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Abstract

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JEL Classification: R31, R38

Keywords: Housing prices, Policy evaluation, Construction, public housing

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Public housing development and segregation: SRU law in France[a]

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1 Introduction

In many developed countries, low-income rental housing assistance programs have been implemented to help households with modest income access dwellings below market rent. In France, these programs cost 27 billion euros in 2019, that is 1.3% of the gross domestic product, and public housing represents as much as 56% of these costs (SDES, 2020). This is not surprising since public support represents as much as 35% of the average total cost for the construction of a public dwelling which amounts to 140 thousand euros in 2016 (DGALN, 2017).

The French government supports scattered development of public housing to favor social mixity. An important goal is the de-segregation of low-income households living in poor neighborhoods by giving them access to public housing in better neighborhoods. This lead to the SRU law imposing 20% of public dwellings to all medium and large municipalities of large-enough cities in December 2000.[1] Indeed, some of these municipalities did not construct much public housing in the past, whereas they are quite rich. This paper evaluates to what extent this law affected the construction and location of public housing, and lead to a larger presence of low-income households and more social mixity in treated municipalities.

Government intervention in France contrasts with de-segregation policies in the US that rely to a larger extent on the distribution of means-tested housing vouchers. They are preferred over public dwellings which construction and maintenance are considered too costly. There is important evidence on the Moving-to-Opportunity program that proposed vouchers conditionally on moving to low-poverty neighborhoods (Chyn and Katz, 2021). Voucher recipients indeed relocated to richer places, but effects on individuals depended on the outcome and varied across demographic groups. For adults, the program had no effect on employment and income, but was beneficial for health and safety (Katz *et al.*, 2001; Kling *et al.*, 2007). For the young, the program decreased criminal behaviours for females whereas evidence is mixed for males (Kling *et al.*, 2005). It is actually children below thirteen, who benefited from the program in the long run, but not older ones (Chetty *et al.*, 2016).[2] They had higher education and earnings, lived in better neighborhoods and were less likely to be single parents when adults.

Due to the high maintenance costs and low-income segregation, several US cities demolished public dwellings and distributed housing vouchers to favor the relocation of displaced families in better places. Following the demolition of public dwellings in Chicago, displaced children did no better or worse than those in neighboring non-demolished public dwellings in the short run (Jacob, 2004). However, they were more likely to be employed and earn more when they became young adults, and experienced fewer violent crime arrests (Chyn, 2018), in line with results obtained for the Moving-to-Opportunity program.

The SRU law is based on municipalities rather than individuals. It created incentives to construct new public dwellings where they were considered to lack, since municipalities not abiding to the law had to pay fees. In fact,

[1]SRU stands for *Solidarité et Renouvellement Urbain*, ie. Solidarity and Urban Renewal.

[2]See also Davis *et al.* (2021) and Chyn and Daruich (2022) for equilibrium models rationalizing estimated effects on children.

fees were inflated if these municipalities did not construct enough public dwellings during triennial periods. Despite these fees, public housing construction is not granted because penalties are not prohibitive and public housing may be considered as a negative amenity by rich inhabitants because they may want to live together rather than with the poor (Bayer *et al.*, 2007; Guerrieri *et al.*, 2013) and their home values may be negatively affected. It could happen that local authorities in rich municipalities prefer to pay the fees rather than construct public dwellings to satisfy potential voters at the next elections. Alternatively, local authorities in a rather rich municipality with a few poor neighborhoods may decide to build public housing in those neighborhoods to avoid bothering the rich. These considerations relate to the literature on the political economy of land use policies that considers the specific role of homeowners as voters against new constructions (Fischel, 2005; Hilber and Robert-Nicoud, 2013; Sollé-Ollé and Viladecans-Marsal, 2013; Ortalo-Magné and Prat, 2014; Mast, 2022).

We are interested in the de-segregation effects of the SRU law on public dwellings and low-income households. We study these effects using fiscal data at the municipality level over the 1996-2008 period. We restrict the sample to municipalities around the municipal threshold of the law such that our sample is more homogeneous. We then show with a difference-in-differences approach that the law led to a yearly increase in the number of public dwellings of 2.0% over the 2000-2008 period. This increase occurred mostly in treated municipalities with a proportion of public housing below 5%, for which the yearly increase in the number of public dwellings due to the law was 5.0%. The law also decreased public housing segregation within these treated municipalities, but it did not decrease much low-income segregation. To understand this contrasted result, we investigate where new public dwellings were built at the block level. We show that, within these treated municipalities, the concentration of public dwellings increased to a larger extent in blocks with below-average income and below-average concentration of public housing. Overall, our findings suggest that the SRU law caused public housing construction in treated municipalities where it was barely present, but it did not desegregate low-income households.

The paper is organized as follows. Section 2 gives information on the public housing sector and related policies in France. Section 3 then describes our different data sources and Section 4 details our empirical strategy. Section 5 discusses stylized facts on characteristics and evolution of public housing in treated and control municipalities. Section 6 presents the main results of our policy evaluation and Section 7 provides robustness checks and additional results. Finally Section 8 concludes.

2 The French context

2.1 Public housing in France

In France, public housing is built and managed by public and private not-for-profit landlords (there are respectively 268 and 244 of them in 2013). They benefit from attractive funding arrangements in exchange for supplying dwellings at rents below the market level. Access is means-tested and subject to an administrative allocation mechanism that relies on a specific committee (*Commission d'attribution de logements*). Overall, public housing in France is similar to that in the US, although it represents a much larger proportion of the housing stock (around 15% vs. less than 1% in 2021).[3]

There are four types of public dwellings that are characterized by different income and rent ceilings for occupants. Two of them, called PLAI and PLUS, have the lowest ceilings and are intended for very poor and poor households, respectively. The two other ones, called PLI and PLS, are intended for medium-income households for whom renting in the private sector would remain a significant burden.[4] Households need to be eligible only at entry into a public dwelling and can then keep it as long as they wish. Overall, rent ceilings yield a significant subsidy to public housing tenants since they pay rents around 40% below those in the private sector market (Laferrère and Blanc, 2001; Trévien, 2014; Chapelle and Eymeoud, 2022). In particular, this implicit subsidy facilitates wealth accumulation and access to homeownership (Goffette-Nagot and Sidibé, 2016).

In 2011, the public sector consisted of 4.4 million dwellings, and represented 13% of the residential housing stock and one-third of the rental sector.[5] Public dwellings are located mostly in large cities, especially in disadvantaged suburban neighborhoods. Single-headed families, aged individuals and immigrants are over-represented. In particular, public housing would attract new immigrants and, in 1999, it concentrates around 50% of Algerians and Moroccans, which are two large groups of immigrants (Verdugo, 2016). Immigrants have a higher propensity to be in a public dwelling even all else equal for socio-economic characteristics (Fougère *et al.*, 2013).[6] In fact, segregation of immigrants within cities has increased over the 1968-1999 period, and the increased concentration of immigrants in public housing in some neighborhoods is a major cause of this evolution (Verdugo, 2011). Mayors can influence the municipal concentration of immigrants through public housing policies. Schmutz and Verdugo (2022) show that, in municipalities where a left-wing mayor was elected (rather than a right-wing one), the proportion of non-European immigrants grew faster, largely because of public housing construction and a change in the composition of public housing occupants.

There is both under-population and over-population of public dwellings (Jacquot, 2007). Under-population is due to small-size aged households and occurs in both small and large cities. It arises because ageing parents can remain in their public dwelling as long as they want, even when kids are gone, and they do so to benefit from low

[3]In France, incentives were also created to promote constructions in the private rental sector through income tax rebate in some specific locations under the condition that access is means-tested (Chapelle *et al.*, 2018). The corresponding program was the Scellier Tax Credit introduced in 2009 that evolved later into the Duflot and Pinel tax credit. It is similar to the Low-Income Tax Credit Development (LITHC) which effects on construction crowding, segregation and housing prices have been widely studied (Baum-Snow and Marion, 2009; Eriksen and Rosenthal, 2010; Diamond and McQuade, 2021).

[4]In 2003, the proportions of households eligible for PLAI, PLUS, PLS and PLI are respectively: 28%, 64%, 80% and 86% (Dallier, 2016). Hence, access to public housing for medium-income households is not much restricted.

[5]These statistics were computed from FILOCOM data that are presented later in the text.

[6]This result is obtained from French censuses which do not include income.

rents even if their income has increased (Laferrère, 2013). Over-population arises because of families with children and occurs mostly in large cities. Over-population usually goes along with low income and poor housing conditions. Even if most individuals living in public dwellings earn modest revenues, households in the last income quintile of the overall population still represent around 5% of occupants (Trévien, 2014). Over the 1999-2015 period, public housing segregation has decreased but income segregation has increased. Some explanations are that public housing remains segregated with public tenants who have become poorer, and richer municipalities have received wealthier public tenants (Beaubrun-Diant and Maury, 2022).

A concern of the government is that public housing would be under-represented in rich municipalities. Figure 1 represents a scatter plot of the proportion of public dwellings as a function of average household gross income at the municipality level in 2000 for municipalities with 1999 population above 1,500. A non-parametric trend shows that the proportion of public dwellings is decreasing with income. This trend goes along with a decrease in the spread of the proportion of public dwellings.

[Insert Figure 1]

2.2 The SRU law

An increased awareness of poverty in the suburbs and large disparities within cities led to the creation of the Ministry of Urban Affairs in 1990. This creation occurred while the government was decreasing its involvement in the construction of social dwellings, which was increasingly funded by local authorities. The next year occurred the first step towards a global monitoring of public housing with the *Loi d'Orientation de la Ville* (LOV) voted in July 1991 which was meant to allow households with modest income to access good-quality neighborhoods and to promote social mixity.

This law stated that municipalities should have a proportion of public dwellings in the housing stock of residential dwellings above 20% when they were located in a city with 1990 population above 200,000 and had a proportion of social benefits recipients below 18%.[7] Municipalities not reaching the target would be fined. The law was not implemented for a while because of political alternation, heated debates on its procedural complexity and on the population threshold above which municipalities should be concerned.[8] Implementation finally occurred in January 1995 for the 209 municipalities with a population above 3,500. In January 1999, this threshold was decreased to 1,500 for municipalities in the Paris region.

[7]Here, a city refers to an urban unit which is defined in France as a municipality or a group of municipalities that includes a built area populated with at least 2,000 inhabitants where no dwelling is separated from its neighbor by more than 200 meters. Moreover, each municipality in the urban unit must have more than half of its population in the built area.

[8]For instance, it is mentioned in *Le Moniteur* newspaper on October 21, 1994 that deputy Gilles Carrez proposed to fix the municipality population threshold to 2,000 because excluding smaller municipalities should not decrease much the number of public dwellings that should be built (whereas the final chosen threshold ends up being different at 3,500 outside the Paris region).

At the end of the nineties, the LOV law was considered an inappropriate tool because it was too complex and its coverage was too small.[9] It is in this context that a major change in the legal framework was implemented with the *Solidarité et Renouvellement Urbain* (SRU) law. First discussions of the law project began in February 2000 before article 55 of the SRU law was voted in December 2000 with the purpose of inducing more medium and large municipalities to construct public housing with a transparent fine system in case they did not comply.[10] This law was supposed to help solve coordination failures between the State and local authorities.

Article 55 of the SRU law makes it compulsory for municipalities to have a proportion of public dwellings above 20% when their 1999 population is above 3,500 outside the Paris region and 1,500 inside the Paris region, and they belong to a city with population above 50,000 that involves at least one municipality with population above 15,000. Municipalities are exempted if they belong to cities that experienced a decrease in their population between the 1990 and 1999 censuses and they are part of an urban plan that favors urban renewal and social mixity. In practice, only a few municipalities are exempted. Compared to LOV law, SRU law has overall a much wider perimeter since it also concerns smaller cities (ie. those with population between 50,000 and 200,000) and does not involve any criterium on social benefits recipients.

Municipalities violating the SRU law have to pay a fine equal to 20% of their fiscal potential multiplied by the gap between their proportion of public dwellings and the 20% objective expressed in percentage points.[11,12] It is capped to 5% of the municipality operating expenditures of the previous year and it is not enforced if its amount is below 3,811 euros. Put differently, the fine is given by:

$$Fine_t = A1_{\{A \geqslant 3,871\}} \text{ with } A = \max\left[.2F_t\left(20 - P\right), .05E_{t-1}\right] \quad (1)$$

with $Fine_t$ the fine, F_t the fiscal potential, P the proportion of public dwellings (in %) and E_{t-1} the municipality operating expenditures of the previous year.

Municipalities that do not reach the objective of the law have to catch up over a fifteen-year period. The evaluation of efforts to reach the objective is conducted every three years by government authorities. During a three-year period, municipalities violating the law are supposed to bridge 20% of their initial gap. Moreover, after 2006, the public dwellings built during one of the three-year periods should not represent less than 30% of those built during the previous three-year period. If a municipality does not meet these conditions, its fine is increased.

[9] Note though that a bit more than two-third of concerned municipalities complied with the law in the sense that they met their triennial commitment about public housing construction (*JO*, July 20, 1998, p. 4025).

[10] http://www.senat.fr/dossier-legislatif/sru.html.

[11] The fiscal potential of a municipality corresponds to the fiscal resources obtained if local tax rates were equalized to the average national values over the whole territory. The higher the fiscal potential, the richer the municipality.

[12] This simplified formula is introduced in 2006. Before that date, the fine is equal to 152.45 euros per missing dwelling for municipalities with a fiscal potential lower than 762.25 euros, but to 20% of the fiscal potential multiplied by the gap between their proportion of public dwellings and the 20% objective for the other municipalities.

The increase rate is at most equal to the ratio between the number of missing public dwellings and the three-year period objective. Put differently, the fine becomes $(1+m)\,Fine_t$ with $0 \leqslant m \leqslant (N_{obj} - N_{constr})/N_{obj}$ where N_{constr} is the number of public dwellings constructed in the three-year period and $N_{obj} = .2\,(.2 - P/100)\,N_{princ}$ is the three-year objective with N_{princ} the number of primary residences. Moreover, regional authorities may then settle an agreement with a public housing provider to meet the objectives fixed by the law, either by constructing new public dwellings or by acquiring existing dwellings, and the municipality is constrained to participate to the financing of the project.

In our analysis, we need to take into account the LOV law since its specifics are related to those of the SRU law. We could identify municipalities concerned by the LOV law from digitalized archives (see Appendix A.1). We experimented with a LOV global list that includes all municipalities concerned by the law when ignoring the municipal population thresholds, and a time-varying LOV restricted list that restricts this sample to municipalities with population above the threshold specified by the law in a given year. Indeed, it is not clear whether the behaviour of municipalities was really affected by population thresholds during the 1996-2000 period, since these thresholds were not announced beforehand and their specifics were subject to recurrent debates. At the end, we use the LOV global list below since it is the only one that allows to make pre-trend isssues disappear when taking into account the LOV law.

