Journal of Urban Economics 105 (2018) 149–161

Contents lists available at ScienceDirect

Journal of Urban Economics

journal homepage: www.elsevier.com/locate/jue

A long-term evaluation of the first generation of French urban enterprise zones

Pauline Givord*[,a], Simon Quantin[b], Corentin Trevien[a]

[a] INSEE-CREST, France
[b] INSEE, France

ARTICLE INFO

Keywords:
Enterprise Zones
Local employment
Propensity score matching
Evaluation

JEL classification:
C23
H71
R5

ABSTRACT

This paper provides a new empirical assessment of the long-term efficiency of locally targeted tax incentives in revitalizing distressed areas and improving local population situation. We focus on the first generation of the French "Enterprise Zone" (EZ) initiative, implemented in 1997. Contrary to previous results from similar programs in France, we observe a strong positive impact of the EZ initiative on economic activity in the short run, and this finding is robust to several identification strategies. However, long-run estimates suggest that this program fails to propel self-sustaining economic development. After five years, the early positive results level off as the increase in business locations is partially offset by more frequent business discontinuations. Moreover, the small impacts of the program on resident employment and local services suggest that the program lacked accurate targeting.

1. Introduction

"Enterprise Zone" (hereafter EZ) programs have a long history. The first were implemented in the UK in the 1980s, and others followed in several US states and elsewhere. These programs usually provide financial incentives to businesses to locate in economically distressed areas. The rationale guiding policy makers when opting for an EZ program is quite simple: reductions in taxation are meant to offset the numerous disadvantages associated with deprived areas, such as a shortage of skilled labor, a lack of public services, a dearth of inputs, or poor market potential. The EZ initiative may stimulate local economic activity, by attracting businesses that will employ the resident workforce, and may "revitalize" these neighborhoods by improving local amenities (health services, convenience stores, etc.) for the local population. The hoped-for spillover effects include increased local demand and greater incentives for other new businesses to choose the same location because of agglomeration economies. Once this initial boost had been delivered, the EZ initiative was expected to come to an end, having run its intended course.

However, as Neumark and Simpson (2014) note in a critical review of the already large economic literature on place-based policies, the theoretical foundations of such policies are not well established, and the empirical evidence on their efficiency is mixed. Moreover, a common designation of place-based policy may mask highly heterogeneous features of a program, which would make any generalization difficult. Convincing evidence capable of guiding policy is still lacking in several areas. While one of the main challenges of place-based policy is generating self-sustaining economic activity, little evidence exists of the long-term effects of these programs. Furthermore, one should ask who gains from place-based programs and whether they would be sufficient for revitalizing the targeted neighborhoods and, ultimately, benefit disadvantaged residents.

This paper presents new empirical evidence on these issues, through the evaluation of the first wave of a French EZ program, the "Zones Franches Urbaines" implemented in 1997 by the French government. Using a highly detailed database over a fifteen-year period allows us to evaluate the long-term impact of this program and to focus on resident employment. The French EZ program has features that distinguish it from similar previous efforts. Its careful evaluation may provide design guidance that may make an EZ program effective. The French program targeted small businesses (specifically those with fewer than fifty employees), with very few requirements apart from respecting the fact of being located within the boundaries of the EZ. The program provides not only tax exemptions but also substantial exemptions from payroll taxes. This design was indeed in line with recommendations of, for instance, Butler (1989). Butler, who is usually credited with introducing the idea of urban enterprise zones to the United States in the 1980s, strongly advocated the idea of targeting EZ to small businesses and

* Corresponding author.
E-mail addresses: pauline.givord@insee.fr (P. Givord), simon.quantin@insee.fr (S. Quantin), corentin.trevien@insee.fr (C. Trevien).
URLS: https://sites.google.com/site/paulinegivord/home (P. Givord), http://www.crest.fr/pagesperso.php?user=3214 (C. Trevien).

http://dx.doi.org/10.1016/j.jue.2017.09.004
Received 9 March 2015; Received in revised form 22 September 2017; Accepted 22 September 2017
Available online 29 September 2017
0094-1190/

focusing tax incentives on payroll taxes. Labor cost reductions have an almost immediate impact on firm cash flows (as a payroll tax has to be paid monthly or quarterly).

However, the choice of targeting small businesses has no clearcut effect on the ability of a program to yield long-term, self-sustaining economic activity. On the one hand, small businesses are commonly considered to be the main engine of job creation (see Neumark et al., 2011, confirming the influential works of David Birch for the US economy). Furthermore, small businesses may locate (or relocate) more easily than large companies in a spatially limited area. On the other hand, small businesses exhibit, on average, a very high death rate. One should thus consider the risk that the EZ program might financially support non-viable businesses. The French EZ program secures grants for a long duration: the subsidies are available for small businesses located in the disadvantaged areas at the full rate for five years and at decreasing rates for the following five years. Such long-term financial support may help new businesses to grow and survive the first, usually critical, years of their existence. However, one has to check whether this support is sufficient to promote viable businesses, which will survive once the subsidies have gone. In this article, we thus provide a yearly evaluation of the impact of a program over a fifteen-year period, meaning that we are able to evaluate its impact on the aggregate level of activity while including those businesses that no longer benefit from these subsidies. Such an evaluation is all the more important since previous evidence on the long-term effects of similar programs is scarce.

Furthermore, one has to evaluate whether the EZ program has been able to achieve its main objectives. The ultimate goal of such place-based policies is usually to revitalize deprived neighborhoods. Whether the economic activity generated by the EZ program is sufficient to achieve this goal remains an open question. The French EZ program only indirectly targets resident employment. The subsidies depend primarily on the location of the business and are weakly related to where workers live. Payroll tax exemptions are granted to a business on the basis of the total number of employees rather than the number of new jobs created. In principle, a local hiring clause exists, but in practice it imposed very weak requirements on businesses for the first years of the program. It is thus an open question whether businesses attracted by the program eventually provided job opportunities to local residents or instead drew their labor force from outside.

The evaluation here is based on a yearly dataset of companies over the period from 1995 to 2012. This exhaustive administrative data provides employment and business locations for all French establishments. It contains geolocalized information on both businesses and workers. We can identify whether a business is located within the boundaries of an EZ and whether a worker is a local resident. As emphasized by Bondonio and Greenbaum (2007), establishment-level data help to accurately analyze the economic dynamics underlying any economic impact. We can compare figures concerning establishment openings and closings and the resulting number of establishments located in the areas in question. We can estimate whether the program generates self-sustaining economic activity and whether businesses attracted by the financial incentives hire local residents, which may reduce local unemployment, but also operate locally and may give the local population access to the sort of "basic" services (health workers, convenience stores, independent retailers such as bakers and tradesmen, etc.) that are more likely to suffer hardship from being located in distressed urban areas (because of limited market potential in low-income neighborhoods, low accessibility for non-local employees, and high rates of criminality). As we have also information on worker characteristics, we can identify directly whether zone residents hold the jobs created by the EZ program.[1] The results are obtained for fifteen years after the implementation of the EZ initiative. This allows us to analyze the temporal profile of the program's impact: as the subsidies are granted to small businesses for five to ten years, the steady-state assessment of the program is expected to be observed only once businesses are no longer eligible for the subsidy.

The identification strategy is provided by the approach used to determine which areas are selected for EZ status. The first 44 EZs were selected from among a set of 450 areas previously identified as deprived, which provides a control group. A population of over 10,000 was required for an area to qualify as an EZ. Above this threshold, deprived areas were selected according to an explicit deprivation index based on various socio-economic criteria, such as the concentration of young people and the unemployment rate. In line with the usual practice in the empirical literature on EZs, the identification strategy combines difference-in-differences estimates with a propensity score method to compare EZs to areas that have similar socio-economic profiles. The use of a population threshold in the selection process improves the comparability of the EZs to other areas but requires us to adapt the estimation of the propensity score. Our empirical conclusions appear robust to several alternative identification strategies, including a linear factor models method that relaxes the common trend assumption underlying the difference-in-differences estimator (see Gobillon and Magnac, 2016).

The paper is organized as follows. Section 2 gives a brief overview of related literature. Section 3 presents the French EZ program, the EZ areas, and estimates of the magnitude of the financial incentives provided by the program. The data are briefly presented in the following section. Identification issues are discussed in Section 5. Section 6 displays the results and Section 7 discusses the results and draws conclusions.

2. Related literature

Despite a large body of literature, the empirical evidence on the efficiency of place-based policies to improve the economic situation of local populations is mixed. In an influential paper, Papke (1993) finds no noticeable impact of the Indiana program on the well being of zone residents (measured by economic status such as income and labor market characteristics). According to Freedman (2013), the Texas Enterprises Zones had a positive impact on resident employment, but Elvery (2009) do not reach such a positive conclusion for the California and Florida programs. Ham et al. (2011) and Busso et al. (2013) conclude that the federal Empowerment Zones reduced poverty and unemployment. However, Reynolds and Rohlin (2015) observe that such a positive mean impact may be achieved at the expense of the most impoverished residents. Peters and Fisher (2002) find that the US State Enterprise Zones may have attracted more well-off households. All in all, these seemingly conflicting findings on the impact of these policies on the economic situation of local populations reflect the variety of incentives that are offered by EZ programs.[2] For instance, as suggested by Neumark and Grijalva (2017) in a related context, hiring credits may be more effective if payment is linked to job creation goals, which is not the case in the French EZ program, which subsidizes small businesses without any specific requirements on hiring.

Moreover, little is known about the long-term impact of similar place-based policies as there are few existing studies on this issue. Kline and Moretti (2014) provide an evaluation of impact of the Tennessee Valley Authority (TVA), which was implemented nearly one century ago. According to their results, the "big push" provided by this ambitious program had a long-lasting positive impact. However, the TVA is hardly comparable to the EZ program, both in scale and design.

[1] Most of the related literature rely either on Census data that provide accurate information on resident outcomes but not on employers, or on establishment registers that inform on business, and not worker, characteristics (see for instance Busso et al., 2013).

[2] See, for instance, the "State Enterprise Zone Update" of the U.S. Department of Housing and Urban Development that provides an exhaustive description of the variety of incentives provided by the 36 EZs existing in 1991.

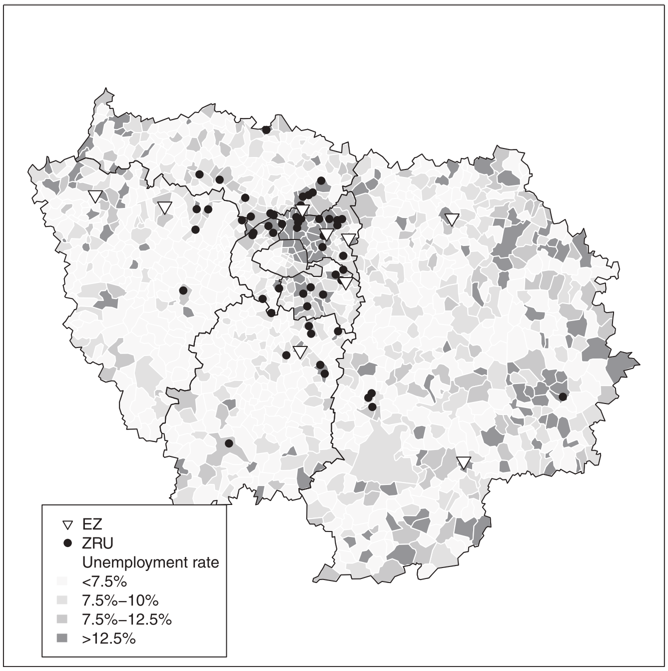

Fig. 1. EZ locations and unemployment rate in 1990 in the Paris metropolitan region.

