Journal of Public Economics 96 (2012) 881–892

Contents lists available at SciVerse ScienceDirect

Journal of Public Economics

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

Do unemployed workers benefit from enterprise zones? The French experience☆

Laurent Gobillon [a,b], Thierry Magnac [c,*], Harris Selod [d,e]

[a] Institut National d'Etudes Démographiques, INED, 133 boulevard Davout, 92245 Paris Cedex, France
[b] PSE, France
[c] Toulouse School of Economics, Université de Toulouse (GREMAQ & IDEI), Manufacture des Tabacs, 21 allée de Brienne, 31000 Toulouse, France
[d] The World Bank, Development Economics Research Group, 1818 H Street, NW, Washington, DC 20433, USA
[e] INRA-Paris School of Economics, France

ARTICLE INFO

Article history:
Received 25 October 2010
Received in revised form 18 May 2012
Accepted 20 June 2012
Available online 29 June 2012

Keywords:
Enterprise zones
Policy evaluation
Unemployment
Economic geography
Duration models

ABSTRACT

This paper presents an impact evaluation of the French enterprise zone program which was initiated in 1997 to help unemployed workers find employment by granting a significant wage-tax exemption (about one third of total labor costs) to firms hiring at least 20% of their labor force locally. Drawing from a unique geo-referenced dataset of unemployment spells in the Paris region over an extensive period of time (1993–2003), we are able to measure the direct effect of the program on unemployment duration, distinguishing between short- and medium-term effects. This is done by implementing an original two-stage empirical strategy using individual data in the first stage and aggregate data and conditional linear matching techniques in the second stage. We show that although the enterprise zone program tended to "pick winners", it is likely to be cost-ineffective. It had a small but significant effect on the rate at which unemployed workers find a job (which is increased by a modest 3%). This effect is localized and significant only in the short run (i.e. at best during the 3 years that follow the start of the policy).

1. Introduction

Most cities have distressed neighborhoods where jobs are few and unemployment is rampant. Considering that the lack of labor demand in poor areas is a key contributor to local unemployment, a number of countries, including the US, the UK and France have responded by implementing spatially targeted policies to encourage job creation or firm relocation to these areas. These policies—often designated as enterprise zone (EZ) programs—revolve around the simple idea that granting fiscal incentives to firms in distressed neighborhoods can boost local hires. Although intuitively appealing, enterprise zones are in fact rather controversial as many observers have questioned their ability to reach their objectives and whether achieved benefits are sufficient to balance costs (Peters and Fishers, 2004).

In this paper, we provide an impact evaluation of the French enterprise zone experience, focusing on the Paris region for which there exists an exhaustive and georeferenced dataset of unemployment spells that allows for an adequate evaluation of the policy at the local level. The key measure in the French program is that, in order to be exempted from the wage tax, firms need to hire at least 20% of their labor force locally. In the French context, this is a significant incentive given that the wage tax—which depends on the wage level, the type of work and the work contract—represents more than one third of all labor costs borne by employers. The policy was thus expected to improve local employment through hires made by existing, relocating, or newly-created firms drawing from the local pool of unemployed workers.

Our empirical strategy for the impact evaluation of the program is original in various ways:

We depart from the approach used in previous papers in the literature as we investigate the *propensity of local unemployed workers to find a job*. This is an appropriate, precise, and well-targeted indicator of policy success given the explicit policy goal of helping residents in distressed areas find jobs and given the existence of a unique dataset on unemployment durations and exits with observations at high frequency. Using continuous-time unemployment duration data allows us to focus on the semesters around the implementation of the program and distinguish short run from medium run effects of the policy. This approach contrasts with other evaluations of enterprise zones which have usually focused on the growth in the local number of establishments or on the number of local jobs that were

☆ The authors are grateful to the coeditor Mark Duggan and a referee for their insightful comments and to participants at the following conferences and seminars: NARSC '08, EALE '09, ESEM '09, London School of Economics and the 2nd French Econometrics Conference, for their helpful comments, and particularly to Roland Rathelot, Shawn Rohlin and Jeffrey Zax. We would also like to thank the French Ministry of Health (MiRe-DREES) and the French Ministry of Labor (DARES) for financial support. The opinions expressed in this article are those of the authors and do not necessarily reflect the views of those institutions or of our employers, including the World Bank, its Executive Board, or the countries they represent. All remaining errors are ours.

* Corresponding author.
E-mail addresses: laurent.gobillon@ined.fr (L. Gobillon), thierry.magnac@tse-fr.eu (T. Magnac), hselod@worldbank.org (H. Selod).

0047-2727/$ – see front matter © 2012 Elsevier B.V. All rights reserved.
doi:10.1016/j.jpubeco.2012.06.003

created as a result of the policy. But since job and establishment creations may also benefit residents from non-targeted areas, such indicators can only be suggestive of the true effect on unemployment in targeted areas.

Our methodology allows us to estimate the unemployment duration for each of the 1300 municipalities in the Paris region, the municipality being the finest spatial unit of analysis that is available in the data. Since municipalities have a population size which is broadly twice that of the enterprise zone they contain, this means that we capture the overall effect in the EZ and non-EZ parts of a same municipality. Since municipalities are relatively small, however, we are able to investigate the possibility of spatial spillovers on neighboring municipalities.

Even though we do not have a controlled experiment, we argue that because policy makers selected treated municipalities on observables, matching techniques can be used for the impact evaluation. Moreover, while designation was indeed based on a criterion that included measures of population and labor force composition, political tampering implied imperfect targeting of municipalities so that some municipalities that were not targeted by the program have characteristics similar to those of treated municipalities and can be used as a control group.

As the existence of political tampering does not exclude other sources of selection on unobservables that would bias the results of matching techniques, we address selection issues in our two-stage methodology. In the first stage, we propose a new econometric approach to estimate local effects while controlling for individual variables to avoid composition biases. To do that, we use a proportional hazard model of individual unemployment durations which is stratified by municipality as was originally proposed by Ridder and Tunali (1999) and extended by Gobillon et al. (2011). This Stratified Partial Likelihood Estimator (SPLE) estimates the spatial effects measuring the easiness with which residents exit unemployment for each municipality in the Paris region for each semester between 1993 and 2003. This procedure effectively addresses two issues. Firstly, municipality effects are purged of the composition effects of the residents. Secondly, right censoring that affects unemployment durations is accounted for in the estimation.

In the second stage, in order to assess the effect of the policy, we measure how these municipality effects changed over time (before and after the creation of enterprise zones) comparing municipalities that host an enterprise zone (the "treated" municipalities) and other municipalities of comparable characteristics (the control group). This second stage uses conditional matching techniques to address possible issues of treatment selectivity (see Blundell and Costa-Dias, 2009, for a recent survey). Given our fine control of composition and of right censoring biases in the first stage, and given the way selection into treatment was implemented, we argue that, conditional on the variables that affect treatment probability, trends in unemployment durations in treated and control municipalities would have been on average similar in the absence of treatment. The results of our empirical strategy prove to be robust to a variety of appropriate robustness checks including redefinition of control groups (Smith and Todd, 2005), redefinition of the treatment status so as to capture spatial spillover effects, as well as various weighting schemes and the introduction of other controlling factors.

Our results point to three main conclusions. First, we find evidence that the policy tended to "pick winners", that is to select municipalities in which unemployed workers face better prospects, a common feature in many EZ programs. Second, and more importantly, we find that enterprise zones have a temporary and moderate but significant impact on exit rates from unemployment to employment. At the time the policy was initiated, the average number of unemployed workers residing in municipalities that benefited from the enterprise zone program and who could find a job increased by a modest 3%. Since, on average, about 300 unemployed workers found a job every semester in each municipality in our sample, this means that, over a 6 month period, the policy only helped an additional 10 workers find a job in each municipality. This is very modest in view of the cost of the policy. Furthermore, our results suggest that this positive effect only occurred in the short run (at most 3 years) as we do not find evidence of medium run effects between 3 and 6 years. Finally, the effect on unemployment exits remains localized and no spillover effects are significant.

Our work complements an econometric study of the impact of the French enterprise zone program on the growth in the number of establishments which found that the policy had a significant positive impact. This impact remains limited however when considering the large cost of the policy (see Rathelot and Sillard, 2009). More generally, the limited impact of tax exemption policies is also confirmed by a general equilibrium analysis based on the calibration of matching models of worker and firm mobility (Sidibé, 2011).

The structure of the paper is as follows. The section following this introduction provides a survey of the literature on enterprise zones and presents the enterprise zone program in France. We describe our data in a third section, and in a fourth section we explain our identification strategy. In the fifth section, we present the results of the policy evaluation. A sixth section concludes and discusses policy lessons.

