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journal homepage: www.elsevier.com/locate/regecPlace-based tax exemptions and displacement effects: An evaluation of the *Zones Franches Urbaines* program[☆]Pauline Givord^{a,b}, Roland Rathelot^{b,*}, Patrick Sillard^{a,b}^a Institut National de Statistique et d'Etudes Economiques (INSEE)^b CREST, Bâtiment MK2 Bureau 2020, 15 Boulevard Gabriel Péri, 92245 Malakoff Cedex, France

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ABSTRACT

In this study, we evaluate the impact of the French *Zones Franches Urbaines* on economic activity. This public-funded place-based program, comparable to US enterprise zones, exempts businesses from taxes for a period of at least five years. For the purpose of this evaluation, we merged several administrative datasets at the company level. This allows us to exhaustively observe business creations and stocks, as well as employment and financial outcomes for companies at specific geographical locations. We focus on the second round of the program, during which treated territories were selected among a pool of deprived areas according to a known set of covariates. The way treatment was assigned makes the conditional independence assumption credible in our case. Overall, we find significant effects on both business creation and employment while the impact on companies that were located in the treated areas before the program is not significant, regardless of the outcome. We also provide evidence of significant negative spillovers of the program on neighboring areas.

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1. Introduction

The long-standing debate regarding appropriate remedies to the deteriorating conditions in urban areas with high levels of poverty and unemployment remains open. Policy-makers have tried many approaches to strive against unemployment in these areas. While, on a large scale, people-relocation policies are believed to have been unable to improve labor market conditions for the populations living in the poorest neighborhoods,¹ policy makers have tended to favor programs aiming at making these areas more attractive to businesses, and ultimately, improving both local employment and population

welfare. As these low-income neighborhoods cumulate deficiencies that could impact on the productivity of businesses, location-targeted tax incentives aim at displacing the optimal location decision of companies. If agglomeration effects play a prominent role on business location decisions (see for instance Combes et al., 2011), the level of local taxes can also modify these choices (Devereux and Griffith, 2003; Rathelot and Sillard, 2008; Duranton et al., 2011). The motive behind the first “Enterprise Zones”, first in the UK in the early eighties followed by the U.S., was to use this fiscal tool to attract new businesses in some targeted areas.

In this paper, we evaluate a French program known as the *Zones Franches Urbaines* (hereafter ZFU). This initiative aims at boosting business births and facilitating relocations,² as well as avoiding the exit of already settled establishments through placed-based tax exemptions. Being located within the limits of a ZFU allows a company to be totally exempt from business and corporate taxes, as well as social security contributions. Compared to similar programs in the U.S., the ZFU scheme is particularly generous, with a wide range of tax credits and almost no requirements other than location, although it is restricted to companies with a workforce of less than 50 employees

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¹ See e.g. Rosenbaum and Zuberi (2010) for an overview of the findings of the evaluations of Gautreaux and Moving-To-Opportunity programs.

² In this text, we distinguish business births (also called “pure creations”) from business relocations (also called “transfers”). The former refers to the creation *ex nihilo* of a business activity, the latter to the relocation of a business that was already existing somewhere else.

(plus a less stringent criterion relating to total sales). As smaller companies tend to be more financially constrained, they may be more responsive to tax incentives. This analysis could thus provide relevant elements, in particular regarding the optimal design of place-based tax-exemption programs.

This paper contributes to an already vast but still inconclusive literature on this topic. While several articles indicate that spatial mismatch may explain a large part of urban unemployment,³ no consensus emerges on the effectiveness of place-based tax exemptions to foster economic development in deprived areas. The major part of the empirical evaluation literature finds that US Enterprise Zones have little or no impact on economic activity and employment (Bondonio and Greenbaum, 2007; Neumark and Kolko, 2010), while Busso and Kline (2008) provide opposite evidence for federal-level Empowerment Zones.⁴ In recent contributions, Ham et al. (2011) re-examine all US programs for a wider set of outcomes and find more encouraging results for place-based policies. Krupka and Noonan (2009) show that the effect of these fiscal tools can be seen to some extent in housing prices, maybe by increasing local amenities (see also Landers, 2006). However, the ex-post evaluations of European place-based programs are scarce. Potter and Moore (2000) provide a positive assessment of the capability of the inwards investments created by the UK Enterprise Zones to generate local employment. Accetturo and de Blasio (2012) conclude that the Italian Area Contracts (*Patti Territoriali*) program was largely ineffective in fostering growth. Little evidence exists on the impact of the French program on economic activity.⁵ Besides, little is known so far on the issue of the externalities created by these place-based policies. They are often blamed in public debate for “cannibalizing” neighboring communities by causing a shift of economic activity within a city, from areas that do not benefit from the program to areas that do. It is obviously an important concern for public policy to know whether a revitalizing program spurs the development of one area at the expense of another.

As per Hellerstein and Neumark (2011), most existing evaluations of place-based programs face two kinds of issues. First, as the boundaries of treated zones do not necessarily coincide with administrative ones, a significant proportion of the area considered as treated in most studies is not actually treated. This measurement error is likely to bias downwards the estimator of the impact. Second, the issue of which areas are considered as a valid control group is of paramount importance. Because of the quality of the data we use and because of the selection process of the ZFU, our paper provides a contribution to the international debate on place-based tax-exemption policies. Our data make it possible to assess the impact of the program not only on treated areas but also on the untreated areas which are geographically close to the treated ones, providing evidence on the existence of spatial externalities created by such schemes.

We use exhaustive administrative panel establishment-level data with precise location during the period 2002–2007. For each establishment, its creation date, whether this creation corresponds to a birth or a relocation and its eventual date of bankruptcy are available. For each year, the number of employees, their wages, as well as several outcomes regarding the business' economic and financial health (income, sales, cash flow, debt, investment) are also measured. Therefore, we can

assess the impact of the program on business entry and exit rates, economic activity, employment, as well as on the financial strength of companies. We are also able to differentiate between businesses that settled before and after the implementation of the program. The decisive strength of our data is the access to the specific location of each establishment. This is particularly interesting as, like EZ in the U.S., ZFU borders do not coincide with the limits of pre-existing jurisdictions. We can therefore establish whether a given entity is located within or beyond the boundaries delimiting tax exemption.⁶

The implementation of the ZFU program occurred in three rounds, respectively in 1997, 2004 and 2006. In this paper, we focus on the second step as it benefits from several desirable features for evaluation. In 1997, when the government defined the 44 territories that were granted ZFU status for the first round, 416 other territories were also labeled *Zones de Redynamisation Urbaine* (ZRU). The difference between the two programs in terms of the respective generosity of available exemptions is large (see *infra* for details). The 44 first-round ZFUs were identified as the most distressed areas, while the 416 ZRUs were regarded as slightly less deprived. In 2004, 41 new ZFUs were created, all chosen within the pool of the ZRUs. Our main strategy consists in comparing outcomes between ZRUs that subsequently became ZFUs and ZRUs whose status did not change by propensity score matching. Such a strategy must inevitably deal with selection bias. As ZFUs are supposed to be more economically depressed than ZRUs, companies may naturally choose to locate less frequently in ZFUs, regardless of any tax incentives. This would induce a negative selection bias on the impact of the program.

In practice, two elements may argue in favor of our strategy. First, the most distressed neighborhoods, which might have been difficult to compare, were in principle turned into ZFUs in the first round of the program. Statistical evidence shows that first-round ZFUs are indeed more distressed than second-round ones. Second, the committee in charge of choosing which ZRUs would be granted ZFU status in 2004 had to follow precise guidelines: as determined by an index calculated using five local variables (total population, unemployment rate, tax revenues, proportion of youth, proportion of dropouts).⁷ As we were able to compute these variables and this index in particular, we found that the ZRUs chosen to become ZFUs proved to be quite similar to the areas that did not benefit from the policy. As the summary statistics and propensity score estimation make it clear, the most crucial factor that determined which areas were selected in the second round was their geographical location: an area had more chance to be chosen if it was further from a first-round ZFU and closer to another ZRU.⁸

We find that tax exemptions provided by the ZFU program had a positive impact on the number of businesses located in the treated areas. Such impact is mainly driven by an increase in the number of new companies, both through births and relocations. In relative terms, the increase is mainly due to a surge in the number of businesses relocating. The program also had an impact on total employment in the area, measured in terms of both jobs and worked hours. However, the scheme seems to have had no effect on establishments that already existed in 2002. While these existing companies were as eligible as new ones for tax exemptions on offer, their bankruptcy

³ Gobillon et al. (2011) show that in the case of France, spatial differences contribute to explain more than half of the differences of individual unemployment spell durations. See Gobillon et al. (2007) for a review about spatial mismatch.

⁴ The controversy also involves Bondonio and Engberg (2000), Greenbaum and Bondonio (2004), Elvery (2009) or Lynch and Zax (2010) that find no effect and Papke (1994), O'Keefe (2004) that do.

⁵ Gobillon et al. (2010) provide a complementary analysis on the impact of ZFU on local unemployment. They use public-employment-service data, at the level of municipalities, and find that the first round of ZFU brought about a very small drop of the number of job seeker in treated areas. Recently, Givord et al. (2012) provide analysis on the first round and Briant et al. (2012) links the heterogeneity in estimated impact of the program with the geographical characteristics of areas.