The TVA resulted in large investments in public infrastructure projects (especially for transportation and electricity supply) over a large territory, whereas EZ programs target specific areas. To the best of our knowledge, little evidence exists on the long-term impact of EZ programs with features similar to those of the French EZ program on economic activity. One notable exception is Gobillon et al. (2012) on the unemployment rate of local residents. However, their analysis is restricted to the Paris region (which is quite specific in France in terms of economic development). Furthermore, they use data measured at the municipal-level (ZIP code). As emphasized, for instance, by Neumark and Kolko (2010) and Elvery (2009), using city or ZIP codes as the unit of analysis may blur the identification of the program's impact as EZs are often defined at a much smaller scale. Access to geolocalized data is important for the evaluation of the impact of EZ programs on economic activity. Previous research on the French EZ program that employs such detailed data focuses on the second wave of this program (see, for instance, Givord et al., 2013), which was implemented seven years after the policy evaluated in this paper. The evaluation of the second wave was provided quite early after the introduction of the program[3] and thus was not able to analyze its long-term impact.

3. The French Enterprise Zones

3.1. Selection of the Enterprise Zones

Urban decay has been one of the main topics of French public debate since the 1980s. A range of policies have been implemented in response to social and economic problems experienced in the deprived outskirts of France's cities. Indeed, the so-called "social fracture" ("fracture sociale") was an important theme of the 1995 presidential campaign, with the social and economic circumstances in deprived urban areas being identified as the main causes. The "stimulus for cities" law ("Pacte de relance de la ville"), passed in 1996 by the newly elected government, was intended to address the issue of urban decay and reduce inequalities between urban neighborhoods.

This law resulted in the implementation of tax cuts for businesses located in those deprived areas. More precisely, this policy instituted a three-tiered classification scheme for disadvantaged urban areas. The components of the first and widest tier are known as ZUSs ("Deprived Urban Areas"). There are 757 of these areas, and they constitute the most deprived areas in France[4] according to various indicators of socio-economic development (in particular, high concentrations of social housing and high unemployment rates). The second tier, the ZRUs ("Urban Renewal Areas"), contains the most disadvantaged ZUSs as ranked by a global index of their social and economic position. This index takes into account the unemployment rate, the population size, the proportion of unskilled people, the proportion of young people and the potential tax revenue (the product of the tax base multiplied by the average national tax rate) of the city. It corresponds to the product of the first four indicators, divided by the fifth one. A total of 416 ZRUs were designated in 1996.[5] Finally, the third tier is made up of ZFUs ("Urban Enterprise Zones"), hereafter EZs. These zones were selected in a two-stage process: only the most populous 416 ZRUs were eligible, the official threshold being 10,000 inhabitants, and out of that set the most deprived ZRUs, as defined by the same global rating, were designated EZs. In 1997, during the first phase of this initiative, 44 areas (38 in continental France and 6 in French overseas departments) received the EZ designation, followed by an additional 41 in 2004 and 15 more in 2007.

Fig. 1 illustrates, using the case of the Paris metropolitan region, the uneven local distribution of the unemployment rate, as well as the location of some EZs. This region is the wealthiest in France, but the unemployment rate varies markedly across municipalities. The inner-northeast suburbs of Paris are a site of concentrated economic difficulty. This large sector apart, municipalities characterized by high rates of unemployment are spread throughout the region. The EZs are generally located in such economically distressed municipalities, but not always: this is explained by the fact that the unemployment rate in some neighborhoods (the relevant geographical level for EZs) may substantially exceed the figure estimated at the municipal level. Furthermore, due to the political bargaining involved, and hence the need to disperse targeted areas across France, the designation as an EZ does not rely in a deterministic way on the ranked index of social and economic status. The upshot is that the EZs are uniformly spatially distributed across France, while urban deprived areas are mostly concentrated in a limited number of municipalities.

3.2. Advantages granted by the EZ policy

EZs offer remarkably generous incentives (deep cuts to property, labor and business taxes). They target only small businesses (establishments with fewer than 50 employees, with an additional requirement regarding sales volume), regardless of whether they were located in the area prior the introduction of the EZ policy (see Table 1 for details). Full exemption is granted for a minimum of five years. In comparison to the tax relief available in EZs, the ZRU and ZUS designations provide much shallower tax credits. The ZRU program provides limited tax cuts for newly created businesses only and over a shorter period (one or two years after startup, depending on the tax). Payroll tax exemption applies to all employees in EZs (conditional on a local hiring clause, "clause d'emploi local"), whereas it remains limited to newly hired employees in ZRUs. Finally, the ZUS program merely allows local authorities to exempt businesses from local business taxes, but this tax

[3] This was possible due to the release of geolocalized data on these zones, but the data only covered years after 2002, meaning five years after the introduction the first wave. For this reason, first evaluations of the French EZ program focus on this second wave. This paper takes advantage of the release of data for the period before, which were released later.

[4] There were 717 ZUSs in continental France and 40 in French overseas departments. A total of 4.73 million people lived in ZUSs according to 1990 census data.

[5] The 1996 figures are 396 in continental France and 20 in French overseas departments.

Table 1
French Enterprise Zone tax cuts.

	ZRU (1996–2004)	EZ (1996–2001)
	Payroll tax exemptions	
Business eligibility	With up to 50 employees	
Employee eligibility	New hires	All employees
	Open-ended contracts	
	Fixed-term employment contract of more 12 months	
Exemption	Fraction of salary ≤ 1.5 times the minimum wage	
Duration	1 year	5 years
	Corporate income tax exemptions	
Eligibility	Newly created	All (created, existing, relocating)
	(only in manufacturing, trade or craft industry)	
Exemption	100% the first 2 years, and decreasing the next 3 years	100% during 5 years
	Local business tax exemptions	
Eligibility	Newly created	All (created, existing, relocating)
	with up to 150 employees	with up to 50 employees
Exemption	100% during 2 years	100% during 5 years
	Local property tax exemption	
Eligibility	None	All (created, existing, relocating)
Exemption		100% during 5 years

Source: Legislative texts (*Journal officiel*, 1995).

break is not mandatory.

The first generation EZs were implemented in 1997 and scheduled for five years. As initially planned, the policy ended in 2001: businesses had to locate in an EZ before December 31 to benefit from the tax exemptions. However, the EZ policy was reactivated in 2002 and has been maintained continuously since then. New areas were designated successively in 2004 and 2007, yielding a total of 100 EZs at present.

The successive renewals of the EZ policy testify to the strong support for this policy among policy makers and, especially, local authorities. However, from its very beginning, the program has been accused of creating windfall effects and of being used as a fiscal optimization tool by some businesses. In particular, while the objective of the program was to foster resident employment, opponents of the EZ program highlighted the existence of so-called "mailbox" businesses, meaning businesses that have only a postal address in an EZ but actually operate elsewhere.[6] The objective of fostering resident employment is in theory supported by the local hiring clause. This clause stipulates that the payroll tax exemption for a business (that concerns all its workforce) is conditional upon at least one-fifth of the workers hired by this business since the creation of the EZ, or one-fifth of its overall workforce, being residents of the area (or its surroundings). This clause is not, strictly speaking, comparable to a hiring credit for resident employment. The wage tax relief applies to the whole workforce and not only to employees living in the area. Most important, this condition applied only after the hiring of two new employees since the creation of the EZ (in other words, a business may benefit from the tax exemptions, even if none of its employees live in the targeted area, provided that the business had not hired more than two new employees since the introduction of the tax exemptions).[7] Businesses located in an EZ could benefit from the wage tax exemptions as long as they had not hired two new workers. Furthermore, in the first years of the EZ program, the practical application of this clause was not closely monitored. A new law was passed at the end of 2000[8] to restrain potential misuse of exemptions. In particular, this law states that businesses in EZs have the obligation to report all new recruitment and that the duration of working hours for a local job cannot be less than 16 hours per week. The local hiring clause was also strengthened: after 2002, the exemption depended upon one-third of the workforce being local residents, instead of one-fifth, for firms located in an EZ after this date (for firms located in an EZ before 2002, the previous level still applied). In 2012, this ratio was raised to one-half.

The financial incentives depend on the actual financial burden for small businesses and on the structure of their revenues and costs. To assess the actual generosity of this program, we simulate the benefit using individual databases that provide accurate information (see online Appendix). According to these simulations, payroll tax exemptions account for the largest share of tax reductions. In 1997, the median cut in payroll taxes associated with EZ was 5,900 euros, and this cut represented approximately 15% of the median wage bill. This relative advantage was slightly reduced after the introduction of changes in the national payroll tax scheme in 2003, but EZs remain attractive. Under the payroll tax scheme in use since this date, the median gain for businesses from being located in an EZ still represents approximately 12% of the median labor cost (equal to 4,500 euros).

Eligible businesses also benefit from a full exemption from corporate income tax, up to a limit that cannot exceed 20,000 euros per year. In practice, a closer examination to real data suggests that this exemption is not as attractive as it may seem. Before the implementation of the EZ program, more than three-quarters of small businesses did not pay any corporate income tax. For those that did pay a strictly positive corporate income tax, the median amount paid was 3,700 euros.

4. Data

We exploit two exhaustive administrative databases that enable us to gather rich information on firm demography (number of businesses) and employment.

The French business register (SIRENE) follows all French businesses. Every January 1, it displays the location of each business, its business's legal status, its industry and its year of creation. This register also tracks business creations and relocations throughout the year. It thus enables us to specify whether a new business location is an actual creation or a relocation of an existing business. It also allows us to identify when businesses cease activity. Above all, SIRENE precisely locates all businesses in continental France. Thus, we can accurately identify which businesses have settled in an EZ and which have not, which is crucial because EZs do not correspond to administrative boundaries (see also Givord et al., 2013). Indeed, using data even at the level of the smallest French administrative subdivision (the municipality, or *commune*) would have yielded an underestimation of the impact of tax exemptions, as businesses that benefit from EZ tax breaks would have been grouped with businesses that do not.

The second database (DADS) is an exhaustive administrative employer-employee database with workforce information at the business level. Employment can be measured in various ways at the business level: full-time equivalents over a year or the number of employees at any point in time or as of January 1. We use this latter measure, which is consistent across years and with the French business register. DADS thus provides a measure of local employment, meaning employment in businesses located in the area. The DADS files contain detailed information: job qualifications (we distinguish among low-skilled, skilled and high-skilled employment) and the personal address of the worker (we can thus measure local resident employment, defined as the

[6] This expression is commonly used when referencing the French EZ program, even in official reports. As early as 2001, a report of the French Senate mentioned the *enterprises "boîte aux lettres"* (mailbox businesses); see http://www.senat.fr/rap/r01-354/r01-35417.html. This report optimistically assumed that the introduction of controls would eradicate these windfall effects. However, fifteen years later the risk of "mailboxes" or "hollow shells" is still emphasized by persons "in the field".

