy had a sizeable and significant effect on the average size of residential projects on LVIS zones. Coefficients are well over 100% of the mean in all cases. 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 table 5, a possible interpretation is that the impact is 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 have 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.

Table 7. Estimates for 500 and 1000 metres western buffer zones

	(1) $avmet_{500m}$	(2) $avmet_{1000m}$	(3) $avmet_{hint}$
Z	445.46* (169.066)	576.55*** (135.316)	684.14*** (189.194)
Constant	96.852 (60.354)	70.322 (49.233)	44.409 (72.523)
Mean of dependent	338.51	313.99	290.04

variable			
Observations	378	765	387
R-squared	0.059	0.043	0.048
Number of census tracts	42	85	43

Clusterized robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

6.2.1. Height regulations

It is not clear whether the former increase on the average square metres of each project consists of buildings of greater height or buildings that occupy larger portions of land. In order to evaluate this, I exploit variations in zoning regulations to assess if the impact of the LVIS varies between census tracts with different regulations. In this section, the regulation of interest will be that which imposes maximum height limits to construction projects. All urban parcels in Montevideo have a maximum height level (measured in metres) determined by the subnational government. Construction starts are not approved if they surpass this limit. Hence, if new construction is composed of buildings of greater heights, the policy's impact is expected to be larger on census tracts with higher maximum height limits.

In order to test for possible heterogeneity between census tracts with different height regulations, it is possible to incorporate an interaction variable between the treatment ($zLVIS_i * P_t$) and a variable related to height regulations on each census tract. Information on height regulations is available at a parcel level. As seen on figure 6, not all parcels on a specific census tract share the same maximum height levels, since these are associated with proximity to important roadways. On average, parcels located on control zones in both buffer zones have lower maximum heights than treated zones. To summarize information on height regulations on a specific census tract, I consider the variable H_i, which represents the average maximum height of a parcel on census tract *i*. Higher values of H_i indicate lower regulation levels and vice versa. The estimated model is the following:

$$y_{i,t} = \gamma_i + \theta_t + \beta_1(zLVIS_i * P_t) + \beta_2(zLVIS_i * P_t * \overline{H_i}) + u_{i,t}$$

where $\overline{H_i} = H_i - \overline{H}$, where $\overline{H}$ is the sample mean of H_i. The centred variable is used in order to simplify the interpretation of β_1, which represents the impact of the policy on a census tract with the mean level of height regulations ($\overline{H_i} = 0$). β_2 is the coefficient which recovers the treatment's heterogeneity and it represents the additional impact of the policy when the census tract average maximum height increases in one metre relative to the mean level.

