Empowerment Zones and the Housing Market: the French Case

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Abstract

We empirically assess the impact of local enterprise zones in the price of housing for the Paris Region. In 1996, a new urban policy program was launched in France amounting to the creation of three kinds of Enterprise Zones: first tier (ZUS), second tier (ZRU) and third tier (ZFU) with increasing levels of benefits, i.e. cuts in taxes and employer social security contribution when firms hire local persons. In 2004 some ZRUs have been converted into ZFUs in the Seine-Saint-Denis, the Paris region jurisdiction with the highest share of people living in an Enterprise Zone. We use this natural experiment to try to identify the effect of these urban policies on housing prices. We develop a new semiparametric spatial matching methodology with individual data on housing unit sales from the Paris Region Chamber of Notaries for which the exact location is available. Each sale observation before the reform is matched with a similar post-reform counterfactual (close in geographical distance to the reference sale). This matching procedure is done for housing units within the EZ as well as for units in the vicinity of the EZ. We estimate to what extent price patterns within and beyond the EZ boundaries have been impacted by the urban reform leaving unspecified the relationship between housing values and the distance to the closest EZ. Finally the results are compared to those obtained with a control group (some other ZRUs in the same jurisdiction with similar price level that have not been converted into ZFUs in 2004). Our results evidence that the urban policy actually negatively contributed to housing values in ZFUs compared to the control group, suggesting a significant stigmatizing effect for the Empowerment Zones benefiting from the largest amount of public subsidies.

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A Introduction

Local economic development programs have become an important feature of central and local government tax and expenditure systems in numerous countries, such as the US, the UK or France. Among them, Empowerment Zones (EZ hereafter) programs consist of fiscal incentives in impoverished urban neighborhoods with a high unemployment level and a low density of locally-based firms. Many states in the US launched over the 1980's such spatially-targeted policies – referred to as "enterprise zones"[1] – which grant tax deductions to firms establishing in distressed areas and creating jobs. Following these first experiments, similar programs have been initiated in the US (at the federal government level in the 1990's during the Clinton administration) or in France (launched in 1996, with various extensions in the 2000's). Whether EZ have reached their objectives in terms of local employment level, business establishment or neighborhood change is still controversial (see for example Peters and Fishers, 2004) depending on the country considered and the scale and purpose of the program. However, the vast majority of the literature hardly detects significant direct effects of locally-targeted EZ programs.

The purpose of this paper is to assess with a new econometric methodology some consequences of the French program. We rely on geo-coded databases to assess the magnitude of EZ effects on local housing values. In comparison with existing literature, our contribution is threefold *(i)* The key fiscal measure in the French EZ experiment is a wage tax abatement for firms locally hiring 20% of their labour force. Consequently academic studies mainly focused on the job market or economic activity consequences of this program (on local unemployment rates, see Gobillon, Magnac and Selod, 2010, on business creation, see Givord, Rathelot and Sillard, 2011). However, the purpose of the French EZ policies were broader and extended to subsidies for local home ownership and social landlords or implementation of public amenities. The measures taken within these zones might thus affect the local property market. We hence choose to illustrate their consequences on housing values; *(ii)* A large part of the academic literature cannot use a street-by-street mapping of EZ and must rely on an approximation at the US census tract (Busso and Kline, 2007) or at the French municipality level (Gobillon et al, 2010). We instead use individual geo-coded data with precise location (exact street address) and are then able to know with certainty if a given housing unit is located within or outside of the boundaries of a targeted area. We can also calculate the geographic distance of each unit to the closest EZ boundary. To our knowledge, Neumark and Kolko (2010) and Givord et al. (2011) are two studies using similar data but none of them are focused on the housing market; *(iii)* We propose an integrated econometric setup simultaneously assessing the possible effects of the local program within the boundaries of the EZ (using former and/or

[1]In the US, the term *enterprise zone* refers to the different state programs and *empowerment zone* to the federal program. We adopt this last terminology in this paper as we study the impact of a French Central Government program as will be set out hereafter.

future designated EZ as counterfactual) and beyond the limits of the EZ (we estimate the spatial spillover**'s'** effect with a semi-parametric distance-to-the-EZ-boundary gradient function). Consequently, we can test for the significance of EZ impact on home price levels within targeted zones and on home price gaps between units within EZ and units in the vicinity of EZ, *controlling for the distance between these two units*. Some studies on the evaluation of local development public programs also consider spatial externalities, but to our knowledge our study only proposes a distance gradient function permitting to assess the geographic magnitude of these spillovers.

In France, the establishment of the EZ program was initiated in 1996 (Round I program hereafter). Three types of targeted zones were defined with a growing amount of tax exemptions: 750 ZUS (*Zones Urbaines Sensibles*), 416 ZRU (*Zones de Redynamisation Urbaines*, selected among the 750 ZUS) and 44 ZFU (*Zones Franches Urbaines*) were selected and geographically-delimitated (see the description of French EZs below for more details). The EZ program has been enlarged in 2004 and 2006 (Rounds II and III respectively) with the creation of respectively 41 and 15 new ZFUs selected among the pool of preexisting ZRUs. This three-tier program extends throughout the whole French territory (metropolitan and overseas districts). However, the concentration of targeted zones is much higher in the Paris Region (26 ZFU) than in the rest of the territory. The main Metropolitan Areas (MAs) of the *Province* (i.e. the Paris Region apart) typically include at most two or three ZFU (for *Marseille*, *Lyon* or *Lille* for example), while many medium-size MAs include only one. Consequently working on the whole territory comes at the cost of a wide spatial heterogeneity in real estate prices: each MAs has its own market with a specific housing stock (heterogeneity in average housing units' structure and local supply of amenities). Moreover, it might be difficult to identify a *control* zone for Province's MA to assess the local effects of the EZ program. Our study will then be focused on the Paris Region and more precisely on the *Seine-Saint-Denis* district which concentrates a large part of the social and economic problems, with well above Paris Region poverty and unemployment levels. Accordingly, the local density of ZUS, ZRU and ZFU is much higher than in any other French district. In particular, in 2004 (Round II), 6 ZRUs have been converted into ZFUs, thereby offering an interesting natural experiment to check the local impact of the EZ program.

We take advantage of large and detailed individual geo-coded datasets on the housing sales market to analyze the impact of EZ program in a geographically-restricted area. We collect housing sales values, precise location and attributes over the [2002-2005] period, i.e. two years before and two years after the Round II implementation. We use a semi-parametric method mixing spatial matching (in line with Fack and Grenet, 2010), difference-in difference and hedonic housing prices estimations. Following preliminary treatments to the data, we select all *pre*-2004 sales observations within or in the vicinity (units located at a distance less than 500 meters to the closest boundary) of a Round II ZFU. For each observation (the focal point), we construct a post-2004 (after the Round II implementation) counterfactual by selecting a fixed number of sales at the same location or in a immediate proximity with comparable attributes. We then construct the pre- and post- Round II implementation differences of logarithm of housing prices for each focal point within and beyond ZFU borders. We proceed in the same manner for a selected control group of zones that did not become ZFUs in 2004, but could have been according to different criteria (various control groups will be tested using propensity scores results obtained in previous papers or Round III ZFU).

The log of prices gaps (for ZFU or controls, within or beyond the considered zone) are pooled and a partially linear single-index model is estimated. Among the linear set of covariates, we include the difference of the main home attributes of the reference and counterfactual observations. The nonparametric component (i.e., the single index) includes all other factors that may potentially interact in a non-linear manner: in particular, the way the distance to the closest EZ (or control) boundary affect the log of home price differences can hardly be specified and will be assessed with a nonlinear link function $\eta(.)$. Following Yu and Ruppert (2002), we will use a penalized spline (P-spline) to model $\eta(.)$. This type of spline offers several advantages over other semiparametric approaches: it offers a interesting trade-off between the flexibility of the functional form and the implied computational costs.