[7] *Article 13, loi n° 96–987 du 14 novembre 1996 relative à la mise en oeuvre du pacte de relance pour la ville.*

[8] *Loi du 13 décembre 2000 relative à la solidarité et au renouvellement urbains.*

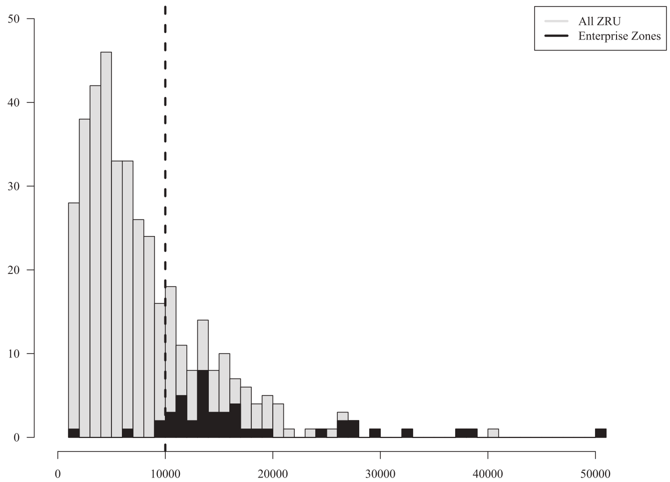

Fig. 2. Distribution of EZs and ZRUs according to the number of inhabitants.

employment of individuals living in municipalities where the EZ is located). We also have information on the person's main occupation in the previous year, and thus, we can identify workers who have moved within the areas and those who had not worked at all in the previous year: thus we can proxy for transitions from unemployment to employment.

These data allow us to probe the long-run effects of EZs, as well as temporal delays or extenuations. SIRENE and DADS are available from 1995 to 2013.[9] This means that we observe data for at least two years before the introduction of the EZ tax exemptions and up to fifteen years after.

Finally, the 1990 Population Census allows us to measure socio-demographic variables used for the designation of an area as an EZ. For this evaluation, the data have been aggregated at the three-tier classification levels presented in Section 3: EZ, ZRU and ZUS.[10]

5. Identification issue and empirical strategy

5.1. Identification issues and the selection process of the EZ program

We use as a control group the set of all ZRUs, which are the non-beneficiary areas most similar to EZs. The panel data allow us to eliminate the potential fixed effects specific to each area. More precisely, our main variables of interest are (log-) outcome-level differentiated based on data from 1995, meaning two years prior to the introduction of the tax exemptions (the aim of using such a lag is to avoid capturing potential anticipation effects of the measure). Time differentiation is not sufficient to accurately estimate the causal impact of the EZ program. Indeed, the EZs were chosen from among the ZRUs suffering from multiple economic handicaps that may also have had an impact on the economic outlook. However, the two-step assignment process does provide us with an identification strategy.

First, the eligibility condition based on the size of the areas (in terms of inhabitants estimated in the 1990 Population Census) ensures that non-EZ areas comparable to EZs in terms of socio-economic development can be found within smaller areas. Indeed, almost all EZs have more than 10,000 inhabitants (see Fig. 2).[11] This assumption is

Table 2
Average socio-economic indicators of the urban areas.

	EZ	ZRU		ZUS
		> 10, 000 pop	< 10, 000 pop	
Number of zones	45	69	282	321
Unemployment rate	21.9	18.2	24.3	17.9
Percentage of social housing	64.4	63.7	66.0	61.1
Percentage of foreign people	21.8	16.3	20.0	17.2
Percentage of unskilled people	43.1	36.4	45.8	37.9
Percentage of young people (aged under 25)	46.7	43.2	45.5	41.2
Average potential tax revenue (in euros)	2,707	3,212	2,609	3,438

Note: The average potential tax revenue is measured in 1996.
Source: Population Census 1990, INSEE.

supported by the descriptive statistics for the socio-economic characteristics (see Table 2). For each criterion (unemployment rate, percentage of social housing, the percentages of young people, foreign people and unskilled people in the area, and potential tax revenue in the municipality), average figures in small ZRUs (meaning those populated by fewer than 10,000 inhabitants) appear to be the closest to the EZs. For instance, the average unemployment rate is 22% in EZs, while it is "only" 18% in large ZRUs but 24% in small ZRUs. The proportion of unskilled people is 43% in EZs, while it is 36% (respectively 46%) in large (respectively small) ZRUs.

Second, as we know and measure the characteristics used in the EZ designation, we can accurately control for differences arising from this selection process. This suggests the use, common in this literature, of estimation based on the propensity score method. One should emphasize that this method is very similar to previous evaluations of the EZ program, which focused on its second wave that was implemented in 2004. For instance, Givord et al. (2013) also adopt a propensity score approach. However, their identification relies on slightly different conditions. For the first wave of the EZ program, the use of the exogenous population criterion provides a large pool of small control areas that are very similar to the EZ areas in terms of socio-economic deprivation. For the second wave, this size criterion was not applied as stringently as it was in the first wave: not only the remaining less-deprived "large" areas were affected by the second wave of the EZ program. Indeed, some new EZs were formed by joining several smaller (among the most disadvantaged) areas. Furthermore, the implicit objective was to obtain an even distribution of EZs at a national scale and, thus, to "provide" an EZ in regions where no first-wave EZ was created. As Givord et al. (2013) cannot rely as much as here on the exogeneity provided by the size criterion, they use, as an additional source of identification, the distance to a previous EZ or smaller disadvantaged areas.

5.2. Subclassification on the propensity score and regression

In practice, we compare the evolution of outcomes in EZs by using areas that do not benefit from the EZ program but are similar in terms of socio-economic characteristics. Specifically, our main assumption combines the standard "common trend assumption" and "conditional independence assumption" (CIA, or unconfoundedness assumption). They state that, in the absence of the policy, no difference would have been observed in the temporal changes in outcomes across zones with comparable observable characteristics. This method is often named "conditional difference-in-differences".

We cannot formally test this assumption, but we can check whether the evolution of outcomes was similar *before* the introduction of the program in the future EZs and in areas similar in terms of characteristics. Geolocalized data are not available before 1995, but we can use

[9] We face a break in the SIRENE gross time series in 2008 and 2009, first because of a change in data field and then the introduction of a new status for self-employed persons. This is innocuous for our estimation strategy provided that the impact on self-employed is the same in the EZ and ZRU groups, an assumption that seems plausible. In all cases, this fact should not have any impact on the estimations relying on the DADS database.

[10] Details on data construction are provided in the online Appendix.

[11] With the exception of four areas, two very small zones that were merged into a larger EZ and two areas that are just below the threshold, with 9,538 and 9,927 inhabitants.

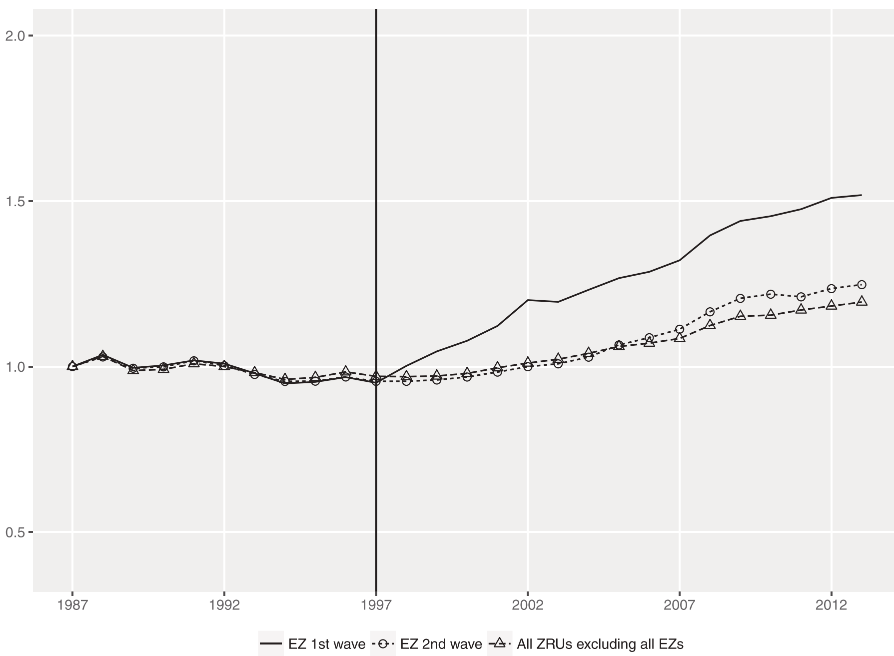

Fig. 3. Change in the number of businesses located in EZs and ZRUs (baseline level, 1987) Sources: French business register (SIRENE). The number of business is available at the municipal level. Each municipality is weighted by the share of businesses within the boundaries of the urban area as observed in 1995 when using geolocalized data.

data at a coarser level, meaning the smallest administrative division available (data at the municipal level[12]). We may then compare the temporal trends over a ten-year period before the introduction of the EZ. We consider as "treated" municipalities those that encompass an EZ, and we use as a control group those that contain a ZRU.[13] Fig. 3 illustrates that the temporal trends observed in the number of businesses were very similar in both groups before the introduction of the first EZ (as well as the number of business creations, see supplementary Figure in online Appendix).

For estimation, we rely on propensity score matching methods. As shown by Rosenbaum and Rubin (1983b), if the CIA holds for observables X, it also holds for the propensity score $P(T_i = 1|X)$, meaning the probability of an area being designated as an EZ, conditional upon observables. In practice, we use as control variables the indicators formally used for the designation of EZs.

However, as our sample size is small, simple propensity-score matching might lead us to compare units with different observable characteristics (as areas with close propensity scores might nevertheless have different observable characteristics). To address this issue, we adopt a strategy that combines regression and propensity score methods to obtain the final estimate of the impact of the EZ program. More precisely, we define four strata corresponding to the level of the propensity score and perform a linear regression using observable covariates X. As discussed by Imbens and Wooldridge (2009), linear regression (originally suggested by Rosenbaum and Rubin, 1983a) helps to eliminate potential remaining bias and improves precision. Within each block, the propensity score does not vary substantially, and the covariate distributions are, on average, similar between the two groups. This ensures that the regression function will not extrapolate, perhaps erroneously, into regions outside the data range. Formally, and using notation posited by Imbens and Wooldridge (2009), we perform, for each year, a linear regression in each stratum j:

$$\Delta_{1995} log(Y_i) = X_i \beta_j + \delta_j T_i + u_{ij} \tag{1}$$

The estimate of the average treatment effect on the treated (ATT) corresponds to the weighted average of these local estimates.[14]

The estimation crucially relies on the estimation of the propensity score. It has to be adapted here to the specific setting created by the eligibility condition based on the number of inhabitants in the area. This size condition reinforces the credibility of our identifying assumption, as it ensures that the characteristics of the control group units are similar to those of the EZ units. Indeed, as discussed in Section 5.1, Table 2 shows that the largest ZRUs (those above the 10,000-inhabitant threshold) are on average better off than EZs on all others indicators used for the designation of an EZ (unemployment rate, average potential tax revenue, proportions of foreigners, youths and unskilled, etc.). Even if we expect that the subclassification on the propensity score will solve part of the selection issue of large ZRUs, considering our small sample size, we do not want to exclude small areas from our control group, as they are very similar to EZs on all these indicators.

