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Externalities from urban renewal: evidence from a French program

Sylvain Chareyron, Florence Goffette-Nagot and Lucie Letrouit*

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Abstract

We contribute to the evaluation of urban renewal policies based on a large-scale program launched in France in 2004. Using an estimator aimed at avoiding bias in the estimation of treatment effects that are heterogeneous across treatment groups or time periods, and complementing its results with a more precise double fixed effects difference-in-differences estimator, we find no significant aggregate impact of the program on housing prices. We identify four dampening factors that can explain this lack of aggregate impact: a sometimes insufficient level of funding per neighborhood, a stigma effect in the most deprived neighborhoods, the isolation of some neighborhoods located far from city centers, and the concentration of the program's funding on types of operations associated with small impacts on housing prices.

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JEL: D62, H23, R21, R31

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Urban renewal programs have been implemented in many countries in the last decades to fight housing decay, poverty concentration and associated social ills. However, compared to other place-based policies, such as Enterprise Zones programs (see Neumark and Simpson, 2015 for a recent survey) or place-based subsidies to housing investments like the LIHTC (Baum-Snow and Marion, 2009; Eriksen and Rosenthal, 2010), only few empirical studies on urban renewal programs' impacts have been conducted and their results are very mixed.[1] Some of them find positive effects of renovation programs on land or housing prices (Galster, Tatian and Accordino, 2006; Rossi-Hansberg, Sarte and Owens III, 2010; Collins and Shester, 2013; Koster and Van Ommeren, 2019), but others find limited (Ding, Simons and Baku, 2000; Aarland, Osland and Gjestland, 2017; Albanese, Ciani and de Blasio, 2021) or insignificant (Barthélémy, Michelangeli and Trannoy, 2007; Ahlfeldt, Maennig and Richter, 2017) effects. The pattern is similar for impacts on household income, employment and poverty rate (Van Beckhoven and Van Kempen, 2003; Collins and Shester, 2013; Guyon, 2016; González-Pampillón, Jofre-Monseny and Viladecans-Marsal, 2019). Nevertheless, urban renewal programs appear to have positive effects on crime (Aliprantis and Hartley, 2015; Alonso, Andrews and Jorda, 2019).

With respect to the scale of study, previous analyses of urban renovation have often focused on city-level programs or state-level programs of moderate magnitudes (Rossi-Hansberg, Sarte and Owens III, 2010; Aarland, Osland and Gjestland, 2017; Ahlfeldt, Maennig and Richter, 2017). Few studies of nationwide renewal programs have been conducted: a study by Collins and Shester (2013) on the city-level impacts on median property values of a large-scale U.S. federal program, another by Aliprantis and Hartley (2015) estimating the impacts of the U.S. program HOPE VI on crime in the city of Chicago, and a third study by Koster and Van Ommeren (2019) on the impacts on housing markets of a Dutch nationwide program in which 83 neighborhoods received about €1 billion between 2007 and 2012. Gibbons, Overman and Sarvimäki (2021) assess the employment effect of an £8.2bn investment in a UK regeneration program but referring more to Enterprise Zones programs because the part of the program they focus on is primarily about regenerating local areas by subsidizing the provision or repair of business floor space. The LAPZ program, evaluated by Kitchens and Wallace (2021), is another example of place-based policy that, while also related to Enterprise Zones programs, shares elements with renewal programs in that a portion of the funding

[1] These studies generally focus on renovation's effects on population social composition, housing prices (as a signal of location attractivity), or crime rate. See Albanese, Ciani and de Blasio (2021) for a comprehensive review of the expected consequences of urban revitalization programs.

is dedicated to neighborhood revitalization.

The aim of the present paper is to contribute to the literature on urban renewal policies by analyzing the impact of a very large-scale, highly-funded French urban renewal program, the *Programme National de Rénovation Urbaine* (PNRU hereafter), launched in 2004. This program essentially consisted in the demolition, construction and renovation of public housing, but also in the creation and improvement of public facilities, with the aim of improving residents' living conditions and attracting new population, so as to increase social diversity in the targeted neighborhoods. It received a financing of more than €47 billions and affected more than half of the housing stock in six hundred neighborhoods in 162 different cities throughout France.[2]

We explore the effect of the PNRU program on housing externalities, as measured by housing prices. Theoretically, externalities produced by urban renewal can be expected to occur at three different levels. First, the renovation of housing benefits its own residents, by improving comfort and reducing inadequate housing conditions. These gains are likely to lead to an increase in the price of the renovated units, unless it is regulated as in the case of French public housing. Second, renovation should also induce housing externalities in the vicinity of the renovated housing units, thereby leading to higher housing prices (Rossi-Hansberg, Sarte and Owens III, 2010). Finally, urban renewal can be expected to lead to city-level externalities, due to a mitigation of the negative spillovers associated with the neighborhood (e.g. related to crime), a dampening of middle-class flight and an improvement in local governments' fiscal problems (Collins and Shester, 2013).

In this paper, we attempt to capture the second category of externalities at the neighborhood level. Indeed, as the PNRU program resulted in very visible improvements of the public housing stock and of public spaces (renewed architectural quality, lower buildings, improvements of building fronts, improved public facilities), it can be expected to have produced externalities on the private housing stock in renovated neighborhoods, which should be reflected in private housing prices. We use data on geolocalized private housing transactions to precisely measure this potential externality. It is important to note that, as PNRU's urban renewal interventions were mostly focused on public housing and public amenities rather than on private housing, our variable of interest, private housing prices, measures the program's externalities and not its direct effects on the price of renovated housing units.

[2] The annual investment for the PNRU was thus on average €3 billions per year between 2004 and 2018. By way of comparison, annual expenditures on the French Enterprise Zone policy evaluated in Briant, Lafourcade and Schmutz (2015) peaked around 2008 with about €0.5 billion that year.

The PNRU possesses several interesting features that allow for a careful and comprehensive investigation of the effects of urban renewal on housing prices. First, the selection of targeted neighborhoods and the timing of the program allow us to use a difference-in-differences strategy with staggered adoption. Indeed, treated areas were selected in 2003 among a large number of distressed neighborhoods delineated in 1996 as part of a larger urban policy framework. Non-selected neighborhoods are good candidates to be used as control observations. Moreover, as evidenced by the history of the program exposed later in the paper, renovation operations began at different dates (between 2004 and 2014) in the different neighborhoods. This feature makes it possible to include not-yet-treated neighborhoods in the control group, which improves the comparability of the control and treated group. It also allows for a better identification of time-varying treatment effects. Finally, the whole set of renovated neighborhoods was chosen in 2003 and we provide evidence in section 3.2 supporting the idea that the starting year of renovation was not decided depending on the evolution of housing prices in these neighborhoods, so that the timing of the program specific to each neighborhood can be considered as exogenous with respect to the housing price evolution.

Second, the PNRU is of exceptional magnitude: each renovated neighborhood received on average €93,000 per household over 15 years and more than half of the housing stock over all these neighborhoods was rebuilt or rehabilitated through the program. It has been argued in the past literature that renovation has an impact only if it is of high enough intensity (Ding, Simons and Baku, 2000; Albanese, Ciani and de Blasio, 2021). As for the PNRU, the level of the amounts invested and the share of the housing stock affected allow to consider that the program is of sufficient magnitude to have significantly affected the built environment in targeted neighborhoods. Additionally, the program involved 572 neighborhoods located in 219 different cities in mainland France, and the selection of treated neighborhoods leaves a set of about 300 untreated but also distressed neighborhoods. This large number of treated and non-treated neighborhoods constitutes an asset for the present study because, as shown by Ahlfeldt, Maennig and Richter (2017), a large number of control and treated areas provides a more accurate estimation of the impact of such a policy. Finally, the large number of treated observations as well as the large variance in the amount of funding by neighborhood makes it possible to explore the potential for heterogeneous impacts of the program on subsamples. We believe this setting ensures a good external validity of our results.

We also pay special attention to the possible existence of heterogeneous effects between neighborhoods and between time periods by choosing an appropriate econometric method. Indeed, because renovation took place in very different contexts and started at different dates depending on the neighborhood, large heterogeneity in treatment effects across space and over time can be suspected.[3] This heterogeneity is combined with imbalance in treatment status along these same two dimensions. To get unbiased estimates of the program's impacts in such a context, we apply the novel methodology proposed by De Chaisemartin and D'Haultfœuille (2020a,b) for this very purpose.

Our results indicate that renovation did not lead to a significant increase in housing prices at the aggregate level during the time period under study. We estimate with a 95% confidence level that the aggregate impact of the program was smaller than 2.3%. This lack of aggregate effect is robust to various robustness checks. Yet, the large scale of the PNRU program and its implementation in different contexts across France allow us to investigate a number of possible explanations for the non-significant aggregate impact of the program.

First, we find some evidence that the size of the program's impact on housing prices depended on the level of funding invested in relation to the initial public housing stock, suggesting that the level of investment in some renovated neighborhoods may have been too low for the urban renewal to affect the housing market. Second, analyzing heterogeneities in the program's impact depending on the revenue level and on the share of public housing in renovated neighborhoods, we find some (weak) evidence in favor of a stigma effect, which may have prevented higher neighborhood amenities to translate into higher housing demand and thus into higher housing prices. Third, considering renewal impacts depending on distance to the city center, a location effect appears, whereby urban renewal increased neighborhood attractivity only for those close enough to city centers. Fourth, a further heterogeneity analysis suggests that some renovation operations, those targeting public equipment, have a larger impact on housing prices as compared to construction/demolition operations and renovation operations, and that the renovation program under study focused on the types of operations that have the lowest impact on housing prices (75% of the program's funding was allocated to construction/demolition or renovation operations). Finally, we rule out the hypothesis that the absence of aggregate effect of the program was due to an increase in housing supply in

[3] As an illustration, four renovated neighborhoods are located in Paris municipality, where tension on the housing market is extreme, and six additional neighborhoods are located less than 8 kilometers from Paris center, whereas renovation also took place in some economically distressed urban units where public housing offices asked the National Agency for Urban Renewal (ANRU) not to replace one-for-one the demolished housing units because of a very low housing demand (Rey-Lefebvre, Isabelle. 2019. *"Des villes détruisent des HLM devenus trop nombreux, trop vieux, inlouables."* Le Monde, May 31. `https://www.lemonde.fr/societe/article/2019/05/31/ces-villes-encombrees-par-un-parc-hlm-devenu-inutile_5469685_3224.html`).

renovated neighborhoods.

The remainder of the paper is organized as follows. Section 1 depicts the urban renewal program under study. Section 2 is dedicated to the presentation of the empirical strategy and Section 3 to the description of the data. We present and discuss our results in Section 4. Finally, we compare them to previous results in the literature and conclude in Section 5.

1 The French National Urban Renewal Program

The French National Urban Renewal Program, launched in 2004 as the result of a law voted in July 2003, had four distinct goals: restructuring neighborhoods, increasing social diversity, supporting sustainable development and reducing inequalities between places and between populations. To this aim, a list of eligible urban neighborhoods located on the whole French territory was established. It included all of the 751 neighborhoods referred to as Sensitive Urban Zones (hereafter ZUS) that were designated by the French government in 1996 as targets for its urban policies, based on the existence of large public housing developments and low employment-to-population ratios. This list also defined 166 additional neighborhoods called "Article 6", selected according to similar criteria. Municipalities to which these neighborhoods belong were asked to elaborate renovation projects in coordination with local actors, in particular public housing offices. These projects were then submitted to the National Agency for Urban Renewal (ANRU hereafter), which was created to ensure the administrative follow-up of urban renewal operations. The Agency, in some cases, asked for modifications of the renovation projects and then decided whether to grant funding.

From the list of eligible neighborhoods, 594 were selected for renovation. Although, some determinants of selection can be observed empirically (e.g., renovated neighborhoods had a higher share of public housing than non-renovated ones), there were no clear and objective criteria for selection. As a result, there was no selection score to rank the neighbourhoods, as was the case for renovation programs in some other countries. This prevents us to use a regression-discontinuity design as in Alonso, Andrews and Jorda (2019) or Koster and Van Ommeren (2019).

The funds were then committed in different phases according to the renovation schedule for each designated neighborhood. On the overall, €47 billions were invested from 2004 to 2020 in as many as 594 neighborhoods.[4] By way of comparison, the Dutch program considered in Koster

[4]The major contributors to the program were public housing offices, which provided about €20.5 billions. The ANRU comes next with around €11.5 billions. The remaining funding was given mainly by local authorities.

and Van Ommeren (2019) involved an investment of about €1 billion in 83 neighborhoods, whereas the Berliner program analyzed by Ahlfeldt, Maennig and Richter (2017) received €1.94 billions for 22 neighborhoods.

For the sake of brevity, we will use the term "urban policy neighborhoods" or even "neighborhoods" to designate the set of eligible neighborhoods, both ZUS and Article 6 neighborhoods. As for treated neighborhoods, we simply speak of "renovated neighborhoods" or QRU (urban renewal area), be they observed before of after the start of renovation. Urban policy neighborhoods have a limited area, with a median at 0.4 km^2. Renovated neighborhoods are somewhat larger, with a median at 0.5 km^2 (Guyon, 2016). They are composed of an average of 2,592 individuals.[5] Urban policy neighborhoods are located in 219 urban units, of which 162 have at least one renovated neighborhood.[6]

As of December 31, 2018, the program affected directly close to 1 million dwellings, with 150,000 housing units demolished, 125,000 built, 335,000 rehabilitated and 345,000 concerned by their building's beautification (e.g. the setting up of a front garden). A lot of large public housing developments erected in the post World War II era had strongly depreciated and were perceived as stigmatizing for these neighborhoods. Many of them were therefore torn down. Thus, more than half of the initial housing stock of renovated neighborhoods was rebuilt or rehabilitated thanks to the PNRU. In addition, a quarter of the PNRU budget was dedicated to the improvement of the urban environment, public facilities and economic and commercial spaces. Overall, an average of €79 millions was invested in each renovated neighborhood, that is a mean of €93,000 per household.

This program contrasts with usual urban renewal programs studied in the literature, due not only to its large scale but also to the key role attributed to the massive demolition and reconstruction of public housing.[7]

[5] As an illustration of the different geographical scales at stake in this paper, Figure A1 represents the urban unit of Grenoble in the Auvergne-Rhône-Alpes region in France. It shows the borders of Grenoble municipality and of the three types of neighborhoods we are interested in: ZUS neighborhoods that were renovated ("ZUS-renov"), Article 6 neighborhoods that were all renovated ("Art6-renov") and ZUS neighborhoods that were not renovated ("ZUS-norenov").

[6] In the French statistical system, an urban unit is a municipality or a group of municipalities which includes a continuously built up zone (no cut of more than 200 meters between two buildings) with at least 2,000 inhabitants. We will also refer to urban units as cities.

[7] An emblematic example of targeted neighborhoods is the Minguettes in the South outskirt of Lyon. This neighborhood was raised in the middle of fields in the early 1960's, in order to accommodate the labor force working in the automobile industry in the surroundings. Between 1965 and 1973, 9,200 housing units were built, among which 40%

As explained in the introduction, it is important to note that the renovation program *per se* did not increase housing supply in the neighborhoods. Indeed, not all public housing units demolished were rebuilt. The rule has been to rebuild "one-for-one" demolished public housing units, but not necessarily within the neighborhood where demolition took place, in line with the goal of the policy to demolish the huge deteriorated public housing projects and rebuild less densely. Numerous inhabitants were proposed another public housing dwelling outside of the neighborhood within the same city. Moreover, some private housing was built, so that some variation in the housing supply evolution across neighborhoods is observed (see Table 2 thereafter).

