Impact of a Housing Tax Credit on Local Housing Markets: Evidence from France

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Abstract

Housing Tax Credits are a popular tool designed to increase the construction of affordable housing units for low and medium income tenants. Several papers in the US, however, document the lacklustre performance of such programs that represent an important amount of public expenditures. In this paper, we exploit a quasi-natural experiment in France (the removal of the Borloo and Robien policies on part of the territory with the implementation of the Scellier Tax Credit (STC)), to identify the impact of such policies on local housing markets. We find that the removal of these tax credits decelerated house prices and lowered the vacancy rate in new dwellings without reducing the production of new housing units. Finally, the income profile of tenants in new dwellings remained unaffected.

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I. Introduction

In France, total subsidies to the housing sector represented about 40 billions euros (around 2% of GDP) in 2012, of which support to the rental sector accounted for 71%. Policies that promote rental property investments through an income tax rebate, such as the Scellier Tax Credit (STC) in France, are a continuous and non-negligible part of it. Indeed, the overall fiscal cost of the Scellier Tax Credit amounted to 2.1 billions of euros between 2009 and 2012 according to the Ministry of Housing.

To benefit from an income tax rebate, the household had to buy a new dwelling between 2009 and the end of 2012 in a municipality eligible to the Scellier Tax Credit, and committed to rent it under a mandatory rent ceiling for at least 9 years. In addition, the tax rebate was higher if the dwelling was rented to low/middle income tenants. The Scellier Tax Credit only applied to part of the territory, the eligible area being divided in 3 areas (A, B1 and B2) according to the degree of tension on the housing market. The rent ceiling was set accordingly with the highest one in A areas, and the lowest in B2 areas.

The Scellier Tax Credit is part of a long tradition of fiscal devices[1] which were barely evaluated in France (Bosvieux (2011)). This paper follows the US literature which has been trying to disentangle which housing policy is the best (see Apgar Jr (1990) or Olsen (2003)). The most important debate tried to find whether project-based or tenant-based subsidies were more efficient (see Chapelle (2015) for a review). We are interested in the particular case of privately owned project-based subsidies, such as the Low Income Housing Tax Credit (LIHTC) in the US or the Scellier Tax Credit (STC) in France.

The main goal of such policies is to boost residential investment in order to increase the number of rental dwellings proposed to low-income tenants. A simple incidence analysis in an economic framework, as performed in Bono and Trannoy (2012), suffices to highlight a potential caveat of such approach: the capacity of such schemes to increase the number of units available depends on the housing supply elasticity. In other words, such subsidy is likely to generate a potential crowding out effect in inelastic areas.[2] In the US, many studies have been documenting such a crowding out effect

[1] The Méhaignerie law (1984-1997), the Périssol (1996-1999), the Besson (1999-2002), the Robien (2003-2006), the refocused Robien and the Borloo (2006-2009), the Scellier (2009-2012), the Duflot (2013-2014), the Pinel (2014 until now). For details on the preceding policies, see Scellier (2008).

[2] The crowding out effect can be explained by the competition on land market, it will be high when access to new land parcels is limited (see Bono and Trannoy (2012)). Alternate explanations invoke

for the LIHTC. For example, Sinai and Waldfogel (2005) and Eriksen and Rosenthal (2010) found a very substantial crowding out, usually well above 50% and even close to 100%. These findings cast some doubts on the potential effectiveness of such policies in France. Indeed, the housing supply elasticity is usually considered much lower than in the US (see Caldera and Johansson (2013) or Chapelle and Eyméoud (2016)).

The Scellier Tax Credit was promulgated right after the financial crisis of 2007-2008, and can be seen as a reform of the previous devices to focus the incentives to build on the tightest local housing markets.[3] Previous tax credits such as the Borloo and the Robien were criticized for having generated strong windfall effects. Hence, the removal of those policies in C areas where the state of the housing market did not justify subsidizing new constructions. The present paper assesses the impact of tax credits by considering the removal of the Borloo-Robien tax credit on part of the territory and comparing areas which kept on benefiting from tax credits after the STC law with those that did not. We consider that the fact that C areas stopped benefiting from fiscal incentives for rental investment similar to the Scellier Tax Credit (the Borloo-Robien) is the treatment.

We exploit the geographical variation of the removal of the Borloo-Robien in a difference-in-difference framework to evaluate its impact on the housing stock, the vacancy rate of new dwellings, house prices, and the number and income of new dwellings' tenants, using fiscal data at the housing block level.

Our paper is related to the literature evaluating place-based policies using fine-resolution data on narrow areas like Gobillon, Magnac and Selod (2012) or Neumark and Kolko (2010). This literature puts forward an important trade-off: while treated areas can be more convincingly compared to neighboring areas because of similar unobservable characteristics, such areas are more likely to be affected by spillovers or externalities from the policy. The evaluation of the STC is subject to similar challenges, we thus combine Einio and Overman (2012) and Kline and Moretti (2014) approaches, to identify comparable treated and control groups while limiting the influence of the treatment on the control groups. Thus, our second identification strategy is to divide our treated and control areas into 1 km-wide rings from the treatment boundary to assess the presence of spillover effects, and drop the rings that present evidence of

competition on the rental market, in such case the effect will be high when the tenants' demand is inelastic (see Eriksen and Rosenthal (2010)).

[3] According to the report 3805 of the National Assembly made for the 2012 finance project law which contains an assessment of the Scellier Tax Credit.

spillover effects, which eventually allow us to estimate the net effect of the policy for similar areas.

Like the small strand of literature evaluating the impact of the Low Income Housing Tax Credit (LIHTC) in the US (see Malpezzi and Vandell (2002) and Sinai and Waldfogel (2005)), or the only other paper on the Scellier Tax Credit (Bono and Trannoy (2012)), our results cast some doubts on the efficiency of such policies.

We find that the removal of the Borloo-Robien tax credit had no impact on the growth of the housing stock compared to similar areas, while it had a deflationary impact on house prices. It decreased house prices by 1% for similar nearby areas. In addition, the removal of the Borloo-Robien tax credit decreased the vacancy rate of new dwellings by 1.6 percentage points more in areas that stop benefiting from these policies compared to areas that still did. The share of owners living in new dwellings increased by 6% more in treated areas. However, the removal of the Borloo-Robien policy did not affect the income profile of tenants in new dwellings.

We evaluate a local impact only valid for the periphery of urban areas where most of our sample is located. Therefore, our results might be seen as an upper bound in absolute terms for the effect on quantities (and a lower bound in absolute terms for the effect on prices), because these areas are believed, according to the Scellier zoning, to have a higher housing supply elasticity than tenser areas (like A areas). In a nutshell, our estimations suggest that for similar areas on the city fringe, housing tax credit policies had a pure windfall effect, the growth of the housing stock would have evolved at the same pace without those policies. The estimated impact of the removal also suggests that tax credits trigger house price and new dwellings' vacancy rate increases. The absence of impact of the removal on the income of new tenants suggests that, such as the LIHTC, policies that promote rental property investments failed to provide accommodation for low-income households.

The paper is organized as follows. After presenting briefly the reform in Section II, we describe the dataset used in this study in Section III, while Section IV provides the details of our identification strategy. Results are provided in Section V and Section VI concludes.

II. Policy overview

In this paper, we propose an econometric analysis of the removal of the Borloo-Robien tax credit for rental housing. We exploit the geographical variation induced by the creation of the Scellier Tax Credit which can be seen as the French counterpart of the Low Income Housing Tax Credit (LIHTC) in the US. The latter had been under the scrutiny of several papers. The main difference between the LIHTC and the STC was that, for the STC, the income limit of the tenant was only mandatory for one type of investment, the intermediate STC, and was not very binding given that more than 70% of the households were eligible.

Such investment schemes are related to low-income rental housing policies which represent an important amount of public subsidies.[4] Rental housing policies are usually divided in two broad categories: project-based assistance where the subsidy is attached to the dwelling, and tenant-based assistance where it is attached to the household. Project-based rental assistance is itself divided in two categories: public or social housing,[5] and privately owned subsidized projects.

Like the LIHTC, the Scellier Tax Credit (STC) and the previous schemes fell under the category of place-based privately owned subsidized projects. In effect, its aim was to promote rental property investments through an income tax rebate, for taxpayers who owned new dwellings from the first of January 2009 until the 31st of December 2012. The law's recipients had to commit to rent their dwellings at least 9 years under a mandatory rent ceiling to tenants satisfying some resources constraints to get the highest rebate.[6] Each household could only benefit from the tax credit for one dwelling. The STC can be summarized by the following formula:

$$Annual\ Tax\ Rebate = \frac{1}{9}\left(0.25 \times \min(P, 300000) + 0.3 \times R \times \mathbb{1}_{intermediate}\right)$$

where P is the price of the new dwellings in euros, R is the total gross rental income received by the dwelling owner, and $\mathbb{1}_{intermediate}$ is a dummy that is equal to one when the dwelling is under intermediate STC. For example, in the most advantageous case,

[4] Low-income housing assistance in the US amounted to 50 billions of dollars (0.3% of the GDP) in 2014. Project-based policies accounted for 70% of low-income rental assistance policies in the US.

[5] This category usually refers to a non profit housing sector which is very common in Europe. For example, it represents about 18% of the French housing market or 20% of the UK housing market in 2011. See Whitehead and Scanlon (2007) for further details.

[6] For details on the law, see Appendix A. See Tables B1 and B2 in the Appendix B, for details on the rent ceilings and the resource constraints.

the intermediate Scellier, a 300 000 euros investment translated into a tax rebate of, at least, 111 000 euros.[7]

It is worth noting that the Scellier Tax Credit was not the first fiscal device of this kind. Since 1984, 8 tax incentives for rental housing have been succeeding to one another. The salient fact of the Scellier Tax Credit, was that contrary to preceding policies, only part of the territory was eligible to the program. In addition, the Scellier Tax Credit had similar features to the Borloo-Robien policy especially in terms of rent ceilings.[8] These two facts are essential to our identification strategy, since the disappearance of the Borloo-Robien tax credit in some areas will be our main source of identification.