However, the size conditions create a non-linearity in the dependence of the propensity score on observable characteristics and, specifically, on size. If some observable characteristics used for the score are correlated with the size of the area (or, in other words, if the distribution of observables is not the same in small and large areas as shown in Table 2, for instance), following the standard practice of using a common logit regression, which relies on a linear specification on observables, may lead to biased estimates of the propensity score. We in fact face a censoring problem: as we do not observe treated observations with a size under the threshold, we cannot correctly estimate the

[12] Municipal level corresponds to the French "communes" identified by ZIP code.

[13] Most of the EZs are located in municipalities that also encompass a ZRU. For obvious reasons, we do not consider these municipalities in the control group, although this corresponds to a very rough proxy for the actual outcome in the EZ. The share of the EZ or ZRU outcomes in the outcomes measured at the municipal level is indeed usually small (approximately 3% on average in 1995) but varies greatly: it ranges from 1% to 90% depending on the area considered. We use as a proxy for the outcomes in EZs and ZRUs based on municipal-level outcomes this share as observed in 1995. This is still a noisy measure of the real outcomes (especially for the period after the introduction of the program), but it increases the comparability of the different units.

[14] The final estimate of the impact of the tax subsidies on the EZ δ_{ATT} corresponds to $\hat{\delta}_{ATT} = \sum_{j=1}^{J} \frac{N_{jEZ}}{N_{EZ}} \hat{\delta}_j$ and an estimate of its variance is $\hat{V} = \sum_{j=1}^{J} \left(\frac{N_{jEZ}}{N_{EZ}}\right)^2 \hat{V}_j$ where $(\hat{V}_j)_{j=1,\cdots,J}$ corresponds to the estimated variances of $(\hat{\delta}_j)_{j=1,\cdots,J}$ (assuming that the residuals for different strata are independently distributed, which is a standard assumption in this type of method) and N_{jEZ} and N_{EZ} denote the number of EZs in strata j and in the whole sample, respectively. We introduce the number of inhabitants in the area as an additional covariate in (1) as an informal test of the assumption of conditional independence of outcome and size. It is never significant.

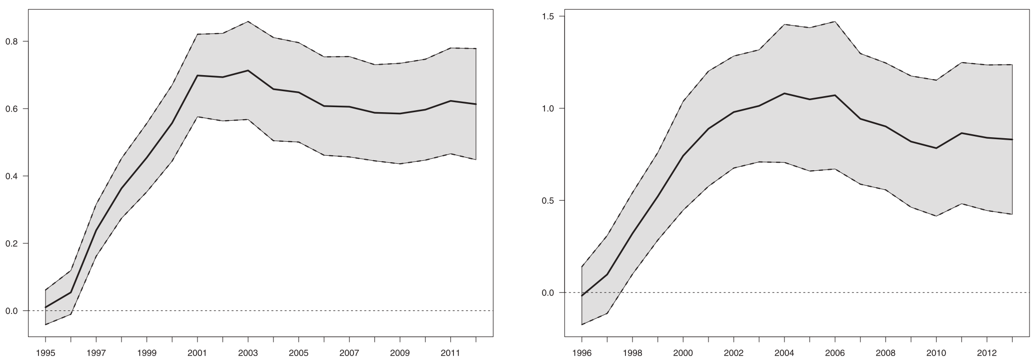

Fig. 4. Impact of EZ program on the (log) number of businesses (left panel) and (log) employment (right panel).

correlation between observed covariates and the score.[15] This is an issue for the estimation. The rationale for using matching on the propensity score is that for similar values of the propensity score, we expect to have similar average values of covariates for the treated and control observations, and we thus estimate the impact of the measure by comparing the average observed outcomes in both subsamples (meaning that the propensity score is a balancing function; see Rosenbaum and Rubin, 1983a or Imbens and Wooldridge, 2009). However, as we rely on an estimation of the propensity score, this property may fail if the estimation of the score is misspecified (as similar values of the biased score may lead to subsamples of treated and controls that are very different in terms of covariates).

To solve this technical issue, we explicitly take into account in the estimation the non-linearity created by the assignment rule in the EZ program. Specifically, we assume that the fact of being selected as an EZ T_i can be decomposed as follows: $T_i = D_i Z_i$, where the dummy $D_i = 1_{S_i > \underline{S}}$ indicates whether the size is greater than the threshold, and Z_i corresponds to the fact of being selected as an EZ (independent of size) that may depend on other covariates X. The propensity score matching framework requires us to have a correct estimate of the conditional probability $P(T_i = 1|X_i)$, which may be decomposed as $P(Z_i = 1|X_i, D_i = 1)P(D_i = 1|X_i)$. Under mild assumptions, we can separately estimate the two components.[16] This decomposition is simply intended to obtain an accurate estimation of the "true" propensity score $P(T_i = 1|X_i)$. Concretely, this means that we estimate, as a function of the covariates, both the probability of being an EZ (restricting the sample to areas with populations above the threshold) and of having a population above the threshold. The second estimation has no causal interpretation but corrects for misspecification due to differences in the distributions of the covariates in large and small areas. In both cases, we rely on logistic specifications. Details on the estimation are provided in the online Appendix.

An alternative strategy using the threshold condition is to restrict the sample to areas with very similar size. This is indeed the intuition behind the regression discontinuity method, which takes into account the "fuzzy" designation process (see the online Appendix). We also check that our results are robust when applying this method.

5.3. Interactive fixed effects methods

The conditional difference-in-differences method crucially relies on a common trend assumption between EZ and control areas. This assumption may failed. For instance, one can imagine that the decision to designate an area as an EZ included, in some way, the economic prospects of the area, which are captured by characteristics observed by local observers but not in our data. Policy makers may have prioritized areas that were the most severely affected by an unobserved economic shock or, conversely, those areas that had the most positive economic dynamics. In such cases, our difference-in-differences strategy would lead to biased estimates, as it would confound the consequences of these unobserved economic shocks with those of the EZ program.

In order to relax the common trend assumption, we follow Gobillon and Magnac (2016), who review several alternatives to difference-in-differences methods for the evaluation of regional policies, and apply the interactive fixed effects method proposed by Bai (2009). This specification addresses potential endogeneity issues (as unobservable common shocks that may similarly affect set of local areas) in the estimation of the EZ impact by setting factor loadings. Factor loadings for a given area are more or less high depending on whether this area is more or less affected by these shocks. However, this specification requires for estimation to observe long pre-treatment period in order to identify common factors. In practice, we apply this interactive fixed effects method to the data at the municipal level. Specifically, we now estimate the equation :

$$\Delta_{1995}\log(\mathrm{Y}^{\mathrm{m}}_{\mathrm{jt}}) = \alpha_j + \delta_t + X_{jt}\beta + f_t'\lambda_j + \epsilon_{jt} \tag{2}$$

where Y^m_{jt} is the outcome measured at the municipality level, α_j is the area fixed effect, δ_t represents period fixed effects, f_t is an Lx1 vector of unobserved common factors, λ_j is a Lx1 vector of factor loadings that capture unit-specific responses to the common shocks, and X_{jt} are treatment status of unit j in period t. We thus measure, as in our previous estimations, the evolution of the impact of the EZ program over time.

Gobillon and Magnac (2016) test various specifications that vary in how the common factors are estimated. According to their Monte Carlo experiments, the Bai's estimator obtained using the full sample performs best both in terms of precision and bias.[17]

6. Results

6.1. Impact on economic activity

According to our results, the EZ program has a strong impact on economic activity in targeted areas. Fig. 4 displays the cumulative impact of the EZ program over time on the number of businesses and salaried employment (detailed estimations are available in the online Appendix). The estimates measure, for each year, the causal impact δ_t of the EZ program on $\Delta_{1995}log(Y_t)$ compared to its counterfactual level

[15] Adding size as an additional variable, or even interacting size with the observables, is not sufficient to address this censoring issue.

[16] The assumption states that in the absence of this eligibility condition, the fact of being EZ Z_i is independent of being a "large" area conditional on the characteristics X. One may easily show that the likelihood of the observations (D_i, Z_iD_i) is separable in both components. In practice, it is T_i which is observed and not Z_i, but we have $P(Z_i = 1|X_i, D_i = 1) = P(Z_iD_i = 1|X_i, D_i = 1) = P(T_i|X_i, D_i = 1)$.

[17] The estimation relies on a recursive estimation procedure. The initialization step consists of an ordinary least squares estimator that ignores the factor components. The procedure then iteratively estimates the factors and their loadings (obtained by a principal component analysis method applied to the least squares residuals) and the regression coefficients (using least squares estimation of the augmented regression with these PCA estimates) until numerical convergence. Statistical analysis was performed using the phtt R package (Bada and Liebl, 2014).

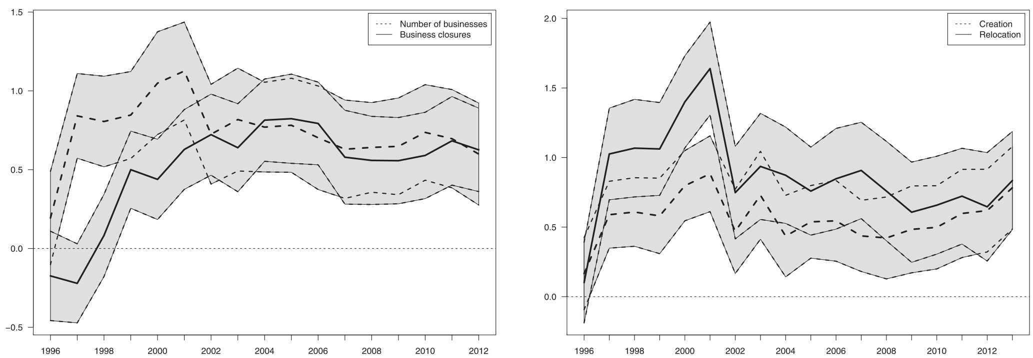

Fig. 5. Impact of EZ program on the (log) number of businesses located in Ezs and closed or relocated outside EZs (left panel) and business relocations and creations (right panel).

$\Delta_{1995} log(Y_t^0)$ (where Y_t^0 denotes the level of outcome that would have been observed in year t in the absence of the EZ program). They thus approximate the impact of the program on outcome growth from the beginning of the period. The ratio of this estimate observed in year t with the number of years since the EZ program's implementation provides an average impact on yearly outcome growth.

Simple differentiation of subsequent yearly estimates also provides an estimation of the impact of the program on yearly growth for these specific years.[18] We can also obtain the magnitude of the impact on the outcomes of interest. Simple calculations indicate that in each year t, the outcome level in EZs is e^{δ_t} higher than its counterfactual level.[19] The impact of the EZ program on the outcome in levels corresponds to $(1 - e^{-\delta_t})Y_{i,t}$.

We observe that tax exemptions result in a steady rise in the number of businesses over the first five years. In 2001, according to our estimates, the cumulative growth in the number of businesses located in EZs is 70 percentage points. As the program was implemented in 1997, this corresponds to an average increase of 14 percentage points of yearly growth in the number of businesses thanks to the EZ program. In other words, this means that the number of businesses in an EZ in 2001 is $e^{0.7} \approx 2$ times higher than the level that would have prevailed without the policy.

In terms of employment, our results suggest that the growth in the number of salaried employees in EZ businesses increased by 98 percentage points from the beginning of the period to 2001 compared to its counterfactual level. In 2001, the number of salaried employees in EZ businesses is 2.6 times higher than its counterfactual level. More concretely, back-of-the-envelope calculation suggests that over the first five years, the entire program would have resulted in the location of between 9,450 and 12,100 businesses, in the zones employing between 36,400 and 53,500 workers.