A first evaluation of the PNRU's impact on the share of public housing, housing size, vacancy rate and share of low-income households was conducted by Guyon (2016). The results show that although the program significantly affected the housing stock, there was no substantial decrease between 2004 and 2013 in the share of low-income households in the renovated neighborhoods. Our aim is to further evaluate the program by looking at the PNRU impact on housing prices, in order to evaluate its externalities.

2 Empirical strategy

In order to estimate the effect of the PNRU program on housing prices, we rely on a difference-in-differences strategy exploiting both the existence of never treated neighborhoods that were initially eligible, and the staggered start of renovation across renovated neighborhoods. Because the selection process in the treatment was not clearly formalized by the authorities, we opt for an approach of eliminating all potential unobserved differences between the treatment and control groups. Moreover, although the program was officially launched with the creation of the ANRU in 2004, not all neighborhoods were designated for renovation at that time. We therefore define the start of renovation in a neighborhood as the date of the first financing commitment, which was the first step of renewal operations and therefore should closely follow a neighborhood's designation. We believe that this choice limits the possibility of anticipation effects on the housing market and seems also more relevant than taking a single starting date for all neighborhoods.

small detached houses and 60% public housing units in high-rise buildings offering comfortable low-rent housing. In the 1970's, workers, many of whom were immigrants, were heavily hit by the economic crisis. Middle-class households left, and cumulative negative dynamics started. The Minguettes is known for having been the first place where urban riots took place in France. The PNRU started there in 2004. 700 housing units were destroyed and replaced by new units, 2,400 housing units were rehabilitated and commercial spaces rebuilt for a total investment of €170 millions.

The traditional difference-in-differences estimation consists in using a double fixed effects estimator (DFE hereafter), where group fixed effects control for time-constant group heterogeneity, and time fixed effects control for general time trends. Under the assumption of common trends and with constant treatment effects, this strategy allows for the unbiased estimation of the average treatment effect. However, recent papers considering staggered adoption designs, in which treatment of different groups starts at different times, have shown that the treatment effect estimated with a DFE regression is actually a weighted average of individual treatment effects, with weights that might be negative (Borusyak and Jaravel, 2017; Goodman-Bacon, 2018; De Chaisemartin and D'Haultfœuille, 2020a,b; Athey and Imbens, 2021). If treatment effects are heterogeneous, then the DFE-based average treatment effect is likely to be biased. Moreover, De Chaisemartin and D'Haultfœuille (2020a) (hereafter DCDH) demonstrate that the DFE estimate is especially likely to involve negative weights and therefore give biased estimates if treatment effects differ between periods with many versus few treated groups, or between groups treated for many versus few periods. As we will show in the following, imbalance in treatment status across groups and time periods is present in the case of the PNRU and heterogeneity of treatment effects on housing prices can be suspected.

To overcome a potential bias, De Chaisemartin and D'Haultfœuille (2020b) propose an alternative estimator (DID_l in the following), which is robust to treatment effect heterogeneity across groups and time periods. This estimator consists in computing, at each point in time, a difference-in-differences based on switchers (i.e. groups for which treatment status changes at that time) as compared to groups with stable treatment.[8] These differences-in-differences are then averaged over the whole observation period, with weights that depend on the number of switchers at each time.

In this section, we first describe how we implement the traditional DFE estimator, and then turn to the DID_l estimator. Since each of them has advantages and drawbacks, the results of both estimators will be presented afterwards.

[8] Other estimators have been proposed to overcome this potential bias (Sun and Abraham, 2021; Callaway and Sant'Anna, 2021; Borusyak and Jaravel, 2017). These estimators generally involve modifying the set of units that can act as effective comparison units in the estimation process, but they differ in terms of how the effective comparison units are selected (Baker, Larcker and Wang, 2021). De Chaisemartin and D'Haultfœuille (2020b)'s estimator is very comprehensive in terms of the policy design that can be evaluated, with binary and continuous treatment, and monotonic and non-monotonic treatment. For a recent and comprehensive survey of these estimators, see De Chaisemartin and D'Haultfœuille (2022)

2.1 Double fixed effects estimator

To estimate the average treatment effect on housing prices based on a DFE estimator, we use a hedonic regression in which the logarithm of the price of a housing transaction is regressed on the characteristics of the transacted housing unit, year dummies and time trends at the city level. While some studies such as Koster and Van Ommeren (2019) or Rossi-Hansberg, Sarte and Owens III (2010) have considered price per square meter as the dependent variable, we follow Ahlfeldt, Maennig and Richter (2017) by considering the price of housing transactions as the dependent variable and controlling for housing size. This allows to account for the common observation that prices per square meter are lower for large dwellings.

The analysis is conducted on a sample including all transactions that occurred in an urban policy neighborhood. A dummy indicates whether the housing unit is located in a neighborhood under renovation. Its coefficient is the estimate of the average time-constant effect of the PNRU program on prices in renovated neighborhoods. In practice, the following equation is estimated:

$$\log(P_{itnu}) = \beta_T \mathbf{1}_{n\in\mathcal{R},t-T(n)\geq 0} + X_{it}\beta + \phi_t + \mu_n + \gamma_u t + \gamma_u' t^2 + \eta \mathbf{1}_{n\in\mathcal{R}} \times t + \epsilon_{itnu} \tag{1}$$

where i is the identifier of the housing unit, t is the year of the transaction, n is the neighborhood in which the housing unit is located, u is its urban unit, $\mathcal{R}$ is the set of renovated neighborhoods, $T(n)$ is the date at which renovation was launched -i.e. the date of the first financial commitment- in neighborhood n (if $n \in \mathcal{R}$), $\mathbf{1}_{n\in\mathcal{R},t-T(n)\geq 0}$ is a dummy function equal to one if the transaction takes place in a renovated neighborhood after the beginning of the renovation and 0 otherwise. X_{it} is a vector of characteristics of the transacted housing unit: the number of rooms interacted with an apartment or house indicator, floor space per room, number of bathrooms, period of construction, dummy equal to one if the dwelling is less than 5 years old, and distance of the transaction to the city center.

ϕ_t is a time fixed effect, μ_n is a neighborhood fixed effect, γ_u (resp. γ_u') is the linear (resp. square) time trend of housing prices in urban unit u. The urban unit specific time trends allow to control for the evolution of housing prices at the city level, which can indeed be very different among the 145 urban units represented in the estimation sample.[9] The quadratic functional form is especially useful to account for the reversal of housing prices after the 2008's crisis, while keeping the number of estimated coefficients reasonable. We separate Paris municipality from the rest of the Parisian urban unit in order to account for a specific time trend.

[9] Among which 27 have no renovated neighborhood.

We also include a linear trend ($\eta \mathbf{1}_{n\in\mathcal{R}} \times t$) specific to renovated neighborhoods, to account for potential differences in trends between renovated and non-renovated neighborhoods. This trend is defined in terms of calendar years. Indeed, the evolution of housing prices over the observation period in renovated neighborhoods, that is, among the most deprived ones within each city, possibly differs from that of other neighborhoods. This approach is similar to that of Ahlfeldt, Roth and Seidel (2018) and Dustmann et al. (2021) and adapted to a staggered adoption design.

In this specification, neighborhood fixed effects account for time-constant heterogeneity while year dummies, urban units time trends, the renovated-neighborhood specific trend and transacted housing units characteristics control for some time-varying heterogeneity.[10]

Following Ahlfeldt, Maennig and Richter (2017), we also analyze the program's time-varying effects by introducing a time flexible structure into the previous equation:

$$\log(P_{itnu}) = \sum_{k\in S} \beta_k \mathbf{1}_{n\in\mathcal{R},t-T(n)\in\{k-1,k\}} + X_{it}\beta + \phi_t + \mu_n + \gamma_u t + \gamma'_u t^2 + \eta \mathbf{1}_{n\in\mathcal{R}} \times t + \epsilon_{itnu} \qquad (2)$$

In this time-varying specification, $k \in S$, where $S = \{-11, -9, -7, -5, -3, -1, 1, 3, 5, 7, 9, 11\}$. Time-varying effects are grouped by pairs, because our dataset on housing prices is available only every even year. $\mathbf{1}_{n\in\mathcal{R},t-T(n)\in\{k-1,k\}}$ is then a dummy function equal to one if and only if renovation in neighborhood n started $k-1$ or k years before the transaction.[11] The reference years for the estimation of the program impacts are years $T(n) - 2$ for neighborhoods where renovation started on an even year and $T(n) - 1$ for neighborhoods where renovation started on an uneven year. β_k is then the effect of the program after $k-1$ or k years.

To avoid contamination from a renovated neighborhood to a nearby control neighborhood (as some non-renovated neighborhoods were adjacent to renovated ones), we exclude from the sample transactions located in control neighborhoods less than 500 meters away from the border of one of the treated neighborhoods .[12] The choice of 500 meters is motivated by Ding, Simons and Baku (2000) who observe that *"residential investment usually does not have any impact on property values beyond 300 feet (90 meters) away for new construction and 150 feet (45 meters) away for rehabilitation"* and by Rossi-Hansberg, Sarte and Owens III (2010) who find that *"housing externalities decrease by half approximately every 1,000 feet"*. Thus, the 500-meters buffer seems reasonable to avoid the risk of contamination of the control group. We also tested the sensitivity of our main estimate to

[10]Note that the urban unit trends are likely to absorb any city-level general equilibrium effects induced by the PNRU program.

[11]If k is negative, the dummy is equal to one when renovation started $|k|$ or $|k| + 1$ years after the transaction.

[12]In doing so, 1,526 transactions are removed from the control group.

the value of the chosen threshold.[13] With a 1000m and then a 2000m threshold, the estimated coefficient for the global impact of the treatment decreases as compared to the baseline, showing at the very least that the control neighborhoods close to renovated ones did not experience any increase in their housing prices. It follows that the set of control observations consists of transactions made in non-renovated urban policy neighborhoods outside of the 500 meter buffer around the renovated neighborhoods. The inclusion of non-renovated urban policy neighborhoods as controls is the most natural, as renovated neighborhoods have been chosen among urban policy neighborhoods and are relatively similar (see Table 3). In the robustness checks, we also employ weighting procedures to improve the comparability of the treated and control groups, which is useful to address the concern that they may differ in terms of unobserved trends. Moreover, given the staggered adoption design and our empirical specification, the identification of treatment effects also exploits transactions made in renovated neighborhoods before the start of renovation.

The difference-in-differences method is based on the common trend hypothesis, which states in a staggered difference-in-differences design that the evolution of housing prices relative to the starting year of renovation would have been the same in the non-renovated and renovated neighborhoods in the absence of renovation. In practice, this hypothesis is tested by checking the absence of pre-trends, that is, by verifying that the estimated β_k is not significantly different from 0 when $k < 0$.

In our investigations of the reasons that might explain the null estimated aggregate effect of the program, we also analyze a slightly different DFE specification in which the treatment dummy is interacted with a measure of treatment intensity, namely the level of funding received by the renovated neighborhood in relation to the initial housing stock or the initial public housing stock. The corresponding estimates of the program's impact are respectively denoted by $\beta_{T,fh}$ and $\beta_{T,fph}$. We also further introduce the square of these continuous variables in some of the specifications. We also estimate the impact of the program on the volume of transactions. To this aim, observations are aggregated at the level of neighborhoods and equations similar to Equations 1 and 2 are estimated, replacing the log price by the number of transactions in relation to the initial number of housing units in the neighborhood, suppressing housing unit characteristics and considering again a continuous treatment with estimated coefficient $\beta_{T,fph}$.

[13] Results available on request.

2.2 DID_l estimator

As previously explained, the DFE estimator is likely to yield biased estimates in staggered adoption designs if treatment effects are heterogeneous across time periods or across groups. This bias is especially of concern if treatment effects differ between periods with many versus few treated groups, or between groups treated for many versus few periods.

In the case of the PNRU program, treatment effects on housing prices are likely to be highly heterogeneous across periods and neighborhoods for at least four reasons. First, with respect to heterogeneity across time periods, treatment effects might differ depending on period, especially because the 2008 housing crisis may have altered the impacts of the program in the following years. Then, with respect to heterogeneity across neighborhoods, the impact of renovation on housing prices is likely to depend on the tension on the urban unit's housing market. Given that our sample includes renovated neighborhoods located in 118 different urban units, this is likely to be a major source of heterogeneity. Furthermore, treatment effects may also depend on characteristics of the neighborhood itself, such as the share of public housing or the local level of criminality, or on characteristics of the renovation operations, such as the level of funding invested in the neighborhood or the extent of demolitions. Finally, one can think that the most promising renovation projects were launched first, so that neighborhoods belonging to the first generations of renewal might have stronger treatment effects. This potential heterogeneity in treatment effects is combined with imbalance in treatment status in our data, as will be shown in Table 1.

Given these features, we use the alternative DID_l estimator proposed by De Chaisemartin and D'Haultfœuille (2020b).

The DID_l estimator consists, in our case, in computing the average over the study period of difference-in-differences estimated for each year on neighborhoods where renovation started during this year (these neighborhoods are called "switchers") compared to neighborhoods not entering the treatment at the same date (i.e. non-renovated ZUS and renovated neighborhoods in which renovation has not started yet).[14] It is robust to treatment effects heterogeneity and can be used when the treatment is not binary and when it can increase or decrease over time. The DID_l estimator allows to compute dynamic treatment effects l periods after the treatment starts and can be averaged in order to compute the overall effect of the treatment compared to the status quo

[14] This estimator is equivalent to the DID_M estimator presented in De Chaisemartin and D'Haultfœuille (2020a) when the treatment is binary and staggered, meaning that individuals can switch in but not out of the treatment, which is the case in our setting. We however refer here to the more general DID_l estimator.

situation with no change in treatment status. In the estimation of dynamic treatment effects, the average evolution of housing prices between $T(n)-1$ and $T(n)+l$ in neighborhoods where renovation starts at time $T(n)$ is compared to that of neighborhoods that remain untreated at the same dates. As with the DFE estimator, we estimate the impact of the treatment {0,1}, {2,3}, {4,5}, {6,7}, {8,9} and {10,11} years after renovation started in the neighborhood. We use the same control variables, but we can not include a renovated-neighborhood specific trend.[15] The standard errors are computed using a block bootstrap at the neighborhood level.

The DID_l estimator relies on two assumptions. The first one requires that, in each period with switchers, at least one neighborhood keeps its treatment status. This condition is met for sure because the sample includes never treated neighborhoods. The second hypothesis generalizes the common trend hypothesis of more traditional difference-in-differences estimations. It requires that the mean evolution of switching groups' outcomes would have been the same, in the absence of treatment, as that of control groups. In our case, this implies that, had renovation not been undertaken, housing prices in neighborhoods where renovation starts would have evolved in the same way as in non-renovated or in not-yet-renovated neighborhoods. The test of this hypothesis is based on the estimation of placebo effects for housing prices' evolution between periods before the start of the treatment. More specifically, the placebo effect $-i$ for groups entering the treatment at time $T(n)$ is based on the evolution of housing prices between $T(n)-1$ and $T(n)-i-1$.

The DID_l estimator is demanding in terms of data, as the estimate of the $+l$ temporal effect is based on neighborhoods in which transactions occurred at time $T(n)-1$, $T(n)$ and $T(n)+l$.[16] By way of comparison, the classical DFE estimator only requires each neighborhood to have transactions at two distinct dates at least, whatever the date. Moreover, the DID_l estimator is valid for controls at the neighborhood $\times$ period level and is therefore estimated on data aggregated at this level.[17]

[15] The DID_l estimator is implemented using the `did_multiplegt` Stata package. Because the package only allows linear or non parametric trends, and the latter would consume too many degrees of freedom given the number of urban units in the sample, we include linear urban unit trends and not quadratic ones as with the DFE estimator, using the `trends_lin` option. Moreover, as it is not possible to include two linear trends at the same time, the only thing we can do in the DID_l estimations to control for the situation of renovated neighborhoods is to include a dummy in the estimation of single difference-in-differences, which are further aggregated to compute the DID_l estimate. The placebo coefficients show that this change in the specification used with the DID_l estimator still allows to verify the extended parallel trends assumption.

