

GENES

ADRES

Do Enterprise Zones Help Residents? Evidence from France

Author(s): Pauline Charnoz

Source: *Annals of Economics and Statistics*, No. 130 (June 2018), pp. 199-225

Published by: GENES on behalf of ADRES

Stable URL: <https://www.jstor.org/stable/10.15609/annaeconstat2009.130.0199>

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at <https://about.jstor.org/terms>



GENES and ADRES are collaborating with JSTOR to digitize, preserve and extend access to *Annals of Economics and Statistics*

JSTOR

DO ENTERPRISE ZONES HELP RESIDENTS? EVIDENCE FROM FRANCE

PAULINE CHARNOZ^a

Enterprise zones are place-based policies whose purpose is to help people who live in deprived neighborhoods. They can affect equilibria on both the labor and housing markets. Using the French case, the paper proposes results on the impact of enterprise zone programs for residents, focusing on displacement and social composition effects that have not been much studied. The paper first shows that the unemployment rate of French EZ residents has decreased significantly as a result of the program and that it is due, in part, to the fact that some exemptions were made conditional on local hiring. In the long run, some composition effects occurred in the targeted areas and the decrease of unemployment was partly due to an increase in the share of high-skilled residents.

JEL Codes: R10, J00, R580.

Keywords: Urban, Evaluation, Enterprise Zone, Place-Based Policy.

INTRODUCTION

Enterprise zone programs (hereafter EZ) were launched in the 1970s in the UK as a remedy for deprived urban neighborhoods. Numerous programs have been implemented subsequently: in the US since the 1980s and in France since the 1990s. They are based on more or less generous tax exemptions granted to firms located in chosen areas. They generally target labor and/or capital expenditures. EZ programs are still quite popular (Jibrayel, 2013), in particular among local politics, but they are costly and the urban problems they were meant to solve are still pervasive decades later. In France for instance, in August 2012, outbreaks of violence occurred in poor urban neighborhoods. More generally, the unemployment rate is still much higher in these zones (24% in 2010 against 9.5% for the whole country, ONZUS (2011)). Evaluating these programs is therefore important from a public policy point of view. But Neumark and Simpson (2014), Lynch and Zax (2011) and Ham, Swenson, Imrohorglu, and Song (2011) reviewed the evidence on EZ, and found it difficult to achieve a general statement about their efficiency, despite numerous studies. EZ can in fact be evaluated on various dimensions as they affect equilibria on both the labor and housing markets. Labor market outcomes from the firm or the worker point of view, land prices and displacement effects are therefore all relevant outcomes. For the French case, there are substantial evidence on firm outcomes and mixed evidence on land prices. This paper provides new results on the effect of French EZ on the labor market outcomes of those who reside in these zones. And it focuses on displacement and social composition effects that have not been much studied (some exceptions on US enterprise zones are Freedman (2013) who found the observed impacts of the US empowerment zones on neighborhoods are attributable to changes in the composition of residents and Reynolds and Rohlin (2015) who found evidence that they became more attractive to high-income households) but are quite crucial for understanding the mechanism of EZ.

The goal of EZ programs is the decrease of territorial inequalities through an increase in economic activity and an improvement of the labor market situation of residents of

^aDares, 39-43 quai André Citroën 75015 Paris. pauline.charnoz@travail.gouv.fr

deprived neighborhoods. These programs are a form of “territorial affirmative action”, since they introduce a break in the equality of treatment between territories. In order to evaluate them from a public policy point of view, it is therefore necessary to understand the determinants of the inequalities between neighborhoods and the efficiency of EZ program to reduce them. Determinants of inequalities between neighborhoods can be divided into three main sources : exogenous differences such as infrastructure or natural endowments, the effect of “space” or “distance” on the labor and housing markets and social composition effects due to a sorting of residents or firms (Gobillon, Magnac, and Selod (2011)).

EZ programs are focused on the second channel, that is often presented as spatial mismatch (Gobillon, Selod, and Zenou (2007)) : distances between job and residential locations prevent some people from finding a job. EZ programs implicitly assume that bringing jobs closer to residents is a solution to their deprived situation. But bringing jobs into the zones might not be enough. Indeed, the financial incentive may be enough to have firms move into the area but they will not necessarily hire those living nearby, in particular, if the skills of residents do not correspond to the needs of the firm. In other words, spatial mismatch might not be the only reason for the difficulties experienced by the residents on the labor market; other factors such as skills mismatch or discrimination could be responsible too. In that case and if the cost of commuting to the zone is not too high, firms would keep on employing people from outside the zone. Briant, Lafourcade, and Schmutz (2015) have indeed shown on French data that the less isolated EZ attract more firms : this is an important clue that the commuting possibilities may play a part in the success of EZ. ¹

To counteract this mechanism, some programs have a local hiring condition : exemptions are granted only if a percentage of the workforce resides in the zone. The addition of this condition is an indirect indication that bringing firms into the zone is not enough, and thus that spatial mismatch alone cannot account for the poor performance of residents on the labor market. The presence of this condition may hinder firms from hiring outside the zone, but it may then raise the incentives for workers with profiles different from those of the EZ to move into the EZ. Gottlieb and Glaeser (2008) summarized this concern this way “place-based policies that throw enough resources at a small community may indeed be able to improve the quality of that place, but it is not obvious that the poorer residents of that community will benefit. Some community-based policies may just lead employers to come to the area and hire new migrants.”

The French enterprise zone program was implemented in 1997, then renewed and extended in 2004 and 2006. It grants very large tax exemptions to firms located in the enterprise zones and it has a local hiring condition on the payroll exemptions only : they are granted only if at least 20% of the workers are residents of the enterprise zone, 33% after 2002. Most econometric evaluations of French EZ used establishment level data and are thus concentrated on firm outcomes. Rathelot and Sillard (2008), Givord, Rathelot, and Sillard (2013), Mayer, Mayneris, and Py (2017), Trevien, Givord, and Quantin (2012) and Briant, Lafourcade, and Schmutz (2015) all studied business creations and/or firm employment located in the zones. They found a small positive effect of the EZ on these outcomes for the 2004 program, and a stronger effect for the one of 1997 (about 50 000

¹The firms might also be motivated by the ease of reaching suppliers or customers.

jobs after 5 years for 38 zones, (Trevien, Givord, and Quantin, 2012)). Gobillon, Magnac, and Selod (2012) partly address the issue of the impact on residents by studying the unemployment exit rate of the EZ residents, controlling by their characteristics. But they provided results for the Paris region, and at a broader spatial level than the zone itself (since they observed only municipalities containing an EZ). Although they studied the 1997 EZ for which the effect on firms was quite strong, the effect they found is both small and temporary. Because of the local hiring condition, these results may appear contradictory. This analysis complements and reconciles these results by showing that, while there was a significant effect on the employment of residents, it was to some extent driven by an effect on the social composition of the zones. The paper therefore both extends the results of Gobillon, Magnac, and Selod (2012) to the whole territory and provides evidence on EZ effects on social composition of the targeted areas. The analysis shows that the program impacted not only the location of firms but also the location of the residences of the workers and thus the social composition of the EZ.

More precisely, the unemployment rate of residents of 1997 French EZ has significantly decreased thanks to the program, and this effect strengthened when the local hiring condition tightened. This hints at the higher unemployment rate in the EZ being probably caused more by skills mismatch or discrimination than by spatial mismatch. Second, a significant part of the effect was driven by composition effect, i. e. the unemployment rate of the EZ decreased because of an increase in the share of high-educated residents in the zones.

Section 1 presents the French enterprise zone program ; section 2 describes the evaluation strategy. Results are presented in section 3 and discussed in section 4. Section 5 concludes.

1. THE FRENCH PROGRAM OF ENTERPRISE ZONES

This section presents the design of the French EZ program and the characteristics of the priority zones before the start of the program.

1.1. *The Design of the Program*

The "Pacte de Relance pour la ville" (Urban Stimulus Package) was enacted in 1996 (effective in 1997). It created 3 types of priority zones, roughly nested. First 751 ZUS (Zones d'Urbanisation Sensibles, sensitive urban zones) were defined. The choice was inspired by previous urban programs and by qualitative criteria such as "derelict housing stock" and "unbalance between residential and working zones." Second, among these ZUS, 416 ZRU (Zones de Redynamisation Urbaine, urban redynamisation zones) were then defined, and, third, from that set, 44² ZFU (Zones Franches Urbaines, urban enterprise zones, hereafter designated as ZFU 1G) which were the most disadvantaged according to a synthetic index of deprivation. This index was computed using the unemployment rate, the proportion of residents under 25 years old, the proportion of residents without a diploma of the zones and tax potential of the hosting municipality.³ The ZFU program was renewed and extended to 41 new zones in 2003 (effective in January 2004, hereafter designated as ZFU

²38 without French overseas territories

³The tax potential is defined as a theoretical product of local taxes in case the average national rate were applied to the municipality for each of the local rates.

