Online Appendix (not for publication) of "Promoting Social Housing : Insights from Redevelopment Policies in Paris"

Lauriane Belloy* and Fabien Candau†

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Abstract

In this supplementary analysis of our article "Promoting Social Housing: Insights from Redevelopment Policies in Paris," currently under revision at the Journal of Housing Economics, we conduct a series of robustness checks to ensure that the results remain consistent across different methodological choices. In the paper, we find that laws aimed at redeveloping social housing from vacant offices and commercial units have been largely ineffective. Only one law (in 2009) has shown a significant effect in the center of Paris for two years; thereafter, the redevelopment of social dwellings - which represent more than 80% of the social housing supply in Paris - has not been significantly influenced by the law, except possibly in a narrow area on the periphery of Paris. Our findings hold under various and significant changes, including changes to the definitions of the center and the periphery of Paris, the choice of estimator (OLS, PPML, and Non-Parametric Synthetic Control Method), and the introduction of different controls.

JEL Classification: R12, R20, R52.

Key Words: Neighborhoods, Real Estate Demand, Redevelopment Supply, Gentrification

1 Introduction

In Belloy and Candau (2024), we analyze three redevelopment policies that increase incentives to convert offices and other commercial units into social dwellings. We find that only the most restrictive law of 2009 has successfully increased the redevelopment of social housing in central Paris. Our analysis

*Universite de Pau et des Pays de l'Adour, E2S UPPA, CNRS, TREE, Pau, France.

†Universite de Pau et des Pays de l'Adour, E2S UPPA, CNRS, TREE, Pau, France. Mail: fabien.candau@univ-pau.fr

utilized two empirical methods. Firstly, we used a Spatial Regression Discontinuity design (SRD) at the boundary of the areas where these laws were applied (the so-called compensation or reinforced area). This analysis, by design, examines the effect of the law on the periphery of Paris. To analyze the effect in the center, we employed a Synthetic Difference-in-Differences (SDID). To keep the paper concise and clear, we have excluded several robustness checks, which are presented here.

To gain a better understanding of the mechanisms at play, we present a stylized model in Section 2 that help us to present the market of compensation and redevelopment in a simple way. In Section 3, we redefine the center and periphery by testing the impact of varying distances (bandwidths) from the boundary of the compensation area. In Section 4, we set differently the dummies that defined each laws to measure the cumulative effects of these regulations. Section 5 introduces several controls such as income, Airbnb listings - which, by converting housing into commercial use, can trigger a shift towards social housing under these laws - and other variables. While the paper primarily utilizes a Difference-in-Discontinuity (Diff-in-Disc) estimation for the periphery, Section 6 introduces a Synthetic Difference-in-Differences (SDID) estimation. In Section 7, we exclude all areas with no social housing redevelopment (which are quite numerous at the beginning of the period) and apply the OLS estimator to the remaining data. Section 8 employs a Non-Parametric Synthetic Control Method to verify our results using a different approach to compute the counterfactual weights. In Section 9, we construct a synthetic control three times, before each law, to determine if results vary compared to computations based on the period prior to the implementation of the first law in 2009.

2 A Stylized Model

We present here a stylized model similar to Garcia-López et al. (2020) to separate the drivers of supply and demand in property conversion. In this model, property redevelopment developers choose to market their converted homes either to individual households or social landlords. A testable outcome of this model is that these laws increase the long-term supply of social housing, contingent on several factors. In particular, an increase in household income, a decline in state investment in social housing, or a reluctance among households to reside in areas with social housings, counteracts the regulation by diminishing the long-term demand for social housing. This model also illustrates that the costs to compensation tend to be higher in areas with high tension. This model is not integrate in the text since it oversimplifies the effects of the different laws. In particular, the trade-off to invest at the Periphery of the compensation zone or at the Core that changes with the different laws (as discussed on Proposition 1 and 2) is not introduced in this analysis. The model is nevertheless useful to discuss the supply of conversion and also to pinpoint demand shifters that should be controled in the empirical section.

Applicants for conversion, hereafter called Property Redevelopment Developers (PRD), aim to sell their goods at a price p_g to social landlords or at a price p_h to high/middle-income households (more precisely to households with an income too high to benefit from social housing), hereafter called "households". We consider two different areas: the compensation zone, named the center or the core, represented by a superscript c, and the rest of the city, the suburb or the periphery, labeled by an upper script p.

Each PRD faces a cost κ to convert its building. Since the "2:1 rule" implies that a PRD has to pay twice as much compensation titles for private housing[1] than for social housing in the center of the city, this law implies a comparision between $p_h - 2\kappa$ and $p_p - \kappa$. This cost of conversion κ can be magnified (or reduced) depending on the spatial constraint of the law. The regulation can indeed be very restrictive by requiring the doubling of the surface area in the same district, less restrictive by imposing at least 50% in the district, or not very restrictive by enabling the applicant to compensate elsewhere.[2] As explained earlier, the compensation rates tend to be higher in areas with high tension. Therefore, the requirement to provide compensation in the same district makes this regulation much more restrictive there than in other places.

Furthermore, we assume that past regulations concerning social housing involve several additional costs that vary from one PRD to another. These heterogeneous costs are denoted β_n for each developer, n, that converts an unit in social housing.

At the equilibrium, there is a marginal PRD who is indifferent between selling to an household or to a social landlord at:[3]

$$p_h^c - p_g^c = \kappa - \beta_n^*. \tag{1}$$

PRD with $\beta_n < \beta_n^*$ convert their goods to social housing, while those with $\beta_n > \beta_n^*$ convert to private housing.

The indirect utility of an household h that has chosen its optimal private housing in the neighborhood c is $V_h^c = Y - p_h^c - \alpha\mathbb{P}(\beta_n) + e_h^c$, where Y is its (exogeneous) income while $\alpha\mathbb{P}(\beta_n)$ represents a negative externality which increases in the number of premises converted in social housing in this neighborhood. This externality can be explained by an homophily in social preferences that leads these individuals to prefer environment with people sharing

[1]In fact, the PRD first redevelops housing into offices and then has to double the surface of private housing, we take a short-cut here by not mentionning the intermediate step concerning offices/commercial premises redevelopment.

[2]The current model presents only one part of the mechanism, indeed, the compensation title κ which act as a production tax for the PRD is somewhat like a subvention for the specialized entities that sell these titles. Hence an increase in the restrictiveness of the law may, in a second step, reduce the demand of κ and then the production of social housing by the seller of these titles. Adding this second step, however, could make the presentation more cumbersome without adding much clarity.