Our estimated deliver at least two important results for French EZ. First, we provide evidence of a significantly *negative* effect of the EZ program, at least in the short run (i.e. in 2004, immediately after Round II implementation). Housing price growth rates between the pre- and the post-reform periods have been substantially higher within the boundary of selected zones in *the control group* than in Round II ZFU. This result is at odds with most of the preceding contributions which often suggested no significant or very small, but positive, impact of EZ programs[2]. This counter-intuitive outcome may be the consequence of the lack of a suitable control group: those chosen here may still differ from the EZ along different unobservable factors. In spite of our efforts to work on a geographically-restricted area, the *Seine-Saint-Denis* district may nowadays includes many different home submarkets and spatial heterogeneity is still present. However, several control groups have been tested based on different criteria (propensity scores according to socioeconomic factors, pre-2004 home price level, spatial proximity with EZ) leaving our results unchanged. This result might therefore come from a *stigmatizing* effect for the areas targeted by the local development programs. In the considered period – the mid-2000's before the 2007/2008 slowdown – home price growth rates have been tremendously high (above 8% on average) in the whole country and especially in areas with initially low price levels. Indeed, the *Seine-Saint-Denis* district experienced housing price growth rates largely above 10% and even 15% in some locations, due to a surge of housing demand from real estate investors and young households with too low income profile to become homeowners in Paris inner city, where prices were initially very high. Our contribution suggests that Round

[2]Notice nowadays that studies focusing on home prices or commercial real estate values with a comparable methodology generally found even more limited effect of local programs (Landers, 2006, Busso and Kline, 2007).

II EZ have been neglected by these owners/investors and have not been subject to an increased demand.

Second, we evidence an overall positive incidence of the distance to the closest EZ boundary on home price growth rates (though our P-splines estimates do not suggest a strictly monotonic relationship). Housing prices have not grown as much in the immediate vicinity than within an EZ. This is consistent with a negative spillover effect: some business creations might have occurred or some local public subsidies might have been granted to areas close to Round II EZ, had the latter not been implemented. Consequently, home prices rose even less in the EZ periphery than within it. Givord et al. (2011) obtained similar results regarding the spatial externalities effect on firm locations and local employment.

The rest of the paper is as follows. We provide a survey of the literature on empowerment zones in the US and in France in the second section. We focus on papers studying the local employment, firms location and housing prices effects. In the third section, we present the French three rounds EZ program illustrating the purpose and implementation of these measures in the *Seine-Saint-Denis* district. We give a complete description of our dataset in the fourth section. The fifth section provides our detailed econometric methodology: spatial matching process, control group selection, penalized spline estimation of the partially linear single index model and robust standard errors construction. The identification strategy is detailed. The sixth section presents the results: estimates of the model and numerical simulations of the impact of EZ. The last section concludes.

B Literature Review

B.1 US studies

There is a important body of literature dealing with the consequences of enterprise or empowerment zones on various outcomes such as business location decisions, corporate investment, unemployment or real estate values. Following the initiative of different US states in the early 1980's, some papers evaluated the impact of these state level programs and in general provided evidence of only modest results. For example, Papke (1994) analyses the outcome of the Indiana EZ program adopted in 1983 on investment and employment and reaches mixed conclusions: using annual data from 1981 to 1989 for 16 local jurisdictions and with a standard difference in differences (DID) econometric methodology, she find contrasting results on capital (about 8% increase in the value of inventories, but 13% drop in the value of machinery and equipment), but rather significant results regarding labor market (around 19% decline in unemployment claims). Boarnet and Bogart (1996) studies the consequences of the New Jersey urban EZ program using data at the municipal level from 1982 to 1990 on both municipal employment (in various sectors) and municipal property values. Relying on a standard DID panel data approach with municipal fixed effects and a time trend, but also dealing with the endogeneity of EZ selection (they instrument the Urban EZ with a preliminary probit regression run on applicants for the program), they fail to detect any positive effect of the local development tool on employment, nor on the real estate market. In line with Papke (1993, 1994) and with a similar methodology, Dowall (1996) suggests that California EZ programs had a positive average impact on local investment and employment levels using data from 1986 to 1990, but also detects substantial time and spatial heterogeneity with certain jurisdictions benefitting from almost no gains in capital and labor markets. More recently, Bondonio and Engberg (2000) simultaneously examine the results of various state-level EZ programs on local employment. They use panel data at the ZIP code level for five US states (California, Kentucky, New York, Pennsylvania and Virginia) from 1981 to 1994 and distinguish establishments according to four-digit SIC and employment class size. In line with Boarnet and Bogart (1996), they handle the bias due to the non random nature of the EZ selection process (with a similar propensity score approach). Their results suggest no statistically significant impact of the EZ programs on local job markets even for large monetary values of incentives or when incentives are specifically tied to employment level. Notice that the authors suggest that this low average effect could be due to a job reallocation process from non EZ to EZ with the same ZIP code. Moreover, they do find a significant positive employment effect of EZ in new establishments (this is offset by an opposite sign effect for preexisting establishments). This last result has been further confirmed by Bondonio and Greenbaum (2007). Elvery (2009) relies on census data from 1970 to 1990 and administrative records of establishment with employees from the mid's 1980 to assess the effects of California and Florida's EZ programs on resident employment. He proposes a three stage econometric setup with two preliminary probit models controlling for the tract fixed effects of employment probability and the probability for a zone to be treated. With a kernel matching method to control for unobserved heterogeneity, he finds no measurable improvement in local employment. Neumark and Kolko (2010) also estimate the impact of the California's EZ program on the local job market, but use much finer geographic mapping methods. Instead of working at the census tract or zip code level (which does not exactly follow EZ boundaries), they use a new establishement-level data and match it with street-by-street GIS. They fail to detect any significant employment effect of EZ, even for lower wage workers.

Some studies also specifically examine the housing or non residential real estate consequences of the state level EZ programs. For example, Engberg and Greenbaum (1999) estimate the impact of EZ on the local housing market in 22 US states using data for $4,107$ cities with population between $5,000$ and $50,000$ between 1980 and 1990. They provide regression estimates of EZ impact for three dependent variables: the growth rates of housing values, monthly rents and vacancy rates. They detect no average effect of EZ on housing values, but find evidence of varying impact of these program according to the initial tightness of local housing markets: EZ has a larger impact on housing values in places with low vacancy rates. Landers (2006) studies the impact of the Ohio Urban Jobs and Enterprise Zone program on commercial and industrial property values. He estimates a hedonic price model with individual real estate data to control for the structural attributes of commercial and industrial properties. With fixed effects (for neighborhood characteristics) and yearly dummies, the empirical results suggest that EZ may have a positive but moderate impact on property values as long as no EZ has been selected in other nearby locations.

The US federal Empowerment Zone program was implemented in 1994 and was much larger in terms of scale and purpose than the state level Enterprise Zones. It involved not only tax abatements to local businesses, but also tax credits focused on the employment of local residents and a set of grants from the federal government to states devoted to social services and local amenities. The main contribution that studied the consequences of the federal EZ program is the report of Hebert, Vidal, Mills, James and Gruenstein (2001), which finds large effects on job creation. However, their report has been criticized on several grounds (see Busso and Kline, 2007): lack of rigorous correction for the selectivity process in EZ designation, choice of census tracts which are likely to overstate the federal EZ program effects. Recently, Busso and Kline (2007) propose a large study on the impact of the federal EZ program on three outcomes (residential sorting behavior, local labor and housing markets) using informations from the Decennial Census (four decades), the county databook and HUD (US Housing and Urban Development) data. They work at the census tract level and propose a selection method to get an appropriate control group among the set of Round I rejected applicants (tracts) that became EZ at a later round and have similar Census characteristics than Round I selected zones. Relying on Difference in Differences method with a prior propensity score model and taking block-heteroskedasticity into account, they provide evidence of sizable positive labor market and moderate housing market effects on EZ compared to later round zones. They do not find substantial changes in the neighborhood demographic composition. Hanson (2008) also examines the consequences of the federal EZ program on employment and housing market. Instead of a propensity score, this contribution relies on an instrumental variable approach to control for the endogeneity bias due to the EZ selection process. Using census data at the tract level between 1990 and 2000, IV results evidence that the actual effects of the federal EZ program on employment and poverty reduction are not significant. Krupka and Noonan (2009) is the sole study on the impact of the federal EZ program that is specifically focused on neighborhood and housing outcomes. Their empirical model is quite different from the prior literature with a simultaneous autoregressive equation approach for housing prices (hedonic equation), housing stock and neighborhood demographic characteristics. They use block-group level data for decennial census 1980, 1990 and 2000 and check the EZ designation exogeneity. They distinguish the direct effect of EZ on housing values from the indirect one, related to changes in neighborhood. They find strong evidence supporting a positive direct effect of EZ program on housing prices, but a modest indirect effect.