⁶ To our knowledge, only Peters and Fisher (2002), Billings (2008), and Neumark and Kolko (2010) have used data at such a precise scale.

⁷ Dropouts are individuals who left the educational system with no degree.

⁸ This result is consistent with anecdotal evidence that suggest that, as local authorities pressured the government to benefit from ZFU funding, an implicit criterion for selection in the second-round was to make sure that almost all regions had its own ZFU. Besides, the size of the zone was one of the main eligibility criteria, with an implicit threshold of 10,000 inhabitants. Most of the biggest zones were turned ZFU in the first round; in 2004, there were some instances in which two or more smaller ZRUs were merged when geographically close in order to meet these size criteria in 2004. See Section 2 for details.

rate, employment level and general economic situation remained essentially unaffected by the program. Finally, we find that the ZFU program had significant negative spillover effects at least during the first three years, on the areas situated just beyond the ZFU boundaries. Negative effects are observed on economic activity outcomes (number of establishments, creation rates, etc...) but not on employment outcomes. Spillovers can be quantified, on average, with the same order of magnitude as the impact of the program, meaning that the positive impact on treated areas is likely to be totally offset by negative effects on their immediate vicinity.

The following section introduces the institutional context and the features of the evaluated program. Section 3 presents the data as well as some summary statistics. In Section 4, we detail the methodology used. The results of the estimations – both the direct impact of the policy and some evidence about displacement effects – are provided in Section 5. Finally, in the last section, the main results are summarized and discussed from a policy perspective.

2. A brief description of the French *Zones Franches Urbaines*

In France, sporadic outbursts of violence (as recently as 2005 for example) have called for revitalizing policies in poor suburban areas characterized by a high concentration of social problems. Since the early 1980s, the French government has taken a series of measures targeting these economically distressed neighborhoods,⁹ generically named *Politique de la Ville* (Urban Policy). Common features of these neighborhoods include high unemployment and poverty rates, limited access to public services and retail as well as relatively low property prices. The first two development stages of French urban policy in the 1980s and early 1990s have not been evaluated. In many deprived neighborhoods, these measures have not been sufficient to resolve issues.

The *Pacte de Relance de la Ville* (Urban Revival Pact), initiated in 1996, constituted the third stage. Stressing the need for economic revival in sensitive urban areas (ZUS) as a prerequisite for their social renovation, the pact listed several hundreds of priority neighborhoods and granted them tax exemptions to attract new business and raise employment levels. Three area categories were created. First, 750 neighborhoods were labeled *Zones Urbaines Sensibles* (ZUS). In practice, the fact of being defined as a ZUS alone does not grant any tax exemptions, but rather signals that local funding should be directed there as a priority. Then, 416 particularly distressed ZUS neighborhoods were declared *Zones de Revitalisation Urbaine* (ZRU), while the 44 most underprivileged were assigned to the ZFU program. In contrast to ZUS, businesses entering either a ZRU or a ZFU are entitled to tax rebates and exemptions.

The main objective of the ZFU and ZRU programs is to promote economic development in these areas. These schemes grant substantial tax reliefs to existing and new businesses, with a much higher exemption level in ZFUs than in ZRUs (see Appendix A for details). ZFU-located companies with fewer than 50 employees benefit from five-year exemptions from local business taxes, corporate income tax and property taxes. They are also exempt from employers' social security contributions for a period of five years on the fraction of salaries lower than 1.4 times the minimum wage. This aid is, however, limited in time, as the measures on offer terminates over a gradual three- to nine-year period, following the full-rate exemption for the first five years. Ten years after its entry in the program, a company with fewer than five employees still benefits from a 60% tax rebate. Moreover, in practice, all ZFUs have been extended beyond the theoretical deadline and are still active as of today.

French ZFUs share some features with similar programs, especially with U.S. schemes.¹⁰ The U.S. scheme offers a number of tax credits to businesses located within the zone boundaries. U.S. authorities define eligible areas based on socio-economic criteria, such as high unemployment rate or low income levels (Bondonio and Engberg, 2000).

By contrast, the initial UK Enterprise Zones program focuses directly on areas witnessing the contraction or relocation of existing industries.¹¹ The Italian program *Patti Territoriali* adopts a much more decentralized approach, as a broad set of regions are eligible, establishing a local agreement signed by both local governments and representatives of civil society to obtain public and private investments.

French and similar programs differ in many other respects. First, in the French case, the designation process does not require a strategic development plan, which is the case in some U.S. Enterprise Zones. French ZFUs tend to be generous in magnitude, but restricted in scope. They provide a wide range of tax exemptions, which can be sizable for eligible companies (see Givord et al., 2012, for an estimation). While in most Enterprise Zones, the tax incentives granted to businesses are tied to specific requirements (e.g. a threshold for job creations or capital investment), such requirements are rather weak in France. For example, in the case of exemptions from payroll taxes that are applied to all employees of a business establishment located in the zone, the only requirement stipulates that one third of new hirings should be local residents. In contrast with the Enterprise Zone program, ZFU specifically targets very small businesses. Only those with less than 50 employees are eligible (with an additional restriction on sales). These small businesses are likely to be particularly sensitive to tax incentives, as their limited access to credit means that are financially constrained.

The second round of ZFU creations occurred in January 2004, with 41 new ZFUs. These areas were selected out of the existing ZRUs. Officially, the selection criteria were specified by a decree, and consist of a combination of 5 local indicators, namely: the total population of the area, the unemployment rate, the proportion of youth (under 25), the proportion of drop-outs with no qualifications and the tax potential of the municipality.¹² The composite index was calculated as the product of the first four elements, divided by the fifth.¹³ In addition, it was specified that a ZFU should have more than 10,000 residents. However, anecdotal evidence from conversations with officials of the Ministry of Urban Affairs suggests that the main selection criteria used in the second round were clearly: (i) the metropolitan area in which the ZRU is located and (ii) its size.

The objective of the decision makers was to achieve a more even distribution of ZFUs at a national scale. While many major metropolitan areas (Bordeaux, Montpellier, Toulon, Nice...) had been allocated a ZFU in 1997, some important ones had not (Toulouse, Clermont-Ferrand, Nantes, Grenoble...). Fig. 1 demonstrates the uneven distribution of ZFUs at a national level. Further evidence relevant to this feature is given below, in the section which discusses the estimation of the propensity score.

The determination of the composite index (and in particular the threshold of 10,000 inhabitants) suggests that size matters more

¹⁰ See Bondonio and Engberg (2000) for details on state Enterprise Zones and Ham et al. (2011) for a comparison with federal Empowerment Zones and Enterprise Communities programs.

¹¹ For instance areas concerned by coal, steel and shipbuilding closures.

¹² The tax potential was defined as the product of local taxes for the municipality if the average national rate was applied to the municipality for each of the local rates, given the tax basis of the municipality. This variable characterized the potential fiscal wealth of the municipality.

¹³ The index was calculated based on the 1999 population census (INSEE) and fiscal sources (General Tax Office and Ministry of Finance) which enable the determination of the tax potential for the latest available year.

⁹ In what follows, a neighborhood has typically a population of 9000 to 30,000.

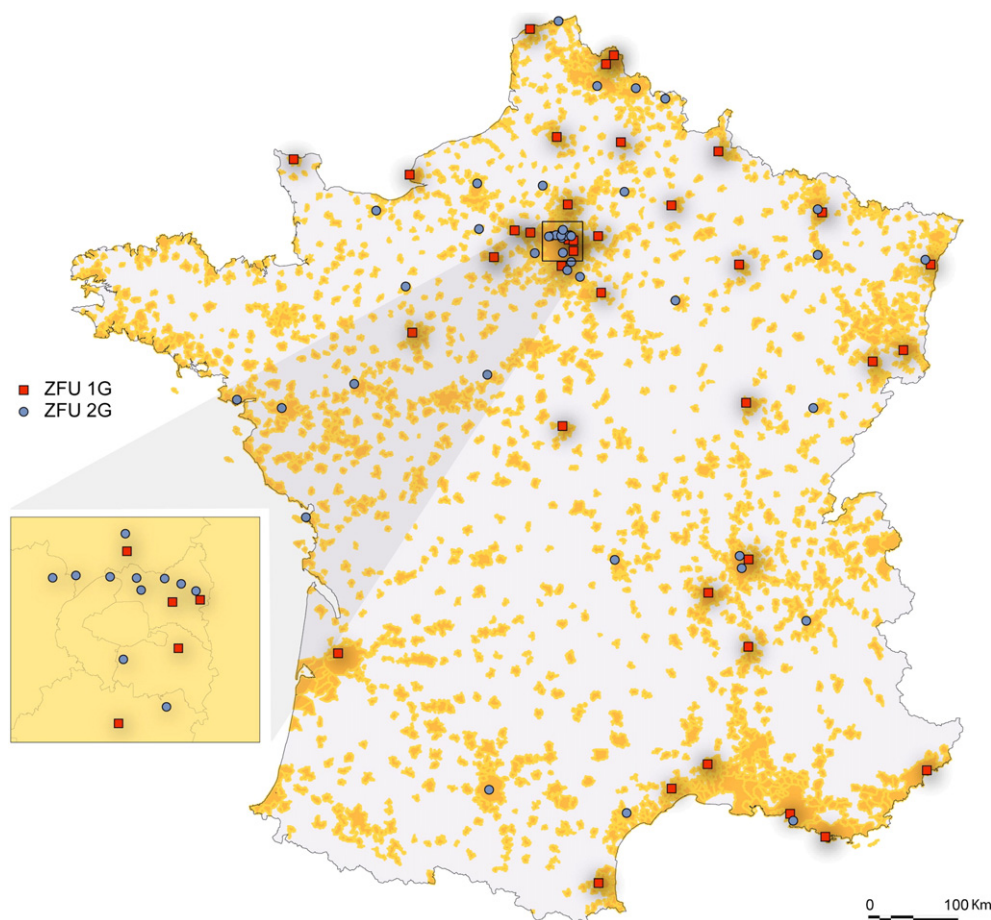


Fig. 1. Distribution of ZFUs created in 1997 (ZFU 1G) and 2004 (ZFU 2G) over the French metropolitan territory. The inset map represents the Île-de-France region.

than other local factors. Large ZRUs that had not been picked in the first round had to be included in the second one.

