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A Long-Term Evaluation of the First Generation of the French Urban Enterprise Zones

Une évaluation de long terme de la première génération des Zones Franches Urbaines

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RÉPUBLIQUE FRANÇAISE

MINISTÈRE DU TRAVAIL, DE L'EMPLOI ET DE LA SANTÉ

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Résumé

Cette étude propose de nouvelles estimations empiriques de l'efficacité des incitations fiscales ciblées pour revitaliser des territoires désavantagés. Nous nous concentrons sur la première génération des ZFU, mise en place en 1997 en France métropolitaine. Nous utilisons un panel de données géolocalisées au niveau entreprise sur une période de 12 ans. Le mode de désignation des zones suggère deux stratégies empiriques : une méthode de différences de différences avec une classification sur le score de propension et des régressions sur discontinuités. Les deux méthodes donnent des résultats proches. Nous mettons en évidence un fort effet positif de la politique de Zones Franches sur l'emploi et les implantations d'entreprises pendant les premières années du dispositif.

D'après nos estimations, la politique de Zones Franches Urbaines a permis de doubler le nombre de firmes situées dans ces zones (par rapport au niveau qui aurait été atteint sans abattements fiscaux). Cependant, cette évaluation positive doit être nuancée, car elle semble surtout due à des entreprises qui ont peu d'impact sur l'emploi local ou l'activité économique locale. De plus, après quelques années, ces premiers résultats positifs sont atténués, l'augmentation des créations d'entreprises étant contrebalancée par des disparitions plus fréquentes.

En ce qui concerne l'emploi, nous trouvons que l'augmentation de l'emploi salarié résulte du transfert d'entreprises déjà existantes dans les ZFU. Par ailleurs, les incitations fiscales n'ont pas conduit les entreprises déjà implantées dans ces zones à créer de nouveaux emplois.

Mots clés: Zones Franches Urbaines, Emploi Local, Matching sur le score de propension, Évaluation.
JEL: C23, H71, R5

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A Long-Term Evaluation of the First Generation of the French Urban Enterprise Zones

Abstract

This paper provides new empirical assessment on the efficiency of locally-targeted tax incentives in revitalizing distressed areas. We focus on the first generation of the French "Enterprise Zone" intiative, implemented in 1997 in continental France. We use new georeferenced panel data at the firm level over a twelve-year period. The zone designation process suggests two empirical strategies: difference-in-difference regressions with subclassification on the propensity score and regression discontinuity design. Both methods yield similar results. We highlight a strong positive impact of the Entreprise Zone policy on employment and business location during the first years of the policy.

According to our estimates, the French EZ initiative caused the number of firms located in the designated zones to double (compared to the baseline level that would have been achieved without tax rebates). However, this favourable assessment has to be moderated because it seems mostly due to firms that are less prone to stimulate either local employment or economic activities. In addition, after some years, the early positive results are reduced as the increase in business locations is partially offset by more frequent business discontinuations.

As far as employment is concerned, we find that jobs were created by the relocation of firms to the designated EZ areas. However, the initiative did not induce firms already operating in these areas to create new jobs.

Keywords: Enterprise Zones, Local Employment, Propensity Score Matching, Evaluation.
JEL: C23, H71, R5

1 Introduction

This paper provides an evaluation of the long-run efficiency of Enterprise Zones (hereafter "EZs") in promoting economic activity and employment. This program provides generous tax incentives to small firms located in designated areas plagued by social and economic difficulties. Focusing on the first wave of EZ in continental France, created in 1997, we provide evidence that those locally targeted tax incentives have encouraged the location of small firms in deprived urban areas. According to our estimates, the French EZ initiative caused the number of firms located in the designated zones to double (compared to the baseline level that would have been achieved without tax rebates). This impact is partially offset in the long run as most of these firms subsequently disappear following the eventual expiry of the tax cuts. As far as employment is concerned, we find that jobs were created by the relocation of firms to the designated EZ areas. However, the initiative did not induce firms already operating in these areas to create new jobs.

Our findings are derived from panel data of French firms from 1995 to 2007. As EZ borders do not correspond to existing administrative boundaries, we use GIS (geographic information system) to accurately locate companies within urban areas. Our identification strategy rests on the assignment mechanism employed by the French government: the 44 designated EZs were selected according to a deprivation index based on various socio-economic criteria such as the concentration of social housing and the unemployment rate. It should also be noted that, in order to qualify as an EZ, an urban area must have a population of over 10,000. For the purposes of this study, deprived urban areas which were not selected as EZs are used as a control group. We employ two different methods: the first (and most reliable) method is based on a combination of regression and subclassification on the propensity score, while the second method is based on regression on discontinuity and uses eligibility requirements as an alternative source of identification. Both methods yield similar results.

The first "Urban Enterprise Zones" were implemented in the 1980s in the UK, followed by several US states, in the aim of enhancing the business environment in certain neighborhoods suffering from socio-economic difficulties. More concretely, precisely delineated zones were granted "special dispensation" status, and firms who chose to locate and invest in these zones benefited from temporary incentives such as tax rebates, job-trainings or relaxed regulatory barriers.

The rationale behind this policy is to compensate for the numerous disadvantages associated with socio-economically deprived areas, such as the shortage of a skilled labor force (i.e., spatial mismatch), a lack of public services and amenities such as security, a dearth of inputs and poor market potential. In a word, the EZ designation is intended to offset the deficiencies inherent to low-income neighborhoods which reduce the productivity of firms. The effectiveness of this compensatory policy is disputed amongst economists.[1] Previous empirical evaluations of EZ policies yield mixed results (Fisher and Peters 2002): although most evaluations find no significant increase in employment, a notable few do (O'Keefe 2004, Papke 1994, Busso and Kline 2008). Existing economic theory provides few tools for determining the optimal tax scheme for improving the economic performance of deprived areas. EZs usually propose a wide range of services and subsidies on certain inputs, so the effectiveness of an EZ policy depends on the relative importance of each of these inputs in the firm's production process. The optimal EZ schema is therefore difficult to determine, as it depends on the needs of diverse firms: for instance, a labor-intensive firm would be more responsive to a labor tax reduction than would a capital-intensive one. Recent papers emphasize strong discrepancies in the effectiveness of Enterprise Zones: these discrepancies may depend on the variety of tax cuts and services provided (Bondonio and Greenbaum 2007), the manner in which the zone is managed (Kolko and Neumark 2010), or even on the age of the firm (Bondonio and Greenbaum 2007) or the sector of activity in which the firm is involved (Hanson and Rohlin 2011). Similarly, the EZ's impact on local employment, which is usually regarded as a key indicator, also varies with the tax cuts proposed (Lynch and Zax 2008). In other words, the existing literature does not offer any general conclusions on the optimal scheme.

Compared to EZs in the US and the UK, French Enterprise Zones offer generous tax subsidies, but these subsidies are limited to small firms with less than fifty employees. To our knowledge, the first generation of French EZs has not yet been evaluated on a national scale.[2] This type of evaluation is pertinent - and, indeed, necessary - because it enables the evaluation of the long-term effects of the policy. Our evaluation is made possible by the availability of accurate geo-referenced data, which has only recently been made available. Using these data, Rathelot and Sillard (2008) and Givord, Rathelot, and Sillard (2011) analyze the impact of the second generation of French EZs which were implemented in 2004. They highlight a positive but small impact on employment and business creation. Using similar data and methodology, we examine the first generation of French EZs, and find a much wider effect. The discrepancy between the effectiveness of the first and second generations of EZs can be partly explained by an temporary change in the national tax system. In the early 2000s, generous payroll tax deductions have been introduced at national scale, rendering the tax reductions available in EZs less attractive for firms[3]. Our results are consistent with previous evaluations, as we notice that

[1]But it is still a tool widely used by policy makers. The UK government recently announced the creation of 21 new Enterprise Zones in 2012. Some new geographically targeted incentive program called "Growth Zones" could be created in the US in replacement of previous federal Empowerment Zones program, created under the Empowerment Zones and Enterprise Communities Act of 1993. In France, the "Zones Franches Urbaines", firstly created in 1997, were expanded and prolonged several times: scheduled to end December 31st 2011, they were extended once again.

[2]Since 2002, an annual administrative report presenting monitoring data for French EZs has been published but it does not provide an evaluation of the causal impact of the EZs.

[3]Tax reliefs on low wage were implemented in France from 1993 and have been gradually increased since that time. More precesily, in 1996, French Government grants a 18.2 percent reduction for workers paid at the minimum wage, which decrease gradually and vanished at 1.33 times the minimum wage.

the effect of EZ on business creation fades in the early 2000s, which corresponds to the implementation of both the payroll tax at a national scale and the second phase of the EZ initiative. Besides, the effect varies widely by sector of activity. The positive effect seems mostly due to firms which do not foster neither local employment nor economic activity, because they belong to sector of activity that do not need to operate in-situ or hire local workers.

These results are in line with Gobillon, Magnac, and Selod (2010). In a study on EZ in Paris region, they find no impact of first phase of the initiative on local unemployment rates in Paris metropolitan region. They detect only a small and non-persistent effect on the unemployment duration for local workers. Overall, these results suggest that firms are sensitive to tax cuts, however, they call into question the capacity of EZs to improve the local economy.

2 The French Enterprise Zones

Urban decay has become a main issue in the French public debate since the 1980s. Some disadvantaged neighborhoods on the outskirts of cities experience problems of failed integration of immigrants, high unemployment and violence. Many policies have been implemented in response to social and economic problems encountered by deprived outskirts of French cities. Indeed, the so-called "social fracture" ("fracture sociale") was an important theme of the 1995 presidential campaign and the social and economic issues of deprived urban areas were identified as the main causes of this "social fracture". The "stimulus for cities" law ("Pacte de relance de la ville"), passed in 1996 by the newly elected Government, aimed at addressing the issue of urban decay and reducing 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-tier classification scheme for disadvantaged urban areas. The first tier is known as ZUS ("Deprived Urban Area"). They correspond to the 757 most deprived areas in France,[4] according to various indicators of socio-economic development (in particular, high concentration of social housing and high unemployment rates). The second tier, the ZRU ("Urban Renewal Area"), contains the most disadvantaged ZUSs according to a global rating on social and economic position. This rating takes into account the unemployment rate, the population size, the proportion of unskilled people, the proportion of young people and the potential tax

The last significant tax reduction has been gradually introduced from 2003. Companies benefit from a 26 percent tax cut for workers paid at the minimum wage which reduces until it reaches 1.6 times the minimum wage.