In the Scellier Tax Credit device, the country was divided in 4 areas: A, B1, B2 and C. Only the areas A, B1 and B2 were eligible to the STC. The rent cap varied in the different eligible areas, it was higher in the tense local housing markets, the A areas, and lower for the areas with moderately tense housing markets, the B areas (B1 and B2). The policy zoning was based on municipalities, a municipality was either eligible or not. The spatial distribution of the different areas can be seen on Figure 1. According to the 2007 INSEE Census, 38% of the French population lived in a non eligible area (C areas), 43% in B areas (21% in B1 areas and 22% in B2 areas) and 19% in A areas.

From the first of January to the third of May 2009, the areas were those of the preceding law, the Robien law. However the differences between the Scellier and Robien areas were small, only 1068 municipalities changed zones and none went from being eligible to being non eligible.[9] Given the short length of the initial zoning, we focus on the most recent one.

[7] If the household kept renting the dwelling under intermediate Scellier after 9 years, a 2% rate applied until the 15th year. The tax rebate was of 8333 euros per year during 9 years, and 6000 euros per year until the 15th year. This accounting of the tax rebate doesn't take into account the rebate linked to the gross rent perceived. If the dwelling was rented for 9000 euros per year, there would be a supplementary tax rebate of 2700 euros.

[8] For a comparison of the Borloo, the Robien and the Scellier, see http://vse91174.nfrance.com/~v1039/upload/comparatif-robien-borloo-populaire-loi-scellier-scellier-social.pdf

[9] 720 municipalities went from C to B2, 255 from B1 to B2, 36 from C to B1, 23 from B2 to B1, 18 from A to B1, and 16 from B1 to A.

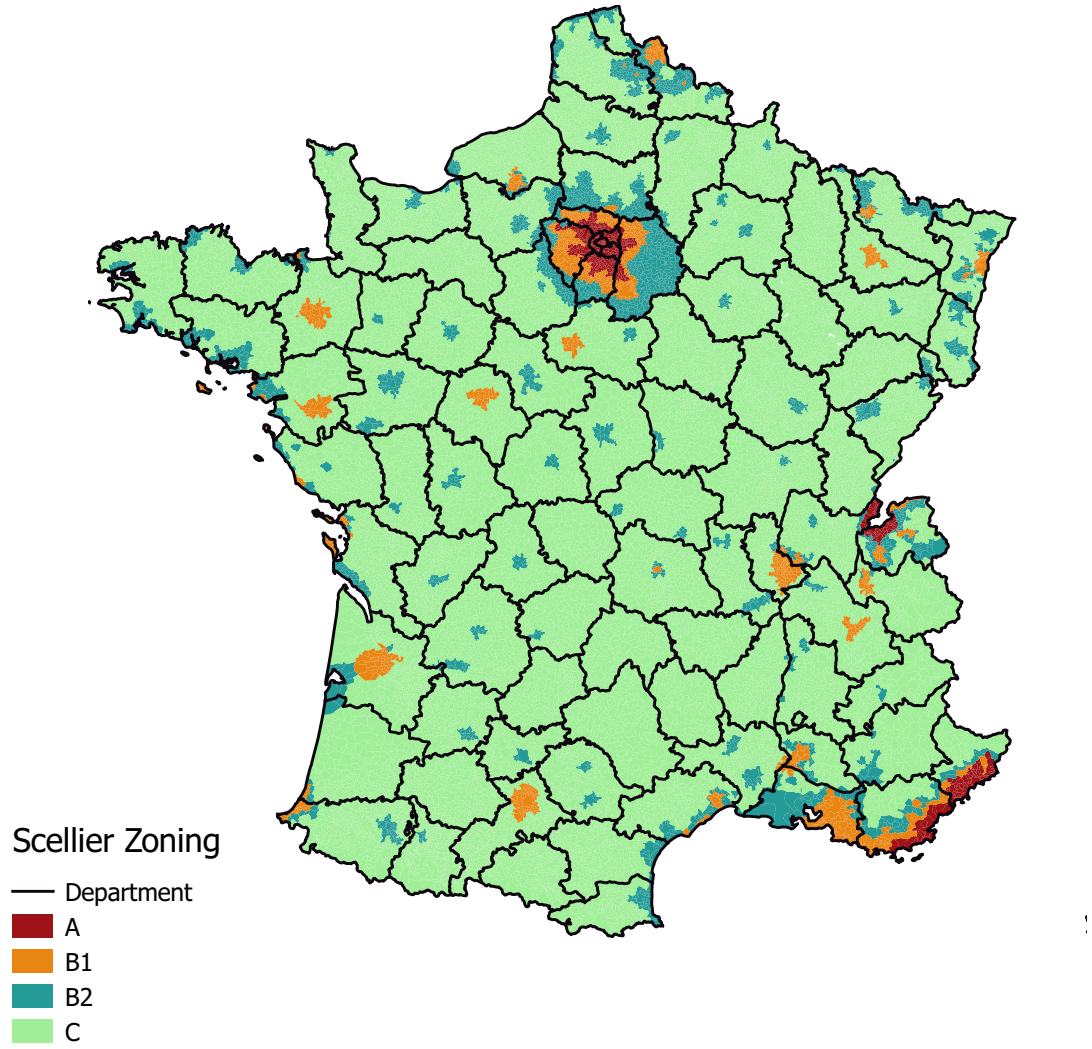

FIGURE 1. SCELLIER ZONING FOR SUBSIDIES

Source: Authors computations from IGN GEOFLA(R) and French Ministry of Housing. Areas A, B1 and B2 were eligible to the Scellier Tax Credit whereas areas C were not eligible.

III. Data

This paper uses two different datasets: fiscal data on the housing stock and data on housing unit transactions and prices.

A. *Fiscal data*

The FiLoCom dataset (for "Fichier des Logements dans les Communes" in French or file of housing units in municipalities) is produced jointly by the French fiscal administration and the French Ministry of Housing. It consists of an exhaustive fiscal census of each housing unit in Metropolitan France, which is edited on January 1st of every two years since 1995 from households' fiscal declarations (each file contains approximately 34 millions observations). They include information about each housing unit location (at the so-called *"sections cadastrales"* (housing blocks) scale), characteristics (surface, number of rooms and building period), residence conditions (is the dwelling full time or part time occupied? Is it occupied by its owner, rented or vacant? Is the owner private or public?) and resident themselves (the number of residents, their marital status, their ages and income levels). However, they contain no information on prices or transactions.

B. *Data on dwelling transactions*

The data is collected by the French solicitors (the so-called *"notaires"*), who, in France, enact housing unit transactions. However, they are not exhaustive because the collection of the data is not compulsory and of variable quality. A re-weighting of the observations is constructed at the French department scale from a confrontation of these data with fiscal ones on dwelling transactions, which are exhaustive but exist only at this aggregated geographic level. This re-weighting is used notably by the national French statistics institute (INSEE) to produce the French residential property prices index.

The data contains information on second-hand dwelling prices, location, transaction date and characteristics more complete than those contained in the fiscal data (information on housing characteristics includes furthermore number of bathroom, cellars, balconies, parking and the land surface for houses). All these variables are used to estimate hedonic equations. We have to signal that the exact surface is missing for about one third of the apartments and half of the houses, so they have been imputed with the fiscal data previously presented, by computation of the mean surface at the

housing block level with the same characteristics (using this method to impute existing surfaces and regressing it on their imputations give respectively R^2 of 55 percents for houses and 75 percents for flats).

C. *Units of observation*

While most of the previous studies on housing market policies have been using municipal data (Bono and Trannoy (2012), Gobillon and Vignolles (2016) or Chapelle (2015)), or Iris Data (Baumont et al. (2004)), we take advantage of the high precision of our dataset to assess the policy using new units of observation: fiscal blocks from the French cadastre. These units are small and relatively homogeneous blocks. Some minor alterations were brought to take into account for some changes in their limits, by splitting or merging blocks over the period.

We use a GIS software to compute the minimum distance between the border of these units of observation and the closest frontier between a B and a C area. As an example, since representing the entire country would be unreadable, we represented the housing blocks and their distance from the frontier within 5 km for the urban area of Rennes in Figure 2.

As the allocation to B and C area was realized at the municipality level, observing housing blocks and controlling for their observables characteristics is a source of quasi randomization of the treatment. We consider this level of observation as an important strength of our identification strategy.

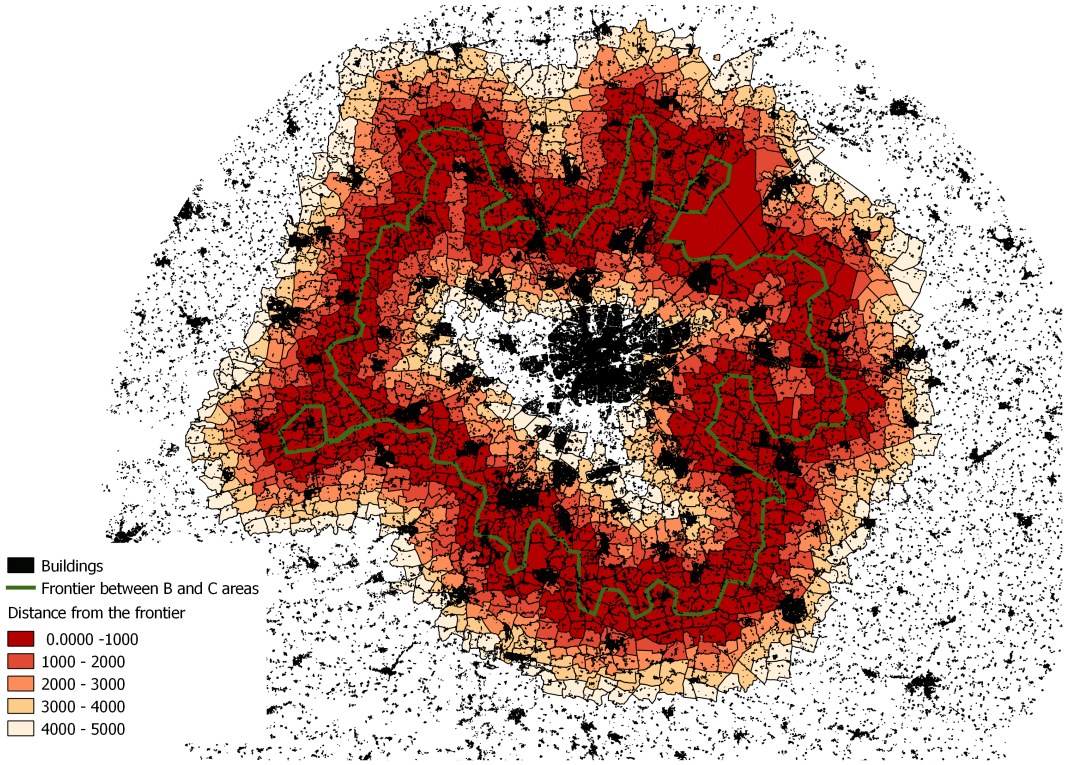

FIGURE 2. DISTANCE FROM B/C BOARDER IN RENNES URBAN AREA

Source: Authors' computations from IGN GEOFLA(R) and French Ministry of Housing

IV. Empirical Strategy

The methodologies used in our paper build on the previous literature that evaluated the impact of place-based policy using fine-resolution spatial data. The most recent and innovative methods were used in papers assessing the impact of enterprise zones (see Neumark and Simpson (2015) or Gobillon, Magnac and Selod (2012) for a review).