The geographical extent of a ZFU can be flexible in order to meet the size criterion. It encompasses the territory of the underlying ZUS or ZRU but also often includes additional contiguous pieces of land. Fig. 2 shows for instance the boundary of a ZFU and the pre-existing ZRU. In addition to the territory of the ZRU, the ZFU includes land intended to be used by incoming businesses. In some cases, the ZFU even combines several ZRUs from the same conurbation. Indeed, the 41 second-round ZFUs were created out of 51 initial ZRUs. One consequence of this is that a particular ZRU has more chance of being designed as ZFU if it is located near another existing ZRU.

A closer look at the distribution of the variables composing the composite index (plus total population and the proportion of inhabitants in social housing), in the ZFUs created in 1997 and 2004 and the ZRUs that were not upgraded into ZFUs in 2004 show indeed that the second ZFU round does not correspond to the most deprived areas (Fig. 3).¹⁴ For example, the second round of ZFUs has on average a smaller share of dropouts than the other ZRUs (the average rate over these areas is 29% against 33%) as well as a lower unemployment rate (the averages are respectively 28% and 30%). The ZFUs of 2004 look even better off than both other groups in terms of the financial capacity of municipalities. If the share of foreigners is higher in the first round of ZFUs than in other areas, it is distributed in a similar way in the new ZFUs of 2004 and ZRUs.

Finally, in 2006, 15 new ZFUs were created from the remaining ZRUs. As we focus our analysis on the second round, we have removed

from the sample all those ZRUs that were not assigned in 2004 but were assigned in 2006.

As we evaluate the second round of the policy, 44 ZFUs had already been active since 1997.¹⁵

One may suspect that the existence of the first round treated zones might weaken the efficiency of the tax incentives to attract businesses to the newly designated zones, as more locations would now be competing. However, spatial mobility is weak in France. The cost of moving a long distance from their “natural” location can be dissuasive for small businesses (those eligible for the reduced tax rates).

3. Data

3.1. Data description

The data we use come from several sources. The SIRENE directory (INSEE) contains exhaustive information on businesses in manufacturing, trade and services industries, in particular their location. The stock of businesses is reported yearly. New businesses are also reported, as well as their creation date and the business origin (whether a birth or a relocation). The bankruptcy file (INSEE) contains the records of all bankruptcy proceedings brought before a trade tribunal.¹⁶

¹⁵ For a recent analysis of this wave, see Givord et al. (2012).

¹⁶ A bankruptcy is characterized by insolvency, i.e. when a company is no longer able to repay its debts. Not all legal decisions to open bankruptcy proceedings (company filing for bankruptcy as part of legal proceedings) lead to liquidation. In addition, bankruptcy does not give the whole picture in terms of discontinuance of business. A company can, for instance, put a stop to its activity because its owner decides to retire and his assets are not taken over.

¹⁴ All variables presented in this figure are defined in more detail in Section 3.

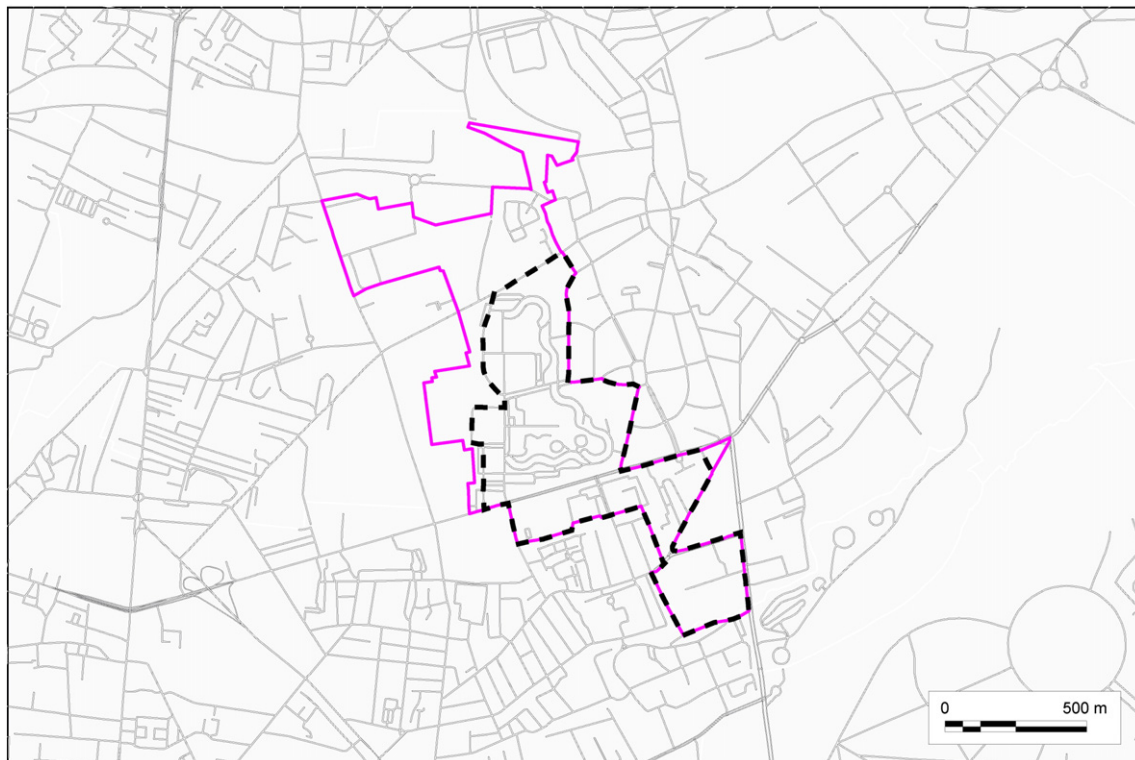


Fig. 2. The ZRU (dotted line) and the ZFU (plain line) in Stains, in the Northern *banlieue* of Paris.

The DADS (annual declarations of social security data) dataset (INSEE) contains exhaustive information on employment and salaries in these businesses. The fiscal records, called BRN and RSI, are provided by the tax administration. These files provide accounting information (balance sheet and income statement) as well as the amount of taxes paid by every business. These data are available and matched over the period 2002–2007, that is from two years before the introduction in the ZFU policy. We also use the 1999 Census (INSEE) to extract variables which characterize the socio-demographic situation of the areas under study. In particular, some of these characteristics are the criteria that were considered by decision-makers during the selection process.

These data allow us to evaluate the impact of the ZFU program over a broad range of indicators. We estimate the impact of the ZFU program on economic activity as measured by the stock of companies located within the boundaries of the targeted areas, on business demography (creations and bankruptcy), and on paid employment. We can also evaluate whether the tax subsidies result in the location of new businesses or affect the economic situation of previously existing ones. Because we have access to establishment-level panel data, we can isolate businesses with at least one employee which are eligible for tax exemptions (less than fifty employees, total revenue under €10 million, not held by higher corporate), located in ZRUs in 2002. We follow the commercial outcomes for these establishments during the period, according to whether the ZRU in which they are located was granted ZFU in 2004 or not. As it is unlikely that these businesses anticipated receiving these tax concessions two years before the ZFU assignment, we can rule out possible selection issues due to businesses locating there later. We can therefore measure whether tax exemptions are effective in fostering economic development in existing small businesses.

As micro-businesses are often said to be confronted with financial difficulties jeopardizing their survival, tax exemptions can improve their financial situation. We measure the impact of the subsidies on a large number of outcomes, in order to evaluate whether businesses use the money to improve employment, investment, or their financial situation (which may, in turn, have an effect on their risk of bankruptcy).

We will therefore focus on several indicators of the economic and accounting situation: sales, current taxable income, debt, cash flow, investment levels, as well as the share of bankruptcies in the area. We also measure whether the impact on employment is achieved at the intensive (with more worked hours per employee) or at the extensive margin (resulting in more employees).

Depending on their industries, businesses may respond differently to tax incentives. For instance, some industries may benefit more from agglomeration externalities or may rely more on skilled labor. The economic activity can be located in places other than the business establishment (and so not be affected by the negative externalities of a distressed neighborhood). Finally, it appears essential to determine whether the impact of these geographically-targeted tax cuts is made at the expense of surrounding neighborhoods, which are themselves quite poor.