2. Enterprise zones: lessons from other impact evaluations

Enterprise zone (EZ) programs are territorial discrimination policies that consist in providing tax incentives and exemptions from regulations to specific blighted areas. The objective is to promote local economic development and, in particular, to improve the level of local employment through incentives for firms to invest, hire, locate or relocate to the targeted areas. Following the UK and US experiences, France voted its first EZ program in 1996, and implemented it the following year.

A comparison of existing EZ programs shows that the specific fiscal tools that are used vary widely from different forms of relief on capital taxation to employment and hiring tax credits, or a combination of both. In what follows, we will focus on whether they can succeed in promoting employment by subsidizing labor (e.g. relief on wage taxes) which should have an unambiguous effect on employment by strengthening the incentives to hire workers.

Nonetheless, several criticisms grounded in economic theory have been formulated. A first issue is that fiscal incentives may turn out to provide windfall effects to firms who would have hired workers in any case, with little impact on the local level of employment. The effects of enterprise zones could also be transitory only due to the exhaustion of opportunities for local job creation or because of the phasing out of subsidies. They could cause geographical shifts in jobs from non-EZ to EZ areas only although this need not be considered a failure of the policy if it is socially desirable to spatially redistribute jobs to places of low employment. Furthermore, in the absence of tax revenue compensation, enterprise zone programs may lead to a decrease in the local provision of public services, which in turn may render targeted localities less attractive for firm and harm local residents. Lastly, it can be argued that providing only fiscal incentives could be insufficient to improve local employment when there is above all a mismatch between unemployed workers' skills and job requirements. Area designation could even result in the stigmatization of the targeted neighborhood, further exacerbating the redlining behavior of employers.

2.1. A brief survey of recent evaluations

In view of the above arguments, whether enterprise zones successfully manage to improve employment may thus strongly depend on the specificity of each program. Some implementation options may indeed be more favorable to employment creation than others (capital subsidies, for instance, may have an ambiguous effect on labor demand if capital and labor are substitutes in the industries affected by the policy). The success of enterprise zones may also depend on whether the local context is conducive to producing results (the scarcity of land in targeted areas, for instance, may restrict opportunities for job creation). Whether studies find that enterprise zones are successful may also depend on the geographic scope retained for the impact evaluation as neighboring areas which could be affected by spatial spillovers may or may not be included in the analysis. Spatial spillovers can be positive if workers in neighboring areas benefit from the expansion of the activity in the EZ. This can arise from a higher labor demand in EZ or indirectly from agglomeration economies benefiting to firms in neighboring municipalities which may open additional job positions. A "positive" externality on non-EZ areas may also occur if the policy adversely leads to the stigmatization of EZ residents, with employers discriminating against EZ residents and becoming more likely to hire workers residing outside the EZ. Alternatively, negative spillovers may arise if jobs are relocated away from neighboring areas, or if some substitution of non-EZ jobs with EZ jobs occur.

These issues clearly make the evaluation of EZ programs a key but intricate empirical matter and explain the relatively abundant and mixed literature on the topic (see Peters and Fisher, 2004, and Hirasuna and Michael, 2005, for recent surveys). The main usual challenge in such evaluations is to address selection issues in the designation of areas and this requires resorting to quasi-experimental techniques using panel data for instance to control for local unobserved heterogeneity as in the present paper.

In the US, both the econometric evaluations of state EZ programs already reported in the above-mentioned surveys and the more recent economic literature provide mixed results. We restrict our discussion below to the most recent studies on the effect on employment which resort to now standard econometric tools used for evaluation. Elvery (2009) who studies the EZ programs in California and Florida, finds no evidence that enterprise zones have affected the individual probability of employment for zone residents. Results are more nuanced in Bondonio and Greenbaum (2007) who focus on the effects of enterprise zone programs in ten States and Washington, DC and separately evaluate the effects of the EZ program on new, existing, and vanishing establishments. They find that enterprise zone programs increase employment in new establishments in spite of being offset by the accelerated loss of employment in vanishing establishments. They are also able to identify which features of the programs have greater positive impacts on existing businesses, stressing the role of incentives tied to job creation and of strategic local development plans.

Earlier findings by O'Keefe (2004) on the California program report evidence of a transitory effect on employment in targeted zones. This result is challenged and contradicted by Neumark and Kolko (2010) who resort to a finer geographic scale of analysis. Checking whether establishments are located within precise street boundaries of enterprise zones over the 1992–2004 period, they find that the effect of Californian enterprise zones on employment is insignificant both in the short and the long run.

Since 1994, federal programs have complemented the enterprise zone policies that were initiated by states and their evaluations are reported in several studies. Busso and Kline (2008), in particular, compare census tracts in designated zones with tracts in empowerment zones that were rejected by the program (according to a competitive process) or which ended up designated only at a later date.[1] They find that empowerment zone programs had a positive effect on local employment and a negative effect on the local poverty rate. Obviously, the validity of these results hinges upon the comparability of selected and non-selected zones. This is challenged by Hanson (2009) who argues that zone designation might have been endogenous. When instrumenting empowerment zone designation by political variables, the empowerment zone program is found to have no effect on employment. Finally, Ham et al. 2011 evaluate the effect of Enterprise Zones, Empowerment Zones, and Enterprise Community programs on targeted areas with various double-difference methods using the 1980, 1990 and 2000 census tract data and addressing selection issues. Their results are overall supportive of these three programs which, in particular, decrease the unemployment rate in targeted areas.

2.2. Enterprise zones in France

France launched its first enterprise zone program on January 1, 1997 by creating 44 enterprise zones (*Zones Franches Urbaines* in French), among which 38 are located in metropolitan France, and 9 in the Paris region.[2] Figures from the 1999 Census of the Population indicate that the 9 enterprise zones in the Paris region hosted about 220,000 inhabitants, i.e. 2% of the population of the region. They also accounted for a significant portion of the population in the municipalities in which they are located (between 22% and 68%). Enterprise zones are the third and smallest level of a nested three-tier zoning system of distressed areas around which France organizes its urban policy interventions. While the first and second tier are mostly the focus of social programs and urban revitalization projects, the third tier areas are the most distressed and were aimed as specific targets of the French EZ program (see DIV, 2004, for more details).

The selection of those areas was clearly not random. Municipalities or groups of municipalities had to apply to the program and projects were selected taking into account their ranking given by a synthetic indicator. This indicator, which has never been publicly released, aggregates five criteria based on the population of the proposed zone, its unemployment rate, the proportion of youngsters (less than 25 years old), the proportion of workers with no skill, and the so-called "fiscal potential" of the municipality or of the municipalities in which the zone is located.[3] Nevertheless, the views of local and centralized government representatives who intervened in the geographic delimitation of the zones also played a role in the selection process. After application of the criteria and consideration of local interests, enterprise zones ended up being large neighborhoods of at least 10,000 inhabitants that had particularly severe unemployment problems.

The fiscal incentives were uniform across the country and consisted in a series of tax reliefs on property holding, corporate income, and above all on wages (see DIV, 2004, for more details).[4] The key measure was that firms needed to hire at least 20% of their labor force locally (after the third worker hired) in order to be exempted from wage taxes (i.e. to be relieved from employers' contributions to the national health insurance and pension system). This is a significant exemption that represents around 30% of all labor costs (gross wage). Under the policy, an employer paying a worker the minimum wage (a net monthly wage of approximately 800 Euros in 1997) would be exempted from

[1] In the US, empowerment zones (and enterprise communities) refer to enterprise zones that are enacted by the Federal government as opposed to the States.

[2] The 9 targeted neighborhoods in the Paris region are located within or across 13 municipalities. The list is as follows: Beauval/La Pierre Collinet (in the municipality of Meaux), ZUP de Surville (in Montereau-Fault-Yonne), Le Val Fourré (in Mantes-la-Jolie), Cinq Quartiers (in Les Mureaux), La Grande Borne (in Grigny and Viry-Châtillon), Quartier Nord (in Bondy), Grand Ensemble (in Clichy-sous-Bois and Montfermeil), Le Bois L'Abbé/Les Mordacs (in Champigny-sur-Marne and Chennevières-sur-Marne), Dame Blanche Nord-Ouest/La Muette/Les Doucettes (in Garges-lès-Gonesse and Sarcelles).

[3] The "fiscal potential" is the fictive local amount of taxes that would be collected if local tax rates were uniform across all municipalities in France. The formula of the synthetic indicator for a given area is the product of the first four criteria computed at the area level divided by the fifth criterium computed for the municipality where the area is located (see DIV, 2004).