B.2 Description of the French program and its outcomes

In 1997, the French authorities initiated the first Empowerment Zone program following the UK and US experiments. Three categories of EZ have been created with a growing amount of public aid: 750 Zones Urbaines Sensibles (ZUS, first tier), 416 Zones de Revitalisation Urbaines (ZRU second tier) selected among the ZUS and 44 Zones Franches Urbaines (ZFU, third tier) selected among the most distressed ZUS. The selection of ZFUs has been made according to several criteria (EZ population, unemployment rate, tax base of the whole municipality, share of people under 25 and share of unskilled workers) among ZRUs with more than $10,000$ inhabitants through a synthetic index.

First tier EZ (i.e., ZUS that are not ZRU or ZFU) may theoretically be exempt from local taxes, but this grant is at the discretion of local authorities and is not compensated by the central government. Moreover, local authorities (municipalities) with first tier EZ must establish a local housing program as well as promote urban revitalization projects. On the contrary, second and third tier EZs (ZRU and ZFU) benefit from substantial tax abatements for new and existing firms, but at higher levels for ZFU than for ZRU. In fact, only the ZFUs are actually comparable to the US EZ designated by the federal program. Firms with less than 50 employees are granted a five-year exemption from local corporate and income taxes when located in ZFU (against only one year for ZRU). They also benefit from large reliefs in their social security contribution (wage tax). The grants were supposed to progressively vanish (between 3 and 9 years) after the first five years of exemption, but ultimately all these deadlines have been extended by the authorities. These aids are partly contingent on the residence location of employees: there is a hiring condition (starting from the third employee) to recruit among the inhabitants of the EZ. Hence, this measure may create a boundary effect in the way property values are capitalized between properties within and beyond the EZ.

Notice also that households wishing to become homeowners in ZFUs will benefit under certain conditions from substantial increase in the maximum amount of public zero-interest loan (*Prêt à Taux Zéro*, PTZ) they are entitled to. This increased public support for homeownership is also granted in first tier zones. However, we might expect their impact on housing demand and then housing values to be higher in ZFUs through a possible cumulated effect with the larger employability for households located in ZFUs coming from tax abatements for local hirings.

Moreover, we can expect that local authorities with a ZFU might be less reluctant in granting aids for social programs or designing urban revitalization schemes than those with only a ZUS or a ZRU in their administrative area. Hence, we suspect that even though central government aids for ZFUs are mainly focused on business creation and local employment (which can indirectly affect the housing market), they may also impact local urban projects (housing program, local amenities) through enlarged incentives to conduct such projects for local authorities which benefit from this support. All in all, the effects of ZFUs creation compared to ZUS or ZRU may be much broader that the strict labor market and these effects may extend to other outcomes. Thus, real estate values is an ideal tool for assessing the effect of local ZFUs, since the wealth created by these programs should be capitalized in the local housing stock.

The second Round EZ program was launched in 2004 and 41 new ZFUs were created (exclusively selected among the existing ZRUs). The geographical distribution of EZs in this Round II natural experiment is quite different from that of the first round, with many new ZFUs in the Paris Region area. In particular, the *Seine-Saint-Denis* département (the most impoverished district in the Paris Region) includes 8 ZFUs against only two before Round II. We focus on this district, as such a density of third tier EZs on a small geographic area is an exception in the metropolitan territory (see the data description section). Finally, the third round EZ plan was launched in 2006 with the creation of 15 new ZFUs (still among the existing ZRUs) and in particular two new ZFUs in the Seine-Saint-Denis (SSD hereafter) district. These two third-tier EZs will serve as one possible control group in our econometric setup (see methodology section).

Many descriptive studies have been published on the evaluation of different rounds of the French EZ program with contrasted conclusions (see Thélot, 2004, Gilli, 2006, or Ernst, 2008). The two most significant statistical contributions are Gobillon, Magnac and Selod (2010) and Givord, Rathelot and Sillard (2011). Gobillon et al. (2010) proposes an assessment of the effects of the first round French EZ program. They use a two-stage procedure to estimate the impact of EZs on individual unemployment durations. First, they estimate a stratified partial likelihood estimator with a panel dataset at the municipality level from 1993 to 2003. They obtain average municipal effects and study of these effects have changed over time. Comparing these results for treated municipalities (those including an EZ) and counterfactual municipalities with similar characteristics, they provide evidence of moderate but significant effects on the French program in the short run (i.e. for the semesters immediately following the policy implementation). No significant effect is detected at larger horizons. Givord et al. (2011) evaluate the consequences of the Round II French EZ program on economic activity: business creations and employment. They use establishment level data from 2002 to 2007 with a geographic mapping methodology to get the exact establishment street address. They investigate the impact of the program on treated areas (firms located with EZs boundaries) with a two stage method (propensity score to control the probability for an area to become a ZFU, plus a kernel matching method) and find significant effects of the program on both business creation and employment. Notice that they evidence negative spatial spillover effects on areas in the vicinity of EZs.

C Data

Data on sales prices and volumes in the eight administrative units[3] of the Paris Region (or *Île-de-France*) come from the Notaries Chamber of Paris. In France, all property sales have to be registered by a Notary who collects the fees to be paid to the Inland Revenue. We restrict our sample to *second-hand flat* transactions. New properties and house sales both represent only a small share of the total sales for the Paris Region and their price and physical attributes strongly differ from those of the second-hand flats. We collect individual information on the sale price, the floor area of the flat, the floor level, the construction period of the building, the presence of an elevator in the building, the number of rooms, of bathrooms, of garages, etc. We also have the precise location (street address) of each housing unit. This will be of crucial importance for the geographic mapping procedure of the data (i.e. distinction of dwellings inside or beyond the EZ's boundaries and distance to the closest EZ boundary). Data are available for the [1996-2007] time period. The month of transaction is recorded.

As previously explained, we only consider sales occurring in (some areas of) the Seine-Saint-Denis district, one of the eight Paris Region administrative unit. This district which has approximately 1,5 millions inhabitants (out of 11 millions in the Paris Region) concentrates some of the largest social or economic problems in France: low employment rates, high job seekers rate, low average reported net income per household (19, 749 € per year in 2008 against 30, 198€ for the whole Paris Region). The Seine-Saint-Denis is the administrative unit with the highest share of people living in an EZ (20.5% in 2006 according to the French National Census, while only 11.1% in the Region and 6.8% for the metropolitan territory). Moreover, according to Musiedlak (2011), EZs (either first, second or third tier) located in the Seine-Saint-Denis concentrate on average most of the social insecurity (in terms of job/income, but also share of social housing, educational attainment, supply of medical or hospital services, etc.) of the whole administrative unit.

[**Insert Figures 1 and 2**]

Figures 1 and 2 give a view of the spatial distribution of EZs (first tier, ZUS, and third tier, ZFU, respectively) in the Paris inner suburbs. There are 36 ZUS and 10 ZFU in the Seine-Saint-Denis administrative unit. Interestingly, only two ZFUs in this district had been selected in the first Round (i.e. in 1997), while 6 of them are Round II ZFUs (i.e. ZRUs converted into ZFUs in 2004). The last two ZFUs have been implemented in 2006. Hence, this Round II

[3] The eigth units are Paris, Hauts-de-Seine (92), Seine-Saint-Denis (93), Val-de-Marne (94), Essone (91), Yvelines (78), Seine et Marne (77) and Val d'Oise (95).

program is an interesting natural experiment to assess the local consequences of EZ programs. The geographical density of ZFUs enable us to gather many housing sales occurring within or in the neighborhood of these local development areas. We proceed to this geographical mapping using the exact geocoding coordinates of edges of the segments that make up the boundaries of these zones (using data made available by the *IGN*, the French Geographic Institute). For example, we may distinguish two nearby transactions located on both sides of a street corresponding to an EZ's border. Table 1 below gives the number of flat sales observations within a 500 meters radius of a Round II ZFU per transaction year[4].

Table 1: Nb observations within or in the neighborhood of a Round II EZ. Seine-Saint-Denis district.