2G) and 15 in 2006 (effective in 2006, hereafter designated as ZFU 3G) thus creating a second and third generation of ZFU. The map in figure 1 shows that the 3 generations of ZFU are located all over the French territory in major urban areas and that an important share is located in the Paris region.



Source: French Urban policy department

Figure 1: Map of French Enterprise Zones

The ZFU are the French equivalent of US and UK enterprise zones. Firms located in the ZFU can benefit from large tax exemptions (corporate income tax, local business tax, payroll tax) for 5 years. The payroll tax-exemption is fully granted to the portion of wage below 1.4 times the minimum wage and partially up to 2 times the minimum wage. These exemptions are granted to firms with less than 50 employees, and to new firms as well

as to firms already in the zone before designation. As a comparison, firms in the ZRU benefit from exemptions for only one year and only when they are new incomers. The ZRU are thus very lightly treated in comparison to the ZFU (see table A1 for a detailed comparison). There are no systematic exemptions for the ZUS.

In 2002, apart from the creation of new ZFU, the program was also renewed for the existing ZFU, and a progressive exit from the program was implemented : instead of a total halt of the exemptions after 5 years, a degressive rate is applied to the exemptions over the following 3 to 9 years. A firm can thus be treated for up to 14 years.

Another key feature of the program is the fact that payroll tax exemptions (but not the other exemptions) are made conditional on local hiring : a proportion of at least 20% of employees must be living in the zone for the firm to benefit from payroll tax exemptions. The threshold was extended to 33% in 2002 and 50% in 2012. In 2002, it was also extended to a larger population that included not only residents located in the ZFU but also those located in the ZUS of the same urban unit⁴ as the ZFU. The residents of a ZUS (or ZRU) who are in the same urban unit as a ZFU can therefore be directly impacted by the program since 2002. Lastly the only jobs eligible for the payroll tax-exemption condition are the ones that employ workers on long-term contracts, a category that covers open-ended contracts (CDI, "contrat à durée indéterminée") and fixed-term contracts of more than 12 months.

1.2. *Some Descriptive Statistics*

In this part, I describe the 3 types of zone in 1990 before the start of the program and compares them to the municipalities where they are located (hosting municipalities). I look into some labor market characteristics and some characteristics used explicitly or implicitly in the choice of the zones.

I compare in columns 1 and 2 of table I all the priority zones of the program (ZUS) to their hosting municipalities. The unemployment rate in the priority zones is on average twice the one of hosting municipalities. Residents are younger with an average 31 compared to 37 and a share of residents under 25 of 47% to 50% against 41%. They are less graduated with a share of residents without a degree of 57% compared to 50%.

"Derelict housing stock" is mentioned as a criteria in the designation of the priority zones. No direct measure is available for the housing quality but these neighborhoods were famous for large and old social housing. Indeed the share of social housing is very important and is an important feature of these zones. In the priority zones (ZUS) more than 60% of the population lives in social housing while it is only 15% in the hosting municipalities. The average number of persons per room living in a dwelling is also used as a measure of low housing quality. It is higher in the priority zones than in the hosting municipalities.

The situation of young residents is often pointed out by media or politics as a factor for outbreak of violence in these neighborhoods. Indeed 15-25 year old residents of the priority zones are less in studies or employment. Lastly, segregation issues are often linked to immigration and integration issues in the public debate and these zones are indeed

⁴Urban units are defined by the INSEE (French national institute of statistics) as a municipality or a group of municipalities forming a continuous built-up zone (no space of more than 200 meters between any two buildings) and with at least 2000 inhabitants.

TABLE I
CHARACTERISTICS OF THE DIFFERENT TYPE OF ZONES IN 1990

	Municipalities with a ZUS	ZUS	ZRU	ZFU1G
Average age	37	31	31	30
Share of under 25 year old	41%	47%	48%	50%
Share of men	45%	47%	47%	48%
Share of no diplomas	50%	57%	58%	60%
Unemployment rate	9%	18%	19%	20%
Unemployment rate of 15-25 year old	20%	28%	30%	31%
Share of 15-25 years old studying	54%	48%	48%	47%
Share of 15-25 years old not in employment nor studying	14%	20%	20%	21%
Share of foreigners from European Economic Community	2%	3%	3%	3%
Share of foreigners from outside EEC	4%	15%	15%	17%
Share of French by naturalization	3%	5%	5%	5%
Share of French by birth	84%	74%	75%	72%
Share of public housing	15%	63%	67%	68%
Number of persons per room of the dwelling	0.85	1.05	1.05	1.09
Average population size of a zone	44993	6717	7221	19136

Source: 1990 Census

Note: 1990 European Economic Community definition is used : the members are Denmark, France, Germany, Greece, Ireland, Italy, Luxembourg, Netherlands, Portugal, Spain, United Kingdom.

zones with a much higher share of foreign residents with a nationality from outside the European Economic Community.⁵

Columns 3 and 4 of the table I present the same measures for ZRU and ZFU1G and they are indeed the most deprived among the priority zones (ZUS). ZFU1G are also much more populated, approximately 19 000 residents on average compared to roughly 7000 for the other zones, which is due to the fact that having a population over 10 000 was a criteria to become a ZFU.

2. EVALUATION STRATEGY

2.1. Data

Previous studies on the French case have shown that a significant number of jobs were created by firms in the ZFU first generation (Trevien, Givord, and Quantin (2012)) and second generation (Rathelot and Sillard (2008)). The purpose of this evaluation is therefore to study the effects of the EZ program on the residents and on the social composition of the zones. It is necessary to use data that includes information on the location of the residences of the workers. The French Labor Force Survey is a survey conducted at the main residence of households, and gathers fine geographical information (at the census block level, finer than municipality). All household members aged 15 and over are interviewed. The survey generates rotating panel data (dwellings are interviewed several times) and the sample is stratified and clustered. Areas of 40 dwellings (or 20 in cities of more than 100 000 inhabitants) are sampled, and all the dwellings in each area are surveyed.

From 1993 to 2002, surveys were annual and conducted in March. Each dwelling was

⁵In 1990, the members are Denmark, France, Germany, Greece, Ireland, Italy, Luxembourg, Netherlands, Portugal, Spain, United Kingdom.

interrogated thrice. A third of the sample was renewed each year. The sample rate was 1/300 and there were thus approximately 75 000 dwellings per year. Since 2003, surveys have been conducted every quarter. Each dwelling is interrogated 6 times and a sixth of the sample is renewed each quarter. The sample rate is 1/600 per quarter and there are approximately 36 000 dwellings per quarter. Note that the unit of observations is the dwelling, hence when people move in or out a dwelling, they also enter or exit the sample. The weights provided by the producer (INSEE) are used for statistics and estimations.

This data set is of particular interest for this evaluation, as the geographical level is sufficiently fine to identify the priority zones, and it provides a great deal of information on the labor market situation of residents. The available geographical information allows to know if a dwelling is located in a priority zone (ZUS) or in a one of the 3 generations of ZFU but the information about the ZRU is available only in the data prior to 2002. A proxy for ZRU status is thus used which is the fact of being a priority zone (ZUS) but not a ZFU and being located in a municipality containing a ZRU. A comparison has been made between these ‘approximated’ ZRU and the actual ones in the 1990 Census on various dimensions such as gender, age, share of people without diplomas, unemployment and they are indeed almost identical (results available upon request).

Last, a comparison of the sample of priority zone (ZUS) and ZFU of the first generation (ZFU1G) in the 1999 LFS to the 1999 Census confirms that the LFS sample is representative along various dimensions (results available upon request). In the main sample (the one for unemployment estimations), there are on average around 2000 individuals per year before 2002 and 900 per quarter after 2002.

2.2. Econometric Strategy

The empirical strategy is based on a standard Difference-in-Difference method (hereafter DD), implemented on the treated group (ZFU residents) and a control group that is described in the next section. As the sample is clustered, it is very likely that errors for individuals of the same area are correlated. To take it into account, data is aggregated at the sampling area level as suggested by Bertrand, Duflo, and Mullainathan (2004). To deal with the serial correlation problem due to the rotating panel pattern and take into account potential heteroscedasticity, I allow for a correlation between the errors of each sampling area and compute a White style robust covariance matrix.⁶ In that case, the estimation of the variance of the estimator does not require the number of observations of a sampling area to be the same. Here, this is important as some sampling areas at the beginning and end of the period are observed only once or twice (due to the sample design).