[16] Observations at $T(n)-1$ and $T(n)$ are required to define the change in the treatment status of the neighborhood. As noted by DCDH, the properties of the DID_l estimator hold even if some groups appear or disappear over time.

[17] The aggregation is implemented within the `did_multiplegt` command.

These two features of the DID_l estimator yield larger standard errors than with the more classical DFE estimator. In addition, the method used to compute the DID_l estimator does not allow to include both urban units trends and a trend specific to renovated neighborhoods. This is why we show results based both on DID_l and two-way fixed effects estimations.

3 Data

3.1 Data sources

Our analysis exploits a dataset derived from notaries registration of housing transactions (French Notaries Association datasets *Bien* and *Perval*) and covering metropolitan France between 2000 and 2014 in even years. The data comprises the date and price of transaction, the geographical coordinates of the housing unit and some of its characteristics (among which apartment or house, number of rooms, number of bathrooms, floor space, construction period and plot land area for houses). Given our empirical strategy, we only retain from this dataset transactions located in an urban policy neighborhood. We apply to this dataset the same treatments as those employed by the French National Institute for Statistics (INSEE) to produce housing prices indices at the local level (*Indice Notaires-INSEE*). First, we suppress atypical housing units.[18] Second, we keep only private sales of housing units free of occupation, exclusively dedicated to housing and acquired in full ownership. Third, we suppress housing units with non-standard dimensions or price.[19] Eventually, in order to deal with some records of bad quality in terms of match between characteristics of the housing unit and transaction price, we run the baseline regression corresponding to Equation (1) and set aside the 5% of transactions for which the gap between predicted and observed price is the largest. After these treatments, the database contains about 72,000 transactions located in 647 neighborhoods.

We merge this data with information on PNRU operations at the neighborhood level. This information, provided by the ANRU (*Engagements* and *Enquête Livraison* datasets), includes for

[18] i.e. for apartments: artist's workshop, maid room, attic awaiting conversion, caretaker's dwelling, loft, villa, high standing; and for houses: chalet, tower, windmill, old train station, farm, large property, castle, private mansion, host house, villa.

[19] i.e. we keep, for apartments: units with less than or 9 rooms, a floor space comprised between 10 and 200 square meters, a price between €1,500 and €5,000,000, a square meter price of less than €25,000; for houses: units with less than or 13 rooms, more than 20 square meters and less than 300 square meters, more than 9 square meters lot size, a price of more than €1,500, and, outside the Parisian urban area, the house must have a price lower than €15,000,000.

each renovated neighborhood the amounts allocated by the Agency, the amounts provided by other financing partners, such as social landlords or municipalities, the list of operations conducted, the number of housing units demolished, constructed and renovated, and the dates at which the Agency supplied funds. Finally, we complete the data with local-level socio-economic and income characteristics from the 1999 French Census (*Recensement de la Population* 1999) and localized fiscal revenues data (*Revenus Fiscaux Localisés* 2001) provided by the French Institute of Statistics (INSEE).

These different data sources are not all at the same geographical scale. While housing transactions are located according to their geographical coordinates, information on renewal operations is available at the level of urban policy neighborhoods while Census and revenue data are available at the IRIS level (an infra-communal geographical unit defined for statistical purposes and corresponding to around 1,800 to 5,000 inhabitants). Using a geographical information system, we find the urban policy neighborhood corresponding to each transaction and define treatment indicators at the transaction level. Then, as neighborhood borders do not match IRIS borders, the former comprising several IRIS or parts of IRIS, we use the intersection rates between IRIS and neighborhoods to estimate the socio-economic characteristics at the urban policy neighborhood level and report this information for each transaction.

In summary, our final dataset is a 14-years long annual panel of housing transactions, precisely located within non-targeted and targeted neighborhoods, for which we know the date when renovation started, the amounts invested and the types of renovation operations, as well as the urban unit it is located in.

3.2 Descriptive statistics

The distribution of transactions in the renovated and non-renovated neighborhoods in the estimation sample is displayed in Table 1. It can be noted that the treatment group is larger than the control group, both in terms of number of neighborhoods and of transactions, with 415 renovated neighborhoods and 232 non-renovated ones. In addition, the number of renovated neighborhoods represented in our sample is lower than the 572 selected in the PNRU in metropolitan France. Indeed, in a sizable number of renovated neighborhoods, no transaction took place at the dates under study, because these neighborhoods are small and largely constituted of public housing units. There are on average 112 transactions (s.d. 204) per neighborhood for the 14-years period observed in our dataset.[20] The number of transactions over the period is higher in renovated neighborhoods by about 34 transactions as compared to non-renovated ones.

Table 1 also shows the distribution of the year of first funding obtained from the ANRU for transactions located in renovated neighborhoods. In most renewed neighborhoods (230 out of the 415 renovated neighborhoods in our sample), ANRU signed its first financial commitment as early as 2004. In a sizable number of neighborhoods (122), the first funding was obtained between 2005 and 2007. Only in 63 neighborhoods did funding start between 2008 and 2014. The distribution of treatment adoption over time and across neighborhoods is therefore highly non-uniform, especially because there are neighborhoods treated for most of the observation period and others treated only at the end of the period.

Table 1: Number of neighborhoods and transactions in the estimation sample

	Non-renovated neighborhoods		Renovated neighborhoods	
	# neighb.	# transactions	# neighb.	# transactions
Total	232	20,851	415	51,342
Starting year of renov.				
2004			230	32,895
2005			69	7,947
2006			22	2,099
2007			31	2,059
2008			22	1,826
2009			18	1,711
2010			6	300
2011			7	1,735
2012			2	14
2013			3	469
2014			2	127
2015			3	160

Source: French Notaries Association datasets *Bien* and *Perval*; ANRU, *Engagements* and *Enquête Livraison* datasets.

Table 2 presents descriptive statistics of renovated and non-renovated neighborhoods, as well as some characteristics of the renovation program in renovated neighborhoods. In terms of socioeconomic characteristics, renovated neighborhoods have a higher unemployment rate, a lower proportion of French citizens and lower revenues as compared to non-renovated neighborhoods. But the difference between the two types of neighborhoods lies primarily in their housing stock

[20] As explained above, only even years are observed.

in 1999: renovated neighborhoods have a larger housing stock than non-renovated ones, a larger share of public housing and a lower share of owner-occupiers. Between 1999 and 2014, the public housing stock in renovated neighborhoods was reduced by 0.4% on average while it increased by 11% in non-renovated neighborhoods, with however large differences across the latter. The private housing stock, instead, followed in both types of neighborhoods a similar growth of around 18%.

Regarding renovation, the large average amount of funding reported to the initial housing stock is associated with a large variance. This variance is even larger when funding is reported to the public housing stock. There is thus quite large differences in the intensity of renovation depending on neighborhood. Furthermore, larger investments were made in neighborhoods with a large share of public housing, but it is however not the case that all funding was concentrated on public housing. Indeed, the correlation of the public housing share with funding is 0.148 when funding is reported to the initial housing stock, but this correlation is -0.186 when funding is reported to the public housing stock.

This is related to different types of renovation operations which were conducted in different neighborhoods. Table 2 shows that most of the financing was devoted to the destruction and construction of housing (43%) and the rest of the financing was equally divided between equipment and rehabilitation of existing housing units. Actually, the largest investments were made in order to demolish very large public housing projects, as illustrated by the correlation of the share of investments devoted to demolition with total funding by housing unit, which is 0.483, and with the share of public housing, which is 0.167.

The share of total funding devoted to public housing can be approximated by the share of investment devoted to demolitions, new constructions and rehabilitation. Its correlation with the initial share of public housing is 0.224. Altogether, although these correlations are significant, they are not very large, which means that there is some heterogeneity in the type of neighborhoods which received large investments, and in the types of operations they received.

Table 3 presents some descriptive statistics of the housing transactions data. It shows that the mean transaction price is lower in renovated than in non-renovated neighborhoods, with €105,000 per transaction in renovated neighborhoods versus €136,000 in the others. Yet, the average floor space, the share of dwellings with 4 rooms or more and the share of houses are greater in renovated neighborhoods.

Figure 1 shows the housing price evolution (in logarithm) for a reference housing unit in renovated and non-renovated neighborhoods (1) in the Parisian urban unit, (2) in Lyon, Marseille and

Table 2: Characteristics of renovated and non-renovated neighborhoods and characteristics of renovation

	Non-renovated neighborhoods		Renovated neighborhoods		Eq. test
	mean	sd	mean	sd	p-value
	Characteristics of neighborhoods				
Housing stock					
% public housing	50.8	21.2	56.3	20.5	0.001
% owner-occupiers	29.0	14.3	25.3	12.7	<0.001
Number of dwellings	1584	2011	2809	2951	<0.001
Δ public housing 1999-2014 (in %)	11.0	110.2	-0.4	33.6	0.068
Δ private housing 1999-2014 (in %)	17.8	28.4	18.6	32.6	0.753
Population in 1999					
% blue-collar workers	36.2	10.7	36.9	9.0	0.430
% executives	9.9	5.6	9.3	4.5	0.107
% French citizenship	87.9	7.7	85.6	8.5	<0.001
Unemployment rate	22.1	7.2	24.6	7.5	<0.001
Median annual income 2001 (in €)	11,015	2,413	10,056	2,339	<0.001
	Characteristics of renovation				
Renovation funding					
Total funding / housing unit stock	.	.	93,002	101,028	
Total funding / public housing unit stock	.	.	191,761	1,220,317	
Share of renovation expenditures on					
Housing constr. and demolition	.	.	43.2	24.1	
Rehabilitation	.	.	27.8	18.9	
Equipment	.	.	28.9	20.0	
Observations	231		415		

Note: The last column shows the p-value of two-sample tests of equality of mean or proportion.

Source: Population Census 1999 and ANRU, *Engagements* and *Enquête Livraison* datasets.

Lille (the three largest urban units after Paris) pooled together, and (3) in all the other urban units in our sample. Although confidence intervals are large, we observe that prices exhibit similar evolution patterns in each of the three subsamples. Consistently Table 3, prices in non-renovated neighborhoods are on average higher than in renovated neighborhoods, and the gap appears to slightly increase over time. Figure 2 complements this figure by decomposing the sample of transactions depending on the starting year of renovation in the neighborhood. It shows that the evolution of prices before renovation is similar whatever the starting year. This supports the assertion that the starting year of renovation was not decided depending on the evolution of housing prices in the neighborhoods. The average trends after renovation are also not significantly different (s.e. are not shown for readability). Figure A2 in Appendix presents the neighborhood average evolution of the volume of transactions per housing unit in 1999. Here also, there are no significant differences between the treatment group and other neighborhoods.

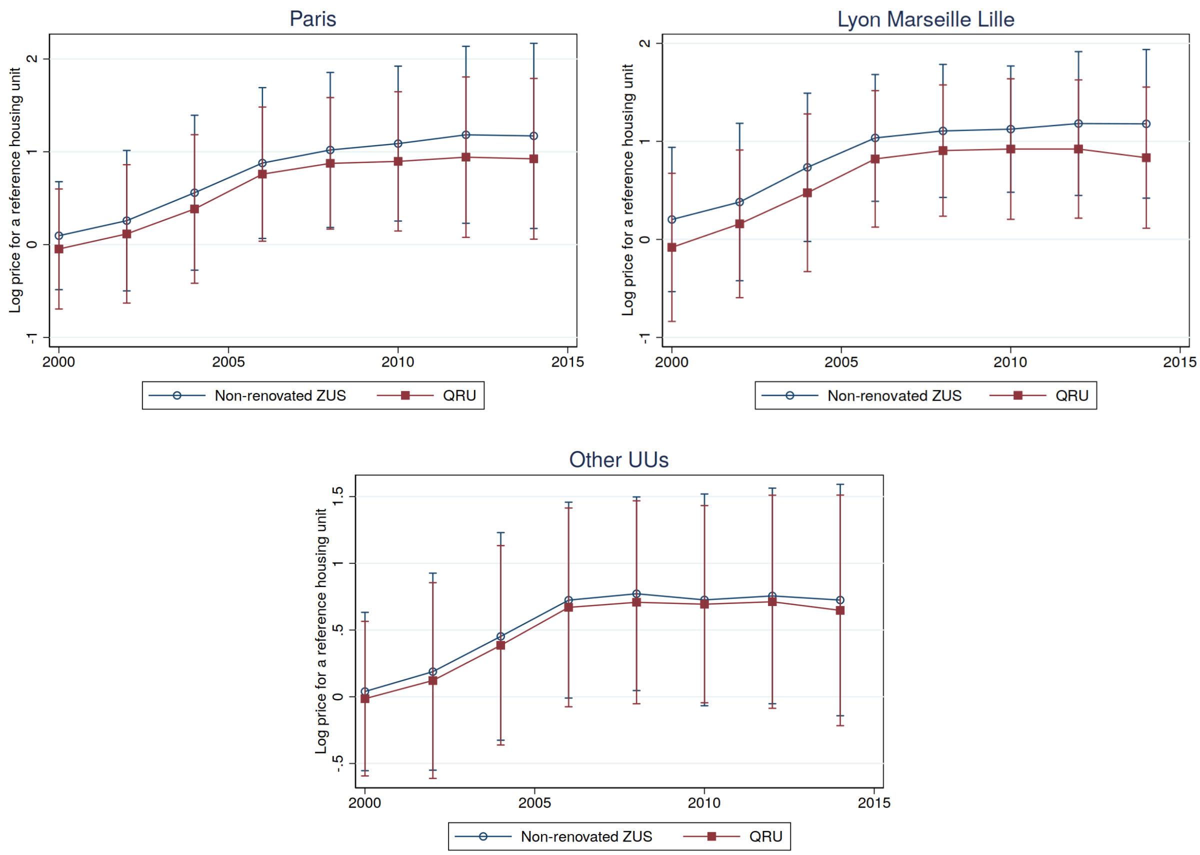
Table 3: Descriptive statistics on transacted housing units in urban policy neighborhoods

	Non-renovated neighborhoods mean	Non-renovated neighborhoods sd	Renovated neighborhoods mean	Renovated neighborhoods sd	Eq. test p-value
Price (Euros)	136,119	105,395	105,504	64,881	<0.001
Floor space (m^2)	63.5	28.0	68.0	26.7	<0.001
Number of rooms					
1	0.12	0.32	0.08	0.27	<0.001
2	0.23	0.42	0.18	0.39	<0.001
3	0.27	0.44	0.29	0.45	<0.001
4	0.25	0.43	0.28	0.45	<0.001
≥ 5	0.14	0.34	0.17	0.37	<0.001
Number of bathrooms					
None	0.03	0.17	0.02	0.15	<0.001
1	0.88	0.33	0.89	0.32	<0.001
≥ 2	0.04	0.20	0.04	0.20	0.423
Unknown	0.05	0.22	0.05	0.22	0.054
Detached housing (%)	0.13	0.33	0.16	0.37	<0.001
Building less than 5 years	0.00	0.05	0.01	0.07	<0.001
Building period					
≤ 1913	0.13	0.34	0.05	0.23	<0.001
1914-1947	0.09	0.29	0.10	0.30	<0.001
1948-1969	0.23	0.42	0.23	0.42	0.013
1970-1980	0.20	0.40	0.28	0.45	<0.001
1981-1991	0.06	0.24	0.05	0.23	<0.001
1992-2000	0.03	0.16	0.02	0.14	<0.001
2001-2010	0.01	0.12	0.02	0.12	0.7621
2011-2020	0.00	0.05	0.01	0.07	<0.001
Unknown	0.24	0.43	0.24	0.43	0.862
Year of transaction					
2000	0.13	0.34	0.13	0.33	0.465
2002	0.12	0.33	0.12	0.33	0.805
2004	0.14	0.35	0.15	0.36	0.003
2006	0.12	0.32	0.12	0.33	0.021
2008	0.13	0.33	0.13	0.34	0.066
2010	0.13	0.34	0.12	0.33	0.002
2012	0.11	0.32	0.11	0.31	0.183
2014	0.13	0.33	0.12	0.33	0.009
Distance to city center (km)					
Paris	16,96	7.21	14.66	8.60	0.081
Lyon, Marseille, Lille	6.35	7.30	5.84	4.53	<0.001
Other urban units	3.92	4.92	3.01	2.13	<0.001
Observations	20,851		51,342		

Note: The last column shows the p-value of two-sample tests of equality of mean or proportion.