When analyzing the long-term effects of the program, we observe that the impact of the program on the number of businesses and on the level of employment stabilizes at a high level after five years. This can be considered as"steady state" for the program.

Recall that for one business located in a EZ, tax exemptions are granted for five years, provided that it remains within the area (whatever the date of the first location in the area). That means that starting from 2002, businesses that were the first to locate in EZs (in 1997) no longer benefit from full tax exemptions. Estimates show that from this date, the number of relocations outside the EZs and business closures cancel out the number of in-zone business locations (see the left panel in Fig. 5). Whereas between 1997 and 2001, EZs had a notably higher impact on the creation than on the shutdown of businesses with salaried employees, from 2002, the two levels are no longer significantly different.

One can speculate that the increase in the number of business closures observed in EZs is due to a negative impact of the program on incumbent businesses. Bondonio and Greenbaum (2007) observe that the EZ created in the US in the 1980s fostered the creation of businesses but that this positive effect was offset by losses and closures among existing businesses. Bondonio and Greenbaum interpret this impact as indicating that existing establishments in the U.S. case must suffer from a competitive disadvantage, as the incentives there target new establishments. Evidence from the French EZ program suggests, however, that the competitive impact on incumbent businesses from new establishments is not substantial. The EZ program has no significant impact on the closure rate of incumbent businesses (see supplementary Table S1 - column (6) in the online Appendix). This is presumably because in the French EZ program, tax exemptions are not targeted toward new establishments and also benefit incumbent businesses. However, it seems that the program has neither negative nor positive impacts on businesses already present in the EZs when the program was implemented. The employment in the businesses already present in the EZs before 1997 (see supplementary Table S2 in the online Appendix) did not noticeably increase thanks to the exemption.

A more plausible explanation for this high turnover observed after 2002 relies on small business demography. Very few small businesses, such as those targeted by the French EZ program, survive in the long run. Descriptive statistics show that in the non-treated areas, the survival rate of these businesses is only 40% after five years and less than 25% after ten years.[20] The EZ program does not substantially improve this situation. Although the survival rate of these businesses in the first years is slightly higher in EZs (thanks to tax exemptions) than in non-treated areas, this is no longer the case once the tax exemptions end. In fact, a detailed analysis reveals that this is mainly due to an increase in relocations outside the areas. After the conclusion of the tax exemption period, businesses may decide to relocate outside of the targeted areas.

These figures should be put into perspective. As there were numerous businesses created before the implementation of the EZs (they represent 82% of new locations in the EZs in 1995), in absolute value, they are still predominant in the targeted locations: they represent approximately 23% of new locations in EZs after 2002. Nevertheless, part of the economic activity generated by the EZ program is fueled by relocations.

In all cases, this high turnover suggests that a temporary EZ program is not sufficient to generate self-sustaining activity within EZs. The seemingly steady state observed after 2002 requires the continual founding of new businesses. Indeed, while the first French EZ program had been planned to progressively cease starting in 2001 (all businesses created in or moved into the EZs before December 31, 2001, still benefit from the tax exemption for five years, but these exemptions were expected to be denied to businesses created after this date), a change in the political majority in May 2002 led to the reactivation of the

[18] One can easily obtain that $\delta_t - \delta_{t-1} \approx \frac{Y_t - Y_{t-1}}{Y_{t-1}}$.

[19] Recall that the program was not in place in 1995, and thus, we have $Y_{1995} = Y^0_{1995}$.

[20] See supplementary Figure in online Appendix.

Table 3
Impact of the EZ program on changes in employment compared to 1996 levels, detailed by location and status.

	Residents	Non-residents	Share of residents among employment	Share of new residents among residents	Share of unemployed among employment
1996	0.094 [−0.053, 0.241]	0. 141* [−0.017, 0.3]	−0.024 [−0.106, 0.058]	0.001 [−0.298, 0.299]	0.198 [−0.152, 0.549]
1997	0.251*** [0.071, 0.432]	0.379*** [0.185, 0.572]	−0.058 [−0.163, 0.047]	0.195 [−0.109, 0.5]	0.378** [0.041, 0.716]
1998	0.434*** [0.237, 0.63]	0.568*** [0.331, 0.804]	−0.072 [−0.192, 0.048]	0.171 [−0.169, 0.511]	0.179 [−0.203, 0.56]
1999	0.587*** [0.345, 0.828]	0.798*** [0.463, 1.133]	−0. 111* [−0.242, 0.021]	−0.024 [−0.373, 0.326]	−0.024 [−0.387, 0.339]
2000	0.79*** [0.547, 1.033]	1.025*** [0.665, 1.385]	−0.109 [−0.26, 0.042]	0.163 [−0.177, 0.503]	0.08 [−0.285, 0.446]
2001	0.914*** [0.662, 1.167]	1.084*** [0.769, 1.399]	−0.066 [−0.209, 0.077]	0.212 [−0.106, 0.53]	0.183 [−0.174, 0.539]
2002	0.959*** [0.696, 1.221]	1.1*** [0.778, 1.423]	−0.058 [−0.213, 0.096]	0.02 [−0.331, 0.371]	0.201 [−0.215, 0.617]
2003	1.031*** [0.717, 1.345]	1.155*** [0.781, 1.529]	−0.054 [−0.209, 0.101]	−0.17 [−0.558, 0.218]	0.134 [−0.301, 0.569]
2004	1.015*** [0.681, 1.349]	1.119*** [0.719, 1.519]	−0.042 [−0.199, 0.116]	−0.221 [−0.607, 0.166]	0.093 [−0.295, 0.481]
2005	0.995*** [0.657, 1.334]	1.169*** [0.755, 1.583]	−0.079 [−0.244, 0.085]	−0.131 [−0.522, 0.26]	0.041 [−0.364, 0.446]
2006	0.842*** [0.532, 1.152]	1.08*** [0.711, 1.449]	−0.109 [−0.282, 0.064]	−0.027 [−0.39, 0.336]	0.05 [−0.326, 0.426]
2007	0.752*** [0.44, 1.064]	1.009*** [0.637, 1.38]	−0.107 [−0.269, 0.055]	−0.023 [−0.392, 0.346]	−0.062 [−0.433, 0.309]
2008	0.665*** [0.376, 0.954]	0.944*** [0.553, 1.335]	−0.132 [−0.311, 0.047]	0.045 [−0.295, 0.386]	0.113 [−0.282, 0.508]
2009	0.696*** [0.394, 0.998]	0.918*** [0.501, 1.336]	−0.087 [−0.305, 0.131]	0.095 [−0.262, 0.452]	0.005 [−0.402, 0.412]
2010	0.672*** [0.352, 0.993]	1.025*** [0.602, 1.447]	−0. 198** [−0.388, −0.007]	0.023 [−0.351, 0.397]	0.041 [−0.383, 0.465]
2011	0.688*** [0.351, 1.024]	1.009*** [0.577, 1.441]	−0. 162* [−0.356, 0.031]	0.031 [−0.31, 0.372]	−0.091 [−0.513, 0.331]
2012	0.711*** [0.358, 1.064]	0.998*** [0.568, 1.428]	−0.137 [−0.317, 0.043]	0.045 [−0.343, 0.434]	−0.127 [−0.531, 0.276]

Notes: Estimates of the impact of the EZ program on $\Delta_{1995}\log(Y_{it}+1)$, the difference in log outcomes between year t and 1996. Estimations are based on propensity score and subclassification (four strata based on the propensity score) using all covariates in the index and the number of inhabitants in the area. Sample size: 394 observations (42 EZs and 352 ZRUs). Confidence intervals at 95% in brackets. Significance levels: ***1%, **5%, *10%. Sources: Administrative employer-employee database on wages (DADS) and French business register (SIRENE), INSEE.

program. It has been continually renewed since then.[21]

6.2. Impact on local population

Moreover, the impact of the EZs has to be compared to their original purpose, that is, to contribute to urban renewal. Increasing the economic activity of businesses was viewed as a way to improve the situation of local residents and low-skilled workers. This was also assumed to increase the quality of living in these areas, by improving the supply of local or social services. Estimates on both resident employment and industry suggest that the EZ program has only partly achieved this primary purpose. Indeed, the French EZ program does not explicitly target resident employment or local services. Concerning the latter, it provides tax exemptions for the entire workforce of the targeted businesses, not only for resident workers. As emphasized above, the local employment clause is not very constraining.

The impressive impact of the EZ program on local employment may partly benefit workers who live farther afield and not only the resident population. The precise data allow us to distinguish between resident employees (i.e., employees who live in the municipality in which the EZ is located and who work in the EZ) and non-resident employees. Estimates indicate that resident employment did increase at a steady pace between 1997 and 2004 thanks to the EZ program (see Table 3).[22] However, this pace is slower than that observed for non-resident employment, and the proportion of resident employment in total employment decreased: it declined from 30% in 1996 before the program to 26% 15 years later. Note, however, contrary to the results obtained by Reynolds and Rohlin (2015) for the Federal Empowerment Zone program in the US (indicating an increase in the – apparently not targeted – high-income households in those zones), the French EZ policy does not induce workers to move into the EZs because they anticipate that more job opportunities are available in these areas (or were encouraged by their employers to move, to satisfy the local hiring clause). The share of new residents in residents overall remains flat throughout the period considered (at an average level of 5%). However, the policy does not seem to have any impact on the proportion of employees who were not employed (at all) during the previous year (this proportion is 8% on average over the period considered). This last finding is in line with Gobillon et al. (2012), who specifically focus on the Paris metropolitan region and show that the EZ program had only a small and non-persistent effect on the unemployment rates of people living in the cities targeted by the EZ program. Note that in all cases, resident employment can only be defined at the municipal level, as workers' place of residence is not known as precisely as business location. The impact of EZs on resident inhabitants may consequently be even smaller, as we include employees who do not live in the EZ.

Moreover, the estimates suggest that the program did have a positive impact on unskilled workers: after five years, unskilled employment in these areas increased to 2.7 times the level that we should have expected in the absence of the policy (see online Appendix). Low-skilled residents are indeed over-represented in EZs, and a positive effect on low-skilled workers could be considered an achievement of the EZ program. However, while low-paid workers benefit from higher subsidies, the impact on low-skilled employment is not significantly different from those observed for skilled employment and high-skilled employment. A positive gap is observed when comparing point estimates for low-skilled and higher-skilled employment after 2002, but it is never statistically significant.

Another way of evaluating the impact of the program on the resident population is obtained by disaggregating the results at the industry level. Policy makers originally intended to support local amenities, for instance small retail shops such as bakeries, and professional services such as physicians. These correspond to the industrial sectors defined as "trade" and "health, education and community services," respectively. According to the estimates, the EZ initiative had a positive impact on both sectors (see Table 4). The increase in the number of trade businesses and health businesses from 1995 to 2012 is 43 and 36 percentage points higher than the corresponding counterfactual levels, respectively. However, the impact is smaller than the overall effect

[21] The sharp increase in the number of business locations, and especially relocations, in 2001 (the right panel of Fig. 5) reflects this – erroneous – anticipation of the end of the measure. The announcement of the end of the measure likely hastened the creation or relocation of businesses – which were incentivized to benefit from five-year tax exemptions.