Year	Within EZ	<50 meters	$[50-100]$ m.	$[100-200]$ m.	$[200-500]$ m.
2000	108	17	16	47	221
2001	91	36	15	53	227
2002	111	28	19	47	217
2003	111	39	19	48	244
2004	111	39	13	106	353
2005	142	37	21	88	360
2006	149	41	23	67	378
2007	135	39	28	80	369
Total	958	276	154	536	$2,369$

According to Givord et al. (2011), while Round I ZFUs in 1997 had been selected among the most impoverished areas (based on the synthetic index described in the previous section), the Round II selection process of new ZFUs seems to have been largely grounded on two criteria: the metropolitan area where the EZ is located and its size. This may explain why so many new ZFUs have been chosen in the Seine-Saint-Denis district (among the most populated former ZRUs since the lower limit population size for a Round II ZFUs is $10,000$ inhabitants). On the contrary, the synthetic index factors (fiscal potential of the city, proportion of youth, local unemployment, etc.) do not seem to have played a significant role in the selection process. This has been evidenced by Givord et al. (2011) with a logit estimation for the whole French territory. We collected these factors for the Seine-Saint-Denis and did not detect significant average differences in one of these criteria between new Round II ZFUs and non selected ZRUs in 2004.

[4]Notice that the newly selected ZFUs in Round II may have been geographically extended compared to their former ZRUs sptial delimitation. This extra territory is mainly intended to the creation of new businesses. Hence, the number of housing sales in this supplemetary land is extremely low. We nevertheless use the enlarged delimitations of ZFUs in Table 1 as well as in forthcoming estimates (a sensitivity analysis with the restricted Round I boundaries will be conducted).

Consequently, it may be possible that some of the most deprived ZRUs in the Seine-Saint-Denis administrative unit have not been selected for specific tax cuts in 2004. For example, if we compute the average flat price per s.m. for each Round II ZFUs in 2004 and compare it with its counterpart for the remaining ZRUs in the same administrative unit. As expected, we detect that home prices are on average higher in Round II ZFUs than in other ZRUs (6.21% gap), but also that the local average price level in more than 33% of the other ZRUs is above the global average for Round II ZFUs. This suggests that suitable control groups for the EZ program evaluation effects might be selected among the set of non selected ZRUs. The underlying process governing the ZFUs selection does not seem to depend on index factors, nor on the initial housing price level. Endogeneity in ZFUs selection may not be a major issue here. We propose the following control group selection scheme: we first choose all (non-selected in Round II) ZRUs. These areas are on average more populated and more likely to be selected to become ZFUs than first tier EZs (ZUS). We also include Round III ZFUs (i.e. first or second tier zones that became ZFUs in 2006). We obtain a set of 8 EZs. We then discard 2 of these EZs : the first one located in Stains since it is much smaller in size than the others (around $2,500$ inhabitants only) and the second one in Neuilly-sur-Marne (which became a Round III ZFU in 2006) since the 2001 flat price level is largely above other ZRUs (see also Jacquesson 2006 and Musiedlak, 2001, which propose a classification analysis suggesting that this EZ was substantially wealthier than the other ZRUs or ZFUs). We end up with 6 *control* EZs which can be compared to the 6 Round II (*treated*) ZFUs. Initial price levels are close (1.54% higher in the treated zones on average) and the average population size for the control group is $6,627$: most of the ZRUs could theoretically have been spatially extended to meet the $10,000$ inhabitants level for ZFUs as has been done for Round II selected zones. Remaining differences (in population size or home price levels) will be controlled for in the regression procedure. Figure 3 below is a map of Seine-Saint-Denis EZs (all tiers) with color distinctions for the reference group (treated Round II ZFUs), the control group ZRUs, discarded ZRUs and other EZs (mostly ZUS).

[**Insert Figure 3**]

D Methodology

Let $p_{i,c,t}$ be the log price of individual housing unit i, within (or in the vicinity of) EZ c, at time t. We suppose that this outcome is generated by a "seemingly-hedonic" semiparametric model of the form:

$$p_{i,c,t} = X'_{i,c,t}\beta + \mu\left(T_c, I_{i,c}, D_{i,c}, Z_{i,c,t}\right) + \theta_{c,i} + \eta_{c,t} + \varepsilon_{i,c,t} \tag{D.1}$$

where T_c is a treatment dummy variable (indicating whether or not EZ's c has been treated at Round II program in 2004). Hence, $T_c = 1$ is for the observations in the treated group and $T_c = 0$ in the control group. $I_{i,c}$ is a dummy variable indicating if housing unit i is located within (treated or control) EZ's c and $D_{i,c}$ is the geographic distance between housing unit i and the closest EZ border ($D_{i,c} = 0$ if $I_{i,c} = 1$). $X_{i,c,t}$ and $Z_{i,c,t}$ both are vector containing informations on the housing unit structural attributes, EZ's characteristics and housing market trends. We will further discuss which variables should be linearly modelled (i.e. included in X vector as done in the traditional housing price hedonic models) and which ones should be included as arguments of the unspecified function $\mu(.)$. β is the hedonic parameter vector. $\theta_{c,i}$ is a spatial location fixed effect. It captures unobserved local neighborhood characteristics that may potentially be correlated with the treatment variable T_c or the variables summarizing the distance to the nearest EZ ($I_{i,c}$ and $D_{i,c}$). $\eta_{c,t}$ is a term modelling the heterogeneity in time dynamics across different EZs. Indeed, we might expect that even though our focus is on a geographically-restricted area, some local housing submarkets with different time trends may coexist. In particular, some local markets (including an EZ) may have reacted differently to the Round II program implementation depending on their initial (before the EZ program implementation) average home price level or some factors included in the synthetic index (unemployment rate, fiscal potential of the city). Some of these EZ level variables will be included in the $\eta_{c,t}$ term. $\varepsilon_{i,c,t}$ is the *iid* error term. Notice that at this stage, model (D.1) is based on the assumption that there does not exist any interaction between $\theta_{c,i}$ and $\eta_{c,t}$. However, it might be the case that the value of some housing units may respond differently to the treatment depending on their specific attributes or location. In particular, the spatial homogeneity of housing prices reactions within a treated EZ following the program implementation is questionable. Some housing units might have a larger relative change in their value than other in the same zone depending on local amenities, i.e. $\theta_{c,i}$ could be time-varying. This issue is discussed in the sensitivity analysis.

Model (D.1) is a partially linear semiparametric model that encompasses the standard linear hedonic setup. The linear part $X'_{i,c,t}\beta$ corresponds to the usual hedonic specification. The link function $\mu(.)$ is not specified *ex ante*. This semiparametric form allows us to let the impact of some home attributes $Z_{i,c,t}$, or the distance to the nearest EZ $[I_{i,c}, D_{i,c}]$ and their interaction with the treatment variable T_c unspecified. This setup permits a large flexibility in the modelling of the impact of these variables on home prices. For example, we expect home prices to be positively related to $D_{i,c}$, but this relation may depend on some flat's attributes, its size for example: the immediate proximity of an EZ may influence price differently for large family dwellings (households with children) than for small flats. We then include the dwelling's floor area variable among $Z_{i,c,t}$ instead of $X_{i,c,t}$. Moreover, we do not know how the distance gradient may evolve after the new policy implementation: consequently, the interaction between T_c and $D_{i,c}$ should not be specified ex ante (both are arguments of $\mu(.)$).

The estimation procedure is as follows. We first collect all pre-reform housing sales $p_{i,c,t}$ ($t < 2004$). Let

n denote the total number of such observations. To control for individual and local unobserved heterogeneity, each reference sale i is spatially matched with a fixed number ($\overline{n}$) of post-reform housing sales $\overline{p}_{j,i,c,t'}$ ($t' \geq 2004$, $j = 1, ..., \overline{n}$). Different values for the number of nearest neighbors will be tested. At this stage, the matching procedure is done according to the sole spatial criterion (i.e. we gather the $\overline{n}$ post-reform sales nearest to the pre-reform reference point i – $\overline{n}$ nearest neighbors approach[5]). We then compute the housing price differential between transaction i and the counterfactual transactions j, i.e. $\triangle p_{i,j,c,t,t'} = \overline{p}_{j,c,t'} - p_{i,c,t}$. All these $n \times \overline{n}$ observations are pooled and weighted according to the distance between the focal and counterfactual sales. Let $w_{i,j}$ denote the weight between sales i and j. $\overline{n}^{-1} \sum_{j=1,...,\overline{n}} w_{i,j} = 1$ for each i. In our framework, each pair (i, j) shares the same fixed unobserved effect $\theta_{c,i}$. This is reasonable if the distance between i and j remains low. Accordingly, the housing price differential becomes

$$\triangle p_{i,j,c,t,t'} = \left[\left(X'_{j,c,t'} - X'_{i,c,t}\right)\beta + \left(\eta_{c,t'} - \eta_{c,t}\right)\right] + \left[\mu\left(T_c, I_{j,c}, D_{j,c}, Z_{j,c,t'}\right) - \mu\left(T_c, I_{i,c}, D_{i,c}, Z_{i,c,t}\right)\right] + \left(\varepsilon_{j,c,t'} - \varepsilon_{i,c,t}\right) \tag{D.2}$$

where we take advantage of the fact that $I_{j,c} = I_{i,c}$ (if the focal point is within an EZ's boundaries, the spatial matching procedure is done inside the EZ, no counterfactual sale is selected beyond the EZ boundaries and similarly if the focal point is outside the EZ) and $D_{j,c} - D_{i,c} \approx 0$ to reformulate the nonparametric part of the model (second term in bracket in the RHS of equation (D.2))

$$\overline{\mu}\left(T_c, I_{i,c}, D_{i,c}, Z_{i,c,t}, Z_{j,c,t'}\right) \equiv \mu\left(T_c, I_{j,c}, D_{j,c}, Z_{j,c,t'}\right) - \mu\left(T_c, I_{i,c}, D_{i,c}, Z_{i,c,t}\right) \tag{D.3}$$