The national office for urban planning and housing provided us with figures on the municipalities concerned by the SRU law over the 2002-2004 triennial period. These data allowed us to identify 728 such municipalities (there are 36,000 municipalities in France) and they will constitute our treated group in our empirical analysis. Among them, 50.8% did not fulfill their triennial objective of public housing construction. We could also access yearly figures on the proportion of public dwellings, fine and global budget for those municipalities. In 2002, their median proportion of public dwellings was 8.9% and their first quartile was 5.0%. The median fine was 35.0 thousand euros and the first quartile reached 11.7 thousand euros. The magnitude of the fine (including its raise when the triennial objective is not fulfilled) can be compared with the municipality budget. The resulting ratio is rather modest with a median of 1.6%, a first quartile as small as 0.5%, and a 99^{th} percentile at only 5.5%. The rules used to fix the fine suggest that the relationship between the fine (when non-zero) and the number of additional public dwellings required by the law may be rather close to being linear in logarithm. This is in line with Figure C.1, the elasticity of the fine with respect to the number of additional public dwellings required in 2002 being 0.93 (and the R^2 of the corresponding regression being large at 0.73).

2.3 Potential mechanisms

The SRU law is expected to foster the construction of public housing in treated municipalities because of penalties when not complying with the objective of the law. Still, penalties are not prohibitive since they can represent at most a small share of municipal budget, and local authorities may prefer to pay the fees rather than displease inhabitants. In particular, this might occur because inhabitants of treated municipalities can be rather rich and may want to avoid mixing with poor public housing occupants due to their preferences or the fear of crime. Moreover, public buildings may spoil the aestheticism of their living environment and decrease the value of their properties. These mechanisms are related to the not-in-my-backyard (NIMBY) phenomenon that has been shown for public housing in France (Goujard, 2011) and private social housing in the US (Diamond and McQuade, 2021). Even when complying with the law, some treated municipalities may try to alleviate the concerns of local inhabitants by constructing public housing intended for medium-income households (PLI and PLS) rather than public housing intended for poor ones (PLAI and PLUS). We will investigate the effects of the SRU law on both the number of public dwellings and the proportion of those intended for low-income households. We will also assess whether it succeeded in affecting the local income composition by studying its effect on the concentration of low-income households in treated municipalities.

When constructing public housing, treated municipalities also need to choose the location of the new dwellings. They may avoid blocks concentrating rich households not only to respect their wishes, but also because local land prices are high, and constructing or purchasing buildings in these blocks to create public housing is costly. Among remaining blocks, treated municipalities may choose those where there is already a high concentration of public housing to avoid creating negative externalities for inhabitants in other blocks. Alternatively, they may avoid concentrating too much public housing inside blocks since concentration may potentially generate very harmful externalities above a given threshold, and they may rather choose blocks where public housing is rather scarce and the population is rather poor (provided that such blocks exist in the municipality). This scattered development is likely to decrease public housing segregation within treated municipalities, but not low-income segregation if blocks hosting new public dwellings are rather poor. We will investigate the effects of SRU law on public housing and low-income segregation within municipalities. We will also conduct an analysis at the block level to evaluate the extent to which the average income and number of public dwellings affect the construction of new public dwellings inside blocks.

2.4 Past evaluations

Evaluations of the effects of SRU law are scarce. Bilek *et al.* (2007) computed that, for the Paris region, 13,000 public dwellings were expected to be built over the 2001-2004 period because of the law. However, at the end of 2004, only 7,200 additional public dwellings had been built. In 2005, 7.5 million euros were collected in the Paris region because the triennial objective had not been met by municipalities violating the law and 36.2 million euros for the whole French territory.

So far, the effect of the SRU law on the evolution of public housing has been evaluated by Bono *et al.* (2013). They use municipal data over the 1998-2009 period on the stock of public dwellings from the Survey on the Social Rental Sector (*Enquête sur le Parc Locatif Social - EPLS*) and on the stock of residential dwellings from FILOCOM dataset that are fiscal data collected for tax purpose (see below for more details on this data source). As EPLS sampling frame is not exhaustive, data are missing for some municipalities and years. It is considered in the study that the construction of public dwellings over the 1998-2005 period was decided before the SRU law. The municipal evolution of the number and proportion of public dwellings before and after 2005 is then quantified. Approaches relying on difference-in-differences and changes in trends (compared to trends predicted from past values) show a modest positive effect of the law: Over a four-year period, this effect would amount to 0.34 points or, equivalently, to the construction of forty dwellings for a municipality of 20,000 inhabitants.

The effect of the SRU law on within-municipality segregation has been studied by Beaubrun-Diant and Maury (2022) who borrow our identification strategy (Gobillon and Vignolles, 2016) and resort to a global income segregation index. They find a non-significant positive effect over the 1999-2015 period. This long period actually involves changes in the design of the law with its expansion to some groups of cooperating municipalities in 2008 (article 11 of DALO law). By constrast, we are interested in the within-municipality segregation of low-income households, and we conduct a full policy evaluation exercise in which we quantify the heterogeneous impacts of the SRU law on different outcomes over the 1998-2008 period. Interestingly, we will see that we find a small but significant negative effect of the SRU law on low-income segregation for treated municipalities far from the objective of the law.

3 Data

Our main analysis at the municipality level is based on FILOCOM (*Fichier Logement Communaux*) data for uneven years over the 1997-2009 period. The FILOCOM dataset is an exhaustive panel of individual dwellings located in mainland France. It is constructed from the information collected for ordinary dwellings eligible for income or housing tax (whether households are taxed or not). For a given year, information is reported for the stock of dwellings on January, 1. In this study, we refer to year t when data were collected at the beginning of year $t+1$. The dataset contains the municipality and cadastral section identifiers, dwelling characteristics, the housing tenure and the annual gross income of households occupying dwellings. In particular, the cadastral sections correspond to blocks and they will be named as such below.[13]

This information is used to construct the municipal stock of public dwellings and their municipal proportion in the stock of main residences. Indeed, these two variables are available from the national office for urban planning and housing only for municipalities violating the law, whereas they will be needed for both the treated and control municipalities. The data are also used to compute the municipal average household income and a low-income specialisation index measuring the low-income concentration in a municipality relatively to its city. This specialisation index is defined as the ratio between the proportions of households in the first income quintile in the

[13] Municipalities and cadastral sections are small geographic units. The French territory is divided into $36,000$ municipalities. Each municipality is divided into cadastral sections and there are around $270,000$ of such sections.

municipality and in its city, where the threshold defining the first income quintile is computed at the city level. Finally, we compute dissimilarity indices measuring the within-municipality segregation of public dwellings and households in the first income quintile (labelled respectively "housing dissimilarity index" and "income dissimilarity index"). These dissimilarity indices are computed from cadastral sections within each municipality using the formula proposed by Duncan and Duncan (1955). They take values between 0 and 1, and correspond to the proportion of public dwellings (or households in the first income quintile) which should be reallocated to different cadastral sections within the municipality to obtain a uniform distribution of public dwellings (or households in the first income quintile) within the municipality.[14] A larger index value indicates a higher level of within-municipality segregation. More details are given in Appendix A.2.[15]

The number of public dwellings at the municipal level may be under-estimated in FILOCOM data because it only includes individual dwellings. Indeed, workers' hostels, senior citizens' residences, homes for disabled people, and accommodation and social rehabilitation centres are omitted from the data. Nevertheless, the corresponding bias is negligible for the restricted sample used in our analyses which only includes medium-size municipalities. Indeed, omitted dwellings are mostly concentrated in large municipalities (Meunier *et al.*, 2013). The number of public dwellings at the municipality level may also be slightly over-estimated in FILOCOM data as it includes public dwellings old enough to be out of the funding-related agreement with local authorities, but these dwellings might not be included in the official data.[16]

4 Empirical approach

4.1 Identification strategy

The SRU law is not a random treatment since it affects municipalities in a deterministic way depending on their size and that of their city. In absence of treatment, the behavior of treated and non-treated municipalities with respect to the construction of public dwellings can greatly differ since the municipal housing stock, socio-economic composition and elected officials are likely to vary with municipality and city sizes.

To identify the causal effect of the law, we exploit the discontinuity of the treatment rule with respect to municipality size. Our approach consists in comparing the evolutions of the log-number of public dwellings in treated

[14]A specificity of these dissimilarity indices is that they do not involve the internal geography of a municipality, and in particular distances between cadastral sections with small and large proportions of public dwellings (or proportions of households in the first income quintile).

[15]In particular, we restrict below our sample of municipalities to urban ones with population in the interval [800 ; 6,000] in the Paris region, and in the interval [2,800 ; 6,000] in the other regions. For these municipalities, the mean (resp. median) number of cadastral sections reaches 11 (resp. 13), suggesting enough cadastral sections in studied municipalities for dissimilarity indices to be meaningful.

[16]For our sample of treated municipalities, in 2002, the median and mean numbers of public dwellings in municipalities are respectively 114 and 107 (resp. 133 and 128) in FILOCOM (resp. official data), and the correlation between the two sources is as high as 0.92.

and non-treated municipalities using a restricted sample that includes only urban municipalities (ie. municipalities located within urban units) just above and below the municipality size threshold (1,500 inhabitants in the Paris region and 3,500 inhabitants in other regions).[17] The sample restriction makes treated and non-treated municipalities more comparable since it is unlikely that municipalities close to the population threshold were able to manipulate their population before the implementation of the policy since population is measured in 1999, ie. two years before the SRU law was implemented.

The treatment group contains all the urban municipalities whose population is above the municipality threshold and whose city has a population above the city threshold (50,000 inhabitants), and such that the proportion of public dwellings is below 20% according to official data from the national office for urban planning and housing. The non-treated group contains all the other urban municipalities. Considering this, the treatment effect is obtained by comparing treated and non-treated municipalities of slightly different sizes (above and below the municipality threshold), as well as of similar sizes (but located in cities above and below the city threshold, or with a proportion of public dwellings above and below the 20% threshold).[18]

In practice, the choice of a population interval around the threshold to construct our sample is the result of a trade-off. This interval should be small enough to include only municipalities of rather similar population, but large enough to contain enough municipalities to get statistical meaningful results. In our application, we consider urban municipalities in the Paris region (resp. other regions) with population in the interval [800 ; 6,000] (resp. [2,800 ; 6,000]), and conduct robustness checks when varying this range. Non-treated municipalities verifying these restrictions constitute our control group. In fact, treated and control groups differ to some extent in municipal and city characterics that may affect the growth of our municipal outcomes, and we will thus include control variables in our regressions to deal with these differences (see more below).

4.2 Econometric specifications

We estimate the yearly treatment effect at the municipality level over the 1996-2008 period, the data being available only every two years. Our specification takes into account the previous housing policy *Loi d'Orientation de la Ville*

[17]Studying the municipal evolution of the log-number of public dwellings amounts to studying the municipal growth rate of public housing. Indeed, denote by n_{it} the number of public dwellings in municipality i at date t such that $t = 0$ (resp. $t = 1$) is a date before (resp. after) the reform. We have: $\log n_{i1} - \log n_{i0} = \log\left(1 + (n_{i1} - n_{i0})/n_{i0}\right)$ where $(n_{i1} - n_{i0})/n_{i0}$ is the municipal growth rate of public housing in the municipality between dates 0 and 1.

[18]One may consider restricting the non-treated group to municipalities with a proportion of public dwellings below 20% so that the support of the proportion of public dwellings is similar for treated and non-treated municipalities. To make that restriction, consistency with the definition of treated municipalities suggests to rely on the official measure of the proportion of public dwellings used to determine whether or not municipalities comply with the law. This is not possible since this measure is available only for treated municipalities. Nevertheless, a robustness check will be conducted when restricting both the treated and control groups to municipalities whose proportion of public dwellings is below 20% according to FILOCOM data.

(LOV) that may affect pre-trends. Our main outcomes of interest are the log-number of public dwellings, the housing and income dissimilarity indices and the specialisation index.[19] We also propose a variant of our specification that allows the treatment effect to depend on the initial proportion of public dwellings in 2000.

Our specification for a municipality i in year t is given by:

$$Y_{i,t} = \alpha_t 1_{\{S_i=1\}} + \theta_t 1_{\{L_i=1\}} 1_{\{t \leq 2000\}} + Z_i \delta_0 .t + u_i + \mu_t + \epsilon_{i,t} \tag{2}$$

where $Y_{i,t}$ is the outcome and $1_{\{S_i=1\}}$ is a dummy for the municipality being treated with the SRU law. Our parameter of interest α_t is the yearly treatment effect of the SRU law at time t. Note that the SRU law is allowed to have a treatment effect before treatment, ie. before year 2002, in order to investigate pre-trends. The specification also includes the interaction between the dummies $1_{\{L_i=1\}}$ and $1_{\{t \leq 2000\}}$ to take into account the effects of the LOV law before 2000 (included) but no later, and θ_t is the related yearly treatment effect. Finally, the specification controls for heterogeneity in municipality trends through the interactions between t, which serves as a linear time trend, and Z_i which is a set of explanatory variables that includes region dummies and eight additional explanatory variables measured in 2000 (the logarithm of municipal average income and its square, the logarithm of municipal number of dwellings and its square, as well as the same variables at the city level). Parameters δ_0 are the yearly effects of these explanatory variables and $\epsilon_{i,t}$ is the residual.

We are particularly interested in the effect of the SRU law over the 2000-2008 period. In fact, equation (2) can be rewritten in difference between year 2000 and year t as:

$$Y_{i,t} - Y_{i,2000} = \alpha_t 1_{\{S_i=1\}} - \theta_{2000} 1_{\{L_i=1\}} + Z_i \delta_t + \mu_t - \mu_{2000} + \varepsilon_i \tag{3}$$

where $\delta_t = \delta_0 (t - 2000)$ and $\varepsilon_i = \epsilon_{i,t} - \epsilon_{i,2000}$. Importantly, this specification takes into account the fact that the initial outcome level may be affected by the LOV law. Otherwise, this is the usual difference-in-difference specification.

An important assumption that helps for identification is that the LOV law is supposed to have no effect after 2000. The sources of identification under this assumption can be discussed from first differences. Municipalities treated with the SRU law but not the LOV law bring identification power for $\alpha_t - \alpha_{t-1}$. Municipalities treated with both the LOV and SRU laws bring identification power for $\alpha_t - \alpha_{t-1} + \theta_t - \theta_{t-1}$ for any $t < 2000$, $\alpha_t - \alpha_{t-1} - \theta_{t-1}$ for $t = 2002$, and $\alpha_t - \alpha_{t-1}$ for any $t > 2002$. In particular, identification of the SRU treatment effects comes from both sets of municipalities. If the LOV law was allowed to have an effect after 2000, SRU treatment effects

[19] Note that the log-number of public dwellings and housing dissimilarity index are not well defined when the number of public dwellings is zero. We discard the municipalities where it happens. In 2000, the proportion of municipalities with no public dwelling is 5.4% for our selected sample. Still, this problem with zeros does not occur when studying the effects of the SRU law on the number and proportion of public dwellings. Corresponding results presented below are consistent with our main results. A similar issue may occur for the income dissimilarity index, but in practice the municipal number of households in the first income quintile is always strictly positive and there is thus no issue in that case.

would be identified only from the subset of municipalities that are treated with the SRU law but not the LOV law, which is an issue knowing that the number of municipalities treated with the SRU law is not that large in small population intervals around the municipality population thresholds. As only first differences of the SRU treatment parameters are identified, we need one identification restriction and we fix $\alpha_{2000} = 0$. This implicitly means that any yearly treatment effect is measured in difference with its value in 2000. Finally, as regards LOV treatment parameters, all of them are identified. Indeed, first differences of these parameters are identified from first differences in the outcomes of municipalities treated with both the LOV and SRU laws before 2002, and the 2000 parameter is identified from first differences in their outcomes between 2000 and 2002.