[22] The reference period is 1996 instead of 1995, as in the main specifications, because some of the variables use lagged information (new residents and the formerly unemployed), which is not available before 1996 in our data. For the sake of simplicity in Table 3, we use 1996 as the reference year even for variables available from 1995 (residents, non-residents and the corresponding shares). The results are similar whatever reference year is used.

Table 4
Impact of the EZ program on changes in the number of businesses compared to 1995 levels by industry.

	Business services	Trade	Health, education, community work	Construction	Manufacturing
1995	−0.011 [−0.141, 0.12]	0.001 [−0.074, 0.076]	0 [−0.066, 0.066]	0.019 [−0.117, 0.155]	−0.046 [−0.147, 0.054]
1996	0.083 [−0.078, 0.244]	0.059 [−0.031, 0.149]	−0.009 [−0.105, 0.086]	0.024 [−0.139, 0.186]	−0.007 [−0.153, 0.139]
1997	0.519*** [0.333, 0.705]	0.203*** [0.093, 0.314]	0.044 [−0.086, 0.173]	0.272*** [0.096, 0.449]	0. 139* [−0.024, 0.301]
1998	0.842*** [0.615, 1.07]	0.223*** [0.103, 0.343]	0.123 [−0.025, 0.272]	0.477*** [0.283, 0.672]	0.276*** [0.104, 0.448]
1999	0.941*** [0.716, 1.166]	0.284*** [0.15, 0.418]	0.191** [0.019, 0.362]	0.585*** [0.37, 0.801]	0.339*** [0.144, 0.534]
2000	1.111*** [0.872, 1.349]	0.352*** [0.213, 0.491]	0.243*** [0.067, 0.42]	0.668*** [0.429, 0.908]	0.488*** [0.273, 0.703]
2001	1.388*** [1.148, 1.628]	0.427*** [0.283, 0.571]	0.355*** [0.175, 0.534]	0.785*** [0.536, 1.034]	0.605*** [0.371, 0.838]
2002	1.372*** [1.141, 1.604]	0.429*** [0.277, 0.581]	0.353*** [0.168, 0.537]	0.816*** [0.566, 1.066]	0.572*** [0.339, 0.805]
2003	1.306*** [1.07, 1.542]	0.49*** [0.328, 0.652]	0.39*** [0.196, 0.585]	0.737*** [0.479, 0.995]	0.578*** [0.34, 0.816]
2004	1.231*** [0.987, 1.474]	0.418*** [0.242, 0.594]	0.345*** [0.151, 0.54]	0.634*** [0.368, 0.9]	0.547*** [0.283, 0.81]
2005	1.234*** [0.972, 1.495]	0.403*** [0.237, 0.57]	0.352*** [0.161, 0.543]	0.632*** [0.36, 0.903]	0.56*** [0.294, 0.827]
2006	1.121*** [0.854, 1.387]	0.372*** [0.2, 0.545]	0.395*** [0.194, 0.596]	0.572*** [0.304, 0.841]	0.554*** [0.273, 0.836]
2007	1.025*** [0.746, 1.304]	0.382*** [0.212, 0.552]	0.459*** [0.238, 0.68]	0.537*** [0.274, 0.799]	0.542*** [0.259, 0.824]
2008	1.005*** [0.722, 1.288]	0.352*** [0.192, 0.512]	0.42*** [0.196, 0.645]	0.626*** [0.362, 0.89]	0.494*** [0.227, 0.762]
2009	1.003*** [0.694, 1.313]	0.352*** [0.182, 0.522]	0.393*** [0.162, 0.625]	0.607*** [0.339, 0.875]	0.51*** [0.23, 0.789]
2010	1.005*** [0.702, 1.308]	0.383*** [0.209, 0.558]	0.451*** [0.202, 0.7]	0.541*** [0.276, 0.806]	0.526*** [0.235, 0.817]
2011	1.03*** [0.721, 1.34]	0.391*** [0.22, 0.562]	0.464*** [0.203, 0.724]	0.543*** [0.275, 0.811]	0.52*** [0.229, 0.812]
2012	1.037*** [0.714, 1.36]	0.394*** [0.208, 0.58]	0.492*** [0.224, 0.761]	0.519*** [0.237, 0.8]	0.457*** [0.174, 0.741]

Notes: Estimates of the impact of the EZ program on $\Delta_{1995}\log(Y_{it}+1)$, the difference in log outcomes between year t and 1995. Estimations based on propensity score and subclassification (four strata based on the propensity score) and using all covariates in the index and the number of inhabitants in the area. Sample size: 394 observations (42 EZs and 352 ZRUs). Confidence intervals at 95% in brackets. Significance levels: ***1%, **5%, *10%. Sources: Administrative employer-employee database on wages (DADS) and French business register (SIRENE), INSEE.

estimated.

Furthermore, a closer examination suggests that the industrial sector most responsive to tax breaks is business services. The impact of the EZ program is impressive here, as the estimated impact on the fifteen-year growth in the number of business service businesses in the area is 104 percentage points higher than its counterfactual level. In the first years of the program, this increase is even more impressive. On average, over the first five years, the *yearly* growth increases by 28 percentage points. These businesses correspond, for instance, to IT services or office cleaning services, meaning companies with activities that are not necessarily performed in the neighborhood but have a legal address that can easily be located within the EZ. Such companies may also relocate easily when they no longer benefit from tax exemptions.

6.3. Robustness checks

Our results suggest that the first wave of the EZ policy did have strong short-term impacts on economic activity, in contrast to previous evaluations that focus on the second wave of the EZ policy and report a much smaller impact of the – in principle – same policy (see, for instance, Givord et al., 2013). This calls for a careful examination of the validity of our results, and we thus perform several robustness checks.

The difference-in-differences strategy relies on the assumption that we do not observe any different time trend in EZ and control groups. This assumption may not hold if EZs were chosen from among areas with the most promising economic prospects, something that may be not captured by observable characteristics.

First, following the intuitions of Fig. 3, we challenge the so-called common trend assumption and compare the economic trend in our EZ group and in the areas used as controls *before* the introduction of the program. We apply the same estimation method but use the period prior to the implementation of the EZ program (corresponding to a "placebo" or "falsification" test). When using both geolocalized data[23] (see Tables 3 and 4 for instance, as well as detailed results in the online Appendix) or data at the coarser municipal level (available ten years before the introduction of the program, see supplementary Table S3 in the online Appendix), we cannot reject the null hypothesis of a null impact of being an EZ before the implementation of tax exemptions in 1997.

However, in the latter case, one should be cautious, as when using data at the municipal level instead of the more accurate area level, the underlying differences between the future EZ and control areas may be underestimated. Intuitively, the estimation of the impact of EZs is "diluted". Observations at the municipal level combine EZ areas and areas that never benefited from the EZ policy. The growth in outcome measures at municipal level $\Delta_{1995}\log(Y_t^m)$, corresponds to a weighted average of the growth in outcome (for instance, employment) located in the EZ and the non-EZ part of the municipality.[24] However, in the estimation, it is assumed that the entire territory of the municipality does benefit. This "contamination bias" (referring to social experiment terminology) results in estimates that are smaller by a factor corresponding to the proportion of the municipality actually affected by the EZ program. The estimated impact when using coarser data is thus always smaller than the true impact.[25] We can approximate this bias, using geolocalized data when available. For instance, the average share $\overline{\pi_j}$ of businesses that are located within the boundaries of an EZ as observed in 1995 is 16.7%. The estimated impact of the EZ program when using accurate data is 0.239 for the first year of the program in 1997. We observe that the corresponding estimate when using municipal-level data is 0.041, which is indeed close to the product $0.239 \times 0.167 = 0.0399$.

Moreover, because of this attenuation bias, our placebo analysis may also fail to detect previous differences between future EZ and control municipalities. Indeed, using classical hypothesis testing and relying again on a linear approximation, the minimum detectable effect

[23] Available only from 1995, that is two years before the introduction of the program.

[24] $\Delta_{1995}\log(Y_t^m) \approx \frac{Y_t^m - Y_{t0}^m}{Y_{t0}^m} = \frac{Y_{t0}^{ZE}}{Y_{t0}^m}\frac{Y_t^{ZE} - Y_{t0}^{ZE}}{Y_{t0}^{ZE}} + \frac{Y_{t0}^{nonZE}}{Y_{t0}^m}\frac{Y_t^{nonZE} - Y_{t0}^{nonZE}}{Y_{t0}^{nonZE}}$.

[25] To see this, consider the estimation of the program by simple OLS regression. The estimation at the area level for a year t is $\Delta_{1995}\log(Y_{it}) = X_i\beta + T_i\delta + u_i$. When aggregating equation at the municipal level, we obtain $\Delta_{1995}\log(Y_{jt}^m) = \widetilde{X}_j\beta + T_j\pi_j\delta + \tilde{u}_j$ where $\widetilde{X}$ and $\tilde{u}$ corresponds respectively to the weighted average of variables X and u measured in the EZ (if any) and non-EZ parts of the municipality j, π_j the weight in 1995 of the EZ areas in municipality j, and T_j a dummy for encompassing at least one EZ. When ignoring that only a part of the municipality benefits from the EZ program, the estimation equation is simply $\Delta_{1995}\log(\widetilde{Y}_{jt}) = \widetilde{X}_j\beta + T_j\tilde{\delta} + \tilde{u}_j$. Combining both equations, we easily obtain that $E(\tilde{\delta}) = \overline{\pi}E(\delta)$. As $\pi_j \le 1$, we have that $\overline{\pi} < 1$.

Table 5
Impact of the EZ program on changes in the number of businesses compared to 1987 levels - estimations at the municipal level with (k) linear factor models.

	Number of factors				
	1	2	3	4	5
1996	− 0.003	− 0.003	0.011*	0.007*	0.002
	(0.008)	(0.006)	(0.006)	(0.004)	(0.005)
1997	0.030***	0.020**	0.047***	0.036***	0.021*
	(0.009)	(0.010)	(0.013)	(0.011)	(0.012)
1998	0.050***	0.032**	0.075***	0.052***	0.035*
	(0.011)	(0.015)	(0.022)	(0.018)	(0.020)
1999	0.064***	0.040**	0.094***	0.064***	0.048*
	(0.013)	(0.020)	(0.028)	(0.023)	(0.026)
2000	0.084***	0.050*	0.117***	0.080***	0.066*
	(0.014)	(0.026)	(0.036)	(0.030)	(0.034)
2001	0.117***	0.072**	0.153***	0.107***	0.097**
	(0.016)	(0.033)	(0.045)	(0.037)	(0.044)
2002	0.112***	0.065*	0.149***	0.102***	0.101**
	(0.017)	(0.036)	(0.047)	(0.039)	(0.050)
2003	0.121***	0.066	0.158***	0.107**	0.123*
	(0.018)	(0.041)	(0.054)	(0.044)	(0.065)
2004	0.124***	0.060	0.154***	0.108**	0.127*
	(0.018)	(0.047)	(0.059)	(0.048)	(0.071)
2005	0.124***	0.050	0.147**	0.106**	0.134
	(0.019)	(0.054)	(0.066)	(0.052)	(0.083)
2006	0.127***	0.041	0.139*	0.108*	0.141
	(0.020)	(0.062)	(0.073)	(0.057)	(0.092)
2007	0.134***	0.035	0.134*	0.113*	0.140
	(0.021)	(0.071)	(0.081)	(0.063)	(0.097)
2008	0.136***	0.027	0.125	0.115*	0.135
	(0.022)	(0.078)	(0.087)	(0.067)	(0.098)
2009	0.139***	0.022	0.116	0.118*	0.131
	(0.023)	(0.083)	(0.091)	(0.070)	(0.098)
2010	0.137***	0.013	0.105	0.119	0.120
	(0.024)	(0.088)	(0.096)	(0.074)	(0.096)
2011	0.141***	0.008	0.097	0.122	0.110
	(0.025)	(0.094)	(0.100)	(0.078)	(0.095)
2012	0.138***	− 0.000	0.087	0.120	0.100
	(0.026)	(0.097)	(0.103)	(0.080)	(0.095)

Notes: Each estimation includes individual and time fixed effects. Number of periods before treatment : 9. Number of observations (treated : 45, untreated : 281). Standard errors in parentheses are robust to heteroskedasticity and correlation in the time dimension. Significance levels: ***1%, **5%, *10%. Sources: Administrative employer-employee database on wages (DADS) and French business register (SIRENE), INSEE.

that can be estimated when applying our difference-in-differences strategy is rather high.[26] And because of this lack of accuracy (and the small sample size), we may fail to detect differences between future EZ and control areas. In other words, the common trend assumption between these two groups may be invalid.