We follow Bono and Trannoy (2012) and the strategy used in Einio and Overman (2012) by exploiting the geographical variation of the Scellier Tax Credit in a difference-in-difference framework.

A major challenge of the literature exploiting geographical variation to evaluate the impact of place-based policies, is to select appropriate control groups. The idea is to select control areas that are similar to treated areas but where the policy was not applied. The most recent research tried to construct reliable control groups by using very detailed geographic information on narrow areas. For example, Bono and Trannoy (2012) used municipalities in B2 areas (eligible to the STC) contiguous to municipalities in C areas (non-eligible to the STC). The rationale of choosing control groups that

border treatments areas is that economic conditions and unobservable characteristics are likely to be similar, aside from the effects of the policy, between two contiguous areas.

Nevertheless, it is important to account for potential displacement effects between treated areas and nearby control areas as in Einio and Overman (2012) and Neumark and Kolko (2010). To account for this, we use the method developed in Einio and Overman (2012) and examine the distribution of changes in the outcomes of interest, by splitting the treatment and control zones into one-km-wide areas based on the distance from the treatment area frontier.

A. *The choice of a counterfactual: comparing B and C areas close to the frontier*

To assess the impact of tax credits, we consider that the fact that C areas stop benefiting from fiscal incentives for rental investment similar to the Scellier Tax Credit (the Borloo-Robien), is the treatment. The goal of the identification strategy is to find a convincing counterfactual for these areas. We thus exploit the fact that B areas close to the treatment frontier, that continued to benefit from these kind of tax incentive, had relatively close characteristics to C areas. In contrast, A areas which are considered too dissimilar, are excluded from the analyses, because they included metropolitan areas such as Paris and its surroundings, which presented a high level of market tightness.

There was no precise rule to define B and C areas, but the delimitation took into account the average rent and the share of household receiving housing allowances. As we compare housing blocks on both side of the frontier, one key assumption is that observable and unobservable characteristics vary smoothly across the frontier.

As emphasized by Neumark and Kolko (2010) and Duranton, Gobillon and Overman (2011), using control groups geographically close to treatment groups could be an interesting strategy to deal with the unobserved characteristics that could vary between C and B areas.[10] The idea is that unobserved local characteristics affecting the outcomes vary smoothly across nearby locations as observables illustrated in Figures D1 and D2 in the Appendix. Indeed, nearby locations are at equal distance of amenities, share the same local labor market, experience the same shocks : the smaller the distance, the more likely the assumption.

[10] In the absence of treatment, the outcome of the treated and control groups would have been different because of these unobserved characteristics which would invalidate the conditional treatment ignorability.

In addition, for any unit, the potential outcomes must be independent of treatment assignment once we condition on observed covariates. This assumption is sometimes called the conditional treatment ignorability or unconfoundedness. We will thus control for observed characteristics of each section as we do with X_{it} in equations (1) and (2).

Following Bono and Trannoy (2012), we perform our estimations restricting our sample to the blocks or the transactions 5 km around the border. This a simple difference-in-difference estimate, taking B and C areas close to the treatment boarder respectively , as control and treated groups.

We estimate the following equation on blocks for the size of the housing stock[11] and the characteristics of new dwellings (the share of owner-occupiers, the income of tenants and the vacancy rate):

$$Y_{it} = \lambda_i + \mu_t + \delta^{tr} D_{it} + \beta X_{it} + \gamma Z_{i,2005} \times \mu_t + \epsilon_{it} \tag{1}$$

where Y_{it} and X_{it} are respectively the outcome variable and the controls for the housing block i at time t. λ_i is the housing block fixed effect and μ_t is the time fixed effect. δ^{tr} is the estimated treatment effect, D_{it} is taking value one when the section is treated in the post-treatment period i.e. when the STC was implemented and C areas stopped benefiting from the tax credit. Housing blocks belonging to the same urban areas could be affected by the same shocks, introducing a potentially time-varying urban area component in ϵ_{it}. Following Angrist and Pischke (2008) and Bertrand, Duflo and Mullainathan (2004), we cluster the residuals by urban areas to allow for maximum flexibility in the variance-covariance matrix of residuals. $Z_{i,2005} \times \mu_t$ interact the observed characteristics of the blocks at the beginning of the period with time dummies.

As observed in Table C3, before the policy change, B and C areas close to the B/C frontier had different characteristics, B areas were more densely built, with richer households, more flats occupied by private and social tenants. We thus control for the size of the housing block, as more densely built area offer less land for new developments. Without these controls, we could underestimate the impact of the removal of the Robien-Borloo policy. To control for the observed and unobserved heterogeneity we use two complementary methods. First, we have fixed effects at the housing block level which allow us to control for the heterogeneity between B and C areas.

[11] Following the housing stock instead of housing starts allow us to avoid the methodological problems connected with having an important number of zeros which would lead us to use Poisson regression or related approaches.

Second, we also add time varying controls which are mainly the average income and the average space per inhabitant in the housing block. Finally, $Z_{i,2005} \times \mu_t$ allows for heterogeneous trends following the initial characteristics of the blocks as explained in the next section.

As far as housing prices are concerned, we apply the identification strategy in an hedonic regression framework, following Rosen (1974), that we adapt to the difference-in-difference estimator. We carry our estimations at the transaction unit level because in our sample, there are a lot of cadastral blocks without transaction during the period. We regress the log of the price $ln(p_{j,t})$ of transaction j at time t on a set of hedonic characteristics (X_j), a municipality fixed effect ($\theta_{k(j)}$) to control for local unobserved variables, a year fixed effect (μ_t) and a treatment indicator (D_{jt}) with value one when the transaction took place in the unsubsidized area after the first of January 2009. As price varies with the distance to the frontier, we test two specifications to control for this, using distance to the frontier or rings fixed effects. Both specification yield similar results. However, we prefer the ring specification as it allows controlling for the asymmetrical effect of the distance to the frontier. Standard errors are clustered at the municipality level. We thus estimate the following equation:

$$ln(p_{j,t}) = \theta_{k(j)} + \mu_t + \delta^{tr} D_{jt} + \beta X_{jt} + \epsilon_{jt} \tag{2}$$

B. *The common trend assumption*

To be able to identify the average treatment effect, several assumptions must hold. The first one, is the fact that in the absence of treatment both treated and untreated units would have evolved along the same temporal path. To have treatment and control groups which are comparable (unconfoundedness hypothesis) and follow the same trend before the treatment date, we discard observations located more than 5 km from the border between C and B areas.[12]

To give credit to the common trend assumption, we will perform placebo tests estimating equations (1) and (2) until the 1st of January 2009, and considering that the placebo treatment could have started in 2005 or 2007 for equation (1), or in 2008 for equation (2). We consider that the common trend assumption is indirectly satisfied if

[12] For most of the observable characteristics, such as the income of households living in new and existing dwellings, the surface per person and number of person per household, the share of vacant dwellings in existing dwellings, and the number of new dwellings built, the difference between B control areas and C treated areas is less important when restricting the sample between 1 and 5km away from the treatment boundary than by comparing all C areas to all B areas (see Table C3 in the Appendix).

the treatment dummies for the pre-treatment periods are not significant. As noted in equation (1), we allow for heterogeneous trends due to different initial characteristics between B and C areas by interacting control variables in 2005 with time dummies. The controls interacted are the housing stock size, the share of vacant dwellings, the share of owner-occupied dwellings, the share of flats and of housing units built between 1949 and 1970. These variables can explain heterogeneous trends resulting from the initial physical characteristics of the urban environment, which are correlated with their attractiveness and their capacity to receive new housing units. For example, larger or denser housing blocks might not have the same trends in terms of growth of the housing stock or house prices compared to smaller housing blocks, whether they are treated or not. The homeownership rate controls for the fact that housing blocks with more homeowners might be more regulated and have less housing units built (see Fischel (2001)). It is worth noting that our methodology does not allow controlling for heterogeneous trends due to unobserved characteristics.

C. *The importance of the SUTVA assumption*

The last assumption of particular importance is the single unit treatment value assumption (SUTVA), according to which there should not be interference or spillover effects between treated and control groups. In other words, the treatment should not affect the outcome of the control group. This problem is of particular importance in the evaluation of place-based policies, given that spatial spillovers are likely to arise: developers could easily displace a project to another municipality in order to benefit from the demand induced by the tax credit. We clearly see a trade-off arising when using our geographical approach: adopting areas closer to the border as control and treatment groups, increase the comparability between both groups, but also the likelihood to violate the SUTVA assumption.