3.2. Summary statistics

Table 1 presents basic statistics for the areas under consideration in 2002, before the creation of the second round of the ZFUs. We compare areas included in the 2004 ZFU program in 2004 with those that were not involved. As stated above, the 51 areas chosen to become ZFUs are on average much bigger than the 284 other areas in terms of available land and number of inhabitants. Because of this size effect, more companies are initially located in these areas (187 on average in ZFUs, compared to 84 in non-ZFU areas). However, a large fraction of these businesses were not eligible for the tax exemptions granted by the program (i.e. they had more than 50 employees or/and a total revenue above €10 million). Finally, on average 37 new businesses located in the future ZFUs in 2002, whereas only 15 businesses did so in the other areas. Most of this difference is due to scale effects: when reported relative to the initial stock, the rates of settlement of new businesses are quite similar across areas. In both groups, new businesses mainly correspond to births (only 15% correspond to transfers).

The characteristics of eligible businesses in both groups are quite comparable across areas in 2002 (Table 2). On average, these companies are far below the 50 employees threshold: independent of zoning, the

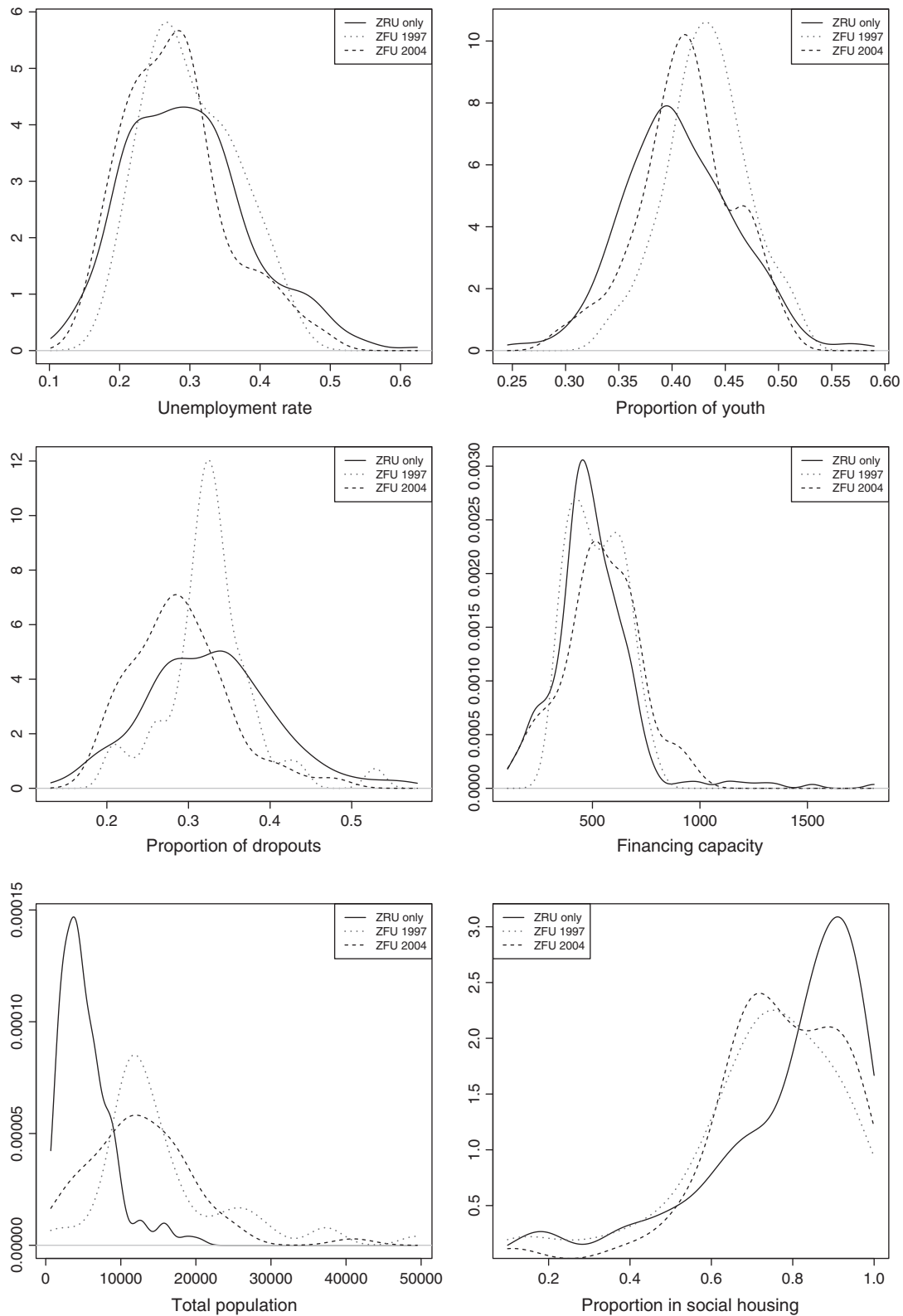


Fig. 3. Estimated densities of unemployment rate, proportion of youth, proportion of dropouts, municipal financing capacity, total population, proportion of social housing in the first generation of ZFU ('ZFU1G'), the second generation of ZFU ('ZFU2G') and ZRU that did not become ZFU ('ZRU').

average labor force was around 5 employees in 2002. Their accounting situations were also similar.

One potential issue with our identification strategy is that we compare ZFU vs ZRU and not ZFU vs areas without any kind of incentives. As companies also benefit from tax incentives to set up in ZRUs, which

are our control group, we run the risk of a downward bias in the estimation of the causal impact of the ZFU program. The main reason why this issue should not be overstated is provided by Fig. 4. This figure displays the evolution over time of the average yearly amounts of tax exemptions for an eligible company already present in the ZRU in 2002, depending

Table 1
Economic situation in the areas in 2002.

	ZRUs not turning ZFU after 2004	ZRUs turning into ZFU in 2004
Total population	5433	12,644
Area (km ²)	77.2	135.4
Number of businesses	84	187
Number of workers	389	685
<i>Business demography (per yr.)</i>		
New businesses	15.27	37.45
Births	12.68	31.47
Transfers	2.59	5.98
New businesses (/ Stock in 2001)	0.19	0.21
Births (/ Stock in 2001)	0.16	0.18
Transfers (/ Stock in 2001)	0.03	0.03
<i>Industries</i>		
Share of manuf.	0.11	0.12
Share of retail	0.36	0.32
Share of construction	0.24	0.25
Share of services to hh.	0.15	0.15
Share of services to b.	0.08	0.10
Share of transportation	0.04	0.04
Number of ZRUs	284	51

Notes: Restriction to eligible companies (50 employees or/and a total revenue above €10 million).

Table 2
Descriptive statistics for eligible companies in 2002.

	Area not turning into ZFU after 2004	Area turning into ZFU in 2002
Number of workers	4.96	5.10
Hourly wage (euro)	10.54	10.64
Sales (×1000 euro)	250.22	232.00
Income (×1000 euro)	35.50	33.03
Cash flow (×1000 euro)	26.61	25.87
Investment (×1000 euro)	12.55	11.58
Debt (×1000 euro)	93.67	93.38
Average number of eligible companies per area	21	38

on the treatment status. First, we can see that the situation in the treated and control groups are almost identical before 2004. Second, we can observe a downward trend for non-treated ZRUs, corresponding to a fade-out of the tax rebate scheme in this area. In 2003, eligible companies in ZRU reported on average a rebate of 1000 euros: 10 times less than in ZFU in 2004 and 20 times less than in ZFU in 2005 and 2006. As a consequence, we can consider the ZRU tax exemptions to be negligible with respect to those provided by the ZFU program.

Figs. 5–6 show the evolution over the period of the average amount of business tax and social security contributions paid by companies located in ZRUs in 2002, depending on the treatment status. For both types of tax, the curves have similar patterns in 2002 and 2003 but then diverge in the following years. Declared business tax patterns are especially spectacular, growing throughout the period in the ZRUs while remaining at low levels in ZFUs. This is consistent with the fact that the sample considered in these figures is for establishments that already existed in 2002. The number of businesses in the sample decreases over time and their average size (in terms of employment or sales) increases. In 2007, the companies of our sample located in second-generation ZFUs claimed they paid €800 in business tax, i.e. five times less than similar companies located in ZRUs which are not ZFUs. A comparable evolution is observed for social security contributions: the ZFU program has resulted in a stabilization although the amounts tend to increase after 2005.¹⁷ The amount

¹⁷ As the exemptions from social security contributions are conditional on hiring local workers, this result might signal that, whether they would not or could not, a substantial share of companies in treated areas did not fulfill this local hiring condition.

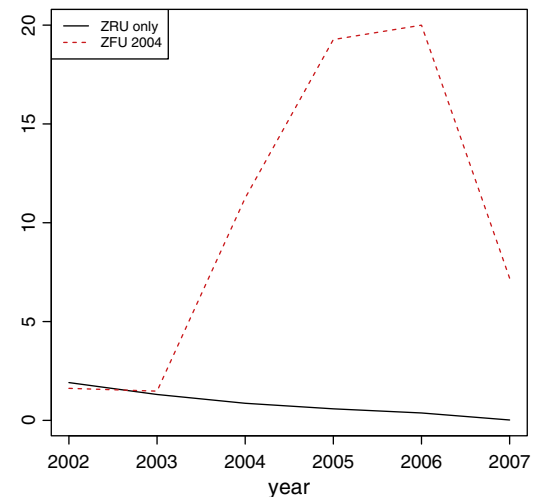


Fig. 4. Declared exemption amount per area (thousand euros).