We also need additional restrictions for other parameters to be identified. We fix one region fixed effect and one year fixed effect to zero. Interactions between explanatory variables and time trend are introduced to take into account heterogeneous trends across municipalities that might affect the outcome and be correlated with quantities determining treatment. In particular, the introduction of specific regional trends (through the interactions between region fixed effects and time) implies that the first differences of SRU treatment effects are identified by comparing the treated and control groups within regions. Explanatory variables are measured in 2000 for consistency with the long-difference specification (3) but robustness checks will also be conducted considering their values at the current date in level (ie. introducing $Z_{i,t}$) or interacted with the time trend (ie. considering $Z_{i,t}\delta_0.t$ rather than $Z_i\delta_0.t$).

We then estimate heterogeneous treatment effects which vary with the initial proportion of public dwellings. We consider the following two categories for this proportion: strictly less than 5% and more than 5%. We modify specification (2) that becomes:

$$\begin{aligned} Y_{i,t} &= \sum_{c=1}^{2} \alpha_{c,t} 1_{\{C_i=c\}} 1_{\{S_i=1\}} + \sum_{c=1}^{2} \theta_{c,t} 1_{\{C_i=c\}} 1_{\{L_i=1\}} 1_{\{t \leq 2000\}} \\ &+ \gamma_t 1_{\{C_i=2\}} + Z_i\delta_0.t + u_i + \mu_t + \epsilon_{i,t} \end{aligned} \tag{4}$$

where $1_{\{C_i=c\}}$ is a dummy taking the value one if municipality i belongs to category $C_i \in \{1,2\}$ (it does not matter for the analysis which category is numbered 1), γ_t is a year fixed effect concerning only category 2 since it is interacted with a category-2 dummy, and $\alpha_{c,t}$ (resp. $\theta_{c,t}$) are category-specific yearly SRU (resp. LOV) treatment effects.

Specification (4) allows the treatment effect to vary depending on whether a municipality is very far from the 20%-threshold imposed by the SRU law for the proportion of public dwellings (0-5% category) or closer to that threshold ($\geqslant$5% category). Since we introduce in the specification a category-2 dummy with a time-specific effect, the treatment effects are estimated by comparing treated and control municipalities within the same category.

As before, restrictions are needed for the parameters to be identified, and we impose that a region fixed effect, a time fixed effect, the category-2 fixed effect in 2000 and the category-specific treatments effects in 2000 are zero. In particular, the last restriction implicitly means that the difference in outcomes between treated and control municipalities is measured in difference with its value in 2000 for each of the two categories. The rewritting of equation (4) in difference between 2000 and year t is again a difference-in-differences specification, but it takes into account this time the category-specific LOV treatment effect on the outcome value at the initial date.

5 Stylized facts

We now provide stylized facts on municipalities located in cities, ie. urban municipalities, distinguishing them according to their treatment status. As shown in Table 1, the average proportion of public dwellings in urban municipalities in 2000 is below the objective and reaches 10.1%. There are 5,220 non-treated municipalities but only 725 treated ones, which makes the proportion of treated municipalities quite small at 9.1%.[20] Treated municipalities have a proportion of public dwellings lower than the average at 8.5% and are also characterized by a larger number of dwellings and a larger 1999 population. For instance, their average population is 13.1 thousands compared to only 6.9 thousands for non-treated municipalities. They are located in much larger cities, and their average income is also more important.

To make the subsamples of treated and non-treated municipalities more similar, we restrict our attention to urban municipalities whose population is just above or below the municipality threshold (ie. whose population is in the 800-6,000 interval when located in the Paris region and in the 2,800-6,000 interval when located in another region). The number of non-treated municipalities shrinks to 1,472, and they represent only 28.2% of the original subsample. From now on, we will refer to non-treated municipalities verifying the restriction as control municipalities. The number of treated municipalities decreases to a lesser extent to 321, which is still 44.3% of the original subsample. Consequently, the proportion of treated municipalities becomes much larger at 21.8%. There is a sizable gap in the proportion of public dwellings between control municipalities (10.6%) and treated ones (6.7%). The housing and income dissimilarity indices have similar values for the two subsamples of municipalities, but the income specialisation index is much larger for control municipalities than for treated ones. This is not suprising since differences in income remain between the two subsamples (whereas population and number of dwellings are now much closer). There are also large differences in the number of dwellings and population of cities between the two subsamples. We will control for these differences in our empirical evaluation of the SRU law.

[Insert Table 1]

[20]Note that the figure given for the number of treated municipalities in Table 1 is lower than 728 which was given before in the text. This is because the three largest municipalities, Paris, Lyon and Marseille, are excluded from these descriptive statistics. Indeed, it is not clear whether municipalities as a whole or *arrondissements* should be considered (and it does not matter later for our analysis since they do not enter our restricted sample anyway). Note also that the official list of treated municipalities does not distinguish *arrondissements*, and the whole three largest municipalities are considered to be treated.

We now assess which municipalities contribute to the identification of the treatment effect. For that purpose, we examine to what extent the proportion of municipalities concerned by the law varies with municipality size in brackets above the municipality threshold (Figure 2). For the Paris region, this proportion is always strictly positive and oscillates around 35% across municipality size brackets (panel A). In other regions, the proportion of municipalities concerned by the law remains between 20 and 30% whatever the municipality size bracket (panel B). Overall, all the size brackets in the Paris region and in other regions contribute to the identification of the treatment effect.

It is important to note that municipalities whose population is below the municipality threshold also play an important role for identification since they represent respectively 23.3% and 40.9% of control municipalities in the Paris region and other regions. Differences in municipality sizes between control and treated municipalities are taken into account in our regressions by considering the municipal number of dwellings and its squared as control variables.

[Insert Figure 2]

We will also be interested in the impact of the SRU law depending on how far concerned municipalities are from the 20%-threshold for public housing. We use as a measure of the gap to the threshold, the difference between 20% and the proportion of public dwellings. This gap can be computed from official data only for treated municipalities, whereas it can be computed from FILOCOM data for both treated and control municipalities. We first assess whether official and FILOCOM data provide similar figures for the treated group. Panel A of Figure 3 shows that the densities of the gap for treated municipalities obtained from the two data sources are close. Official density increases until a value of the gap around 14 percentage points and then decreases. Hence, the bulk of treated municipalities are far from the objective of the law.

We also represent the densities of the gap separately for treated and control municipalities when using FILOCOM data (panel B). Of course, the gap is negative for a significant proportion of control municipalities (16.4%) which are those in line with the objective of SRU law when it is implemented. Note that this gap is also negative for a very few treated municipalities because FILOCOM data are not fully consistent with official data, but their proportion is very small (0.6%). Otherwise, the density has a shape that is rather similar for treated and control municipalities. It increases to reach a peak around 17 percentage points before decreasing. There is a large bulk of control municipalities that are far from the 20%-threshold. When restricting the sample to municipalities which proportion of public dwellings is below 20% as will be done in a robustness check, the densities are rather similar for the treated and control groups (panel C).

[Insert Figure 3]

We then turn to the average outcomes for treated and control municipalities (Figure 4). The average log-number of public dwellings increases faster for treated municipalities than for control ones after the introduction of the SRU law (Panel A). However, there are some differences in pre-trends that turn out to be smaller when considering only treated municipalities that were not concerned by the LOV law. We will see that differences in pre-trend disappear when controlling for being treated with LOV, and our income and size variables. Interestingly, the housing dissimilarity index, which measures public housing segregation within municipality, decreases steadily for treated municipalities but not for control ones after treatment whereas evolutions are similar before treatment (Panel B). This is also the case for the income dissimarity index that measures low-income segregation within municipality, although at a smaller scale considering the range of values (Panel C). Finally, for the income specialisation index that captures low-income municipal concentration, variations are quite small with some differences in evolutions before treatment between treated and control municipalities (Panel D). We will not be able to eliminate differences in pre-trends, but we will see that the differential evolution between treated and control municipalities has a slope that changes in a sizable way just after treatment.

[Insert Figure 4]

6 The impacts of the SRU law

6.1 Treatment effects on public housing outcomes

We then evaluate the impact of the SRU law on public housing using specification (2). We assess how the log-number of public dwellings evolves in treated municipalities compared to control ones when taking into account municipality characteristics. We regress the log-number of public dwellings on interactions between treatment and year dummies (except for 2000 which serves as a reference) and controls.[21] Figure 5.A represents yearly treatment effects and shows that there is no significant pre-trend (see also Table B.2). Treatment effects are positive from 2002 onwards and increase over time to reach 0.138 in 2008 (see Panel A of Table 2). On average, treated municipalities are characterized by a growth rate of their number of public dwellings which is $(\exp(0.138) - 1) * 100 = 14.8$ percentage points larger than that of control municipalities between 2000 and 2008.[22] This corresponds to a difference in the yearly growth rate of $\left[(1 + 0.148)^{1/8} - 1\right] * 100 = 2.02$ percentage points. This figure is slightly more than half the average yearly growth rate of the number of public dwellings for treated municipalities, which

[21] These controls already detailed in subsection 4.2 include year dummies, interactions between a linear time trend and region dummies, the logarithm of the total number of dwellings in the municipality in 2000 and its square, the logarithm of average household income in the municipality in 2000 and its square, as well as the four corresponding variables at the city level, and interactions between year dummies and dummies for being treated with the SRU law and the LOV law (before 2002). Estimated coefficients are reported in Table B.1.

[22] The magnitude of other effects is computed in a similar way below, but details of the computation are not reported to save space.

is 3.85 percentage points over the 2000-2008 period. To get an idea of the magnitudes, the average number of public dwellings in treated municipalities has increased by 31 between 2000 and 2008 (from 127 to 158). The 19 additional public dwellings due to the SRU law represent 61% of this increase. Note however that this estimated number of additional public dwellings is rather imprecise since its confidence interval is [7; 31].

We then assess if evolutions are different for treated municipalities depending on whether or not they are far from the objective of the SRU law. For that purpose, we consider two dummies reflecting the distance of municipalities to the objective: One for the proportion of public dwellings to be below 5% and one for it to be above 5%. We estimate specification (4) which includes interactions between treatment, year dummies and our two dummies for distance to the objective.[23] Figure 5.B shows that, for municipalities with a proportion of public dwellings above 5%, treatment effects are close to zero and non significant from 2002 onwards. For municipalities with a proportion below 5%, treatment effects are positive and larger than in the homogenous case. The estimated 2008 treatment effect is 0.481 (see Panel B of Table 2), which means that the growth rate of the number of public dwellings is 61.8% larger for treated municipalities than for control ones over the 2000-2008 period. This corresponds to a difference in the yearly growth rate of 5.0 percentage points. Whereas there is an increase of 38 public dwellings during the period (from 52 to 90), the 23 additional public dwellings due to the SRU law represent 61% of that increase. Once again, the estimated number of public dwellings is rather imprecise since its confidence interval is [14; 34].

We then turn to the impact of the SRU law on public housing segregation within municipality, which is measured with the housing dissimilarity index. Figure 5.C represents yearly treatment effects and shows that there is no significant pre-trend. From 2004 onwards, yearly treatment effects are negative and decreasing. The 2008 treatment effect is -3.356 percentage points (see Panel B of Table 2), which represents most of the 4 percentage point decrease in the average public housing dissimilarity index over the 2000-2008 period (from 62.7 to 58.7). Put differently, due to the SRU law, one needs to reallocate around 3.4 percentage points fewer public dwellings to different cadastral sections in treated municipalities than in control ones to reach a uniform distribution within the municipality. As before, one can assess if treatment effects are different for municipalities depending on whether or not they are far from the objective of the SRU law. Figure 5.D shows that the profiles are rather similar for the two groups of municipalities with the treatment effect being larger (in absolute terms) for municipalities far from the objective and reaching -4.606 in 2008.[24] The differences in yearly treatment effects between the two groups are not statistically

[23] As mentioned in Subsection 4.2, we additionally include in the set of controls interactions between a dummy for the proportion of public dwellings being larger than 5% and year fixed effects. This way, the treatment effects are estimated by comparing treated and control municipalities within the same category of distance to the objective of the SRU law (ie. proportion of public dwellings below or above 5%).

[24] When restricting our sample to municipalities in cities with population between 50,000 and 200,000, the effect of the SRU law on treated municipalities with a proportion of public housing above 5% disappears (see Table B.4). It suggests that the effect obtained for those municipalities in our main estimations could be due to their location in cities of different sizes with different practices.

significant.

[Insert Figures 5]

[Insert Table 2]

6.2 Treatment effects on income outcomes

We next turn to the impact of the SRU law on low-income segregation within municipality, measured with the income dissimilarity index. Figure 6.A represents yearly treatment effects and shows that, while there is no pre-trend, the income dissimilarity index is negative and decreasing after the introduction of the SRU law. Nevertheless, estimated treatment effects are small and non-significant every year. We then assess whether treatment effects vary depending on whether or not municipalities are far from the objective of the SRU law. Figure 6.B shows that the decrease of treatment effects after 2000 occurs only for municipalities far from the objective, for which this decrease is steeper than when considering homogenous treatment effects. For them, the 2008 treatment effect is significantly negative and reaches -1.034 percentage points. It represents most of the decrease in the income dissimilarity index over the 2000-2008 period for treated municipalities far from the objective, which amounts to -1.2 percentage points (from 20.4 to 19.2). Put differently, due to the SRU law, one needs to reallocate 1.2 percentage point fewer households of the first income quintile to different cadastral sections in treated municipalities that in control ones to reach a uniform distribution within the municipality. The SRU law would thus decrease low-income segregation to some extent. However, considering the values of treatment effects, the impacts is smaller than for public housing segregation.

Finally, we assess the impact of the SRU law on low-income concentration measured with the specialisation index. Figure 6.C shows that treatments effects between 1996 and 2008 are U-shaped. In particular, it means that there is a negative trend before treatment but it reverts after treatment. In fact, treatment effects are rather small after the introduction of the SRU law since 2008 treatment effect reaches only 0.016 compared to an average specialisation index for treated municipalities of 0.56 in 2000. We investigated further if the effect is robust to alternative specifications and sample definitions. In particular, treated and control municipalities are located in cities which sizes are not comparable. To make them more comparable, we restrict our sample to municipalities in cities with population between 50,000 and 200,000. In that case, the 2008 treatment effect is then close to zero and non significant (Table B.4), suggesting that the positive effect obtained previously would come from specific trends in cities of different sizes. By constrast, results remain qualitatively similar for other outcomes.