[4] 717 in continental France and 40 ZUS in overseas departments. 4.73 millions of people lived in ZUS according to 1990 census data.

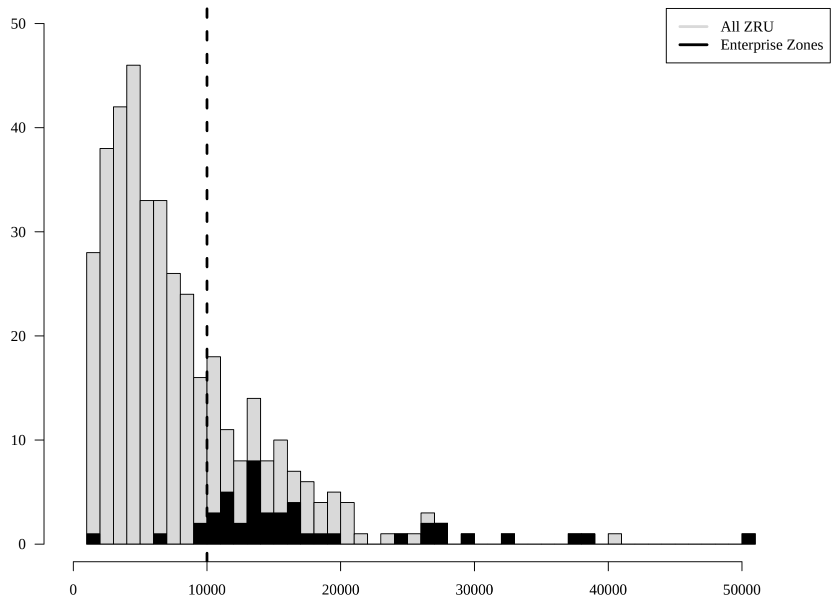

Figure 1: Distribution of EZ and ZRU according to the number of inhabitants.

revenue of the town:[5] it corresponds to the product of the four first indicators divided by the fifth one. 436 ZRUs were designated in 1996.[6] Finally, the third tier is constituted by ZFU ("Urban Enterprise Zone"), hereafter EZ. These zones are chosen upon a two-stage process: only the most populous ZRUs are eligible, the official threshold being 10,000 inhabitants; amongst these areas, the most deprived ZRUs, as defined by the same global rating, are designed as Enterprise Zones. In 1997, during the first phase of this initiative, 44 areas received the EZ designation, followed by an additional 41 in 2004 and 15 more in 2007.

Figure 1 shows the distribution of the size (here the population) of ZRUs and EZs. EZs are populated by more than 10,000 inhabitants, in accordance with the threshold set by law, with the exception of four areas (two very small zones that were merged to bigger Enterprise Zones, and two areas just below the threshold with respectively 9,538 and 9,927 inhabitants). Table 1 shows that EZs are the most disadvantaged areas according to the socio-economic indicators that are components of the global rating previously mentioned. For each variable, EZs get the worst results, especially compared

[5] Potential tax revenue of the town corresponds to the product of the tax base by the medium tax rate

[6] 416 in continental France and 20 in overseas departments in 1996.

Table 1: 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 (less than 25)	46.7	43.2	45.5	41.2
Average potential tax revenue (in euros)	2,707	3,212	2,609	3,438

to the ZRUs populated by more than 10,000 people and which were not designated as Enterprise Zones. However, smaller ZRUs (under the 10,000 inhabitants threshold) are closer to EZs in terms of socio-economic indicators. The average unemployment rate is 22% in EZs while it is "only" 18% in big ZRUs but 24% in small ZRUs. The proportion of unskilled people is 43% in EZs while it is 36% (respectively 46%) in big (respectively small) ZRUs. The ZUSs show slightly better performance but remain close to big ZRUs.

Enterprise Zones offer remarkably generous incentives (high tax cuts on property taxes, as well as labor and business taxes). They target only small firms (establishment with less than 50 employees, with an additional condition on the sales), whether located in the area prior the introduction of EZ policy or not. Full exemption is granted for five years and decreases progressively over the following years (see the Appendix A for details). In comparison with the tax relief in EZ, ZRU and ZUS provide much less profitable tax benefits. The ZRU designation provides limited tax rebates, only for newly located businesses over a very short period (one or two years after setting up depending on the tax). Payroll tax exemption applies to wages paid to all employees in EZs, while it only applies to wages paid to newly hired employees in ZRU. Finally, the ZUS designation allows local authorities to exempt firms of local taxes, but this tax break is not mandatory.

This three-tier classification scheme has been alternatively reinforced and weakened under subsequent governments. The first generation EZs were implemented in 1997 by a right-wing government, scheduled for five year. In 1999-2000, the left-wing government partially reduced the tax breaks. As initially planned, the policy ended in 2001: business had to locate in a Enterprise Zone before December 31 to benefit from the tax rebates. However, the newly elected conservative government has reactivated EZ in 2003, and has designated 41 new areas in 2004.

All in all, the attractiveness of the EZs depends on the structure of the initial tax burden on each company. In order to roughly evaluate the effective gain for a firm, we calculate taxes owed by a company for both normal and EZ rates. To do so, we use two administrative fiscal databases, the BRN-RSI and the DADS that are used by the French administration to calculate respectively the business taxes and the payroll taxes (social security contributions). The simulations are computed for all French companies with less than fifty employees. It assures us that our sample only contents EZ eligible companies. We use the year 1995, to avoid potential changes in the financial structure or wage distribution due to the implementation of EZ. As for business tax, eligible compagnies benefit from a full exemption below a certain amount. Note that most small firms do not pay any business tax, for instance because their yearly sales are too low. Concerning the payroll tax exemptions, firm's tax returns provide gross wages of each employee. We can apply both normal and EZ payroll tax rates for each worker. By aggregating the results, we can estimate how much a firm gains by locating in EZ.

Our simulations also take into account the fact that the employer payroll rates (social security contribution) for the lowest wages have evolved over the period (see the Appendix A). For a worker earning the minimum wage, EZ payroll tax is 10 percentage points lower that standard payroll tax (national level rates) before 2003 while it is close to zero at the end of 2007. EZ tax exemption applies to all wages (the tax reduction is applicable to the share of the wage below 1.5 minimum wage) while national exemption only applies to low wages (below 1.6 minimum wage in 2003, only 1.3 or 1.33 before). The effective gain for a company depends on its wage distribution. We simulate the tax gain (for all small companies) using the 1997 tax scheme and the 2005 tax schemes.

The exemptions of payroll taxes account for the largest share of tax reductions. The median tax cuts in payroll taxes associated with EZ is 6,000 euros and accounts for about 15% of the median wage bill (see Table 3), using the 1997 tax scheme. By contrast, more than three quarter of small companies did not pay any business tax in 1995 and logically they do not benefit from any business tax cut (see Table 2). For those paying a strictly positive amount, the median is 3,700 euros. These amounts strongly vary according to the sectors however: for instance the proportion of firms in the sector of business services that do not declare any business tax is 60%, while the rate is 85% for the building sector. Considering only firms which pay business tax, the median amount varies from 2,000 euros to 8,000 euros. The median payroll tax cut associated with EZ varies from 3,000 euros to 22,000 euros amongst sectors.

When applying for the sake of simulation the scheme of payroll taxes used since 2005, the median gain in payroll taxes for firms to be located in EZ amounts to 4,500 euros. Again, we note strong variations with sectors: from 1,700 euros (sector of household services) to 18,600 euros (car industry).

Table 2: Summary statistics on business taxes (1995)

	Share of non-taxed firms	Median of business tax (for taxed firms – thousands of euros)
Total	76.2	3.7
Food industry	84.9	3.8
Final good ind.	68.5	3.5
Car industry	52.0	6.1
Capital good ind.	62.5	5.5
Intermediate good ind.	55.1	7.2
Building	83.7	3.2
Energy	58.9	7.5
Retail	73.9	3.8
Transportation	79.5	4.1
Finance	51.7	8.2
Real estate	83.0	2.6
Business services	60.5	3.8
Household serv.	85.4	2.0
Health, educ.	57.0	3.8

Sources : Fiscal database (BRN-RSI).

Reading note: for all French companies present in 1995, we estimate the proportion that did not pay any business tax and estimate the median business tax for those having a strict positive business tax.

Table 3: Simulation of labor cost in French small companies and EZ payroll tax cuts, 1997 and 2005 tax schemes. (thousands of euros)

	1997 payroll tax scheme		2005 payroll tax scheme	
	Labor cost (normal rate)	EZ payroll tax cuts	Labor cost	EZ payroll tax cuts
Total	40.4	5.9	38.3	4.5
Food industry	37.7	6	35.1	3.8
Final good ind.	68.2	9.5	65.7	7.7
Car industry	148.5	22.1	145.8	18.6
Capital good ind.	111.8	15.8	109.2	13.5
Intermediate good ind.	136.0	19.2	131.8	15.5
Building	44.6	7.3	42.3	4.9
Energy	63.6	9.1	63.1	8.5
Retail	44.6	6.6	42.5	4.7
Transportation	70.5	10.6	67.8	8.4
Finance	42.4	5.8	41.3	4.7
Real estate	19.4	2.9	18.1	1.8
Business services	60.0	7.7	58.5	6
Household serv.	23.1	3.6	21.1	1.7
Health, educ.	23.5	3.8	21.9	1.8

Sources : Administrative employer-employee database on wages (DADS), year 1995, restriction to small companies eligible (less than 50 employees).

Reading note: Using 1997 tax schemes (respectively 2005 tax scheme), the estimated median labor cost in French small companies is 40.4 thousands of euros (resp. 38.3). The estimated median payroll tax cuts for being in EZ is 5.9 thousands of euros (resp. 4.5).

3 Descriptive statistics

We merge two administrative datasets to obtain rich information on firm demography as well as employment in urban areas on a small scale: the employment level (broken down by skill level), the firm creations and firm stock (broken down by sector of activity). We can also determine whether a new firm location is an actual creation or a relocation. Finally, since our database is not reliable for the measure of discontinuance of business, we propose an alternative method. More precisely, we only focus on firm which disappear definitively of employment data to estimate a business closure rate.