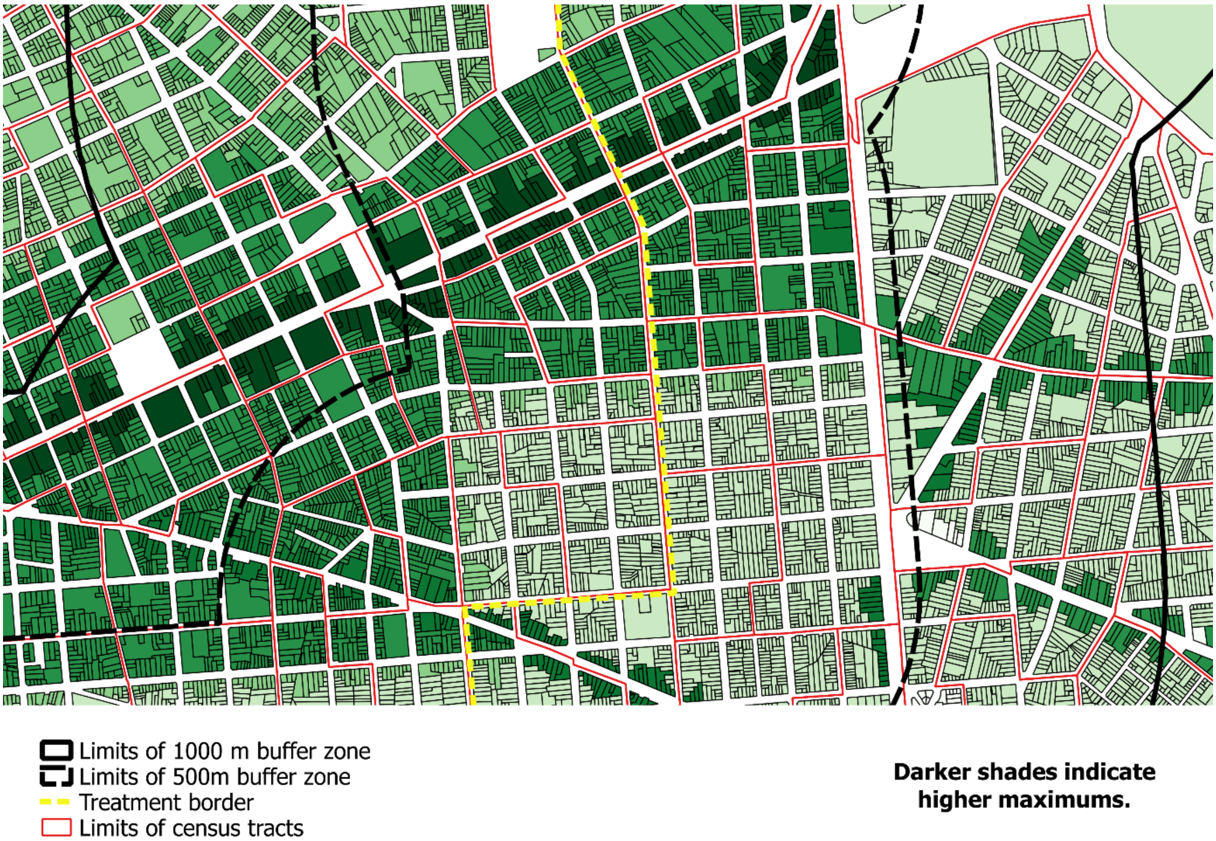


Figure 6. Height regulations per parcel.

Table 8. Estimates for 500 and 1000 metres western buffer zones

	(1) met_{500m}	(2) met_{1000m}	(3) met_{hint}	(4) $nperm_{500m}$	(5) $nperm_{1000m}$	(6) $nperm_{hint}$	(7) $avmet_{500m}$	(8) $avmet_{1000m}$	(9) $avmet_{hint}$
Z	1031.6***	1143.8***	1427.9**	.64738***	1.073***	1.7501***	442.31**	483.01***	594.35**
	(214.696)	(202.121)	(521.813)	(0.104)	(0.284)	(0.412)	(145.040)	(90.449)	(206.531)
Z*H	64.747***	49.914*	23.234	.019065**	.009351	.000069384	35.887***	29.849**	16.062
	(9.381)	(26.332)	(50.278)	(0.005)	(0.011)	(0.038)	(7.752)	(10.134)	(19.043)
Constant	189.79	165.24	141.27	1.2857***	1.1412***	1***	96.852	70.322	44.409
	(116.188)	(121.029)	(195.398)	(0.250)	(0.171)	(0.273)	(59.976)	(44.558)	(67.105)
Observations	378	765	387	378	765	387	378	765	387
R-squared	0.073	0.058	0.057	0.031	0.049	0.128	0.081	0.053	0.050
Number of census tracts	42	85	43	42	85	43	42	85	43

Clusterized robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 8 shows the results of this model. Estimates of β_1 are almost identical to those found in table 5, which can be considered as a first robustness check of the model since they shouldn't differ because they both represent the average impact of the treatment. Estimates of β_2 are the parameters of interest, and they are always positive but not always statistically significant. In fact, they only seem to be significant when considering the 500 metres buffer zone. This indicates that construction projects that locate on this zone are different than those which locate on the hinterland. Specifically, the former seem to be higher than the latter, since the effect of the treatment is higher on less regulated census tracts. Results are consistent with the interpretation that developers do not locate residential construction more intensely on census tracts with higher maximum height limits in zones that are farther away from the border;

while the opposite occurs on zones that are nearer to the border. Therefore, it is likely to expect higher density levels in this last zone, whereas areas that are farther away from the border seem to be attracting larger projects but of similar height to the previously existing ones. Nevertheless, this could be associated with the fact that height regulations vary more in the 500 metres buffer zone compared to the hinterland zone. Also, the underlying assumption in this interpretation is that construction located on census tracts with lower regulation levels will automatically be taller. This may not necessarily be the case since the limits do not inhibit the construction of low-rise buildings on parcels with higher maximum height levels.