The model is then the following :

$$Y_{it} = \alpha ZFU_i + \beta ZFU_i * 1_{t \geq 97} + \gamma_t + \lambda_{uu} + u_{it}$$

with

$$\widehat{Var}(\widehat{\beta}) = \frac{(\sum_{i=1}^N \widehat{x}_i' \widehat{x}_i)^{-1} (\sum_{i=1}^N \widehat{x}_i' \widehat{u}_i \cdot \widehat{u}_i' \widehat{x}_i) (\sum_{i=1}^N \widehat{x}_i' \widehat{x}_i)^{-1}}{N}$$

with i indexing a sampling area, t the year (or year \times quarter after 2002), N the total

⁶see Wooldridge (2009) for more details.

number of sampling areas and I_N the identity matrix. Y is the outcome of interest (for instance the unemployment), ZFU is a dummy for being in a treated zone, γ_t is a year (or year \times quarter after 2002) fixed effect, λ_{uu} a urban unit fixed effect, u the error term and x the set of covariates.

The β coefficient in the linear regression gives the effect of the program under the hypothesis that the difference between treated and control would have been constant over time in the absence of treatment. The urban unit fixed effect allows to control for local specificities. In the case where there is only one type of zone (treated or control) in each urban unit, the pre-treatment control ZFU_i is redundant. A sampling area fixed effect could not be used because of the structure of the data : too few sampling areas are observed before and after the start of the program. $\widehat{Var}(\hat{\beta})$ is the White style robust covariance matrix.

Regressions are estimated on the 1993-2007 period.⁷ This means that the effect is estimated up to 10 years after the start of the program. The sample is restricted to the 15-65 year old residents as the interest is mostly in labor market outcomes. I chose to run estimations using sampling weights based on Davezies and D'Haultfoeulle (2009) results which suggest it is more robust.

2.3. Choice of the Control Group

The validity of the estimation strategy strongly relies on the choice of a control group. This section presents the chosen group and alternative choices.

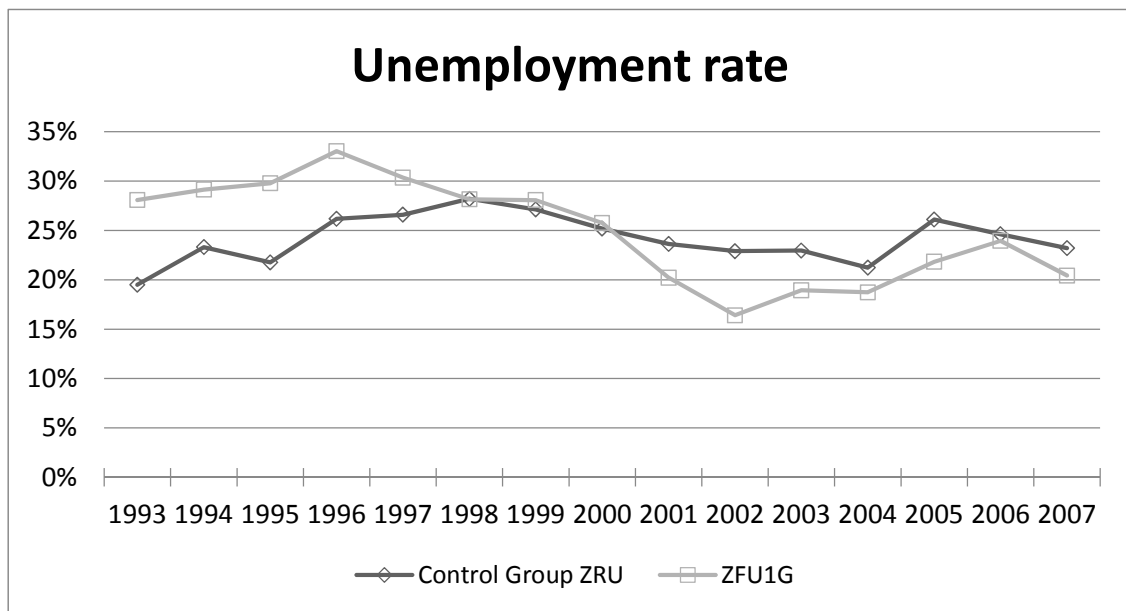
ZFU have been chosen from among ZRU, which are very specific urban neighborhoods as seen in the descriptive statistics. The control group has therefore been chosen among the remaining ZRU. The ZFU have been chosen among the most deprived ZRU according to some social indicators (aggregated to construct the synthetic index mentioned earlier). By nature they were distinguished by a different level of deprivation, but since all ZRU are deprived zones, I can also assume (and partly test) that their evolutions were similar before designation and would have remained similar without the program. The estimated effect is thus the effect of being a ZFU compared to being a ZRU. Only the implementation of first-generation ZFU is evaluated here (38 zones treated in 1997). There are too few observations of 3rd generation zones in the sample and the placebo tests rejected control groups among the ZRU for the 2nd generation. Several ways to construct a control group from among the set of ZRU were tested :

- First simply all ZRU. (control group 1)
- ZRU which are too close geographically to the ZFU might be affected by the treatment. A second approach excluding the ZRU that are in the same urban unit of a ZFU is thus tested. (control group 2)
- Third, since there have been several waves of the program, the zones that became ZFU later (in 2004 and/or 2006) could be used as a control group for the first generation. I may assume that the ZFU of the subsequent generations have common features with those of the first generation. This is a standard strategy in the evaluation literature. (control group 3)

⁷Estimations on the 1993-2011 period have been performed and the results are similar. The 1993-2007 period is preferred as it excludes the 2008 crisis and also reduces the difference in number of years before and after treatment.

- Lastly, a very common approach in the literature is to use a propensity score matching method : I estimate the probability of being treated according to some characteristics. Each zone is assigned a score according to this model, and each treated zone is matched to the non-treated zone with the closest score (closest neighbor method). I test this strategy using information about the zones from the 1990 census (population size, share of 15-25 year olds, unemployment rate, share of public housing) to find a match for the ZFU1G among the ZRU that are not located in the same urban unit. (control group 4)

In any case, observations of the ZRU which became ZFU later (2G or 3G) are excluded after they turned into a ZFU (in 2004 and 2006). Moreover, because of the extension in 2002 of the local hiring condition to all residents of the priority zone (ZUS) falling within the same urban unit as a ZFU, the observations of ZRU in the same urban unit as a ZFU are excluded, but only after 2002 and after the ZFU is created. As these cases are not too numerous, it is simpler and not too costly to exclude them rather than keeping them and controlling for all these events.



Source: French Labor Force Survey, 1993-2007

Figure 2: Unemployment Rate of Treated and Control Group

To choose between these groups, I run placebo tests for various outcomes. The idea underlying placebo tests is to check that the treated and control groups are really similar in trend before the treatment of 1997. The test can be performed only if several years of observation before the treatment are available, since it is necessary to measure not only the difference between the two groups but also their time trends. The years 1993 to 1996 are used. A regression is then estimated with a “fake” or placebo treatment in the years 1994, 1995 and 1996 while controlling for the 1993 initial difference in level of the outcome between the two groups. Time and urban unit fixed effects are also included.

Tests can be run on various observable characteristics. Results are available upon request for some socio-demographic characteristics used in the choice of the ZFU relatively to ZRU : age, education and the nationality of residents (the latter is not in the index but immigration issues are often mentioned in relation to urban issues). Placebo tests are also

run for various labor market outcomes with and without control variables (table A3 for unemployment and other results available upon request) as both specifications are next estimated.

In most cases, the placebo treatment is not significant which means that the control and treated group do have similar trends on observable characteristics before the start of the program. A few placebo treatments for some variables are slightly significant but without a pattern except for the share of people with a low degree when using the ZRU that will be ZFU in the second and third generation as a control group (control group 3). A few placebo treatments are also significant for the matched ZRU (control group 4). After the placebo test, a criteria I retained to choose among valid control groups is the number of observations to get as much power as possible in the estimations. For this reason, all the ZRU (control group 1) could have been kept but those belonging to the same urban unit of a ZFU could be impacted by the program. And they would contribute only before 2002 as after that date they are excluded because of the modification of the hiring condition that impacts them. ZRU that are not in the same urban unit of a ZFU (control group 2) is thus the chosen control group for the estimation of the effect of the program.

Although our placebo tests show that treated and control group have similar trends before the treatment (see also figure 2), the strategy still relies on the assumption that the trend would have remained similar without the program. It could be argued that ZFU have been selected because of their expected reaction to treatment or because of their expected economic trend. One limit of our strategy is that it does not control for this potential dynamic selection. Since ZFU were chosen among ZRU with a synthetic index, in principle, I could implement a Regression Discontinuity Design (RDD) estimation. I could compare ZRU and the ZFU with close values of the index and argue that the fact to be just under or above the threshold is exogenous. This could allow to get rid of the dynamic selection issue. The rule was indeed not strictly applied but no clear information is available on other considerations that might have entered in the process of ZFU designation. A fuzzy RDD strategy might be possible but I do not have enough observations to implement this strategy. I therefore prefer to rely on a DD strategy rather than a RDD strategy to estimate the effects of the French ZFU program and assume that the trends of the control and treated groups would have remained parallel if the program had not been implemented.