Such a general setup is subject to the curse of dimensionality due to the large number of covariates (arguments of function $\overline{\mu}$). We limit our approach to a partially linear *single-index.* The nonlinear link function is then defined as $\eta\left(\gamma Y_{i,j,c,t,t'}\right) \equiv \overline{\mu}\left(T_c, I_{i,c}, D_{i,c}, Z_{i,c,t}, Z_{j,c,t'}\right)$ where $Y_{i,j,c,t,t'} = \left(T_c, I_{i,c}, D_{i,c}, Z_{i,c,t}, Z_{j,c,t'}\right)$ and γ is a parameter vector. This single-index assumption implies a lesser degree of generality since all interactions of elements of Y are captured by the eventual non-linearity in the univariate function $\eta(.)$, but it may still capture important features of our high-dimensional problem and will greatly facilitate the numerical estimation procedure. Application of partially linear single-index model may be found in various fields, including housing economics.(see for example, Wang et al. (2007) which propose an application of this model to the case of Boston Housing Prices in a different setup).

We implicitly assume that conditionally on the covariates, the growth in housing prices is not related to the treatment variable, $\triangle p_{i,j,c,t,t'} \perp T_c \mid (X, Z, \eta, I, D)$. This assumption implies in particular that no unobserved EZ-level variable potentially influencing the probability an EZ has been selected in Round II might be related to the housing prices growth rates. Though no rigorous qualitative variable estimation procedure has been conducted to determine the covariates affecting the probability an EZ is treated (the number of EZs in the Seine-Saint-Denis administrative

[5] In the sensitivity analysis, the set of matching variables is enlarged and includes home structural attributes.

unit is much too low[6]), such a regression delivered poor results on the whole French territory (see Givord et al., 2011) with almost all covariates being non significant. As explained in the data section, these factors play no role in our administrative unit either. We are confident that conditionally on some key factors included in $\eta_{c,t}$ (the initial average price level in each EZ notably), our exogeneity assumption is reasonable.

Following Yu and Ruppert (2002), we model $\eta(.)$ by a penalized spline (P-spline hereafter). As detailed by Ruppert and Carroll (1997) or Ruppert (2002), P-splines are a generalization of smoothing splines allowing more flexibility in the choice of knots and penalty parameter. Moreover, according to other methods with different link function specifications, P-splines allow for easier and faster regressions procedures. For example, we have also estimated our setup with local linear approximations and minimum average variance estimation (MAVE, see Xia and Härdle, 2006), but convergence was much more difficult to obtain. The P-spline function for η is

$$\eta(a) = \delta_0 + \delta_1 a + ... + \delta_p a^p + \sum_{k=1}^{K} \delta_{p+k} (a - \kappa_k)_+^p \tag{D.4}$$

where $\{\kappa_k\}_{k=1}^{K}$ are the spline knots. They capture the jumps in the p^{th} degree of the polynomial. $\delta = \{\delta_0, \delta_1, ..., \delta_{p+K}\}$. The choice of the polynomial degree p is discussed in the results section. Following Yu and Ruppert (2002), the estimation is done with several number of knots K,($K = 3,\ 5, 10$, more than 10 knots is above the usual practice in applications). The knots are equally spaced along the quantiles of the index, $\gamma Y_{i,j,c,t,t'}$. We adopt the standard penalized and weighted least squares estimators of β, γ and δ which minimizes

$$P_{n,\overline{n},w,\lambda}(\beta,\gamma,\delta) = (n\overline{n})^{-1} \sum_{i=1}^{n} \sum_{j=1}^{\overline{n}} \left\{ w_{i,j} \left[\left(\overline{p}_{j,c,t'} - p_{i,c,t}\right) - \left(X'_{j,c,t'} - X'_{i,c,t}\right)\beta - \left(\eta_{c,t'} - \eta_{c,t}\right) - \eta\left(\gamma Y_{i,j,c,t,t'}\right) \right] \right\}^2 + \lambda \delta' D \delta \tag{D.5}$$

where D is a positive semidefinite symmetric matrix. We select D equal to a diagonal matrix with its last K diagonal elements equal to 1 and the rest is zero (Ruppert and Carroll (1997)). We penalize the jumps in the P-spline. $\lambda \geq 0$ is the penalty parameter. Its value is chosen by a standard generalized cross-validation (GCV) selection process which is simultaneously run with the nonlinear least squares estimation algorithm. The complete procedure (estimation + GCV) is fully described in Yu and Ruppert (2002). After selecting the size of the nearest-neighbors kernel ($\overline{n}$), the polynomial degree (p), the number of knots (K), we obtain initial estimates for β and γ with OLS on a linear (fully parametric) version of the model. A grid of values for the penalty parameter (λ) is created and $P_{n\overline{n},w,\lambda}(.)$ is first minimized over δ (with OLS estimates of β and γ) and then over (β, δ, γ) for each value of λ. A GCV score is computed for the selection of λ (see equation 21 in Yu and Ruppert, 2002) and the estimation outcome with this value is finally chosen.

We may then estimate the effect Average Treatment for the Treated (ATT), i.e. we compare the growth

[6]For this same reason, we cannot proceed to a spatial matching procedure of treated and control EZs.

in log prices $\Delta p_{i,c,t,t'}(X_{i,c,t}, X_{i,c,t'}, Y_{i,i,c,t,t'})$ between two given dates $t < 2004$ and $t' \geq 2004$ for a treated and a control group zones. We choose a given housing unit i with specified characteristics $X_{i,c,t}$ and $Z_{i,c,t}$. This unit is in a treated area $T_c = 1$, $I_{i,c} = 1$ and $D_{i,c} = 0$. We simulate $\Delta p_{i,c,t,t'}(X_{i,c,t}, X_{i,c,t'}, 1, 1, 0, Z_{i,c,t}, Z_{i,c,t'})$ choosing a post-reform counterfactual j with the same characteristics $X_{j,c,t'} = X_{i,c,t'}$ and $Z_{j,c,t'} = Z_{i,c,t'}$. This estimation is compared with the growth rate in value of a similar unit (same structural attributes), but located in a control EZ: $\Delta p_{i,c,t,t'}(X_{i,c,t}, X_{i,c,t'}, 0, 1, 0, Z_{i,c,t}, Z_{i,c,t'})$ which is used as a proxy of prices dynamics in treated areas, had they not been treated. The gap between these two growth rates is an evaluation of the ATT. We can reproduce this exercise for other combinations of values of T_c, I_c and $D_{i,c}$. In particular, we can compute the difference in home price growth rates for $I_c = 0$ and for a given value of $D_{i,c} > 0$ to measure spatial spillover effects.

E Results

We present here the results of our P-spline partially linear single-index model. The chosen sample period is [2002-2005], i.e. we consider housing sales two years before Round II implementation (years 2002 and 2003) and two years after (years 2004 and 2005). We select the control group as explained in the data presentation section: second-tier EZ (ZRUs) than have not been converted into ZFUs in 2004, but could have been according to their size (i.e. yellow zones in Figure 3). As above mentioned, other criteria such as local job seeker's rate, home price level, fiscal potential of the city, etc. do not seem to significantly impact the probability a zone was selected in Round II. However, such local factors may still influence the home prices growth rates and we will include some of them in our estimation equation (D.5) among vector $\eta_{c,t}$. Moreover, a robustness analysis with other control groups will be conducted in the sensitivity analysis section.