French business register (SIRENE) displays the location, legal status, sector of activity and year of creation for all firms. The DADS (an administrative exhaustive employer-employee database on wages) provides data on the company's workforce (mainly employment, wages and worker skills). Census data allow us to measure socio-demographic variables used for the designation of an area as an Enterprise Zone. Firm stock and employment data are available from 1995 to 2007 (at January 1st of each year), so this information is available 3 years before the introduction of the EZ tax rebates and 10 years after. Business creation and relocation are also available from 1995 to 2007. Note that business creation and relocation data are measured over a one-year period, while data on the number of firms and level of employment record a situation at a given date. In other words, the first data are flow data while the latter are stock data. This should be carefully taken into account when interpreting our results. For example take the year 1997: stock data should show no effect as they describe the situation on january 1st 1997 while creation and relocation data could reveal a impact of EZ as they correspond to a record for the whole year 1997. As a consequence, creation and relocation data are consequently available 2 years before the beginning of the policy and 9 years after. These data allow us to identify long-run effets of Enterprise Zones, as well as temporal delays, mitigates or extenuations.

More important, data from INSEE locate precisely all companies in continental France,[7] which is crucial as EZ do not correspond to administrative boundaries. Using data at the ZIP code[8] would have led to underestimate the impact of the tax rebates, because companies which benefit from EZ tax rebates would have been mixed up with companies which do not. For this evaluation, the data have been aggregated on the exact EZ (for more details, see Appendix B). These data have been made available recently and thus made this study possible (for a previous use on the second generation French EZs see Givord, Rathelot, and Sillard 2011).

[7]Location of establishments is available by census block.

[8]Corresponding to the French *code commune*

Table 4: Average economic outcomes in different types of urban areas in 1995

Average number per area	EZ	ZRU > 10,000 pop	ZRU < 10,000 pop	ZUS
Firm creation	34.2	31.5	8.4	18.0
Firm relocation	7.6	6.8	2.0	5.1
Existing firm	237.4	222.3	60.0	142.3
Employment	708.2	533.2	175.3	350.0

Sources: Administrative employer-employee database on wages (DADS) and SIRENE

Reading note: in 1995, on average 34.2 pure business creations are observed in (future) EZ, 31.5 in ZRU with more than 10,000 inhabitants, only 8.4 in smaller ZRU and 18 in others ZUS.

The number of new and existing firms is higher in Enterprise Zones than in small ZRUs (less than 10,000 inhabitants) and ZUSs (see Table 4). Conversely, these levels are similar in big ZRU (more than 10,000 inhabitants). This is mostly due to a size effect as EZ are specifically designated amongst big ZRUs. We address this potential issue by using the logarithmic differentiation of interest variables.

4 Identification issue and empirical strategy

4.1 Identification issue

The empirical evaluation of the Enterprise Zones has to deal with the usual problem of selection. The Enterprise Zones are often located in blighted and economically depressed areas. Conversely, some areas designated as Enterprise Zones may be ripe for development; firms would have located within such areas regardless of tax incentives. The pervasive challenge for evaluation is to assess what would have happened if not for the Enterprise Zone.

Our argumentation aims to determine how selected areas would have performed in the absence of tax rebates provided by this policy. As it became standard in the ex post evaluation literature, we adopt as a baseline guideline the framework of potential outcomes known as the Roy-Rubin model. This model postulates the existence of two potential outcomes, usually denoted by Y_{i0} and Y_{i1}: the former corresponds to the outcome (i.e. the (log) number of firms or employment level) observed in the absence of the Enterprise Zone system while the latter corresponds to the outcome we do observe with the EZ system. Our main parameter of interest is the average impact of the EZ system on Enterprise Zones (i.e. Average Treatment on the Treated). It corresponds to the average difference between the potential outcome under the EZ system and the potential outcome without the EZ system, as applied to the unit benefiting from the system (denoted by the dummy T):

$$\delta = E(Y_{i1} - Y_{i0} | T_i = 1)$$

The so-called "fundamental problem of causal inference" relies on the fact that we do not observe Y_{i0}. We thus have to estimate this counterfactual outcome by using other geographical units that do not benefit from the Enterprise Zone system. More specifically, we restrict the comparison set of non-beneficiary areas (i.e. the control group) to ZRUs, which are most similar areas to EZs according to the French three-tier classification scheme for disadvantaged urban areas. However, direct comparison of all treated and all non-treated areas could lead to spurious results. As stated below, Enterprise Zones policy actually targets ZRUs stricken by multiple economic handicaps which can cripple local economic vitality. Thus, a direct comparison might wrongly attribute the effect of socio-economic differences between ZRUs and Enterprise Zones to the EZ system.

4.2 Empirical strategy: propensity score matching

To address this issue, our empirical strategy is based first on the use of panel data prior and after the introduction of the Enterprise Zones program, and second on an adaptation of the standard propensity score method using areas that do not benefit from the EZ initiative but are similar in terms of socio-economics characteristics.

Panel data let us eliminate the potential fixed-effect specific to each area. More precisely, our main variables of interest are (log) outcome level differentiated with its level in 1995, meaning two years prior to the introduction of the tax exemptions. Let us denote by $\Delta_{1995} Y_{it} = log(Y_{it}) - log(Y_{i1995})$. In order to estimate the causal impact of the EZ program we want to compare this evolution to the counterfactual evolution $\Delta^0_{1995} Y_{it}$.

This counterfactual evolution is estimated by a propensity score method. It relies on the designation mechanism used for the French Enterprise Zones, which we actually know. As stated before, Enterprise Zones are chosen amongst the most deprived areas, according to socio-economic indicators (see section 2). Besides, (almost) only areas populated with more than 10,000 inhabitants are eligible for the EZ initiative. This latter condition assures that comparable zones (in terms of socio-economic development) could be found amongst areas under the threshold of 10,000 inhabitants, provided that the size is not a determinant of the outcome. As we work with the time-differentiated log variable, this restriction appears credible and could be informally tested. The former condition let us choose the most comparable areas, as we could estimate those having the most comparable characteristics. More specifically, our main assumption is the classic "Conditional Independence Assumption" (or unconfoundedness assumption) that states that no difference would have been observed in zones with comparable observable characteristics in the absence of the treatment:

Assumption 4.1 *(Conditional Independence)* $\Delta^0_{1995} Y_{it} \perp T_i | X_i$

As shown by Rosenbaum and Rubin (1983), if the conditional independence assumption holds for observables X, it also does for the propensity score (i.e. the probability for an area of being designated as an Enterprise Zone conditional on observables).[9] We thus have:

$$E(\Delta^0_{1995}Y_{it}|T_i = 1, p(T_i = 1|X_i)) = E(\Delta^0_{1995}Y_{it}|T_i = 0, p(T_i = 1|X_i))$$

Conditioning variables are the indicators used for the designation of Enterprise Zones: unemployment rate, percentage of young people, of foreigners, of executives and of unskilled people in the area, the rate of social housing of the area and the potential tax revenue of the town in 1996.

The eligibility condition reinforces the credibility of our identifying assumption, but it can impact the estimation of the propensity score. It indeed leads to a censoring for the observed status of the treatment. If the distribution of the propensity score is not exactly the same for the smallest areas, a naive estimate of the propensity score will be biased. We thus propose an estimation that take this specific setting into account. Details and estimates are provided in the Appendix 6.

Figure 2 shows the density of the propensity score on both treated and control group. The treated group contains 45 areas and the control group contains 331 areas.

For the final estimate of the impact of the EZ, we follow Imbens and Wooldridge (2008) and adopt a strategy that combines regression and propensity score methods. Despite the fact that our sample size is small (around 400 areas), this method can achieve some robustness to potential misspecification of the parametric specification for the propensity score. More precisely, we use a sub-classification method based on the propensity score: we define four strata corresponding to the level of the propensity score. Using units in each stratum, we perform a linear regression using observable covariates X. Within each stratum, the propensity score varies little and covariates distributions are on average similar between both groups, meaning that the regression function is not used to extrapolate far "out of sample". The estimate of average treatment on the treated finally corresponds to the weighted average of these local estimates.

Formally and using notation of Imbens and Wooldridge (2008), we perform the linear regression in each stratum j:

$$\Delta_{1995}Y_{it} = X_i\beta_j + \delta_j T_i + u_{ij} \tag{1}$$

The final estimate of the impact of the tax subsidies on the EZ (ATT) δ_{ATT} corresponds to:

[9] Or more generally for any balanced function of the covariates: even if we do not observe the real score, CIA could hold for an estimation of this score, provided that this estimated score balances the observables between the group of Enterprise Zones and the control group: formally $T \perp X|\hat{p}(X)$.

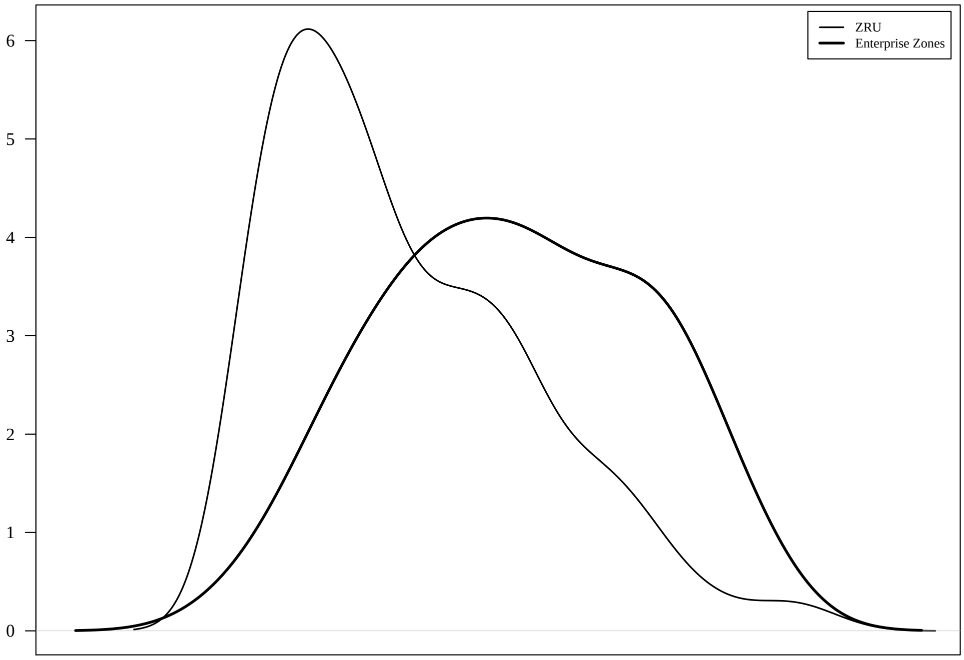

Figure 2: Score density for the treated and control groups

$$\hat{\delta}_{ATT} = \sum_{j=1}^{J} \frac{N_{jEZ}}{N_{EZ}} \hat{\delta}_j$$

and an estimate for its variance is:

$$\hat{V} = \sum_{j=1}^{J} \left(\frac{N_{jEZ}}{N_{EZ}} \right)^2 \hat{V}_j$$

with $(\hat{V}_j)_{j=1,\ldots,J}$ corresponds to the estimated variances of $(\hat{\delta}_j)_{j=1,\ldots,J}$ (assuming that the aleas for different strata are independently distributed, which is a standard assumption in this kind of method). J is the number of strata (4 in our estimates). We introduce the size of the area as an additional covariate in regression 1, as an informal test of the assumption of conditional independence of outcome and size. It is never significant. This estimation turns out to be our favorite specification, but we have also performed standard kernel propensity score matching (Heckman, Ichimura, and Todd 1998) as a robustness check (results are provided in the Table 13 in the Appendix 6).

