TEPP WORKING PAPER

N° 2015 - 06

Spatial mismatch through local public employment agencies
Answers from a French quasi-experiment

MATHIEU BUNEL, ELISABETH TOVAR

www.tepp.eu

TEPP - Travail, Emploi et Politiques Publiques - FR CNRS 3435

Spatial Mismatch through Local Public Employment Agencies?

Answers from a French Quasi-Experiment

Mathieu Bunel* and Elisabeth Tovar**

October 2015

Abstract

Using the unanticipated creation of a new agency in the French region of Lyon as a quasi-natural experiment, we question whether distance to local public employment agencies (LPEAs) is a new channel for spatial mismatch. Contrary to past evidence based on aggregated data and consistently with the spatial mismatch literature, we find no evidence of a worker/agency spatial mismatch, which pleads for a resizing of the French LPEA network. However, echoing with the literature on the institutional determinants of the local public employment agencies' efficiency, we do find detrimental institutional transitory effects.

Keywords: spatial mismatch, unemployment, public employment service, quasi-experiment

JEL codes: C218, J58, R53

The authors would like to thank Mohamed Hilal for providing information distance data and the Agglomeration of Lyon for giving us access to regional exhaustive data. We would also to thank the participants of the TEPP conference (2015). As usual, the authors remain responsible for any mistakes and inaccuracies presented in this paper.

* corresponding author, Université de Bourgogne, LEDi (UMR CNRS 6307) and TEPP (FR-3126) mathieu.bunel@u-bourgogne.fr

** Université Paris Ouest, EconomiX (UMR CNRS 7235) and TEPP (FR-3126) etovar@u-paris10.fr

1 Introduction

In many countries the unemployment rates that soared after the 2008 financial crisis are still unprecedentedly high: 10.9% in the Euro zone, 22.2% in Spain, 9.5% in Ireland, 10.4% in France, 12.0% in Italy, 9.7% in Finland (Eurostat data for July, 2015). In France, in particular, between January 2009 and January 2015, the number of jobseekers grew from 3.9 to 6.2 million (i.e. a 58% increase) while the number of completely unemployed jobseekers increased by 52%. At the same time, the average unemployment spell progressed from 390 to 542 days and the proportion of long-term jobseekers escalated from 30.3 to 43.3% (Cour des Comptes, 2015).

In parallel, an extensive literature show the mostly positive effects of the active labour market public policies[1] implemented since the 1990s in OECD countries[2]: Dolton and O'Neill, 2002; Blundell et al., 2004; van der Berg and van der Klaauw, 2006; Graversen *and al.*, 2008a and 2008b; Heckman et al., 1999; Bell and Orr, 2002; Boone and van Ours, 2009; Borldand and Tseng, 2011; Graversen and van Ours, 2011, Graversen and Larsen, 2013 and Bernhard and Kopf, 2014. On the French context, evidence was also found that the active labour market public policies are effective: Crépon et al., 2005; Behaghel et al., 2009; Fougère et al., 2010; Fontaine and Le Barbanchon, 2012 and Crépon et al., 2013.

In this context, the role of local public employment agencies (LPEAs) on job matching efficiency has received an increased attention in recent empirical literature.

First, some papers question the caseworkers' marginal efficiency: on Dutch data, Koning (2009) finds that each additional marginal caseworker significantly increases the unemployment outflow rates for short-term jobseekers, reduce the inflow rate into social assistance protocols and increases the number of registered vacancies by agency. Although these effects are modest in absolute terms, he concludes that raising the number of caseworkers is cost-effective, and that extra costs are compensated by the resulting reduction in assistance benefits expenses. On Swedish data, Lagerstöm (2011) also shows that, when controlling for the jobseekers' characteristics, caseworkers have a significant role in the jobseekers' employment rates and future earnings.

Second, other papers focus on understanding the causes of the heterogeneous efficiency of the intermediation service provided by LPEAs (Rosholm, 2014). In this respect, two main dimensions are investigated: 1) institutional effects and 2) geographical spatial mismatch effects.