6.3. Robustness checks

Two robustness checks will be carried out. Firstly, the criteria reviewed in section 5.1. for constructing the buffer zones and the treatment status will be altered: all census tracts which intersect a buffer zone or a treatment zone are excluded from the sample. Therefore, the number of observations will be somewhat reduced. Secondly, placebo tests will be conducted by randomly assigning treatment status to each census tract.

As seen on table 8, estimates are still positive, large and statistically significant when restricting the sample to those census tracts which are fully within buffer zones or treatment areas. In comparison with the original model in table 5, coefficients are slightly higher except for the case of the metres in the 500 metres buffer zone. Table A2 shows the enhanced model with the restricted sample, which is similar to the one shown in table 6. Nevertheless, although the behaviour of the number of housing starts is not systematically different in pre-treatment years, some years show statistically significant differences. This does not occur for the square metres model.

Table 9. Estimates for 500 and 1000 metres western buffer zones

	(1) met_{500m}	(2) met_{1000m}	(3) met_{hint}	(4) $nperm_{500m}$	(5) $nperm_{1000m}$	(6) $nperm_{hint}$
Z	907.61**	1494.1**	1964.2*	.73056***	1.2075**	1.7552**
	(274.762)	(442.972)	(776.849)	(0.118)	(0.390)	(0.464)
Constant	199.54	149.33	102.58	1.5926**	1.1964***	.82759**
	(110.876)	(139.806)	(264.589)	(0.304)	(0.154)	(0.255)
Mean of dependent variable	612.30	840.02	1052.04	1.52	1.25	1.00
Observations	243	504	261	243	504	261
R-squared	0.065	0.055	0.070	0.054	0.054	0.119
Number of census tracts	27	56	29	27	56	29

Clusterized robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

In the second robustness check, a placebo treatment is assigned to each census tract randomly, following a Bernouilli distribution with $p = 0.5$. If estimates are robust, coefficients shouldn't be statistically significant since this placebo is not correlated with the eligibility of each census tract. As seen in table 8, independently of which variable or buffer zone is selected, all statistical significance is gone when considering the placebo treatment. This placebo test was repeated 500 times with no statistical significance obtained in any of the tests.

Table 10. Placebo estimates for 500 and 1000 metres western buffer zones

	(1) met_{500m}	(2) met_{1000m}	(3) met_{hint}	(4) $nperm_{500m}$	(5) $nperm_{1000m}$	(6) $nperm_{hint}$	(7) $avmet_{500m}$	(8) $avmet_{1000m}$	(9) $avmet_{hint}$
ZZ	539.83	404.31	294.37	.2146	.077345	-.063766	322.33	140.41	-20.701
	(425.552)	(348.628)	(371.725)	(0.289)	(0.149)	(0.298)	(171.092)	(119.977)	(102.268)
Constant	189.79	165.24	141.27	1.2857***	1.1412***	1**	96.852	70.322	44.409
	(129.325)	(159.840)	(252.041)	(0.265)	(0.207)	(0.380)	(70.626)	(65.263)	(87.803)
Observations	378	765	387	378	765	387	378	765	387
R-squared	0.046	0.028	0.028	0.019	0.009	0.023	0.055	0.021	0.026
Number of census tracts	42	85	43	42	85	43	42	85	43

Clusterized robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

6.4. Spillovers

As it has been already said, a possible interpretation of the difference between estimates in table 5 is that the policy has positive spillovers on immediately neighbouring areas. This could be related to the fact that higher levels of residential construction can increase the attractiveness of the neighbourhood as a whole. Since the policy only gives tax breaks to projects with more than one housing unit, the fact that the impact on the number of starts is lower than the impact on the square metres could be consistent with higher levels of residential high rises on subsidized zones and spillovers in the form of increased construction of individual homes on non-subsidized zones.

Another possible spillover relates to higher levels of commercial activity on subsidized zones. More residential construction will increase the number of people living on treated areas. These people will increase the demand for services in their neighbourhoods. Therefore, if these spillovers exist, construction starts with commercial purposes should exhibit a similar pattern to the observed for the housing starts. It should be noted that this argument makes two assumptions. Firstly, that all new commercial activity implies at least some construction activity. Secondly, that the spatial discontinuity identification strategy is also valid for the location of commercial construction. While the second assumption can be assessed through graphical analysis and the enhanced model used in section 6.1., the first one is less clear. Furthermore, since only a fraction of LVIS construction has been finished (and therefore inhabited), the absence of any surges in commercial activity does not necessarily imply the absence of spillovers. Therefore, robustness of estimates in this section is likely suboptimal.