[3]With $\kappa > \beta_n$ the price of private housing is higher than the price of social housing. There are many evidences of this, for instance a French real estate developer declared in 2013 that the different regulations imply that "the developer finds themselves obliged to raise the price for private buyers who, as a result, largely finance social housing".

the same level of income (Currarini et al., 2009), or due to an insecurity feeling in disadvantaged areas, or to the bad reputation of schools there.[4]

Finally, this household has a choice to live in the center or at the periphery with e_h^c the idiosyncratic preference to live in the center.

We denoted $\overline{V_h^p}$ the indirect utility of the household in p. By comparing these two levels of welfare, the marginal household that chose to live in c proposes the following price:

$$p_h^c = Y - \alpha \mathbb{P}(\beta_n^*) + e_h^* - \overline{V_h^p}. \tag{2}$$

All the households with $e_h > e_h^*$ lives in the center, while other are at the periphery.

The indirect utility that a social landlord gets in c is $V_g^c = G - p_g + e_g$ where G is the public good or service provided to low-income individuals. Indeed, the primary objective of the social landlord, as established by law, is to provide good-quality housing for low-income and disadvantaged individuals. e_g is its idiosyncratic preference to acquire a unit in c. $\overline{V_g^p}$ is its indirect utility at the periphery. The price proposed by the marginal social landlord is then given by:

$$p_g^c = G + e_g^* - \overline{V_g^p}. \tag{3}$$

Social landlords with $e_g > e_g^*$ invest in the center, while other are at the periphery.

Finally we assume that β_n, e_h and e_g follow a normal distribution on the support [0,1], such as $1 - e_h^*$ and $1 - e_g^*$ are the shares of residents and social landlord that invest in c. With C the number of unit converted, we have $C\beta_n^* = 1 - e_g^*$ such as the supply of social housing equals the demand, while market clearing for private housing gives $C(1 - \beta_n^*) = 1 - e_h^*$.

Inserting (2) and (3) in (1), and using the market clearing condition gives:

$$\beta_n^* = \frac{\kappa + G - Y + C + \overline{V_h^p} - \overline{V_g^s}}{1 - \alpha + 2C} \tag{4}$$

This expression shows that an increase in the restrictiveness of the law κ, foster an eviction effect from private housing to social one. Then the first proposition of this model is that the three laws of 2009, 2011, and 2012 should have resulted in an increase in the number of social housing.

This equation also indicates that the share of HLM increases with the aversion of household to social mixity α. An increase in the concentration of social housing in a particular location deter households from settling there, which in turn, automatically elevates the proportion of social housing.

[4]There is indeed some evidence about a link between territorial stigmatization and schools. In particular, Garrouste and Lafourcade (2022) carefully identify how a zoning reform in France, that signals the poverty of the neighborhood, has triggered a drop in pupil enrollment from parents that avoides the public schools in this policy area. This "zone-and-shame" effect, in part illustrates what the aversion to live near HLM, α, may be.

An increase in the income of households Y also reduces the share of social housing. This result matters for the empirical analysis, since our period of analysis is characterized by successive variation in income.[5] Not modelled here a change in the market potential of districts, may also have a similar role than Y_r, abeit with multiple additional effects (See Redding, 2023 for a survey).[6]

An higher effort of landlord to provide good-quality housing G, fosters the share of social housing. An higher effort of the central and/or local government to stimulate the investment in social housing, or to reduce the risk of investment for the PDR[7] can also be understood via an increase in G.

Finally, β_n is a decreasing function of the stock of unit, C, that are converted. When there is a high number of converted housing, the law is less bidding for conversion toward private housing. The larger the number of units, the easier it is to comply with the legal rule of doubling the surface when converting on office into a private housing. This can have an impact that differ from one law to another. Indeed the rule to compensate in the same district in 2009, makes this law more difficult to implement in the center due to the limited stock of conversion available.

3 Bandwith choices

The SRD and SDID were based on specific definitions of the center and the periphery, which depended on the choice of bandwidths. For example, when we selected a bandwidth of [-300, 300] for the SRD, we analyzed a narrow periphery where the treated areas were within 300 meters of the compensation area's boundary (with control areas just beyond this line within 300 meters). To assess how the law affects the rest of the compensation area, we employed the SDID. In this case, we considered all treated areas inside but excluded those within 300 meters of the boundary, thus defining a larger center. The two analyses are complementary, yet based on ad-hoc definitions. To demonstrate that the choice of bandwidths does not significantly influence the results, we present outcomes for various distances from the boundary of the compensation zone

[5]For instance, the financial crisis of 2007-08 has led to a fall of income that might have affected the demand of household in the districts. In contrast, the rise of income in central Paris over the period 2010-2018 may have play a central role by contradicting the effect of the law.

[6]In that case the mechanism may play as follows, an increase in the number of firms in one location, directly reduce the supply of converted unit and increase the price of land, but also indirectly increase the potential number of resident for a given supply of floor space. This, in turn, bids up the price for residential floor space, which reduces the expected utility to convert a premise in HLM. If firms operates under increasing returns or benefit of technological spillovers, agglomeration economies are likely, and then may increase the wage of residents pushing social housing elsewhere *via* Y_r.

[7]For instance in France, the 2000 law on urban solidarity and renewal (SRU) has allowed landlord to directly buy housing from real estate developer before the construction (called "ventes en état futur d'achèvement"). This has been a powerful mechanism by which real estate developers have secured their investment for private housing. Indeed the buying of entire block by social landlord, before the construction, reduces the credit rationing of PDR by banks in period of recession.

(from 300 to 900 meters).

One objective of this section is to determine whether the significant effects of the 2011 and 2014 laws at the periphery, and the significant impact of the 2009 law in the center, are consistently observed.

Table (1) provides the main results of the Diff-in-Disc at the border of the compensation area. We find here similar results than those presented in the paper. The 2009's law is not significant at the periphery of compensation area, while the two others laws have a significant effect. Like in the paper, Table (2) shows a significant positive result on the redevelopment of social housing in the center for many different bandwidths.