3. RESULTS

The results are presented with and without control variables. I use as control variables : gender, age, age squared, level of education, nationality. For education, residents who finished their studies are split in 3 groups : low degrees (up to junior high-school), medium degrees (low vocational and high-school degrees) and high degrees (college and university degrees). For nationality, the share of residents with a nationality from outside the members of the European Economic Community is computed.⁸ All control variables are aggregated at the sampling area level and are time-varying.

The social composition of the zones might have changed during the studied period for reasons unrelated to the program. Placebo tests with and without controls show no difference between control and treated group before the treatment on outcome variables

⁸More precisely, here the members of EEC in 1999 (except Finland that could not be identified in the data) that are : Austria, Denmark, France, Germany, Greece, Ireland, Italy, Luxembourg, Netherlands, Portugal, Spain, Sweden, United Kingdom.

and on socio-demographic characteristics. Therefore it seems reasonable to assume that phenomenons or events, other than the ZFU program, that might affect social composition, do not impact the difference between control and treated groups. Hence, if the EZ program has no effect on the social composition of the zones, adding time-varying socio-demographic controls should not alter our estimates but mostly increase our precision. Comparing results with and without controls is a first empirical way to assess the potential importance of displacement effects. The direct effect of the program on the social composition of the zones will also be studied to shed light on this issue. Note that the global effect of the policy for the zone is the effect without control variables which includes composition effects potentially induced by the program.

3.1. Unemployment and the Local Hiring Condition

Our first variable of interest is the EZ residents unemployment rate as this is a major labor market outcome and its reduction is one of the main objectives of the program.

The DD estimation shows a significant reduction – 11 percentage points – of unemployment in the ZFU1G (column 1 in table II) when its level is roughly 30% just before the start of the program. As the only previous study (Gobillon, Magnac, and Selod, 2012) evaluating the effect on residents focused exclusively on the Paris region and found a small significant effect, a regression was estimated without the Paris region to check if this region is driving the results. The effect is very close – 9.5 percentage points (available upon request) – and still significant, so results are not driven only by the Paris region.

TABLE II
DD ESTIMATIONS FOR UNEMPLOYMENT

	Unemployment rate					
	(1)	(2)	(3)	(4)	(5)	(6)
ZFU1G × post97	-0.110*** (0.029)	-0.059*** (0.022)	-0.085** (0.043)	-0.069** (0.035)	-0.055** (0.028)	-0.052** (0.022)
ZFU1G × post2002					-0.076** (0.030)	-0.011 (0.024)
ZFU1G × post97 × t			-0.004 (0.006)	0.002 (0.005)		
obs. (sampling areas × year)	2669	2669	2669	2669	2669	2669
R ²	0.312	0.462	0.312	0.462	0.314	0.462
year fixed effects	yes	yes	yes	yes	yes	yes
UU fixed effects	yes	yes	yes	yes	yes	yes
Control variables	no	yes	no	yes	no	yes

Source: French Labor Force Survey, 1993-2007, 15-65 year old.

Note: *** significant at 1%. ** significant at 5%. * significant at 10%. Estimations with sampling weights. The number of observations can be slightly different when studying subpopulation : for example when computing unemployment rate, some areas may have only people out of the labor force and are therefore not used.

Control variables : gender, age, age², level of education divided in high, medium and low degrees, foreigners from outside EEC.

According to theory (Busso, Gregory, and Kline (2013), Kline and Moretti (2013) and Kline and Moretti (2014)), EZ has an affect on unemployment if workers are not fully mobile and/or if housing supply is quite inelastic. There is a lot of social housing in these areas which could lead both to a low elasticity of housing supply and a low residential mobility of workers. Since it is very difficult to get social housing in France, residents have strong incentives not to move once they got one. However workers outside EZ might be more mobile and move into the zone. Moreover, since the local hiring condition is

much less than 100%, outside workers can also commute into the EZ and might not need to reside in the EZ to benefit from the program. Residential mobility is investigated here and commuting in section 3.4.

Residents might be “trapped” in these neighborhoods because of housing market equilibria. This is one of the mechanism of the spatial mismatch hypothesis. If the deprived situation of the residents on the labor market is mainly due to distance to jobs, bringing jobs making closer to them should be enough to improve their situation and the local hiring condition should not be necessary. Therefore testing the effect of making the local hiring condition tighter is a way to assess the importance of spatial mismatch in these neighborhoods.

The local hiring condition was tightened in 2001 (effective in 2002) : it was raised from 20% to 33% and was extended to workers residing in all priority zones (ZUS) that were part of the same urban unit as a ZFU. Column 5 in table II reproduces the previous estimation adding a dummy for an additional specific effect after 2002. The effect on the unemployment rate is indeed stronger by 7.6 point of percentage since 2002. So the tightening of the local hiring condition does appear to have had an effect.⁹

The local hiring condition has had an effect but it might not have benefited “original” residents if it happened through residential mobility, attracting more qualified workers from outside the zone. This is tested adding sociodemographic controls in the estimation : if social composition was impacted by the program, the estimation of its effect will vary whether social composition is controlled for or not. The difference between the estimations with and without controls can be interpreted as the effect of the program due to social composition changes. With sociodemographic controls, the global effect of the program is still significant but smaller (column 2 in table II).¹⁰ Hence, some composition effects have occurred, which implies some residential mobility. There is no significant effect of the tightening of local hiring condition anymore, while the 1997 initial effect of the program is still significant (column 6 in the table II). The additional effect on unemployment induced by the tightening of the local hiring condition was thus mostly due to composition effects. In other words, during the first years of the program, “original” residents got more jobs but with the tightening of the local hiring condition, some change in the social composition of the zones occurred. The further decrease in unemployment was due to a change in the social composition of the zones rather than an increase in the probability of finding a job for the residents that would have lived there in the absence of the program.

One of the requirements of the local hiring condition pertains to the type of labor contract. Hence another way to evaluate the program is to study its effect on jobs with a long term contract. There is indeed a significant positive effect of 6 percentage points of the program on private sector long term contracts share among the 26-65 year old residents (see table III).

When studying the effect of the tightening of the local hiring condition in 2002, estimations on long term contracts lack power but using controls, the 1997 effects are stronger and significant; it is consistent with the interpretation that the program had no further

⁹A progressive effectiveness of the program could be mistaken for the effect of the local hiring condition. However, since the estimation allowing for a trend in the effect is not significant (column 3 in table II), it does not seem to be the case.

¹⁰Although the coefficients are not significantly different but it may be due to a lack of power in the estimations.

TABLE III
DD ESTIMATIONS FOR THE SHARE OF LONG TERM CONTRACTS

	Share among 26-65 years old of private sector workers with long term contracts					
	(1)	(2)	(3)	(4)	(5)	(6)
ZFU1G × post97	0.060** (0.027)	0.059** (0.025)	0.033 (0.024)	0.042* (0.022)	0.051 (0.042)	0.068* (0.038)
ZFU1G × post2002			0.037 (0.028)	0.023 (0.026)		
ZFU1G × post97 × t					0.002 (0.007)	-0.002 (0.006)
obs. (sampling areas × year)	2702	2702	2702	2702	2702	2702
R ²	0.257	0.374	0.258	0.374	0.257	0.374
year fixed effects	yes	yes	yes	yes	yes	yes
UU fixed effects	yes	yes	yes	yes	yes	yes
Control variables	no	yes	no	yes	no	yes

Source: French Labor Force Survey, 1993-2007, 15-65 year old

Note: *** significant at 1%. ** significant at 5%. * significant at 10%. Estimations with sampling weights. The number of observations can be slightly different when studying subpopulation : for example when computing unemployment rate, some areas may have only people out of the labor force and are therefore not used.

Control variables : gender, age, age², level of education divided in high, medium and low degrees, foreigners from outside EEC.

effect for the “original” residents after 2002.

These set of results thus show that the local hiring condition has had an effect : without it, residents might have got less jobs or with less favorable contracts. Hence spatial mismatch was not the only cause of the high unemployment of these zones. Skill mismatch or discrimination might also explain it. Moreover the results also show that when the local hiring condition was strengthened, it did not benefit the “original” residents so its efficiency to improve their situation was limited. Is it plausible that the program could help only the workers who were the most employable but it was not sufficient for workers with a very low level of “employability”.

3.2. Composition Effects and New Residents

According to our results, the program decreased residents’ unemployment, thanks partly to the local hiring condition. Here I further investigate the changes in social composition that seem to have occurred. In column 1 of table IV, the same DD regression is estimated but on the age (as a proxy for experience) and the level of education of EZ residents which are two major determinants of labor market outcomes. It shows that since 2002, EZ residents are a bit older and more educated than they would have been without the program. The program induced a change in the resident social composition of the zone and it happens after 2002. This is consistent with the previous result that the additional decrease in unemployment after 2002 is due to composition effects.