[4] Exemptions concern the specific following taxes: *taxe professionnelle* (business rate), *impôt sur les bénéfices* (profit tax), *taxe foncière* (property tax), *cotisations sociales personnelles maladie et maternité* (individual health insurance contributions) and *charges sociales patronales* (employers' social security contribution). The two latter categories constitute the "wage tax" and exemptions from the wage tax represented 48% of the 123 million that the policy cost in its first year of implementation (DIV, 2001).

additionally paying a wage tax of approximately 340 Euros every month. These exemptions were meant to be temporary and were more advantageous for small firms (i.e. for establishments with less than 5 salaried workers) which benefited from a 9-year rather than a 5-year exemption completed by a 3-year degressive exemption. The program was meant to last until January 1, 2002 but was eventually extended beyond that date.

Surprisingly, no evaluation of the French enterprise zone program was initially planned and descriptive studies which were subsequently carried out by different public authorities, yielded opposite conclusions from "no effect" to "considerable effects" (DIV, 2001; André, 2002, Ernst, 2008, Gilli, 2006, Thélot, 2004 and Ministère délégué à la ville et à la rénovation urbaine, 2002). An econometric evaluation of enterprise zones is provided by Rathelot and Sillard (2009) who focus on the effect of enterprise zones on establishment creation and salaried employment in the next round of EZ creation in 2004, whereby some areas already zoned for urban revitalization projects became designated as enterprise zones (moving from the second to the third tier of the zoning system of distressed areas). Using difference in differences techniques, they find that enterprise zones had only a modest effect on the creation of establishments and salaried jobs. Our study departs from theirs in two important respects. First, we focus on the creation of the first wave of enterprise zones in 1997. This enables us to measure the whole effect of the enterprise zone creation rather than just an incremental effect of the territorial policy. Secondly, we focus on the effect of the policy on local unemployment rather than on local jobs (which may partly benefit non-residents). To this end, we use individual data on unemployment rather than firm data on employment.

3. Data and descriptive statistics

We focus on the Paris region, which roughly corresponds to the Paris metropolitan area. This region of 10.9 million people is subdivided into 1300 municipalities including the 20 subdistricts of the city of Paris. These municipalities have very different population sizes that range from 225,000 residents in the most populous Parisian subdistrict to small villages located some 80 km away from the city center (Source: 1999 Census of the Population).

We use the historical file of job applicants to the National Agency for Employment ("*Agence Nationale pour l'Emploi*" or ANPE hereafter) for the Paris region. This dataset covers the large majority of unemployment spells in the region given that registration with the national employment agency is a prerequisite for unemployed workers to claim unemployment benefits in France.[5] It contains information on the exact date of an application (the very day), the unemployment duration in days, the reason for which the application came to an end, the municipality where the individual resides, and a set of socio-economic characteristics reported upon registration with the employment agency (age, gender, nationality, diploma, marital status, number of children and disabilities).

We use a flow sample of unemployment spells that started between July 1989 and June 2003. After eliminating the very few observations for which some socio-economic characteristics are missing, we are able to reconstruct 8,831,456 unemployment spells ending in the period of interest running from July 1993 to June 2003.[6] This period includes the implementation date of the enterprise zone program (January 1, 1997) and allows us to study the effect of enterprise zones not only in the short run but also in the medium run. These unemployment spells may end when the unemployed find a job, drop out of the labor force, leave unemployment for an unknown reason or when the spell is right censored. Given the focus of the paper, we will mainly study exits that end with finding a job, all other exits being treated as right-censoring in the analysis.

Regarding the geographic scale of analysis, given that enterprise zones are clusters of a significant size within or across municipalities, it would be desirable to try to detect the effect of the policy at the level of an enterprise zone as well as on neighboring areas. Nevertheless, our data does not allow us to work at this fine level of disaggregation and our approach retains municipalities as our spatial unit of analysis. Municipalities have on average twice the population of the EZ they contain. Any aggregate effect at the municipality level will measure the effect of local job creation net of within-municipality transfers.

Descriptive statistics on the number of unemployed workers at risk and the number of exits to a job are reported by semester in Table 1 for the whole region (first two columns). The number of unemployed workers at risk is nearly constant from 1993 to 1999 and then decreases before increasing again in 2001. This is consistent with a sharp decrease in the unemployment rate after 1999. The number of exits to a job does not follow exactly the same pattern as the decrease occurs sooner, in 1996. Over the whole period, the proportion of exits to a job decreases from 11.2% to 7.2%.

We also reported in Table 1 the same statistics for municipalities whose size is in the 8000–100,000 range as our working sample is restricted to that range in the policy evaluation section.[7] It contains all treated municipalities and comprises at this stage 271 municipalities (258 controls and 13 treatments). There are no noticeable differences between this restricted sample and the full sample. Roughly speaking, an average of 90,000 unemployed workers finds a job each semester and this corresponds to about 300 exits per semester in each municipality. In view of these figures, we chose semesters as the time intervals in our analysis since using shorter periods would have implied too much variability due to small sample size.

The raw data used in the evaluation of the EZ program are described in Fig. 1. This figure reports the evolution of the exit rates in the sample of treated municipalities and in three control groups: a sample composed by non-treated municipalities between 8000 and 100,000, and two subsamples of that group made of municipalities located at a distance within 5 km, or within a band of 5 to 10 km around an EZ. For readability, we drew a vertical line at semester 8 (first semester of 1997) when the policy started to be implemented. The curves for the control groups are broadly decreasing and exhibit parallel trends throughout the period. The curve for the treatment group slightly diverges from the trends observed for the control municipalities between semesters 1 and 12 (second semester of 1993 to first semester of 1999). In particular, the exit rate to a job remains flat in the treatment group between semesters 7 and 8 (second semester of 1996 and first semester of 1997) when the policy enters into effect whereas it is decreasing in the control groups. The estimation of the treatment parameter that we undertake in the remaining sections of the paper is a way of formalizing and testing that these diverging trends are statistically significant.

None of these differences appears in the evolution of exit rates to non-employment and the evolution of exit rates for unknown reasons (see our working paper, Gobillon et al., 2010).

Lastly, Fig. 2 represents the evolution of exit rates to a job, distinguishing between two groups of municipalities depending on the

[5] In the only study that we know of regarding registration with the National Agency for Employment, Blasco and Fontaine (2010) find that 61% of unemployed workers in the French Labor Force Survey report that they are registered. The authors acknowledge that the true percentage could be significantly higher given that their figure is for self-reports and definitions of unemployment in the two sources might differ. In addition, the mobility of unemployed does not seem to be a key issue as discussed in Gobillon et al. (2011).

[6] We artificially censored the few spells which lasted longer than 4 years. This is because the assumptions underlying our duration model described below are unlikely to be satisfied for very long spells.

[7] The reason for excluding the municipalities over 100,000 inhabitants is that this group includes Paris inner districts and one close neighbor, Boulogne-Billancourt, which are at no risk of being selected because of their affluence. We chose the lower bound of 8000 as it allows us to include neighbors of treated municipalities under different definitions of the control group. We do not know the identity of municipalities that applied to the program but were not selected.

Table 1
Descriptive statistics, by semester.

Year	Semester	All municipalities		Municipalities whose population is between 8000 and 100,000 in 1990	
		Nb. at risk	Exit to job	Nb. at risk	Exit to job
1993	2	1,139,991	127,748	795,570	89,404
1994	1	1,144,764	144,094	799,234	100,743
1994	2	1,201,196	140,438	837,624	98,051
1995	1	1,153,306	140,389	802,327	98,364
1995	2	1,168,106	135,768	813,158	94,885
1996	1	1,131,391	139,655	790,664	97,521
1996	2	1,171,410	123,759	818,334	86,350
1997	1	1,111,631	124,091	778,704	86,490
1997	2	1,140,782	111,852	800,008	77,843
1998	1	1,090,633	114,619	768,067	79,910
1998	2	1,122,653	102,765	791,357	71,850
1999	1	1,085,102	105,976	765,103	73,381
1999	2	1,101,209	100,188	776,471	70,061
2000	1	1,026,096	103,761	723,854	72,330
2000	2	970,200	95,736	687,451	67,035
2001	1	905,301	86,233	640,140	60,183
2001	2	936,464	76,388	661,347	53,769
2002	1	960,918	77,619	678,313	54,336
2002	2	1,061,983	79,513	747,329	55,657
2003	1	1,074,594	77,036	755,211	53,521

Nb. at risk: number of unemployed workers whose unemployment spell began within the four-year period before the beginning of the semester and who are at risk at least 1 day during the semester. Exit to job: number of unemployed workers exiting to a job during the period.

share of their population residing in the enterprise zone. The "flattening" effect between semester 7 (before treatment) and semester 8 (after treatment) which was noticeable in Fig. 1, is much more pronounced in municipalities in which the enterprise zone hosts a larger fraction of the population. As a matter of fact, rates of exit to a job even increased in those municipalities. This is suggestive of a local effect on unemployment spells that is more concentrated in EZs than in the non-EZ parts of the same municipalities.