If there is a substitution effect at the border, the average treatment effect estimated is biased. To avoid this issue, one solution applied in Kline and Moretti (2014) is to drop the nearest untreated locations from the control group. However, the choice of the buffer of untreated areas to drop is arbitrary. To investigate potential displacement effects and choose which buffer to take, we follow Einio and Overman (2012) in their non parametric approach, by interacting the treatment with ring dummies. Our treatment effect will be the effect with respect to a reference ring. To assess whether there are spillover effects between B and C areas near the Scellier area border, we augment equation (1) with dummy variables for 1 km wide control and treatment rings that run

parallel to the Scellier boundary. The equation estimated becomes:

$$Y_{it} = \lambda_i + \mu_t + \sum_{k=1}^{5} \delta_k^{co} CO_{it}^k + \sum_{h=1}^{5} \delta_h^{tr} TR_{it}^h + \beta X_{it} + \gamma Z_{i,2005} \times \mu_t + \epsilon_{it} \quad (3)$$

where Y_{it} and X_{it} are respectively the outcome variable and the controls as before. CO_{it}^k are the set of 5 one-km-wide control ring dummies, they are equal to one if the distance to the Scellier area border (i.e. the distance to the nearest treated housing block) is between $k-1$ and k kilometers in the post-treatment period, and zero otherwise. Symmetrically, we define TR_{it}^h the 5 treatment ring dummies. We represented the rings in Figure 2 for the urban area of Rennes. We restrict our sample to housing blocks 5 km away from the STC frontier at the maximum, which represent 40% of the B and C areas (see Table C1 in Appendix C).

Equivalently, this gives for housing prices:

$$ln(p_{j,t}) = \theta_{k(j)} + \mu_t + \sum_{k=1}^{5} \delta_k^{co} CO_{it}^k + \sum_{h=1}^{5} \delta_h^{tr} TR_{it}^h + \beta X_{jt} + \epsilon_{jt} \quad (4)$$

In equations (3) and (4), $\delta_h^{tr} - \delta_k^{co}$ is the difference in the average conditional growth rate for the outcome of interest between treatment ring at distance h and control ring at distance k. However, this difference identifies the treatment effect of the removal of the Borloo-Robien only if the unconfoundedness and the single unit treatment value assumption (SUTVA) hold. Nevertheless, the unconfoundedness assumption is more likely to hold the closer from the treatment border if characteristics vary smoothly across space. Yet, displacement effects are more likely to increase closer to the border. For example, if the treatment effect is negative, evidence of displacement would be seen if $\delta_k^{co} - \delta_h^{tr}$ is higher near the border, δ_k^{co} would decrease and δ_h^{tr} would increase further away from the frontier if the displacement effect is only present around the border in the B area.

We use this approach to verify that the single unit treatment value assumption (SUTVA) holds, and drop the rings where we find evidence of displacement effects.

V. Results

A. *The Impact of the Removal on the Housing Stock*

We investigate the impact of the removal of the Borloo-Robien tax credit on the (log of) housing stock at the housing block level. Focusing on the stock[13] instead of new units discard our concerns about the way to deal with an important amount of zeros.

First of all, we carry our estimations on all the observations located between 0 and 5 km around the border between areas C and B to have comparable areas. However, the geographical pattern of our treatment effect, as displayed in Figure 3, reveals a displacement effect in the area between 0 and 1 km around the border, since all the other rings experienced a lower growth of the housing stock since the removal of the tax credit. In other words, we see an evidence of a growth peak for areas eligible to the Scellier Tax Credit which are within 1 km from the treatment boundary. To verify this conclusion, we took the 1-2 km control ring as the reference which makes the peak appears clearer (Figure 4). Therefore, we drop the 0-1 km B area ring in our estimation sample here and thereafter, since a displacement of the building of dwellings could impact all the other characteristics of the local housing markets.

Our preferred specification is in column (6) of Table 2, where the treatment is the removal of the Borloo-Robien tax credit in 2009 and we control for several characteristics of the local housing market, and drop the buffer area where displacement happened. We find no impact of the removal of the tax credit on the growth of the private housing stock in C areas compared to B areas. When controlling by observable characteristics, there is no evidence of a break of the common trend assumption (the coefficient is not significant for placebo treatments). When comparing the B areas to the C areas in Table 1 without removing the buffer area, we overestimated the impact of the removal of the tax credit on housing supply growth because of the displacement effect around the border. As expected, not controlling for the number of dwellings in the housing block lead us to overestimate the impact of the removal as the blocks outside the frontier in C areas have usually more developable land available.

Compared to preceding studies on the Robien law, Brest (2008) and Rigaud, Gay and Barthélemy (2008), which estimated that between 11 and 17% of the total new dwelling construction in their respective regions (Brest and Rhône-Alpes) was due to

[13] The variation of the total housing stock depends mainly on new constructions but also marginally on demolitions, flats merging or splitting.

Table 1 – Impact of the Removal on the Housing Stock (within 0 and 5km from the Treatment Boundary)

	Placebo				STC	
Treatment period	2005-2009		2007-2009		2009-2013	
	(1)	(2)	(3)	(4)	(5)	(6)
Removal	0.005**	0.002	0.004**	0.001	−0.008	−0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.006)	(0.003)
Controls	No	Yes	No	Yes	No	Yes
Observations	190230	190230	190230	190230	317050	317050
No. of sections	63410	63410	63410	63410	63410	63410
R^2 within	0.14	0.16	0.14	0.16	0.18	0.20
Estimation Period	2005-2009				2005-2013	

Note: In this panel fixed-effects regression, the dependent variable is the log of the stock of private housing. The treatment variable is the interaction between a group dummy (equals to 1 for the treated group, 0 otherwise) and a time dummy (equals to 1 for the post-treatment period, 0 otherwise). Time fixed effects are included for all the regressions. Controls include the household revenues, the share of owners, the number of dwellings, the average square meters per person, the share of apartments, the share of dwellings built between 1949 and 1970. All the controls are for 2005 and are interacted with time dummies except the household revenues and the average square meters per person which are contemporaneous. The standard errors are clustered by 1999 urban areas.

Sample: Balanced panel for all the odd years between 2005 and 2013. The observations are all cadastral blocks where we always observe our control and dependent variables.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

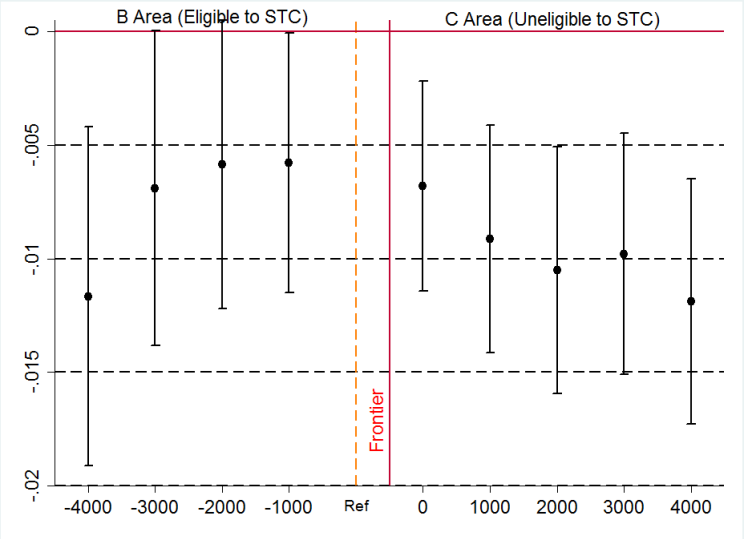

FIGURE 3. THE EFFECT OF THE REMOVAL ON THE HOUSING STOCK AROUND THE BOUNDARY

Difference-in-difference estimates by 1km-wide control and treatment rings. The outcome is the log of the private housing stock. The reference category is the control ring which is within 1km from the treatment boundary.
Sample: Balanced panel for all the odd years between 2005 and 2013. The observations are all cadastral blocks where we always observe our control and dependent variables.

the Robien policy, we find an important windfall effect, the housing stock would have kept on growing at the same pace (reported in Figure D4) without the policy.

Most of the papers analyzing the impact of tenant-based subsidies documented a strong inflationary impact, tending to confirm such a low elasticity. For example, Grislain-Letrémy and Trevien (2014), Fack (2006) or Laferrère and Le Blanc (2004) demonstrated that housing benefits, which represent the most important public spending in the rental sector,[14] had a strong and positive impact on rents. Labonne and Welter-Nicol (2015) showed that subsidized loans for new homeowners, also generated house price increases. Furthermore, Chapelle (2015) has been documenting an important crowding effect of private construction by the non-profit sector which might be caused by competition for both land and tenants. Finally, on the Scellier Tax Credit per se, Bono and Trannoy (2012) showed that it had a strong impact on land prices, and thus did increase the competition for land.

To conclude, for areas with similar characteristics, the end of the tax credit had no impact on the housing supply growth, the housing stock would have kept on growing

[14] About 18 billions of euros (43% of the public spending in the housing sector in 2014).

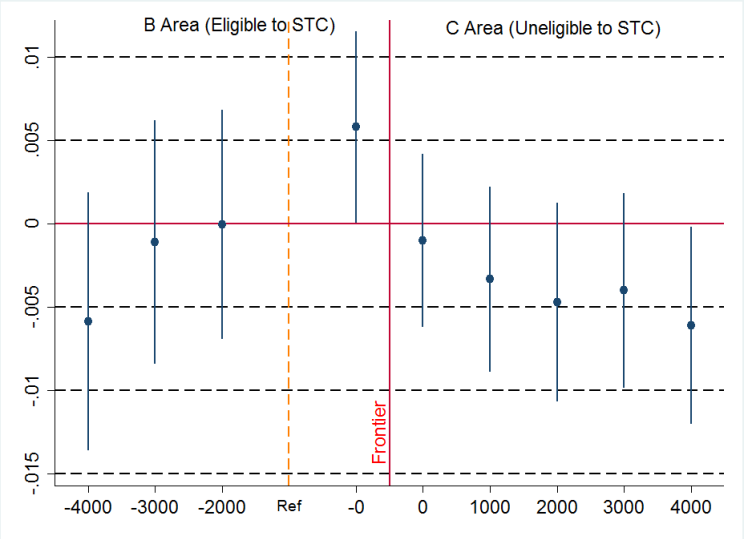

FIGURE 4. THE EFFECT OF THE REMOVAL ON THE HOUSING STOCK AROUND THE BOUNDARY (1-2 KM REFERENCE)

Note: Difference-in-difference estimates by 1km-wide control and treatment rings. The outcome is the log of the private housing stock. The reference category is the control ring which is within 1 and 2 km from the treatment boundary.
Sample: Balanced panel for all the odd years between 2005 and 2013. The observations are all cadastral blocks where we always observe our control and dependent variables.

at the same pace without the tax credit. This suggests that tax credits have a strong windfall effect even in relatively elastic areas. One limit of our interpretation is the fact that our estimates might be biased by the inertia of the housing market: projects started under the Borloo-Robien tax credit in C areas might have been finished only after the removal of this policy. However, this concern is limited since the average time to construct a new dwelling between 1990 and 2010 has been between 13 and 17 months in France (see Boutier (2012)).