Institutional effects such as heterogeneous caseload congestion between agencies (Hainmueller *et al*., 2011), caseworker strategies (Behncke *et al*., 2010a; Lagerstöm, 2011; Bech, 2015) and social proximity with her clients (Behncke *et al*., 2010b), the managerial governance of agencies (Hill, 2006) or a residual effect resulting from a combination of these factors (Suárez Cano *et al*. 2015) have a

[1] Active labour market policies focus on affecting the behaviour of jobseekers to improve their job search efficiency and/or their employability. They furthermore involve a "mutual obligations" principle, where jobseeker benefits are keyed to their compliance to the active programs, with possible temporary benefit suspensions and/or exclusions (OCDE, 2007). This paper is not focused on the evaluation of active labour market policies. For the latest literature reviews on these issues, see for example Martin and Grubb, 2001; Card, Kluve and Weber, 2010; Kluve, 2010; Fougère *and al*., 2010; Parent, Sautory and Desplatz, 2013; Fontaine and Malherbet, 2013; or Biewen and al., 2014.

[2] Other papers have worked on the theoretical mechanisms though which public employment services can impact on the quality of the job matching process. For example, after Boone and van Ours (2009) and Plesca (2006), Fougères and al. (2009) propose a structural search model with fixed and variable costs of search in which unemployed workers select their optimal search intensity given the exogenous arrival rate of job contacts coming from the public employment agency. More recently, Caliendo et al. (2015) show that the jobseekers' search effort and success in finding a new job are affected by the subjective beliefs on the relative impact on the job search process of one's search effort vs. of external factors, such as, in particular, the perceived efficiency of the local public employment agencies.

significant influence on the employment prospects of the jobseekers. Launoy and Wälde (2015) show that organizing the work of a LPEA in a more efficient way has much better result on unemployment than creating pecuniary incentives through unemployment assistance benefits.

In parallel, a growing number of paper questions the effects on unemployment of the geographical distance between jobseekers and LPEAs and show that the spatial distribution of local public good providers (and, in particular, LPEAs) does not match the distribution of these public goods recipients (Allard and Danziger, 2003; Joassart-Marcelli and Wolch, 2003; Bielefeld and Murdoch, 2004; Joassart-Marcelli and Giordano, 2006; Allard, 2009; Suárez Cano *et al*. 2012a, 2012b, 2015, Wathen and Allard, 2014).

This question of the effect of the accessibility to LPEAs on unemployment is relevant in two regards.

First, from a public policy perspective, the link between distance to LPEAs and unemployment tends to support the preservation of a dense spatial network of LPEAs. In a context of scarce public spending, the cost of this network has recently been questioned. In the French context, the annual rent cost of maintaining the network of 900[3] public employment agency network now exceeds 250 million euros (Cour des Comptes, 2015. Maintaining a dense local network is also a source of organizational deleterious effects, hampering, for example the specialization of caseworkers. Thi is particularly the case in France where 25.3% of the agencies have 15 caseworkers of less; 71.0 have 25 caseworkers of less (Le Monde, 2013). In public policy terms, examining whether distance to LPEAs affects the jobseekers' employment prospects is relevant because it conditions the choice between to alternative, equalitarian vs. Rawlsian, policy orientations. In the egalitarian scenario, equal accessibility to the public placement service is guaranteed to all jobseekers by financing a dense network of LPEAs. In the Rawlsian option, spatial accessibility differentials to LPEAs are tolerated but compensating schemes are put in place for the jobseekers with the poorer accessibility to the agencies' network (payment of transportation costs, extra monitoring through Internet meetings…).

Second, from a theoretical perspective, finding an effect of the jobseeker/agency distance on unemployment suggests a new kind suboptimal friction to the matching process and creating a new source of Spatial Mismatch (Kain, 1968; Gobillon *et al*., 2007).

In this paper, we rely on French exhaustive administrative geo-located data on both jobseekers and LPEAs location and characteristics to question this issue. Measuring the effect of distance to LPEAs on unemployment has methodological pitfalls due to the potentially endogeneity of the distance variable for two reasons. First, the agencies are not spatially randomly distributed. Second, in most datasets the true distance between agencies and jobseekers is affected by a measurement error bias: jobseekers are arbitrarily assigned to centroid of their census tract. To deal with these methodological problems, we take advantage of a quasi-natural experiment with the installation of a new agency in the French region of Lyon.