To further examine this question, I wish to distinguish, within the zone, the arriving, staying and leaving residents. This information is not available, but thanks to the survey design, it is possible to know if the residents are living in the same dwelling as the year

TABLE IV
DD ESTIMATIONS OF COMPOSITION EFFECTS

	All (1)	New residents (2)	New residents from other municipalities (3)	New residents from the same municipality (4)
Age				
ZFU1G × post97	0.733 (0.672)	0.435 (1.033)	-0.592 (1.650)	1.375 (1.101)
ZFU1G × post2002	1.862** (0.933)	-1.170 (1.147)	-1.459 (1.616)	-1.619 (1.479)
obs. (sampling areas × year)	2713	1387	927	1080
R ²	0.36	0.22	0.31	0.25
Share of 15-25 year olds				
ZFU1G × post97	-0.007 (0.024)	-0.008 (0.056)	0.038 (0.093)	-0.042 (0.055)
ZFU1G × post2002	-0.029 (0.028)	-0.015 (0.055)	-0.095 (0.087)	0.074 (0.064)
obs. (sampling areas × year)	2713	1387	927	1080
R ²	0.27	0.25	0.32	0.27
Share with a high-degree				
ZFU1G × post97	0.007 (0.023)	0.051 (0.041)	0.010 (0.071)	0.070* (0.036)
ZFU1G × post2002	0.074*** (0.027)	0.069 (0.053)	0.178** (0.084)	-0.001 (0.060)
obs. (sampling areas × year)	2711	1342	846	1052
R ²	0.27	0.22	0.26	0.25
Share with a medium degree				
ZFU1G × post97	-0.010 (0.025)	-0.029 (0.046)	-0.046 (0.076)	-0.018 (0.058)
ZFU1G × post2002	0.013 (0.030)	0.003 (0.049)	-0.010 (0.090)	0.016 (0.062)
obs. (sampling areas × year)	2711	1342	846	1052
R ²	0.33	0.20	0.25	0.23
Share with a low degree				
ZFU1G × post97	0.003 (0.035)	-0.022 (0.054)	0.035 (0.083)	-0.052 (0.060)
ZFU1G × post2002	-0.086** (0.043)	-0.072 (0.062)	-0.168* (0.094)	-0.015 (0.073)
obs. (sampling areas × year)	2711	1342	846	1052
R ²	0.37	0.26	0.30	0.30
year fixed effects	yes	yes	yes	yes
UU fixed effects	yes	yes	yes	yes
Control variables	no	no	no	no

Source: French Labor Force Survey, 1993-2007, 15-65 year old

Note: *** significant at 1%. ** significant at 5%. * significant at 10%. Estimations with sampling weights. The number of observations can be slightly different when studying subpopulation : for example when computing unemployment rate, some areas may have only people out of the labor force and are therefore not used.

before. A new resident is therefore defined as someone who did not live in the same dwelling the year before. This is only a proxy of what it would be useful to measure, as they might have arrived from another dwelling in the same zone. However it is possible to know if they came from another municipality; so they are split between the ones coming from within and from outside the municipality.

First, the proportion of new residents with either definition was not much impacted by the program (table A2 in appendix). In a second step I examine whether the characteristics of these new residents changed because of the program (columns 2, 3 and 4 of table IV).

There is no effect of the program on the age of new residents : the aging of the residents induced by the program is not linked to new residents. It may be that the people leaving the zones are younger or that the ones staying are older. A possible story is that, before the program, the residents, when getting older and achieving a more stable situation on the labor market, tended to leave the zone but with the program and the local hiring condition they stayed more.

Results for education level are different : the new residents coming from the same municipality since 1997 and new residents coming from outside the municipality since 2002 were indeed more educated. So from the beginning of the program, people with a high degree living relatively close to the neighborhood and who had better ex ante chances to be hired, moved more inside the zone. Since they were living close by, the move might not be too costly. With the tightening of the local hiring condition people with a high degree from further away decided to move inside the zone. This means that when incentives to hire residents increased, it did not increase the probability of getting a job of the residents but it attracted people with better education and who had more chance to be hired and maybe fitted better firm requirements.

To sum up, the program in the first years seems to have achieved its goal to help some of the “original” residents but the additional effect of the tightening of the local hiring condition occurred through social composition effects.¹¹ Maybe firms that were the most likely to hire local residents were the ones already present or the ones which arrived at the beginning but, in the long term, ZFU1G have hosted firms less interested in the skills available and attracted other workers with better skills. Moreover, since workers do not adapt their residence choices at once, changes in social composition of the zones were probably slow. Hence, there was an initial effect for some of the “original” residents and in the long term an additional effect through the social composition of the zone .

3.3. *Heterogeneity of the Effect by Education Level*

A major concern of the program is also the low education level in these zones and its effect on the situation of the residents, particularly on the labor market. Indeed the education level of ZFU residents was used to compute the deprivation index. The effect of the program on the unemployment rate of residents by degree level is estimated in table V. It allows to assess if low-educated workers benefited more or less of the program than high-educated workers and therefore gives elements on redistributive aspects of the program. Results are stronger for the residents with a low degree, with and without control

¹¹These results can be related to Freedman (2012) who also found, for another type of place-based policies, an effect on the social composition. However, he also found an increasing effect on residents' turnover, which is not the case with the ZFU program. It might be due to a more reactive housing market in the US.

variables. For the residents with a high degree, results are weaker and not significant. So it seems that the program indeed benefited to the residents with a low level of education. But regarding the effect of the local hiring condition, an interesting pattern shows up : the effect since 1997 is mainly concentrated on the unemployment of low-degree residents while the additional effect after 2002 is mainly for high-degree residents (with and without controls). This means that from the beginning firms hired residents of the zone with low education level but in a second time, residents with a higher level of education. This adds up with the composition effects observed previously in this way : the local labor demand induced by the program was directed more toward low-skilled jobs and thus no strong composition effects occurred as the supply was sufficient in the zone. But with the tightening of the local hiring condition, the additional local demand turned to higher skills leading to an increase of residents with high degrees as there was probably not enough supply in the zone. This could have happened by attracting new residents with high degrees or retaining them if they were previously leaving the zone. It may also have been through an increase in the education level of people in their studies at that time who might have been encouraged to remain at school in order to take advantage of new opportunities. But since no effect is observed on the rate of 15-25 year olds residents nor on employment or studies (see table VII), it does not seem that young people lengthened their studies in a significant way. It is more plausible that residents with higher education were attracted or retained in the zone.

3.4. *Geographical Spillovers and Neighbors*

I investigate here commuting, another form of mobility that can be an issue in the evaluation of the program. If commuting is possible, firms might employ residents from outside the zones. On the contrary, if firms and/or workers prefer to minimize commuting distance and with the addition of the local hiring condition, there could also be negative spillovers on neighboring areas (Neumark and Simpson (2014)) : unemployed residents of the ZFU would be employed instead of neighbors living in closer proximity. Then the number of jobs at the ZFU level would be higher but there would be a negative effect on the near neighbors. The effect of the program on the unemployment rate of non EZ residents within commuting distance of an EZ is thus also estimated. For that purpose, I use people living in a municipality hosting an EZ but not in the EZ itself. EZ population amounts on average to 20% of the hosting municipality population. This way, I can reasonably assume they can commute to the EZ. Columns (1) and (2) of table VI present a DD estimation of the effect of the ZFU program on the neighbors of the zones.¹² No significant effect on the unemployment rate of the neighbors is detected, whether I use or not control variables. There thus might not be externalities on the neighbors or at least not of a significant magnitude. Nonetheless estimations in column (3) and (4) of table VI show that there was a decrease in the unemployment of EZ neighbors after 2002. This could be due to the fact that their residents commute into the zone. Indeed if commuting cost is not too high, workers who have strong preferences for their neighborhood, might still have benefited from the ZFU program by commuting into the zone. This would be

¹²Neighbors are defined as those living in a municipality which contains a ZFU without actually living in the ZFU. The control groups are the neighbors of the ZRU; a placebo test has been performed to verify that they are a valid control group.