[Insert Figure 6]

6.3 Descriptive regressions at the block level

Overall, treatment effects are more important for public housing segregation than for low-income segregation. A possible explanation to this difference is the construction by treated municipalities of public housing intended for medium-income households (PLI and PLS). In fact, the overall proportion of PLI/PLS in public housing for treated municipalities constructing public dwellings reaches 24.2% compared to 16.0% for control municipalities.

Alternatively, treated municipalities may avoid constructing public housing in rich neighborhoods to avoid bothering the rich. We now propose descriptive linear regressions at the block level to investigate the dynamics of public housing in neighborhoods. Since the number of public dwellings in blocks is often zero, we transform this variable using the inverse hyperbolic sine rather than the logarithm (see Bellemare and Wichman, 2020).[25] Indeed, this function is defined in zero (where its value is zero) and it tends to a logarithm when its argument grows. We regress the variation in the inverse hyperbolic sine of the number of public dwellings over the 2000-2008 period on explanatory variables measured in 2000 and municipality fixed effects. Hence, estimation relies on deviations from municipality means. Our explanatory variables are the inverse hyperbolic sine of the number of public dwellings to assess whether public housing concentrates in blocks where it was already present, the average income outside the public housing sector to capture NIMBY and land/housing price effects, and the building density (defined as the number of dwellings divided by the block area). The expected sign of the building density effect is ambiguous. It might be negative because there is a lack of room for new public buildings when density is high, but it might also be positive since the presence of buildings indicates that land use regulation allow for construction.

Table 3 shows that, within treated municipalities with a proportion of public dwellings below 5%, the number of public dwellings increases in blocks where the inverse hyperbolic sine of the number of public dwellings and income outside the public housing sector are below municipal means. Interestingly, the pattern is different for corresponding control municipalities for which the average outcome outside the public housing sector has much less influence. Overall, these results are consistent with both public housing de-segregation, and NIMBY and land/housing prices effects for treated municipalities far from the objective of the SRU law. For those municipalities, building density has a positive and significant effect.[26]

[Insert Table 3]

[25] There is no public housing in 71% of blocks in 2000, and the change in the number of public dwellings between 2000 and 2008 in zero in 79% of blocks. Hence, action occurs only in a limited number of blocks.

[26] Since inverse hyperbolic sine is a specific functional form, one can wonder whether results are robust when estimating alternative specifications. When replacing the inverse hyperbolic sine of the number of public dwellings with the proportion of public dwellings in both the left- and right-hand sides, results are qualitatively similar except for building density which positive effect is not significant anymore for treated municipalities with a proportion of public dwellings below 5% (see Table B.3). Note though that the proportion of public dwellings involves both the numbers of public and private dwellings at its denominator, and the private housing sector may react to building density. By contrast, the number of private dwellings does not intervene in the public housing variables included in our main specification.

We now conduct additional robustness checks, and study additional outcomes and mechanisms.

7 Further estimations

7.1 Robustness checks

We now propose several robustness checks of our main results on the effect of the SRU law on the log-number of public dwellings. We first examine in more details what happens when we restrict the sample to municipalities in cities with population between 50,000 and 200,000 so that cities of treated and control municipalities are of more comparable sizes (see Table B.4). Estimated treatment effects are qualitatively similar but quantitatively larger although less precise. In particular, for municipalities far from the objective, the 2008 treatment effect is 0.747 rather than 0.481 for our main regression. Alternatively, we restrict control municipalities in our main sample to those in cities with at least one treated municipality (see Table B.5). The 2008 treatment effect for municipalities far from the SRU objective is quite similar to our main estimate at 0.542. At the same time, there may be spillovers from treated municipalities to control ones within the same city. We thus re-estimate our specification after restricting control municipalities in our main sample to those in cities with no treated municipality. Results are very close to the main ones.

We then change the municipality population interval used to construct our sample. We consider the three following restrictions: [0 ; 6,000], [800 ; 10,000] in the Paris region and [2,800 ; 10,000] outside that region, and no selection (ie. we keep all municipalities). Results remain qualitatively similar although estimated coefficients vary (see Table B.6). In particular, when considering all municipalities, treatments effects are smaller and the 2008 treatment effect for municipalities far from the objective is now 0.374. Since treated municipalities have a proportion of public dwellings below 20%, we also consider a robustness check in which we keep only treated and control municipalities below that threshold. Results are quite close to main ones.

When estimating heterogeneous effects of the SRU law, we have considered only two categories of municipalities depending on whether the proportion of public dwellings is below or above 5%. We now consider a variant in which we stratify municipalities to a larger extent into 4 groups: below 5%, $[5\%, 10\%[$, $[10\%, 15\%[$ and greater than 15%. Interestingly, the 2008 treatment effect on the log-number of public dwellings varies across categories above 5% (see Table B.7). It is close to zero and non-significant on the $[5\%, 10\%[$ and $[10\%, 15\%[$ intervals, and significantly negative when the proportion of public dwellings is above 15% (but there are only 21 corresponding treated municipalities). These results could be due to heterogeneous behaviors of municipalities with respect to the law such that their compliance varies with factors correlated with the proportion of public dwellings.

We also assess whether results are robust when expanding the list of control variables. We first consider more complex effects by adding interactions between the time trend and the interactions between the logarithm of the total number of dwellings in the municipality in 2000 and the logarithm of average household income in the municipality in 2000, between the logarithm of the total number of dwellings in the municipality in 2000 and the logarithm of the total number of dwellings in the city in 2000, between the logarithm of the total number of dwellings in the city in 2000 and the logarithm of average household income in the city in 2000, between the logarithm of the average household income in the municipality in 2000 and the logarithm of average household income in the city in 2000. We also additionally introduce the greater circle distance of the municipality to the city center interacted with the time trend.[27] Table B.5 shows that our results are only slightly affected.

7.2 Alternative outcomes for public housing and income variables

We then estimate specifications with alternative outcomes for public housing construction. Table 4 shows that results are qualitatively and quantitatively similar when using the number of public dwellings in level and the proportion of public dwellings. In particular, the 2008 treatment effect on the number of public dwellings is found to be 18.6, which is similar to the figure derived when using the log-number of public dwellings.[28] The 2008 treatment effect on the proportion of public dwellings is 0.010. Considering that the average number of dwellings in treated municipalities is 2142, this yields an additional number of public dwellings around 21.

So far, we have studied changes in the number of public dwellings that capture several phenomena: destructions, constructions and transformation of private dwellings into public ones. We now quantify the effect of the SRU law on the construction of new public dwellings using the 2013 RPLS (*Répertoire des logements locatifs des bailleurs sociaux*) which includes information on construction date (see Appendix A.3). To mimic our main approach, our outcome is now the logarithm of the number of public dwellings in 1996 to which is added the number of new public dwellings until the year which is considered. Table 4 reports a 2008 treatment effect of 0.107, which corresponds to a growth rate of 11.3% over the 2000-2008 period and around 14 additional public dwellings. This figure is lower than the 19 additional public dwellings obtained previously, and it suggests that part of these additional public dwellings may come from the transformation of private dwellings into public ones.

Finally, we assess whether our results on low-income segregation are robust when considering the income dissimilarity index for the first quartile of household income rather than the first quintile. As before, SRU has an effect only for municipalities far from the SRU objective (see Table 4). The point estimate at -1.046 is very close to the one obtained when considering the first quintile of household income which is -1.034.

[Insert Table 4]

[27]Municipality coordinates are latitude and longitude of the city hall. The city center coordinates are computed as a the baricenters of municipality coordinates, weighting by the 1999 census population.

[28]Note that estimations when the outcome is the number of public dwellings include observations with value 0 which are excluded when the outcome is the log-number of public dwellings, and results are barely affected.

7.3 Additional mechanisms

The impact of the SRU law may vary across municipalities concerned by the law depending on the local political colour since right-wing local authorities may feel more pressured into avoiding the construction of public dwellings due to the preferences of inhabitants. Table 5 reports results when stratifying the estimations for the log-number of public dwellings depending on whether or not the municipal proportion of right-wing expressed votes at the first round of the 2002 presidential elections is above the median of our sample.[29] When considering municipalities far from the SRU objective, the 2008 treatment effect is smaller in municipalities which are more right wing. At the same time, the estimated difference between municipalities with proportions of right-wing expressed votes above and below the median is not statistically significant.

We also investigate the argument that some municipalities may prefer to raise local taxes when violating the SRU law to pay penalties rather than increase their proportion of public dwellings. For that purpose, we evaluate the impact of the law on housing tax rates described in Appendix A.5. We do not find any significant effect of the law on this tax rate (see Table 5).

We finally turn to the impact of the SRU law on housing prices of second-hand dwellings in treated municipalities. Indeed, we want to investigate the claim that treated municipalities may experience a decrease in their attractiveness. For that purpose, we construct a municipality price index from notary databases (see Appendix A.6). We find that the 2008 treatment effect on housing prices is negative but the order of magnitude is small and the estimated effect is not statistically significant (see Table 5). When considering municipalities far from the SRU objective, the estimated effect is still negative and the magnitude is larger, but again the estimated effect is not significant. Overall, if there is an effect of SRU law on attrativeness, it is not strong enough to be significant.

[Insert Table 5]

8 Conclusion

In this paper, we studied the effects of the SRU law introduced in December 2000 to help low-income households access housing and to increase social mixity. This law imposes medium and large municipalities in large-enough cities to reach a proportion of public housing above 20%. Our empirical investigation was conducted using municipal data constructed mostly from fiscal datasets. A difference-in-differences approach showed that the law increased the number of public dwellings in municipalities with a proportion of public housing below 5%, and decreased the segregation of public housing within those municipalities. It also decreased the segregation of low-income households but to a lesser extent. The effect of the SRU law on social mixity is thus quite small. Still, it would be worth investigating whether accessing public housing in better neighborhoods was beneficial for adults and children

[29] An expressed vote is a vote for a candidate. Blank votes are thus excluded. Right-wing votes include far right ones here.

of low-income families. The design of the law also deserves more attention. Consistently with our results, more social mixity might be obtained by applying the SRU law to blocks rather than municipalities, and by imposing that new public dwellings have to be occupied by low-income households in rich municipalities.

More generally, the different thresholds on municipality size, city size and proportion of public housing used to determine the treatment group are quite ad-hoc. It could be of interest to estimate a spatial general equilibrium model such that public housing is constructed accross space according to a given cost function, demand can exceed the available supply at the local level, and public housing is only accessible to households with modest income (see Geyer and Sieg, 2013; Sieg and Yoon, 2020). Counterfactual situations could then be evaluated such that the law imposes different proportions of public dwellings to municipalities according to a given scheme, with a particular attention to the optimal proportions that maximize welfare. Public housing policies could also be compared to housing subsidy policies that are often considered as less costly.

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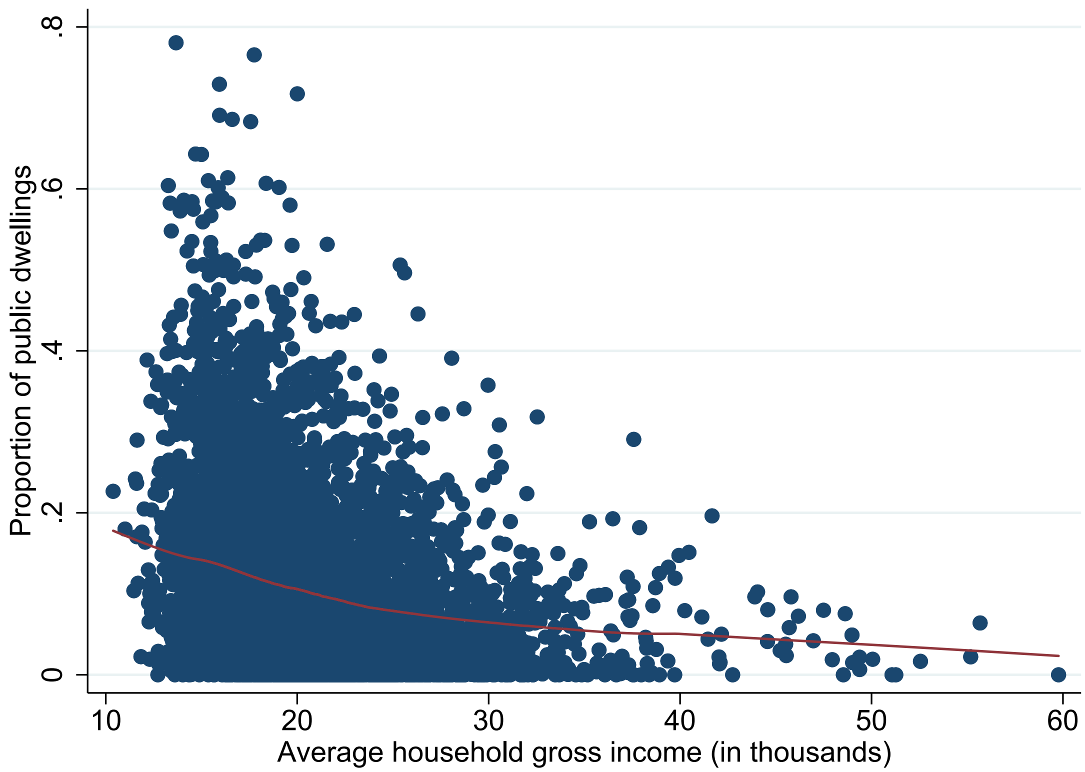
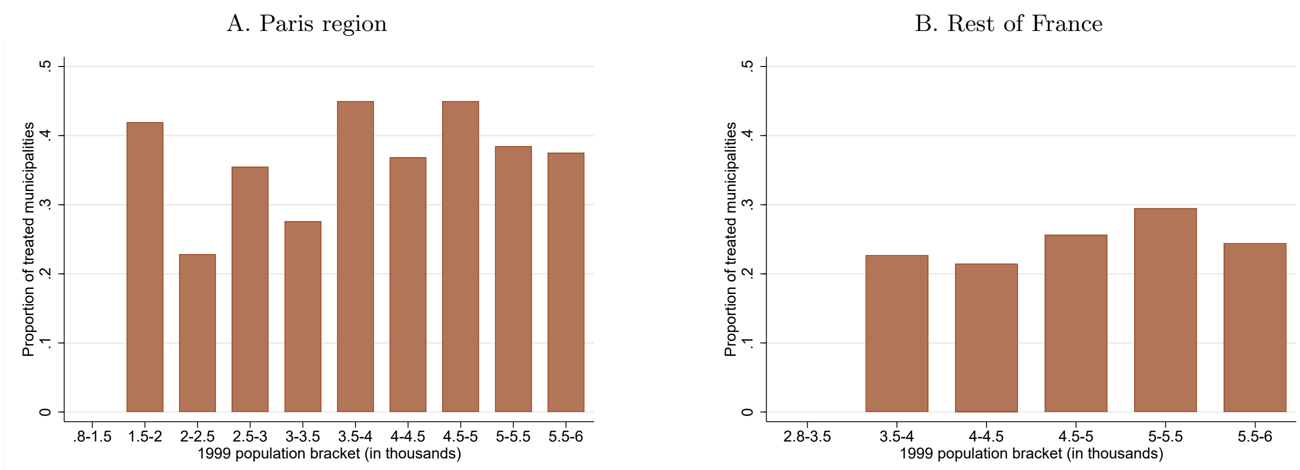
Figure 1: Scatter plot of the proportion of public dwellings as a function of average household gross income at the municipality level