We pool all pre-reform sales observations within or close to (within 500 meters radius to the nearest EZ) treated areas (ZFUs, i.e. red zones in Figure 3) and within or close to control group zones. We end up with $1,520$ pre-reform observations: 222 within treated zones, 661 in the vicinity of treated zones, 155 within control zones and 482 in the vicinity of control zones.

We set $p = 1$ (first degree P-spline polynomial part). This choice is usual in the literature, implying that each knot corresponds to a jump in the first order of the P-spline. We set $\overline{n} = 5$ (results are qualitatively similar when $\overline{n} = 3$ or 10). Each pre-reform sales is then spatially matched with 5 post-reform counterfactual sales. The latter are selected among the $2,500$ observed post-reform sales (253 within treated EZs, $1,017$ in the vicinity of treated EZs, 244 within control zones and 986 in the vicinity of control zones). Hence, the whole (differences in the log of housing prices, Δp, between pre and post-reform periods) sample size is $7,600$ with our benchmark control group choice.

Among vector $X_{i,c,t}$, we include a large set of dwelling's structural attributes : the floor level and presence of an elevator (five dummy variables: first floor, second floor, third floor, fourth or above floor with an elevator and fourth or above floor with no elevator – ground floor is the reference), the number of rooms (four dummy variables: two rooms, three rooms, four room, five or above rooms – studio flat is the reference), the presence of a garage (one or more garage, no garage is the reference), of a bathroom (the reference is no bathroom – i.e. shower), the period of construction of the building (three dummy variables : construction year $\in [1948, 1969]$, $[1970 - 1980]$ or ≥ 1981 – < 1948 is the reference). The year of transaction is also recorded: since we compare pre and post-reform transactions, we have to choose two reference years (we arbitrarily select year 2002 for the pre reform period and year 2005 for the post-reform period). We also include the sale's quarter.

We include some EZ-level factors in $\eta_{c,t}$. First, we estimate the initial (i.e. year 2001) average home price level $\log\left(\overline{p}_{c,2001}\right)$ for each (treated or control) zone[7]. This variable is likely to affect the local prices growth rate: $[2002, 2005]$ has been a rapidly growing period for home prices in the whole Paris metropolitan area and it is now well-established that price's trends have been steeper in local areas with initially lower price levels. Hence, we have to control for this potential effect in our geographically-restricted sample. Our final estimates enable us to compare prices' growth rates of treated and control zones with the same initial average home price levels[8]. We also considered other factors such as the log of total population of each zone (as used for the calculation of the synthetic index), the fiscal potential of the city (or cities), the local unemployment rate, etc. but most of these covariates do not appear to have a significant impact on the local prices' growth rates.

At this stage, the only covariate included in $Z_{i,c,t}$ (i.e. in the index) is the log of floor area (in square metre). This continuous[9] variable allow us to capture interactions between the size of the dwelling and the treatment effect. Finally, the list of variables used to constitute the index are: T_c, the treatment dummy, $I_{i,c}$ the within-EZ dummy, $D_{i,c}$ the distance (in hundred meters) to the nearest EZ boundary, $Z_{i,c,t}$ the log of floor area of the pre-reform reference housing sale i and $Z_{j,c,t'}$ the log of floor area of post-reform counterfactual housing sale j.

After some preliminary estimates (obtained with standard parametric methods) to assess the relationship be-

[7] We run standard hedonic estimates of the log of flat prices for transaction year 2001. We then collect the residuals and compute their average *per* zone.

[8] Notice that the initial price $\log(\overline{p}_{c,2001})$ will be included directly in level in the difference equation (D.5), i.e. it is a component of vector $\tau_{c,t,t'} = (\eta_{c,t'} - \eta_{c,t})$.

[9] The other *discrete* variables $X_{i,c,t}$ summarizing the structural attributes of the dwelling are kept out of the index to prevent from poor identifiability issues in small samples.

tween the endogenous variable – the difference in the log of housing prices $\Delta p_{i,c,t,t'}$ – and the univariate index $\delta Y_{i,j,c,t,t'}$, we select the number of knots $K = 5$. As suggested by Ruppert and Carroll (1997), the total number of knots should not exceed 10. In our case, additional knots do not appear to capture significant new locally-linear trends in the relationship between the endogenous variable (controlling for the linear hedonic term) and the index.

Table 2 gives estimates of model (D.2). Results are reported for the three different sets of parameters: β, δ and γ. We emphasize that the estimates of the index parameter vector γ should not be directly quantitatively interpreted since they depend on the link function $\eta\left(.\right)$ and its parameter estimates $\widehat{\delta}$. The optimal value for the penalty parameter λ is obtained with the generalized cross-validation procedure described in the methodology section. Nevertheless, our spatial matching procedure is likely to generate biased inference through serial correlation in residual differences $(\varepsilon_{j,c,t'} - \varepsilon_{i,c,t})$. The same counterfactual unit may be matched with several pre-reform reference sales. Consequently, we proceed to block-bootstrap (at the EZ level distinguishing block of sales within an EZ and those in the vicinity of an EZ) to account for the possible bias in standard errors.

We first comment the parameter estimates for the standard hedonic part of the model, i.e. $X\beta + \eta$. We detect no significant impact of the floor level of the dwelling on the price level in our sample[10], except for dwellings above the third floor with no elevator which price is significantly below other types of flats. This result somewhat differs from those obtained in other part of the Paris metropolitan area (and particularly Paris itself) where ground floor or first floor level flats are substantially cheaper than high-floor level flats. The number of rooms of the unit has a significant impact on its value: three-rooms flats being more expensive than others for a given floor area (i.e. controlling for the number of square metre of the unit). As expected, units with a garage or a bathroom are significantly more expensive than those with respectively no garage or with a shower only. The construction period of the building seems to affect dwelling values: buildings built during the $70's$ being less expensive than old buildings while recent buildings (in the $80's$ or the $90's$) being more expensive. In line with the rest of the Paris metropolitan area, home price levels have heavily risen in our sample. *Ceteris Paribus*, the price level is approximately 12% higher in 2003 than in 2002 and 18% lower in 2004 than in 2005. Pre and post-reforms comparisons in home price levels will be made when detailing the results of the index estimation. Quarter dummies estimates show that price are on average steadily rising during a transaction year.

[10]Notice that since the $X\beta$ terms are included in difference in equation (D.5), their impact on the price should be interpreted in level.

Table 2: Parameters Estimates – $[2002, 2005]$ period

Hedonic Terms: $X\beta + \eta$	Estimates	s.e.
Level 1 (ref = ground level)	0.0065	0.0088
Level 2	0.0027	0.0087
Level 3	0.0075	0.0094
Level 4 with elev.	0.0006	0.0084
Level 4 no elev	-0.0701^{*}	0.0386
2 rooms (ref = studio)	0.0302^{**}	0.0133
3 rooms	0.0846^{**}	0.0174
4 rooms	0.0583^{**}	0.0206
5+ rooms	0.0599^{**}	0.0254
garage (ref = no)	0.0845^{**}	0.0081
bathroom (ref = no)	0.0670^{**}	0.0091
constr. $\in [1948, 1969]$ (ref<1948)	-0.0026	0.0091
constr. $\in [1970, 1980]$	-0.0325^{**}	0.0125
constr. > 1981	0.0458^{*}	0.0256
2^{nd} quarter (ref = 1^{st})	0.0584^{**}	0.0068
3^{rd} quarter	0.1056^{**}	0.0072
4^{th} quarter	0.1264^{**}	0.0074
pre-reform year 2003 (ref = 2002)	0.1190^{**}	0.0071
pre-reform year 2004 (ref = 2005)	-0.1780^{**}	0.0075
log (population)	-0.0281	0.0168
$\overline{p}_{c,2001}$	0.0433^{**}	0.0218

Index parameters: γ	Estimates	s.e.
T_c	0.0175	0.0388
$I_{i,c}$	0.2306^{**}	0.0501
$D_{i,c}$	0.0151^{*}	0.0085
$T_c \times I_{i,c}$	-0.2140^{**}	0.0544
$T_c \times D_{i,c}$	0.0075	0.0114
$Z_{i,c,t}$ (log floor area reference)	0.5384^{**}	0.0486
$Z_{j,c,t'}$ (log floor area counterfactual)	-0.7795^{**}	0.0482

Table 2 (continued): Parameters Estimates

P-spline parameters: δ	Estimates	s.e.
δ_0	0.9577^{**}	0.0225
δ_1	0.2738^{**}	0.0138
δ_2 (jump)	0.0416^{**}	0.0176
δ_3 (jump)	0.0297^{*}	0.0168
δ_4 (jump)	0.0085	0.0164
δ_5 (jump)	0.0157	0.0165
δ_6 (jump)	-0.0451^{**}	0.0179

$\widehat{\lambda} = 0.0025$, $N = 7,600$. ** = 5% signif, * = 10% signif

standard errors are calculated with block-bootstrap procedures

The log of total population does not seem to significantly impact home price growth rates, but the initial average price level in the EZ does. Thus, the higher the price level in 2001, the higher their growth rate during the $[2002, 2005]$. This stands at odds with well-established empirical results on the whole Paris Region housing market where areas with the lowest initial values have caught up on the most expensive areas.