The rest of the paper is organized as follows: in Section 2, we discuss the literature. In Section 3, we present the administrative datasets, the natural quasi-experiment and the econometric strategy. In Section 4, we present the results and discuss the findings. We conclude on public policy issues and further research in Section 5.

[3] The French local public agency network has 951 agencies for a population of 66.3 million and 2.9 million jobseekers; by comparison the German network has only 621 local public employment agencies for a population of 81 million and 2.8 million jobseekers.

2 The Spatial Dimension of Public Intermediation in the Labour Market

2.1 Converging Empirical Evidence

Many recent papers put the emphasis on the spatial dimension of the public intermediation in the labour market as an important factor in the efficiency of the job/worker matching process.

This concern is classically found in recent papers that focus on the evaluation of active labour market policies, where geographical differences are used to introduce variability in the labour market policies frameworks (Frölich and Lechner, 2010; Altavilla and Caroleo, 2013 and Ferracci *et al*., 2014).

Other papers directly question the potentially detrimental effects of the geographical distance between LPEAs and their recipients.

These papers echo the twin literature on the spatial distribution of local public goods produced by non-profit organization, where converging papers unearth spatial discrepancies between the spatial distribution of the non-profit agencies and the distribution of their recipients. Many papers show that when relative needs are considered, the density of non-profit agencies is looser in poorer neighbourhoods than in more affluent communities. See for example, Allard and Danziger (2003) for the Detroit metropolitan area; Joassart-Marcelli and Wolch (2003) for Southern California; Bielefeld and Murdoch (2004) for the metropolitan areas of Boston, Dallas/Fort Worth, Indianapolis, Memphis, Minneapolis/Saint Paul, Orlando, Pittsburgh, Portland (Oregon), and San Diego; Allard (2009) for Chicago, Los Angeles and Washington D.C and Wathen and Allard (2014) for a comparison between the United States and Russia.

For LPEAs, Joassart-Marcelli and Giordano (2006) find a significant negative link between accessibility to LPEAs and unemployment. At the census tract level, they show accessibility differentials by race/ethnicity, age, and location. They also find that access to Californian One-Stop Career Centres reduces aggregated unemployment, with larger effects for groups who experience limited mobility due to gender or race, such as black and female job seekers.

Suárez Cano *et al*. (2012a, 2012b, 2015) study, in the Spanish context, the effect of the accessibility to local public employment offices on local unemployment rates according to the distribution of three different types of municipalities: large urban, small urban and non-urban. They also find that, at the municipality level, accessibility to employment offices significantly affects the labour market outcomes of the jobseekers, and that this effect is particularly important in non-urban areas where employment opportunities are limited.

2.2 Public Policy Implications

These results have direct public policy implications, suggesting that a denser spatial network of LPEAs would effectively decrease unemployment, especially in rural areas.

In France, this concern underlies the ongoing debate on reform of Pôle Emploi, the public employment service. Pôle Emploi was created in December 2008 by the merging of the former institutions in charge of job seeker monitoring and control (ANPE[4]) and of the distribution of unemployment benefits (ASSEDIC[5]).

Its creation, coincidental with the 2008 financial crisis, led to many institutional dysfunctions, without any real managerial re-organization of the new Pôle Emploi agencies (Iborra, 2013). There was also no redefinition of the LPEA network (Cour des Comptes, 2015): the 830 ANPE local agencies were became 873 Pôle Emploi local generalist agencies and 41 agencies specialized in niche labour markets (entertainment, handicapped workers…). The result is a very dense agency network: in 2009, 80% of jobseekers could reach their LPEA under 30 minutes, versus 96.4 % in 2012. Comparatively, the average commuting time was 72 minutes for students and employed workers.

In terms of managerial efficiency, the Audit Court[6] criticizes the relative dispersion of caseworkers, their reduced specialization and the unnecessary duplication of tasks across agencies (human resources, benefit distribution, call centres). In 2014, almost a quarter (24%) of the Pôle Emploi workforce was not actively devoted to the counselling and monitoring of the jobseekers. More, the actual monitoring of the jobseekers only filled up to 37% of actual caseworkers' time, and the prospection of vacancies only represented up to 7% of their time. In terms of cost, the Court reports that the surface occupied by LPEAs showed a 15.9% increase between 2009 and 2013; with 85% of this surface being rented to private landlords, the yearly rent of the LPEAs increased by 21.6% between 2011 and 2014, reaching 264 million euros in 2014. As a consequence, the Court prescribes the reduction of the number of agencies in the years to come; in a Rawlsian fashion, this measure is to be compensated by specific mechanisms for the jobseekers who live far away from LPEAs.