Table 1: Difference in Discontinuity of Social Housing Change - Triangular weights

Bandwidth	400	500	600	700	800	900
Treated in 2009	0.222	0.192	0.144	0.0969	0.0836	0.0367
	(0.173)	(0.157)	(0.146)	(0.142)	(0.144)	(0.146)
Treated in 2011	0.647*	0.626*	0.658**	0.657**	0.641**	0.657***
	(0.370)	(0.347)	(0.322)	(0.292)	(0.269)	(0.252)
Treated in 2014	0.492**	0.527**	0.545**	0.533**	0.514***	0.519***
	(0.238)	(0.230)	(0.221)	(0.210))	(0.199)	(0.190)
Constant	6.773***	6.689***	6.592***	6.552***	6.549***	6.520***
	(0.0926)	(0.104)	(0.114)	(0.114)	(0.121)	(0.128)
Observations	1,792	2,100	2,352	2,660	2,912	3,234
Pseudo-R2	0.9027	0.8987	0.8948	0.8905	0.8865	0.8837
Neighborhood FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses; a p<0.01, b p<0.05, c p<0.1

Table 2: Difference in Differences of Social Housing Change - Core Estimation

Bandwidth	400	500	600	700	800	900
Treated in 2009	0.737**	1.065**	1.236**	1.376	2.794**	3.109
	(0.364)	(0.486)	(0.620)	(0.898)	(1.388)	(2.420)
Treated in 2011	-0.128	-0.426	-0.433	-0.931*	-1.551***	-0.620
	(0.284)	(0.318)	(0.406)	(0.529)	(0.583)	(0.855)
Treated in 2014	-0.431	-0.454	-0.607	-1.065	-1.466	-2.427*
	(0.488)	(0.628)	(0.720)	(0.904)	(1.092)	(1.434)
Constant	5.590***	5.655***	5.719***	5.788***	5.733***	5.580***
	(0.250)	(0.265)	(0.279)	(0.294)	(0.333)	(0.462)
Observations	7,826	7,672	7,574	7,462	7,364	7,182
Pseudo-R2	0.835	0.836	0.847	0.853	0.857	0.857
Neighborhood FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses; a $p<0.01$, b $p<0.05$, c $p<0.1$

4 Cumulative effects of laws

In the article, the dummies are set to 1 following each law until the end of the period. This analyzis enables to measure the additional impact of each change in law, always comparing the effects with the pre-intervention trend (before 2009), which exhibits parallel trends. But we can set these dummies differently in order to measure the cumulative effect of each laws as presented in the next table. Column 1 uses a single variable taking the value 0 before 2009; 1 between 2009 and 2010; 2 between 2011 and 2013 and 3 between 2014 and 2019. Column 2 corresponds to different dummies time1=1 in 2009 and 2010 and 0 otherwise; time2=1 in 2011, 2012 and 2013 and 0 otherwise and time3=1 in 2014, 2015, 2016, 2017, 2018 and 2019 and 0 otherwise. Finally, column 3 is the one used in the paper, with different dummies that equal to time1=1 between 2009 and 2019 and 0 before; time2=1 between 2011 and 2019 and 0 otherwise; time3=1 between 2014 and 2019 and 0 otherwise. The results are similar of what we found in the paper.

Table 3: Social housing changes according to the time variables used at the periphery

Dependent variable	Social housing Change		
Bandwidth (in meter)	[0-600]		
	Time 1	Time 2	Time 3
	(1)	(2)	(3)
Treated in 2009	0.1444	-0.1045	0.1444
	(0.146)	(0.105)	(0.146)
Treated in 2011	0.8028***	0.4882**	0.6584***
	(0.379)	(0.283)	(0.322)
Treated in 2014	1.3473***	1.2674***	0.5445***
	(0.499)	(0.504)	(0.221)
Constant	6.592***	6.5943***	6.592***
	(0.114)	(0.118)	(0.114)
Observations	2,352	2,352	2,352
R^2 adj.	0.8948	0.8945	0.8948

Notes: Standard errors are cluster at the neighborhood level in parentheses [a] p<0.01, [b] p<0.05, [c] p<0.1. Results are obtained from a Synthetic difference in differences and Difference in Discontinuity using the PPML estimator. Columns (1) use 600m bandwidth and represent social housing change from Difference in Discontinuity with time=0 before 2009 ; time=1 between 2009 and 2010 ; time=2 between 2011 and 2013 and time=3 after 2013. Columns (2) use 600m bandwidth and represent social housing change from Difference in Discontinuity whith time1=0 before 2009 and after 2010 and 1 in 2009 and 2010 ; time2=0 before 2011 and after 2013 and 1 in 2011, 2012 and 2013 ; time3=0 before 2013 and 1 after . Column (3) use 600m bandwidth to the center and represent social housing change from Difference in Discontinuity with time1=0 before 2009 and 1 in 2009 and after ; time2=0 before 2011 and 1 in 2011 and after ; time3=0 before 2014 and 1 in 2014 and after . Each estimate includes neighborhood and year fixed effects.

Table 4: Social housing changes according to the time variables used at the center

Dependent variable	Social housing Change		
Bandwidth (in meter)	]600-center]		
	Time 1	Time 2	Time 3
	(1)	(2)	(3)
Treated in 2009	1.2343**	1.2324**	1.2343**
	(0.620)	(0.621)	(0.620)
Treated in 2011	0.8048	1.3900	-0.4295
	(0.831)	(0.851)	(0.405)
Treated in 2014	0.1851	0.6574	-0.6197
	(1.083)	(1.023)	(0.720)
Constant	5.725***	5.5499***	5.7252***
	(0.278)	(0.306)	(0.278)
Observations	7,574	7,574	7,574
R^2 adj.	0.8469	0.8461	0.8469

Notes: Standard errors are cluster at the neighborhood level in parentheses ^{a} p<0.01, ^{b} p<0.05, ^{c} p<0.1. Results are obtained from a Synthetic difference in differences and Difference in Discontinuity using the PPML estimator. Columns (1) use 600m bandwidth and represent social housing change from Synthetic Difference-in-Differences with time=0 before 2009 ; time=1 between 2009 and 2010 ; time=2 between 2011 and 2013 and time=3 after 2013. Columns (2) use 600m bandwidth and represent social housing change from Synthetic Difference-in-Differences whith time1=0 before 2009 and after 2010 and 1 in 2009 and 2010 ; time2=0 before 2011 and after 2013 and 1 in 2011, 2012 and 2013 ; time3=0 before 2013 and 1 after . Column (3) use 600m bandwidth to the center and represent social housing change from Synthetic Difference-in-Differences with time1=0 before 2009 and 1 in 2009 and after ; time2=0 before 2011 and 1 in 2011 and after ; time3=0 before 2014 and 1 in 2014 and after . Each estimate includes neighborhood and year fixed effects.