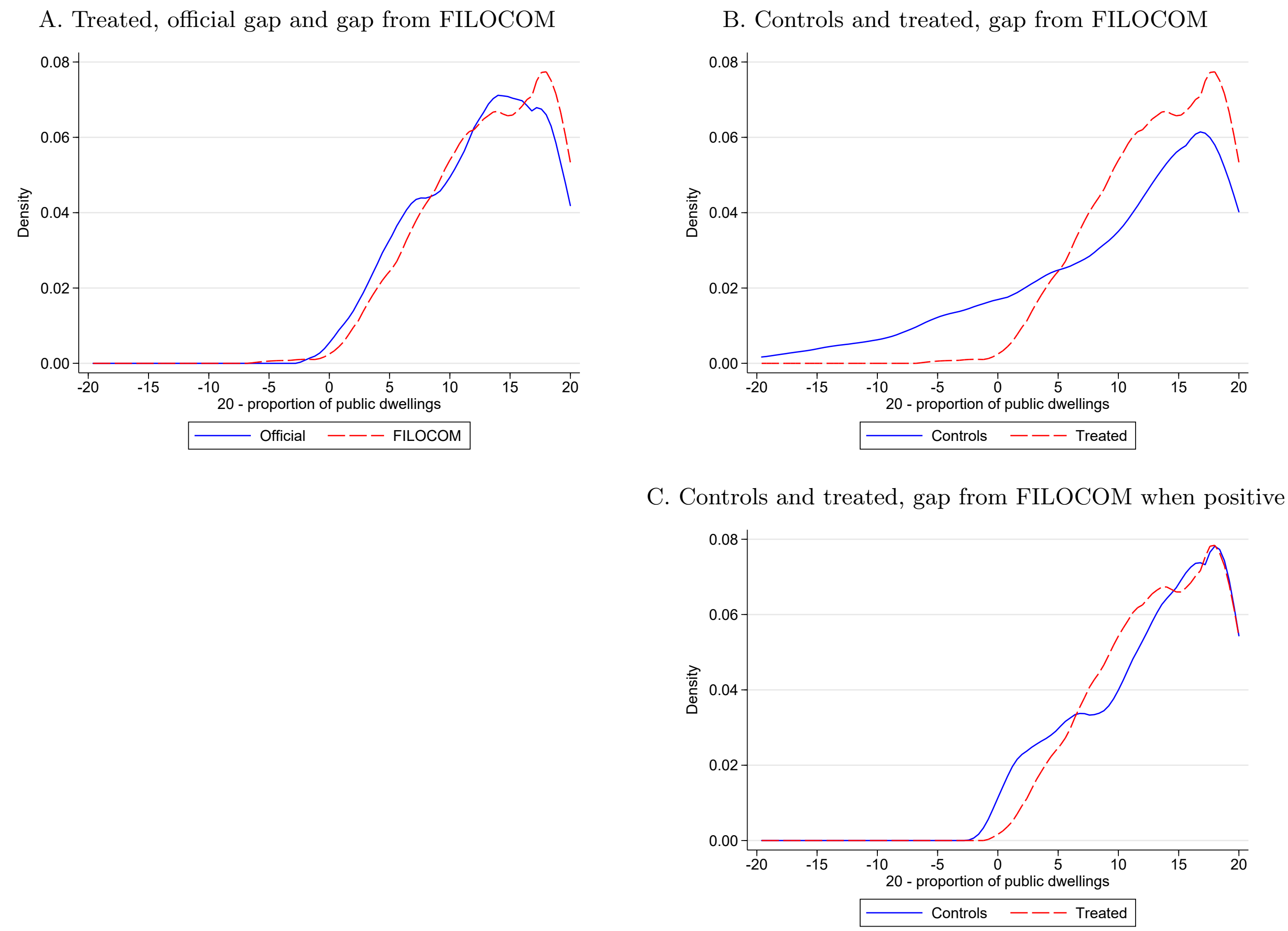
Note: Figures are computed from FILOCOM data for municipalities with 1999 population above 1,500. Non-parametric trend obtained with local linear regressions is represented with a solid line.

Figure 2: Proportion of treated municipalities by 1999 population bracket in the Paris region and the rest of France



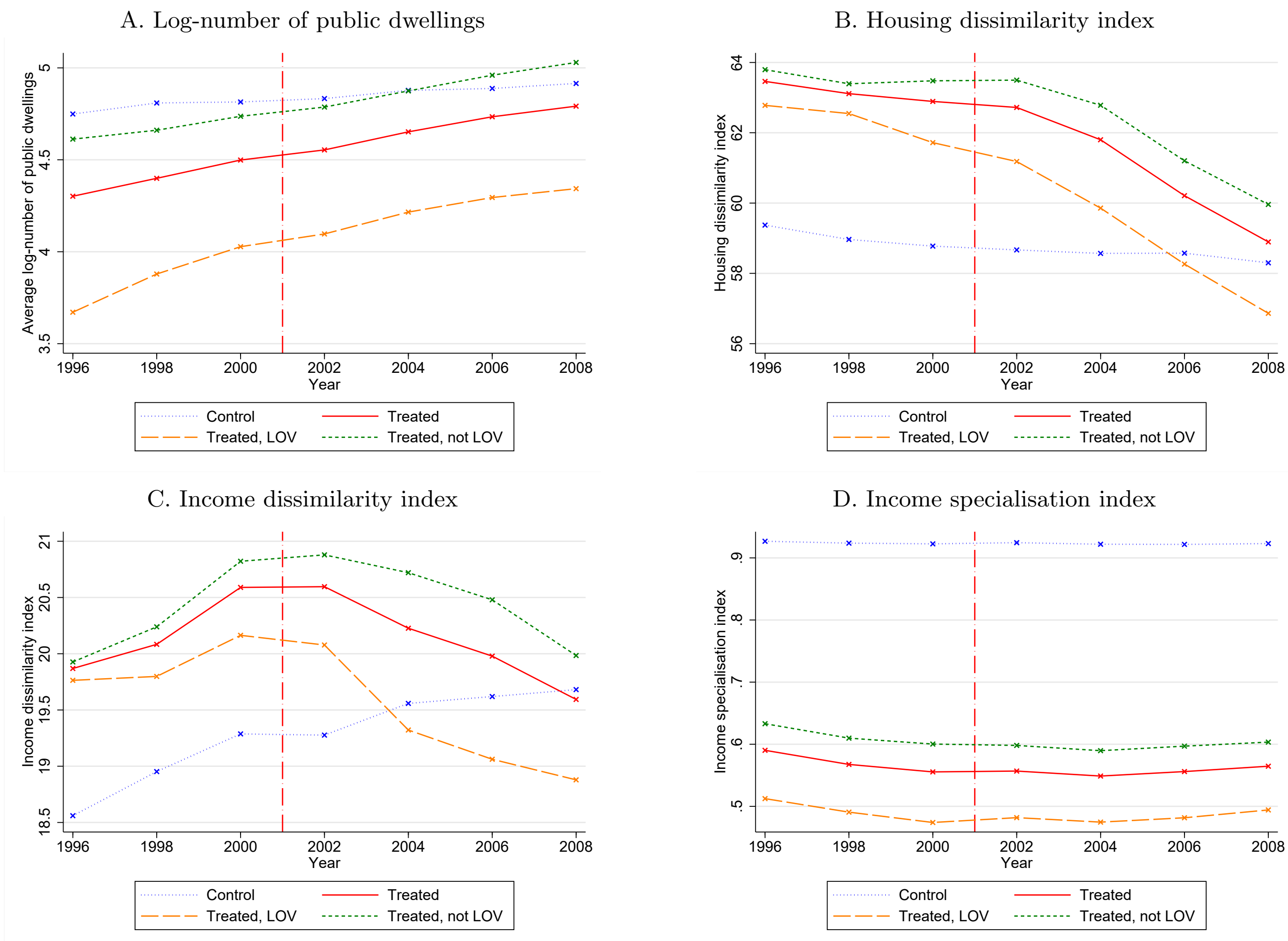
Note: Figures are computed from FILOCOM data for urban municipalities whose 1999 population is between 2,800 and 6,000 when located in the Paris region, and between 800 and 6,000 when located in another region.

Figure 3: Distribution of the gap 20 - proportion of public dwellings (in %) for treated and control groups in 2000



Note: Figures are computed from FILOCOM data for urban municipalities whose 1999 population is between 2,800 and 6,000 when located in the Paris region, and between 800 and 6,000 when located outside that region. For panel C, only municipalities with a positive gap between 20% and the proportion of public dwellings are considered.

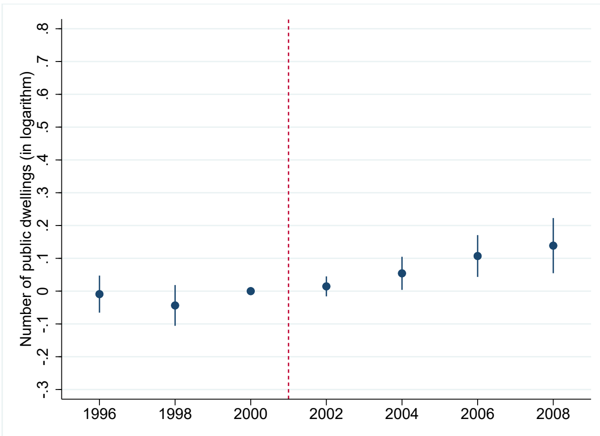
Figure 4: Yearly averages of outcomes for treated and control municipalities over the 1996-2008 period



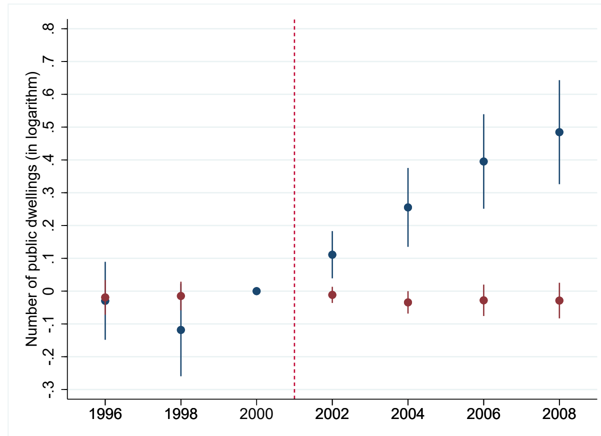
Note: Figures are computed from FILOCOM data for urban municipalities whose 1999 population is between 2,800 and 6,000 when located in the Paris region, and between 800 and 6,000 when located outside that region. Housing dissimilarity index is the Duncan-and-Duncan for public housing; income dissimilarity index is the Duncan-and-Duncan index for households in the first income quintile, where the threshold defining the first income quintile is computed at the city level; income specialisation index is the ratio between the proportions of households in the first income quintile in the municipality and in its city. Municipalities with a single cadastral section are not taken into account for descriptive statistics on dissimilarity indexes.

Figure 5: Yearly difference in the log-number of public dwellings and housing dissimilarity index between treated and control municipalities

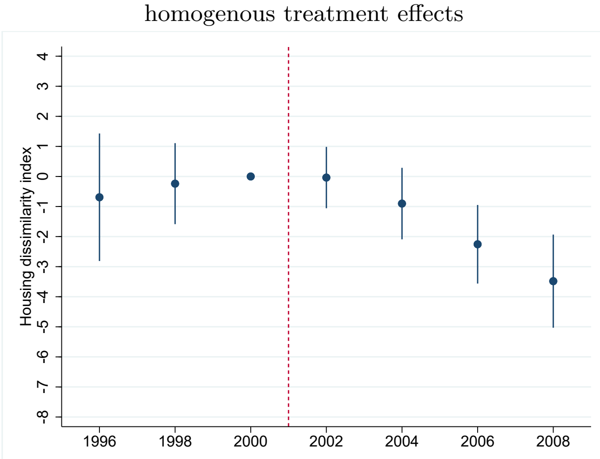
A. Log-number of public dwellings, homogenous treatment effects



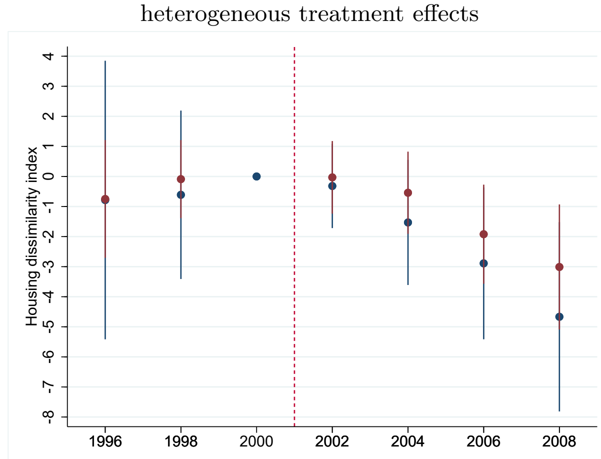
B. Log-number of public dwellings, heterogeneous treatment effects



C. Housing dissimilarity index, homogenous treatment effects



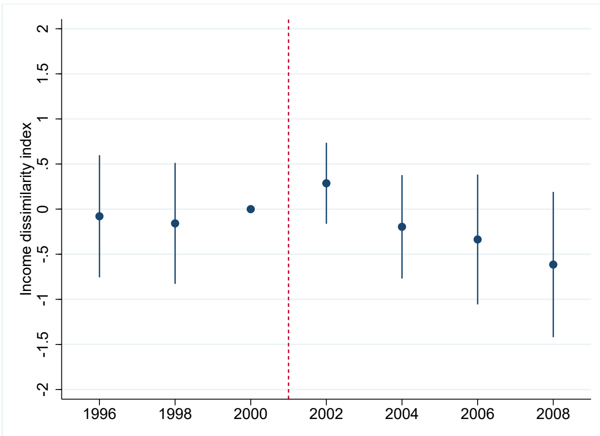
D. Housing dissimilarity index, heterogeneous treatment effects



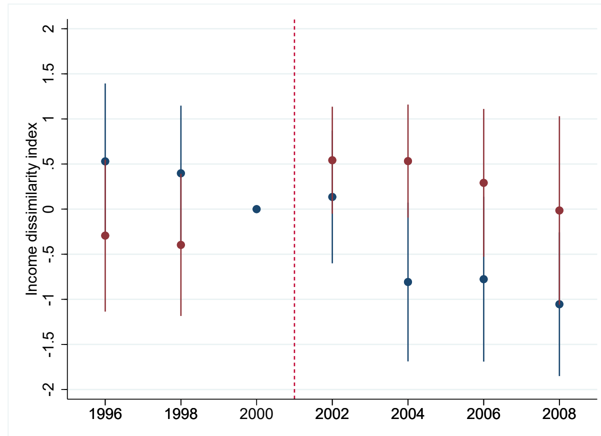
Note: Results are obtained from FILOCOM data on the restricted sample of urban municipalities in mainland France whose 1999 population is between 2,800 and 6,000 for municipalities in the Paris region, and between 800 and 6,000 for those outside that region. "homogenous treatment effects": estimated coefficients of year dummies interacted with treatment; "heterogeneous treatment effects": estimated coefficients of year dummies interacted with dummies for being far or not from the SRU objective and treatment. Estimated coefficients are represented with dots and their confidence intervals with vertical bars (coefficients being fixed to zero in year 2000 which serves as a reference). In the four panels, explanatory variables include municipality fixed effects, and interactions between a time trend and region dummies, the logarithm of the total number of dwellings in the municipality in 2000 and its square, the logarithm of average household income in the municipality in 2000 and its square, as well as the four corresponding variables at the city level. In panels A and C, explanatory variables also include year dummies and the interactions between year dummies and LOV global list treatment (before 2002). In panels B and D, they rather include interactions between year dummies and dummies for being far or not from the SRU objective, and interactions between year dummies and dummies for being far or not from the SRU objective and LOV global list treatment (before 2002). In blue (resp. red): municipalities far (resp. not far) from the SRU objective. Municipalities with a single cadastral section are not taken into account in Panels C and D. Standard errors are clustered at the city level.