We detect a significant *negative* impact of the treatment dummy variable T_c on the growth rate of housing prices between a post-reform reference year (here 2002) and a pre-reform reference year (2005). This conclusion comes from the combination of several factors: first, the sign of the first order parameter estimates of the P-spline is always strictly positive whatever the index value. For low values (below the 20% percentile), it equals 0.2738, culminates at 0.3693 and then decrease to 0.3242 for high values (above the 80% percentile of the index sample distribution). Second, the impact of covariate $(T_c \times I_{i,c})$ on the price growth rate between the pre and post-reform periods is significantly negative. Taking the direct (though very small and non significant) effect of T_c and the range of values of the P-spline parameters into account, our model suggests that home price growth rates between 2002 and 2005 have been on average approximately 6% lower for units located *within treated areas* than for units located within control areas. On a yearly basis, this means a nearly 2 percentage points lower growth within treated areas. This outcome stands after correcting for EZ-level differences between treated and non-treated areas and, in particular for similar initial home price levels (i.e. controlling for η_c). This is a non negligible result since the average annual price growth rate on the whole $[2002 - 2005]$ period is around 15%.[11]. Consequently, according to our estimates, the Round II local

[11] This figure is obtained by simulating the average growth rate for the whole sample (with both treated and non-treated EZs and units within or beyond EZs' boundaries).

development program implementation seem to have contributed negatively to the local value of the housing market, suggesting a stigmatizing effect on areas that became ZFUs in 2004.

We provide evidence that home prices growth rates are significantly higher within *non treated* EZs ($I_{i,c} > 0$ and $T_c = 0$) than in their immediate vicinity ($I_{i,c} = 0$, $T_c = 0$ and $D_{i,c} \simeq 0$), since the impact of $I_{i,c}$ on home price growth rate is positive. Home located in control EZs have benefited more from the mid-2000′s surge in demand than nearby units, since prices are lower and housing is more affordable. Such a pattern is consistent with what has been observed for most of the less expensive districts in Paris area: cheaper areas encountered larger increase in home values. However, this result is no longer true when considering *treated* zones: the positive effect $I_{i,c}$ is completely dampened by the negative effect of $(T_c \times I_{i,c})$. There is no substantial difference between value increases of housing units located within a treated zone and those located near the border of such a zone. Consequently, we do not detect any negative spatial spillover effect following Round II implementation (see Givord et al., 2011): some business creations might have occurred or some local public subsidies might have been granted to areas close to Round II EZ, had the latter not been implemented, but this does not seem to be a significant issue. The value of units near a treated zone did not grow less that near a control zone, either in absolute terms or relatively to within-EZ housing units.

Moreover, we evidence an overall positive incidence of the distance to the closest EZ boundary on home price growth rates (our P-splines estimates suggest a strictly monotonic relationship): $D_{i,c} > 0$. Notice that this effect is somewhat less pronounced for units located close to a control zones than for those close to a Round II ZFU since $(T_c \times D_{i,c})$ is slightly (but non significantly) positive. This suggests another possible stigmatizing effect for control or treated EZs. Owner-occupied housing demand or investment have been preferentially focussed on *remote* areas of implemented EZs. Price growth have been larger in areas of the Seine-Saint-Denis administrative unit with higher initial price levels (i.e. far from the EZ), even though the district as a whole (with below national average price levels) have been subject to a spatial shift in demand from households with a limited housing affordability in Paris.

All in all, our estimates suggest that the implementation of Round II ZFUs did not generate any positive effect for local housing units, either when comparing them to nearby units beyond ZFU's boundary or to housing units within EZs that have not been selected in Round II, but could have been. In the latter case, the final effect is actually significantly negative. Notice that the magnitude of these effects appear to interact with the size (floor area) of the dwelling due to the gap in parameter estimates for $Z_{i,c,t}$ (floor area of the pre-reform reference sale) and $Z_{j,c,t'}$ (floor area of the post-reform counterfactual sale). If we compare similar dwellings ($Z_{i,c,t} = Z_{j,c,t'}$), the larger its floor area, the lower the index value since the parameter estimates is lower for $Z_{i,c,t}$ in absolute value. This means -that the negative effect of the treatment $(T_c \times I_{i,c})$ is much more pronounced for small flats (corresponding to large index values – above the 60% percentile of the sample distribution) than for large flats. For example, simple calculations suggests that price growth rates are more than $7,1\%$ lower for 30 square meters housing units within treated zones compared to unit within control zones, instead of only $5,4\%$ for 120 square meters units. The stigmatizing effect is slightly less stringent for large family households which affordability has been particularly impacted by the overall rise in real estate values.

F Sensitivity analysis

We check the robustness of our results according to the control EZs selection process and to the chosen time period. More precisely, we reestimate model (D.5) with two other control groups : a) Round III ZFUs, i.e. the two second-tier EZs that became ZRUs in 2006 (i.e. the yellow area in Bobigny and the Green Zone in Neuilly-sur-Marne in Figure 3), b) all other EZs (i.e. including all blue areas in Figure 3). Indeed, our stigmatizing result may be the consequence of the lack of an appropriate control group, due to small-scale spatial heterogeneity in the Seine-Saint-Denis administrative unit. We also reproduce our estimation exercise on a restricted $[2003, 2004]$ time period, i.e. one year before policy implementation and one year after, to test for the temporal stability of our results. in this last case, we use the benchmark control group. Each case is compared to our benchmark through simulation of the difference in home prices growth rates between units located within a Round II ZFU and those located in a control area. We select a reference flat (above the fourth floor, with an elevator, 3 rooms with 60 square metre floor area, a garage, a bathroom, built between 1948 and 1969). We set η_c at its average value. All results are given in Table 3.

Table 3: Gap in price growth rates between home located in ZFUs and control EZs. Percentage points. Annualized

	Benchmark	Control = Round III ZFUs	Control = all other EZs	$[2003, 2004]$ time period
Gap	-2.0878^{**}	-2.7956^{**}	-2.4451^{**}	-3.1206^{*}
	(0.5928)	(0.9638)	(0.5653)	(1.6199)

** = 5% signif, * = 10% signif

Results are qualitatively robust to the control group specification: the stigmatizing effect seems to be even more pronounced (lower price growth rate for home located within a Round II ZFU compared to control zones) when considering Round III ZFUs or the whole set of EZs as controls. Moreover, our main result still holds when working on the $[2003, 2004]$ though not significant at the 5% due to the smaller sample size.

G Conclusion

In this paper, we illustrate with a new econometric methodology the consequences of the French EZ program on housing prices. We use large and detailed individual geo-coded datasets on the housing sales market to analyze this impact in an approach based on the comparison of housing price changes in areas upgraded in a second round of the French EZ program and in their vicinity with those observed in areas which had not been upgraded, taking into account the physical characteristics as well as the distance to the boundaries of the EZs. We cannot evidence a selectivity bias with respect to the variable under study. Our model suggests that home price growth rates between 2002 and 2005 have been on average approximately 6% lower for units located *within treated areas* than for units located within control areas. Home located in control EZs have benefited more from the mid-2000′s surge in demand than nearby units, since prices are lower and housing is more affordable, but this result is no longer true when considering *treated* zones. The negative effect of the treatment is larger for small flats The results can be read as an illustration of a stigmatizing effect. Owner-occupied housing demand or investment have been preferentially focussed on *remote* areas of implemented EZs.