5 Additional controls

In this section we add several control variables to analyze how our baselines results are affected.

5.1 Data

Income

Income levels, by impacting the demand for private housings, directly affect their prices and may subsequently diminish the supply of redevelopment from private housing into commercial units. On the other hand, observing the goods market, a concentration of high incomes can encourage commercial activities, which in turn boosts the demand for such redevelopment. The introduction of this variable however poses several challenges, first reverse causality is likely, the level of income in one district may directly depends on the share of social housing leading to a bias in our estimates. Furthermore, income are potentially correlated with other variables of controls. Finally, controlling for incomes but not for other variables that simultaneously affects it and our dependent variable (which is quite unavoidable) creates a new pattern of bias, since the variable of income is in that case a collider or, in the word of Angrist and Pischke (2009), a bad control. Such a bad control can provide false results by, for instance, changing the sign of coefficient of interest. Then, we introduce income progressively to detect multicolinearity. We also consider a rich set of fixed effects to control for omitted variables in order to reduce the collider bias when income is considered, and finally we use other variables of controls than income for which the poblem of reverse causality is less acute.

Income data comes from INSEE (Institut Nationale de la Statistique et des Etudes Economiques) and represents the median annual income of individuals living in each neighborhood studied between 2006 and 2019. To give an idea of income distribution, the average median income in the compensation zone was €27636 per year in 2006, compared with €35877 in 2019. In the control zone, median annual income was €18961 in 2006 and €23883 in 2019.

Short-term rentals

As explained in the data section, any change of use from a private unit to a commercial unit to develop short-term rental *cause* a change in designation. In other term, Airbnb is a mechanical determinant of change in designation from commercial unit to residential unit. Obviously we cannot rule out reverse causality, namely that an owner entering in a new designated private housing chose to change the use of one part of it its dwelling, however such a change is more likely to happen some years latter. A second problem of this control, is that it is not sure that all the owners that enter in the short-rental market declare it to the administration, in particular at the start of the period.

Airbnb data comes from the Open Data Soft platform, a French company that makes these data available. The origin of these data comes from Inside Airbnb, a site created by Murray Cox, an American activist, making available data from short-term rentals web-scraped on the Airbnb site in the city of Paris. As we don't have the historical data, we used the web-scraped information on the October 3^{rd} 2020 using the account creation dates of owners with a listing on the Airbnb site. These data indicates that the last Airbnb account creation was in 2017, furthermore there is few data before 2012, then when using these controls we restricted our analyses to the period 2012-2017.

Reputation of secondary schools

To consider another aspect of the location choice of households that plays of the choice of redevelopment, we use a measure of the reputation and quality of schools. There is indeed a large literature that emphasizes the role of schools in residential choices (Bayer et al., 2007). In France, parents are partially constrained by a legal map of school districts that depends on the residence, which explains why the residential choice and the school choice are linked. Like the income variable, a simple indicator of the reputation/quality of schools is problematic due to reverse causality. It can be negatively affected by the number of social housing in this district. We thus chose indicators that may be less prone to this problem. These indicators, named “indicators of added value” (IAV) of secondary schools, measure the school’s ability to support its students through to the baccalaureate (namely the first academic degree that grants the completion of secondary education).

Computed by the statistics department of the French Ministry of Education, hereafter DEPP (Direction de l’Evaluation, de la Prospective et de la Performance), the added value is obtained by the difference between the result of each student and its expected result. The modeling relies on a multilevel logistic estimation at the student level. The expected result, for a student, is obtained by applying the model’s coefficients to their individual characteristics (e.g. grades) and collective characteristics (socio-economic variables such as parents’ professions), from which the effect of the school is subtracted. The predicted rate corresponds to the probability of the student’s success (or access) if they were in an "average" high school. Or to put it differently, since these indicators control for the profile of each school according to the social background and age of the school’s pupils, questions about the endogeneity of this variable are less obvious than for simple indicators of performance.

Another interest is that these indicators are regularly used to rank secondary schools by newspapers[8] and then represent an available source for individuals.

5.2 Results

5.2.1 At the periphery

We introduce here, in the diff-in-disc estimation, the number of units converted that may influence the supply over time depending on the location as well as other variables that influence the demand side such as the median income in the district. As discussed at lengh in the data section, these variables generate several problems (multicolinearity, endogeneity) and are thus analyzed successively to observe how our coefficients of interest (β) are affected. Table (5) presents the results. The total number of conversion has a positive effect on the number of social housing (Column 1), while the median income has a negative effect (Column 2). In these two cases, the effect of the 2011 and 2014 laws are still verified as well as the unsignificant effect of the 2009 law. Finally the last column shows that fixed effects play a real role in controlling for the median income which are no longer significant. Finally the effect of the different laws are similar to those presented previously in the paper.

[8]These indicators have an history that is really linked to their mediatization (Evain, 2020). They were created in 1987 by the French Ministry of Education as an administrative, and not public tool, to manage secondary schools. But after an inadvertent leak in the media, they became public in 1993 and have since been regularly used to rank secondary schools by newspapers.

Table 5: Social Housing Change from Difference in Discontinuities with controls

Bandwidth choice	CE-opt [-461,461]			
	Conversion	Income	All	FE
Treated in 2009	0.180	0.214	0.221	0.146
	(0.173)	(0.164)	(0.191)	(0.169)
Treated in 2011	0.560*	0.650*	0.542*	0.627*
	(0.337)	(0.348)	(0.325)	(0.340)
Treated in 2014	0.620***	0.511**	0.598***	0.462**
	(0.239)	(0.228)	(0.230)	(0.233)
Number of conversion	0.0408***		0.05***	0.01***
	(0.0043)		(0.006)	(0.003)
Median income (/100)		-0.003**	-0.004**	-0.003
		(0.0013)	(0.001)	(0.003)
Constant	4.35***	5.15***	5.19***	7.38***
	(0.648)	(0.930)	(0.893)	(0.709)
Neighborhood FE				✓
Year FE				✓
Observations	5726	5726	5726	2002
R^2 adj.	0.06	0.06	0.10	0.90

Notes: Standard errors are clustered at the neighborhood level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Results are obtained from a spatial difference-in-discontinuity using the PPML estimator. All estimations use a triangular distribution of weight with the Coverage Error (CE) probability neighborhood method. The dependent variable is the number/area of HLM conversions (in m^2). Column (1) introduces the total number of conversions (in m^2), Column (2) includes the median income in each district, Column (3) presents results with these two controls, and Column 4 incorporates individual fixed effects and time effects.