This view is strongly opposed by Pôle Emploi, local public opinions[7] and local public authorities, who claim that a denser network of LPEAs is necessary for strict equality's sake but also to curb the potentially damaging effects of spatial mismatch. Besides warning against institutional adaptation costs, they also underline potential welfare effects of longer commutes to LPEAs for jobseekers who live in rural areas or have limited access to public transportation and car ownership.

2.3 Theoretical Ambiguity

The theoretical intuition between distance to LPEAs and unemployment directly echoes the Spatial Mismatch literature: distance to agencies is presented as a new source of friction in the matching process between jobs and jobseekers, necessarily leading to an increase of local unemployment rates.

Since John Kain's (1968) seminal paper, it is widely acknowledged that spatial mismatch, i.e. the geographical distance between jobs and workers, is a key factor when understanding individual differences in unemployment and job search success rates. Empirical evidence on the spatial mismatch between jobs and workers is plentiful, both in the US and the European context[8].

[4] ANPE: Agence Nationale pour l'Emploi (National Agency for Employment).
[5] ASSEDIC: Association pour l'emploi dans l'industrie et le commerce (Association for Employment in Industry and Trade).
[6] In charge of conducting the financial and legislative audit of the French public institutions.
[7] For an example, see Merlin (2015).
[8] Empirical evidence on the spatial mismatch between jobs and workers is plentiful. On the US context, Ong and Miller (2005) show that poor accessibility to jobs lead to adverse effects on job search outcomes. On the European context, studies are fewer and more recent, since until recently the compact structure, good public transportation systems and lower segregation rates were believed to protect European cities from spatial mismatch issues (Korsu and Wenglenski, 2010). Spatial Mismatch evidence was however found in British cities (Houston, 2005; Patacchini and Zenou, 2005), in Dutch cities (Musterd et al., 2003; van der Klaauw and van Ours, 2003), in Brussels (Dujardin et al., 2008), Madrid and Barcelona (Matas et al., 2009) and Paris (Choffel and Delattre, 2003; Gobillon and Selod, 2007; Duguet et al., 2009; Korsu and Wenglenski, 2010). Further, using data on the 36 600 French municipalities, Détang-Dessendre and Gaigné (2009) showed a gradation of the spatial mismatch intensity between metropolitan, urban and rural areas, with an insignificant relationship between unemployment duration and job access for workers living in large urban centers but a linear relationship for the other workers.

Theoretically, different factors channel the spatial mismatch (see Gobillon *et al.*, 2007 or Zenou, 2009, for a literature review): 1) transportations costs limit the area where it will be profitable for jobseekers to search for jobs, so that they limit their prospection area; 2) information on jobs decreases with distance, whether because jobseekers are ignorant of the specificities of the labour market of unfamiliar parts of the cities or because job opportunities are advertised using local recruiting methods such as 'help wanted' signs in windows; 3) firms implement territorial statistical discrimination against the jobseekers who live in neighbourhoods with a bad reputations 4) firm redline jobseekers because too long commutes reduce their productivity and 5) cheap housing in the areas located the furthest from the jobs means less search incentives for the jobseekers.

How should these determinants work if we take into account the intermediation of LPEAs between jobs and jobseekers? One could argue that placement agencies should be considered as a partial solution to rather than a further cause of spatial mismatch between jobs and workers. Indeed, public placement agencies are precisely dedicated to act as matching facilitators, collecting the best possible information on vacancies and working with firms to help them define their needs and revise their biases and expectations, which should help prevent spatial mismatch arising through channels 2, 3 and 4.

Still, distance to the LPEA could create its own spatial mismatch issues, by creating, in particular, frictions between jobseekers and agencies. This could arise, in line with the theoretical Spatial Mismatch literature, if higher transportation costs discourage jobseekers to travel to their agency, or if agencies discourage the enrolment of far-living, less employable jobseekers.