4. The identification strategy

As our raw data consists of individual unemployment spells observed over time, we rely on a two-stage approach to measure the effect of the EZ program. In a first stage, we start by estimating semester-specific municipality effects on the propensity to find a job while netting out the economic conditions (using calendar time effects) and the effects of observed individual characteristics (gender, age, nationality, diploma, family structure, disability). These municipality effects measure the chances of finding a job for unemployed workers in each municipality during each semester over the period, all other things being equal. In a second stage, we then resort to various evaluation techniques to compare the evolution of these estimated municipality effects *before* and *after* the implementation of the policy between treated municipalities and various control groups of other municipalities.

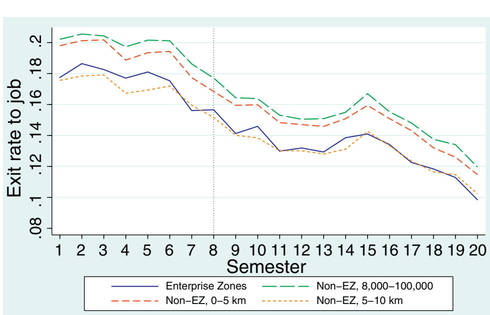

Fig. 1. Exit rate to employment, by group of municipalities. Note: The exit rates to employment are reported for semesters between 1 (2nd semester of 1993) and 20 (1st semester of 2003). Semester 8 (1st semester of 1997) is the first semester during which some municipalities are treated. Non-EZ: municipalities which do not include an EZ. 8000–100,000: population between 8000 and 100,000 in 1990. 0–X km: between 0 and X km of a municipality including an EZ. Enterprise zones: municipalities which include an EZ.

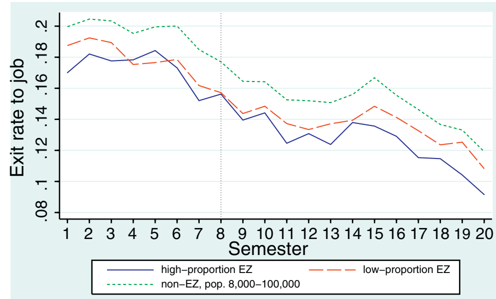

Fig. 2. Exit rate to employment, by proportion of EZ population within the municipality. Note: The exit rates to employment are reported for semesters between 1 (2nd semester of 1993) and 20 (1st semester of 2003). Semester 8 (1st semester of 1997) is the first semester during which some municipalities are treated. High-proportion EZ (resp. low-proportion EZ): municipalities including an EZ which accounts for more (resp. less) than 50% of the population of those municipalities in 1990. Non-EZ: municipalities which do not include an EZ. 8000–100,000: population between 8000 and 100,000 in 1990.

Our identification strategy for the causal effects of EZs on the propensity of unemployed workers located in treated municipalities to find a job relies on constructing data at the municipality level that measure the easiness with which residents exit from unemployment. The use of individual data in the estimation of municipality effects allows to control for municipality composition effects and to account for right censoring. Our approach aims at reducing the extent of the correlation between municipality unobservables in the trends of unemployment exits and municipality unobservables affecting selection into treatment. This in turn justifies our empirical strategy in the second stage that is based on the assumption that treated municipalities are selected on observables only. We also check that our results are robust to key issues such as the variation in the definition of control groups, a change in the periods of observation, a change in the weighting scheme or the selection of observations according to propensity scores, the inclusion of various additional variables such as entry rates or lagged endogenous variables, and finally the presence of placebo effects.

To implement this strategy, we first briefly explain how we estimate the semester-specific municipality effects and discuss the arguments underlying our definition of treatment and control groups. Then, we define our parameter of interest as the average treatment on the treated and we make explicit our identifying restrictions and our estimation strategy.

4.1. Estimating the municipality effects

We follow the approach described in the methodology paper (Gobillon et al., 2011) which extended the set-up proposed by Ridder and Tunali (1999) of Stratified Partial Likelihood Estimation (SPLE) to the estimation of unemployment duration models. It used a single flow sample and as its main empirical result, decomposed the variance of local effects explaining unemployment duration in terms of various factors such as education and nationality. In the present paper in contrast, we apply this methodology to a policy impact evaluation, relying on 20 semester specific samples to estimate 20 semester specific effects for each municipality (7 semesters before and 13 after the policy implementation).

We start from the specification of the proportional hazard model of the duration d of an unemployment spell until an exit to a job:

$$\lambda(d|X_i, j(i)) = \alpha_s^{j(i)}\theta(d)\exp(X_i\beta_s) \quad (1)$$

in which X_i are individual covariates and calendar time dummies, $j(i)$ is the municipality of residence for individual i, parameters α_s^j are semester specific municipality effects which flexibly affect the hazard function and constitute the dependent outcome of the EZ program at the evaluation stage,[8] function $\theta(d)$ is the baseline hazard function in the region, and β_s are semester-specific coefficients.

As the estimation uses a generalization of Cox Partial Likelihood, parameters β_s are directly estimated by partial likelihood methods that are tractable in spite of millions of observations and hundreds of municipality effects. The estimation proceeds by using risk sets defined in each semester. Moreover, using the Breslow estimator, one can then recover the estimates of the semester and municipality specific baseline hazard function $\alpha_s^j\theta(d)$. These estimates are further used to finally recover estimates of α_s^j or rather their logarithm, $\log(\alpha_s^j)$, which measure the propensity of unemployed workers to find a job in each municipality j in each semester s.[9]

4.2. Definitions of treatment and control groups

We estimate the effect of the EZ program using various dates *before* and *after* the creation of EZs, and using various treatment and control groups. The treatment group is composed of municipalities which comprise an enterprise zone. In robustness checks we depart from this construction and distinguish municipalities for which enterprise zones represent a large section of their population (more than 50%) from the other treated municipalities. Later on, we also modify the treatment group by including neighbors of treated municipalities.

When defining the control group, there is a potential conflict between two objectives. The first objective is to retain municipalities that are similar to those in the treatment group along various dimensions. The second objective is to avoid contamination of the effects through spatial spillovers (Blundell et al., 2004). To address the trade-off between these two objectives, we develop various empirical strategies controlling for different municipality variables and experimenting with different ways of constructing the control group.

We start by forcing the control group to comprise municipalities that are the closest to those in the treatment group in the space of characteristics and this includes neighbors of treated municipalities. Furthermore, population size has a very different support in the treatment and control groups since the non-treated group comprises many small and very small municipalities (less than 1000 inhabitants) while the smallest treated municipality has 17,500 residents. To address this issue, as already explained above, we choose to restrict the control group to municipalities whose population is between 8000 and 100,000. Moreover, we estimate propensity scores of being designated as a municipality comprising an EZ and then restrict the control group to contain only municipalities whose estimated propensity score belongs to the same support as that of treated municipalities. Note that this selection changes the definition of the treatment parameter of interest which now refers to municipalities which have ultimately been included in the working sample.

Moreover, it is important to note that the probability of treatment for a given municipality is never 0 or 1 for several reasons. First, we use municipality rather than neighborhood characteristics, second the selection indicator calculated by public authorities to select EZ was not publicly released and finally the designation process was imperfect. Since political actors had a say in the designation of enterprise zones, the selection process was only partly based on the ranking according to the aggregate indicator. It also depended on political influence and on the desire of policy makers to spread out enterprise zones throughout the region. Both reasons make it easier to find control municipalities whose characteristics are similar to those that are treated.

4.3. Identification and estimation of the policy effect

We can now turn to the definition of the impact of enterprise zones on the semester specific municipality effects α_s^j estimated in the first stage described above. These effects measure the ease with which the unemployed find a job in municipality j at semester s. We distinguish semesters before the creation of enterprise zones (i.e. between the second semester of 1993 and the second semester of 1996) that we generically denote s_0 and semesters after the creation of EZs (i.e. between the first semester of 1997 and the first semester of 2003) that we generically denote s_1. Using the notations of Rosenbaum and Rubin (1983), denote $\ln\alpha_{s_1}^j(1)$ the (logarithm of) municipality effect in the case in which municipality j is treated. It is the estimated effect in the case the municipality comprises an enterprise zone in semester s_1 and the counterfactual if the municipality does not host an enterprise zone in semester s_1. Similarly, the municipality effect is denoted $\ln\alpha_{s_1}^j(0)$ when municipality j does not contain an enterprise zone in semester s_1.