Source: French Notaries Association datasets *Bien* and *Perval*, *Recensement de la Population 1999*, *Revenus Fiscaux Localisés 2001* and authors' treatments.

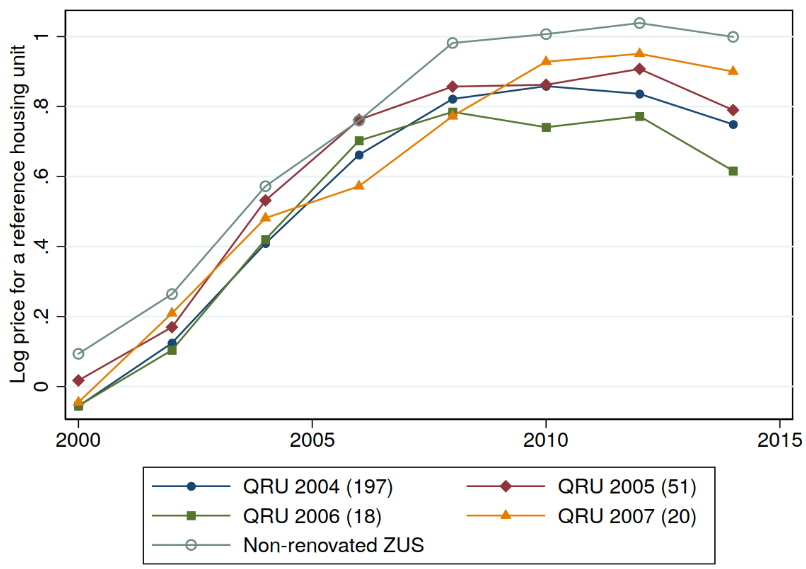
Figure 1: Housing price evolution for a reference housing unit in renovated and non renovated urban policy neighborhoods in the Parisian urban unit (upper left), Lyon, Marseille and Lille urban units (upper right), in the other urban units (bottom). .



Notes: Transaction prices are adjusted for housing characteristics, taking into account their valuation in 2000. For this purpose, a hedonic regression is estimated on housing transactions that took place in 2000. The estimated coefficients from this regression are then used to predict housing prices for transactions in the following years. We obtain a constant quality price index (with a base of 0 in 2000 for the average over renovated and non-renovated neighborhoods) assuming that the valuation of hedonic characteristics is constant over time. The characteristics included in the hedonic regression are: number of rooms, floor space per room, number of bathrooms, period of construction, dummy equal to one if the dwelling is less than 5 years old, distance to the city center and urban unit fixed effects. Error bars indicate the 95% confidence intervals.

Source: French Notaries Association datasets *Bien* and *Perval* and authors' treatments.

Figure 2: Housing price evolution for a reference housing unit in non-renovated and renovated urban policy neighborhoods depending on the starting year of renovation.



Notes: Number of neighborhoods in parentheses. Neighborhoods where renovation started after 2007 and confidence intervals are not shown for the sake of readability. Prices are adjusted for housing characteristics, following the same procedure as in Figure 1.

Source: French Notaries Association datasets *Bien* and *Perval* and authors' treatments.

4 Results

In this section, we first discuss the respective advantages of the two estimators we use. We then present the results, starting with the global impact of the program on housing prices over the whole period following the first renovation operation and then the time-varying treatment impacts. After that, we test the common trend hypothesis and present some robustness checks. Finally, we explore different explanations for our main result, based on the study of heterogeneities on different subsamples of neighborhoods.

4.1 Relative advantages of the DFE and DID_l estimators

In order to assess the respective advantages of the DFE and DID_l estimators, we start by applying the two steps suggested by De Chaisemartin and D'Haultfœuille (2020a) to evaluate the potentiality of bias in the DFE estimate. We first compute the weights implied by the decomposition of the DFE estimator into an average of neighborhood-level treatment effects.[21] We find that 27.1% of these weights are negative and that they are significantly correlated with the year of the transaction (with a correlation coefficient of 0.468) and with the starting year of renovation (with a correlation coefficient of -0.375). Given that the individual treatment effects are likely to depend on these two dimensions for the reasons previously explained, the estimation of the average treatment effect based on the DFE estimator might be biased. We then compute the ratio of the absolute value of the DFE estimate ($|\hat{\beta}_T|$ in Equation (1)) over the standard deviation of weights, which is an indicator of the amount of treatment effect heterogeneity that would lead to a DFE estimate with a sign opposite to that of all individual treatment effects. The value of this ratio is 7.1, which suggests that only a large amount of treatment effect heterogeneity would lead to a DFE estimate of opposite sign as compared to the real impact of the program. Although this observation tends to be reassuring with respect to the quality of the DFE estimate, such a large heterogeneity in treatment effects cannot be totally excluded. The results regarding the reliability of the DFE estimate are thus ambiguous and advocate in favor of DID_l estimations, of which unbiasedness is ascertained.

On the other hand, the DFE estimator also possesses a number of advantages. First, it can be combined with weighting procedures in order to improve the comparability of treated and control groups, which is not possible with the DID_l estimator. Additionally, the DFE estimator has the advantage of yielding more precise estimates than the DID_l estimator. Indeed, for reasons exposed in

[21] The following results are obtained with the `twowayfeweights` Stata package.

Section 2 (stringent data requirements and aggregation of observations), the DID_l estimator is likely to have larger standard errors than the DFE estimator. Moreover, as noted by De Chaisemartin and D'Haultfœuille (2020a), in the very stylized case where errors can be assumed to be homoskedastic and uncorrelated, the Gauss-Markov theorem implies that the DFE estimator is the lowest variance estimator.

Consistently with these theoretical remarks, we can observe that the DID_l estimates presented in the next paragraphs have larger standard errors than the DFE estimates. The bias-variance trade-off between the DID_l and the DFE estimators conjectured by DCDH is hence verified in our setting, with the DID_l estimator allowing for the alleviation of bias and the DFE estimator for the reduction of variance.

Given the ambivalent results regarding the DFE estimate's bias and its merit in terms of variance, we thereafter present results based on both DFE and DID_l estimators.

4.2 Urban renewal policy's impact on property prices

Global treatment effects. We start by analyzing the global effect of the program on housing prices at the aggregate level of France, estimating Equation 1 for the DFE estimation and then its DID_l counterpart. These results are presented in Table 4, where explanatory variables are progressively introduced. In Column (1), naive results in which only neighborhood and year fixed effects are included in the regression are shown. In the following columns, we progressively introduce housing controls (in Column (2)), the distance of the transaction to the center of the urban unit (in Column (3)), urban unit trends (in Column (4)), QRU trends (in Column (5)).[22] More precisely, as the DID_l method does not allow for the introduction of an additional trend, its Column (5) estimation includes a dummy control for renovated neighborhoods instead of a QRU trend. Table 4 indicates that the estimated effect of the PNRU program on housing prices tends toward zero with the introduction of additional control variables. Indeed, the estimated effect varies, in the case of the DID_l method, from a non-significant effect of around -5.3% in Column (1) to a non-significant effect of around 1.1% in Column (5).[23] With the DFE method, the estimated effect varies from a non-significant -5.8% in Column 1 to a non-significant effect of around 0.4% in Column (5). We can note that including housing characteristics mitigates the negative estimated coefficient,

[22]Table A1 in Appendix presents estimated coefficients for all control variables, most of which are very significant and in line with common sense.

[23]We use the conventional interpretation of dummy variables in semi-log models: $\%\Delta y = 100 \times (e^{\widehat{\beta}} - 1)$.

which suggests that housing units transacted in neighborhoods under renovation probably were of lower quality relative to housing units transacted in control neighborhoods. The change in the estimated coefficient when urban unit trends are included shows the importance of controlling for the general evolution of the housing market at the city level. The estimates of global treatment effects given by the DFE estimator and the DID_l estimator are very close to each other in the first four specifications. They diverge only in Column (5) due to the (already noted) non-introduction of the renovated-neighborhoods' specific trend for the DID_l estimator. On the overall, Table 4 suggests that the program did not have a significant impact on housing prices, or that this effect was smaller than 5.8% (resp. 2.3%) on average according to the upper bound of the 95% confidence interval of the DID_l (resp. DFE) coefficient in Column (5). The similarity of results with the DFE and DID_l estimators in all of the specifications tends to show that the DFE estimates are unbiased.

Time-varying treatment effects, pre-trends and placebos. Figure 3 displays the time-varying effects of the program and pre-trends and placebos based on DID_l and DFE estimates, with the same control variables as in Column (5) of Table 4.[24] For the DFE estimation, this corresponds to the temporal decomposition of global effects. As to the DID_l estimator, these time-varying coefficients are those which are averaged in order to compute the global effect shown in Table 4. Time-varying effects have larger confidence intervals than static treatment effects due to a decrease in the number of observations available for the computation of each estimate. Additionally, the confidence intervals are larger for more distant time-varying effects for two reasons. First, the number of observations used to estimate more distant dynamic time-effects is lower. Indeed, given the timing of renovation, fewer neighborhoods are available to estimate the effect of renovation 10 years after its start as compared to 2 years after its start. A second reason is that uncertainty increases (with any type of estimation) when looking at longer term impacts as compared to short-term ones (notably due to the possibility that more and more external exogenous shocks come to disturb the measured effects...).

Regarding the treatment effects, the two graphs seem to corroborate the results of Table 4. Indeed, non-negligible point estimates are obtained after 4-5 and 6-7 years in the DFE method, but they are not significant at the 5% level and point estimates go down to 0 after 8-9 and 10 years. The DID_l method, which is more robust but leads to less precise estimators, especially in periods further

[24]The time-varying effects obtained with the same control variables as in Columns (3) and (4) of Table 4 are presented on Figure A3 in the Appendix. By comparison with Figure 3, they show that the inclusion of a linear trend specific to renovated neighborhoods tends to flatten the pre-trend.

Table 4: Impact of the PNRU program on housing prices - Different sets of controls

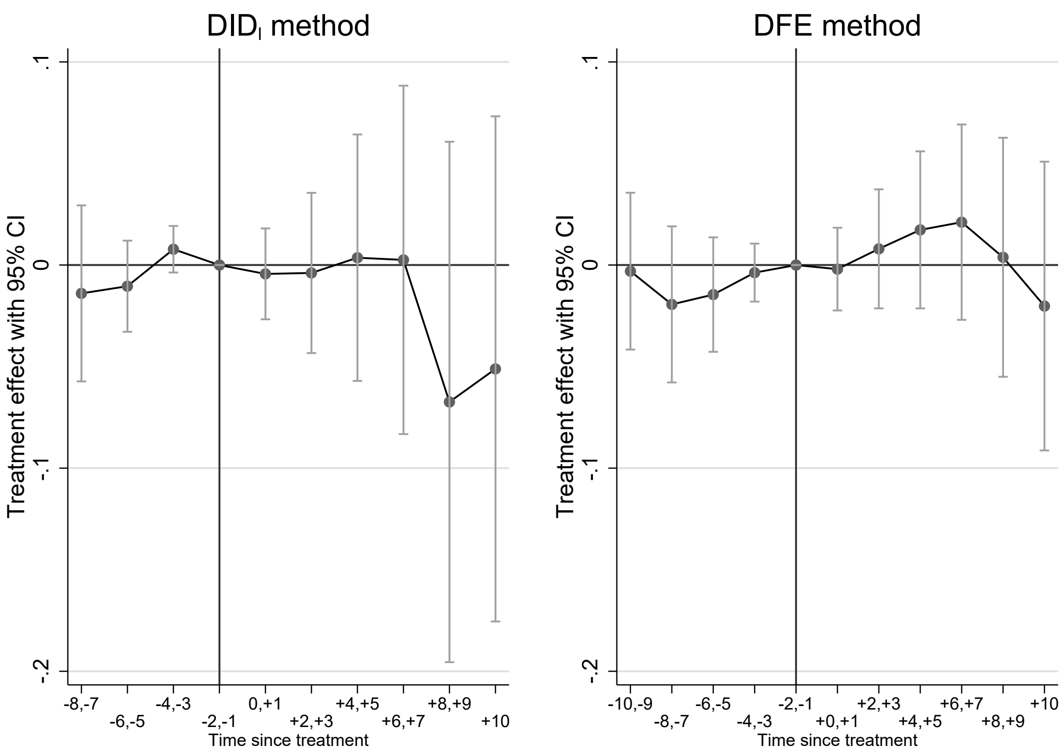
	Fixed effects (1)	+ Housing controls (2)	+ Distance (3)	+ Urb. unit trends (4)	+ QRU trend (5)
DID_l estimator					
β_T [(a)]	-0.0545	-0.0340	-0.0323	0.0117	0.0106
	(0.0480)	(0.0259)	(0.0247)	(0.0247)	(0.0248)
Total observations[(b)]			93,252		
Total switchers[(c)]			32,755		
DFE estimator					
β_T [(a)]	-0.0594*	-0.0351	-0.0347	0.0105	0.00440
	(0.0354)	(0.0225)	(0.0226)	(0.0095)	(0.0095)
R-squared	0.371	0.772	0.772	0.802	0.802
Observations			72,142		
Controls					
Years fixed effects	X	X	X	X	X
Neigh. fixed effects	X	X	X	X	X
Housing unit charac.		X	X	X	X
Distance to urb. unit center			X	X	X
Urban unit time trends				X	X
QRU trends					X

Notes: The dependent variable is the log price of the housing transaction. (a) β_T is the estimated average effect of renovation computed over the years following the start of renovation. For the DFE method, it corresponds to β_T in Equation (1). For the DID_l estimation, it is computed as a weighted average of time-varying effects, with weights proportional to number of switchers used in each time-varying effect estimation (see De Chaisemartin and D'Haultfœuille, 2020b and `average_effect` option in the `did_multiplegt` Stata package) (b) The total number of observations is the sum over all time-varying effect of the numbers of transactions used to estimate each effect. (c) The total number of switchers is the sum over over all time-varying effect of the numbers of transactions in neighborhoods entering the treatment. Housing unit characteristics are described in the data section. Urban unit time trends are quadratic for the DFE estimator and linear for the DID_l estimator. A trend specific to the municipality of Paris is included. In Column (5), the DID_l estimation includes a dummy control for renovated-neighborhoods instead of a trend. Robust standard errors clustered at the neighborhood level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

away from the renovation date, exhibits a flatter evolution. Overall, the time-varying estimates thus confirm the absence of a significant or sustained impact of the program on housing prices.