Obviously the previous controls does not encompass the richness of the determinants that drive the demand of conversion. We then introduce data on Airbnb (a good proxy of the change in use that directly cause the change of designation studied here). We also introduce indicator of schools added value that may influence the housing market.

As already mentionned, data on school added value and on Airbnb are not available for the whole period (only on 2012-2017) but enable to study the 2014 law in Table (6). As in the previous estimation, the total number of conversions is significant while the median income is not. Interrestingly, the coefficient of Airbnb is positive (when introduced alone) which is the expected effect of this law. However this positive effect loses its significance once fixed effects are introduced. Similarly all the indicators of school added values lose their significant impact once fixed effects are introduced. This illustrates both the challenge of identifying variables that influence HLM conversion and the effectiveness of fixed effects in controlling for determinants that does not vary a lot over such a short period of time.

Table 6: Social Housing Change from Difference in Discontinuities with added value indicators for schools and Airbnb from 2012 in Paris intra muros

Bandwidth choice	CE-opt [-461,461]					
	Conversion	Income	School AV	Airbnb	All	FE
Treated in 2014	0.537**	0.446**	0.427**	0.452**	0.591***	0.404**
	(0.210)	(0.211)	(0.211)	(0.208)	(0.213)	(0.199)
Nb of conversion	0.033***				0.033***	0.013***
	(0.006)				(0.006)	(0.003)
Income (median)		-0.002			-0.003**	-0.002
		(0.001)			(0.001)	(0.002)
School Added Value						
Performance			-0.444**		-0.471**	-0.064
			(0.223)		(0.212)	(0.075)
Accompanying			-1.809***		-1.622***	-0.115
			(0.514)		(0.489)	(0.136)
Below expectation			-1.155**		-1.248**	-0.201*
			(0.506)		(0.492)	(0.110)
Selective			-1.243**		-1.169**	-0.017
			(0.483)		(0.456)	(0.106)
Airbnb				0.029***	0.034***	0.005
				(0.009)	(0.009)	(0.004)
Constant	4.62***	5.32***	5.33***	4.49***	5.54***	7.28***
	(0.572)	(0.812)	(0.640)	(0.607)	(0.877)	(0.601)
Neighborhood FE						✓
Year FE						✓
Observations	2118	2118	2118	2118	2118	768
R^2 adj.	0.037	0.035	0.074	0.033	0.134	0.936

Notes: Standard errors are clustered at the neighborhood level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Results are obtained from a spatial difference-in-discontinuity using the PPML estimator. All estimations use a triangular distribution of weight with the Coverage Error (CE) probability neighborhood method. The dependent variable is the number/area of HLM conversions (in m²). Column (1) introduces the total number of conversions (in m²), Column (2) includes the median income in each district, Column (3) presents results with indicators of the added value of secondary schools. Performance schools exhibit a positive difference in both access and success rates. Accompanying schools are those where students take longer to obtain their diploma, but where the dropout rate is low. Schools that fall below expectations have poorer results in both success and access rates in comparison to their predicted outcome. Selective schools are characterized by high schools with a high success rate difference and a low access rate difference. Column (4) introduces the Airbnb variables. Column (5) presents results with all these controls. Column (6) incorporates individual fixed effects and time effects.

5.3 At the center

We pursue our analysis in Table (7) by focusing of the 2014 law that enables to introduce additionnal controls. We still find that this regulation does not have a discernible effect on the surface of social housing built.

Table 7: Social Housing Change from Synthetic Difference in Differences with added value indicators for schools and Airbnb from 2012 in Paris intra muros

Bandwidth choice	CE-opt [-461,461]				
	Conversion	Income	School AV	Airbnb	FE
Treated in 2014	-0.233	-0.253	-0.273	-0.286	-0.579
	(0.268)	(0.276)	(0.279)	(0.270)	(0.481)
Nb of conversion	0.0206***				0.0065***
	(0.006)				(0.002)
Income (median)		-0.0028***			-0.0012
		(0.001)			(0.001)
Airbnb				0.0232	-0.0108
				(0.015)	(0.011)
Constant	4.777***	5.521***	4.460***	4.621***	7.018***
	(0.333)	(0.403)	(0.435)	(0.379)	(0.256)
School Added Value			✓		✓
Neighborhood FE					✓
Year FE					✓
Observations	4,740	4,740	4,740	4,740	1,518
R^2 adj.	0.0624	0.0873	0.108	0.0483	0.925

Notes: Standard errors are clustered at the neighborhood level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Results are obtained from a Synthetic Difference in Differences using the PPML estimator. All estimations use a triangular distribution of weight with the Coverage Error (CE) probability neighborhood method. The dependent variable is the number/area of HLM conversions (in m²). Column (1) introduces the total number of conversions (in m²), Column (2) includes the median income in each district, Column (3) presents results with indicators of the added value of secondary schools. Performance schools exhibit a positive difference in both access and success rates. Accompanying schools are those where students take longer to obtain their diploma, but where the dropout rate is low. Schools that fall below expectations have poorer results in both success and access rates in comparison to their predicted outcome. Selective schools are characterized by high schools with a high success rate difference and a low access rate difference. Column (4) introduces the Airbnb variables. Column (5) presents results with all these controls. Column (6) incorporates individual fixed effects and time effects.

6 A synthetic periphery

In the paper we use two differents methods for the center and for the periphery of the compensation area. While for the center, we have no choices, for the periphery we have presented results using a Diff-in-Disc estimation, while a Synthetic Diff-in-diff (SDID) estimation was also possible. Here, we do precisely such an exercise to verify the robustness of the presults obtained at the periphery of the compensation area.

6.1 Method

We reproduce here the same analysis than the one presented in Section 3 concerning the Periphery but we change the control group, which is now a synthetic area of the treated one. This analysis is thus a robustness check, we replace here the Diff-in-Disc estimation presented in the text by a Synthetic Diff-in-diff (SDID) estimation.