As an additional robustness check, we use a specification that relaxes this common trend assumption. Specifically, we apply the interactive fixed effects method described in Section 5.3. This specification addresses potential endogeneity issues (as unobservable common shocks that may similarly affect set of local areas) in the estimation of the EZ impact by setting factor loadings. In practice, this specification requires us to specify the number L of factors that would be estimated, which is unknown. Several tests have been proposed to determine it (see, for instance, Bai and Ng, 2002 and Ahn and Horenstein, 2013). In our case, the estimated optimal number of factors varies from 3 to 5 (in addition to temporal and individual fixed effects). Table 5 shows the estimations of the impact of the EZ interacted with each period after the introduction of the program on the total number of establishments located in the EZ. Reassuringly, our general conclusions do not vary with the number of factors. Consistent with previous estimates, we observe that the EZ program has a significant positive effect on the total number of businesses.[27] However, as we use municipal-level data, the magnitude of the impact is lower than the results obtained using accurate data.

Another concern we want to address is whether our control group provides an accurate counterfactual of the situation without local taxes. The counterfactual situation we wish to measure is the total absence of any tax exemptions. As businesses located in ZRUs do benefit from some (limited) tax exemptions, our results may underestimate the total effect of the EZ program on economic activity, if these exemptions also have a positive impact. We thus estimate the impact of the ZRU program, applying the same methodology as for the EZ program. ZRUs are now considered as the treated group. We use disadvantaged urban areas that do not benefit from tax breaks as a control group (i.e., ZUSs, the first tier of French urban renewal policy; see Section 3). According to our estimates, the tax exemptions provided by the ZRU program are inefficient at fostering economic activity (see supplementary Table S5 in the online Appendix). The changes in the number of businesses and business creations in ZRUs are never significantly different from the case in ZUSs over the full period. The ZRU program thus had no significant impact on economic activity, and we may be confident that ZRUs provide a suitable counterfactual for estimating the impact of the EZ program. Indeed, compared to EZs, ZRUs provide considerably less generous tax breaks to businesses, and moreover, they apply only to new businesses (corporate and local business taxes) or new hires (the payroll tax). Recent studies suggest that this specific tax scheme is less likely to attract businesses. Duranton et al. (2011) find no impact of local taxation on non-residential property on entry by English manufacturing establishments, and Rathelot and Sillard (2008) find a significant but negligible impact for French businesses.[28]

7. Discussion and conclusion

7.1. Main results and discussion

In conclusion, the overall efficiency of a place-based policy such as the French EZ program in creating employment is unclear. Our results suggest that this program was able to attract businesses to disadvantaged areas.

French businesses appear to be strongly reactive to tax breaks proposed by the EZ initiative. The changes in the number of businesses and in the level of local employment are impressive. Five years after the introduction of the policy, the number of businesses doubled compared to the level that would have prevailed without the tax exemptions. Regarding the resident population, the consequences are also significant. We observe an increase in resident and unskilled employment, as well as a clear but weaker rise in industries that provide local services. After five years, the impact of the EZ program stabilizes.

However, a closer examination of the results led to a less optimistic appraisal. While the policy was presented as a way of "bringing jobs to the unemployed" and to the inhabitants of these disadvantaged areas, resident workers and unemployed people represent only a minor part of

[26] For a power $\kappa = 0.80$ (the probability of correctly rejecting the fact that the program had no effect) and a significance level $\alpha = 0.05$ (the probability of incorrectly accepting that the program had an effect), the minimum detectable effect is approximately $\beta_{MDE} = \frac{\sigma_u}{E(\pi_j)} \frac{t_{1-\kappa} + t\alpha/2}{\sqrt{Np(1-p)}} \approx \frac{\sigma_u}{0.167} \frac{1.96 + 0.84}{\sqrt{326 \times 0.14(1-0.14)}} \approx 2.68\sigma_u$, where N is the number of observations, p the proportion of EZs in the sample, $t_{1-\kappa}$ and $t_{\alpha/2}$ are quantiles of the normal distribution, and finally, σ_u is the standard error of the outcome. The precise formula is $\beta_{MDE} = \frac{\sigma_u}{E(\pi_j)} \frac{t_{1-\kappa} + t\alpha/2}{\sqrt{Np(1-p}} \frac{1}{\sqrt{1 - \frac{\sigma_\pi^2 (t_{1-\kappa} + t\alpha/2)^2}{\pi^2 (Np(1-p))}}}$ in which the last term is very close to 1.

[27] Similar results are obtained for the number of establishment openings in these areas, see supplementary Table S4 in the online Appendix.

[28] We have also checked that we obtain similar conclusions when excluding areas selected for the second and third waves of the EZ policy from the control group. Some ZRUs, which are included in our control group, became EZs in 2004 and 2007 (the second and third waves of the EZ program). Our main results are thus slightly underestimated for the end of the period, as the control group contained some of these future EZs. We observe slightly higher results in 2004 with the new EZs excluded from the control group than in the main specification (see again the online Appendix). However, these small differences are never significant and do not alter our main conclusions.

hirings resulting from the EZ program. Moreover, the EZ initiative also does not appear to have been able to improve the situation of local amenities in the targeted areas. It has a positive effect on location decisions by businesses engaged in providing local services (trade, health or community services), as the number of these businesses increased by 50% after ten years. However, this figure is much smaller than the corresponding one for the "footloose" businesses of the business services sector (office cleaning, security, IT services, etc.). The number of such businesses that do operate locally, but may easily move somewhere else when the tax breaks end, increased by 300% after ten years thanks to the EZ program.

Besides, we also observe that the long-term assessment of the French EZ initiative differs noticeably from the short-term one. The apparent stabilization in employment and businesses numbers, which is as expected (as one cannot expect an endless increase in businesses locating in – by definition – delimited areas) was achieved through a high level of business activity, with a steady rate of business creations but compensated by a high rate of business closures. This challenges the intuition that an EZ can induce a change in the economic spatial equilibrium by creating a "virtuous circle." The fact of the matter is the EZ initiative has been prolonged repeatedly, although it was originally planned to be temporary. The rise in jobs located in the EZs thanks to the policy should be fueled by constant fundings.

These conclusions call for several comments. First, a precise analysis of the design of the first wave of the EZ program helps to understand what appears to make this program successful at least in the short run, compared with similar place-based programs. Indeed, place-based policies may propose a wide range of services, tax rebates and subsidies on certain inputs. Recent papers emphasize strong disparities linked to the variety of tax cuts (Lynch and Zax, 2011), the services provided (Bondonio and Greenbaum, 2007), the manner in which the zone is managed (Neumark and Kolko, 2010), or the industrial sector in which business operates (Hanson and Rohlin, 2011; Burnes et al., 2011). Briant et al. (2015) highlight the importance of geographic context in the success of the second wave of the EZ program.

In this regard, the comparison of the impact of the two successive waves of the French EZ program is particularly insightful. In the short run, the results of the first wave evaluated in this paper are strikingly larger than those obtained for second wave of the same EZ program by Givord et al. (2013) using similar data and an identification strategy similar to that adopted here (see also Mayer et al., 2015). Givord et al. (2013) observe a positive impact of the program but of a much smaller magnitude. According to their estimates, the second wave of the EZ program led to a 5–7% increase in the yearly growth in the number of establishments located in EZs in the first years of the program, with no significant impact on employment. Our results suggest that the corresponding figure is twice as large for the first wave. On average, the first wave of the EZ program led to a 14% increase in the yearly growth in the total number of establishments.

However, while seemingly similar, the first and second waves of the French EZ program have distinctive settings that may explain the discrepancies observed between their respective short-term results. The several adjustments made in payroll tax exemptions are the most notable of these. According to several sources, the labor cost reduction provided by the EZ program appears to be one of its most appealing incentives. At an aggregate level, it accounts for more than half of the financial cost of the EZ program.[29] While cash flow issues are one of major challenges for small businesses, exemption from payroll taxes (which in principle should be paid quarterly) may alleviate the financial burden for these businesses and thus favor employment.

However, since 2002, the EZ program has been made less favorable in this regard (see online Appendix). First, an extensive program of payroll tax cuts has been implemented on a national scale (*loi Fillon*). It reduced the tax gap between EZs and the rest of the territory. Second, adjustments have been made to the EZ program because of suspicions of windfall effects. The alleviation of payroll taxes has been made conditional on the hiring of local workers, with a local employment stipulation ("clause d'emploi local"). This clause had been already in effect between 1997 and 2001, but there is evidence to suggest that it was not strictly enforced. Real or supposed difficulties in hiring adequately skilled workers from among the population of the area may have discouraged new businesses from locating there.[30] Moreover, the reduction in payroll taxes has been reduced by half for *relocated* businesses. Indeed, as shown by our results and consistent with previous evaluations of the French EZ program, a meaningful share of hirings were due to relocations of already existing businesses, not to actual job creation. Subventions do not create genuinely new economic activity. They may have negative externalities on other neighborhoods, which not especially advantaged themselves, that lie close to the EZ, as observed by Givord et al. (2013). Indeed, when considering the first wave of the EZ program, we observe that half of the relocated businesses were previously located in the very same municipality as the EZ to which they choose to relocate. The fact that part of the positive impact of the program was due to relocation is another explanation for the less positive impact of the EZ program observed from 2002. As the number of EZs has nearly doubled, there is greater competition among these EZs to attract businesses that are likely to locate in deprived urban areas.

7.2. Main conclusions

Our results suggest that the stabilization of employment and the number of businesses located in EZs observed since this date has been achieved through continuous financial support. This calls for a discussion of the cost efficiency of the EZ program. According to official reports, the overall cost of the first wave of the EZ program was estimated at 287.7 million euros in 2001.[31] This has to be compared with the economic impact of the program. Regarding employment, back-of-the envelop calculation using our estimates suggests that the EZ program led to 36,400–53,500 more jobs in 2001, meaning a cost per job of between 5,400 and 7,900 euros. However, one should consider the main target of the EZ program, that is, residents and/or low-skilled employment. As less than one-third of the labor force of businesses located in an EZ live there, the cost per local job appears much higher (it can be evaluated at between 18,000 and 26,300 euros). When considering low-skilled employment, a similar back-of-the envelop calculation provides a cost per job of between 19,400 and 31,400 euros.