The placebo estimated coefficients are small and not significantly different from 0, which suggests that treated and control groups indeed experienced similar housing price trends before the start of renovation.[25]

Figure 3: Estimated time-varying impacts of the PNRU program on housing prices, and placebo or pre-trend coefficients



Note: Error bars indicate the 95% confidence intervals.

Source: French Notaries Association datasets *Bien* and *Perval* and authors' treatments.

[25] Whatever the estimator, we do not show coefficients estimated on 20 or less renovated neighborhoods. Joined with the different requirements for the DFE and DID$_l$ estimators, this explains why we do not estimate the same number of coefficients for pre-trends and for placebos. Moreover, the `longdiff_placebo` option of `did_multiplegt` used for the DID$_l$ estimations compute a placebo coefficient only if the corresponding dynamic treatment effect can be estimated for the same treated groups. For example, the lth placebo assesses if parallel trends holds over $l+1$ periods, and is estimated only if the lth dynamic effect can be estimated on the same set of treated groups. This is why we can estimate only three placebo coefficients. The neighborhoods for which pre-trends are estimated with -10,-9 with the DFE estimator are not present in the +6,+7 dynamic effects, nor in the following ones. Note also that, as for dynamic treatment effects, fewer neighborhoods are available to estimate the pre-trend 10 years before the start of renovation as compared to 2 years before its start, which contributes to explain larger standard errors for the first placebo coefficient (-10,-9; -8,-7; ...) as compared to the last one (-4,-3).

4.3 Robustness checks

In order to assess the robustness of these results, we first experiment alternative clustering levels. Secondly, we use weighting procedures combined with the DFE estimator to see whether improving the comparability between the treatment and control groups alters the results. Thirdly, we verify that restricting our dataset to obtain a constant set of treated neighborhoods for all estimated time-varying coefficients does not change the results for the time-varying effects. Eventually, we check that accounting for the Enterprise Zone program that took place in some of the renovated neighborhoods does not affect our results.

Varying cluster levels. Table A2 shows estimates of the program's effect obtained with the DFE method, when varying clustering levels. Column (1) repeats the baseline estimate shown in Column 4 of Table 4. The clustering is shifted to the urban unit level in Column (2), and to the infra-communal IRIS level in Column (3).[26] It is clear from this table that variations in the clustering level do not drastically affect the results, as the impact of the program remains insignificant in all of these alternative estimations.

Weighting method. In Table A3 in Appendix, we experiment alternative control groups with the DFE estimator, by using a weighting technique in order to reduce the differences between the treated and control groups. This method, also used in Ahlfeldt, Maennig and Richter (2017), consists in estimating the probability for each urban policy neighborhood to be renovated, based on its characteristics. Then, we weight each neighborhood (renovated and non-renovated ones) depending on the difference between their predicted probability to be renovated and the threshold value that best predicts designation (i.e. the observed share of renovated neighborhoods).[27,28] The smaller this difference, the larger the weight associated to the neighborhood, following the idea that, around this threshold, selection is more likely to be as good as random. We then run a DFE regression, using these weights. In Column (1), the optimal bandwidth is used (Silverman, 1986). In Column (2), the bandwidth is twice the optimal bandwidth. In Column (3) (resp. (4) and (5)) , the 25% (resp. 50% and 75%) of non-renovated neighborhoods that are the most distant from the

[26]We do not perform the same robustness check with the DID_l estimator because this estimator is valid only on data aggregated at the neighborhood × period level, which means that clustering at the Iris level is not possible.

[27]The following neighborhood-level weights are used: $w_s = \frac{1}{\lambda\sqrt{2\pi}} \exp\left(-\frac{1}{2}\left(\frac{S_S - \bar{S}}{\lambda}\right)^2\right)$ with λ the bandwidth.

[28]This technique is, in spirit, close to a fuzzy regression-discontinuity design as used in Koster and Van Ommeren (2019), in a setting where there is however no explicit eligibility criteria.

threshold are dropped while all the remaining neighborhoods are given a weight of 1. It is clear from Table A3 that improving the comparability between the treated and control groups does not alter the results, as none of the estimated coefficients are significantly different from 0.[29]

Estimation on a constant set of neighbourhoods. As a third robustness check, we restrict the panel to neighbourhoods that entered the program first. Indeed, given the timing of renovation and the observation period in our data, the temporal effects of the program are not all estimated on the same sets of neighborhoods. For example, the impact of the program after 2 years is estimated on neighborhoods where renovation started in any year between 2004 and 2012, while the impact after 10 years is computed only on neighborhoods where renovation started in 2004. It is also the case for the estimation of pre-trends. This pattern leads to potential composition effects. Consequently, as a last robustness check, we estimate DID_l time-varying effects, keeping only neighborhoods in which renovation started in 2004 or 2005 and restricting the sample to time-varying effects comprised between (-4,-3) and (+8,+9), so that each effect is estimated on exactly the same set of neighborhoods. The results, depicted in Figure A5, suggest that the program did not have any sizable impact on housing prices, consistent with the effects depicted in Figure 3.

Together with Figure 2, this robustness check contributes to rule out potential identification threats due to anticipation effects or endogeneity of program entry date. Indeed, there might potentially be a concern that housing prices could have begun to increase as soon as information about the program was made public, so that considering the year of first funding as the start of renovation would miss the early impacts of the program. As the PNRU was voted in mid-2003, housing prices in neighborhoods in which renovation began several years later could be affected by this type of anticipations. However, neighborhoods in which renovation started in 2005, let alone 2004, are unlikely to be concerned. Therefore, the similarity of the results obtained when restricting the sample to neighborhoods renovated in 2004 and 2005 shows that the overall absence of effect of the program cannot be attributed to anticipations.

Accounting for the Enterprise Zone program. In parallel to the PNRU renovation program, some of the neighborhoods in our sample were part of an Enterprise Zone program targeting a list of neighborhoods called "*Zones Franches Urbaines*" (ZFU in the following). This program was led in three waves, with new neighborhoods successively joining the program in 1996, 2003 and 2006.

[29] We also verified the absence of any pre-trend by estimating Equation (2). The corresponding graph for the specification in Column (2) is presented in Figure A4 in Appendix.

We focus on the second and third waves, which dates of implementation coincide with our period of study. 51 renovated neighborhoods and 5 non-renovated ZUS in our sample belong to the second or third wave of the Enterprise Zone program. As a robustness check, we added into our baseline specification a dummy equal to zero before the inclusion of the neighborhood into the ZFU program and equal to one after, in order to control for the specific effect of this other program. We further add a time trend specific to the ZFU neighborhoods, to account for a potentially different housing price dynamic in the ZFU as compared to the other ZUS.

Table A4 reports the results obtained. Columns (1) and (2) simply report the results obtained with the same specifications as in Columns (4) and (5) of Table 4, but a few observations were lost in the merging with the ZFU database. Columns (3) and (4) show a barely significant negative impact of belonging to a ZFU, which in any case does not affect our results regarding the renovation program's impacts.

4.4 Possible explanations for the absence of significant impacts of the program

The limited size and non-significance of the estimated aggregate impact of the renovation program could be explained by a variety of rationales, which we hereafter successively discuss. For this discussion, we focus on DFE estimations, which have lower standard errors and allow to account for a renovated-neighborhoods specific trend as explained before.

A first explanation for the absence of a significant aggregate impact of the renovation program on housing prices could be based on the fact that, although the total amount of funding dedicated to the program at the national level was large, some of the renovated neighborhoods received too little of it, which prevented any reaction from the local housing market or may even have been counterproductive by increasing the stigma around these neighborhoods (see Ding, Simons and Baku, 2000). As shown in Table 2, the amount of funding per initial housing unit has a large variance, and even more so when considering the amount of funding divided by the number of public housing units alone, which makes sense because renovation is largely focused on public housing buildings and their surroundings, even if a larger neighborhood is targeted by the program. To test this explanation, we investigate whether the level of funding received by the neighborhood for its renovation modifies the estimated impact of the program. In practice, we add to our previously preferred specification (of Column (5) in Table 4) a continuous treatment variable equal to 0 before the start of renovation and to the total amount of funding invested in the neighborhood during the program, with respect to either all housing units or public housing units, after the start of renovation. We also include in some columns the square of this continuous variable.

More specifically, in Table 5, Column (1) replicates the baseline DFE estimate of the program's impact, Columns (2) and (3) include the continuous treatment effect of the amount of funding per housing unit (along with its square in Column (3)), and Columns (4) and (5) the continuous treatment effect of the amount of funding per public housing unit (along with its square in Column (5)).

Table 5: Impact of the program depending on the level of funding per housing unit or per public housing unit

	β_T (1)	$\beta_{T,fh}$ (2)	$\beta_{T,fh}$ quad. (3)	$\beta_{T,fph}$ (4)	$\beta_{T,fph}$ quad. (5)
DFE estim. (w/ QRU trend)					
β_T	0.00440	0.0118	0.00811	-0.00392	-0.00785
	(0.00953)	(0.0109)	(0.0132)	(0.0103)	(0.0108)
$\beta_{T,fh}/\beta_{T,fph}$		-0.00600	-0.00110	0.0108*	0.0170**
		(0.00401)	(0.0109)	(0.00627)	(0.00835)
$\beta_{T,fh^2}/\beta_{T,fph^2}$			-0.000766		-0.000552**
			(0.00144)		(0.000255)
R-squared	0.802	0.802	0.802	0.802	0.802
Observations			72,142		
DFE estim. (w/o QRU trend)					
β_T	0.0105	0.0180*	0.0138	0.000829	-0.00362
	(0.00953)	(0.0108)	(0.0129)	(0.0101)	(0.0107)
$\beta_{T,fh}/\beta_{T,fph}$		-0.00561	-0.000215	0.0113*	0.0176**
		(0.00398)	(0.0110)	(0.00632)	(0.00837)
$\beta_{T,fh^2}/\beta_{T,fph^2}$			-0.000846		-0.000570**
			(0.00145)		(0.000256)
R-squared	0.802	0.802	0.802	0.802	0.802
Observations			72,142		

Notes: Dependent variable is the log price of the housing transaction. β_T stands for the coefficient of the treatment dummy variable, $\beta_{T,fh}$ (resp. β_{T,fh^2}) for the coefficient of the continuous treatment variable defined as the (resp. squared) ratio of funding to the initial stock of housing (€100,000/pub. housing unit in 1999), and $\beta_{T,fph}$ (resp. β_{T,fph^2}) for the coefficient of the continuous treatment variable defined as the (resp. squared) ratio of funding to the initial stock of public housing (€100,000/housing unit in 1999). Control variables are housing unit characteristics, distance, urban unit time trends and year and neighborhood fixed effects. Robust standard errors clustered at the neighborhood level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

While the amount of funding per housing unit clearly does not have a significant impact on private housing prices, the amount of funding per public housing unit is associated with a positive point estimate, significant at the 95% confidence level both with and without the QRU trend. Moreover, this effect of program intensity increases at a decreasing rate, as evidenced by the negative coefficient of the squared continuous treatment variable (Column (5)) and by Figure A6 in Appendix.[30] In Columns (9) and (10) of Table 6, the estimation of the program's impact on two subsamples corresponding to renovated neighborhoods that received less or more than the median funding per public housing unit (that is, €53,950) corroborates the preceding result: the treatment effect is significantly different from 0 at the 90% confidence level only for neighborhoods with a funding above the median. This suggests that the intensity of the program with respect to the public housing stock does mediate its impact on housing prices.

In the following of our investigations, we thus focus our efforts on understanding the factors that may influence the effects of the program intensity, measured as the amount of funding in relation to the initial public housing stock.

A second explanation for the lack of a significant aggregate impact of the renovation program could be the existence of heterogeneous effects, depending on the renovated neighborhoods' characteristics. These heterogeneous effects could cancel each other out at the aggregate level, as shown for example in Briant, Lafourcade and Schmutz (2015) and Albanese, Ciani and de Blasio (2021). We therefore investigate four potential heterogeneous effects along four distinct characteristics of neighborhoods: their median annual revenue level, their share of public housing (both measured before the start of renovation), the size of the city they belong to, and the distance of the neighborhood to the CBD. More precisely, for each dimension, we divide our sample of urban policy neighborhoods into two, according to their position with respect to the median, except for the city size for which we separate neighborhoods located in the four largest cities of France (Paris, Lyon, Marseille, Lille) and the others.[31,32] We evaluate the intensive impact of the program on each subsample. The re-

[30]Figure A6 shows that the confidence interval of the estimated treatment effects increases strongly with the treatment variable. Still, the treatment effect is significantly different from 0 starting at about €300,000 per public housing unit.

[31]We chose to compute the median according to the number of observations rather than the number of neighborhoods, and considering only the renovated neighborhoods, so as to ensure having enough observations for renovated neighborhoods on both sides of the median. This is the reason why the number of observations differ in the subsamples above and below the medians.

[32]The corresponding medians are: €10,040 for the median annual revenue, 49% for the share of public housing in 1999, and 4.1 km for distance to the CBD.

sults are depicted in the two upper panels of Table 6 (Columns (1) to (8) follow the aforementioned list of heterogeneities).[33] To ease the interpretation of the quadratic form, Figure A9 in Appendix presents the plots of marginal treatment effects depending on the amount of funding for subsamples with significant coefficients.

We observe that, in neighborhoods with a (relatively) high revenue level or a low share of public housing, the program's impact appears to have been positive (although only slightly significant) and non linear, while this is not the case for neighborhoods with a low revenue level or a high share of public housing. This weakly suggests that a stigma effect may have played a role in the poorest neighborhoods and in neighborhoods with a high share of public housing, thereby preventing the renovation to trigger an increase in housing prices. This idea is supported by a recent report from the French Court of Auditors (Cour des Comptes, 2020), which acknowledges the stigma around renovated neighborhoods. The results depending on distance to the CBD are in line with González-Pampillón, Jofre-Monseny and Viladecans-Marsal (2019)'s results and consistent with the idea that the attractivity of the neighborhoods' location mediates the renovation program's impact. The PNRU appears to have significantly increased housing prices in neighborhoods located within a radius of 4.1 km from the CBD. On the contrary, we do not find any evidence in favor of a differenciated impact depending on city size.

Thirdly, an additional line of explanation for the lack of aggregate impact could be that urban renewal operations differ with respect to their impact on housing prices (as shown by Albanese, Ciani and de Blasio, 2021) and that this program puts a lot of emphasis on specific types of renovation operations that only have a limited impact on housing prices. Table 2 shows that, on average, close to 45% of funding were devoted to public housing demolition and construction, and less than 29% to equipment, that is improvements of public spaces and public facilities such as schools, sport areas or social centers. To test this possibility, we once more rely on the comparison of pairs of subsamples defined according to the median: we compare the program's impacts in neighborhoods with a low versus high share of funding devoted to public housing units demolition and construction, neighborhoods with a low versus high share of funding devoted to public housing units renovation and beautification and, eventually, neighborhoods with a low versus high share of funding devoted to the building or renovation of public equipment. The results are presented in Column (11) to

[33] The graphs for placebos and dynamic treatment effects for each of these subsamples can be found in Figures A7 and A8 in the Appendix. For the sake of simplicity, these graphs of dynamic treatment effects include only a linear continuous treatment.

Column (16) in the bottom panel of Table 6.[34] Estimated coefficients show that neighborhoods with a low share of funding devoted to construction/demolition (at the 95% confidence level) or to renovation of public housing (at the 90% confidence level) and neighborhoods with a high share of funding devoted to equipment (at the 99% confidence level) experienced an increase in their housing prices due to the renovation. Altogether these results provide some evidence that renewal operations on equipment have a larger effect on housing prices than construction/demolition and renovation operations that target almost exclusively public housing. This appears to be sensible as equipment operations directly benefit private housing owners by improving the city services to which they have access, while construction/demolition and renovation operations mainly benefit public housing tenants and therefore can be expected to have a lower impact on private housing prices. The fact that the program focused most of its funding (75%) on public housing demolition, construction and renovation rather than on equipment and public spaces (25%) may thus also contribute to explain the lack of aggregate impact on housing prices. Figure A9 shows that, for neighborhoods with a high share of funding for equipment, the impact of renovation is the largest and most significant, together with the impact for neighborhoods with a high median revenue.