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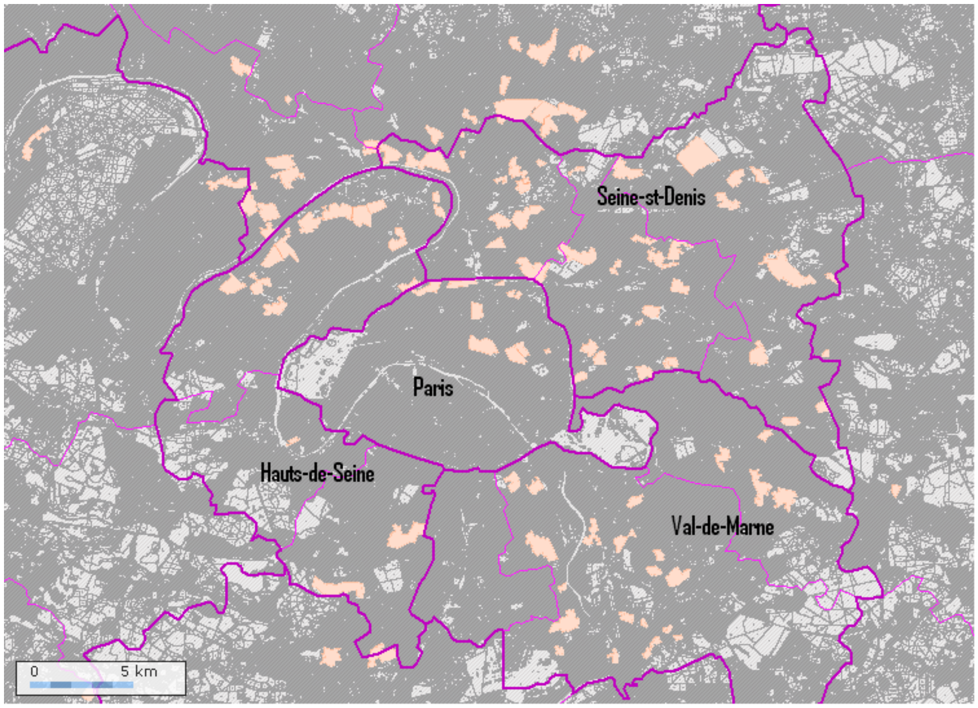

Figure 1: First Tier EZs (Zones Urbaines Sensibles) in Paris Inner Suburbs. The Seine-Saint-Denis district is located northeast of Paris.

H Tables and Figures

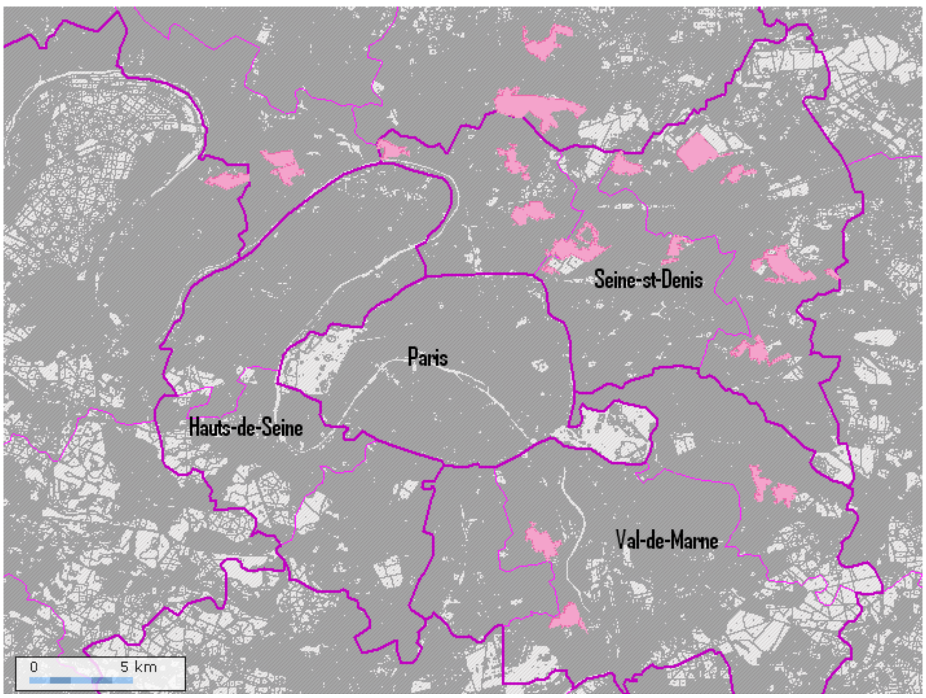

Figure 2: Third-Tier EZs (Zones Franches Urbaines, ZFU) of Paris inner suburbs.

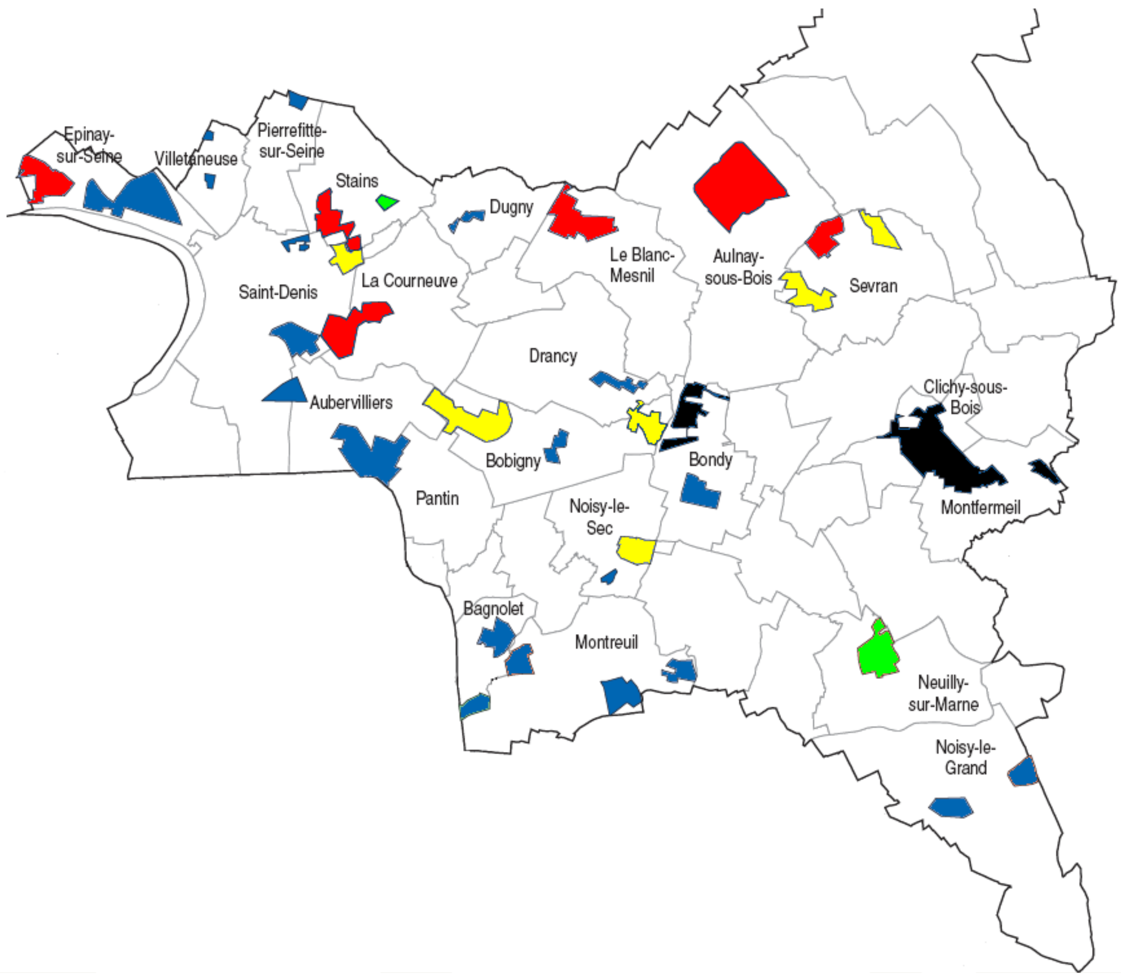

Figure 3: EZs in Seine-Saint-Denis. Red areas are Round II ZFUs (treated zones). Yellow areas is for control group ZRUs. Green areas is for discarded ZRUs. Black areas (Round I ZFUs) and Blue areas (other EZs) are not considered in the estimation.