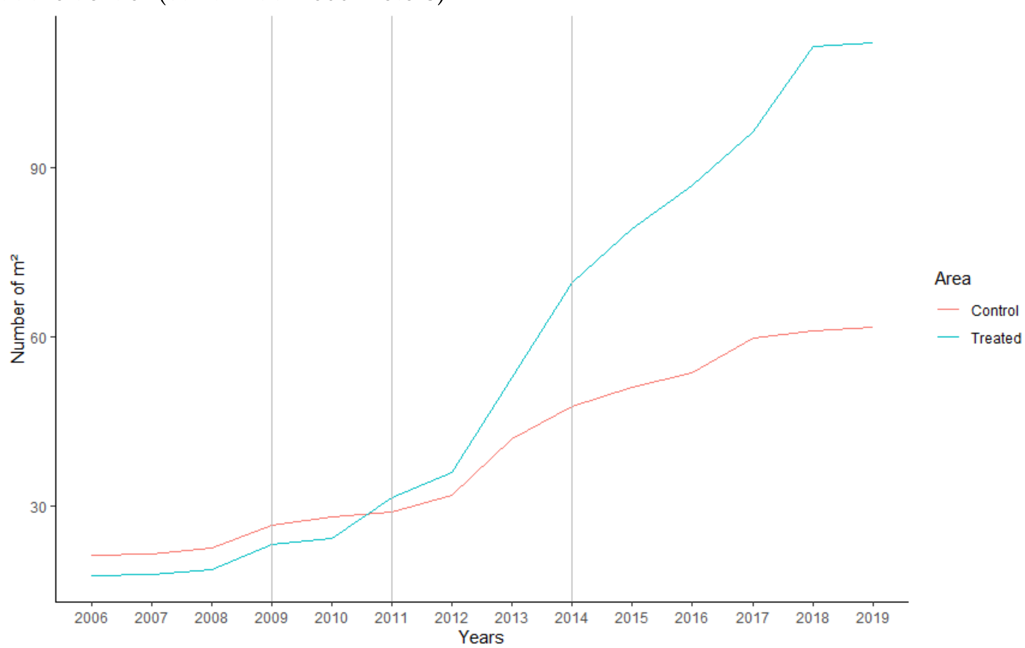
Based on the assumption that in the absence of the treatment, the treated unit and the synthetic control group would have followed parallel trends over time, the SDID helps to mitigate the bias of the Diff-in-Disc method presented in the paper. The similarity of the control group is indeed always debatable in spatial Diff-in-Disc and the SDID provides a good alternative.

We conducted a series of estimations by considering the same set of treated districts than in the section with the Diff-in-Disc for the Periphery, namely with distance from the border of the reinforced area using the different bandwidth already presented. The aim is to provide results that are directly comparable with those presented until now.

The control zone obviously differs from the Diff-in-Disc by constructing a synthetic area using untreated observations located beyond the border.

Figure (1) illustrates change in the conversion of HLM (in m²) for the synthetic area and for the districts where the different laws have been applied. We observe a clear divergence between the two group after 2010, which increases over time.

Figure 1: Treated and Control Areas in the Synthetic difference-in-differences at the border (bandwidth: 600 meters)



Results

Table 8 presents the results of the synthetic difference-in-differences analysis, taking into account different distances from the edge of the compensation zone. Our findings reveal similar outcomes than the one presented in the text with a different method, the 2009 law demonstrating no significant effect, while the 2011 and 2014 reforms successfully promoted the construction of social housing.

Table 8: Social Housing Change at the Periphery

Treated:	Border				
Bandwidth choice	ad-hoc		MSE-opt	CE-opt	
Bandwidth (in meter)	[0-300]	[0-600]	[0-1314]	[0-461]	[0-461]
Treated in 2009	0.153	0.0632	0.123	0.107	0.0443
	(0.195)	(0.156)	(0.176)	(0.147)	(0.148)
Treated in 2011	0.534	0.499*	0.400*	0.412	0.437
	(0.381)	(0.289)	(0.204)	(0.322)	(0.307)
Treated in 2014	0.479**	0.467**	0.426***	0.505**	0.443**
	(0.227)	(0.188)	(0.143)	(0.201)	(0.195)
Number of conversion					0.0079***
					(0.0022)
Median income (/100)					-0.0019
					(0.00358)
Constant	5.320***	5.146***	5.500***	5.199***	5.79***
	(0.228)	(0.199)	(0.199)	(0.213)	(1.084)
Observations	7,378	7,714	8,358	7,574	7,574
R^2 adj.	0.776	0.760	0.820	0.780	0.785

Notes: Standard errors are cluster at the neighborhood level in parentheses a: p<0.01, b: p<0.05, c: p<0.1. Results are obtained from a Synthetic difference in differences using the PPML estimator. Column (1) use 300m bandwidth. Column (2) use 600m bandwidth. Column (3) use the Coverage Error (CE) probability neighborhood method. Columns (4) and (5) use the MSE optimal bandwidth. The dependent variable is the number/area of HLM conversions (in m²). Each estimate includes neighborhood and year fixed effects

The inclusion of additional controls in the last Column of Table (8) and in Table (9) that concerns only the 2014 reform (due to data limitation regarding controls) have hardly any effect on the coefficient estimated. To conclude, we verify here the main result obtained so far, only the 2011 and 2014 laws have significantly fostered the conversion of buildings/offices in social housing at the border of the treated area.

Table 9: Social Housing Change from Synthetic difference-in-differences with added value indicators for schools and Airbnb from 2012 in Paris intra muros

Bandwidth choice	CE-opt [-461,461]				
	Conversion	Income	School AV	Airbnb	FE
Treated in 2014	0.370*	0.377**	0.401**	0.366**	0.335*
	(0.204)	(0.183)	(0.192)	(0.185)	(0.179)
Nb of conversion	0.0429***				0.0120***
	(0.0077)				(0.0045)
Income (median)		-0.0024**			-0.0045***
		(0.0009)			(0.0045)
Airbnb				0.0199*	0.0108
				(0.0119)	(0.00897)
Constant	4.884***	5.697***	5.266***	4.925***	7.096***
	(0.327)	(0.395)	(0.354)	(0.360)	(0.781)
School Added Value			✓		✓
Neighborhood FE					✓
Year FE					✓
Observations	4158	4158	4158	4158	1452
R^2 adj.	0.0847	0.0610	0.0404	0.0416	0.868

Notes: Standard errors are clustered at the neighborhood level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Results are obtained from a Synthetic difference-in-differences using the PPML estimator. All estimations use a triangular distribution of weight with the Coverage Error (CE) probability neighborhood method. The dependent variable is the number/area of HLM conversions (in m²). Column (1) introduces the total number of conversions (in m²), Column (2) includes the median income in each district, Column (3) presents results with indicators of the added value of secondary schools. Column (4) introduces the Airbnb variables. Column (5) presents results with all these controls. Column (6) incorporates individual fixed effects and time effects. Since indicators of added value of secondary schools are not significant when fixed effects are introduced in Column (5), they are not reported here.