Finally, a last line of explanation for the lack of aggregate effect could be that the positive price effect implied by the renovation is offset by a supply effect. Indeed, the construction of new public housing units might be expected to have resulted in an increase in the global housing supply which, if sufficiently large, could cancel out the price effect of improved attractivity. However, we can observe in Table 2 that the public housing stock decreased in renovated neighborhoods between 1999 and 2014, while it increased on average in non-renovated ZUS. Additionally, over the same period, the private housing stock grew in similar proportions in renovated and non-renovated neighborhoods. This suggests that, if any supply effect was at play, it should not have led to a *ceteris paribus* reduction of housing prices in renovated neighborhoods relatively to non-renovated ones. We also measured the impact of the program on yearly housing transactions rates (namely, the ratio of the yearly number of transactions to the number of housing units in the neighborhood in 1999) in renovated neighborhoods (see Table 7) and found no evidence that the program affected the level of activity on the housing market. These observations suggest that the lack of aggregate effects of the program on housing prices cannot be explained by a supply effect.

[34] The corresponding medians are: 38.5% for the share of funding devoted to public housing units demolition and construction, 13.8% for the share of funding devoted to public housing units renovation and beautification, 27.0% for the share of funding devoted to building or renovation of public equipment.

Table 6: Heterogeneities in impacts depending on characteristics of neighborhood, urban unit size, distance to CBD, and funding share devoted to a given operation type.

	2001 med. annual revenue		% pub. housing in 1999	
	Low	High	Low	High
	(1)	(2)	(3)	(4)
DFE (w/ QRU trend)				
β_T	-0.0267	0.00152	-0.0109	0.0183
	(0.0196)	(0.0151)	(0.0157)	(0.0167)
$\beta_{T,fph}$	0.00755	0.0247*	0.0180**	-0.0322
	(0.0185)	(0.0142)	(0.00884)	(0.0336)
β_{T,fph^2}	0.000396	-0.000648*	-0.000513**	0.00816
	(0.00174)	(0.000367)	(0.000251)	(0.0118)
Observations	29,716	42,426	40,814	31,328

	City size		Dist. to CBD		Fund. per pub. hous.	
	Four largest	Others	Low	High	Low	High
	(5)	(6)	(7)	(8)	(9)	(10)
DFE (w/ QRU trend)						
β_T	-0.0192	0.00901	-0.0147	-0.00457	0.0205	-0.00764
	(0.0175)	(0.0141)	(0.0152)	(0.0174)	(0.0315)	(0.0157)
$\beta_{T,fph}$	0.0202	0.0152	0.0230**	0.00737	-0.247	0.0170*
	(0.0208)	(0.00941)	(0.00934)	(0.0231)	(0.237)	(0.00904)
β_{T,fph^2}	-0.000856	-0.000509**	-0.000739***	0.000394	0.542	-0.000483*
	(0.00227)	(0.000253)	(0.000245)	(0.00247)	(0.402)	(0.000256)
Observations	38,780	33,362	39,171	32,971	46,484	46,458

	Construction/demolition		Renovation		Equipment	
	Low	High	Low	High	Low	High
	(11)	(12)	(13)	(14)	(15)	(16)
DFE (w/ QRU trend)						
β_T	-0.00943	0.00239	-0.00819	0.0248	-0.00477	-0.00740
	(0.0156)	(0.0254)	(0.0154)	(0.0168)	(0.0200)	(0.0122)
$\beta_{T,fph}$	0.0236**	0.0174	0.0155*	-0.0220	0.0165	0.0265***
	(0.00932)	(0.0358)	(0.00850)	(0.0276)	(0.0233)	(0.00854)
β_{T,fph^2}	-0.000766***	-0.00368	-0.000452*	0.00467	-0.00209	-0.000746***
	(0.000258)	(0.00738)	(0.000246)	(0.00626)	(0.00256)	(0.000223)
Observations	46,477	46,465	46,527	46,415	47,049	45,893

Notes: Dependent variable is the log price of the housing transaction. β_T stands for the coefficient of the treatment dummy variable. $\beta_{T,fph}$ (resp. β_{T,fph^2}) is the coefficient of the continuous treatment variable defined as the (resp. squared) ratio of funding to the initial stock of public housing (€100,000/pub. housing unit in 1999). Column (1) (resp. (2)) restricts the treated group to renovated neighborhoods where the 2001 median annual revenue is above (resp. below) 10,040 euros. Column (3) (resp. 4)) is for a subsample of neighborhoods with more (resp. less) than 49 % public housing in 1999. Column (5) (resp. (6)) corresponds to France's four largest urban units (Paris, Lyon, Marseille, Lille) (resp. other urban units). Column (7) (resp. (8)) is for a subsample of neighborhoods with below (resp. above) 4.1km distance to the CBD. Column (9) (resp. (10)) is for a subsample of renovated neighborhoods with below (resp. above) €53,950 funding per public housing unit. Column (11), (13), (15) (resp. (12), (14), (16) are for subsamples of neighborhoods with below (resp. above) median share of funding devoted to public housing construction/demolition, public housing renovation/beautification, improvement and construction of public facilities (which medians are respectively 38.5%, 13.8% and 27.0%). Control variables are the same as in Column (5) of Table 4. Robust standard errors clustered at the neighborhood level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Impacts of the program on the number of transactions per 1000 housing units (in 1999)

	Fixed effects	+ Urb. unit trends & dist.	+ QRU trend
	(1)	(2)	(3)
β_T	0.415	0.307	-0.176
	(0.428)	(0.455)	(0.571)
$\beta_{T,fph}$	0.0803	0.141	0.133
	(0.258)	(0.312)	(0.310)
β_{T,fph^2}	-0.00102	-0.00215	-0.00191
	(0.00633)	(0.00761)	(0.00756)
R-squared	0.866	0.881	0.881
Observations	4,241	4,241	4,241

Note: The dependent variable is the number of private housing transactions per year divided by the number of housing units in 1999 (x 1,000). Robust standard errors clustered at the neighborhood level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

5 Discussion and conclusion

In this article, we analyze the effects of the French *Programme National de Rénovation Urbaine*, an ambitious urban renewal program targeting large public housing developments and their environment in around six hundred deprived neighborhoods. Using the DID_l estimator proposed by De Chaisemartin and D'Haultfœuille (2020b) and complementing it by the potentially biased but also more precise traditional double fixed effect difference-in-differences estimator, we do not find any significant impact of the French renovation program on local housing prices at the aggregate level: our results indicate that the program had, in any case, an average impact of less than 2.3% on housing prices.

However, this aggregate result appears to hide some heterogeneous effects according to the amounts invested, depending on the characteristics of the renovated neighborhoods and of the renovation operations. Four distinct factors appear to have played a role in modulating the program's impact on housing prices. First, an intensity effect is shown to influence the impact of renovation on housing prices: the higher the funding with respect to the initial size of public housing stock, the more positively housing prices evolved. Second, a stigma effect seems to have been at play in the poorest neighborhoods and in neighborhoods with the highest share of public housing, cancelling any positive impact on housing prices. Third, closeness to the CBD appears to be important, as only renovated neighborhoods located close enough to their urban unit's CBD experienced an increase in attractivity. Lastly, an important share of the PNRU funding was targeted specifically on the public housing stock, for its demolition, reconstruction or renovation, so that the benefits for private homeowners was limited, while it has been larger in neighborhoods where renovation focused on the improvement of the neighborhoods' public spaces and public facilities. Additionally note that, as the private housing stock evolved similarly in renovated and non-renovated neighborhoods while the public housing stock in renovated neighborhoods was the only one to decrease, and as the program does not appear to have affected the volume of housing transactions, we rule out the hypothesis that the absence of aggregate effect of the program was due to an increase in housing supply in renovated neighborhoods.

In addition to the four distinct factors identified in the paper, a fifth potential explanation, which can unfortunately not be properly tested with our dataset, could be that the limited effect of the program on the social composition of renovated neighborhoods prevented the increase in public housing's and in local equipment's quality to translate into higher housing prices.[35] If

[35] Guyon (2016) found that the PNRU program led, in renovated neighborhoods, to a 2% decrease in the poverty

this was indeed the case, this paper could be said to highlight the role of endogenous amenities on the housing market, meaning that the main component of a neighborhood's attractivity is its social composition and not the quality of the built environment, and that this leads to cumulative mechanisms, which are difficult to change.

Our results contribute to shed new light on the existing literature on urban renewal's impacts. First, they allow to corroborate some of the factors identified in the literature as important mediators of the impact of urban renewal policies on housing markets. Indeed, the key role played by the level of funding, which implies that neighborhoods receiving too little funding do not see their housing prices increase, is in line with Ding, Simons and Baku (2000)'s results that the housing subsidy policy led in Cleveland as of 1991 only had an effect on housing prices in neighborhoods that received enough funding. Additionally, the (weak) evidence found in this article for the existence of a stigma effect that may dampen renovation's impact echoes Santiago, Galster and Tatian (2001)'s results, who find that the urban renewal policy led in Denver in the 1990s only had a significant effect on housing prices in neighborhoods where the share of black residents was low enough. Our results are also in line with those of González-Pampillón, Jofre-Monseny and Viladecans-Marsal (2019) regarding the role of the attractivity of the renovated neighborhood's location.

Second, our results about the heterogeneity of urban renewal's impacts depending on the dominant type of renewal operations led in the neighborhood sound a note of caution regarding any hasty generalization regarding urban renewal's impacts on housing markets. Indeed, the urban renewal programs studied in the literature are very diverse, not only with respect to the neighborhood context in which they take place, but also with respect to the types of urban renewal operations implemented. These operations range from intense and concerted demolition/reconstruction operations led by Local Public Agencies created for this purpose in the US Federal program studied by Collins and Shester (2013) to much less invasive policies such as the First Berlin Renewal Program analyzed by Ahlfeldt, Maennig and Richter (2017), in which private investment in housing was encouraged through *"tax reductions, loans, cash advances and further financial support such as cofinancing"*. Some programs focused on the private housing stock (Ahlfeldt, Maennig and Richter, 2017), some on the public housing stock (Koster and Van Ommeren, 2019) and still others on the improvement of public equipment (González-Pampillón, Jofre-Monseny and Viladecans-Marsal, 2019). The results of these diverse programs are thus not necessarily comparable.

Comparing the setting of the program under study with that of the other urban renewal programs

rate in the public housing sector, a 1% increase in the private housing sector and a 1% decrease on the overall.

studied in the literature, some other factors likely to contribute to the lack of aggregate effect of the PNRU program can be identified.

First, the US urban renewal programs studied by Collins and Shester (2013) and Rossi-Hansberg, Sarte and Owens III (2010), which appear to have had positive impacts, although they share with the PNRU program the recourse to large demolitions, differ from it with regards to the targeting of the program on private housing (rather than public housing in our case) and with regards to the share of private housing in renovated neighborhoods (obviously much higher in the US contexts than in our setting). Additionally, the program studied by Collins and Shester (2013) was based on clearing out entire areas to eliminate slums in order to redevelop central cities areas in a context of economic growth, while the PNRU program was not that radical in its approach and did not benefit from a favorable economic context. These factors may contribute to explain why these two previous studies find a significant effect of their urban renewal program while we do not measure any for the PNRU program.

Then, the Dutch urban renewal program studied by Koster and Van Ommeren (2019) shares with the PNRU program its focus on the renovation of public housing. However, public housing does not play the same role in the two contexts: while in the Netherlands public housing represents 90% of rental housing and does not strongly concentrate low-income households, this is not the case in France, where public housing represents 17% of the housing stock in 2002 (that is, 45% of rental housing) with a much higher concentration of low-income households. The gap between housing prices in renovated neighborhoods and the rest of the city in the Dutch context, which is only of 3.5%, confirms that the neighborhoods targeted for renovation were not nearly as deprived as in the French context. The fact that Koster and Van Ommeren (2019) find a significant impact of urban renewal while PNRU's impact is not sizable therefore could be partly explained by the higher level of stigma in French renovated neighborhoods as compared to the Dutch ones.

Eventually, the context of the German urban renewal program studied by Ahlfeldt, Maennig and Richter (2017) is not far from ours in what concerns the homeownership rate in renovated neighborhoods (much lower than in the other contexts of urban renewal studied in the literature). This program also shares with the PNRU its top-down approach, which was stressed, in the case of the PNRU, by the French Court of Auditors (the *Cour des Comptes*) in a recent report as a lack of coordination between state authorities and local actors during the PNRU (Cour des Comptes, 2020). These two features may also contribute to explain the fact that these two programs are found to have had no significant effect on housing prices.

With regards to theoretical works on the impacts of urban renewal, neither Rossi-Hansberg, Sarte and Owens III (2010)'s nor González-Pampillón, Jofre-Monseny and Viladecans-Marsal (2019)'s model account for the presence of a large share of public housing in the renovated neighborhoods or for the targeting of renovation on the public housing stock. Therefore, it is not straightforward to confront our results to theory. However, we can note that Rossi-Hansberg, Sarte and Owens III (2010)'s model of housing externalities could potentially be marginally amended to allow for an exogenous density of public housing in the renovated neighborhoods. In such a setting, considering that only public housing units are renovated, the magnitude of the impact of renovation on private housing prices in the neighborhood would be predicted to depend on the geographical distribution of public and private housing within the neighborhood. If public and private housing are largely isolated from each other, for example if each stock forms a compact area, then the impact of public housing renovation on (private) housing prices would be predicted to be limited. If, on the opposite, public and private housing are mixed, then a larger impact of renovation would be expected through externalities. As a larger share of private housing is likely to be associated with more mixed public and private housing stock, this adaptation of Rossi-Hansberg, Sarte and Owens III (2010)'s model would predict that a larger share of private housing increases renovation's impact. The results of our heterogeneity analysis depending on the share of public housing in the neighborhood seems to be in line with this prediction: we only obtain a significant positive effect of renovation in neighborhoods with a (relatively) high share of private housing (i.e. more than 51%). The effect of a variation of supply cannot really be discussed with this model as it makes the assumption that the neighborhood is open. The model proposed by González-Pampillón, Jofre-Monseny and Viladecans-Marsal (2019), although it can explain a lack of effect of renovation on the share of migrants in a neighborhood if the share of migrants is too high in the poor neighborhood before the start of the renovation program, leads systematically to a positive effect of renovation on housing prices, which is not in line with the results we obtain.

As a final word, it must be stressed that our results do not imply that renovation had no positive aggregate effects for public housing residents. On the contrary, these effects can be expected to be non-negligible, especially in terms of housing condition and comfort, given that a large share of the public housing stock was rebuilt or renovated. However, they are hard to estimate in our case given the non-market value of rents in public housing in France.