TABLE V
DD ESTIMATIONS FOR LABOR MARKET OUTCOMES BY EDUCATION LEVEL

	(1)	(2)	(3)	(4)
Unemployment rate high-degree owners				
ZFU1G × post97	-0.058 (0.052)	-0.052 (0.050)	0.056 (0.058)	0.034 (0.057)
ZFU1G × post2002			-0.156*** (0.052)	-0.119** (0.051)
obs. (sampling areas × year)	1654	1654	1654	1654
R^2	0.269	0.336	0.272	0.338
Unemployment rate of medium-degree owners				
ZFU1G × post97	-0.083** (0.036)	-0.049 (0.032)	-0.058 (0.038)	-0.048 (0.036)
ZFU1G × post2002			-0.035 (0.040)	-0.002 (0.040)
obs. (sampling areas × year)	2505	2505	2505	2505
R^2	0.191	0.269	0.191	0.269
Unemployment rate of low-degree owners				
ZFU1G × post97	-0.114*** (0.033)	-0.075*** (0.028)	-0.071** (0.034)	-0.064** (0.029)
ZFU1G × post2002			-0.061 (0.038)	-0.016 (0.033)
obs. (sampling areas × year)	2536	2536	2536	2536
R^2	0.299	0.390	0.299	0.390
year fixed effects	yes	yes	yes	yes
UU fixed effects	yes	yes	yes	yes
Control variables	no	yes	no	yes

Source: French Labor Force Survey, 1993-2007, 15-65 year old

Note: *** significant at 1%. ** significant at 5%. * significant at 10%. Estimations with sampling weights. The number of observations can be slightly different when studying subpopulation : for example when computing unemployment rate, some areas may have only people out of the labor force and are therefore not used.

Control variables : gender, age, age², level of education divided in high, medium and low degrees, foreigners from outside EEC.

consistent with the results of Briant, Lafourcade, and Schmutz (2015) who show that the most accessible ZFU are the most successful in terms of jobs creation.

TABLE VI
DD ESTIMATIONS FOR GEOGRAPHICAL SPILLOVERS

	Unemployment rate			
	(1)	(2)	(3)	(4)
Neighbors of a ZFU 1G × post1997	-0.013 (0.009)	-0.007 (0.007)	0.012 (0.009)	0.009 (0.007)
Neighbors of a ZFU 1G × post2002			-0.029*** (0.009)	-0.018** (0.007)
obs. (sampling areas × year)	18108	18108	18108	18108
R^2	0.09	0.25	0.09	0.25
year fixed effects	yes	yes	yes	yes
UU fixed effects	yes	yes	yes	yes
Control variables	no	yes	no	yes

Source: French Labor Force Survey, 1993-2007, 15-65 year old

Note: *** significant at 1%. ** significant at 5%. * significant at 10%. Estimations with sampling weights.

Control variables : gender, age, age², level of education divided in high, medium and low degrees, foreigners from outside EEC.

4. DISCUSSION

4.1. Comparison to Other Studies

The size of the effect estimated here can be compared to other results on the first generation of ZFU. First Trevien, Givord, and Quantin (2012) estimated that the program induced a creation of 41500 to 56900 jobs (estimates for the year 2001, 5 years after the start of the program). According to an administrative report of 2002¹³, there were 72 409 jobs in ZFU in December 2001 and among them 63 325 were exonerated. So it appears that the majority of these jobs were due to the program.

But these jobs were not necessarily occupied by residents : only 25% of them were occupied by residents (ratio available for 1999¹⁴). If the ratio is assumed identical in 2001, this means that there were roughly 16 000 jobs exonerated and occupied by residents of the ZFU (25% of 63 325). However some of these jobs might have existed without the program. The direct effect of the program for the residents is the portion of these jobs that would not have existed without the program and/or would not have been occupied by residents.¹⁵ There is no evaluation of this portion. Therefore 16 000 is an upper bound.

There are approximately 294 000 people in the labor force in the ZFU1G in the 1999 Census. So the estimated 5.5 percentage points reduction in unemployment (which is the most comparable to that estimation) corresponds to approximately 16 000 jobs : it is very

¹³DIV (2002)

¹⁴DIV (2002)

¹⁵This does not take into account potential positive externalities : the program might also have increased the number of non-exonerated jobs available to the residents through an increase in local economic activity for instance.

close to that upper bound.¹⁶ Note however that this estimation takes into account direct and indirect effect of the program as it estimates the effect on the number of jobs occupied by residents, exempted or not. It could thus in theory be higher than the upper bound of the direct effect.

Gobillon, Magnac, and Selod (2012) found that the program created 10 jobs per semester per enterprise zone in the Paris region, so $10 \times 2 \text{ semesters} \times 5 \text{ years} \times 13 \text{ zones} = 1\,300$ jobs for the Paris region from 1997 to 2001. Applying their estimates to all enterprise zones proportionally to labor force size, this leads to an estimation of roughly 5 000 jobs created in 2001 thanks to the program and benefiting to the residents. Their estimation was made with control variables so it can be compared to the estimated 5.2 percentage points, which corresponds to roughly 15 000 jobs.¹⁷ Therefore it can be concluded that there is an effect on residents for the whole program, not just for the Paris region, and that this effect seems of a larger magnitude than estimated by Gobillon, Magnac, and Selod (2012).¹⁸

I also compute a cost per created job and compare it to other programs. In 2001, the exemptions in ZFU amounted to 293 million euros (DIV (2001)). In France since 1993, several payroll tax exemptions for low wage jobs have been implemented. The gross cost is defined as the cost without taking into account that jobs could benefit from other exemptions without the program, while the net cost takes it into account. I can very roughly estimate the net cost to be half of the gross cost (Benatsou (2009)). This gives an estimated yearly net cost of 9 500 euros per job created. It can be compared to the estimation of 31 000 euros per job created by Rathelot and Sillard (2007) for the 2004 ZFU. Bunel, Emond, and L'Horty (2012) reviewed the various estimations of the gross cost per job created by the French payroll tax exemptions that can be found in the literature and it varies between 10 000 euros and 50 000 euros (to be compared to our estimation of a 19 000 gross cost per job created). Our estimation thus falls within the lower part of this range. It thus seems that the first ZFU generation was relatively efficient compared to other payroll tax exemption policies. Note however that estimations can vary strongly according to the way they are computed (as shown by the review of Bunel, Emond, and L'Horty (2012)) and our estimation is merely indicative. Moreover this is far from being enough to perform a cost-benefit analysis or a welfare analysis since, among other things, general equilibrium effects are not taken into account.

4.2. *Sensitivity to the Employment Measure*

The unemployment rate is one way to look at the effect of the program, but it depends on both the number of jobs occupied by residents¹⁹ and their rate of participation in the labor force. So I might want to look as well at the employment rate of residents. Moreover, for the 15-25 year olds the participation in the labor force is closely related to the completion

¹⁶However this estimation is not very precise due to the small number of observations : the 95% confidence interval is 120 to 32000 jobs

¹⁷The 95% confidence interval is 2 700 to 27 800 jobs.

¹⁸Note that Gobillon, Magnac, and Selod (2012) measured the effect at the municipality level. If there was a negative effect on the municipality residents outside the ZFU, this would lead to smaller effect of the program at the municipality level.

¹⁹Note that it is not possible to determine whether the jobs are indeed located in the ZFU, but there is no reason why job opportunities outside the zones would evolve differently between treated and control groups.

of studies, unemployment rate is not a good measure for this age group. A more relevant way to look at this age interval is therefore to look at the share who is nor in employment nor studies. The sample is therefore split at 25 years old and for the 26-65 year olds, I estimate the effect on both the unemployment and employment rate.

TABLE VII
DD ESTIMATIONS FOR LABOR MARKET OUTCOMES

26-65 year old unemployment rate				
ZFU1G × post97	-0.109*** (0.028)	-0.069*** (0.024)	-0.054* (0.028)	-0.054** (0.023)
ZFU1G × post2002			-0.077*** (0.029)	-0.022 (0.025)
obs. (sampling areas × year)	2655	2655	2655	2655
R ²	0.268	0.395	0.270	0.395
26-65 year old employment rate				
ZFU1G × post97	0.108*** (0.031)	0.077*** (0.022)	0.035 (0.025)	0.050** (0.021)
ZFU1G × post2002			0.046 (0.029)	0.038 (0.025)
obs. (sampling areas × year)	2702	2702	2702	2702
R ²	0.293	0.527	0.239	0.527
Share of 15-25 year olds not in employment nor studies				
ZFU1G × post97	-0.055 (0.034)	-0.003 (0.021)	-0.020 (0.031)	-0.015 (0.022)
ZFU1G × post2002			-0.051 (0.036)	0.019 (0.023)
obs. (sampling areas × year)	2452	2452	2452	2452
R ²	0.274	0.557	0.275	0.557
Share among 26-65 years old of private sector workers				
ZFU1G × post97	0.069** (0.027)	0.063** (0.025)	0.035 (0.025)	0.045* (0.023)
ZFU1G × post2002			0.046 (0.029)	0.026 (0.026)
obs. (sampling areas × year)	2702	2702	2702	2702
R ²	0.238	0.378	0.239	0.379
year fixed effects	yes	yes	yes	yes
UU fixed effects	yes	yes	yes	yes
Control variables	no	yes	no	yes

Source: French Labor Force Survey, 1993-2007, 15-65 year old

Note: *** significant at 1%. ** significant at 5%. * significant at 10%. Estimations with sampling weights. The number of observations can be slightly different when studying subpopulation : for example when computing unemployment rate, some areas may have only people out of the labor force and are therefore not used.