In order to contribute further to the literature, we also investigate the impact of the removal of the Borloo-Robien policy on several other dimensions of local housing markets such as the vacancy rate of new dwellings, the house price, the share of owners occupiers and the income of new tenants.

B. *The Impact of the Removal on the Vacancy Rate of New Dwellings*

Another important concern regarding tax credits policies was their potential impacts on the vacancy rate of new dwellings. One can expect that new investors attracted by tax credits would invest in eligible areas and try to find tenants to benefit from the tax credit. If the number of potential tenants was low, because the government

Table 2 – Impact of the Removal on the Housing Stock (within 1 and 5km from the Treatment Boundary)

	Placebo				STC	
Treatment period	2005-2009		2007-2009		2009-2013	
	(1)	(2)	(3)	(4)	(5)	(6)
Removal	0.008***	0.003	0.007***	0.003	0.006	−0.001
	(0.003)	(0.003)	(0.002)	(0.003)	(0.004)	(0.004)
Controls	No	Yes	No	Yes	No	Yes
Observations	127428	127428	127428	127428	212380	212380
No. of sections	42476	42476	42476	42476	42476	42476
R^2 within	0.14	0.15	0.14	0.15	0.17	0.20
Estimation Period	2005-2009				2005-2013	

Note: Panel fixed-effects regression where the dependent variable is the log of housing stock. The treatment and controls are the same as before. Time fixed effects are included for all the regressions. The standard errors are clustered by 1999 urban areas

Sample: Balanced panel for all the odd years between 2005 and 2013. The observations are all cadastral blocks where we always observe our control and dependent variables. This sample covers about 8 millions of housing units while there are about 30 millions of housing units in France.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

misperceived the tension on the local housing market, the vacancy rate could increase. In the US, such a concern was raised by McClure (2006, 2012) who found that, between 2000 and 2004, over 90% of subsidies went to neighborhoods where there was a surplus of dwellings, i.e. areas where there were more units in the price range of a LIHTC project than tenants in the relevant income category. Such a criticism was also raised against the Borloo-Robien tax credit and partly justified the end of the tax credit in the C areas.

Our results go in the same direction. Table 3 shows that vacancy rate among new dwellings increased less in C areas compared to the B areas eligible to the Scellier (between 1 and 5 km around the border). More precisely, the share of the newly built private dwellings that were vacant increased by 1.6 percentage points more in the B areas eligible to the Scellier compared to C areas (see column (6)).

When applying our non parametric estimation, the spatial patterns display a robust drop of the vacancy rate among new dwellings in C areas after 2009 (see Figure D3 in Appendix), confirming the findings in Table 3.

A lack of demand for new rental units in the areas eligible to the tax credit could explain these patterns. However this fact might be puzzling given that we did not find any impact of the removal on the growth of the stock of housing units. One

Table 3 – Impact of the Removal on the Vacancy Rate of New Dwellings (within 1 and 5km from the Treatment boundary)

	Placebo				STC	
Treatment period	2005-2009		2007-2009		2009-2013	
	(1)	(2)	(3)	(4)	(5)	(6)
Removal	−0.000	0.002	−0.001	0.003	−0.017***	−0.016***
	(0.004)	(0.005)	(0.004)	(0.005)	(0.003)	(0.003)
Controls	No	Yes	No	Yes	No	Yes
Observations	22392	22392	22392	22392	37320	37320
No. of sections	7464	7464	7464	7464	7464	7464
R^2 within	0.0018	0.0029	0.0018	0.0029	0.0027	0.0035
Estimation Period	2005-2009				2005-2013	

Note: Panel fixed-effects regression where the dependent variable is the vacancy rate of new dwellings built. The treatment and controls are the same as before. Time fixed effects are included for all the regressions. The standard errors are clustered by urban areas.

Sample: Balanced panel for all the odd years between 2005 and 2013. The observations are all cadastral blocks where there was always at least one new unit built every two year between 2005 and 2013.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

complementary explanation could come from the impact of the law on the use of dwellings. In the next section, we find that, after the removal, the number of owner occupied dwellings increased at the expense of the private rental sector in C areas. As the private tenants sector have a higher turnover than owner occupiers, a drop in the frictional vacancy rate could explain this phenomenon.

C. *The Effects of the Removal on House Prices*

We now turn to the impact of the removal of the Borloo-Robien policy on house prices. There are much less observations on dwelling transactions than for the housing stock since a lot of housing blocks have no transactions at all or just a few ones, especially in rural areas. As a consequence, we perform our estimations at the transaction level and not at the housing block level for this part. For the same reason, we use municipality fixed effect and not housing block fixed effect. Interestingly, this method let us introduce control variables related to the characteristics of sold dwellings at the housing unit level, in order to take into account in a better way, structural differences between them.

Our results on individual dwellings (i.e. houses) are more robust than for flats. Indeed, in France, flats are more concentrated in urban units and our strategy could be

less appropriate to evaluate the impact of the removal of the tax credit on the price of apartments.

We estimate a simple difference-in-difference regression, completed by our geographical identification strategy. In Table 4, the results for houses show no significant pre-trends and a significant decrease in the growth rate of house prices in the areas where the tax credit was removed compared to the areas where it was maintained. Prices' growth dropped by approximately one percentage point more in C areas. These results are confirmed by the non parametric approach reported in Figure D3 in the Appendix. Such effect is sizable when compared with house price trends reported in figure D4, it is of the same size as the price variation between 2010 and 2012 for the whole sample.

The results on flat prices are mixed close to the treatment frontier, we found that the removal had not a significant different impact on prices' growth between B and C areas (see columns (1) and (3) of Table 4). However, Figure D3 shows a peak 10km away from the frontier. As these results are estimated with about half the number of observations when compared with houses, the urban concentration of housing transactions in the B area could explain this peak.

The results on house prices confirm those of Bono and Trannoy (2012), who found that the Scellier Tax Credit was essentially capitalized in land prices. This could be explained by physical and institutional constraints on land release, and consequently by the low housing supply elasticity.

A first puzzle comes from the fact that we observe an impact on house price growth without any movement in the growth of the quantities produced. The results of the next section suggest that poorer homeowners might have been substituted by richer investors with higher willingness to pay. For example, if households invest in areas with a limited knowledge of the local market (for example, a Parisian household buying a Scellier good in a suburban area of another city), housing developers can set a higher price for their dwellings. The asymmetry of information due to the specific characteristics of the investors concerned by the Scellier Tax Credit could explain such patterns.

Another puzzle is the fact that a program such as the tax credit (or its removal) had an impact on prices of existing units. This can be easily understood if we refer to the replacement cost framework when studying the housing market. In such a framework, the arbitrage of households between new and existing units, lead us to split the value

of a unit between its structure and its land component. If such an arbitrage exists, we can think that an increase in building plot prices will tend to be capitalized into the land component of existing units.

Finally, our identification strategy rely on a zoning that is also used for the "zero interest loan" policy, our estimates might be biased by the overlap of both policies. However, as explained in Labonne and Welter-Nicol (2015), this is highly unlikely as no change were brought to the zero interest loan policy over our period of study.

Table 4 – Impact of the Removal on House Prices (within 1 and 5 km from the Treatment Boundary)

	Log of the price per square meters			
	Placebo		STC	
	(1)	(2)	(3)	(4)
Removal	0.0113	-0.00147	-0.00500	-0.00985***
	(1.52)	(-0.36)	(-0.95)	(-3.27)
N	101685	133101	185787	233149
R^2	0.5635	0.4841	0.5693	0.4705
Type of goods	Flats	Houses	Flats	Houses
Estimation Period	2006-2008		2006-2012	

Note: Hedonic regression where the dependent variable is the log of the transaction price. The treatment and controls are the same as before. Rings and time fixed effects are included for all the regressions. We control for all hedonic characteristics available: log of the surface and its square, presence of an elevator, number of rooms, number of bathroom, presence of a basement, presence of a garden, presence of a balcony, period of construction. We also add fixed effects for municipalities. The standard errors are clustered at the urban area level.
Sample: All observed transaction of even years between 2006 and 2012.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

D. *Impact of the Removal on the Rental Sector*

1. On the Composition of the Local Housing Market

Since the goal of the Scellier Tax Credit was to promote rental property investments, we expect to see an acceleration of the number of tenants or conversely a drop of the homeownership rate in newly built dwellings in areas eligible to the Scellier Tax Credit compared to areas that were not. We focus on new units as they were the only ones eligible to the Scellier Tax Credit.

As it can be seen in Table 5, there was a significant rise in the homeownership rate (6 percentage points) in C areas compared to B areas after the removal of the Borloo-Robien tax credit. This is confirmed in Table D1, who shows a supplementary decrease of 16 percentage points of the number of tenants in new dwellings in C areas compared to the B areas eligible to the Scellier Tax Credit.

Since the removal of the tax credit had no different impact on the growth of the housing stock in B and C areas, this effect is a composition effect. New units potentially bought by owner-occupiers were eventually bought for investment purposes and occupied by tenants in the B areas. This could be explained by the inflationary impact of the Scellier Tax Credit on the second-hand housing stock, which would reduce the housing purchasing power of households who lived in B areas which remained eligible to the Scellier Tax Credit.