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A Appendix

Table A1: Baseline results - Impact of the PNRU program on housing prices (from the left to the right: progressive introduction of explanatory variables)

	Fixed effects (1)	+ Housing controls (2)	+ Distance (3)	+ Urb. unit trends (4)	+ QRU trend (5)
β_T	-0.0594*	-0.0351	-0.0347	0.0105	0.0044
Year of transaction					
2000	ref.	ref.	ref.	ref.	ref.
2002	0.156***	0.165***	0.165***	0.227***	0.225***
2004	0.501***	0.482***	0.482***	0.561***	0.561***
2006	0.827***	0.810***	0.810***	0.887***	0.886***
2008	0.914***	0.892***	0.892***	0.955***	0.955***
2010	0.910***	0.882***	0.881***	0.894***	0.893***
2012	0.934***	0.908***	0.908***	0.852***	0.851***
2014	0.913***	0.846***	0.846***	0.706***	0.705***
Dwelling type * nb. rooms					
Apart.#1 room		-1.697***	-1.700***	-1.693***	-1.693***
House#1 room		-1.747***	-1.751***	-1.782***	-1.782***
Apart.#2 rooms		-1.078***	-1.080***	-1.080***	-1.080***
House#2 rooms		-0.885***	-0.884***	-0.887***	-0.887***
Apart.#3 rooms		-0.721***	-0.724***	-0.723***	-0.723***
House#3 rooms		-0.498***	-0.500***	-0.496***	-0.496***
Apart.#4 rooms		-0.501***	-0.503***	-0.503***	-0.503***
House#4 rooms		-0.217***	-0.218***	-0.215***	-0.215***
Apart.#5 rooms		-0.312***	-0.314***	-0.317***	-0.317***
House#5 rooms		ref.	ref.	ref.	ref.
Less than 5 years old		0.0462	0.0468	0.0600**	0.0599**
Floor area per room		0.0341***	0.0341***	0.0341***	0.0341***
Number of bathrooms					
0 bathroom		-0.230***	-0.230***	-0.183***	-0.183***
1 bathroom		ref.	ref.	ref.	ref.
2 bathrooms		0.103***	0.102***	0.103***	0.103***
3 bathrooms		-0.0860***	-0.0855***	-0.0875***	-0.0876***
Construction period					
Const bef. 1850		-0.141***	-0.141***	-0.116***	-0.117***
Const 1851-1913		-0.206***	-0.207***	-0.167***	-0.167***
Const 1914-1947		-0.149***	-0.150***	-0.145***	-0.144***
Const 1948-1969		-0.109***	-0.110***	-0.108***	-0.108***
Const 1970-1980		-0.0765***	-0.0763***	-0.0759***	-0.0758***
Const 1981-1993		ref.	ref.	ref.	ref.
Const 1992-2000		0.0884***	0.0880***	0.0869***	0.0871**
Const 2001-2010		0.144***	0.145***	0.140***	0.140***
Const 2011-2020		0.292***	0.297***	0.293***	0.293***
Const unknown		-0.0832***	-0.0836***	-0.0867***	-0.0865***
Dist. to urban unit center			-2.69e-05***	-2.63e-05***	-2.63e-05***
QRU trend					0.00278
Neighborhood fixed effect	Y	Y	Y	Y	Y
R-squared	0.371	0.772	0.772	0.802	0.802
Observations	72,142	72,142	72,142	72,142	72,142

Notes: Dependent variable is the log price of the housing transaction. β_T is the estimated average effect of renovation over the years after renovation started. Explanatory variables according to row headings. Quadratic trends at the urban unit level are included in Column (4) but their coefficients are omitted from the table for lack of space. Robust standard errors clustered at the neighborhood level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A2: Robustness checks: Alternative clustering levels - DFE estimator

	FE neigh. Cluster neigh. (1)	FE neigh. Cluster urb. unit (2)	FE neigh. Cluster IRIS (3)
β_T	0.00440	0.00440	0.00440
	(0.00957)	(0.0107)	(0.00657)
R-squared	0.847	0.847	0.847
Observations	72,142	72,142	72,142

Notes: Dependent variable is the log price of the housing transaction. β_T is the estimated average effect of renovation over the years after renovation started. In Column (1), fixed effects and standard error clustering are at the neighborhood level. In Column (2), fixed effects are at the neighborhood level and standard error clustering at the urban unit level. In Column (3), fixed effects are at neighborhood level and standard error clustering is at the infra-communal IRIS level. Explanatory variables included are housing unit characteristics, distance, urban unit time trends, renovated-neighborhoods linear trend and year and neighborhood fixed effects. Robust standard errors clustered at the level indicated in column title in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A3: Robustness checks: Difference-in-differences with neighborhood weighting - DFE estimator

	Weighting procedure				
	Bandwidth value		W/o most distant ZUS		
	Optimal	Opt. $\times$ 2	25%	50%	75%
	(1)	(2)	(3)	(4)	(5)
β_T	0.0202	0.000944	0.00703	0.00715	-0.00233
	(0.0180)	(0.0135)	(0.00998)	(0.0105)	(0.0140)
R-squared	0.839	0.834	0.837	0.830	0.838
Obs.	71,570	72,142	65,512	54,213	23,854

Notes: Dependent variable is the log price of the housing transaction. β_T is the estimated average effect of renovation over the years after renovation started. In Columns (1) and (2), neighborhoods are weighted based on the predicted probability to be treated estimated in a preliminary step, using Silverman (1986)'s methodology, with a bandwidth set at the optimal level in Column (1) and at twice the optimal level in Column (2). In Column (3) (resp. (4) and (5)), the 25% (resp. 50% and 75%) of non-renovated neighborhoods, of which estimated probability to be renovated is the most distant from the proportion of renovated urban policy neighborhoods, are dropped from the sample and all the remaining transactions are given a weight equal to 1. Explanatory variables included are housing unit characteristics, distance, urban unit time trends and year and neighborhood fixed effects. The variables used for predicting treatment are at the neighborhood level: the percentage of public housing in the housing stock, the share of homeowners, the unemployment rate, the median household income, and the same four variables squared. Robust standard errors clustered at the neighborhood level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A4: Robustness checks - Accounting for the "*ZFU*" Enterprise Zone Program

	(1)	(2)	(3)	(4)
β_T	0.0118	0.00435	0.00639	0.00411
	(0.00959)	(0.00964)	(0.00973)	(0.00948)
QRU trend		0.00339	0.00413	0.00597
		(0.00373)	(0.00378)	(0.00374)
$\beta_{T,ZFU}$			-0.0294**	-0.00738
			(0.0117)	(0.0161)
ZFU trend				-0.00643*
				(0.00351)
Observations	71,024	71,024	71,024	71,024
R-squared	0.803	0.803	0.803	0.803
Number of neighborhoods	636	636	636	636

Notes: Dependent variable is the log price of the housing transaction. β_T is the estimated average effect of renovation over the years after renovation started. Columns (1) and (2) report the results obtained with the same specifications as in Columns (4) and (5) of Table 4. In Column (3) a ZFU dummy is introduced. In Column (4) a ZFU dummy and a ZFU time trend are introduced. Control variables are housing unit characteristics, distance, urban unit time trends and year and neighborhood fixed effects. Robust standard errors clustered at the level indicated in column title in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Figure A1: The urban unit of the city of Grenoble and its urban policy neighborhoods

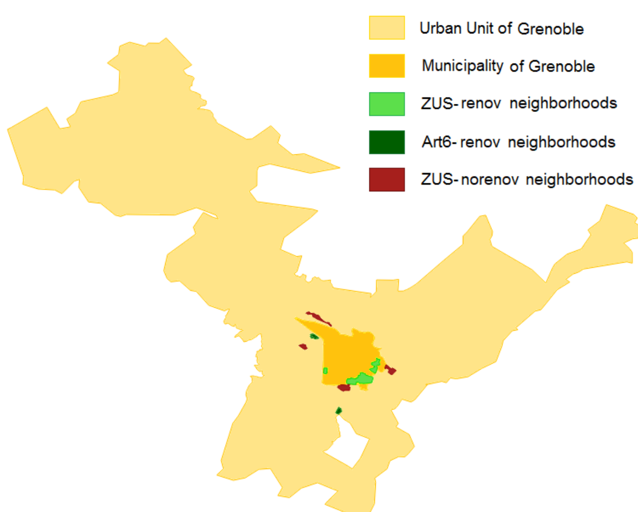
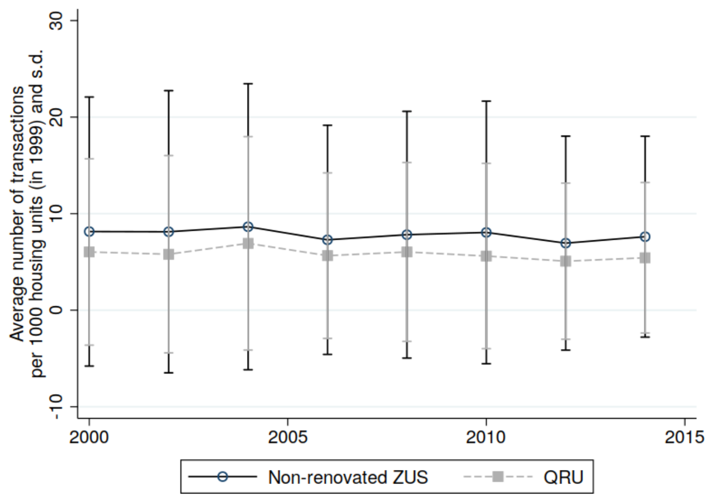
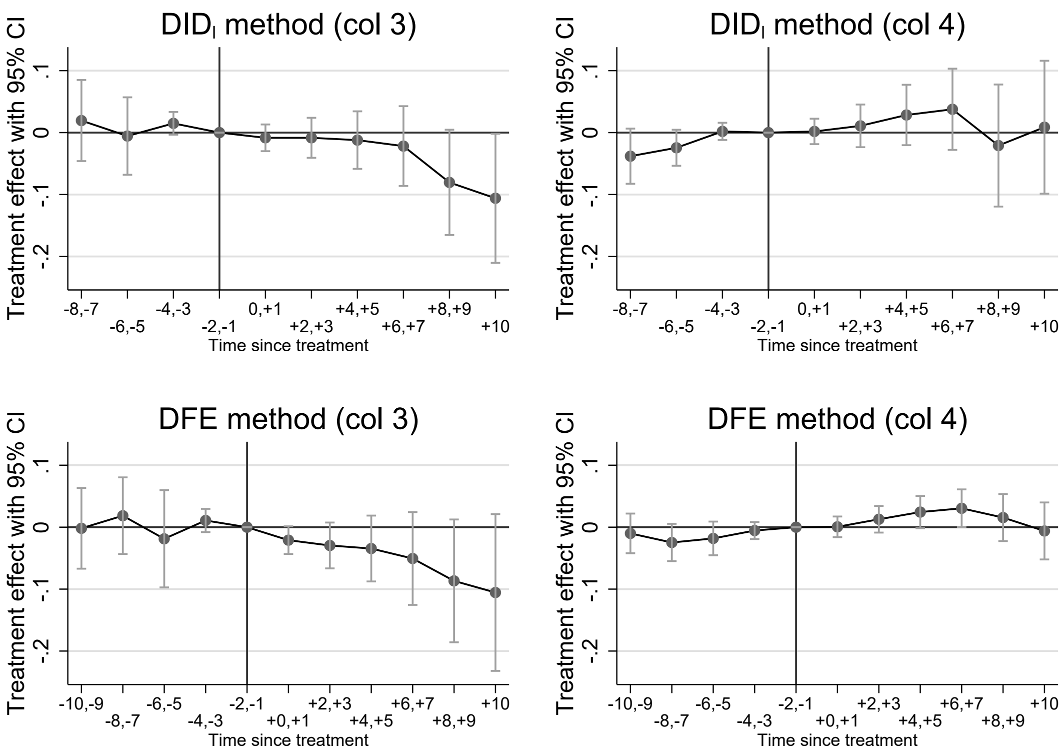


Figure A2: Average number of transactions (per 1000 housing units in 1999) per neighborhood in renovated and non-renovated urban policy neighborhoods



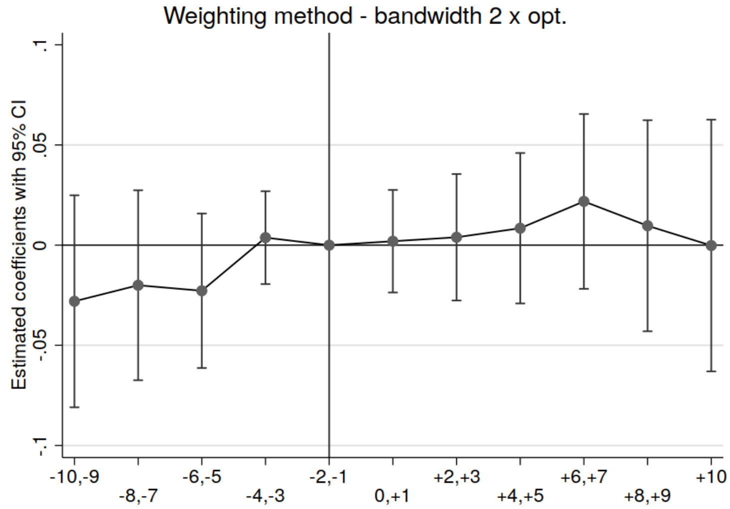
Note: Error bars indicate the 95% confidence intervals.
Source: French Notaries Association datasets *Bien* and *Perval* and authors' treatments.

Figure A3: DID$_l$ (top panel) and DFE (bottom panel) time-varying effects with the same control variables as in Columns (3) and (4) of Table 4



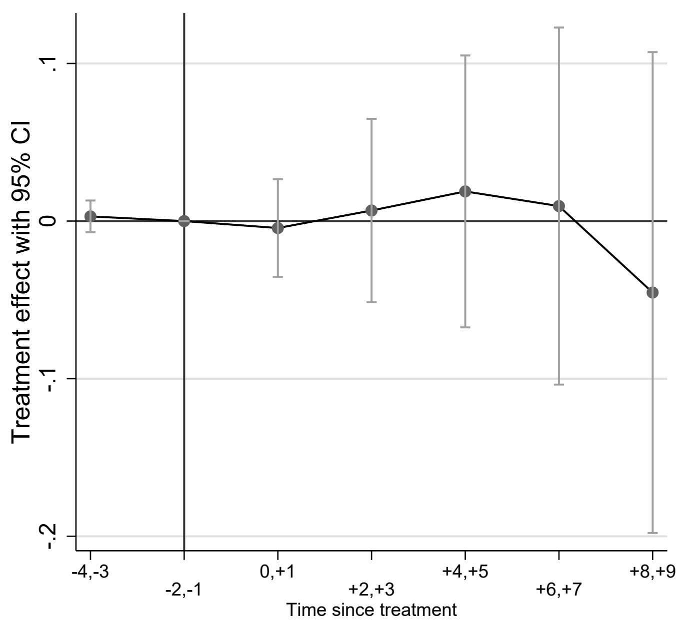
Note: Error bars indicate the 95% confidence intervals.
Source: French Notaries Association datasets *Bien* and *Perval* and authors' treatments.

Figure A4: Estimated time-varying impacts of the PNRU program on housing prices, and placebo or pre-trend coefficients with neighborhood weighting - DFE estimator



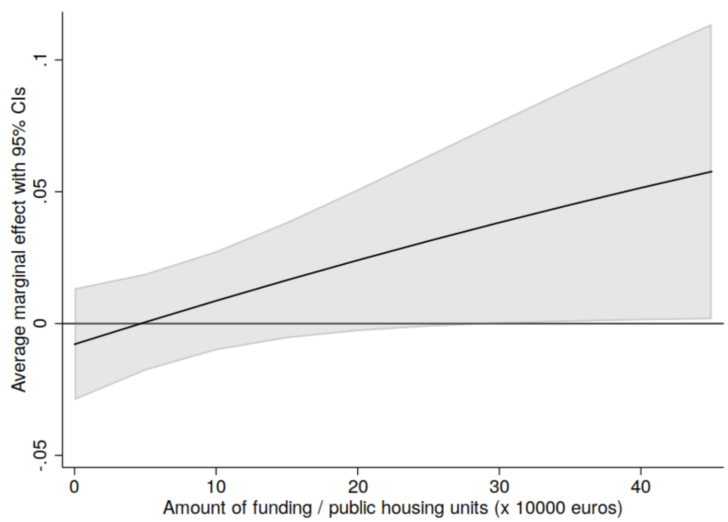
Note: Error bars indicate the 95% confidence intervals.
Source: French Notaries Association datasets *Bien* and *Perval* and authors' treatments.