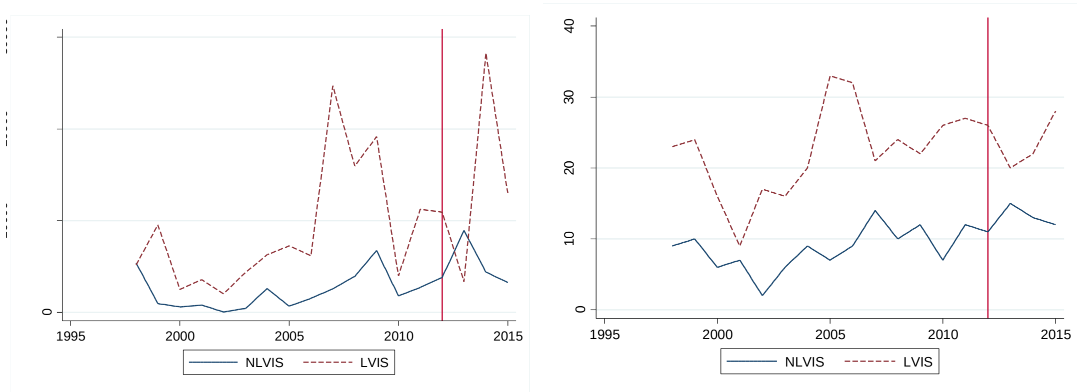


Figure 7. Total square metres (right) and number of commercial starts (left) on western 1000 metres buffer zone.

Subsidized zones receive more commercial construction, as seen on figure 6. Pre-treatment trends are not similar at first glance, especially for the case of the square metres. Table 10 shows mostly negative coefficients, although there are no statistically significant differences between commercial construction levels between treated and non-treated zones. Evidence supporting the validity of the identification strategy is given in table A3, although this seems to be less clear for the hinterland buffer zone. Despite the fact that several caveats apply; the policy does not appear to have positive spillovers on commercial activity on treated zones.

Table 11. Estimates for 500 and 1000 metres western buffer zones (commercial starts)

	(1) met_{500m}	(2) met_{1000m}	(3) met_{hint}	(4) $nperm_{500m}$	(5) $nperm_{1000m}$	(6) $nperm_{hint}$
Z	-104.8	-73.365	-83.469	.017412	-.060345	-.1168*
	(220.896)	(124.129)	(84.761)	(0.203)	(0.135)	(0.059)
Constant	388.75	321.04**	254.9***	.35714***	.41176***	.46512***
	(269.126)	(122.827)	(59.213)	(0.056)	(0.039)	(0.099)
Mean of dependent variable	235.84	220.61	205.74	0.407	0.421	0.434
Observations	378	765	387	378	765	387
R-squared	0.015	0.009	0.016	0.017	0.002	0.025
Number of census tracts	42	85	43	42	85	43

Clusterized robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

7. Conclusion

This paper makes four relevant contributions to existing knowledge of the LVIS program. A methodology that combines a spatial discontinuity design with difference-in-differences is proposed to recover the causal effect of the policy. Furthermore, the first estimates of the LVIS program's relative effect on treated zones are provided. A large and statistically significant effect is found, which is over 130% of the mean for the case of square metres and over 60% for the number of housing starts. Estimates are robust and their magnitude reflects an intense relocation process of residential construction from non-subsidized zones to subsidized ones. In addition, it shows that the policy increased the size of residential projects, measured by the average square metres per housing start. The effect on the densification of the city, however, is heterogeneous: while zones nearer to the treatment's border seem attract more high-rise buildings, zones that are farther away do not exhibit a similar pattern. Finally, the policy seems to have some positive spillovers on residential construction on nearby non-subsidized areas but no evidence of spillovers on commercial activity is found.

Estimates are obtained for geographical areas that have a relatively modest extension. Hence, the external validity of these results is unclear which suggest caution when extrapolating the findings of this study to the entire city of Montevideo. Also, previous estimates do not measure the effect of the program on the total housing stock of Montevideo. A pending issue is to precisely quantify the program's impact on each zone individually instead of the difference between them. Determining this magnitude is crucial in order to design hypothesis on the program's impact on housing prices in Montevideo.