Control variables : gender, age, age², level of education divided in high, medium and low degrees, foreigners from outside EEC.

The effect on the unemployment rate of the 26-65 year old residents is significant and of a magnitude of approximately 11 percentage points without controls and 7 percentage points with controls (see table VII). The result for their employment rate are similar. The results for the effect of the local hiring condition on unemployment are also the same as for the 15-65 year olds. For the 15-25 year old residents, there is no significant effect of the program. So it appears that the “youth” situation on the labor market has not been improved by the program.

Moreover, another concern is this: at the same period some public jobs were massively created for young people (“emplois jeunes”) and especially young people from these neighborhoods.²⁰ The “emplois jeunes” program is the same across all ZFU and ZRU but it will be reassuring that the effect of the EZ program examined in this paper is not driven by public jobs of the 15-25 years olds. More generally, the program targets private sector firms, so it is interesting to distinguish employment in the private versus the public sector, to check if the effect was on private sector jobs. Since economic activity and the tax revenue of the municipalities increased, the program could also have had an effect on public jobs but this would be an indirect effect. The effect of the ZFU1G on the share of 26-65 years old working in the private sector is thus investigated to assess a “direct” effect of the program. It is significant, around 6 percentage points(see table VII). It is lower than the effect on the 26-65 year old employment rate although not significantly different. When using control variables, estimations are much closer. So the effect of the program occurs mainly in the private sector.

From these results, I conclude that our main results on resident’s employment are not too sensitive to the chosen indicator : unemployment, employment or employment in the private sector.

4.3. Robustness Checks

A first set of robustness checks is performed using the other possible control groups. Table A3 in appendix shows the estimates of the main specifications²¹ when using all ZRU as a control group rather than ZRU not in the same urban unit than a ZFU1G. Results are very similar although in a few cases, they do not reach the same level of significance. The main result on unemployment is lower although not significantly different. In general the significance level is much lower as the number of observations is divided by more than 2.

A second set of robustness checks is done using different period of study : 1993-2003 (before the start of ZFU of second generation) and 1993-2011 (last year available); see table A3 in appendix. Results are very similar except that estimations regarding the local hiring condition are quite weak in the case where only 2 years are used (2002 and 2003).

Globally, results are quite robust to a change of the control group or of the period of study.

²⁰The “emplois jeunes” are specific jobs for people under 26 years old and a portion of the unemployed aged 26-30. They are created in the public or non-profit sectors and the remuneration is partially paid by the State. The program was created in 1997 and ended in 2002. Only contracts already existing were maintained after 2002.

²¹The other estimations are available upon request

5. CONCLUSION

To conclude, this paper shows that the ZFU program significantly decreased unemployment among residents, by more than 11 percentage points. In this regard, the ZFU program was beneficial to the zones. However, it was probably not enough : the ZFU unemployment rate decreased as a result of the program to reach a level close to that of the control group, but it remains much higher than in the rest of the country. Moreover it is not clear whether it is an efficient way to help the most disadvantaged residents. Indeed, the decrease in unemployment was due to an improvement of the situation of "original" residents of the zones but also to changes in the social composition of the zones. The program helped to attract or retain residents with high degrees. This hints that the deprived situation of the ZFU is not only due to spatial mismatch. As the program and, in particular the local hiring condition, led to an increase in the share of high degree workers, it is plausible that the mismatch between the level of skills of ZFU residents and job offers is a serious issue. From a public policy point of view, it might therefore be more efficient to attach benefits to people rather than to places. The French government in fact proposed in 2013 a new policy of "emplois francs" (roughly, "free-range jobs") in which hiring subsidies were linked to the residents of deprived zones whatever the location of their workplace. However, the program was not renewed, due to a very low level of hirings during the 3-year experimental period.

However social composition changes might be a worthy goal in itself and ZFU might be a good way to achieve it. If it leads, for example, to positive peer effects, it could be positive for deprived neighborhoods. In that case, potential effects on the housing market must be carefully studied. Kline and Moretti (2014)) states that, if workers are mobile, the ZFU credits are capitalized in land prices. In the US, there is indeed some evidence that ZFU program led to an increase in land prices (Freedman (2012), Krupka and Noonan (2009)). For France, Poulhes (2015) showed that there was an increase in commercial property values, whereas Gregoir and Maury (2012) found a decrease in housing values on a small subsample of ZFU. It is plausible that indeed part of the ZFU program was capitalized into land prices. If landowners and ZFU residents are separate, which is probably the case as the share of social housing is very high in ZFU, there would be a gain in welfare for landowners but it might not be the case of the total welfare. Further research is needed to shed light on this issue.

APPENDIX

TABLE A1

ZFU AND ZRU EXEMPTIONS IN 1997

	ZRU	ZFU
Payroll tax	1 year exemption - within a limit of 50 employees and 1.5 minimum wage - for long term contracts	5 years of exemption - within a limit of 50 employees and 1.5 minimum wage - local hiring condition from the 3rd employee - for long term contracts - firms of less than 50 employees at their date of creation - some activities are excluded
Individual social charges (health system) of artisans and tradespeople	no	5 years of exemption - within a limit of 1.5 minimum wage
Local business tax	5 years of exemption - est. of less than 150 employees - within a limit determined yearly (990 kF or roughly 151 kEuros in 2000 , half for est. already existing) - whatever the activity	5 years of exemption - firms of less than 50 employees at their date of creation - within a limit determined yearly (2835 kF or roughly 432 kEuros in 2000) - some activities are excluded
Profit tax	5 years of exemption - restricted to new firms - degressive : 100% for year 1 and 2, 75 % for year 3, 50 % for year 4, 25 % for year 5 - within a limit since 2000 - no employees number restriction	5 years of exemption - for new firms and firms existing the 01/01/1997 - within a limit of 400 000 F or roughly 61 000 Euros for the yearly profit - no employees number restriction
Tax on properties	no	5 years of exemption for business properties
Specific Paris region tax on creation of offices	no	exemption
Transfer rights for acquisitions of businesses	exemptions up to 700 000 F or roughly 107 000 Euros	exemptions up to 700 000 F or roughly 107 000 Euros

TABLE A2
DD ESTIMATIONS OF THE EFFECT ON THE SHARE OF NEW RESIDENTS

	Share of new residents	Share of new residents from outside the municipality	Share of new residents from within the same municipality
ZFU1G × post97	-0.012 (0.018)	0.000 (0.011)	-0.012 (0.012)
ZFU1G × post2002	-0.005 (0.017)	-0.008 (0.011)	0.004 (0.011)
obs. (sampling areas × year)	2713	2713	2713
R^2	0.19	0.14	0.16
year fixed effects	yes	yes	yes
UU fixed effects	yes	yes	yes
Control variables	no	no	no

Source: French Labor Force Survey, 1993-2007, 15-65 year old

Note: *** significant at 1%. ** significant at 5%. * significant at 10%. Estimations with sampling weights. The number of observations can be slightly different when studying subpopulation : for example when computing unemployment rate, some areas may have only people out of the labor force and are therefore not used.