Table 5 – Impact of the Removal on the Share of Owners in New Dwellings (within 1 and 5km from the Treatment Boundary

	Placebo				STC	
Treatment period	2005-2009		2007-2009		2009-2013	
	(1)	(2)	(3)	(4)	(5)	(6)
Removal	0.015**	0.012	0.014*	0.014	0.064***	0.059***
	(0.008)	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)
Controls	No	Yes	No	Yes	No	Yes
Observations	22392	22392	22392	22392	37320	37320
No. of sections	7464	7464	7464	7464	7464	7464
R^2 within	0.01	0.01	0.01	0.01	0.01	0.01
Estimation Period	2005-2009				2005-2013	

Note: Panel fixed-effects regression where the dependent variable is the share of owners in new dwellings. The treatment and controls are the same as before. Time fixed effects are included for all the regressions. The standard errors are clustered by urban areas.

Sample: Balanced panel for all the odd years between 2005 and 2013. The observations are all cadastral blocks where there was always at least one new unit built every two year between 2005 and 2013.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

2. On the Income Profile of Tenants in New Dwellings

So far, we found that the removal of the Borloo-Robien policy did not decelerate housing supply in C areas compared to B areas, when displacement effect are properly controlled for, and newly built dwellings were less vacant in C areas. Our results also indicate that the removal of the tax credit had a deflationary impact on house prices in C

areas compared to B areas. Taking all together, these results point towards a substantial inefficiency of the housing tax credits policies that promote rental investments.

Nevertheless, despite these negative results, as Malpezzi and Vandell (2002) underline, a policy such as the LIHTC or the Scellier Tax Credit would still be socially useful if it succeeded in creating more mixed income housing. We now investigate, if effectively, the removal of the Borloo-Robien tax credit had a negative impact on the accommodation of lower-income households.

As showed in Table 6, the removal of the tax credit policy had no different impact on the growth of the average income of tenants in new dwellings in C areas compared to B areas. The treatment coefficient is not significant.

In line with our results, McClure (2000) observed that the LIHTC program in the US did not deliver mixed income housing, and Eriksen and Rosenthal (2010) showed that LIHTC served families with much higher incomes than other housing policies like vouchers.

Table 6 – Impact of the Removal on the Tenant's Income in New Dwellings (within 1 to 5km from the Treatment Boundary)

	Placebo				STC	
	2005-2009		2007-2009		2009-2013	
	(1)	(2)	(3)	(4)	(5)	(6)
Removal	−0.012	−0.025	0.015	0.027	−0.003	−0.009
	(0.022)	(0.024)	(0.021)	(0.024)	(0.016)	(0.018)
Controls	No	Yes	No	Yes	No	Yes
Observations	26800	26775	26800	26775	43926	43851
No. of sections	17547	17528	17547	17528	22326	22273
R^2 within	0.002	0.004	0.002	0.004	0.009	0.011
Estimation Period	2005-2009				2005-2013	

Note: Panel fixed-effects regression where the dependent variable is the log of the income of renters in new dwellings. The treatment and controls are the same as before except we do not control for average household income. Time fixed effects are included for all the regressions. The standard errors are clustered by urban areas.

Sample: Unbalanced panel for all even years. The observations are all cadastral blocks where there was at least once a tenant in a unit recently built.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

VI. Conclusion

Our results cast some doubts on the efficiency of the French system of tax credits to promote rental investments. In our specification accounting for spillover effects, we do not find any evidence of an impact of the removal of the Borloo and Robien policies on the evolution of the housing stock for similar areas close to the treatment boundary. The impact of the removal of the tax credits suggests, similarly to the results on the Low Income Housing Tax Credit in the US, that the windfall effect could be substantial in the case of the Scellier Tax Credit.

Our results which concern B and C areas, which are mostly located at the periphery of urban areas, can be seen as an upper bound effect on quantities in absolute terms (and lower bound in absolute terms on prices), because these areas are believed to have a more elastic housing supply, according to the Scellier zoning.

In addition, we find that the removal of the tax credits decreased the share of vacant dwellings among the new dwellings built in C areas compared to B areas, and had a deflationary impact on prices of existing dwellings in C areas compared to B areas, confirming the findings of Bono and Trannoy (2012). Those results point towards an inadequacy of the Scellier zoning, which might have had a counterproductive impact with regard to its initial aim of correcting disequilibrium between housing supply and demand.

The removal of the tax credits also had an impact on the composition of local housing markets, since homeownership grew at the expense of rental dwellings in C areas compared to B areas. In addition, new tenants arriving in dwellings eligible to the Scellier Tax Credit, did not have a significantly lower income than tenants in non-eligible areas. The Scellier Tax Credit failed to achieve a social goal of accommodating low-income households, which was not surprising given that most households could access such units.

Our results follow closely the previous findings of Grislain-Letrémy and Trevien (2014), Fack (2006) or Labonne and Welter-Nicol (2015), which highlight that low supply elasticity reduces the efficiency of housing policies.

To put in a nutshell, our study confirms that nationwide policies uncoordinated with local land use regulation, will tend to generate undesirable effects, as emphasized in Wasmer (2016).

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Appendix A The Scellier Law

The exact text referring to the Scellier Tax Credit is the following:

"L'article 31 de la loi de finance rectificative pour 2008 (numéro 2008-1443 du 30 décembre 2008) réforme les mécanismes d'incitation fiscale à l'investissement locatif. Cette réforme consiste à supprimer à compter du 1er janvier 2010, les dispositifs "Robien" et "Borloo" et à les remplacer par un avantage prenant la forme d'une réduction d'impôt sur le revenu. Cette réduction d'impôt sur le revenu s'applique, à compter du 1er janvier 2009, aux contribuables domiciliés en France qui acquièrent ou font construire des logements neufs dans certaines zones du territoire se caractérisant par un déséquilibre entre l'offre et la demande de logements, qu'ils s'engagent à donner en location nue à usage d'habitation principale pour une durée minimale de neuf ans. Au titre d'une même année d'imposition, un seul logement peut ouvrir droit à la nouvelle réduction d'impôt. L'acquisition du logement, ou le dépôt de la demande de permis de construire dans le cas d'un logement que le contribuable fait construire doit intervenir au plus tard le 31 décembre 2012. La réduction d'impôt s'applique également aux contribuables qui souscrivent, entre le 1er janvier 2009 et le 31 décembre 2012, des parts de sociétés civiles de placement immobilier (SCPI) réalisant ces mêmes investissements. La réduction d'impôt est calculée sur le prix de revient du logement ou le montant des souscriptions, dans la limite annuelle de 300 000 euros. Son taux est fixé à 25 % pour les investissements réalisés en 2009 et 2010 et à 20 % pour ceux réalisés en 2011 et 2012. Elle est répartie sur neuf années, à raison d'un neuvième de son montant chaque année. Lorsque la location est consentie dans le secteur intermédiaire, le contribuable bénéficie, en plus de la réduction d'impôt, d'une déduction spécifique fixée à 30 % des revenus bruts tirés de la location du logement. Lorsque le logement reste loué dans le secteur intermédiaire après la période d'engagement de location, le contribuable bénéficie, par période de trois ans et dans la limite de six ans, d'un complément de réduction d'impôt égal à 2 % par an du prix de revient du logement. Pour les investissements réalisés en 2009, le contribuable peut choisir entre les dispositifs dits "Robien" et "Borloo" et la nouvelle réduction d'impôt, sans toutefois pouvoir cumuler ces avantages au titre d'un même investissement."

Appendix B Information on the STC

As illustrated in Table B1, the STC is subject to a maximum rent which varies according to the type of fiscal rebate (regular or intermediate), the area and the year of investment.

Table B1 – Maximum Monthly Rents per square meter for the STC

	Regular		Intermediate	
	2009-2010	2011-2012	2009-2010	2011-2012
Abis	-	21.70	-	17.36
A	21.84	16.10	17.47	12.88
B1	15.19	13	12.15	10.40
B2	12.42	10.40	9.94	8.48

Note: All the values are expressed in euros.

In addition, while the potential tenants for a regular STC are not subject to any income limit, the tenants of the intermediate STC should have an income below a certain limit, described in Table B2, when signing the contract. It is worth noting that such an income constraint is not very binding, since we can easily infer that more than 70% of the households fulfill these criteria. Indeed, these income limits are well above the income limit of one of the less restrictive social housing category (the "Prêt Locatif Social" or PLS), for which Chapelle (2015) reports that more than 70% of the households were eligible in 2010.

Table B2 – Maximum Yearly Income for the Intermediate STC

	Area		
	A	B1	B2
Single	44793	33272	30500
couple	66943	48860	44789
single or couple with 1 child	80471	58493	53619
single or couple with 2 children	96391	70790	64891
single or couple with 3 children	114109	83085	76163
single or couple with 4 children	128402	93720	85911
per additional child	+14312	+10646	+9758

Note: All values are expressed in euros.

Appendix C Descriptive Statistics on blocks

Table C1 – Distance to the B/C frontier of blocks by Area

	Area		
Distance (km)	B	C	Total
0-1	10,406	10,733	21,139
1-2	7,126	5,742	12,868
2-3	6,262	5,041	11,303
3-4	5,047	4,877	9,924
4-5	4,017	4,667	8,684
5-6	3,025	4,443	7,468
6-7	2,290	4,416	6,706
7-8	1,645	4,385	6,030
8-9	1,423	4,472	5,895
9-10	1,268	4,413	5,681
10-11	1,016	4,329	5,345
11-12	979	4,199	5,178
12-13	751	4,357	5,108
13-14	583	4,172	4,755
14-15	517	4,187	4,704
15-16	500	4,165	4,665
16-17	444	4,138	4,582
17-18	465	4,165	4,630
18-19	452	4,041	4,493
19-20	463	3,863	4,326
20-21	386	3,753	4,139
21-22	325	3,699	4,024
22-23	313	3,479	3,792
23-24	309	3,318	3,627
24-25	315	3,251	3,566
Total	50,327	112,305	162,632

Source: Author's computations.

Note: The values refer to the number of cadastral blocks present in each ring.