An informal test of the assumption 4.1 is provided by the fact that we have data on outcome two years prior the introduction of the EZ program. We will thus estimate the average causal effect of the program on lagged outcomes.

4.3 Regression on discontinuity design

The eligibility threshold also suggests to use an alternative econometric method based on regression on discontinuity design. This method can be seen as follows: the probability of being EZ is a discontinuous function of an underlying continuous variable, the size in terms of number of inhabitants. Indeed, our setting is very similar to that of Battistin and Rettore (2008), where endogenous selection occurred amongst a pool of eligible units. The estimate of the policy impact on the marginal participant (namely, Enterprise Zones with size around the threshold of 10,000 inhabitants) could be provided by classic fuzzy design estimators. This setting has the advantage of relying on a milder assumption (i.e. continuity of potential outcome at the threshold and local independence of the selection around the threshold) than the conditional independence assumption 4.1.

The fuzzy design estimator corresponds to the ratio of the difference in right and left value of respectively the outcome and the probability to be designated as an EZ at the eligibility threshold. As shown by Hahn, Todd, and Van der Klaauw (2001), an estimator of the local impact is given by:

$$\hat{\delta}^{RDD} = \frac{\lim_{S \searrow \underline{S}^+} E(Y|S) - \lim_{S \nearrow \underline{S}^-} E(Y|S)}{\lim_{S \searrow \underline{S}^+} E(T|S) - \lim_{S \nearrow \underline{S}^-} E(T|S)} \tag{2}$$

where S is the selection variable (population size) and $\underline{S}$ the threshold (10,000 inhabitants). Practical estimation can rely on local linear regression, that performs better in this setting (estimation in a single boundary point) than standard non parametric kernel regression (Imbens and Lemieux 2008). In the simplest (and the most popular) case, the fuzzy estimator is numerically identical to two-stage-least-square on the linear regression:

$$\Delta_{1995} Y_{it} = \alpha + \delta T_i + \beta(S_i - \underline{S}) 1_{S_i > \underline{S}} + \gamma(\underline{S} - S_i) 1_{S_i < \underline{S}} + u_i \tag{3}$$

using the indicator $1_{S_i > \underline{S}}$ as an excluded instrument and restricting the estimation sample to units on a neighborhood around the threshold $[\underline{S} - h; \underline{S} + h]$.[10]

However, given the small sample size, the superiority of this method over the method based on propensity score does not appear clearly. The assumption for a valid estimator is local: it means that we should restrict estimation sample to a close vicinity of the eligibility threshold. In practice, as we have few observations, we have to use a wider sample and control for potential dependence of the outcome with the size. As already stated, this dependency appears weak as we use the differentiated log outcome, but it means that we finally rely on a similar assumption with the case of the estimator based on the propensity score.

As suggested by Battistin and Rettore (2008), our particular setting provides a local test for the assumption 4.1 at the threshold of eligibility. If this assumption is valid, estimators obtained by the regression discontinuity design should equate those obtained by propensity score method. We will thus use estimates provided by this method as a robustness check.

5 Results

To make the reading of the results easier, we adopt a convention such as the EZ effects are expected in 1997 for all data. This implies that the definition of the estimators differs slightly depending on the type of data. For business creation and relocation, the estimated impact δ_t for year t corresponds to: $\delta_t = \Delta_{1995} Y_{i,t+1} - \Delta^0_{1995} Y_{i,t+1} = log(Y_{i,t+1}) - log(Y^0_{i,t+1})$. [11] For the employment and the number of firms, the estimated impact δ_t for year t corresponds to: $\delta_t = \Delta_{1995} Y_{i,t} - \Delta^0_{1995} Y_{i,t} = log(Y_{i,t}) - log(Y^0_{i,t})$.[12] In a word, the results on business creation and relocation, which are flow data, correspond to a comparison between year 1995 and year t while the results on the employment and the number of firm, which are stock data, compares January 1st, 1995 to January 1st, t+1.

According to our results, Enterprise Zones have a strong impact on economic activity of targeted areas. Tax rebates result in a steady increase of the number of firms (see Figure 3). In 2001, the estimate of the impact of EZs on time-differentiated log number of companies located in Enterprise Zones is 0.7. It means that the level reached in

[10] That corresponds to uniform kernel and the same bandwidth h for estimation of the four components of 2, see again Imbens and Lemieux (2008).

[11] Thus $Y_{i,t+1} = Y^0_{i,t+1} e^{\delta_t}$ and in level $Y_{i,t+1} - Y^0_{i,t+1} = (1 - e^{-\delta_t}) Y_{i,t+1}$.

[12] Thus $Y_{i,t} = Y^0_{i,t} e^{\delta_t}$ and in level $Y_{i,t} - Y^0_{i,t} = (1 - e^{-\delta_t}) Y_{i,t}$.

2001 is 2 times higher than the level that would have prevailed without the policy. This corresponds to about 1,500 companies. The impact appears to be stabilizing from this year. The estimated impact of EZs on salaried employment is similar. In 1997, which is the first year of the policy, the estimated impact is 0.38. It means that the number of salaried employees in firms in EZs is 1.5 times higher than its counterfactual level. It constantly rises afterward and then stabilizes after 2001. From 2001, the number of salaried employees in EZ firms is more than 2.5 times higher than its counterfactual level, according to our estimates. Surprisingly, this effect strongly exceeds the previous studies that evaluate the second-generation of the French Enterprise Zones (see Rathelot and Sillard 2008, Givord, Rathelot, and Sillard 2011). We discuss this point below.

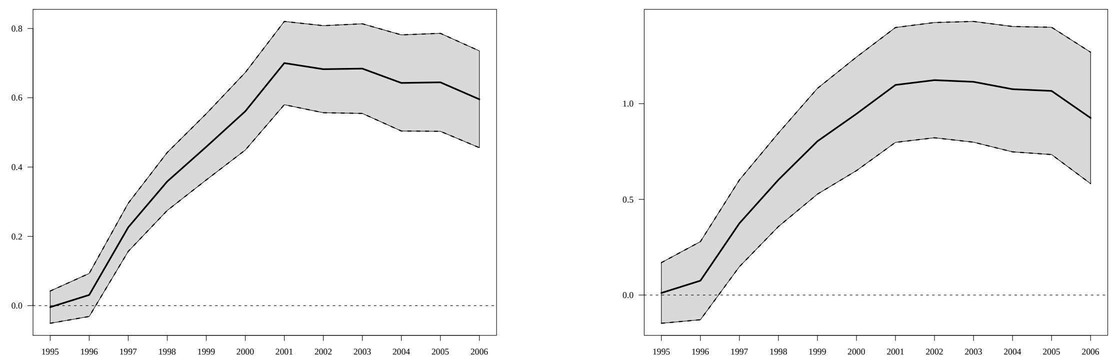

Figure 3: Impact of EZs on the (log) stock of companies and (log) employment.

Two stylized facts emerge from our study that are consistent with those results, however. First, whereas both newly located and existing firms benefit from tax rebates, EZs has no impact on employment in existing companies (see Figure 5). The higher level of employment (compared to the level than would have prevailed in the absence of the EZ policy) appears to be only due to the location of new companies. In other words, the estimated impact is exclusively due to the extensive margin. As already stated by Givord, Rathelot, and Sillard (2011), it calls for a better targeting of the tax exemptions. Second, a large part of this increase is due to business relocation rather than actual creation of new companies. However, our results highlight a larger effect than Givord, Rathelot, and Sillard (2011). The EZ impact increases between 1997 and 2001, which are respectively the first and the fifth years after the policy implementation. This trend is higher for relocations than for company creations. More precisely, the estimated impact for company relocation rises from 0.6 in 1995 to 1.8 in 2001 as it only increases from 0.6 to 1 for company creations (see Figure 4). To put it another way, the number of firm relocation in EZs was 2.5 times higher than the level that would have prevailed without tax rebates, while the number of true business creations was 2 times higher than its counterfactual level.

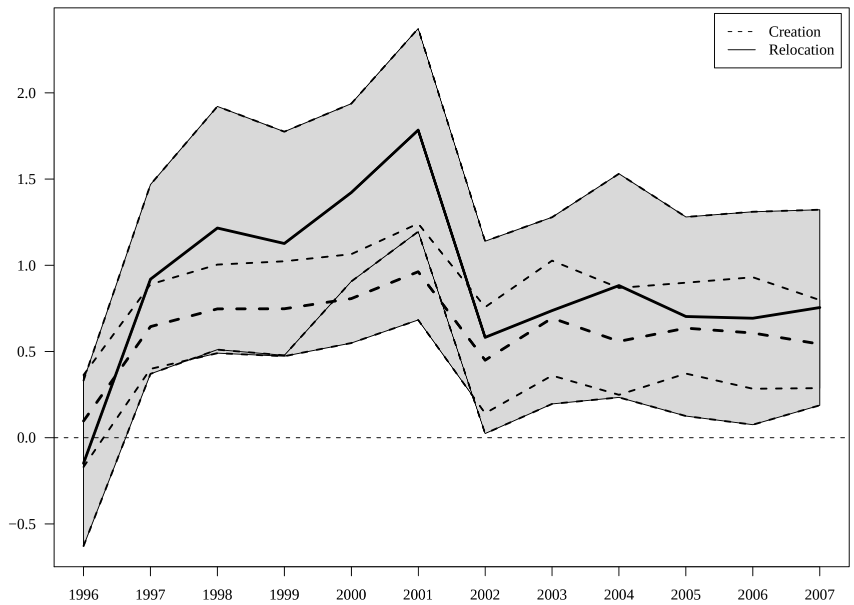

Figure 4: Impact of EZ on the (log) number of company relocations and creations.