Figure 6: Yearly difference in income dissimilarity index and income specialisation index between treated and control municipalities

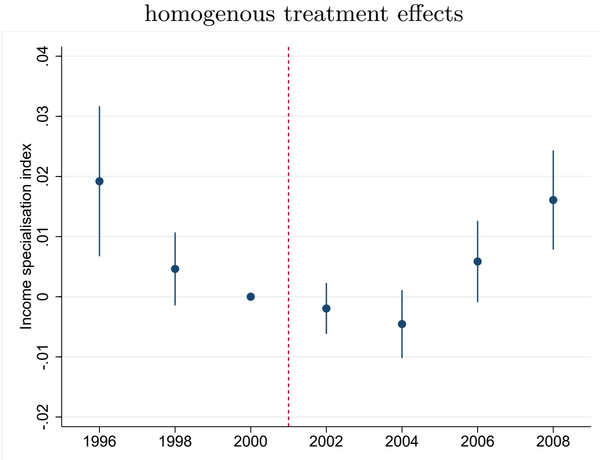
A. Income dissimilarity index, homogenous treatment effects



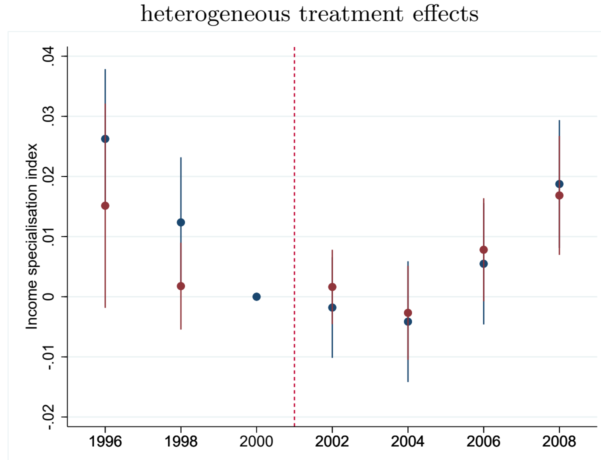
B. Income dissimilarity index, heterogeneous treatment effects



C. Income specialisation index, homogenous treatment effects



D. Income specialisation index, heterogeneous treatment effects



Note: Results are obtained from FILOCOM data on the restricted sample of urban municipalities in mainland France whose 1999 population is between 2,800 and 6,000 for municipalities in the Paris region, and between 800 and 6,000 for those outside that region. "homogenous treatment effects": estimated coefficients of year dummies interacted with treatment; "heterogeneous treatment effects": estimated coefficients of year dummies interacted with dummies for being far or not from the SRU objective and treatment. Estimated coefficients are represented with dots and their confidence intervals with vertical bars (coefficients being fixed to zero in year 2000 which serves as a reference). In the four panels, explanatory variables include municipality fixed effects, and interactions between a time trend and region dummies, the logarithm of the total number of dwellings in the municipality in 2000 and its square, the logarithm of average household income in the municipality in 2000 and its square, as well as the four corresponding variables at the city level. In panels A and C, explanatory variables also include year dummies and the interactions between year dummies and LOV global list treatment (before 2002). In panels B and D, they rather include interactions between year dummies and dummies for being far or not from the SRU objective, and interactions between year dummies and dummies for being far or not from the SRU objective and LOV global list treatment (before 2002). In blue (resp. red): municipalities far (resp. not far) from the SRU objective. Municipalities with a single cadastral section are not taken into account in Panels A and B. Standard errors are clustered at the city level.

Table 1: Descriptive statistics on municipalities in 2000

	Urban	Treated	Non-treated	Urban, restriction	Treated, restriction	Non-treated, restriction
Variables at the municipality level						
Treated	.122	1	0	.179	1	0
	(.327)	(0)	(0)	(.383)	(0)	(0)
Proportion of public dwellings	.101	.085	.103	.099	.067	.106
	(.111)	(.055)	(.116)	(.091)	(.049)	(.097)
1999 Population	6,855	13,133	5,983	3,909	4,337	3,816
	(16,116)	(26,849)	(13,769)	(1,058)	(1,000)	(1,047)
Number of dwellings	3,433	6,745	2,974	1,915	1,870	1,925
	(9,014)	(16,629)	(7,241)	(1,112)	(678)	(1,186)
Average income	20,596	24,991	19,985	20,823	25,442	19,816
	(5,214)	(6,420)	(4,711)	(5,695)	(6,478)	(4,971)
Housing dissimilarity index	58.5	62.6	57.9	59.5	62.9	58.8
	(19.9)	(17.6)	(20.2)	(19.8)	(19.3)	(19.8)
Income dissimilarity index	19	21.1	18.7	19.5	20.6	19.3
	(8.3)	(8.2)	(8.2)	(7.8)	(9.2)	(7.4)
Income specialisation index	.834	.619	.863	.857	.555	.923
	(.268)	(.217)	(.26)	(.247)	(.184)	(.208)
Variables at the city level						
1999 Population	761,665	2,731,645	488,057	666,635	2,527,020	260,940
	(2,384,838)	(3,976,665)	(1,915,669)	(2,242,545)	(3,884,353)	(1,387,153)
Number of dwellings	369,944	1,332,148	236,305	324,014	1,228,867	126,692
	(1,179,360)	(1,975,746)	(946,631)	(1,108,816)	(1,930,136)	(685,355)
Average income	19,487	21,620	19,191	19,762	21,629	19,355
	(3,729)	(3,120)	(3,710)	(4,144)	(3,303)	(4,197)
Number of municipalities	5,945	725	5,220	1,793	321	1,472

Note: Results are obtained from FILOCOM and notary 2000 data on the sample of urban municipalities in mainland France. Arrondissements in Lyon, Marseille and Paris are excluded. A municipality is considered as "Urban" if it is located in a city. Housing dissimilarity index is the Duncan-and-Duncan index computed for public housing; income dissimilarity index is the Duncan-and-Duncan index for households in the first income quintile, where the threshold defining the first income quintile is computed at the city level; income specialisation index is the ratio between the proportions of households in the first income quintile in the municipality and in its city. Municipalities with a single cadastral section are not taken into account for descriptive statistics on dissimilarity indices. Averages are reported, as well as standard deviations in parentheses. "restriction": restriction of the sample to urban municipalities whose 1999 population is between 2,800 and 6,000 when located in the Paris region, and between 800 and 6,000 when located outside that region.

Table 2: Evaluation of treatment effects on main outcomes over the 2000-2008 period

Panel A: Homogenous treatment effects				
	(1)	(2)	(3)	(4)
	Log-number of public dwellings	Housing dissimilarity index	Income dissimilarity index	Low-income specialisation index
Treated	0.138***	-3.356***	-0.602	0.016***
	(0.042)	(0.789)	(0.409)	(0.004)
Region fixed effects	X	X	X	X
Explanatory variables	X	X	X	X
Nb. observations	11,842	11,819	12,532	12,557
R-squared	0.155	0.039	0.035	0.078
Nb. municipalities	1,726	1,723	1,791	1,794
Panel B: Heterogeneous treatment effects				
	(1)	(2)	(3)	(4)
	Log-number of public dwellings	Housing dissimilarity index	Income dissimilarity index	Low-income specialisation index
Treated x proportion $<5\%$	0.481***	-4.606***	-1.034**	0.019***
	(0.080)	(1.607)	(0.404)	(0.005)
Treated x proportion $\geq 5\%$	-0.028	-2.855***	-0.014	0.017***
	(0.028)	(1.084)	(0.531)	(0.005)
Region fixed effects	X	X	X	X
Explanatory variables	X	X	X	X
Nb. observations	11,842	11,819	12,532	12,557
R-squared	0.180	0.041	0.037	0.078
Nb. municipalities	1,726	1,723	1,791	1,794

Note: Results are obtained from FILOCOM data on the restricted sample of urban municipalities in mainland France whose 1999 population is between 2,800 and 6,000 for municipalities in the Paris region, and between 800 and 6,000 for those outside that region. "homogenous treatment effects": coefficients of year dummies interacted with treatment and we report the estimated one for year 2008; "heterogeneous treatment effects": coefficients of year dummies interacted with dummies for being far or not from the SRU objective and treatment and we report the estimated ones for year 2008. The coefficients are fixed to zero for year 2000 which serves as a reference. In all specifications, explanatory variables include municipality fixed effects, and interactions between a time trend and region dummies, the logarithm of the total number of dwellings in the municipality in 2000 and its square, the logarithm of average household income in the municipality in 2000 and its square, as well as the four corresponding variables at the city level. In panel A, explanatory variables also include year dummies and the interactions between year dummies and LOV global list treatment (before 2002). In panel B, they rather include interactions between year dummies and dummies for being far or not from the SRU objective, and interactions between year dummies and dummies for being far or not from the SRU objective and LOV global list treatment (before 2002). Municipalities with a single cadastral section are not taken into account in specifications (2) and (3). Standard errors are clustered at the city level.

Table 3: Regressions results at the block level for 2000-2008 differences in the inverse hyperbolic sine of the number of public dwellings

	Treated	Control	Treated Prop. Soc. $< 5\%$	Control Prop. Soc. $< 5\%$	Treated Prop. Soc. $\geq 5\%$	Control Prop. Soc. $\geq 5\%$
Asinh number of	-0.1185***	-0.0928***	-0.1329***	-0.1050***	-0.1086***	-0.0898***
public dwellings	(0.010)	(0.003)	(0.023)	(0.007)	(0.011)	(0.004)
Log average income outside	-0.1101*	-0.0489**	-0.2344**	-0.0605*	-0.0133	-0.0427
social housing	(0.061)	(0.022)	(0.102)	(0.032)	(0.075)	(0.030)
Log building density	0.1162***	0.0622***	0.1416***	0.0641***	0.0941***	0.0611***
	(0.013)	(0.004)	(0.021)	(0.006)	(0.016)	(0.005)
Municipality fixed effects	X	X	X	X	X	X
Nb. observations	3,880	18,957	1,663	7,279	2,217	11,678
R-squared	0.163	0.180	0.153	0.147	0.168	0.196

Note: Results are obtained from FILOCOM data on the restricted sample of urban municipalities in mainland France whose 1999 population is between 2,800 and 6,000 for municipalities in the Paris region, and between 800 and 6,000 for those outside that region. The observation unit is the cadastral section. "Asinh" stands for inverse hyperbolic sine. Column "treated" (resp. "control") reports results for the subsample of cadastral sections located in treated (resp. control) municipalities. "Prop. Soc. <5% (resp. ≥ 5%) corresponds to the additional restriction to municipalities with a proportion of public housing below 5% (resp. above 5%).

Table 4: Evaluation of treatment effects on alternative outcomes over the 2000-2008 period

Panel A: Homogenous treatment effects	(1) Number of public dwellings	(2) Proportion of public dwellings	(3) Log-number of public dwellings from construction	(4) Income dissimilarity index, 1^{st} quartile
Treated	18.605***	0.010***	0.107**	-0.467
	(2.979)	(0.002)	(0.044)	(0.446)
Region fixed effects	X	X	X	X
Explanatory variables	X	X	X	X
Nb. observations	12,557	12,557	11,885	12,532
R-squared	0.141	0.066	0.315	0.040
Nb. municipalities	1,794	1,794	1,721	1,791
Panel B: Heterogeneous treatment effects				
	(1) Number of public dwellings	(2) Proportion of public dwellings	(3) Log-number of public dwellings from construction	(4) Income dissimilarity index, 1^{st} quartile
Treated x proportion <5%	24.917***	0.017***	0.397***	-1.046***
	(4.415)	(0.002)	(0.069)	(0.401)
Treated x proportion ≥5%	17.483***	0.005**	-0.043	0.208
	(3.672)	(0.002)	(0.033)	(0.503)
Region fixed effects	X	X	X	X
Explanatory variables	X	X	X	X
Nb. observations	12,557	12,557	11,885	12,532
R-squared	0.144	0.072	0.343	0.043
Nb. municipalities	1,794	1,794	1,721	1,791

Note: Results are obtained from FILOCOM data on the restricted sample of urban municipalities in mainland France whose 1999 population is between 2,800 and 6,000 for municipalities in the Paris region, and between 800 and 6,000 for those outside that region. "homogenous treatment effects": coefficients of year dummies interacted with treatment and we report the estimated one for year 2008; "heterogeneous treatment effects": coefficients of year dummies interacted with dummies for being far or not from the SRU objective and treatment and we report the estimated ones for year 2008. The coefficients are fixed to zero for year 2000 which serves as a reference. In all specifications, explanatory variables include municipality fixed effects, and interactions between a time trend and region dummies, the logarithm of the total number of dwellings in the municipality in 2000 and its square, the logarithm of average household income in the municipality in 2000 and its square, as well as the four corresponding variables at the city level. In panel A, explanatory variables also include year dummies and the interactions between year dummies and LOV global list treatment (before 2002). In panel B, they rather include interactions between year dummies and dummies for being far or not from the SRU objective, and interactions between year dummies and dummies for being far or not from the SRU objective and LOV global list treatment (before 2002). Log-number of public dwellings from construction at date t used in specification (3) is computed as the number of public dwellings in 1996 to which is added the number of public dwellings constructed between 1996 and year t. Municipalities with a single cadastral section are not taken into account in specification (4). Standard errors are clustered at the city level.