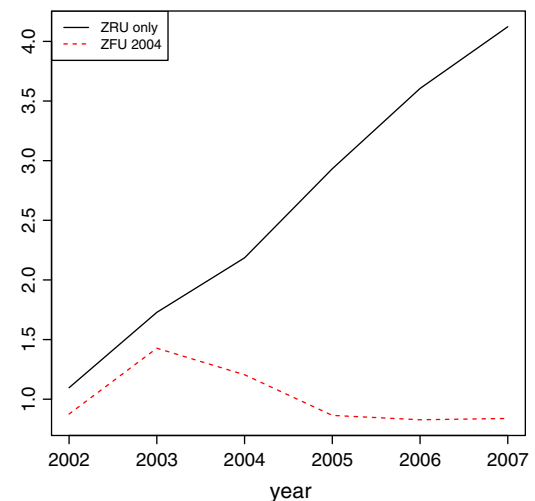


Fig. 5. Declared business tax amount (thousand euros).

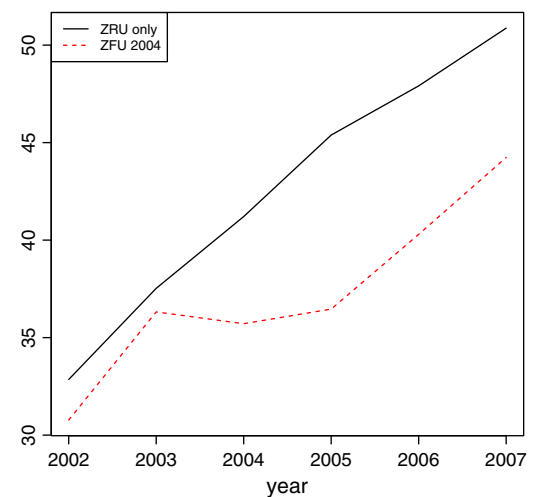


Fig. 6. Declared amount of social security contributions (thousand euros).

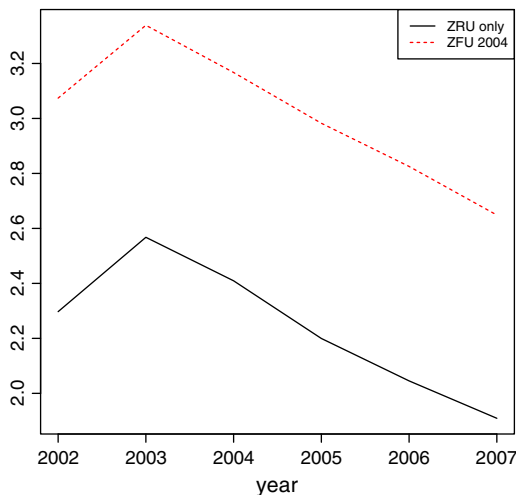


Fig. 7. Number of eligible establishments already present in 2002 (in logs).

of social security contributions declared by the companies in second-generation ZFUs is, as of 1 January 2007, €4300 on average (Fig. 6). For the companies located in ZRUs not classified as ZFUs, this amount is on average €5000 in 2007. As of 1 January 2002, these companies declared average amounts of respectively €3000 and €3250. Fig. 7 shows the mean number of eligible establishments already present in 2002 by ZRU (in logs). The evolution over time is similar across both groups. Overall we can conclude from these figures that throughout the period the ZFU program resulted in a significant reduction in the tax burden of the companies concerned, even compared with neighborhoods in which tax rebates were also proposed.

4. Estimation strategy

Ideally, the effect we want to measure would be obtained by calculating the difference in the dependent variable between the units which are actually treated and the same units but in a scenario in which they would have not been treated. While we can directly observe the former, the latter is unobservable as the units in question have been treated. The ZFU program is granted to areas that have experienced economic and social problems. Therefore, direct comparison of all treated and all non-treated units could lead to spurious results, probably understating the true impact of the program. As detailed below, our empirical strategy involves choosing a control group of areas as close as possible to ZFUs in terms of their observable characteristics. The pool of ZRUs which were not assigned to the program is an obvious choice, given the assignment process of the second round. Our identification assumption states that, conditional on characteristics observed before assignment, the affectation of a ZRU to the ZFU program is not related to the potential outcome.

Formally speaking, we can consider that any ZRU i has virtually two potential outcomes, at time t : one referred to as Y_{it}^1 , if it is located in a ZFU, and the other, referred to as Y_{it}^0 , if it is not. The impact of the ZFU program at time t on this area is equal to $Y_{it}^1 - Y_{it}^0$, with t being after the formation date of the ZFU. It will never be possible to estimate this impact as we can never simultaneously observe both potential outcomes for the same unit at the same time. In order to obtain an evaluation of the counterfactual outcome, we use a conditional difference-in-differences strategy.¹⁸ The identifying assumption states that conditionally to observable characteristics, all remaining differences (that could impact outcome) between the units are not associated with

the treatment. This is supported by the fact that we could use time-differentiated outcomes, and thus eliminate potential systematic differences between the areas (see Heckman et al., 1998).

$$\Delta Y_{it}^0 \perp T_i | X_i \quad (1)$$

To overcome the problem of dimensionality arising when using too many observables, we perform a propensity score matching. As shown by Rosenbaum and Rubin (1983), if the conditional independence assumption holds for observables X , it also holds for the propensity score, i.e. the probability of being treated conditional on these observables. In other words, hypothesis (1) implies

$$\Delta Y_{it}^0 \perp T_i | p(T_i = 1 | X_i). \quad (2)$$

In practice, the estimation is a two-stage process: the estimation of the propensity score followed by the matching itself. We estimate this propensity score using a logit model.

Many conditioning variables are introduced in the specification of the propensity score. First, we include the variables used to build the composite index which was supposed to guide decision makers for the assignment of ZRUs to the ZFU program (see Section 2). Apart from the fiscal potential, measured in 2002, these variables are extracted from the census collected 1999. Then, we introduce two variables related to the relative distance of ZRUs from each other and of ZRUs with first-round ZFUs. Next, a cubic of the total population, measured in 1999, is included. Finally, we use the number of businesses and employment, levels (in 2002) and in first-differences (2002 minus 2001), to control for the diversity of local economic situations.

As expected by our descriptive analysis, the areas that turned into ZFUs in 2004 tend to have slightly more social issues than ZRUs, yet most of the coefficients are insignificant (see Table 3). Only the share of youth is significantly associated with assignment probability. In spite of what appeared in descriptive analysis, population seems to play no significant role, once other covariates are accounted for. In contrast, coefficients related to distance variables are strongly significant. We argued in Section 2 that, for a ZRU in 2003, the distance to a first-round ZFU and to another ZRU should affect positively the probability of being assigned to treatment. The former because it leads to a more even distribution of ZFUs at the national scale, and the latter because ZRUs could be merged in order to constitute a zone achieving the required size (10,000 inhabitants). Fig. 8 shows the density of the propensity score in the treated and control groups.

Table 3
Probability for an area in 2003 to be part of a ZFU in 2004.

Variables	Probability of belonging to a ZFU in 2004
Intercept	496.69
Distance to closest ZRU < 2 km	1.35***
Distance to closest ZRU < 5 km	1.38**
Distance to closest ZFU1997 > 30 km	2.42***
Log total population	−165.05
Log total population, squared	18.02
Log total population, cubed	−0.63
Fiscal potential	−1.06
Proportion of less than 25	5.03**
Proportion of dropouts	0.25
Unemployment rate	−0.72
Log number of establishments	−1.10
Δ Log number of establishments	6.57**
Log employment	0.36
Δ Log employment	−0.67
Δ Log employment (present in 2002)	2.14
Δ Log hours worked (present in 2002)	−1.10
(r)1-2 Number of ZFU 2004 observations	51
Number of non-ZFU 2004 observations	284
Efron's R-squared	0.47

Note: Logit estimation. The standard deviation of the estimator is in brackets. Three (respectively two, one) stars indicate a 1% significance (respectively 5%, 10%).

¹⁸ Similar method have been used on related topics for instance in Billings (2011), Bernini and Pellegrini (2011).

Overall, the results of the propensity score estimation are compatible with our view, built on anecdotal evidence and summary statistics, indicating that selection bias should be of second-order. First, because it turns out that the most important predictor for the assignment to the treatment is the distance to another ZRU or to a first-round ZFU. Second, because social covariates, even those which should have been taken into account by decision-makers, do not differ that much between treated and control groups. Even economic variables, like the number of businesses, are largely insignificant. Note that the results presented in the next section are robust to changes in the specification that concern insignificant variables.