7 No Zero but OLS

As argued in the paper, we use the PPML estimators since the analysis of districts with no redevelopment is particularly important for understanding the effects of these laws. However, one may want to compare the results without these zeroes and with the OLS estimator. In Table (10), we confirm the results for the 2009 law, which is significant in the center but not at the border. We also find results consistent with those presented in the text for the 2014 law, which encourages the redevelopment of social housing at the periphery but not in the core.

Table 10: OLS Estimations

Dependent variable	Social Housing Change		
Bandwidth (in meter)	[0-600]		]600-center]
	Diff-in-Disc	Synthetic DiD	
	(1)	(2)	(3)
Treated in 2009	0.253	-0.712	75.26**
	(5.095)	(4.847)	(34.91)
Treated in 2011	24.85	17.98	31.31
	(15.51)	(13.38)	(24.17)
Treated in 2014	49.02*	49.15**	45.90
	(28.11)	(24.14)	(28.70)
Constant	73.39***	42.44***	41.19***
	(5.377)	(7.129)	(13.20)
Observations	6,678	34,776	35,238
R^2 adj.	0.95	0.63	0.64

Notes: Standard errors are cluster at the neighborhood level in parentheses ^{a} p<0.01, ^{b} p<0.05, ^{c} p<0.1. Results are obtained from a Synthetic difference in differences and Difference in Discontinuity using the OLS estimator. Columns (1) use 600m bandwidth and represent social housing change from Difference in Discontinuity. Columns (2) use 600m bandwidth and represent social housing change from Synthetic difference-in-differences. Column (3) use 600m bandwidth to the center and represent social housing change from Synthetic difference-in-differences. Each estimate includes neighborhood and year fixed effects.

8 Non-Parametric Synthetic Control Method

The computation of weights in synthetic control methods are based on different techniques that have been discussed a lot in the literature (Abadie and Gardeazabal, 2003; Abadie et al., 2010, Arkhangelsky et al., 2021). We present here graphics of the change in redevelopment of social housing for the treated and control groups using these alternative methods as well as the method presented in the text. We use in particular the nonparametric synthetic control method developed by Cerulli (2020). This technique uses a kernel function with a pre-defined bandwidth to calculate counterfactual weights. The bandwidth is chosen to minimize the root mean-squared prediction error (RMSPE). Let's take the example of the analysis at the periphery (600m from the compensation border). Figure 2 indicates an optimum bandwidth of 1 which is then the optimal vector distance (Mahalanobis distance) between treated units and controls within the bandwidth. Figure 3 shows the results of the counterfactuals generated with the non-parametric method, with prediction errors, because the treated and the control have a time lag, they should merge perfectly. In contrast, the synthetic control method minimizes the prediction error between the treated and the series generated by a linear regression of the same variables for the control units (see Figure Synthetic control method), the counterfactual appears to follow the treated observations in the pre-processing period. Finally, Figure Synthetic difference-in-differences method shows the synthetic difference-in-differences method chosen in our analysis. We can both observe how the treated and control observations are parallel in the pre-treatment period, and how the control group is not affected by the treatment.