Table 5: Evaluation of treatment effects on additional outcomes over the 2000-2008 period

Panel A: Homogenous treatment effects				
	(1)	(2)	(3)	(4)
	Log-number of public dwellings, right vote < median	Log-number of public dwellings, right vote ≥ median	Log-housing price index	Log-housing tax
Treated	0.114***	0.226*	-0.013	-0.023
	(0.041)	(0.123)	(0.016)	(0.026)
Region fixed effects	X	X	X	X
Explanatory variables	X	X	X	X
Nb. observations	8,632	3,210	8,903	8,966
R-squared	0.205	0.097	0.785	0.251
Nb. municipalities	1,253	473	1,794	1,794
Panel B: Heterogeneous treatment effects				
	(1)	(2)	(3)	(4)
	Log-number of public dwellings, right vote < median	Log-number of public dwellings, right vote ≥ median	Log-housing price	Log-housing tax
Treated x proportion<5%	0.500***	0.394*	-0.031	0.026
	(0.080)	(0.207)	(0.020)	(0.049)
Treated x proportion≥5%	-0.039	0.066	-0.005	0.009
	(0.028)	(0.074)	(0.017)	(0.031)
Region fixed effects	X	X	X	X
Explanatory variables	X	X	X	X
Nb. observations	8,632	3,210	8,903	8,966
R-squared	0.244	0.103	0.785	0.254
Nb. municipalities	1,253	473	1,794	1,794

Note: Results are obtained from FILOCOM data on the restricted sample of urban municipalities in mainland France whose 1999 population is between 2,800 and 6,000 for municipalities in the Paris region, and between 800 and 6,000 for those outside that region. For specifications (1) and (2), data are available only over the 2000-2008 period. "homogenous treatment effects": coefficients of year dummies interacted with treatment and we report the estimated one for year 2008; "heterogeneous treatment effects": coefficients of year dummies interacted with dummies for being far or not from the SRU objective and treatment and we report the estimated ones for year 2008. The coefficients are fixed to zero for year 2000 which serves as a reference. In all specifications, explanatory variables include municipality fixed effects, and interactions between a time trend and region dummies, the logarithm of the total number of dwellings in the municipality in 2000 and its square, the logarithm of average household income in the municipality in 2000 and its square, as well as the four corresponding variables at the city level. In panel A, explanatory variables also include year dummies and the interactions between year dummies and LOV global list treatment (before 2002). In panel B, they rather include interactions between year dummies and dummies for being far or not from the SRU objective, and interactions between year dummies and dummies for being far or not from the SRU objective and LOV global list treatment (before 2002). Standard errors are clustered at the city level.

A Data appendix

A.1 Data on municipalities treated with *Loi d'orientation de la Ville* (LOV)

LOV law was voted in 1991 but its implementation was postponed several times. It was finally implemented in 1995 but the final list of the 209 municipalities concerned by the law is not available. However, we could construct an approximate list which is quite accurate.

Indeed, we did a search for paper records at National Archives and found the initial list of municipalities considered by the French Ministry of Cities for LOV law. This list contains the 460 municipalities verifying the three criteria: A proportion of public dwellings among main residences below 20%, a proportion of social benefits recipients among primary residences below 18%, and a urban unit population larger than 200,000. Importantly, there is initially no municipal population threshold to be concerned by the law. It was added later in 1995 and it was decided that municipalities would be concerned only if their 1990 population was above 3,500 inhabitants. There was also some bargaining from municipalities at the limits of thresholds and the final list of municipalities is not strictly the list of the 224 municipalities meeting the four eligibility criteria.

We could also access paper records of the 1995-1997 overview report for municipalities with 1990 population above 10,000. Matching these records with the list of municipalities meeting the four criteria, we could identify 16 municipalities in the list that were not included in the set of concerned municipalities. In fact, these are municipalities at the limit of the eligibility thresholds. There were also two municipalities in the overview record that were not in the list (for unclear reasons) and they were added to the list. We end up with a list of 210 municipalities verifying the municipality population constraint that are in the overview report when having a population above 10,000 (and 462 municipalities when ignoring the municipality population threshold, whereas the figure mentioned by *Le Moniteur* newspaper on October 21, 1994 is 466). For the 1998-2000 period, it was decided to lower the population threshold for municipalities in the Paris region to 1,500. Consequently, we extend our list for that period to include the additional municipalities in the Paris region with population between 1,500 and 3,500, and we end up with 232 municipalities.

A.2 Intra-municipal housing and income dissimilarity indexes

The FILOCOM data contain 30.3 million dwellings for mainland France in 2000 (among which 23.4 million are main residences). Our housing and income segregation indices are computed from cadastral sections for each municipality. There are 36.6 thousand municipalities and 212.3 thousand cadastral sections in mainland France in 2000. Comparatively, our selected sample includes 1,785 municipalities and 23.2 thousand cadastral sections. For now on, we only report descriptive statistics computed on our sample. The mean (resp. median) number of cadastral sections in a municipality is 11 (resp. 13). The number of dwellings in cadastral sections varies between 1 and 4,322, and its mean and median are respectively 137 and 72. Housing (resp. income) segregation is measured with the dissimilarity index proposed by Duncan and Duncan (1955) applied to the number of public dwellings among all dwellings (resp. the number of households in the first income quintile among all households). We detail this index for housing. It verifies:

$$D_j = \frac{1}{2} \sum_{h \in \mathcal{H}_j} \left| \frac{p_{j,h}}{p_j} - \frac{d_{j,h} - p_{j,h}}{d_j - p_j} \right| \tag{5}$$

where j indices the municipality, h indices the cadastral section, $\mathcal{H}_j$ is the set of cadastral sections in municipality j, d_j is the total number of dwellings in the municipality, p_j is the total number of public dwellings in the municipality, $d_{j,h}$ is the number of dwellings in cadastral section h of the municipality and $p_{j,h}$ is the number of public dwellings in cadastral section h of the municipality. This index can be computed only for municipalities that include at least one public dwelling. It varies between 0 and 1.

In 2000, the average and median values of the housing dissimilarity index are respectively 0.59 and 0.61. Moreover, the first quartile is as high as 0.47, which points at a significant concentration of public dwellings within municipalities in our selected sample. The average and median values of the income dissimilarity index are respectively 0.19 and 0.20. Moreover, the third quartile is as low as 0.24, which points at a rather low concentration of low-income housholds within municipalities in our selected sample.

A.3 Construction data

For the construction of new public dwellings, we use the 2013 RPLS (*Répertoire des logements locatifs des bailleurs sociaux*) data. RPLS is an exhaustive cross-section dataset of all public dwellings that includes the municipality code and construction date. The number of new public dwellings in a municipality within a given period is approximated with the number of public dwellings located in that municipality whose construction date is reported to be within that period. This approximation is valid if the number of new public dwellings in the municipality that disappear during that period is negligible. This holds in practice since only very few public dwellings built after January 1, 1996 (the first date from which we consider the construction of new public dwellings) were sold to their occupant or demolished before 2013. Indeed, public dwellings are the collateral of very long term loans and are thus very unlikely to be destroyed before the term of the debt contracted by the landlord which is around 40 years (Hoorens, 2013).

A.4 Election data

Data on votes at the first round of the 2002 presidential elections were recovered from the webpage of the French Ministry of Home Affairs. These data give the proportions of votes for the different candidates for each municipality. Proportions are computed considering only expressed votes (ie. votes that are not blank). We reaggregated votes for right-wind candidates, including far right ones, using the classification given by Wikipedia. Those candidates include Jacques Chirac, Jean Saint-Josse, Christine Boutin, Alain Madelin, Bruno Mégret and Jean-Marie Le Pen.

A.5 Housing tax data

We could recover residence tax data by municipality for every year over the 2000-2008 period on the website of the Ministry of Economic and Financial Affairs.[30] We only use data every even year consistently with our other datasets.

A.6 Housing prices data

A.6.1 Datasets

Notary databases gather information on all transactions of second-hand dwellings occurring in mainland France. They are available every even year over the 2000-2008 period. Transactions are reported by the regional chamber of notaries in the Paris region (BIEN database) and other regions (PERVAL database) at the end of the year. For instance, the total number of transactions involving second-hand dwellings is around 800,000 per year in the early 2000s (Friggit, 2008).

Premises not used as residences, agricultural properties, garages and private parkings are excluded from our analysis. Notary databases include the municipal and cadastral section identifiers, the construction year of the dwelling in interval brackets (before 1850, 1850-1913, 1914-1947, 1948-1959, 1960-1980, 1981-1991 and after 1991, or missing which occurs for around 30% of observations), the dwelling type (single-family house or flat), the floor area and the price. A specific procedure is implemented to impute floor areas that are missing for 25.7% of the transactions (see Appendix A.6.2).

To construct a municipal price index, the logarithm of prices per squared meter is regressed on dummies for the construction period in brackets and for the quarter. One regression is conducted for every available year and dwelling type, and dummies for construction after 1991 and first quarter are excluded. The municipal price index for a given year is then defined as the exponential of the municipal average of the constant plus residual for that year.

A.6.2 Floor area imputation in the notary database

We attribute to dwellings with missing values for floor area in Notary databases, the average floor area of dwellings in FILOCOM data located in the same cadastral section, involved in a transaction during the same year, which are of the same type (single-family house or flat) and have the same number of rooms.

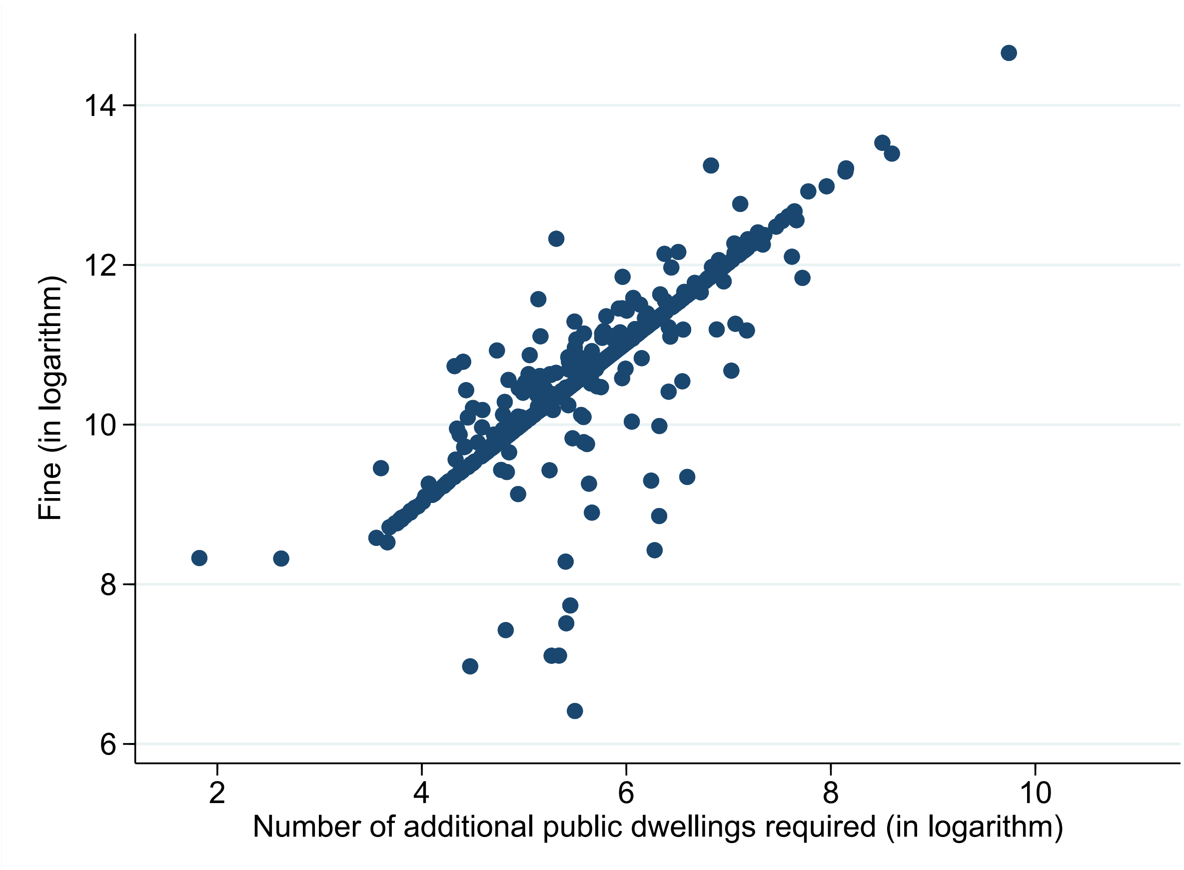
We assess the accuracy of this approach by imputing the floor area for dwellings for which it is not missing and by comparing the imputed values with the observed values. The average absolute error is around 5%, and the R^2 of the regression of the observed floor area on the imputed one is around 0.75. The imputation is more accurate for flats (for which the two values are respectively 2% and 0.83) than for single-family dwellings (for which they are

[30] Data were scrapped from the website: https://www.impots.gouv.fr/portail/statistiques.

respectively 15% and 0.51). The imputation makes the proportion of dwellings with missing floor area decrease to 5.1%, and the observations with remaining missing values are discarded.

B Appendix Figures

Figure C.1: Relationship between the fine and additional public dwellings required according to the SRU law



Note: Constructed from figures provided by the national office for urban planning and housing (DHUP).

C Appendix Tables

Table B.1: Evaluation of treatment effects on main outcomes over the 2000-2008 period, estimated coefficients for explanatory variables when treatment effect is homogenous

	(1) Log-number of public dwellings	(2) Housing dissimilarity index	(3) Income dissimilarity index	(4) Income specialisation index
LOV x year 1996	-0.264***	0.494	-0.492	0.002
	(0.058)	(1.821)	(0.549)	(0.007)
LOV x year 1998	-0.109*	0.440	-0.316	-0.004
	(0.056)	(1.445)	(0.439)	(0.004)
LOV x year 2000	-0.059	0.110	0.277	-0.014***
	(0.038)	(1.229)	(0.366)	(0.004)
Log-number of dwellings x t	0.102**	-0.725	-0.085	0.013**
	(0.051)	(1.429)	(0.431)	(0.006)
Log-number of dwellings^2 x t	-0.007**	0.057	0.009	-0.001*
	(0.003)	(0.092)	(0.028)	(0.000)
Log-mean income x t	0.372	15.374*	0.131	-0.072
	(0.630)	(9.035)	(4.004)	(0.061)
Log-mean income^2 x t	-0.016	-0.804*	-0.029	0.003
	(0.032)	(0.445)	(0.199)	(0.003)
Log-number of dwellings	-0.004	-0.025	-0.134**	-0.004***
in city x t	(0.006)	(0.142)	(0.052)	(0.001)
Log-number of dwellings	0.000	0.001	0.006***	0.000***
in city^2 x t	(0.000)	(0.007)	(0.002)	(0.000)
Log-mean income in city x t	-0.498	-15.083*	0.327	0.060
	(0.613)	(8.571)	(4.151)	(0.062)
Log-mean income in city^2 x t	0.025	0.796*	-0.015	-0.003
	(0.031)	(0.421)	(0.208)	(0.003)
Region fixed effects	X	X	X	X
Explanatory variables	X	X	X	X
Nb. observations	11,842	11,819	12,532	12,557
R-squared	0.155	0.039	0.035	0.078
Nb. municipalities	1,726	1,723	1,791	1,794

Note: Results are obtained from FILOCOM data on the restricted sample of urban municipalities in mainland France whose 1999 population is between 2,800 and 6,000 for municipalities in the Paris region, and between 800 and 6,000 for those outside that region. Municipalities with a single cadastral section are not taken into account in specifications (2) and (3). Standard errors are clustered at the city level.