In the second stage, each treated unit is matched with untreated units with a similar propensity score. We used a kernel matching method: untreated observations are weighted according to their distance from the treated observation, the weights being determined by a (Gaussian) kernel.¹⁹ At the end of this matching process, the estimated effect of the treatment is obtained for each treated unit. The average effect of the treatment is then calculated by considering the average of individual differences. Because we want our results to be robust to variations in the specification of the matching process, we also performed the analysis using nearest-neighbor matching, inverse probability weighting and inverse probability weighting with a control for covariates in a second stage. The results we stress in the next section are all qualitatively unaffected by these specification changes.²⁰

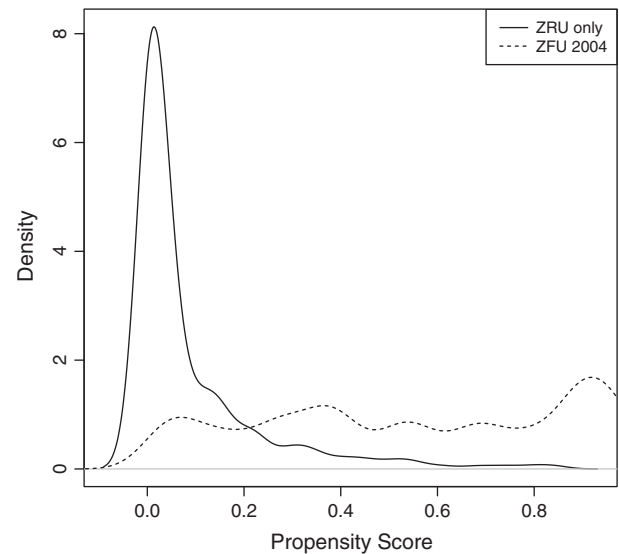
As demonstrated by Bertrand et al. (2004), the inference is biased by both group effects, as businesses in the same areas may experience joint shocks, and serial correlation in residuals. Our estimation is performed at the ZRU level, which reduces the extent of the former issue. Besides we use full bootstrap on ZRUs for inference to take into account serial correlation.

5. The impact of the ZFU program on the treated areas

In this section, we report the results of our propensity-score matching estimations, on a large set of outcomes (company demography, employment, economic situation of preexisting companies) for each year between 2003 and 2007.²¹ It is important to stress that the program started, for the second round of ZFU, in 2004. Year 2003 thus constitutes a “falsification test” in the sense of Manning and Pischke (2006): if the control group is a valid counterfactual for the treatment group then control and treated areas should not differ before the program is enforced. This falsification test can be considered as a test of our identification assumption. For all the outcomes considered in this study, there is no significant difference between our treatment and (matched) control groups before the introduction of the tax exemptions (Tables 4–6).

The overall impact of the ZFU program on economic activity is positive from the starting date of the treatment until 2007 (Table 4). In particular, the growth rate of the stock of companies is 5 to 7 percentage points higher in the treated ZRUs than in the control group between 2004 and 2007. Such a positive effect could be explained in two ways. At the intensive margin, the ZFU program could have helped pre-existing companies not to exit. At the extensive margin, the program could have fostered company births or relocations to the treated areas.

When we focus on companies that were already present in the area in 2002, we observe a positive difference in favor of the treatment group in 2004, especially for the companies with less than 3 employees,²² but this effect is neither significant nor persistent over



Note: Both densities have been estimated by kernel density estimation, with a normal kernel.

Fig. 8. Densities of the propensity score, by group.

time. Moreover, the evolution of the number of bankruptcies is not statistically different across groups. Both results suggest that the survival rates of pre-existing companies are not statistically affected by the ZFU program.

On the contrary, the program seems to be very effective at attracting new businesses into the areas benefiting from tax subsidies. The inflow rate, measured as the ratio of new businesses in the current year over the stock on January 1st, is increased by 5 to 8 percentage points between 2004 and 2007 by the program. These new establishments can be relocations of existing companies or pure creations: we estimate the treatment effect separately for both outcomes and reach two conclusions. Business births and transfers both contribute to the inflow in the ZFUs. However, the timing appears to be different, with more births just after the beginning of the program

Table 4
Impact of the transition to ZFU on stock of companies and business demography.

Variables	Years				
	2003	2004	2005	2006	2007
<i>Stock (Δ Log)</i>					
Number of establishments	0.01 (0.02)	0.05*** (0.02)	0.07*** (0.03)	0.06** (0.02)	0.05** (0.02)
Amongst companies eligible					
already present in 2002	–0.00 (0.03)	0.05 (0.10)	0.01 (0.04)	–0.04 (0.04)	–0.03 (0.05)
...with less than	–0.04 (0.04)	0.10 (0.07)	0.07 (0.06)	0.02 (0.05)	0.02 (0.08)
3 employees in 2002					
...with more than	0.04 (0.04)	0.03 (0.04)	–0.05 (0.05)	–0.06 (0.06)	–0.04 (0.07)
4 employees in 2002					
Amongst companies eligible already present in 2002					
Δ Bankruptcies	0.21 (1.77)	0.30 (2.27)	1.28 (2.33)	–2.45 (1.81)	–0.28 (1.32)
(for 1000 companies)					
<i>Flow (relatively to the previous stock)</i>					
Δ Births and transfers	0.00 (0.02)	0.06*** (0.02)	0.06* (0.03)	0.08*** (0.02)	0.05** (0.02)
Δ Births	0.01 (0.02)	0.04*** (0.02)	0.04* (0.03)	0.05*** (0.02)	0.01 (0.02)
Δ Transfers	–0.00 (0.01)	0.02*** (0.01)	0.02* (0.01)	0.03*** (0.01)	0.04*** (0.01)

Note: All results featured herewith correspond to the preferred specification of propensity-score matching, applied to time differentiated variables. The standard deviation of the estimator is in brackets, estimated by block bootstraps in areas. Three (respectively two, one) stars indicate a 1% significance (respectively 5%, 10%).

¹⁹ The kernel bandwidth is chosen with the rule of thumb: $\sqrt{V(\hat{p})N^{-1/5}}$ (where $V(\hat{p})$ is the variance of the estimated propensity score and N the number of observations). In order to ensure that areas too far from one another are not matched, we impose a caliper of .05, that is, we force the weights to be equal to 0 when the difference between a treated and a control area is higher than .05.

²⁰ The corresponding tables are available from the authors upon request.

²¹ Descriptive statistics, figures production and econometric treatments have been carried out using the software R (R Development Core Team, 2010).

²² We chose this threshold, as the median establishment present in 2002 in our sample has 3 employees.

Table 5
Impact of the transition to ZFU on employment (log).

Variables	Years				
	2003	2004	2005	2006	2007
All companies					
Δ Employment	−0.01 (0.05)	0.04 (0.04)	0.06 (0.05)	0.04 (0.08)	0.12** (0.06)
Δ Hours	−0.01 (0.04)	0.03 (0.04)	0.08 (0.05)	−0.00 (0.08)	0.12** (0.06)
Amongst companies eligible already present in 2002					
Δ Employment	−0.02 (0.04)	−0.03 (0.05)	−0.06 (0.05)	0.09 (0.08)	0.00 (0.06)
Δ Hours	0.00 (0.04)	−0.05 (0.05)	−0.07 (0.05)	0.09 (0.08)	−0.02 (0.08)
...with less than 3 employees in 2002					
Δ Employment	−0.00 (0.06)	−0.08 (0.07)	0.06 (0.09)	0.09 (0.08)	0.12* (0.06)
Δ Hours	0.02 (0.06)	−0.10* (0.05)	0.03 (0.07)	0.14*** (0.05)	0.02 (0.06)
...with more than 4 employees in 2002					
Δ Employment	−0.05 (0.04)	0.01 (0.05)	−0.05 (0.06)	0.09 (0.08)	0.04 (0.04)
Δ Hours	−0.03 (0.04)	−0.00 (0.05)	−0.08 (0.07)	0.07 (0.09)	0.07 (0.05)

Note: All results featured herewith correspond to the preferred specification of propensity-score matching, applied to time differentiated variables. The standard deviation of the estimator is in brackets, estimated by block bootstraps in areas. Three (respectively two, one) stars indicate a 1% significance (respectively 5%, 10%).

and a growing impact of relocations between 2004 and 2007, from 2 to 4 percentage points. Compared to the average birth and relocation rates (see Table 1), the program has a much more important relative impact on relocations. In 2002, the birth rate was on average .16 while the relocation rate was .03. Thus, a 4 percentage points increase on the birth rate represents a 25% relative increase, while a 3 percentage points increase on the relocation rate means there are twice as many relocations as were observed on average. In relative terms, the impact is much higher on relocations than on births.

Table 5 presents the impact of the ZFU program on employment outcomes. Two outcomes are considered: the annual growth rate of the number of employees in a given establishment on December 31st, and the annual growth of the number of hours worked. Interestingly, the impact of the treatment is similar on both measures, which means that the program has no impact on the number of hours worked per worker.²³ Overall, there is a positive effect of the program on employment, yet the effect is significant only for year 2007. The magnitude of the effect of the ZFU program on employment ranges from 4 percentage points (insignificant) in 2004 to 12 percentage points (significant at 5%) in 2007.

Is this relatively small effect on employment due to the formation of new companies or to the growth of pre-existing ones? When we restrict our sample to establishments which were already present in 2002, we find no effect of the program on employment. Focusing on businesses with less than 3 employees in 2002, there are significant and positive effects on hours worked in 2006 and on employment in 2007. On larger pre-existing businesses, no significant effect is observed. In a nutshell, if the program had an impact on business creation, evidence on employment is more mixed. The impact on employment seems to be weak on pre-existing establishments, and, if anything, focused on small businesses.