Denote $Z_{s_1}^j$ the treatment indicator, a dummy variable which indicates whether municipality j actually comprises an enterprise zone from 1997 onwards. The observed municipality effect in semester s_1 can thus be written as:

$$\ln\alpha_{s_1}^j = Z_{s_1}^j \cdot \ln\alpha_{s_1}^j(1) + \left(1 - Z_{s_1}^j\right)\ln\alpha_{s_1}^j(0).$$

The average effect of enterprise zone designation on unemployment exits in municipalities which include enterprise zones after 1997—i.e. the average treatment on the treated—is given by:

$$\delta = E\left[\ln\alpha_{s_1}^j(1) - \ln\alpha_{s_1}^j(0)\middle|Z_{s_1}^j = 1\right], \quad (2)$$

and this parameter is the object of our interest in the empirical analysis. This effect is not directly estimable since the term $E[\ln\alpha_{s_1}^j(0)|Z_{s_1}^j = 1]$ in this expression is a counterfactual (see, for instance, Imbens and Wooldridge, 2009).

To estimate parameter δ, we restrict ourselves to linear models of treatment effects given that the number of treated and control municipalities are quite small (see below). Second, simple difference in difference or within estimates of models in which municipality effects are regressed on a treatment indicator and municipality covariates are not robust to key issues (Gobillon et al., 2010). Our preferred specification is the random growth model as proposed by Heckman and Hotz (1989) in which first differences of the equation that gives the logarithm of unemployment exit rate as a function of the treatment are written as:

$$\Delta\ln{}_{\alpha}^{sj} = \delta\Delta Z_s^j + X_j\beta^* + \alpha_s^* + \Delta u_s^j \quad (3)$$

[8] What follows is a very brief description of the construction of our working samples. The full description of the procedure is detailed in Appendix available upon request and in the working paper Gobillon et al. (2010).

[9] Computing standard errors at the second stage might seem to be a tedious task. We showed however in Gobillon et al. (2011) that taking into account the estimated correlations between the estimates of semester-specific municipality effects had almost no impact at the second stage. What matters is their variance and our robust estimates take care of the multiple step nature of the procedure.

where Δ is the first difference operator, variable Z_s^j is the dummy for treatment status, X_j are some municipality characteristics (which do not vary across time in our database), u_s^j is an error term (including the sampling error on the left-hand side variable due to first-stage estimation) and parameters α_s^* denote semester dummies. Coefficient δ, as defined in Eq. (3), is the average treatment on the treated as defined in Eq. (2) if:

$$E\left(\Delta u_s^j \middle| \left\{Z_s^j\right\}_{s=1,..,T}, X_j\right) = 0, \tag{4}$$

an assumption which was exploited for instance by Heckman et al. (1997).

This set-up provides a significant weakening of the assumptions underlying difference-in-differences estimation. First, estimates are now robust to the presence of a correlation between the treatment and unobserved permanent determinants of unemployment exit rates. Second, in contrast to the similar trend assumption in difference-in-differences across treatment and control groups, the municipalities' unemployment exit rates can now be affected by heterogeneous trends although this heterogeneity depends on observables only. This approach belongs to matching difference-in-differences methods as described by Blundell and Costa-Dias (2009).

In practice, to control for municipality heterogeneity, we included in Eq. (3) the propensity score modeling the probability of being designated as an EZ instead of municipality characteristics. Using an orthogonality argument of Rosenbaum and Rubin (1983), we indeed have:

$$\begin{aligned} E\left(\Delta u_s^j \middle| \left\{Z_s^j\right\}_{s=1,..,T}, X_j\right) = 0 \Rightarrow E\left(\Delta u_s^j \middle| \left\{Z_s^j\right\}_{s=1,..,T}, X_j, p\left(X_j\right)\right) \\ = E\left(\Delta u_s^j \middle| \left\{Z_s^j\right\}_{s=1,..,T}, p\left(X_j\right)\right) = 0, \end{aligned}$$

which shows that the explanatory variables can be replaced by the propensity score $p(X_j)$ in regression (3) to reduce dimensionality, although we also experimented with the full set of variables (see Gobillon et al., 2010).

An interesting generalization of this model is to let the treatment parameter vary with time since treatment could affect exit rates in a cumulative but gradual way instead of having an immediate permanent impact or because this impact could gradually disappear as a new local labor market equilibrium stabilizes. For instance, parameter δ in Eq. (3) could be made dependent on time or more simply, the treatment in level, Z_s^j, could appear on the RHS of this equation so that the average treatment on the treated would be affected by a trend.[10] We shall test the robustness of our results to these alternatives in the empirical section.

Finally, we also used weights to account for the diversity of municipalities. A natural weight to be used is the (square root of the) number of unemployed workers in the municipality at the beginning of each semester. We also checked the robustness of the results using alternative weights such as the inverse of the estimated standard error of the estimate $\widehat{\ln \alpha_s^j}$.

5. Results of the policy evaluation

We performed the first-stage estimation by maximizing the partial likelihood function for all semesters between the second semester of 1993 and the first semester of 2003. We do not report these results here (see Gobillon et al., 2010, for more details) and rather concentrate on the results of the evaluation of the creation of enterprise zones on January 1st 1997. We first report the estimation of the propensity score at the municipality level. We then present estimates of the policy effect and provide various robustness checks.

5.1. Describing the treated municipalities: the propensity score

We now describe the municipality characteristics that determine the creation of an enterprise zone and that will allow us to construct the propensity score. We estimate a Probit model of EZ designation as a function of municipality control variables among which are measures of physical job accessibility, the municipal composition of the population in terms of nationality or education, the rate of unemployment, the proportion of young adults, and household income (proxying for the fiscal potential). We also include in the specification the smallest distance to another municipality comprising an enterprise zone. This is to account for the will of authorities to distribute enterprise zones more or less evenly throughout the region.[11] Results of weighted Probit estimations where the weights are the (square root of the) number of unemployed workers in the municipality are reported in Table 2.

The results of our benchmark weighted Probit specification in which weights are the (square root of the) number of unemployed workers in the municipality appear in the first column of Table 2 although less parsimonious specifications were also estimated (see the notes below this table). Unweighted estimation results are very similar (column 3).

In line with the selection criteria, the larger the average household income in the municipality or the smaller the proportion of persons without a high school diploma in the municipality, the less likely the municipality comprises an enterprise zone although the latter effect is hardly significant. The higher the proportion of individuals below 25 years of age or the larger the size of the population, the larger the probability that the municipality contains an enterprise zone. In terms of distance, the larger the distance to a designated municipality or the larger the density of jobs attainable in less than 60 min by private vehicle, the less likely it is that the municipality will be endowed with an enterprise zone. This is consistent with the targeting of places with relatively lower job accessibility. The distance to the nearest EZ is not significant. In line with Hanson (2009), we also experimented with political variables which are the frequencies of votes for political parties. Even though municipalities whose townhalls were administered by politicians belonging to the governing party at the time of EZ designation were more likely to be selected, the effect is not significant and we chose not to include these variables in the final specification.

We also experimented with an alternative whose results are reported in the second column of Table 2. We included a variable equal to the endogenous outcome (i.e. the municipality effects) averaged over semesters prior to policy implementation. The effect is positive although it is at the limit of significance. This means that municipalities chosen to include an enterprise zone are also those where it is easier for unemployed workers to find a job holding constant the characteristics that explain the treatment. This is a standard result in the evaluation literature where governments often intervene to "pick winners" (Boarnet and Bogart, 1996).

Using the results in the first column, we predict the propensity score for each municipality. Interestingly, it reveals that the supports of the predicted propensity scores in the treated and control groups quite markedly differ as shown in Table 3.

The smallest predicted probability in the treatment group is equal to 0.08%, a low score which is consistent with political tampering in designation. In order to satisfy the common support condition (Smith and Todd, 2005), we further restrict the control group to municipalities whose predicted propensity scores are larger than the value 0.04% (see Table 3). This restriction shrinks the control group by a factor of 2 and it now includes 135 municipalities (instead of 258), which is about ten

[10] We thank the editor for this suggestion.

[11] We assessed endogeneity issues by experimenting with the second-lowest distance as an instrument. It hardly affected results.

Table 2
Propensity score: effect of municipality characteristics on the probability of designation of an enterprise zone.