Table C2 – Summary statistics of Area B and C

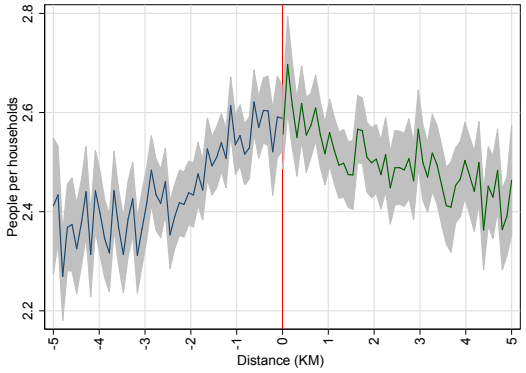
Variable	Mean	Std. Dev.	N
Existing stock			
Number of dwellings	115.9	271.902	220051
Income	19646	24275.586	219293
Persons per household	2.243	0.598	219870
Vacancy rate	0.083	0.086	219742
Share of owners	0.779	0.181	219880
Share of renters of private dwellings	0.137	0.128	219880
Share of social renters	0.037	0.117	219880
Consumption unit	2.73	11.255	220051
New dwellings			
Income	16940	22313.361	101866
Persons per household	2.522	1.267	101901
Vacancy rate	0.059	0.184	100039
Share of owners	0.762	0.348	100039
Share of renters of private dwellings	0.179	0.304	100039
Share of social renters	0.019	0.118	101903
Distance from B/C frontier	16998	15904	220358

Table C3 – Summary statistics on Areas B and C between 1 and 5 km from the Treatment Boundary

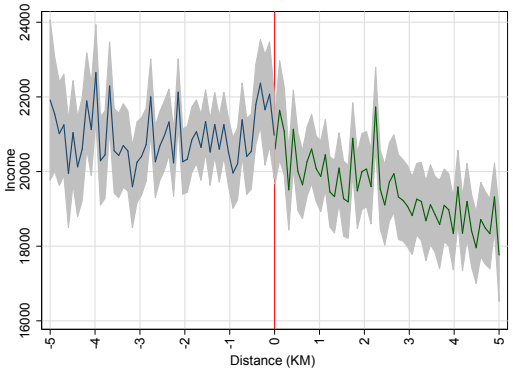
	B Areas		C Areas		Comparison	
	Average	Obs	Average	Obs	Difference	T-test
Existing Dwellings						
Number of units	255.47	25391	84.05	24721	171.42	63.51
	(398.83)		(146.91)			
% of flats	0.29	25391	0.10	24721	0.20	86.60
	(0.32)		(0.16)			
% built in 1949-1970	0.20	25391	0.10	24721	0.10	65.06
	(0.20)		(0.12)			
Surface per person	45.61	25378	47.85	24677	−2.24	−27.44
	(8.46)		(9.75)			
Income	20770.10	25371	19086.42	24651	1683.68	22.39
	(9513.55)		(7089.33)			
People per household	2.42	25381	2.47	24703	−0.05	−10.12
	(0.54)		(0.54)			
% vacant	0.07	25314	0.07	24701	0.001	1.27
	(0.08)		(0.08)			
% owners	0.68	25381	0.80	24703	−0.12	−69.05
	(0.24)		(0.15)			
% private tenant	0.19	25381	0.13	24703	0.06	49.92
	(0.17)		(0.11)			
% social tenant	0.09	25381	0.03	24703	0.07	50.33
	(0.19)		(0.09)			
	(0.18)		(0.11)			
% of flat	0.13	10377	0.09	10706	0.047	2.9e-84
	(.20)		(.146)			
Density	596.36	10369	324.36	10690	271.99	7.15e-97
	(1138.47)		(695.1)			
New Dwellings						
Number of units built	4.86	25391	2.43	24721	2.43	21.85
	(16.23)		(6.62)			
Income	18853.04	12904	16634.10	12756	2218.94	10.71
	(18523.49)		(14365.50)			
Surface per person	2.60	12904	2.72	12763	−0.11	−7.39
	(1.26)		(1.19)			
% vacant	0.07	12551	0.05	12613	0.02	7.44
	(0.18)		(0.17)			
% owners	0.72	12551	0.81	12613	−0.09	−21.02
	(0.36)		(0.32)			
% private tenant	0.22	12551	0.14	12613	0.07	19.58
	(0.32)		(0.28)			
% social tenant	0.05	12904	0.02	12765	0.03	15.66

Source: FiLoCom
Note: The statistics are for the year 2005. Standard errors are in parenthesis.

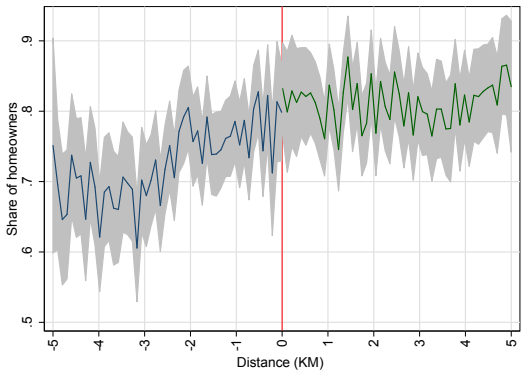
Appendix D Robustness check



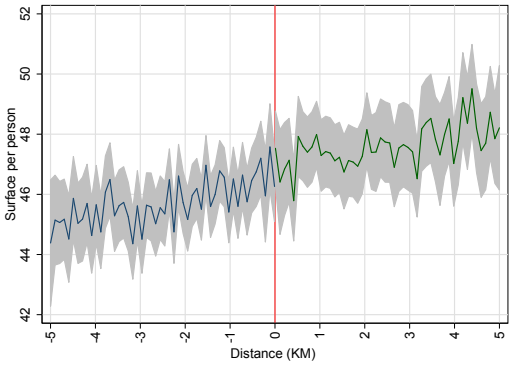
A. Number of people per household



B. Income



C. share of owners

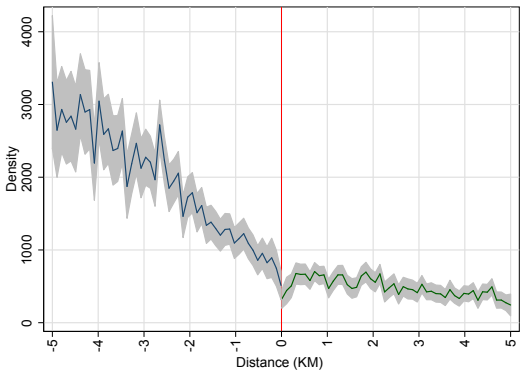


D. Surface per person

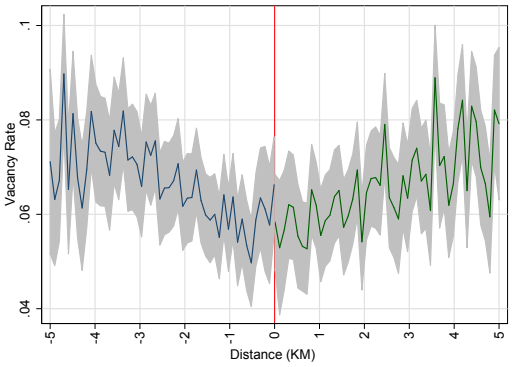
Figure D1. Non parametric regressions of the characteristics of the households on the distance between B and C areas

Source: Author's computation from Filocom for the year 2005

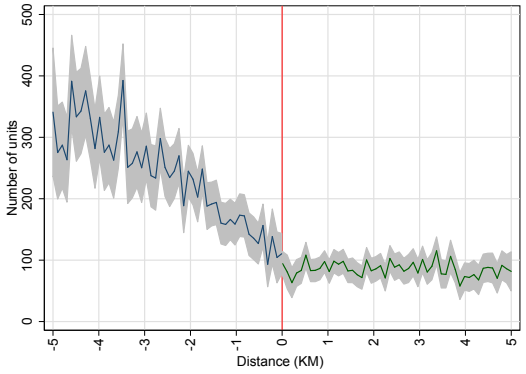
Note: Separate non parametric regressions for each side of the frontier with an Epanechinikov estimator and a bandwidth of 100 meters



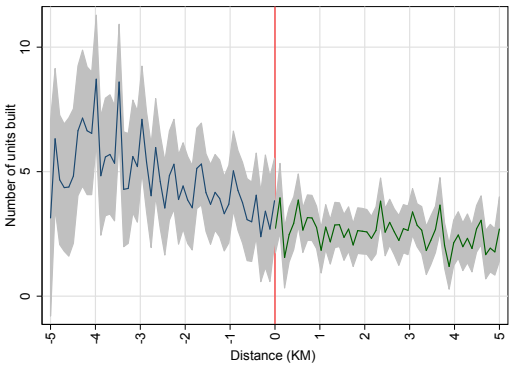
A. Density



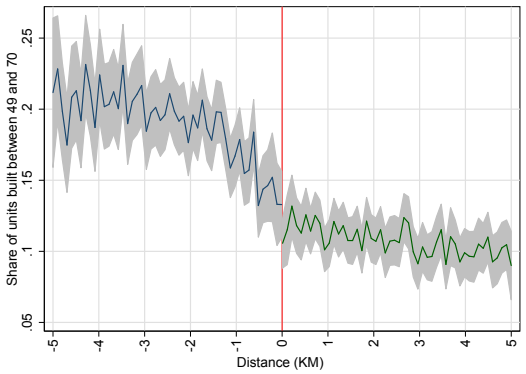
B. Vacancy Rate



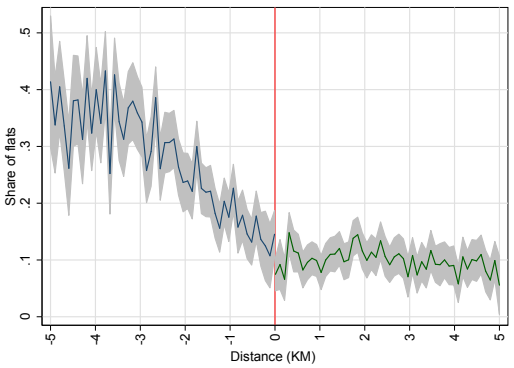
C. Number of units



D. Number of units built in 2005



E. Share of dwellings built between 49 and 70

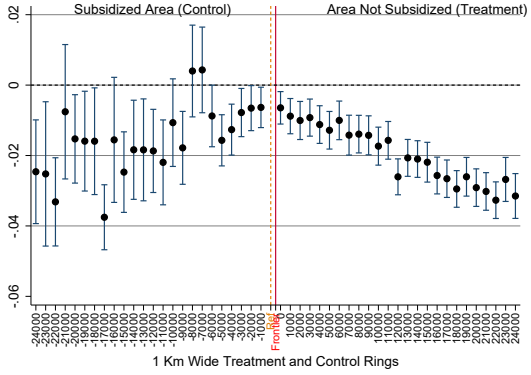


F. Share of flats among the dwellings

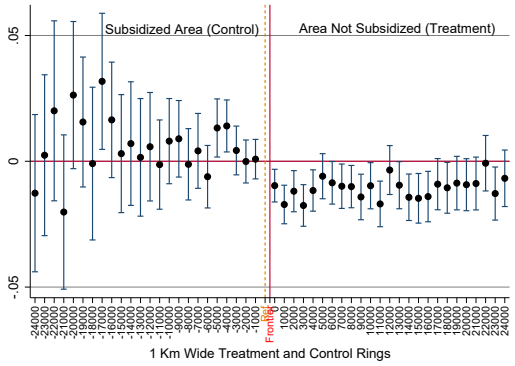
Figure D2. Non parametric regression of the characteristics of the dwellings on the distance between B and C areas