The temporal profile of the EZ impact calls for comments. French firms strongly react to the anticipated end of tax exemptions scheduled for 2001. Recall that the policy was first announced to stop after five years. Businesses had to locate in a Enterprise Zone before December 31, 2001 to benefit from tax rebates. The anticipation of the policy end produces an interesting phenomenon. Firm location, and especially relocations, increases sharply in Enterprize Zones in 2001. Estimated impact of EZs on companies relocation and creation are respectively 6 and 2.6. This results in a dramatic drop the year after. The return of the conservative party to power in 2002 led to the reactivation of Enterprise Zones after 2003.

After 2003, the impact of EZs on relocation and creation of compagnies resumes at a slightly smaller level than during the first period of the policy. Conversely, company stock and employment become stable. These patterns can be explained by the fact that business locations are now canceled out by relocations outside the Enterprise Zone and business discontinuations. Between 1997 and 2001, EZs had a notably higher impact on business creation than on shutdown of company with salaried employees. From 2002, these levels are not significantly different (see Figure 6).

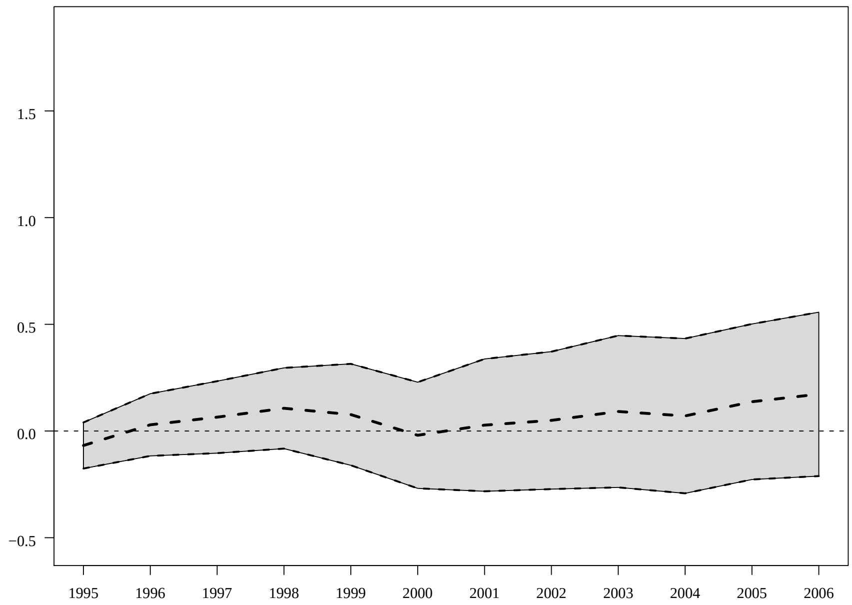

Figure 5: Impact of EZ on (log) employment level in companies which were present before the EZ policy.

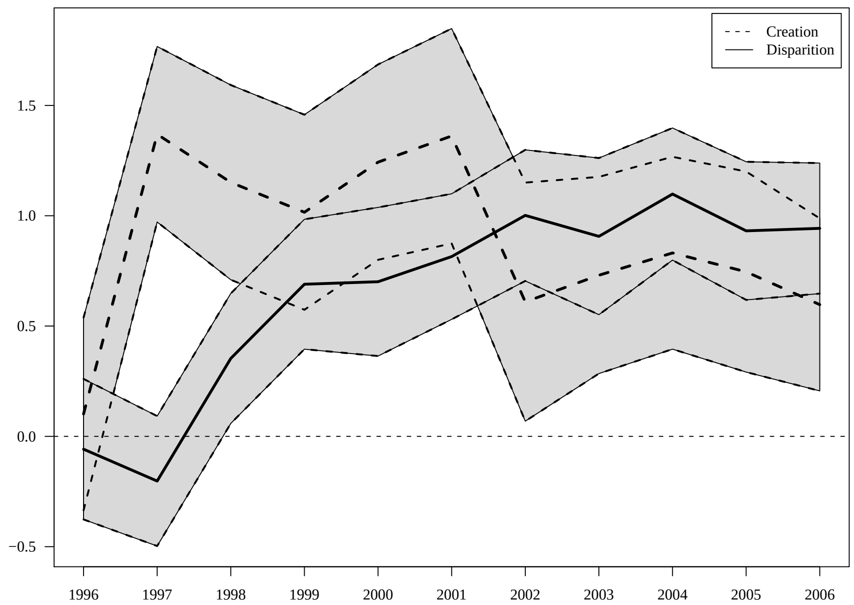

Figure 6: Impact of EZ on firm creation and destruction (restricted to companies with at least one employee).

Note that administrative data do not provide a reliable measure of discontinuance of business.[13] As we know precisely the level of employment for each firm and each year, we are able to know when a firm dismisses its last employee. That is the reason why we only focus on firm which disappear definitively from employment data to provide an approximation of business closure rate. More concretely, a company with employees closes in year $t-1$ if it declares at least one employee on year t and zero employee on year $t+1$. This measurement captures potential relocations outside the area as the identification number of the companies changes in this case. Similarly, we defined an alternative measurement for firm creation in this context. Here, creation of business with salaried employees corresponds to compagnies which declare salaried employees for the first time.

The EZ impact strongly varies with sectors of activity. The impact of tax rebates is considerable for business services. The estimated impact for the number of business

[13] A file records bankruptcies, characterized by insolvency, *i.e.* when a company is no longer able to repay its debts. Not all legal decisions to open bankruptcy proceedings (company filing for bankruptcy as part of legal proceedings) lead to liquidation. Besides, this informs on a small part of discontinuance of business. A company can, for instance, put a stop to its activity because its owner decides to retire and his assets are not taken over.

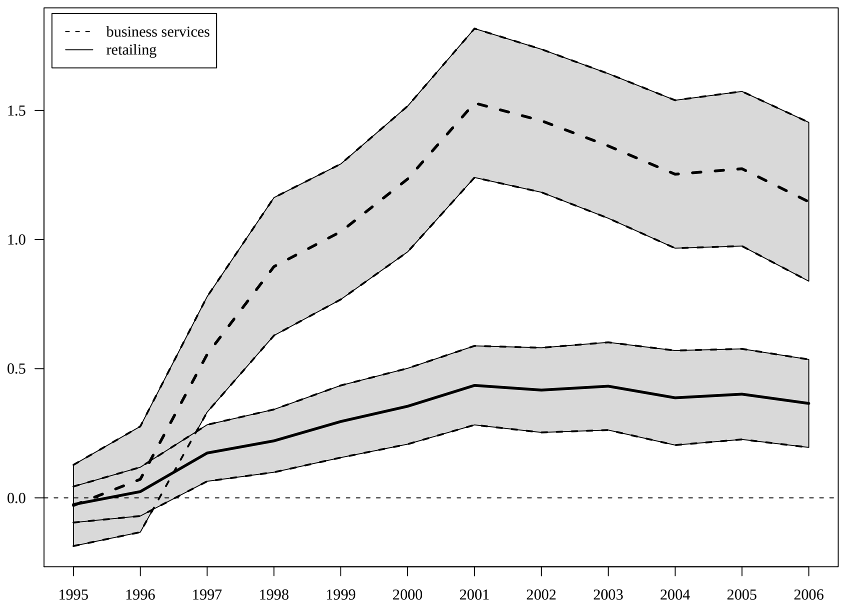

Figure 7: EZ effect on change in the stock of companies for two sectors of activity.

service companies in the area peaks at 1.5 in 2001, meaning that the observed level is 4.6 higher than the counterfactual level (see Figure 7). By contrast, the estimated impact on retail is much smaller and only reaches 0.43 at its highest level. This means that the number of retail businesses is 50 % higher than its counterfactual level (see Table 10 for results on other sectors of activity). These divergent results could be explained by differing production processes. Business service companies do not have to operate in-situ. Conversely, retail businesses may suffer from potential damages associated to distressed urban areas: small market potential in low-income neighborhoods, low accessibility for non-local employees, larger criminality rates...

In 2004, 41 new EZs were created. They have been chosen amongst ZRU. This could bias our main result, as our control group includes some treated areas and could be "contaminated". Excluding areas selected for the second and third waves of Enterprise Zones from the control group does not substantially change our conclusion (see Table 12 in Appendix D). More specifically, we still observe a fading impact of EZs after 2002. This result is again fully consistent with the results of Rathelot and Sillard (2008), who find a small effect of the second wave of Enterprise Zones.

5.1 Robustness checks

This empirical method gives an unbiased estimate of the impact of Enterprise Zones provided that the conditional independence assumption 4.1 holds. We perform a first informal test for this assumption by computing method based on regression and subclassification before the implementation of tax rebates as a falsification test. As data are available since 1995, we can test whether there is any difference between treated and control group prior to the introduction of the policy on January 1st, 1997 or not. We expect not to find any gap if the conditional independence assumption holds. Indeed, our estimate does not show any significant difference. Note that data are available two years before the implementation of EZ for flow data (creation and relocation) and three years before for stock data (number of firms and employment). As stated before, our estimates are computed on time-differentiated variables, therefore, we can display one estimate for firm creation and two estimates for the number of firm and employment prior the introduction of EZs. For the sake of simplicity, only key economic indicators (namely creation, stock of firms and employment) are reported. If we take as null hypothesis that belonging to an EZ as no effect before the implementation of tax rebates in 1997, we cannot reject the null hypothesis neither for firm stock, nor for employment, nor business creation (see table 5, the estimates are the same than in graphs 3 and 4). This gives us some confidence in the validity of our approach.