		Inclusion of past municipality effect	No weights
Job density, 60 min by private vehicle	−3.999*	−3.357	−4.171*
	(2.109)	(2.260)	(2.298)
Proportion of no diploma	37.779*	33.447	24.029
	(22.249)	(23.998)	(22.865)
Proportion of technical diplomas	20.998	5.860	0.974
	(28.215)	(31.527)	(28.900)
Proportion of college diplomas	38.978	27.180	17.299
	(29.889)	(32.809)	(31.336)
Distance to the nearest EZ	−0.027	−0.033	−0.035
	(0.024)	(0.025)	(0.024)
Proportion of individuals below 25 in 1990	17.125***	14.890***	11.834**
	(5.156)	(5.320)	(5.256)
Population in 1990	0.021**	0.022**	0.019*
	(0.009)	(0.009)	(0.011)
Average net household income in 96	−4.975***	−5.140***	−2.033
	(1.563)	(1.636)	(1.593)
Past municipality effect in exit to job		4.014*	
		(2.323)	
Constant	−32.115	−1.447	−16.526
	(21.818)	(29.243)	(22.537)
Nb. observations	271	271	271
Pseudo-R2	.542	.561	.477

Note: The specification is a probit model with the dependent variable being a dummy equal to one if the municipality is designated to receive an EZ (and zero otherwise). The sample is restricted to municipalities with a population between 8000 and 100,000 in 1990. The first and second columns are weighted by the square root of the number of unemployed workers at risk at the beginning of period 8 (1st semester of 1997), and the third column is not weighted. Past municipality effect refers to the average of municipality effects in previous semesters, as estimated in the 1st stage of SPLE (the specification being given by Eq. 1). We also used alternative specifications including in the set of explanatory variables, for instance: the job density within a 60 minutes radius by public transport, the unemployment rate in 1990, the proportions of Europeans (French excluded), North Africans, SubSaharan Africans and other nationalities. The estimated coefficients were not significant and a Chi-square test did not reject the absence of joint significance. Consequently, we dropped these variables from the specification.

*** Significant at 1% level.

** Significant at 5% level.

* Significant at 10% level.

times the number of treated municipalities (13). We will later test the robustness of our results to various levels of restrictive selections.

Using this allocation, we computed the averages of explanatory variables in the treatment and control groups to assess whether those groups are balanced and we report these averages in Table 4.

Since the treatment group is small, it seems difficult to report these averages in strata defined by the propensity score levels (Smith and Todd, 2005). We rather report them globally even if results are less easy to interpret. The covariates of interest seem to be balanced in the two sub-samples except for two variables: the proportion of college graduates and household income. This explains the differences in the propensity score averages between the control and treatment groups. Nevertheless, the coefficient of designated municipalities in linear regressions of those covariates on the propensity score and the designation indicator is not significant even at the 10% level which indicates that samples are approximately balanced.

Table 3
Frequency of non-treated municipalities by propensity score bracket.

Score bracket	Number of non-treated municipalities
[0, .0008)	125
[.0008, .0119)	60
[.0119, .1161)	52
[.1161, .1772)	7
[.1772, .3111)	6
[.3111, .4404)	4
[.4404, .4765)	0
[.4765, .6091)	2
[.6091, .7723)	2
[.7723, .7933)	0
[.7933, .8537)	0
[.8537, .9032)	0
[.9032, .9949)	0
[.9949, 1]	0
Total	258

Note: The observation unit is a municipality between 8000 and 100,000 inhabitants. The propensity score is computed as the predicted probability of a municipality to be designated, the predicted probability being obtained from a probit model which estimated coefficients are reported in Table 2, column (1). Each bracket bound corresponds to the propensity score of a treated municipality, where treated municipalities have been sorted by propensity score in ascending order.

Table 4
Average of municipality characteristics in treatment and control groups.

	Treatment group	Control group, propensity score > .005
Job density, 60 min by public transport	.838	.850
	(.119)	(.119)
Proportion of no diploma	.536	.465
	(.041)	(.074)
Proportion of technical diplomas	.222	.219
	(.009)	(.031)
Proportion of college diplomas	.122	.179
	(.025)	(.075)
Distance to the nearest EZ	9.074	11.016
	(12.193)	(8.051)
Proportion of individuals below 25 in 1990	.416	.372
	(.038)	(.043)
Population in 1990	45.201	43.578
	(18.226)	(26.357)
Average net household income in 96	.375	.509
	(.087)	(.125)
Number of observations	13	135

Note: The observation unit is a municipality between 8000 and 100,000 inhabitants. Only municipalities with propensity score above .0004 are considered in the control group. The standard errors are reported in parenthesis under the means. The propensity score is computed using Table 2, column (1).

Table 5
Effect of treatment on the logarithm of raw rates of entry and exit.

Dependent variable	Entry rate into unemployment	Exit rate to job	Exit rate to non-employment	Exit rate to unknown
EZ treatment effect	.011	.040***	.039**	.013
	(.021)	(.015)	(.024)	(.014)
Propensity score	−.077***	−.009***	−.007*	.001
	(.018)	(.003)	(.004)	(.004)
Nb observations	1628	1628	1628	1628

Note: The entry rate (resp. an exit rate) is defined as the number of unemployed workers entering (resp. exiting) during a given semester divided by the number of unemployed workers at the beginning of the period. The entry and exit rates are regressed in first difference on the EZ treatment dummy, the propensity score and year∗semester dummies (which are not reported here). We only keep semesters between 1 (2nd semester of 1993) and 12 (1st semester of 1999). The reported number of observations corresponds to first-difference observations and is thus equal to $(12-1)*149=1628$ observations. Estimation method: FGLS with a constant within-municipality unrestricted covariance matrix.
*** Significant at 1% level.
** Significant at 5% level.
* Significant at 10% level.

5.2. The evaluation of the policy

A useful benchmark for our evaluation is the estimated treatment effect obtained when using as outcome variable the raw entry rates into unemployment as in Papke (1994) and the three raw exit rates from unemployment (i.e. the rates of exit to a job, to non-employment or to an unknown reason) that we are able to construct from our data. The entry rate (resp. an exit rate) is defined as the number of unemployed workers entering (resp. exiting) in a given semester divided by the number of unemployed workers at the beginning of the period. The results using raw rates should be compared with those obtained when applying our more sophisticated method that purges exit rates to a job from individual characteristics and takes into account the usual censorships that affect unemployment data. This is a useful benchmark since policy analysts often resort to raw rates for policy evaluation. Table 5 reports the estimation results of the random growth Eq. (3) using *raw rates* correcting for the within-municipality autocorrelation of shocks between semesters by FGLS using a constant unrestricted within-municipality covariance matrix.

In column 1, the parameter which measures the effect of the treatment on the log-entry rates in unemployment is not significantly different from zero. Column 2 reports the effect of the treatment on the log-exit rates out of unemployment to a job, our parameter of interest. It is significantly positive and equal to .040. The other raw exit rates are not significantly affected by the treatment and this will be commented later on.

Using the same estimation method as in the benchmark, Table 6 reports our main estimation results using the semester specific municipality effects purged from observed individual heterogeneity in the first stage as explained in Section 4.1. We present results that we obtain when varying the range of semesters used in the estimations.[12]

The first column reports the results of our preferred specification since this specification is robust to various changes in the underlying construction and seems to be a conservative estimate. The estimated treatment parameter is equal to .031 and is significant at the 5% level. This effect is quite small since it implies that the rate of exit to a job increased by a meager 3% when the policy was implemented. Given that there are roughly 300 exits each semester on average in a municipality in the considered range of population size, the policy amounts to generating about 10 new exits per semester only. This estimate is slightly lower but comparable to the benchmark using raw rates. This small effect can probably be interpreted as an indication that job reallocation within municipalities may be relatively large, that there is possibly little substitution of labor to capital, and that any possibly generated agglomeration effects are not favorable to hiring.

In the second column we further restrict the period of evaluation, keeping only two semesters before the reform and two semesters after the reform. The estimate remains significant and stands at .042. If we further restrict the analysis to the period at which the reform was implemented, the estimate is equal to .035 although it becomes insignificant, probably because of the smaller number of observations.[13] These results show no evidence that treatment effects are variable over time and indicate that the very short run effects (the semester after treatment) are similar to the medium term effects (3 years after treatment). We also tested that there is no significant linear trend in the effect of treatment until semester 13. As already mentioned however, treatment effects become undistinguishable from zero after semester 13 although we do not know if this result is substantial or denotes a failure of restriction (4) because trends between treatments and controls are heterogeneous in a way that is not observed. In particular, we cannot fully condition out all time variation sources since we do not have variables that are time varying.

Interestingly, we can distinguish between treated municipalities according to the proportion of the municipality population which resides within the enterprise zone. Specifically, we included in our preferred specification (column 1) an indicator that the proportion of the population living in the enterprise zone in the treated municipality is below 50%. The result is striking since the treatment parameter estimate is now equal to .057 instead of .031 and is significant at a 1% level while the treatment effect in municipalities in which a small proportion of the population lives in an enterprise zone is also positive (.016 = .057–.041) but becomes insignificant. The dilution of the effect will be confirmed below when changing the treatment definition. It indicates that the effect of the policy is very localized with probably little spillover outside the EZ, an issue that we further investigate below.

Finally, we tested for spatial correlation and its pattern is very irregular and certainly not significant beyond 10 km. Since correcting standard errors for the presence of random effects at the level of the "département" (county equivalent) had a marginal impact, we chose to neglect these corrections.