Figure A5: Robustness checks: Impact of the PNRU program on housing prices keeping only renovated neighborhoods where renovation started in 2004 or 2005 and restricting the sample to time-varying effects comprised between (-4,-3) and (+8,+9).



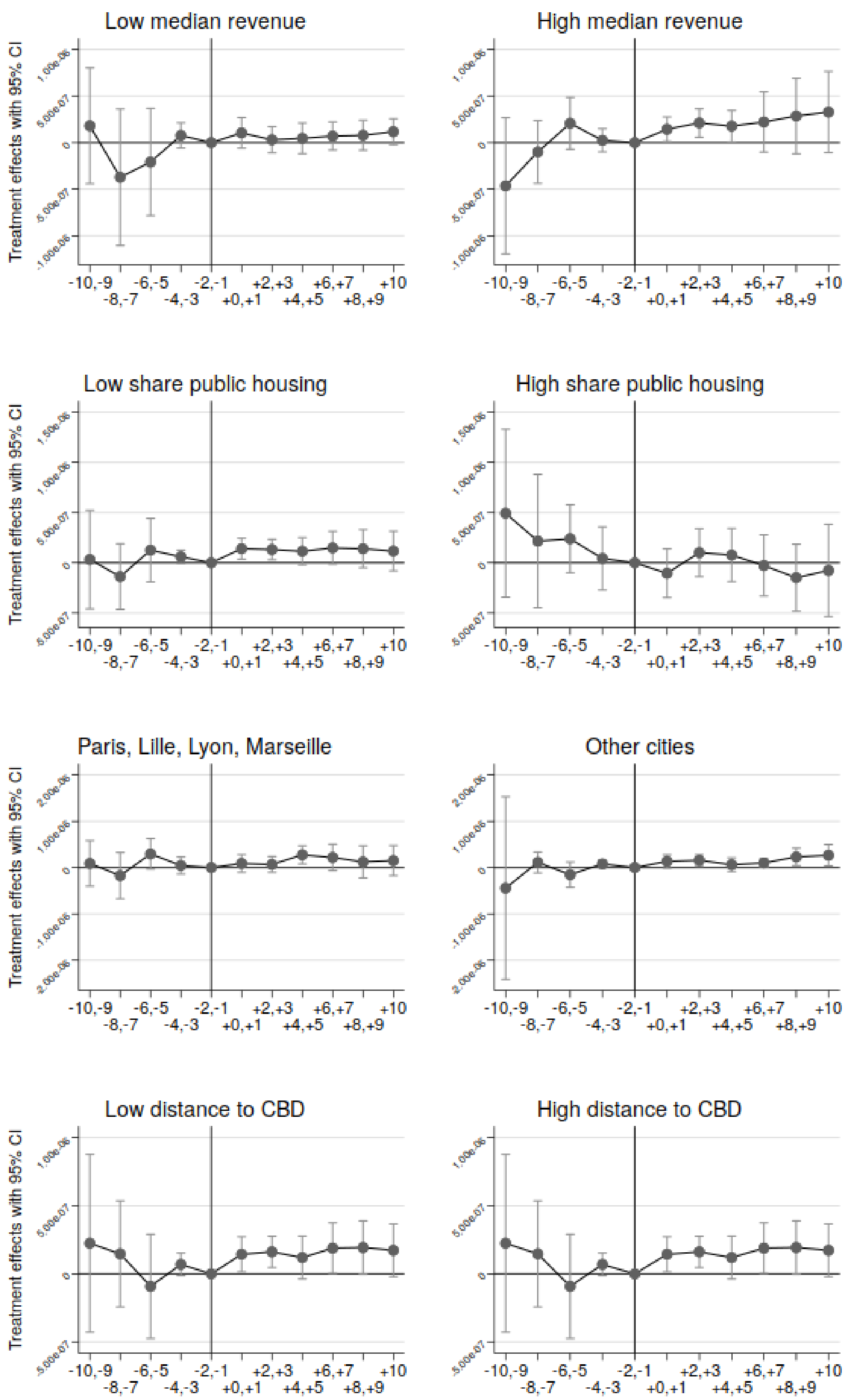
Note: Error bars indicate the 95% confidence intervals.
Source: French Notaries Association datasets *Bien* and *Perval* and authors' treatments.

Figure A6: Average marginal treatment effect depending on the level of funding per public housing unit.



Source: French Notaries Association datasets *Bien* and *Perval* and authors' treatments.

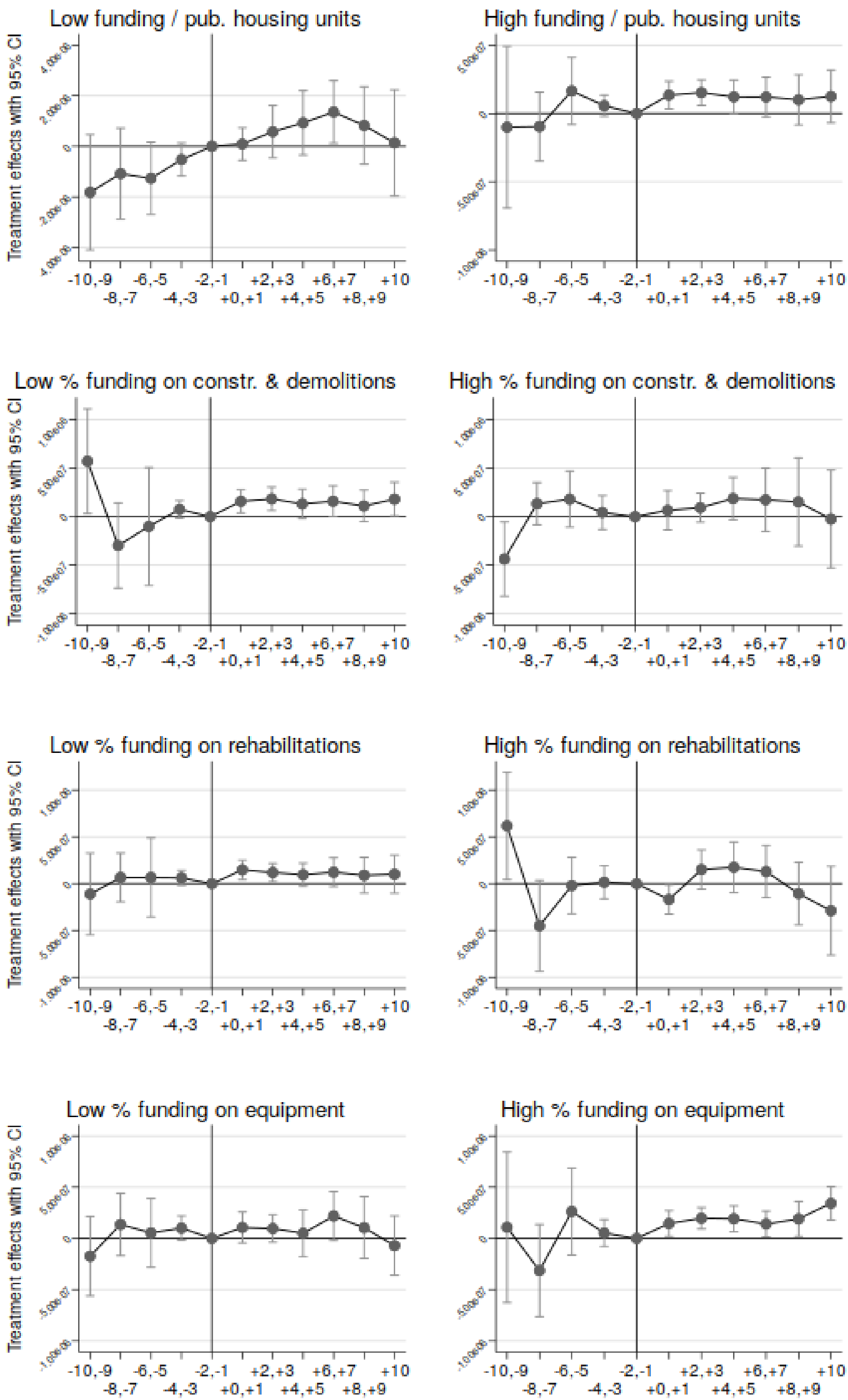
Figure A7: Dynamic impact of the program depending on renovated neighborhoods' characteristics



Note: Each graph corresponds to an estimation on a different subsample, as explained in the notes of Table 6. Error bars indicate the 95% confidence intervals.

Source: French Notaries Association datasets *Bien* and *Perval*, INSEE *Recensements de la population 1999* and *RFL 2001* and authors' treatments.

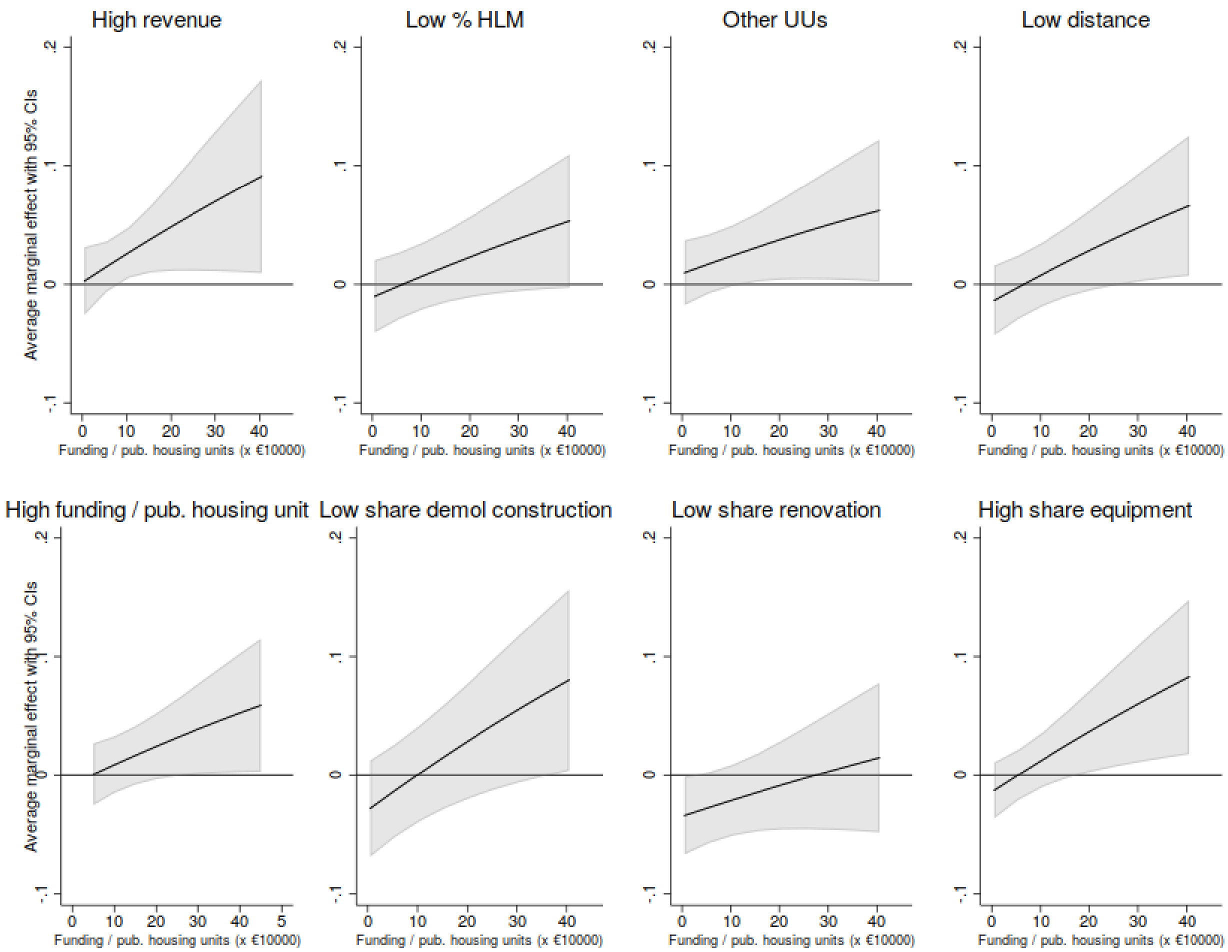
Figure A8: Dynamic impact of the program depending on the renovation operations' characteristics



Note: Each graph corresponds to an estimation on a different subsample, as explained in the notes of Table 6. Error bars indicate the 95% confidence intervals.

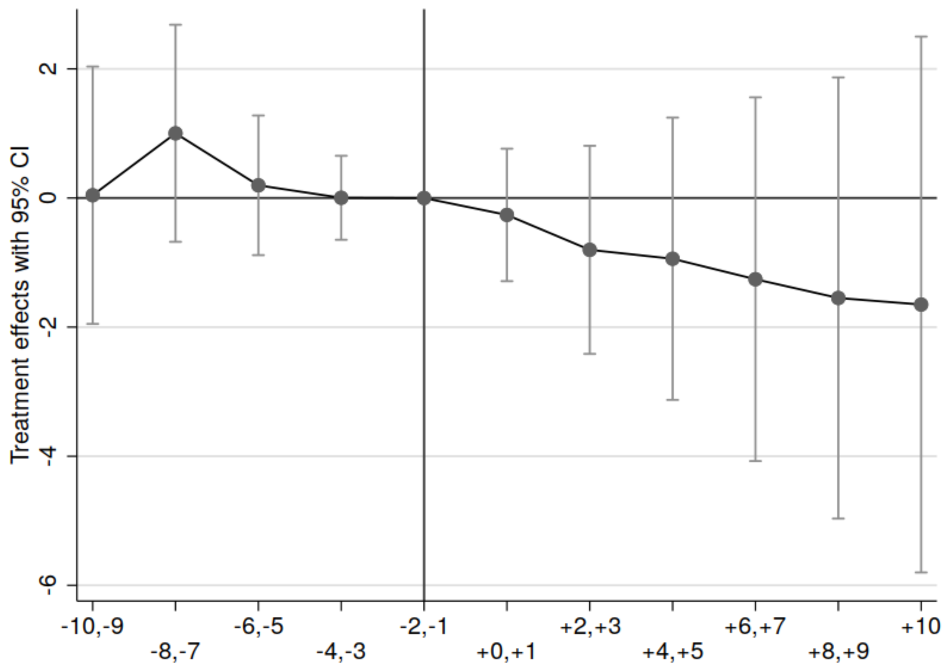
Source: French Notaries Association datasets *Bien* and *Perval*, ANRU *Engagements* and *Enquête Livraison* datasets and authors' treatments.

Figure A9: Average marginal treatment effect depending on the level of funding per public housing unit - for 8 subsamples



Note: Each graph corresponds to an estimation on a different subsample, as explained in the notes of Table 6. Only subsamples with significant treatment effects in Table 6 are considered. Error bars indicate the 95% confidence intervals.
Source: French Notaries Association datasets *Bien* and *Perval*, ANRU *Engagements* and *Enquête Livraison* datasets and authors' treatments.

Figure A10: Impact of the program on the number of transactions per 1000 housing units (in 1999)



Note: Error bars indicate the 95% confidence intervals.
Source: French Notaries Association datasets *Bien* and *Perval*, ANRU enquête livraisons and authors' treatments.