TABLE A3

DD ESTIMATIONS FOR UNEMPLOYMENT, PLACEBO AND ROBUSTNESS CHECKS

All ZRU not in UU of ZFU as control group						
ZFU1G × 1994	-0.036 (0.026)	-0.005 (0.025)	-0.037 (0.026)	-0.005 (0.025)	-0.036 (0.026)	-0.005 (0.025)
ZFU1G × 1995	-0.026 (0.040)	0.009 (0.034)	-0.028 (0.040)	0.009 (0.034)	-0.026 (0.040)	0.010 (0.034)
ZFU1G × 1996	-0.038 (0.052)	0.014 (0.040)	-0.041 (0.052)	0.013 (0.040)	-0.039 (0.052)	0.014 (0.040)
ZFU1G × post1997	-0.138*** (0.038)	-0.055 (0.031)	-0.084* (0.040)	-0.048 (0.031)	-0.113* (0.051)	-0.064 (0.040)
ZFU1G × post2002			-0.077* (0.030)	-0.011 (0.024)		
ZFU1G × post97 × t					-0.004 (0.006)	0.002 (0.005)
obs. (sampling areas × year)	2 669	2 669	2 669	2 669	2 669	2 669
All ZRU as control group						
ZFU1G × 1994	-0.033 (0.025)	-0.006 (0.023)	-0.033 (0.025)	-0.006 (0.023)	-0.033 (0.025)	-0.006 (0.023)
ZFU1G × 1995	-0.024 (0.042)	0.014 (0.034)	-0.025 (0.041)	0.014 (0.034)	-0.024 (0.041)	0.014 (0.034)
ZFU1G × 1996	-0.031 (0.051)	0.019 (0.040)	-0.032 (0.051)	0.018 (0.040)	-0.031 (0.051)	0.019 (0.040)
ZFU1G × post1997	-0.114** (0.039)	-0.034 (0.031)	-0.062 (0.040)	-0.029 (0.031)	-0.087 (0.050)	-0.044 (0.040)
ZFU1G × post2002			-0.076* (0.030)	-0.007 (0.023)		
ZFU1G × post97 × t					-0.005 (0.006)	0.002 (0.005)
obs. (sampling areas × year)	3 074	3 074	3 074	3 074	3 074	3 074
future ZFU 2G/3G not in UU of ZFU as control group						
ZFU1G × 1994	-0.059 (0.035)	-0.015 (0.037)	-0.055 (0.034)	-0.014 (0.037)	-0.057 (0.034)	-0.014 (0.037)
ZFU1G × 1995	-0.063 (0.043)	-0.015 (0.040)	-0.061 (0.043)	-0.015 (0.040)	-0.062 (0.043)	-0.015 (0.040)
ZFU1G × 1996	-0.063 (0.058)	-0.012 (0.052)	-0.061 (0.056)	-0.012 (0.051)	-0.062 (0.057)	-0.011 (0.052)
ZFU1G × post1997	-0.171*** (0.045)	-0.070 (0.044)	-0.077 (0.045)	-0.045 (0.043)	-0.058 (0.060)	-0.037 (0.051)
ZFU1G × post2002			-0.156*** (0.038)	-0.045 (0.031)		
ZFU1G × post97 × t					-0.025* (0.011)	-0.008 (0.008)
obs. (sampling areas × year)	1 144	1 144	1 144	1 144	1 144	1 144
Matched ZRU not in UU of ZFU as control group						
ZFU1G × 1994	-0.070* (0.031)	-0.043 (0.033)	-0.072* (0.030)	-0.044 (0.032)	-0.071* (0.030)	-0.044 (0.033)
ZFU1G × 1995	-0.072 (0.043)	-0.040 (0.041)	-0.075 (0.043)	-0.043 (0.041)	-0.073 (0.043)	-0.042 (0.041)
ZFU1G × 1996	-0.067 (0.057)	-0.045 (0.049)	-0.069 (0.058)	-0.046 (0.049)	-0.067 (0.057)	-0.046 (0.049)
ZFU1G × post1997	-0.115** (0.041)	-0.075* (0.037)	-0.054 (0.046)	-0.038 (0.040)	-0.069 (0.063)	-0.034 (0.060)
ZFU1G × post2002			-0.088* (0.037)	-0.055 (0.036)		
ZFU1G × post97 × t					-0.009 (0.009)	-0.008 (0.010)
obs. (sampling areas × year)	1 105	1 105	1 105	1 105	1 105	1 105
year fixed effects	yes	yes	yes	yes	yes	yes
UU fixed effects	yes	yes	yes	yes	yes	yes
Control variables	no	yes	no	yes	no	yes

Source: French Labor Force Survey. 1993-2003, 15-65 year old

Note: *** significant at 1%. ** significant at 5%. * significant at 10%. Estimations with sampling weights. The number of observations can be slightly different when studying subpopulation : for example when computing unemployment rate, some areas may have only people out of the labor force and are therefore not used.

Control variables : gender, age, age², level of education divided in high, medium and low degrees, foreigners from outside EEC.

REFERENCES

- BENATSOU, F. (2009): "Les entreprises dans les zones franches urbaines : bilan et perspectives," .
- BERTRAND, M., E. DUFLO, AND S. MULLAINATHAN (2004): "How Much Should We Trust Differences-in-Differences Estimates?," *The Quarterly Journal of Economics*, 119(1), 249–275.
- BRIANT, A., M. LAFOURCADE, AND B. SCHMUTZ (2015): "Can Tax Breaks Beat Geography? Lessons from the French Enterprise Zone Experience," *American Economic Journal: Economic Policy*, 7(2), 88–124.
- BUNEL, M., C. EMOND, AND Y. L'HORTY (2012): "Évaluer les réformes des exonérations générales de cotisations sociales," *Revue de l'OFCE*, 0(7), 57–103.
- BUSSO, M., J. GREGORY, AND P. KLINE (2013): "Assessing the Incidence and Efficiency of a Prominent Place Based Policy," *American Economic Review*, 103(2), 897–947.
- DAVEZIES, L., AND X. D'HAULTFOEUILLE (2009): "To Weight or not to Weight? The Eternal Question of Econometricians facing Survey Data," DESE Working Papers g2009-06, Insee.
- DIV (2001): "Bilan des zones franches urbaines, rapport au parlement, Délégation Interministérielle à la Ville," .
- (2002): "Bilan des zones franches urbaines, rapport au parlement, Délégation Interministérielle à la Ville," .
- FREEDMAN, M. (2012): "Teaching new markets old tricks: The effects of subsidized investment on low-income neighborhoods," *Journal of Public Economics*, 96(11), 1000–1014.
- (2013): "Targeted Business Incentives and Local Labor Markets," *Journal of Human Resources*, 48(2), 311–344.
- GIVORD, P., R. RATHELOT, AND P. SILLARD (2013): "Place-based tax exemptions and displacement effects: An evaluation of the Zones Franches Urbaines program," *Regional Science and Urban Economics*, 43(1), 151–163.
- GOBILLON, L., T. MAGNAC, AND H. SELOD (2011): "The effect of location on finding a job in the Paris region," *Journal of Applied Econometrics*, 26(7), 1079–1112.
- (2012): "Do unemployed workers benefit from enterprise zones? The French experience," *Journal of Public Economics*, 96(9-10), 881–892.
- GOBILLON, L., H. SELOD, AND Y. ZENOU (2007): "The Mechanisms of Spatial Mismatch," *Urban Studies*, 44(12), 2401–2427.
- GOTTLIEB, J. D., AND E. L. GLAESER (2008): "The Economics of Place-Making Policies," Brookings papers on economic activity.
- GREGOIR, S., AND T.-P. MAURY (2012): "Quel a été l'effet de l'instauration de Zones Franches Urbaines sur les marchés immobiliers locaux ? Le cas de la Seine-Saint-Denis.," Position paper, EDHEC.
- HAM, J. C., C. SWENSON, A. IMROHOROGLU, AND H. SONG (2011): "Government programs can improve local labor markets: Evidence from State Enterprise Zones, Federal Empowerment Zones and Federal Enterprise Community," *Journal of Public Economics*, 95(7-8), 779–797.
- JIBRAYEL, H. (2013): "Rapport d'information sur les zones franches urbaines, Assemblée nationale," .
- KLINE, P., AND E. MORETTI (2013): "Place Based Policies with Unemployment," *American Economic Review*, 103(3), 238–43.
- (2014): "People, Places, and Public Policy: Some Simple Welfare Economics of Local Economic Development Programs," *Annual Review of Economics*, 6(1), 629–662.
- KRUPKA, D. J., AND D. S. NOONAN (2009): "Empowerment Zones, neighborhood change and owner-occupied housing," *Regional Science and Urban Economics*, 39(4), 386–396.
- LYNCH, D., AND J. S. ZAX (2011): "Incidence and Substitution in Enterprise Zone Programs: The Case of Colorado," *Public Finance Review*, 39(2), 226–255.
- MAYER, T., F. MAYNERIS, AND L. PY (2017): "The impact of Urban Enterprise Zones on establishment location decisions: Evidence from French ZFUs," *forthcoming in Journal of Economic geography*.
- NEUMARK, D., AND H. SIMPSON (2014): "Place-Based Policies," Working Paper 20049, National Bureau of Economic Research.
- ONZUS (2011): "Rapport ONZUS, Observatoire national des zones urbaines sensibles," .
- POULHES, M. (2015): "Are Enterprise Zones benefits capitalized into commercial property values ? The French case," Documents de Travail de la DESE - Working Papers of the DESE g2015-13, Institut National de la Statistique et des Etudes Economiques, DESE.
- RATHELOT, R., AND P. SILLARD (2007): "The impact of French Zones Franches Urbaines on employment and business creation," Documents de Travail de la DESE - Working Papers of the DESE g2007-11,

- Institut National de la Statistique et des Etudes Economiques, DESE.
- RATHELOT, R., AND P. SILLARD (2008): "The Importance of Local Corporate Taxes in Business Location Decisions: Evidence From French Micro Data," *Economic Journal*, 118(527), 499–514.
- REYNOLDS, C. L., AND S. M. ROHLIN (2015): "The effects of location-based tax policies on the distribution of household income: Evidence from the federal Empowerment Zone program," *Journal of Urban Economics*, 88(C), 1–15.
- TREVIEN, C., P. GIVORD, AND S. QUANTIN (2012): "A Long-Term Evaluation of the First Generation of the French Urban Enterprise Zones," *Ersa conference papers*, European Regional Science Association.
- WOOLDRIDGE, J. (2009): *Introductory econometrics: a modern approach*. South Western, Cengage Learning.