5.3. Spillover effects and changes in treatment and control groups

We now investigate the possibility of spatial spillovers on neighboring municipalities, which may be either positive or negative as mentioned earlier. We began with changing the composition of the control group. We selected municipalities in the control group depending on their distance to a treated municipality. We used "as-the-crow-flies" distance between municipality centers and experimented with three distance thresholds at 5, 10 and 15 km. We first restricted the previous control group to municipalities whose center is *farther* than 5 km of the center of a treated municipality (respectively 10 and 15 km). Second, we restricted the control group to municipalities whose center is *within* 5 km of the center of a treated municipality (respectively 10 and 15). Table 7 reports the corresponding results.

The evidence of spillover effects to neighboring municipalities is weak. In all but one of these experiments, the estimates of the treatment parameter remain around .03 and their standard errors remain constant. The only case in which the estimate becomes hardly distinguishable from zero is when the control group is restricted to municipalities outside the 15 km range of a treated municipality. In our opinion, however, the assumption (Eq. (4)) that these municipalities are experimenting the same trends in unemployment as the treated municipalities becomes

[12] We do not report the estimated semester effects which reproduce closely the raw trends in the data. Nor do we correct standard errors for the replacement of the true propensity score by an estimator which usually marginally affects standard errors.

[13] The treatment variable is very much correlated with the propensity score and when we omit the latter, the estimate increases to .058 and is significant at the 1% level.

Table 6
Effect of designation and treatment on semester-specific municipality effects.

	Periods: 1/2 to 11/12	Periods: 4/5 to 8/9	Period: 7/8	Period: 7/8	Periods: 1/2 to 11/12 Specific effect for small-proportion EZ
EZ treatment effect	.031**	.042**	.035	.058***	.057***
	(.014)	(.019)	(.025)	(.019)	(.016)
EZ treatment effect∗small-proportion EZ	−.041**				
					(.018)
Propensity score	−.008*	−.021*	.049		−.007*
	(.004)	(.012)	(.039)		(.004)
Nb observations	1628	592	148	148	1628

Note: We conduct robustness checks to changes of semesters and assess the impact of introducing a specific effect for EZ with a small proportion of the population in the municipality. Semester-specific municipality effects are regressed in first difference on an EZ treatment dummy, the propensity score, year∗semester dummies (which are not reported here) and, in the last column only, an EZ treatment dummy interacted with a dummy for the EZ accounting for less than 50% of the population of the municipalities where the EZ is located (referred to as "small-proportion EZ" in the table). In the table, $t/t+1$ designates the first difference between semester t and semester $t+1$. The first row of the table involves 1/2 (2nd semester of 1993/1st semester of 1994), 4/5 (1st/2nd semester of 1995), 7/8 (2nd semester of 1996/1st semester of 1997), 11/12 (2nd semester of 1998/1st semester of 1999). The number of observations in first difference is reported in the last row of the table. Estimation method: FGLS with a constant within-municipality unrestricted covariance matrix.
*** Significant at 1% level.
** Significant at 5% level.
* Significant at 10% level.

unsustainable since labor market conditions in distant municipalities are likely to be different. These various estimations also confirm that spatial correlation should not be an important concern since standard errors are in most cases not affected by these variants.

We also experimented with changes in the definition of the treatment group. Instead of retaining the municipalities comprising an enterprise zone only, we also retained their neighbors at a distance of less than 2 km (respectively at a distance of less than 3 km). The number of potentially treated municipalities increases from 13 to 24 treated municipalities (respectively 51). Table 8 reports the corresponding results. It is striking that in both cases the estimated treatment parameter value drops by 2/3 and is no longer significantly different from zero. It confirms that the creation of an enterprise zone has a very localized effect on the unemployment exit rate to a job and has no significant positive spillover effects on neighboring municipalities.

5.4. Other robustness checks

We also performed other robustness checks of our results. First, we modified the whole procedure so as to consider in the estimation of the propensity score the role of the before treatment average of the endogenous variable (as in the second column of Table 2). Second, we varied the municipality-and-semester specific weights that we used in the estimation. Instead of using the square root of the number of unemployed workers in the municipality at the beginning of the semester, we either used the inverse standard errors of the estimates of the left-hand side variable as provided by the first-stage estimates or no weights at all. These results are available in Gobillon et al. (2010) and are hardly different from those obtained for the main specification and, if anything, estimates of the treatment parameter become larger.

To evaluate the implications of our support assumptions, we change the lower threshold for inclusion of municipalities in the control group according to their estimated propensity score. We repeated the experiment and varied this (lower) threshold from 0.005 to 0.15 without affecting the magnitude of the estimated coefficient by much. We also experimented with modifying the treatment group by leaving one treated municipality out and re-running 13 different estimations. There is one outlier in this experiment in the sense that if we leave this municipality out the estimated effect becomes larger (.043) and very significant. It is true that leaving out the largest propensity scores municipalities (see Table 3) can decrease by 15% the estimated effect but it is not systematic. Keeping the 13 treated municipalities in the sample is thus reasonable and seems to give a somewhat conservative estimate.

Another issue is that although the construction of the semester-specific municipality effects purges exit rates to jobs from individual characteristics, it does a poor job at controlling for entry effects because of identification issues. We included yearly and monthly dummies in the first stage estimation even though identification of these parameters could be fragile. To address the issue, we re-estimated our preferred specification (see first column of Table 6) controlling for semester and municipality specific entry rates. Although this variable has a significant positive effect, the estimate of the treatment effect is hardly affected.

Our estimates might also reflect that some firms delayed hiring during the last semester of 1996 in order to benefit from the policy in the following semester. As suggested by Manning and Pischke (2006) to measure placebo effects as well, we included in the specification an indicator for the lagged treatment effect. If the policy is anticipated and employers delay hiring decisions, a negative effect could be observed. The lagged treatment effect is found to be not significantly

Table 7
The effect of designation and treatment on semester-specific municipality effects, robustness to changes in the definition of the control group.

	Control group: no municipality within 5 km of EZ	Control group: no municipality within 10 km of EZ	Control group: no municipality within 15 km of EZ	Control group: only municipalities within 5 km of EZ	Control group: only municipalities within 10 km of EZ	Control group: only municipalities within 15 km of EZ
EZ treatment effect	.033**	.036*	−.002	.037***	.029*	.028*
	(.015)	(.019)	(.052)	(.014)	(.015)	(.014)
Propensity score	−.008	−.005	−.008	−.012***	−.012***	−.007*
	(.005)	(.006)	(.009)	(.003)	(.004)	(.004)
Nb observations	1133	737	462	638	1034	1309

Note: Semester-specific municipality effects are regressed in first difference on an EZ treatment dummy, the propensity score and year∗semester dummies (which are not reported here). We only keep semesters between 1 (2nd semester of 1993) and 12 (1st semester of 1999). The number of observations in first difference is reported in the last row of the table. Estimation method: FGLS with a constant within-municipality unrestricted covariance matrix.
*** Significant at 1% level.
** Significant at 5% level.
* Significant at 10% level.

Table 8
The effect of designation and treatment on semester-specific municipality effects, robustness to changes in the specification of the treatment group.

	Treatment group: municipalities with an EZ	Treatment group: including municipalities less than 2 km of an EZ	Treatment group: including municipalities less than 3 km of an EZ
EZ treatment effect	.031**	.010***	.009
	(.014)	(.012)	(.010)
Propensity score	−.008*	−.003	−.001
	(.004)	(.004)	(.004)
Nb observations	1628	1947	1881

Note: Semester-specific municipality effects are regressed in first difference on an EZ treatment dummy, the propensity score and year∗semester dummies (which are not reported here). We only keep semesters between 1 (2nd semester of 1993) and 12 (1st semester of 1999). The number of observations in first difference is reported in the last row of the table. Estimation method: FGLS with a constant within-municipality unrestricted covariance matrix. "Municipalities with an EZ" corresponds to our baseline treatment group and includes 13 municipalities. There are 24 municipalities within 2 km of an EZ and 51 municipalities within 3 km of an EZ.
*** Significant at 1% level.
** Significant at 5% level.
* Significant at 10% level.

different from zero, suggesting no such behavior, and its inclusion does not affect the estimated treatment parameter.

Evidence gathered in Table 5 runs against an argument advanced by Elvery (2009) about indirect effects of employment zones. The local labor market in treated municipalities would become more attractive after the creation of an enterprise zone and non-employed persons would be encouraged to search for a job. This would increase the entry rate into unemployment and the competition for jobs among the unemployed. This probably does not happen with the French EZ program since we do not find that the treatment parameter is affected by entry rates or that entry rates change because of the program.

Finally, the estimates of the treatment parameter for exits to non-employment and exits for unknown reasons reported in Table 5 are not significantly different from zero although the estimate for exits to non-employment is quite large at the same level .039. The result that exits for unknown reasons are not affected by the policy is important for our identification strategy. Our treatment parameter using information on reported exits to a job only would indeed be biased if exits to a job were concealed among the exits for unknown reasons in a way that varies between treated and control municipalities.