Table 5: Falsification test: effect prior to the creation of EZ

	Firm creations	Firm stock	Employment
1995		−0.004 [−0.051,0.042]	0.011 [−0.148,0.169]
1996	0.097 [−0.17,0.364]	0.031 [−0.031,0.093]	0.075 [−0.129,0.279]

Besides, our results are robust to a change in the identification strategy. Using the eligibility threshold of 10,000 inhabitants, a regression discontinuity design method provides close results (see table 6). Although coefficients are less precisely estimated, partly because the sample size is smaller, the point estimates are very close for both methods. For instance, in 2001 we obtain an estimated impact of 0.87 with the regression discontinuity design method and 0.70 with the regression and subclassification method.

Table 6: EZ effect on changes in the main variables compared to 1995 level - Regression on discontinuity design

	Firm stock	Employment	Firm creations
1995	−0.052	0.065	
	[−0.28,0.176]	[−0.575,0.705]	
1996	−0.189	0.004	0.161
	[−0.496,0.118]	[−0.755,0.764]	[−0.846,1.168]
1997	−0.086	0.309	0.67
	[−0.436,0.264]	[−0.542,1.159]	[−0.263,1.603]
1998	0.169	0.313	1.244**
	[−0.168,0.506]	[−0.586,1.213]	[0.173,2.316]
1999	0.344*	0.62	1.339**
	[−0.019,0.707]	[−0.4,1.639]	[0.264,2.413]
2000	0.521**	1.142**	1.819***
	[0.121,0.921]	[0.044,2.239]	[0.671,2.967]
2001	0.869***	1.19**	1.756***
	[0.419,1.32]	[0.126,2.255]	[0.655,2.857]
2002	0.8***	1.259**	1.334**
	[0.32,1.28]	[0.185,2.334]	[0.112,2.556]
2003	0.704***	1.23**	1.345**
	[0.194,1.214]	[0.105,2.356]	[0.154,2.537]
2004	0.661**	1.226**	0.831
	[0.136,1.187]	[0.051,2.402]	[−0.242,1.904]
2005	0.73***	1.029*	1.335**
	[0.21,1.251]	[−0.178,2.235]	[0.265,2.404]
2006	0.679***	0.941	0.968*
	[0.166,1.193]	[−0.377,2.258]	[−0.17,2.105]
2007			0.749
			[−0.285,1.782]

Reading note: year t is ATT conditional DiD estimator for log(variable) between year t and 1995. Confidence intervals at 95 % in brackets. Significance levels: *** 1%, ** 5%, * 10%.

6 Discussion

Previous results clearly highlight that French firms strongly react to EZ tax incentives. However, it calls for several comments.

Our control group is composed of ZRUs. Firms located in this type of zone benefit from some limited tax rebates. By comparing EZs and ZRUs, our method only estimates the difference between both policies, while it would have been more relevant to identify the potential economic outcomes in Enterprise Zones without any tax subsidies. In order to provide some insight on this issue, we separately evaluate the impact of the ZRU system. We apply the same methodology to estimate the impact of the ZRUs. ZRUs are now considered as the treated group. We use disadvantaged urban areas that do not benefit from systematic tax subsidies as a control group (i.e. ZUS, the first tier of the French urban policy, see section 2). According to our estimate, the tax subsidies provided by the ZRU system are inefficient to foster economic activity.

Results are provided in Table 7 and measure the sole impact of ZRU until 2004 only, as some ZRUs became EZs in 2004 and 2007. For every year but one, both business creations and firm stock in ZRUs are not significantly different than in ZUS. The findings are the same for employment, except that we cannot totally rule out a significant impact at the end of the period. This can be due to the classification of some of them as EZs, as Rathelot and Sillard (2009) exhibit a significant (but much smaller impact than for the first wave) impact of the policy on employment and business demography. These results are consistent with previous evidence. Compared to EZs, ZRUs provide much less generous tax subsidies to firms located there, which mostly consists in local business tax rebates (with a higher threshold than in EZs, however) than in payroll tax rebates (firms located in ZRU benefit from one-year subsidies for new employees only, while Firm located in EZs benefit from five-year subsidies for all workers). Recent studies suggest that this specific tax scheme is less likely to attract firms. Duranton, Gobillon, and Overman (2010) find no impact on local taxation on non-residential property on entry of English manufacturing establishments, and Rathelot and Sillard (2008) find significant but negligible impact for French firms. It is thus reasonable to think that our estimates provide the total effect of EZs on employment and business demography, and not only the gap between ZRUs and EZs as ZRU program has very little impact on economic activity.

It is not surprising that the effect of EZ on local firms eventually stabilizes. However, the fact this stabilization occurs in 2003 may be explained by two reasons. First, the consequent payroll tax subsidies for low-wage workers introduced on a national scale may have reduced the comparative advantage of EZs compared to other areas. Second, after 2003 the subsidies were more strictly contingent on hiring local workers. This so-called local employment stipulation ("clause d'emploi local" in French) was already in effect between 1997 and 2001, but evidence has been made that it was not strictly enforced (see

Table 7: ZRU effect on changes in the main variables compared to 1995 level - ZUS as control group

	Firm stock	Employment	Firm creations
1995	−0.01 [−0.053,0.033]	0.116* [−0.013,0.245]	
1996	−0.013 [−0.069,0.042]	0.05 [−0.107,0.207]	−0.055 [−0.27,0.161]
1997	−0.017 [−0.078,0.045]	0.137 [−0.034,0.309]	0.103 [−0.084,0.291]
1998	−0.009 [−0.07,0.052]	0.105 [−0.067,0.277]	0.049 [−0.161,0.258]
1999	0.004 [−0.062,0.071]	0.13 [−0.07,0.33]	0.288*** [0.077,0.498]
2000	−0.002 [−0.074,0.069]	0.124 [−0.085,0.332]	0.064 [−0.148,0.276]
2001	0.009 [−0.069,0.086]	0.187* [−0.025,0.399]	−0.002 [−0.217,0.213]
2002	0.004 [−0.082,0.09]	0.17 [−0.042,0.382]	0.172 [−0.051,0.394]
2003	−0.031 [−0.118,0.055]	0.163 [−0.059,0.384]	0.064 [−0.172,0.301]
2004	−0.041 [−0.131,0.048]	0.191 [−0.038,0.421]	0.11 [−0.091,0.31]
2005	0.01 [−0.089,0.109]	0.239* [−0.001,0.48]	0.038 [−0.166,0.241]
2006	0.02 [−0.072,0.111]	0.276** [0.013,0.539]	0.042 [−0.177,0.261]
2007			0.217** [0.013,0.421]

Reading note: year t is ATT conditional DiD estimator for log(variable) between year t and 1995. Confidence intervals at 95 % in brackets. Significance levels: *** 1%, ** 5%, * 10%.

Appendix A). This is consistent with the fact that Gobillon, Magnac, and Selod (2010) find a very small impact on local unemployment of EZ first wave. Real or supposed difficulties to hire adequately skilled workers amongst the population of the area may have discouraged the location of new firms.[14] Note that we observe that the impact of tax subsidies appears to be not significantly higher for low-skilled than for skilled and high-skilled employment (see Table 11).

All in all, the final assessment of the EZs is complex. The first wave of EZs seems to have created a long-lasting impulse on business creation. The change in stock of firms and employment from 1995 are over 75% larger that those that would have prevailed without the tax subsidies. However, several arguments mitigate this positive results. The impact strongly attenuates after the first five-year period of tax exemptions. The still steady flow of new firms do not lead to an increase in employment. Second, if companies do face difficulties in hiring local workers in distressed areas and if firms give up to move to these areas for this reason, it raises serious concerns about both the potential dissuasive impact of the obligation to hire local labor force and the effectiveness of tax cuts to resolve unemployment issues in targeted areas. Significant differences between sectors are also in line with this concern. Business service firms are less prone to foster local economic activity, given that they mostly operate outside the zones.

[14] In 2008, according to a qualitative survey in the EZs, companies of these zones declare major difficulties to hire employees inside the area (and minor but not major difficulties to hire outside the area), as reported in Givord, Rathelot, and Sillard (2011).

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Appendix A: A brief description of the French EZ tax cuts system

Companies located in ZRU as well as companies in EZ benefit from several exemptions. Their extents vary from EZ to ZRU in terms of concerned amount and duration. Their amount and eligibility conditions are yearly modified, but the main elements could be summarized as follows:

All companies in EZ with less than 50 employees the First of January 1997 (or at the time of the first location in the EZ) benefit from a full exemption of **local business tax** for five years. In ZRU the exemption concerns companies with less than 150 employees at the current date. This exemption is limited, however, and in EZ the ceiling is much higher than in ZRU: FF3,000,000 (around 460,000 euros) per year in 2001 while it was only FF920,000 (around 139,000 euros) per year for companies created after 1997 in ZRU (and FF410,000 - around 62,000 euros- for companies present prior this date). All companies in EZ benefit from a full exemption of **corporate income tax** for five years, limited to FF400,000 (around 61,000 euros) per year and partial exemption for 5 following years. In ZRU the exemption is limited to newly created companies in the area, which benefit from full exemption for 2 years and decreasing exemption for the next 3. Besides, all buildings located in EZ belonging to companies liable for the **property tax on buildings** are exempt for 5 years. No such exemption exists in ZRU. Companies in EZ also benefit from additional exemptions on specific taxes, as the tax on property transfer for shops (to a maximum FF700,000, i.e. around 107,000 euros), fees for creation of new offices buildings in Ile-de-France (Paris metropolitan region), or total exemption for local land tax for 5 years.

Finally, companies located in EZ and ZRU benefit from exemptions for **employer payroll taxes** (occupational injury, transportation, housing, family benefit and social insurance contributions). Employees with open-ended contracts or fixed-term employment contracts of more than 12 months are exempt from employer payroll taxes, on the fraction of salary lower than 1.5 times the minimum wage (Smic). In 2006, the ceilings was lowered to 1.4 times the minimum wages. Artisans and shopkeepers benefit from a total exemption for health insurance contribution until 1.5 minimum wages. The duration of exemption is only one year in ZRU, while in EZ it comes from 5 years of completed exemption completed by a 9-year (companies with more than 5 salaried employees) to 3-year (smaller companies) decreasing exemption.