Results suggest that the LVIS changed the geographical centre 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. 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. 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.

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A. Annex

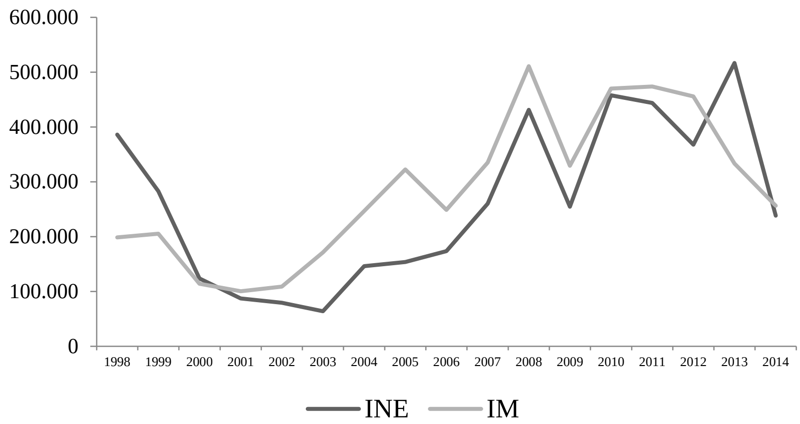
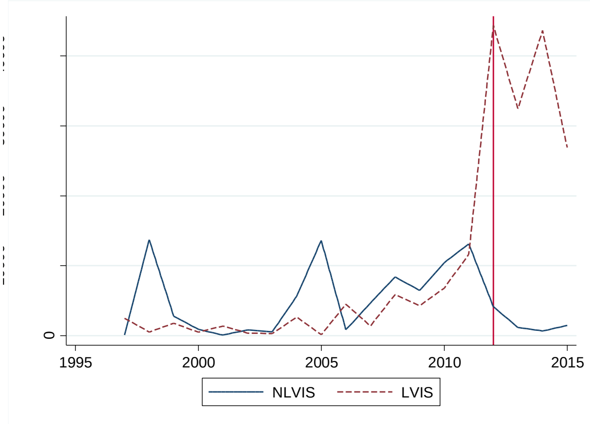


Figure A1. Total square metres built in Montevideo with housing purposes

Figure A2. Number of LVIS subsidized projects, by neighbourhood

Figure A3. Annual square metres built (left) and number of housing starts (right), per year and eligibility status (northern and western buffer zones).



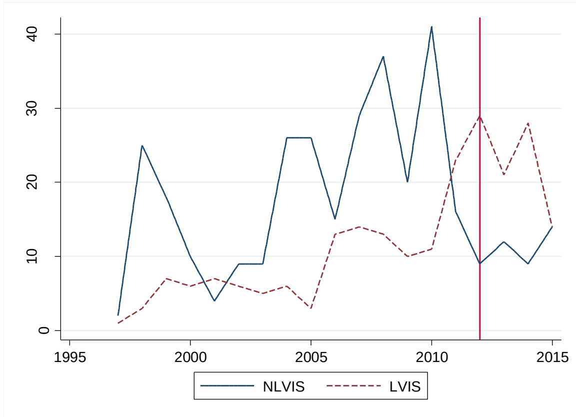


Figure A4. Annual square metres built (left) and number of housing starts (right), per year and eligibility status (hinterland buffer zone).

Table A1. Estimates for 500 and 1000 metres western buffer zones

	(1) $avmet_{500m}$	(2) $avmet_{1000m}$	(3) $avmet_{hint}$
zLV#2008	196.47*	90.03	-39.276
	(81.429)	(66.710)	(108.978)
zLV#2009	299.49	71.203	-187.54*
	(184.715)	(131.842)	(91.302)
zLV#2010	250.32	84.324	-4.2993
	(208.444)	(190.531)	(125.390)
zLV#2011	-307.86	-350.72**	-368.73*
	(241.882)	(132.943)	(171.248)
zLV#2012	561.59**	688.12***	747.94**
	(190.449)	(129.719)	(266.618)
zLV#2013	414.87**	449.41***	504.3**
	(134.458)	(101.325)	(183.434)
zLV#2014	741.62**	651.03***	569.22**
	(205.066)	(156.683)	(221.542)
zLV#2015	414.51*	433.5*	435.21
	(170.351)	(218.167)	(306.998)
Constant	96.852	70.322	44.409
	(58.975)	(50.072)	(73.396)
Observations	378	765	387
R-squared	0.075	0.049	0.052
Number of census tracts	42	85	43