²³ Informal discussions with local stakeholders suggest that, because of the local-hiring requirement, some businesses chose to hire local residents in order to meet this hiring requirement, but only with part-time contracts. This phenomenon should have brought about a smaller increase in total hours than in employment; our results do not confirm this hypothesis.

Table 6
Impact of the transition to ZFU on wages and accounting indicators, for eligible companies existing in 2002.

Variables	Years				
	2003	2004	2005	2006	2007
<i>All eligible companies present in 2002</i>					
Income (Δ Log)	−0.00 (0.01)	−0.02** (0.01)	−0.01 (0.01)	−0.01 (0.01)	0.01 (0.01)
Sales (Δ Log)	0.00 (0.03)	−0.01 (0.04)	−0.12** (0.06)	0.06 (0.07)	0.02 (0.06)
Hourly wage (Δ Log)	−0.00 (0.02)	0.01 (0.02)	−0.00 (0.01)	0.00 (0.02)	0.02 (0.03)
Δ Cash flow/Sales	−0.01 (0.01)	0.01 (0.01)	0.00 (0.01)	−0.03* (0.02)	0.03 (0.03)
Δ Debt/Sales	−0.02 (0.02)	−0.04 (0.02)	−0.00 (0.03)	−0.01 (0.02)	0.00 (0.04)
Δ Investment/Sales	−0.01 (0.02)	0.01 (0.02)	0.01 (0.01)	−0.01 (0.01)	−0.01 (0.02)

Note: All results featured herewith correspond to the preferred specification of propensity-score matching, applied to time differentiated variables. The standard deviation of the estimator is in brackets, estimated by block bootstraps in areas. Three (respectively two, one) stars indicate a 1% significance (respectively 5%, 10%).

Table 6 reports the impact of the program on accounting variables of establishments already set up in 2002. For income, sales and hourly wages, the impact on the growth rate is reported while, for cash flow, debt and investment, the impact is computed on the annual difference in the ratio of the outcomes divided by the volume of sales. The coefficients are small, not persistent with time and, in general, few of them are significant. These results suggest that ZFU had little impact on the economic situation of pre-existing companies.

Finally, Table 7 reports the impact of the ZFU program on economic activity, decomposing the effects by industries. Two kinds of outcomes are selected: the growth in the number of businesses set up in a given area, and the number of new businesses divided by the stock in the previous period. According to both outcomes, there is a

Table 7
Impact of the transition to ZFU on business creation, by industry.

Variables	Years				
	2003	2004	2005	2006	2007
<i>Stock (Δ Log)</i>					
Manufacturing	0.00 (0.06)	0.11* (0.06)	0.03 (0.05)	0.05 (0.07)	−0.05 (0.07)
Construction	0.06 (0.05)	0.05 (0.05)	0.07 (0.05)	0.05 (0.05)	0.14** (0.07)
Retail	0.00 (0.03)	0.05 (0.03)	0.02 (0.03)	0.04 (0.03)	0.02 (0.03)
Transportation	0.04 (0.08)	−0.02 (0.09)	−0.02 (0.06)	0.03 (0.08)	−0.04 (0.10)
B to B	0.03 (0.07)	0.13** (0.06)	0.13* (0.07)	0.00 (0.07)	0.15*** (0.05)
Services to households	−0.03 (0.05)	0.05 (0.04)	0.10 (0.06)	0.03 (0.05)	0.03 (0.04)
<i>Flow (relatively to the previous stock)</i>					
Manufacturing	−0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	−0.00 (0.00)
Construction	0.01 (0.01)	0.01 (0.01)	0.02 (0.01)	0.02* (0.01)	0.01 (0.01)
Retail	0.00 (0.01)	0.03** (0.01)	0.02** (0.01)	0.02 (0.01)	0.01 (0.01)
Transportation	0.00 (0.00)	0.00 (0.00)	−0.00 (0.00)	0.01 (0.00)	0.00 (0.00)
Service to Business	−0.00 (0.01)	0.02*** (0.01)	0.01 (0.01)	0.02*** (0.01)	0.02*** (0.01)
Services to households	−0.00 (0.00)	−0.00 (0.00)	0.00 (0.00)	0.01* (0.00)	−0.00 (0.00)

Note: All results featured herewith correspond to a Gaussian kernel matching method, applied to time differentiated variables. The standard deviation of the estimator is in brackets, estimated by block bootstraps in areas. Three (respectively two, one) stars indicate a 1% significance (respectively 5%, 10%).

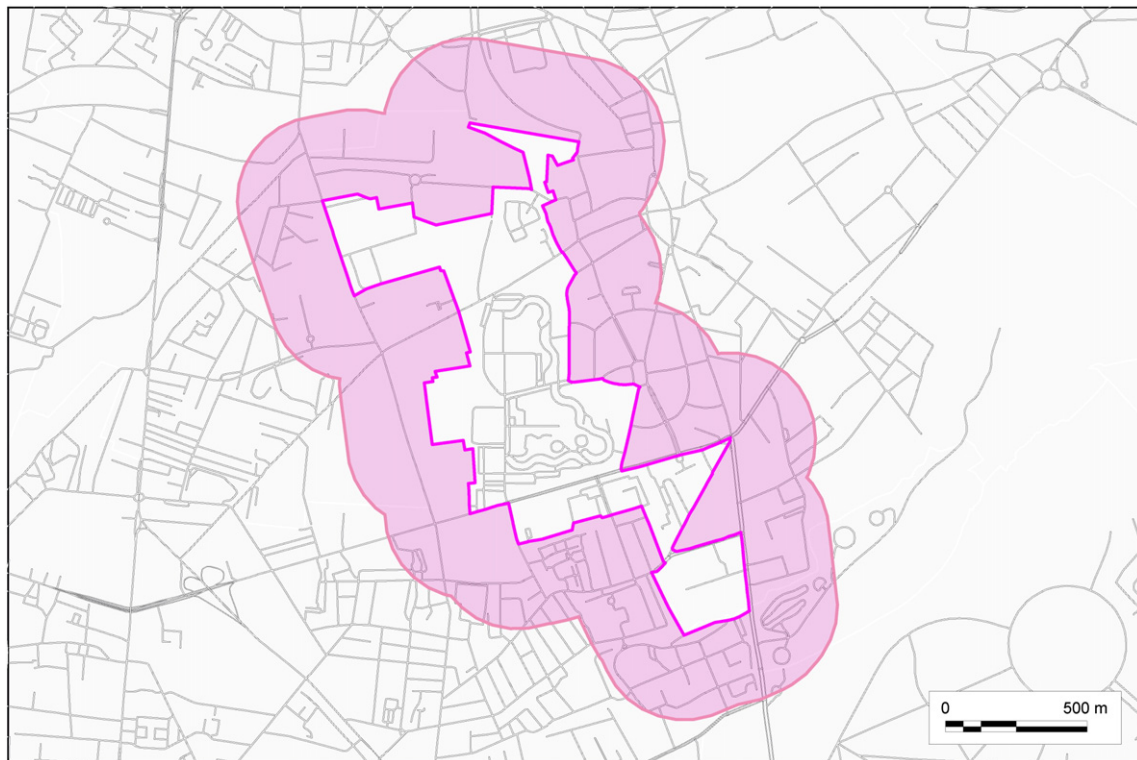


Fig. 9. The ZFU and its 300-meter-wide ring in Stains, in the Northern banlieue of Paris.

strong heterogeneity across industries. Service to business is the industry that benefited most from the program showing both significantly higher growth and creation rates. Retail also appears to have benefited from the program with significantly higher rates of new businesses in 2004 and 2005; growth rates of the stock are also higher but not significantly. Evidence is also mixed for construction with a significant effect on the growth rate in 2007 and on the flow rate in 2006. Conversely, the program had no impact on the activity of transportation and services to households.

6. Spatial externalities

The relative importance of business relocations among the new businesses raises concern about the existence of negative geographical externalities of the ZFU program, at least in the short run. Spillovers could also have affected the location of business births: an entrepreneur willing to create a new establishment at a given locality may relocate the premises if a ZFU is created nearby and set up within the ZFU instead. If theory could predict such negative spillovers in the short run, the geographical concentration of a growing number of companies may also bring about, in the longer run, positive externalities due to agglomeration economies.

We do actually observe that the program produced significant negative spillovers on economic activity within the close neighborhoods of the targeted areas during the period under study. More precisely, we address the issue of the existence of externalities by measuring the impact of the program not on the treated area itself but on a neighboring ring around the treated area. We build a 300-meter-wide ring of land around all the ZFUs (meaning surfaces of land which are comparable to that of the interior of a typical ZFU).²⁴ Fig. 9 displays what the ring looks like in the case of the Stains ZFU. However, the issue here is somewhat different to that discussed in previous sections. We do not measure the impact of local tax subsidies on targeted areas, but the impact of being near some

areas benefiting from these tax subsidies. The control areas, for the sake of comparability, are similarly-built 300-meter-wide rings around the ZRUs which were not included in the ZFU program in 2004. The same setting as previously for the matching procedure: normal-kernel matching with caliper, and full bootstrap for inference. As the program assignment decision was based on the characteristics of the zones themselves, we match a treated ring and a control ring in terms of the propensity scores of the corresponding ZRUs if close enough.²⁵

Summary statistics (see Table 9 in Appendix B) suggest that treated and control rings are very close in terms of characteristics prior to the introduction of the ZFU policy. If there is a scale difference between rings around ZFUs and ZRUs, the former being on average twice as large as the latter, social indicators (measured in 1999) and business creation rates (measured in 2002) are almost identical. A falsification test similar to the one used in the previous section, confirms that the rings around untreated ZRUs are a plausible control group for the rings surrounding ZFUs. Using the propensity score matching for outcomes observed in 2003 we do not find any significant differences between treated and control rings (Table 8).