This should be compared with alternative policies intended to create jobs. Following Rathelot and Sillard (2009), one may compare the cost efficiency of the EZ program with the impact of the main French policy in favor of low-skilled employment, namely, labor tax cuts targeted at low-wage earners. This policy was first implemented in 1993, providing exemption from payroll taxes for wages around the minimum wage (more precisely for earnings between the minimum wage and 1.3 times the minimum wage). It has been extended multiple times since then, with the most considerable extension being in 2002, as noted above. Considerable research has been devoted to the evaluation of this labor cost reduction policy. The consensus view is that it has had a positive impact on employment,[32] with a cost per job evaluated at between 8,000 and 28,000 euros. Unlike the EZ program, the labor cost

[29] See, for instance, the official report http://www.ville.gouv.fr/IMG/pdf/jaune2003_cle07d29f.pdf.

[30] In 2008, according to a qualitative survey in the EZs, companies in these zones reported major difficulties in hiring employees inside the area (and minor difficulties in hiring outside the area), as reported in Givord et al. (2013).

[31] See https://www.senat.fr/rap/a02-405/a02-40513.html. It is not possible to obtain the cost of the first wave of the program after 2002, as official reports, when they exist, combine all waves of the program, and we cannot separate the cost corresponding to the first EZ wave from that of the second or third wave.

[32] For a synthesis, see http://travail-emploi.gouv.fr/IMG/pdf/DE2012-_no169.pdf.

reduction policy directly targets low-skilled employment. These figures are thus to be compared with the 19,400 to 31,400 euros per unskilled job that we obtained from our estimates of the impact of the first EZ program.

Despite a highly positive short-term impact on economic activity, the cost efficiency of the French EZ program is thus questionable over the long term. Our results suggest that large cost reductions could be efficient in attracting small businesses. The high business creation rates in the first years of the program initially reassured policy makers of their confidence in this type of initiative. However, our analysis suggests that it is necessary to look into the "black box" of such a program and to analyze its impact over the long run. The subsidies were initially provided without actual targeting toward local unskilled populations. This generates windfall effects, and the positive impact on local workers was achieved at a high cost. Long-term analysis questions the ability of this program to create self-sustaining activity. Many businesses chose to relocate once they no longer benefited from the subsidies. Although the French EZ program has been regularly renewed since its introduction, several adjustments were recently introduced to avoid these effects. To benefit from the subsidies, businesses should provide evidence that they are engaging in effective economic local activity. The local hiring clause has also been reinforced. Further analysis is required to evaluate whether these new features have helped to improve the efficiency of this program.

Acknowledgments

We thank the participants of the INSEE, Economic Department of Sciences Po and CREST seminars, as well as the French Applied Microeconomics Conference, European Economic Association congress and European Regional Science Association congress. We specifically thank Luc Behaghel, Didier Blanchet, Anthony Briant, Xavier D'Haulfœuille, Laurent Gobillon, Thierry Mayer as well as the Editor, David Neumark and two anonymous referees for useful comments and discussions. Any opinions expressed in this paper are those of the authors and not of any institution.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at 10.1016/j.jue.2017.09.004.

References

Ahn, S.C., Horenstein, A.R., 2013. Eigenvalue ratio test for the number of factors. Econometrica 81 (3), 1203–1227. http://dx.doi.org/10.3982/ECTA8968. https://doi.org/10.3982/ECTA8968.

Bada, O., Liebl, D., 2014. phtt: panel data analysis with heterogeneous time trends in r. J. Stat. Softw. 59 (6), 1–34. http://dx.doi.org/10.18637/jss.v059.i06. Articles http://www.jstatsoft.org/v59/i06/.

Bai, J., 2009. Panel data models with interactive fixed effects. Econometrica 77 (4), 1229–1279. http://dx.doi.org/10.3982/ECTA6135. https://doi.org/10.3982/ECTA6135.

Bai, J., Ng, S., 2002. Determining the number of factors in approximate factor models. Econometrica 70 (1), 191–221. http://dx.doi.org/10.1111/1468-0262.00273. https://doi.org/10.1111/1468-0262.00273.

Bondonio, D., Greenbaum, R.T., 2007. Do local tax incentives affect economic growth? What mean impacts miss in the analysis of enterprise zone policies. Reg. Sci. Urban Econ. 37 (1), 121–136. http://EconPapers.repec.org/RePEc:eee:regeco:v:37:y:2007:i:1:p: 121-136.

Briant, A., Lafourcade, M., Schmutz, B., 2015. Can tax breaks beat geography? Lessons from the French Enterprise Zone experience. Am. Econ. J. 7 (2), 88–124. http://dx.doi.org/10.1257/pol.20120137.

Busso, M., Gregory, J., Kline, P., 2013. Assessing the incidence and efficiency of a prominent place based policy. Am. Econ. Rev. 103 (2), 897–947. http://dx.doi.org/10.1257/aer.103.2.897. http://www.aeaweb.org/articles?id=10.1257/aer.103.2.897.

Butler, S.M., 1989. How to Design Effective Enterprise Zone Legislation. Technical Report 215. The Heritage Foundation.

Burnes, D., Neumark, D., White, M.J., 2011. Fiscal Zoning and Sales Taxes: Do Higher Sales Taxes Lead to More Retailing and Less Manufacturing? National Bureau of Economic Researchhttp://dx.doi.org/10.3386/w16932. Working Paper 16932. http://www.nber.org/papers/w16932.

Duranton, G., Gobillon, L., Overman, H.G., 2011. Assessing the effects of local taxation using microgeographic data. Econ. J. 121 (555), 1017–1046. http://dx.doi.org/10.1111/j.1468-0297.2011.02439.x. https://ideas.repec.org/a/ecj/econjl/v121y2011i555p1017-1046.html.

Elvery, J.A., 2009. The impact of Enterprise Zones on resident employment. Econ. Dev. Q. 23 (1), 44–59. https://doi.org/10.1177/0891242408326994.

Freedman, M., 2013. Targeted business incentives and local labor markets. J. Hum. Resour. 48 (2), 311–344. http://dx.doi.org/10.3368/jhr.48.2.311.

Givord, P., Rathelot, R., Sillard, P., 2013. Place-based tax exemptions and displacement effects: an evaluation of the Zones Franches Urbaines program. Reg. Sci. Urban Econ. 43 (1), 151–163. http://dx.doi.org/10.1016/j.regsciurbeco.20. https://doi.org/10.1016/j.regsciurbeco.2012.06.006.

Gobillon, L., Magnac, T., 2016. Regional policy evaluation: interactive fixed effects and synthetic controls. Rev. Econ. Stat. 98 (3), 535–551. http://dx.doi.org/10.1162/REST_a_00537. https://doi.org/10.1162/REST_a_00537.

Gobillon, L., Magnac, T., Selod, H., 2012. Do unemployed workers benefit from enterprise zones? The French experience. J. Public Econ. 96 (9–10), 881–892. http://dx.doi.org/10.1016/j.jpubeco.2012.06. https://ideas.repec.org/a/eee/pubeco/v96y2012i9p881-892.html.

Ham, J.C., Imrohoroglu, A., Swenson, C., Song, H., 2011. Government programs can improve local labor markets: evidence from State Enterprise Zones, Federal Empowerment Zones and Federal Enterprise Community. J. Public Econ. 95 (7), 779–797. http://dx.doi.org/10.1016/j.jpubeco.2010.11. https://doi.org/10.1016/j.jpubeco.2010.11.027.

Hanson, A., Rohlin, S., 2011. The effect of location-based tax incentives on establishment location and employment across industry sectors. Public Finance Rev. 39 (2), 195–225. https://doi.org/10.1177/1091142110389602.

Imbens, G.W., Wooldridge, J.M., 2009. Recent developments in the econometrics of program evaluation. J. Econ. Lit. 47 (1), 5–86. http://dx.doi.org/10.1257/jel.47.1.5. http://www.aeaweb.org/articles?id=10.1257/jel.47.1.5.

Kline, P., Moretti, E., 2014. Local economic development, agglomeration economies, and the big push: 100 years of evidence from the tennessee valley authority. Q. J. Econ. 129 (1), 275–331.

Lynch, D., Zax, J.S., 2011. Incidence and substitution in Enterprise Zone Programs: the case of Colorado. Public Finance Rev. 39 (2), 226–255. https://doi.org/10.1177/1091142110386210.

Mayer, T., Mayneris, F., Py, L., 2015. The impact of urban Enterprise Zones on establishments' location decisions: evidence from French ZFUs. J. Econ. Geography.

Neumark, D., Grijalva, D., 2017. The employment effects of state hiring credits. ILR Rev. 70 (5), 1111–1145. http://dx.doi.org/10.1177/0019793916683930. https://doi.org/10.1177/0019793916683930.

Neumark, D., Kolko, J., 2010. Do enterprise zones create jobs? Evidence from California's enterprise zone program. J. Urban Econ. 68 (1), 1–19. https://doi.org/10.1016/j.jue.2010.01.002.

Neumark, D., Simpson, H., 2014. Place-based policies. NBER Working Papers 20049. National Bureau of Economic Research, Inc. http://EconPapers.repec.org/RePEc:nbr:nberwo:20049.

Neumark, D., Wall, B., Zhang, J., 2011. Do small businesses create more jobs? New evidence for the United States from the national establishment time series. Rev. Econ. Stat. 93 (1), 16–29.

Papke, L.E., 1993. What do we know about Enterprise Zones ? Tax Policy and the Economy, Volume 7. National Bureau of Economic Research, Inc, pp. 37–72. http://dx.doi.org/10.3386/w4251. http://www.nber.org/papers/w4251. NBER Chapters.

Peters, A.H., Fisher, P.S., 2002. State Enterprise Zone Programs: Have They Worked? W.E. Upjohn Institute for Employment Research., Kalamazoo, MI. http://dx.doi.org/10.17848/9781417524433. https://doi.org/10.17848/9781417524433.

Rathelot, R., Sillard, P., 2008. The importance of local corporate taxes in business location decisions: evidence from French micro data. Econ. J. 118 (527), 499–514. http://dx.doi.org/10.1111/j.1468-0297.2007.02131.x. https://doi.org/10.1111/j.1468-0297.2007.02131.x.

Rathelot, R., Sillard, P., 2009. Zones Franches Urbaines : quels effets sur l'emploi salarié et les créations d'établissements ? Économie Statistique 415–416 (1), 81–96. http://dx.doi.org/10.3406/estat.2008.7021.

Reynolds, C.L., Rohlin, S.M., 2015. The effects of location-based tax policies on the distribution of household income: evidence from the Federal Empowerment Zone Program. J. Urban Econ. 88 (C), 1–15. http://dx.doi.org/10.1016/j.jue.2015.04.003. https://doi.org/10.1016/j.jue.2015.04.003.

Rosenbaum, P.R., Rubin, D.B., 1983. Assessing sensitivity to an unobserved binary covariate in an observational study with binary outcome. J. R. Stat. Soc. Ser. B 45 (2), 212–218.

Rosenbaum, P.R., Rubin, D.B., 1983. The central role of the propensity score in observational studies for causal effects. Biometrika 70, 41–55.