Figure 2: Optimal bandwidth for the non-parametric synthetic control method

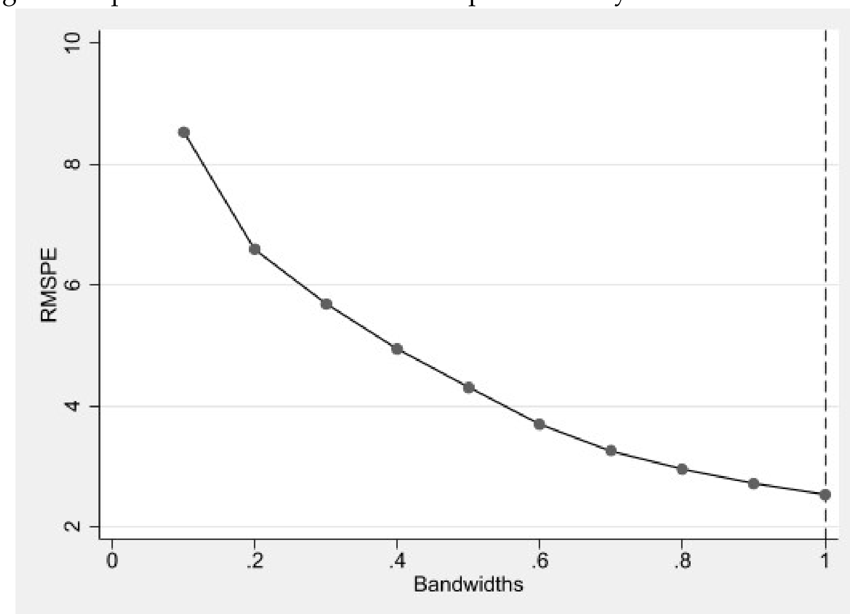


Figure 3: Non-parametric synthetic control method

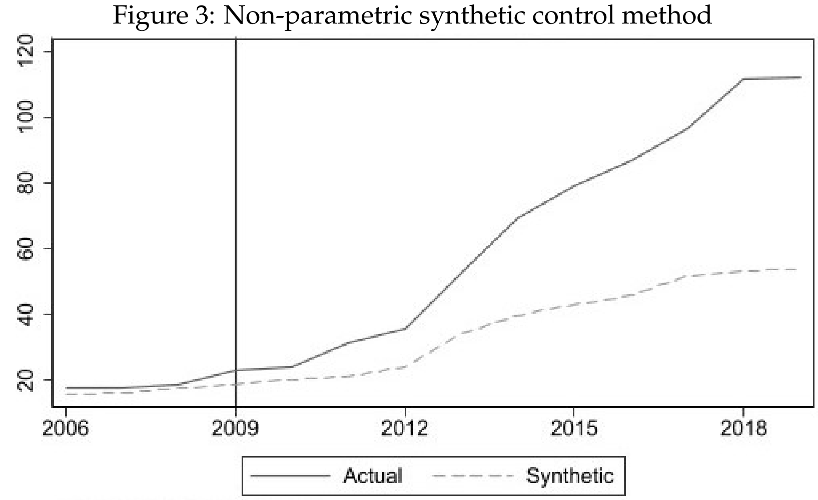


Figure 4: Synthetic control method

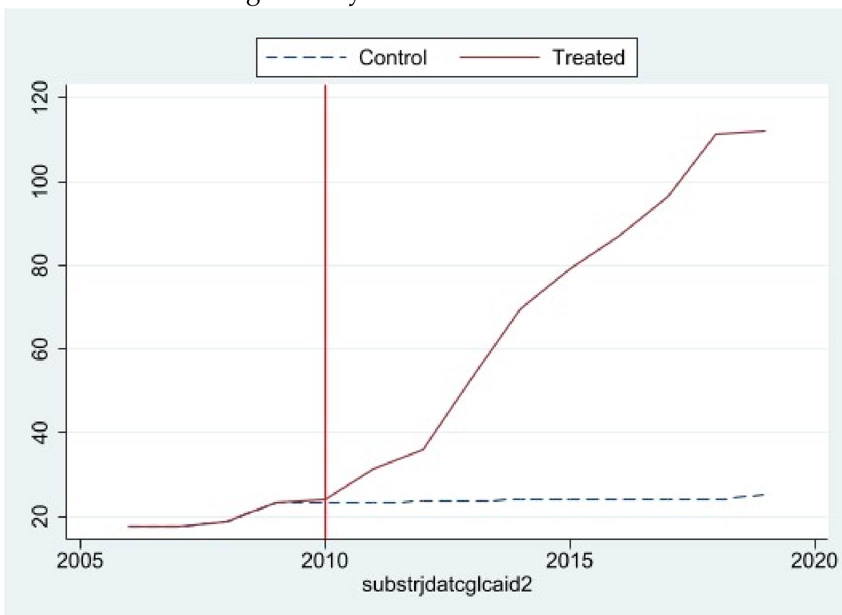
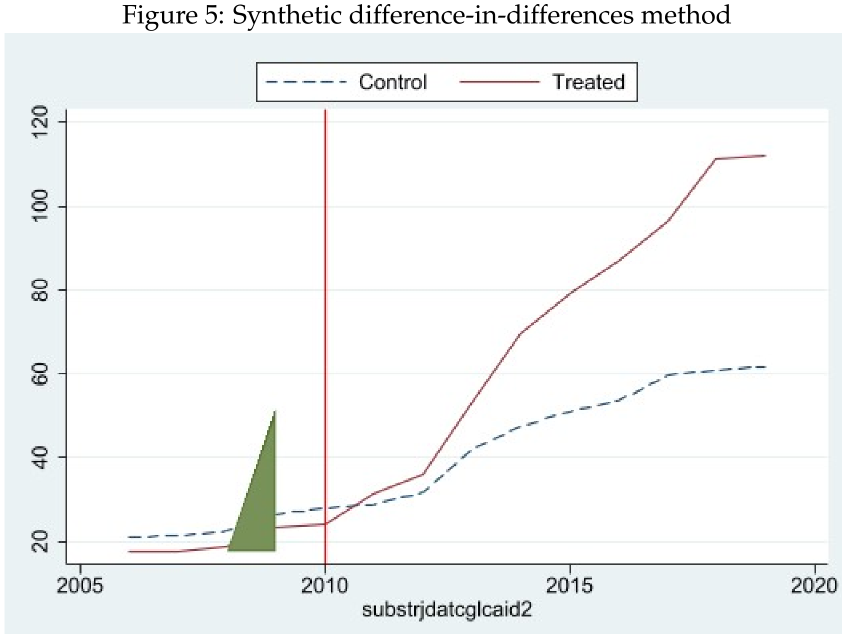


Figure 5: Synthetic difference-in-differences method



9 One Single Treatment by Estimation

In the article, the synthetic center is built only before the first treatment. This means that when evaluating the 2011 and 2014 law, we not longer have a parallel trend between the control and the treated. Thus in Table (11), we build a synthetic center for each policy. Weights are adjusted for the treatment date, and all other post-treatment years are excluded. For example, when the treatment year is 2009, weights are adjusted before 2009 and the treatment period ends in 2011, which is the year of the second treatment. The results are consistent with the results presented in the text when treatment periods are examined individually.

Table 11: Synthetic difference-in-differences with one treatment and placebo test for Differences-in-discontinuities

Dependent variable	Social Housing Change					
Bandwidth (in meter)	[0-461]			]461-center]		
	Synthetic difference-in-differences					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated in 2009	0.043			0.665*		
	(0.14)			(0.439)		
Treated in 2011		0.423*			-0.099	
		(0.302)			(0.372)	
Treated in 2014			0.481*			-0.395
			(0.304)			(0.628)
Nb of conversion	0.01***	0.03***	0.005***	0.02***	0.019***	0.008***
	(0.003)	(0.004)	(0.002)	(0.006)	(0.002)	(0.001)
Income (median)	0.006	-0.001	-0.002	-0.014	-0.0013*	-0.000
	(0.000)	(0.005)	(0.003)	(0.009)	(0.000)	(0.000)
Constant	3.7***	5.3***	6.2***	9.6***	6.32***	6.3***
	(1.01)	(1.41)	(0.97)	(2.55)	(0.210)	(0.254)
Observations	1,338	3,879	7,574	1,380	3,951	7,714
R^2 adj.	0.88	0.779	0.792	0.808	0.865	0.847

Notes: Standard errors are cluster at the neighborhood level in parentheses ^{a} p<0.01, ^{b} p<0.05, ^{c} p<0.1. Results are obtained from a Synthetic difference in differences using the PPML estimator. Columns (1) to (3) use 461m bandwidth at the periphery with social housing change dependant variable. Columns (4) to (6) use 461m bandwidth to the center with social housing change dependant variable. Columns (7) to (9) use 461m bandwidth at the periphery with private housing change dependant variable. Columns (10) to (12) use 461m bandwidth to the center with private housing change dependant variable. Column (13) shows the placebo effect of the Differences-in-Discontinuities estimate with pre-treatment data. Each estimate includes neighborhood and year fixed effects.

10 Conclusion

According to a survey based on of 480 economists done by Chopra et al. (2022), "studies with null results are perceived to be [...] less important than studies with significant results". We believe, on the contrary, that null results are enormously important, especially when analyzing public policies. In our findings, the redevelopment of social housing in central Paris was not affected by the most recent laws of 2011 and 2014. These policies have also been ineffective in fostering greater social diversity in Paris. This is a significant finding since the supply of social housing is primarily driven by these redevelopments. Hence, these null results indicate that local governments need to revise these laws and devise more ambitious policies if they wish to successfully (re)develop social housing in Paris.

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