Source: Author's computation from Filocom for the year 2005

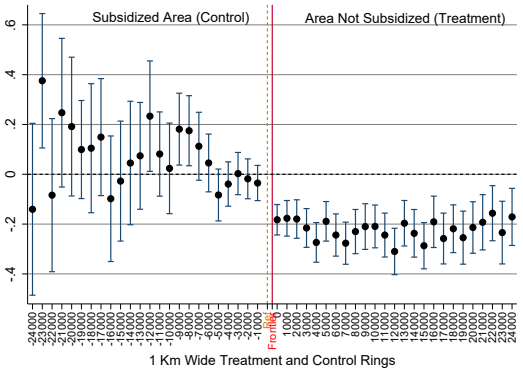
Note: Separate non parametric regressions for each side of the frontier with an Epanechinikov estimator and a bandwidth of 100 meters



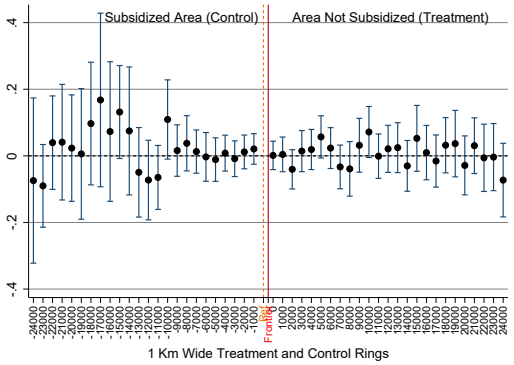
A. Housing stock



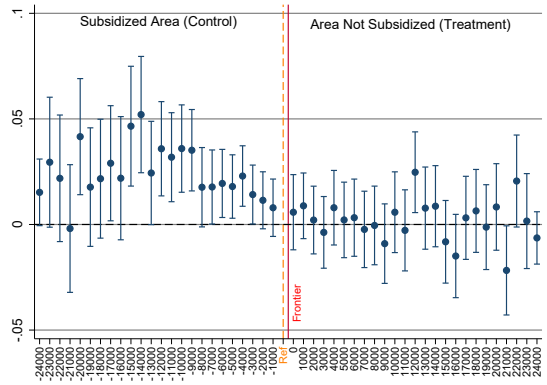
B. Vacancy rate



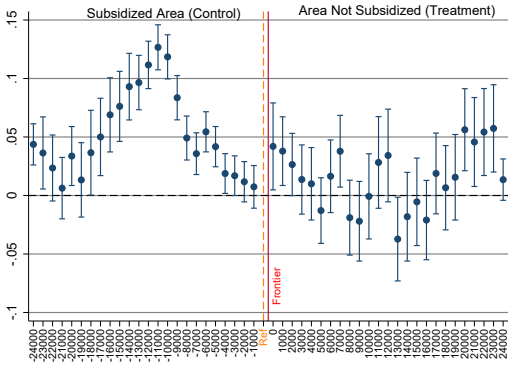
C. Number of tenants



D. Income of new tenants



E. Housing Prices



F. Flat Prices

Figure D3. Non parametric estimate of the impact of the STC with a larger sample of housing blocks (blocks with 25 km from the boarder)

Source: Author's computation

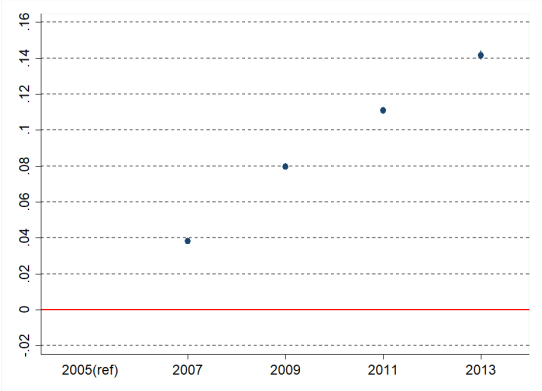
Table D1 – Impact of the Removal on the Number of Tenants in New Dwellings (within 1 to 5km from the Treatment Boundary)

	Placebo				STC	
Treatment period	2005-2009		2007-2009		2009-2013	
	(1)	(2)	(3)	(4)	(5)	(6)
Removal	−0.026	−0.032	−0.099***	−0.040	−0.171***	−0.160***
	(0.031)	(0.033)	(0.031)	(0.031)	(0.023)	(0.029)
Controls	No	Yes	No	Yes	No	Yes
Observations	27220	27195	27220	27195	44652	44575
No. of sections	17754	17735	17754	17735	22547	22493
R^2 within	0.012	0.019	0.013	0.019	0.008	0.013
Estimation Period	2005-2009				2005-2013	

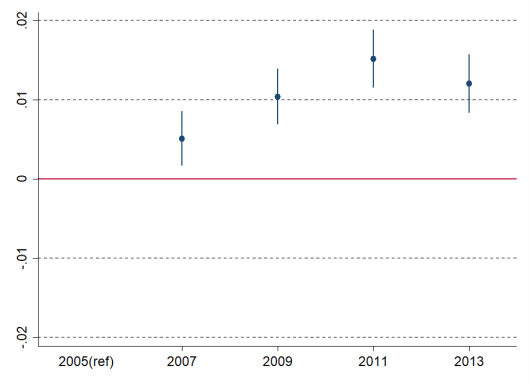
Note: Panel fixed-effects regression where the dependent variable is the log of the number of renters in new dwellings. The treatment and controls are the same as before. Time fixed effects are included for all the regressions. The standard errors are clustered by urban areas.

Sample: Unbalanced panel for all the odd years between 2005 and 2013. Observations are all the cadastral section with at least one tenants in a new unit built between two observed years.

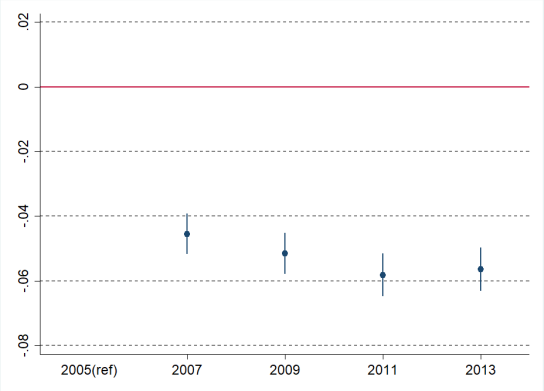
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.



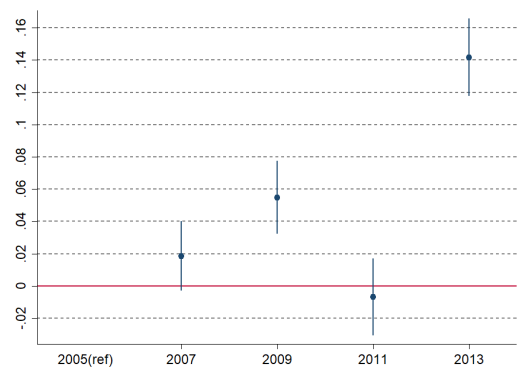
A. Housing stock



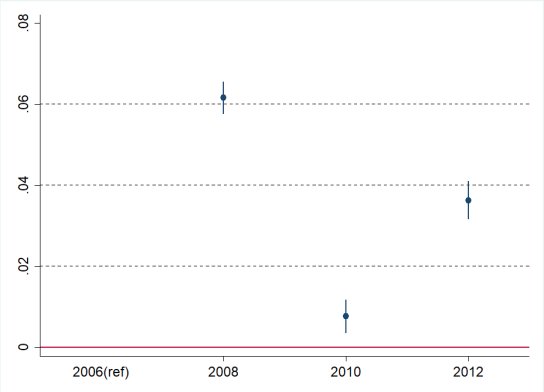
B. Vacancy rate in new dwellings



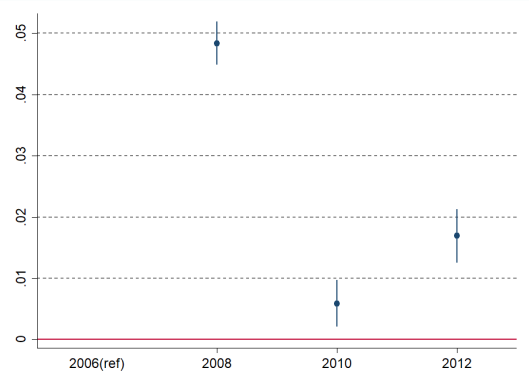
C. Share of owners occupiers in new dwellings



D. Income of tenants in new dwellings



E. Price of houses



F. Price of Flats

Figure D4. Trends of the outcome variables

Source: Author's estimates from Filocom and BIENS/PERVAL. These charts plot the coefficients of the fixed effect regressions for housing blocks/transactions between 1 and 5km from the B/C frontier.