Besides, the exemptions concern only new recruits in ZRU while it benefits to all salaried workers in EZ, conditional to the fact that company hired 20% of their labor force locally ("clause d'embauche locale"). This condition was not applied in practice in the first years of the EZ, so in December 2000 a new law reinforces the firms' obligations with this respect (a specific declaration is required to benefit from tax cuts, and their amounts were reduced for transferred jobs). In December 2002, the needed proportion

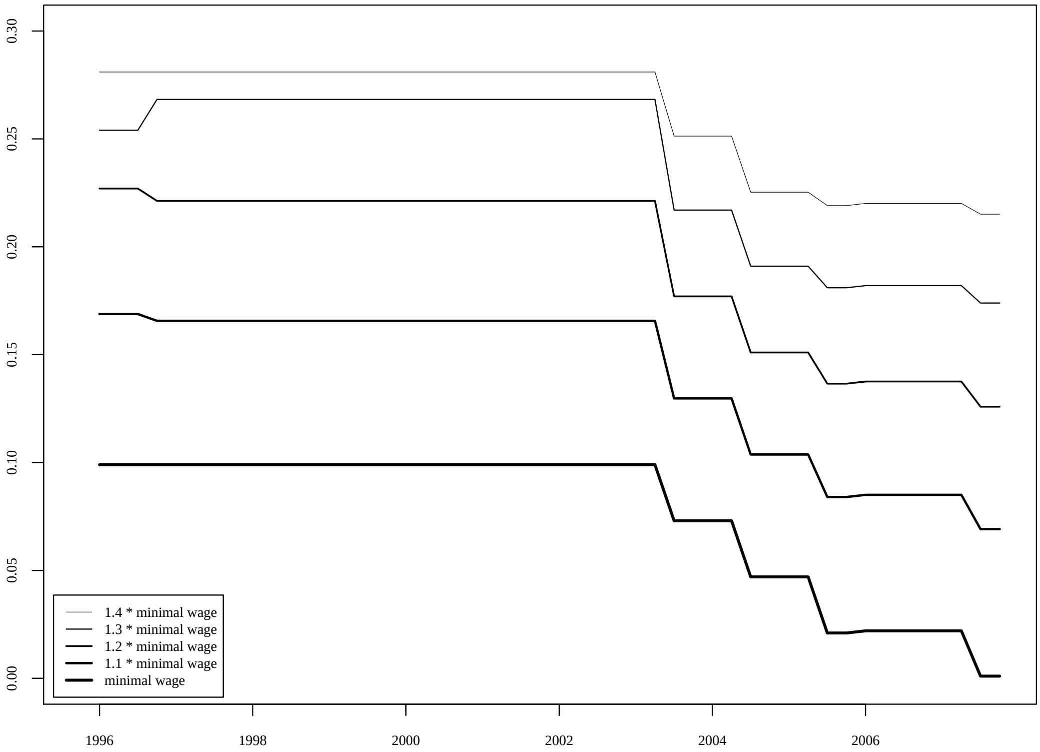

Figure 8: Gap between employer payroll tax at the national level and in EZ according to earning level.

of local hiring was increased from 20% to 33%.

Employer payroll tax rates in general scheme changed since 2003 ("Loi Fillon") in a way that reduces the attractiveness of the EZs. The employer payroll tax rates (social security contribution) for the lowest wages sharply decreased since 2003 compared to the level in EZ (Figure 8). However EZs remain attractive as tax exemption applies to all wages (the tax reduction is applicable to the share of the wage below 1.5 minimum wage) while national exemption only applies to low wages (below 1.6 minimum wage in 2003, 1.3 before).

Appendix B: Database construction

Two administrative database from INSEE have been merged. The DADS database provides yearly employment for each company. The SIRENE database follows all French firms. It contents several files which provides the stock of compagnies on January 1st, each year, as well as firm relocation (the number of new firms created from the 1st of January to the 31st of December of each year). Each company is identified by a registration number. In case of relocation of the company, this registration number changes (in this case, the file corresponding to the flow of companies relates the new and the old registration number). More important, firms are precisely georeferenced. It is crucial for our study as the enterprise zone boundaries do not correspond to the usual administrative borders. The SIRENE database indicates whether the company is located within these boundaries or not, each year from 1995 to 2007.

The data have been modified for the needs of the study. First, georeferenced creation and relocation data are yearly available over the whole period 1995-2007 while the precise location is missing for some years in the data providing the "stock" of firms recorded on January the 1st of each year. More precisely, the geolocalisation are not available in this database in 1996, 1998, 2000 and 2001. This information can be extrapolated from others year, however : we indeed have access to the precise identification number of each firm. This identification number change in case of a relocation : the presence of the same identification number in year t_1 and t_2 means that the firm have not moved over the period. To be more specific, consider the case of 1998 stock data, where all firms are registered, but without precise location. If this very same firm is already registered on database of the previous years, we can use the location variable available in these database. Otherwise, it means that the firm has just located in 1998: in this case we find a record in the database for creation and relocation, that contains a location variable for all years. All in all, geolocation, and more precisely EZ location, can be retrieved for all firms.

Second, the geolocation is not always time-consistent: a company may be registered within an EZ one year and not the other, even if it is located at the exact same address, and even if EZ boundaries are not modified. This is due to some inaccuracies in the GIS. If rare, this misclassification could introduce noise in the estimation. When it is the case, we use, by convention, the first location.

Third, we take into account a subtlety of the enterprise zone boundaries. Recall that enterprise zones are selected among most disadvantaged urban areas (ZUSs). In some rare cases, an EZ merges more than one ZUS. As propensity score variables are available at the ZUS scale and the EZ scale, our study unit is the ZUS. That is why our treated group contains 44 areas whereas only 38 EZs where implemented in continental France in 1997. In addition, EZ boundaries and ZUS boundaries may not exactly match. For the sake of consistency, we choose to restrict to companies located within both a ZUS and a ZFU.

Appendix C: Estimation of the propensity score

We adapt the estimation of the propensity score to our specific setting. The eligibility condition creates a censured process for the treatment, where the censoring depends on a supplementary variable (the size of the area). Ignoring the fact that only most populous areas are eligible to Enterprise Zones system can bias the estimation of the propensity score, if some observable characteristics used for the score are correlated with the size of the area. Assuming conditional independence between the size and unobserved determinant of the treatment, we can separately estimate the probability of being treated on the areas with size above the threshold, and the probability of having a size, conditional to observable.

Formally, we observe T_i only if $S \geq S_0$, where S_0 denotes the threshold of 10,000 inhabitants. Let denote by $D_i = 1_{S \geq S_0}$. Assuming a conditional independance between the treatment and the fact of having a big size conditional to observable:

Assumption .1 *(Conditional independence of treatment and size)* $D_i \perp T_i | X_i$

Under this assumption, we could write the likelihood of the sample corresponding to the couple $(D_i, T_i D_i)$ as:

$$\begin{aligned} lnL(\theta) = & \sum_i (1 - D_i) ln(1 - (P(D_i = 1|X))) \sum_i D_i ln(P(D_i = 1|X)) \\ & + \sum_i (1 - T_i) D_i ln(P(T_i = 1|X)) + \sum_i T_i D_i ln(1 - (P(T_i = 1|X))) \end{aligned}$$

We could thus estimate separately both components. In practice, we estimate the score $P(T_i = 1|X_i, S \geq S_0)$ and the conditional probability of having a size above the threshold of 10,000 inhabitants using logistic specifications.

Table 8: Logit regressions for propensity score estimation

	Area selected as Enterprise Zone	More than 10,000 inhabitants in the area
Intercept	-17,09**	-8,40***
	(7,73)	*(3,20)*
Pct. of executives	-12,78	54,28***
	(43,33)	*(17,87)*
Pct. of stable households	-1,69	0,89*
	(1,06)	*(0,47)*
Unemployment rate	13,24*	-2,24
	(7,38)	*(2,76)*
Pct. of young people	16,22*	4,99
	(8,90	*(3,26)*
Pct. of social housing	-3,45*	-0,85
	(2,06)	*(0,73)*
Potential taxe revenu	-0,001***	0,0003**
	(0,0003)	*(0,0001)*
Pct. of foreigners	14,22***	-0,62
	(5,45)	*(1,87)*
Pct. of unskilled people	61,86**	22,36*
	(27,53)	*(12,05)*
Pct. of unskilled people^2	-78,58**	-32,10**
	(33,18)	*(14,40)*

Appendix D: Detailed results

Table 9: EZ effect on the changes in flows and stock of firms compared to 1995 level

	Stock of firm	Firm creation	Firm transfer	New firm (DADS)	Firm disparition (DADS)
1995	-0.004 [−0.051,0.042]				
1996	0.031 [−0.031,0.093]	0.097 [−0.17,0.364]	-0.148 [−0.628,0.332]	0.101 [−0.335,0.538]	-0.058 [−0.377,0.26]
1997	0.226*** [0.156,0.296]	0.644*** [0.399,0.89]	0.919*** [0.37,1.468]	1.369*** [0.972,1.767]	-0.203 [−0.497,0.092]
1998	0.358*** [0.275,0.442]	0.747*** [0.49,1.004]	1.216*** [0.511,1.921]	1.151*** [0.71,1.593]	0.353** [0.059,0.648]
1999	0.458*** [0.363,0.554]	0.748*** [0.473,1.023]	1.126*** [0.478,1.775]	1.016*** [0.574,1.458]	0.69*** [0.395,0.984]
2000	0.561*** [0.449,0.673]	0.807*** [0.549,1.064]	1.422*** [0.906,1.937]	1.243*** [0.8,1.686]	0.701*** [0.364,1.038]
2001	0.7*** [0.58,0.82]	0.962*** [0.683,1.242]	1.784*** [1.195,2.373]	1.361*** [0.874,1.848]	0.815*** [0.53,1.099]
2002	0.682*** [0.557,0.808]	0.45*** [0.142,0.757]	0.582** [0.024,1.14]	0.61** [0.069,1.15]	1.002*** [0.704,1.299]
2003	0.684*** [0.555,0.813]	0.694*** [0.36,1.027]	0.737*** [0.196,1.278]	0.73*** [0.285,1.176]	0.906*** [0.552,1.261]
2004	0.643*** [0.504,0.782]	0.559*** [0.249,0.869]	0.882*** [0.234,1.531]	0.831*** [0.395,1.267]	1.098*** [0.799,1.398]
2005	0.645*** [0.503,0.786]	0.635*** [0.372,0.899]	0.703** [0.126,1.28]	0.746*** [0.291,1.2]	0.932*** [0.618,1.245]
2006	0.596*** [0.456,0.735]	0.607*** [0.284,0.931]	0.693** [0.076,1.31]	0.597*** [0.206,0.988]	0.943*** [0.647,1.239]
2007		0.543*** [0.288,0.798]	0.755*** [0.188,1.322]		

Reading note: year t is ATT conditional DiD estimator for log(variable) between year t and 1995. Confidence intervals at 95 % in brackets. Significance levels: *** 1%, ** 5%, * 10%.