Table B.2: Evaluation of treatment effects on main outcomes over the 1996-2000 period

	(1) Log-number of public dwellings	(2) Housing dissimilarity index	(3) Income dissimilarity index	(4) Income specialisation index
Panel A: Homogenous treatment effects				
Treated	0.009	0.761	0.082	-0.019***
	(0.029)	(1.085)	(0.345)	(0.006)
Region fixed effects	X	X	X	X
Explanatory variables	X	X	X	X
Nb. observations	11,842	11,819	12,532	12,557
R-squared	0.155	0.039	0.035	0.078
Nb. municipalities	1,726	1,723	1,791	1,794
Panel B: Heterogeneous treatment effects				
	(1) Log-number of public dwellings	(2) Housing dissimilarity index	(3) Income dissimilarity index	(4) Income specialisation index
Treated x proportion $<5\%$	0.028	0.834	-0.523	-0.026***
	(0.060)	(2.357)	(0.440)	(0.006)
Treated x proportion $\geq 5\%$	0.020	0.820	0.292	-0.015*
	(0.027)	(1.013)	(0.427)	(0.009)
Region fixed effects	X	X	X	X
Explanatory variables	X	X	X	X
Nb. observations	11,842	11,819	12,532	12,557
R-squared	0.180	0.041	0.037	0.078
Nb. municipalities	1,726	1,723	1,791	1,794

Note: Results are obtained from FILOCOM data on the restricted sample of urban municipalities in mainland France whose 1999 population is between 2,800 and 6,000 for municipalities in the Paris region, and between 800 and 6,000 for those outside that region. "homogenous treatment effects": coefficients of year dummies interacted with treatment and we report minus the estimated one for year 1996; "heterogeneous treatment effects": coefficients of year dummies interacted with dummies for being far or not from the SRU objective and treatment and we report minus the estimated ones for year 1996. The coefficients are fixed to zero for year 2000 which serves as a reference. In all specifications, explanatory variables include municipality fixed effects, and interactions between a time trend and region dummies, the logarithm of the total number of dwellings in the municipality in 2000 and its square, the logarithm of average household income in the municipality in 2000 and its square, as well as the four corresponding variables at the city level. In panel A, explanatory variables also include year dummies and the interactions between year dummies and LOV global list treatment (before 2002). In panel B, they rather include interactions between year dummies and dummies for being far or not from the SRU objective, and interactions between year dummies and dummies for being far or not from the SRU objective and LOV global list treatment (before 2002). Municipalities with a single cadastral section are not taken into account in specifications (2) and (3). Standard errors are clustered at the city level.

Table B.3: Regressions results at the block level for 2000-2008 differences in the proportion of public dwellings

	Treated	Control	Treated Prop. Soc. $< 5\%$	Control Prop. Soc. $< 5\%$	Treated Prop. Soc. $\geq 5\%$	Control Prop. Soc. $\geq 5\%$
Proportion of public dwellings	-0.1285***	-0.1200***	-0.0805***	-0.2015***	-0.1340***	-0.1139***
	(0.009)	(0.003)	(0.024)	(0.006)	(0.010)	(0.003)
Log average income outside	0.0060	-0.0028*	-0.0123**	0.0003	0.0195***	-0.0056**
social housing	(0.004)	(0.001)	(0.006)	(0.001)	(0.005)	(0.002)
Log-building density	0.0019**	0.0015***	0.0007	0.0016***	0.0026**	0.0015***
	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)
Municipality fixed effects	X	X	X	X	X	X
Nb. observations	3,880	18,957	1,663	7,279	2,217	11,678
R-squared	0.156	0.230	0.113	0.202	0.182	0.238

Note: Results are obtained from FILOCOM data on the restricted sample of cadastral sections in urban municipalities in mainland France whose 1999 population is between 2,800 and 6,000 for municipalities in the Paris region, and between 800 and 6,000 for those outside that region. Column "treated" (resp. "control") reports results for the subsample of cadastral sections located in treated (resp. control) municipalities. "Prop. Soc. $<5\%$ (resp. $\geq 5\%$) corresponds to the additional restriction to municipalities with a proportion of public housing below 5% (resp. above 5%).

Table B.4: Evaluation of treatment effects on main outcomes over the 2000-2008 period, restriction to municipalities in cities with population between 50,000 and 200,000

Panel A: Homogenous treatment effects				
	(1)	(2)	(3)	(4)
	Log-number of public dwellings	Housing dissimilarity index	Income dissimilarity index	Income specialisation index
Treated	0.192**	-2.022	0.706	-0.004
	(0.091)	(1.747)	(0.821)	(0.010)
Region fixed effects	X	X	X	X
Explanatory variables	X	X	X	X
Nb. observations	1,473	1,473	1,526	1,526
R-squared	0.314	0.142	0.112	0.139
Nb. municipalities	214	214	218	218
Panel B: Heterogeneous treatment effects				
	(1)	(2)	(3)	(4)
	Log-number of public dwellings	Housing dissimilarity index	Income dissimilarity index	Income specialisation index
Treated x proportion<5%	0.747***	-6.427**	-1.321	0.002
	(0.170)	(2.830)	(1.046)	(0.015)
Treated x proportion≥5%	0.005	-0.439	1.185	-0.007
	(0.078)	(1.692)	(0.902)	(0.011)
Region fixed effects	X	X	X	X
Explanatory variables	X	X	X	X
Nb. observations	1,473	1,473	1,526	1,526
R-squared	0.390	0.152	0.132	0.145
Nb. municipalities	214	214	218	218

Note: Results are obtained from FILOCOM data on the restricted sample of urban municipalities in mainland France whose 1999 population is between 2,800 and 6,000 for municipalities in the Paris region, and between 800 and 6,000 for those outside that region. "homogenous treatment effects": coefficients of year dummies interacted with treatment and we report the estimated one for year 2008; "heterogeneous treatment effects": coefficients of year dummies interacted with dummies for being far or not from the SRU objective and treatment and we report the estimated ones for year 2008. The coefficients are fixed to zero for year 2000 which serves as a reference. In all specifications, explanatory variables include municipality fixed effects, and interactions between a time trend and region dummies, the logarithm of the total number of dwellings in the municipality in 2000 and its square, the logarithm of average household income in the municipality in 2000 and its square, as well as the four corresponding variables at the city level. In panel A, explanatory variables also include year dummies and the interactions between year dummies and LOV global list treatment (before 2002). In panel B, they rather include interactions between year dummies and dummies for being far or not from the SRU objective, and interactions between year dummies and dummies for being far or not from the SRU objective and LOV global list treatment (before 2002). Municipalities with a single cadastral section are not taken into account in specifications (2) and (3). Standard errors are clustered at the city level.

Table B.5: Evaluation of treatment effects on log-number of public dwellings over the 2000-2008 period, alternative selection on cities to be considered and additional controls

Panel A: Homogenous treatment effects				
	(1)	(2)	(3)	(4)
	Controls in cities with at least one treated municipality	Controls in cities with no treated municipality	Additional controls: 2nd order and interactions	Additional controls 2: same and distance to city centre
Treated	0.162***	0.130*	0.138***	0.140***
	(0.042)	(0.069)	(0.042)	(0.045)
Region fixed effects	X	X	X	X
Explanatory variables	X	X	X	X
Nb. observations	3,555	10,406	11,842	11,842
R-squared	0.268	0.148	0.156	0.156
Nb. municipalities	520	1,516	1,726	1,726
Panel B: Heterogeneous treatment effects				
	(1)	(2)	(3)	(4)
	Controls in cities with at least one treated municipality	Controls in cities with no treated municipality	Additional controls: 2nd order and interactions	Additional controls 2: same and distance to urban unit center
Treated x proportion<5%	0.542***	0.499***	0.485***	0.499***
	(0.074)	(0.112)	(0.079)	(0.082)
Treated x proportion≥5%	-0.006	-0.008	-0.022	-0.020
	(0.031)	(0.053)	(0.028)	(0.029)
Region fixed effects	X	X	X	X
Explanatory variables	X	X	X	X
Nb. observations	3,555	10,406	11,842	11,842
R-squared	0.332	0.175	0.182	0.183
Nb. municipalities	520	1,516	1,726	1,726

Note: Results are obtained from FILOCOM data on the restricted sample of urban municipalities in mainland France whose 1999 population is between 2,800 and 6,000 for municipalities in the Paris region, and between 800 and 6,000 for those outside that region. "homogenous treatment effects": coefficients of year dummies interacted with treatment and we report the estimated one for year 2008; "heterogeneous treatment effects": coefficients of year dummies interacted with dummies for being far or not from the SRU objective and treatment and we report the estimated ones for year 2008. The coefficients are fixed to zero for year 2000 which serves as a reference. In all specifications, explanatory variables include municipality fixed effects, and interactions between a time trend and region dummies, the logarithm of the total number of dwellings in the municipality in 2000 and its square, the logarithm of average household income in the municipality in 2000 and its square, as well as the four corresponding variables at the city level. In panel A, explanatory variables also include year dummies and the interactions between year dummies and LOV global list treatment (before 2002). In panel B, they rather include interactions between year dummies and dummies for being far or not from the SRU objective, and interactions between year dummies and dummies for being far or not from the SRU objective and LOV global list treatment (before 2002). In column (1), control municipalities are restricted to those located in cities with at least one treated municipality; in column (2), control municipalities are restricted to those located in cities with no treated municipality; in column (3), we introduce additional explanatory variables that are interactions between the time trend and: the interactions between the logarithm of the total number of dwellings in the municipality in 2000 and the logarithm of average household income in the municipality in 2000, between the logarithm of the total number of dwellings in the municipality in 2000 and the logarithm of the total number of dwellings in the city in 2000, between the logarithm of the total number of dwellings in the city in 2000 and the logarithm of average household income in the city in 2000, between the logarithm of the average household income in the municipality in 2000 and the logarithm of average household income in the city in 2000; in column (4), we introduce the same additional explanatory variables as in column (3) as well as the distance of the municipality to the city center interacted with the time trend. Standard errors are clustered at the city level.

Table B.6: Evaluation of treatment effects on log-number of public dwellings over the 2000-2008 period, alternative restrictions on the municipalities

	(1)	(2)	(3)	(4)
Panel A: Homogenous treatment effects				
	[0 ; 6,000]	[800 ; 10,000] Paris region [2,800 ; 10,000] rest of France	All urban municipalities	Proportion of public dwellings < 20%
Treated	0.160***	0.100***	0.067***	0.095*
	(0.049)	(0.029)	(0.022)	(0.049)
Region fixed effects	X	X	X	X
Explanatory variables	X	X	X	X
Nb. observations	25,150	16,166	35,519	10,135
R-squared	0.130	0.145	0.121	0.171
Nb. municipalities	3,791	2,347	5,279	1,482
Panel B: Heterogeneous treatment effects				
	(1)	(2)	(3)	(4)
	[0 ; 6,000]	[800 ; 10,000] Paris region [2,800 ; 10,000] rest of France	All urban municipalities	Proportion of public dwellings < 20%
Treated x proportion<5%	0.502***	0.439***	0.374***	0.444***
	(0.090)	(0.062)	(0.056)	(0.083)
Treated x proportion≥5%	-0.026	-0.032	-0.033*	-0.072**
	(0.029)	(0.024)	(0.018)	(0.035)
Region fixed effects	X	X	X	X
Explanatory variables	X	X	X	X
Nb. observations	25,150	16,166	35,519	10,135
R-squared	0.140	0.169	0.131	0.199
Nb. municipalities	3,791	2,347	5,279	1,482

Note: Results are obtained from FILOCOM data on the restricted sample of urban municipalities in mainland France whose 1999 population is between 2,800 and 6,000 for municipalities in the Paris region, and between 800 and 6,000 for those outside that region. "homogenous treatment effects": coefficients of year dummies interacted with treatment and we report the estimated one for year 2008; "heterogeneous treatment effects": coefficients of year dummies interacted with dummies for being far or not from the SRU objective and treatment and we report the estimated ones for year 2008. The coefficients are fixed to zero for year 2000 which serves as a reference. In all specifications, explanatory variables include municipality fixed effects, and interactions between a time trend and region dummies, the logarithm of the total number of dwellings in the municipality in 2000 and its square, the logarithm of average household income in the municipality in 2000 and its square, as well as the four corresponding variables at the city level. In panel A, explanatory variables also include year dummies and the interactions between year dummies and LOV global list treatment (before 2002). In panel B, they rather include interactions between year dummies and dummies for being far or not from the SRU objective, and interactions between year dummies and dummies for being far or not from the SRU objective and LOV global list treatment (before 2002). In column (1), the sample is restricted to urban municipalities with 1999 population in $[0; 6,000]$; in column (2), it is restricted to urban municipalities in the Paris region with 1999 population in $[800; 10,000]$ and in the rest of France with 1999 population in $[2800; 10,000]$; in column (3), we use all urban municipalities; in column (4), the sample is restricted to urban municipalities with a proportion of public dwellings below 20%. Standard errors are clustered at the city level.

Table B.7: Evaluation of treatment effects on log-number of public dwellings over the 2000-2008 period, when considering four categories for distance to the objective

Panel A: 2000-2008	(1) Log-number of public dwellings	(2) Housing dissimilarity index	(3) Income dissimilarity index	(4) Income specialisation index
Treated x proportion $< 5\%$	0.482***	-4.633***	-1.053***	0.019***
	(0.080)	(1.603)	(0.405)	(0.005)
Treated x prop.$\in [5\%, 10\%[$	0.014	-4.426***	-0.826	0.019**
	(0.034)	(1.379)	(0.707)	(0.008)
Treated x prop.$\in [10\%, 15\%[$	-0.029	-1.546	0.490	0.022***
	(0.036)	(1.416)	(0.679)	(0.008)
Treated x proportion $\geq 15\%$	-0.184**	-0.664	1.501	-0.009
	(0.088)	(1.904)	(0.992)	(0.011)
Region fixed effects	X	X	X	X
Explanatory variables	X	X	X	X
Nb. observations	11,842	11,819	12,532	12,557
R-squared	0.182	0.046	0.040	0.082
Nb. municipalities	1,726	1,723	1,791	1,794

Note: Results are obtained from FILOCOM data on the restricted sample of urban municipalities in mainland France whose 1999 population is between 2,800 and 6,000 for municipalities in the Paris region, and between 800 and 6,000 for those outside that region. Treatment effects are the estimated coefficients of year dummies interacted with treatment and dummies for the proportion of public dwellings being in $[5\%, 10\%[$, in $[10\%, 15\%[$ or larger than 15% in 2000, and we report the estimated treatment effects for year 2008. The coefficients are fixed to zero for year 2000 which serves as a reference. In all specifications, explanatory variables include municipality fixed effects, interactions between a time trend and region dummies, the logarithm of the total number of dwellings in the municipality in 2000 and its square, the logarithm of average household income in the municipality in 2000 and its square, as well as the four corresponding variables at the city level, year dummies interacted with dummies for the proportion of public dwellings being in $[5\%, 10\%[$, in $[10\%, 15\%[$ or larger than 15% in 2000, and year dummies interacted with dummies for the proportion of public dwellings being in $[5\%, 10\%[$, in $[10\%, 15\%[$ or larger than 15% in 2000 and dummies for being far or not from the SRU objective and LOV global list treatment (before 2002). Standard errors are clustered at the city level.