6. Conclusion and policy discussion

In this paper, we conducted an evaluation of the impact of the creation of enterprise zones on the propensity of unemployed workers to find a job. Contrary to the previous literature which usually focuses on employment growth or on the local creation of firms, our choice of outcome of interest was motivated by the fact that a main objective of the policy had indeed been to help locals move out of unemployment (and not just to create or displace jobs which may only have an indirect effect on the local population). This evaluation was carried out for the Paris region, using an exhaustive dataset on job applicants registered at the French National Unemployment Agency, and resorting to a varied toolkit of statistical methods. We assessed whether unemployed workers in municipalities with a newly created enterprise zone improved their chances of finding a job compared with unemployed workers living in similar municipalities but where no enterprise zone was created.

Our main results are threefold. Firstly, in line with several studies on enterprise zones, we showed that zone designation tended to favor municipalities with favorable unobserved characteristics. This is not surprising given that policy makers usually tend to select places that are more likely to carry success or choose places that gather prior favorable conditions for economic development. Secondly, we found that the French EZ program had a small positive impact, which is consistent with previous work on the number of local establishments in enterprise zones (Rathelot and Sillard, 2009).

The policy had a short-run impact on the ease with which the local unemployed workers move out of unemployment. This result is robust to a variety of specifications and robustness checks and is broadly in line with the previous works in the US that found that enterprise zones had an impact on employment (Papke, 1994; Lynch and Zax, 2008; Ham et al., 2011), although in our case it is rather small. On the other hand, our result contrasts with previous papers which found that it had no impact on employment (Boarnet and Bogart, 1996; Bondonio and Engberg, 2000; Neumark and Kolko, 2010). Lastly, we find that the effect is very localized. It may capture the direct consequence of the tax rebates on the hiring of local workers in the absence of any spatial spillover.

In each municipality in our sample, while on average about 300 unemployed workers find a job every semester, enterprise zones only help an additional group of 10 workers to find a job over the same duration. It could be argued that this figure represents a lower bound of the effect of tax exemptions since out-of-the-labor-force residents may also have reacted to these new opportunities. Because of missing information, some exits to a job may also have been attributed to other types of exits from unemployment.

However, even if the true impact on job creations benefitting enterprise zone residents is substantially larger than the direct effect on exits from unemployment, the overall impact is likely to be moderate. It is also likely to be small in comparison with the huge cost associated with the policy. In 1997, the first year of program implementation, it is estimated that the tax reliefs associated with the policy amounted to 123 million for the whole of France. The wage tax exemption amounted to 59 million (48% of the total of tax reliefs) and benefited to 26,000 jobs throughout the country. However, 6000 of these jobs only were held by residents of enterprise zones (DIV, 2001). This means that for each job held by an enterprise zone resident, almost 10,000 were granted in wage tax exemptions, and in some case for workers who were already employed before the start of the policy. A fortiori, the cost associated with the new hire of an enterprise zone resident is thus greater. This argues in favor of designing possibly better targeted, more integrated and more cost-effective policies that operate beyond the sole stimulation of labor demand.

Appendix A. Supplementary data

Supplementary data to this article can be found online at http://dx.doi.org/10.1016/j.jpubeco.2012.06.003.

References

André, P., 2002. Rapport d'information fait au nom de la commission des Affaires économiques et du plan sur les zones franches urbaines. N°354, Sénat, Session extraordinaire de 2001–2002.
Blasco, S., Fontaine, F., 2010. Etudier le non-recours à l'assurance chômage. Revue Economique 61 (5), 933–943.
Blundell, R., Costa-Dias, M., 2009. Alternative approaches to evaluation in empirical microeconomics. Journal of Human Resources 44 (3), 565–640 (Summer).
Blundell, R., Costa-Dias, M., Meghir, C., van Reenen, J., 2004. Evaluating the employment impact of a mandatory job search assistance program. Journal of the European Economic Association 2 (4), 596–606.
Boarnet, M., Bogart, W., 1996. Enterprise zones and employment: evidence from New Jersey. Journal of Urban Economics 40, 198–215.
Bondonio, D., Engberg, J., 2000. Enterprise zones and local employment: evidence from the states' programs. Regional Science and Urban Economics 30, 519–549.
Bondonio, D., Greenbaum, R., 2007. Do local tax incentives affect economic growth? What mean impacts miss in the analysis of enterprise zone policies. Regional Science and Urban Economics 37, 121–136.
Busso, M., Kline, P., 2008. Do local economic development programs work? Evidence from the federal empowerment zone program. Yale Economics Department Working Paper 36.
DIV - Ministre Délégué à la Ville, 2001. Bilan des Zones franches urbaines. Rapport au Parlement.

DIV - Ministre Délégué à la Ville, 2004. Observatoire National des Zones Urbaines Sensibles, Rapport 2004.
Elvery, J., 2009. The impact of enterprise zones on resident employment: an evaluation of the enterprise zone programs of California and Florida. Economic Development Quarterly 23 (1), 44–59.
Ernst, E., 2008. L'activité économique dans les zones franches urbaines. INSEE Première 1187, 1–4.
Gilli, F., 2006. Enterprises et développement urbain : les zones franches ont-elles rempli leur mission? In: de Boissieu, Deneuve (Ed.), Les Entreprises Françaises en 2006: Economica, pp. 163–187. chapter 10.
Gobillon, L., Magnac, T., Selod, H., 2010. "Do Unemployed Workers Benefit from Enterprise Zones? The French Experience", CEPR WP 6199.
Gobillon, L., Magnac, T., Selod, H., 2011. The effect of location on finding a job in the Paris region. Journal of Applied Econometrics 26 (7), 1079–1112.
Ham, J., C.W. Swenson, A. Imrohoroglu and H.Song, 2011. "*Government programs can improve local labor markets: evidence from state enterprise zones, federal empowerment zones and federal enterprise communities*", Journal of Public Economics, 95 (7-8), 779–797.
Hanson, A., 2009. Local employment, poverty, and property value effects of geographically-targeted tax incentives: an instrumental variables approach. Regional Science and Urban Economics 39, 721–731.
Heckman, J.J., Hotz, V.J., 1989. Choosing among alternative nonexperimental methods for estimating the impact of social programs: the case of manpower training. Journal of the American Statistical Association 84, 862–874.
Heckman, J., Ichimura, H., Todd, P., 1997. Matching as an econometric evaluation estimator: evidence from evaluating a job training programme. Review of Economic Studies 64, 605–654.
Hirasuna, D., Michael, J., 2005. Enterprise zones: a review of the economic theory and empirical evidence. Policy Brief – Minnesota House of Representatives – Research Department.
Imbens, G., Wooldridge, J., 2009. What's new in econometrics. NBER.
Lynch D. and J. Zax (2008), "*Incidence and substitution in enterprise zone programs: the case of Colorado*", unpublished manuscript.
Manning, A., Pischke, J.-S., 2006. Comprehensive versus selective schooling in England in Wales: what do we know? CEPR Discussion Paper No. 5653.
Ministère délégué à la ville et à la rénovation urbaine, 2002. Rapport au Parlement. Bilan des Zones Franches Urbaines. 61 pages.
Neumark, D., Kolko, J., 2010. Do enterprise zones create jobs? Evidence from California's enterprise zone program. Journal of Urban Economics 68, 1–19.
O'Keefe, S., 2004. Job creation in California's enterprise zones: a comparison using a propensity score matching model. Journal of Urban Economics 55, 131–150.
Papke, L., 1994. Tax policy and urban development. Evidence from the Indiana enterprise zone program. Journal of Public Economics 54, 37–49.
Peters, A., Fisher, P., 2004. The failures of economic development incentives. Journal of the American Planning Association 70, 27–37.
Rathelot, R., Sillard, P., 2009. Zones Franches Urbaines : quels effets sur l'emploi salarié et les créations d'établissements? Economie et Statistique 415–416, 81–96.
Ridder, G., Tunali, I., 1999. Stratified partial likelihood estimation. Journal of Econometrics 92 (2), 193–232.
Rosenbaum, P., Rubin, D., 1983. The central role of the propensity score in observational studies for causal effects. Biometrika 70, 41–55.
Sidibé, M., (2011), "*Equilibrium effects of local tax holiday policies in the presence of search and spatial frictions*", unpublished manuscript.
Smith, J.A., Todd, P., 2005. Does matching overcome LaLonde's critique of nonexperimental estimators. Journal of Econometrics 125, 305–353.
Thélot, H., 2004. Les embauches en zone franche urbaine en 2002. Premières Informations, Premières Synthèses, N°35.1.