Table 10: EZ effect on changes in the number of firms compared to 1995 level by sector of activity

	Business services	Trade	Health, education, community work	Building trade	Industry
1995	-0.029	-0.026	-0.003	0.008	0.01
	[-0.187,0.128]	[-0.096,0.044]	[-0.061,0.056]	[-0.119,0.134]	[-0.109,0.129]
1996	0.072	0.024	-0.028	-0.02	0.068
	[-0.133,0.277]	[-0.07,0.119]	[-0.125,0.069]	[-0.205,0.165]	[-0.1,0.236]
1997	0.555***	0.174***	0.038	0.271***	0.253**
	[0.332,0.779]	[0.064,0.283]	[-0.092,0.168]	[0.067,0.476]	[0.054,0.453]
1998	0.895***	0.221***	0.154*	0.502***	0.439***
	[0.628,1.162]	[0.099,0.342]	[-0.007,0.316]	[0.288,0.715]	[0.225,0.652]
1999	1.031***	0.296***	0.228**	0.643***	0.462***
	[0.768,1.293]	[0.156,0.435]	[0.044,0.412]	[0.412,0.874]	[0.218,0.706]
2000	1.235***	0.355***	0.273***	0.737***	0.626***
	[0.953,1.517]	[0.208,0.502]	[0.081,0.465]	[0.473,1.001]	[0.359,0.893]
2001	1.529***	0.435***	0.402***	0.833***	0.732***
	[1.24,1.817]	[0.282,0.588]	[0.206,0.598]	[0.554,1.112]	[0.442,1.022]
2002	1.46***	0.417***	0.402***	0.869***	0.78***
	[1.183,1.737]	[0.253,0.581]	[0.189,0.614]	[0.584,1.154]	[0.508,1.053]
2003	1.362***	0.432***	0.426***	0.771***	0.855***
	[1.082,1.642]	[0.262,0.602]	[0.213,0.64]	[0.489,1.053]	[0.588,1.123]
2004	1.253***	0.387***	0.387***	0.684***	0.899***
	[0.967,1.539]	[0.204,0.57]	[0.175,0.6]	[0.387,0.981]	[0.63,1.167]
2005	1.274***	0.401***	0.416***	0.678***	0.862***
	[0.975,1.573]	[0.226,0.577]	[0.206,0.626]	[0.38,0.976]	[0.58,1.145]
2006	1.146***	0.366***	0.465***	0.655***	0.849***
	[0.839,1.454]	[0.195,0.536]	[0.244,0.686]	[0.362,0.948]	[0.57,1.128]

Reading note: year t is ATT conditional DiD estimator for log(variable) between year t and 1995. Confidence intervals at 95 % in brackets. Significance levels: *** 1%, ** 5%, * 10%.

Table 11: EZ effect on changes in the employment compared to 1995 level

	Employment	Low-skilled employment	Skilled employment	High-skilled employment	Employment in firms established before 1995	Employment in firms established after 1995
1995	0.011 [−0.148,0.169]	0.033 [−0.158,0.224]	0.015 [−0.159,0.19]	0.019 [−0.162,0.199]	−0.068 [−0.176,0.04]	
1996	0.075 [−0.129,0.279]	0.144 [−0.138,0.427]	0.097 [−0.155,0.35]	−0.012 [−0.248,0.224]	0.029 [−0.117,0.175]	−0.06 [−0.534,0.414]
1997	0.375*** [0.148,0.602]	0.457*** [0.173,0.742]	0.454*** [0.191,0.716]	0.292** [0.021,0.563]	0.065 [−0.104,0.234]	0.714*** [0.197,1.231]
1998	0.602*** [0.358,0.847]	0.602*** [0.277,0.927]	0.717*** [0.442,0.992]	0.522*** [0.235,0.809]	0.107 [−0.083,0.296]	0.984*** [0.456,1.512]
1999	0.804*** [0.527,1.08]	0.903*** [0.56,1.246]	0.867*** [0.537,1.196]	0.752*** [0.458,1.046]	0.077 [−0.161,0.315]	1.134*** [0.604,1.663]
2000	0.947*** [0.65,1.244]	1.06*** [0.688,1.431]	1.032*** [0.684,1.38]	0.876*** [0.567,1.186]	−0.02 [−0.269,0.229]	1.246*** [0.708,1.784]
2001	1.098*** [0.798,1.398]	1.153*** [0.753,1.553]	1.13*** [0.786,1.475]	1.12*** [0.783,1.456]	0.028 [−0.282,0.338]	1.332*** [0.772,1.891]
2002	1.123*** [0.822,1.424]	1.255*** [0.869,1.642]	1.198*** [0.851,1.545]	1.054*** [0.701,1.406]	0.05 [−0.272,0.372]	1.328*** [0.771,1.884]
2003	1.114*** [0.799,1.43]	1.325*** [0.922,1.728]	1.144*** [0.766,1.522]	1.067*** [0.723,1.41]	0.091 [−0.264,0.447]	1.273*** [0.708,1.839]
2004	1.076*** [0.748,1.403]	1.385*** [0.967,1.802]	1.127*** [0.732,1.521]	0.911*** [0.554,1.268]	0.071 [−0.292,0.433]	1.219*** [0.655,1.783]
2005	1.067*** [0.734,1.399]	1.318*** [0.913,1.723]	1.095*** [0.675,1.515]	0.988*** [0.638,1.337]	0.137 [−0.227,0.502]	1.148*** [0.599,1.698]
2006	0.926*** [0.582,1.269]	1.092*** [0.697,1.487]	0.97*** [0.576,1.363]	0.892*** [0.539,1.246]	0.173 [−0.211,0.557]	0.866*** [0.324,1.408]

Reading note: year t is ATT conditional DiD estimator for log(variable) between year t and 1995. Confidence intervals at 95 % in brackets. Significance levels: *** 1%, ** 5%, * 10%.

Table 12: EZ effect on changes in the main variables compared to 1995 level - 2nd and 3rd waves EZ excluded from the control group

	Stock of firm	Employment	Firm creation
1995	−0.016 [−0.069,0.037]	0.108 [−0.129,0.344]	
1996	0.014 [−0.062,0.091]	0.157 [−0.123,0.438]	0.103 [−0.223,0.429]
1997	0.194*** [0.101,0.287]	0.407*** [0.122,0.692]	0.558*** [0.264,0.851]
1998	0.337*** [0.228,0.446]	0.705*** [0.397,1.014]	0.745*** [0.418,1.073]
1999	0.444*** [0.321,0.568]	0.852*** [0.509,1.196]	0.723*** [0.391,1.054]
2000	0.546*** [0.404,0.688]	1.017*** [0.648,1.387]	0.8*** [0.496,1.104]
2001	0.696*** [0.537,0.856]	1.156*** [0.784,1.527]	1.007*** [0.683,1.33]
2002	0.685*** [0.516,0.853]	1.189*** [0.821,1.557]	0.495*** [0.141,0.85]
2003	0.687*** [0.506,0.868]	1.206*** [0.822,1.589]	0.764*** [0.403,1.126]
2004	0.675*** [0.488,0.862]	1.208*** [0.812,1.603]	0.63*** [0.261,0.999]
2005	0.709*** [0.52,0.897]	1.187*** [0.791,1.582]	0.719*** [0.389,1.049]
2006	0.688*** [0.51,0.867]	1.206*** [0.766,1.645]	0.889*** [0.518,1.259]
2007			0.729*** [0.428,1.029]

Reading note: year t is ATT conditional DiD estimator for log(variable) between year t and 1995. Confidence intervals at 95 % in brackets. Significance levels: *** 1%, ** 5%, * 10%.

Table 13: EZ effect on changes in the main variables compared to 1995 level - Propensity score matching

	Stock of firm	Employment	Firm creation
1995	-0.004 [−0.03,0.033]	-0.016 [−0.106,0.057]	
1996	0.033^{*} [−0.002,0.072]	0.007 [−0.096,0.093]	0.151^{**} [0.015,0.285]
1997	0.221^{***} [0.172,0.28]	0.25^{***} [0.13,0.361]	0.55^{***} [0.387,0.701]
1998	0.336^{***} [0.277,0.399]	0.429^{***} [0.282,0.546]	0.642^{***} [0.472,0.791]
1999	0.408^{***} [0.344,0.481]	0.539^{***} [0.361,0.691]	0.574^{***} [0.386,0.733]
2000	0.504^{***} [0.429,0.586]	0.692^{***} [0.489,0.862]	0.698^{***} [0.516,0.84]
2001	0.645^{***} [0.553,0.749]	0.831^{***} [0.603,1.035]	0.979^{***} [0.755,1.169]
2002	0.635^{***} [0.541,0.739]	0.851^{***} [0.64,1.065]	0.326^{***} [0.13,0.499]
2003	0.665^{***} [0.566,0.777]	0.878^{***} [0.638,1.089]	0.666^{***} [0.47,0.84]
2004	0.652^{***} [0.539,0.769]	0.86^{***} [0.622,1.058]	0.572^{***} [0.351,0.753]
2005	0.64^{***} [0.522,0.755]	0.844^{***} [0.601,1.052]	0.557^{***} [0.374,0.725]
2006	0.603^{***} [0.474,0.722]	0.773^{***} [0.546,0.961]	0.584^{***} [0.391,0.769]
2007			0.494^{***} [0.301,0.67]

Reading note: year t is ATT conditional DiD estimator for log(variable) between year t and 1995. Confidence intervals at 95 % in brackets. Significance levels: *** 1%, ** 5%, * 10%.