In contrast, we observe a significant decline in the growth rate of the number of establishments located in the rings surrounding a ZFU, with respect to the rings surrounding ZRUs. We can also see a negative impact of the inflows of new establishments. This decline seems to affect slightly more the relocations than the births of new establishments. The evidence of direct effects on employment were rather tenuous, so are spillover effects. If most coefficients are negative, only one, relating to the number of hours worked in 2004 is significantly different from zero. Finally, we note that spillover effects on economic activity have more or less the same magnitude as direct effects. This suggests that most of the direct impact of the ZFU program is compensated by losses in the neighboring territories, just beyond the ZFU boundary.

²⁴ The mapping technology used to build these areas is quite similar to that used in Neumark and Kolko (2010).

²⁵ In other words, the propensity score used to match treated rings with untreated ones is related to the characteristics of the interiors of the zones and not of the rings themselves.

Table 8
Impact of the ZFU program on a ring around the treated area.

Variables	Years				
	2003	2004	2005	2006	2007
Number of establishments	0.01 (0.03)	−0.05** (0.02)	−0.02 (0.02)	−0.09*** (0.04)	−0.08* (0.05)
Δ Employment	0.03 (0.06)	−0.07 (0.06)	0.02 (0.06)	−0.08 (0.07)	−0.04 (0.06)
Δ Hours	0.05 (0.06)	−0.10* (0.05)	0.01 (0.05)	−0.05 (0.07)	−0.07 (0.10)
<i>Flow (relatively to the previous stock)</i>					
Δ New establishments	−0.05 (0.06)	−0.05 (0.04)	−0.03* (0.02)	−0.07** (0.03)	−0.17 (0.13)
Δ Births	−0.06 (0.06)	−0.04 (0.04)	−0.01 (0.02)	−0.01 (0.02)	−0.06 (0.04)
Δ Transfers	0.01 (0.00)	−0.01 (0.01)	−0.02* (0.01)	−0.06*** (0.02)	−0.12 (0.09)

Note: All results featured herewith correspond to a Gaussian kernel matching method, applied to time differentiated variables. The standard deviation of the estimator is in brackets, estimated by block bootstraps in areas. Three (respectively two, one) stars indicate a 1% significance (respectively 5%, 10%).

These spatial spillovers are very localized and their influence decreases with the distance to the ZFU. With a ring too wide, the spillover effect will be diluted and will become impossible to detect statistically. For example, using the union of all municipalities intersecting the ZFU instead of the 30-meters rings show that, at this scale, spillover effects are too diluted to be detected.²⁶

7. Conclusions

Overall, we observe a significant positive impact of the *Zones Franches Urbaines* place-based tax-exemption program on economic activity, in contrast with part of the empirical literature dedicated to the evaluation of place-based tax-exemption programs. This impact is mostly due to the fact that new companies are created or relocated in the treated areas. These effects are heterogeneous across industries: services to businesses are the industry that benefited most from the program. Somewhat surprisingly, the impact on preexisting companies is almost never significant, despite substantial financial transfers. Moreover, the policy has substantial negative spillovers on neighboring areas: the presence of a ZFU depresses the location of new businesses in the immediate vicinity of the treated area.

Despite a positive impact on economic activity overall, the policy seems to have missed part of its target: businesses which were located in the treated areas before the treatment started experienced no improvement whatsoever. While these establishments were as eligible as new ones for tax exemptions, their survival rates, their employment levels, as well as their economic situation were essentially unaffected by the program.

A possible explanation is that the potential positive impact of the program was offset by the increased competition induced by the creation of the new businesses.

This result is in line with evidences from the US: Greenbaum and Engberg (2004) observe that the average null impact of the Enterprise Zone resulted from the opposite effect on new and already existing establishments within the zone (see also Bondonio and Greenbaum, 2007; Ham et al., 2011).

While the ZFU policy has managed to bring some jobs back to distressed areas, it has been less successful in bringing employment to local job-seekers. Using public-employment-service data, Gobillon et al. (2010) find job-seekers living in cities in which the ZFU program has been implemented exit unemployment only slightly faster than those living elsewhere. Their results are compatible with two

explanations. Firstly, at the city-level, the net impact of policy minus its spatial externalities may be too weak to be detected. Second, more local jobs do not necessarily mean less local job-seekers. According to the results of a survey conducted in 2008 targeting local authorities and some companies located in the ZFU areas, companies declare major difficulties into hiring employees inside the area (and minor but not major difficulties, to hiring from outside the area) in more than two thirds of these areas. This fact could question the efficiency of local tax exemptions as a tool to reduce local unemployment, especially in areas where workers lack basic skills. One possibility could be to combine place-based tax exemptions to increase labor demand with place-based training programs to improve the quality of local human capital. Another possibility is to directly target individuals rather than businesses. The evaluation made by Gutierrez Romero (2009) of the 'New Deal for Communities' program implemented in the UK suggests that area-based financial aids to residents of deprived neighborhoods can improve local employment.

Beside labor market aspects, the above-mentioned survey reports that companies complained about the lack of available land, and more specifically of commercial real estates. A shortage in real-estate supply could hamper the efficiency of the policy, as landlords, either by increasing selling prices or rents, may capture part of the money. Finally, the relative failure of the policy could be due to a lack of information about the policy or uncertainty about the local application of fiscal rules, as emphasized by the local survey. Some companies would, for instance, waive their rights to tax exemptions in order to avoid tax inspection. As shown by Kolko and Neumark (2010), the way the areas are monitored by zone administrators can make a difference in the final assessment of the policy.

Appendix A. Tax exemptions in ZRU and ZFU

According to the 2007 ONZUS (Observatoire National des Zones Urbaines Sensibles) report, companies in ZFU and ZRU benefit from the following tax exemptions:

Local business tax: only companies with less than 50 employees and a total revenue of less than €10 million are concerned by ZFUs. Businesses located in ZFUs are fully exempt for five years, within the limit of a taxable amount of €337,713 (2006) and €100,000 of cumulated aid over 3 years. In ZRUs, all businesses with less than 150 employees are eligible. However, the ceiling of the taxable amount is lower: it amounted to €125,197 in 2006.

Property tax on buildings: all buildings located in ZFUs belonging to companies liable for this tax are exempt for 5 years. ZRUs are not concerned.

Corporate income tax: companies located in ZFUs with less than 50 employees and a total revenue of less than €10 million are exempt for 5 years, within the limit of €100,000 per year (increased by €5000 per new recruit). In ZRUs, this exemption is limited to newly created companies in the area, with no staff limitations. These benefit from full exemption for 2 years and decreasing exemption for the next 3. Corporate income tax exemption is limited to €225,000 per 36-month period.

Employer contributions: companies located in ZFUs with less than 50 employees, a total revenue of less than €10 million and one third of whose staff live in the ZUS where the ZFU is located are concerned. Employees with open-ended contracts or fixed-term employment contracts of more than 12 months are exempt from employer contributions for 5 years, on the fraction of salary lower than 1.4 times the minimum wage (Smic). This measure concerns companies with less than 50 employees in full-time equivalent and is limited to a cumulated €100,000 over 3 years. In ZRUs, there is no staff or total revenue limitation for the company, but this exemption only relates to new recruits and lasts for 12 months (maximum of 50 jobs in full-time equivalent).

²⁶ The results are available from the authors upon request.

Furthermore, in ZFUs, the exemption from local business tax, corporate income tax and property tax is prolonged on a decreasing basis for 3 or 9 years, depending on whether the company has more or less than 5 employees.

Table 9

Economic situation in the rings surrounding ZFUs and ZRUs in 2002.

	300-meter-wide rings around...	
	untreated ZRUs	ZFU
Total population	5341	9620
Area	2.93	4.59
Tax potential	445	447
Youth proportion	0.32	0.34
Dropout proportion	0.20	0.18
Unemployment rate	0.17	0.15
Subsidized housing	0.32	0.33
Number of companies	226	426
Number of workers	2000	5271
Annual number of...		
New businesses	34	63
Births	25	46
Relocations	8	16
Divided by the stock of the previous year		
New businesses	0.14	0.15
Births	0.11	0.11
Relocations	0.03	0.04
Number of rings	284	41

Appendix B. Neighborhoods of ZFU

Table 9 compares the economic situations in the 300-meter neighborhood rings built around ZFUs and untreated ZRUs.

Appendix C. Supplementary data

Supplementary data to this article can be found online at <http://dx.doi.org/10.1016/j.regsciurbeco.2012.06.006>.

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