Clusterized robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A2. Estimates for 500 and 1000 metres western buffer zones (restricted sample)

	(1) met_{500m}	(2) met_{1000m}	(3) met_{hint}	(4) $nperm_{500m}$	(5) $nperm_{1000m}$	(6) $nperm_{hint}$
zLV#2008	524.7	182	-212.67	1.5556*	.825	-.11039
	(304.209)	(159.092)	(139.072)	(0.502)	(0.468)	(0.145)
zLV#2009	-79.107	161.11	378.39	1.0556	1.35*	1.8052***
	(371.701)	(226.457)	(436.068)	(0.874)	(0.643)	(0.215)
zLV#2010	-853.99	-153.31	623.01	.5	.4	.12338
	(917.361)	(550.902)	(380.773)	(0.287)	(0.438)	(0.373)

zLV#2011	-785.77	-414.57	-70.828	2.0556*	1.95***	1.8247***
	(540.939)	(464.311)	(549.459)	(0.666)	(0.415)	(0.357)
zLV#2012	844.42***	1571.7**	2092**	1.6667	2.1625**	2.6623*
	(130.446)	(438.081)	(738.165)	(0.779)	(0.662)	(1.094)
zLV#2013	224.5*	1283.6*	2235.8*	1.6667***	2.125***	2.5325***
	(81.354)	(555.385)	(1,029.002)	(0.175)	(0.340)	(0.483)
zLV#2014	806.66**	1707.6***	2517.1**	1.5556	2.1875**	3.0519***
	(249.088)	(379.554)	(704.549)	(0.837)	(0.829)	(0.420)
zLV#2015	799.53	1233.8*	1586.1*	2.1667**	1.975***	1.6883**
	(472.915)	(539.610)	(666.704)	(0.427)	(0.295)	(0.435)
Constant	199.54*	149.33	102.58	1.5926**	1.1964***	.82759***
	(74.074)	(142.323)	(258.568)	(0.291)	(0.116)	(0.143)
Observations	243	504	261	243	504	261
R-squared	0.091	0.057	0.072	0.089	0.084	0.182
Number of census tracts	27	56	29	27	56	29

Clusterized robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A3. Estimates for 500 and 1000 metres western buffer zones (commercial starts)

	(1) met_{500m}	(2) met_{1000m}	(3) met_{hint}	(4) $nperm_{500m}$	(5) $nperm_{1000m}$	(6) $nperm_{hint}$
zLV#2008	-144.79	-202.64	-215.6*	.29882	.1915	.15323
	(986.746)	(418.729)	(99.991)	(0.428)	(0.223)	(0.091)
zLV#2009	-177.67	-242.76	-314.25**	.23765	.086823	-.064516
	(851.529)	(329.300)	(127.702)	(0.420)	(0.221)	(0.271)
zLV#2010	-469.88	-343.59	-266.35**	.57176*	.33067**	.032258
	(556.304)	(246.865)	(76.650)	(0.227)	(0.126)	(0.277)
zLV#2011	-524.82	-245.2	-9.4548	.098824	.17611	.24462
	(553.944)	(277.124)	(107.314)	(0.190)	(0.145)	(0.272)
zLV#2012	-508.92	-288.61	-83.529	.25882	.19273	.16667
	(523.802)	(255.065)	(79.757)	(0.315)	(0.214)	(0.161)
zLV#2013	-412.38	-600.71*	-908.39***	.38118	-.05234	-.3871**
	(550.530)	(300.865)	(200.256)	(0.333)	(0.215)	(0.129)
zLV#2014	-75.925	1.3212	57.927	.08	.05234	.051075
	(73.454)	(182.193)	(327.079)	(0.308)	(0.247)	(0.232)
zLV#2015	-475.69	-232.82	-44.408	.31529	.19397	-.0053763
	(514.291)	(240.406)	(149.469)	(0.262)	(0.247)	(0.335)
Constant	388.75	321.04**	254.9***	.35714***	.41176***	.46512***
	(269.496)	(125.430)	(43.836)	(0.065)	(0.042)	(0.100)
Observations	378	765	387	378	765	387
R-squared	0.021	0.016	0.046	0.028	0.006	0.034
Number of census tracts	42	85	43	42	85	43

Clusterized robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1