However, this does not happen in practice, for three reasons. First, agencies are required by law to enrol all jobseekers within their catchment area, so they cannot redline workers. Second, since the implementation of active labour market policies, monthly meetings with caseworkers are compulsory: a jobseeker cannot trade off transportation costs and matching perspectives, whatever the housing prices in her neighbourhood. Third, caseworkers monitor the jobseeker's search process and fill the informational gaps she may be facing.

As a result, there should be no significant effect of jobseeker-to-agency distance on the matching process results. This theoretically-driven statement contrasts with the empirical results found in the literature. How is that possible? A possible explanation is that distance to LPEAs conceals other variables otherwise relevant to the spatial mismatch mechanism, for example distance to local central business districts where the administrative centres are located… as well as most of the jobs. Distance to LPEAs probably seems to affect local unemployment rates because it works as proxy for the accessibility to the local labour market. In other words, it is possible that the location of LPEAs is not exogenous relatively to the location of job opportunities. In this case, distance to agencies would work as a proxy for spatial mismatch itself.

To shed further light on this issue, it is necessary to find an empirical strategy that allows the identification of 'pure' LPEA/jobseeker distance effects on individual labour market outcomes, independently of the individual characteristics of jobseekers and of congestion and institutional effects within the agencies. In this paper, we propose a way to do so by relying on a quasi-natural experiment and exhaustive administrative datasets.

They also find that, for workers living outside large urban centers, a rise in accessibility to jobs increases the probability of leaving unemployment.

3 Data and Empirical Strategy

3.1 The Data

We combine previously unexploited exhaustive individual datasets on jobseekers and LPEAs' staff characteristics and structure that were exceptionally available for the French Rhône-Alpes region.

First, we use the longitudinal Pôle Emploi dataset, which provides an exhaustive record for all the unemployed jobseekers (18-65 years old) during a long period of time (8 years). We focus on the June, 2006 to April, 2012 period in order to stay within the parameters of an single active labour market policy framework: as noted by Fontaine and Le Barbanchon (2012), 2005 was a turning point in the generalization of active labour market policies in France, with another drastic modification of the monitoring and control of jobseekers taking place in 2013.

This dataset provides the following variables: unemployment duration, gender, nationality, number of children, marital status, educational level, age, name and location of their LPEA and residential location of jobseekers (at the municipality level[9]). This allows the computation of unemployment recurrence for the 2004 to 2012 period and to control for jobseeker residential moves that could otherwise lead the underestimation of unemployment duration. To take into account unemployment recurrence we first calculate the total duration of unemployment spells during the last 2 years and a half before each new inscription as an unemployed jobseeker in the agency register. To measure the durability of the exits from unemployment, we also compute the gap between the last unemployment spell of a jobseeker and her actual one.

Note that for computing these two elements, we use the exit of a jobseeker from the individual Pôle Emploi dataset, where the motive of the exit is not notified. Global surveys establish that the majority of exits from the Pôle Emploi databases are "true" exits from unemployment, through job matches (46.7% of the exits in March 2011) or the resumption of studies (9.9% of the exits) (Bernardi and Poujouly, 2011). Jobseekers involuntarily exit the Pôle Emploi database because of administrative mishaps ("*Accidental failure to renew the enrolment*" and "*Lack of actualisation followed by a re-inscription*", 24.8% of the exits). Since we use an exhaustive dataset, we can track the immediate re-entry of the jobseekers in the database and exclude these 'false' exits from unemployment from our variables of interest (unemployment duration and durability of the exits from unemployment). Jobseekers also transition to inactivity ("*Retirement, exemption from the job search*", 0.9% of the exits) or temporary suspend their job search for maternity, military, holidays of medical reasons ("*Temporary suspension of the job search*": 8.2% of all exits). In these cases, the nature of our database does not allows us to track and exclude these exits from the computation of our variables of interest; however the impact of this shortcoming is lessened by the fact that there is virtually no link between these motives of exit and the jobseeker/agency distance. By contrast, the rest of the motives ("*Administrative radiation*", "*Voluntary non-renewal of the jobseeker's enrolment*" and "*Other*") are more problematic for us, since they can, at least partially, be caused by the demotivation of jobseekers, which in turn could be potentially affected by a too great distance to the jobseeker's agency. A mitigating factor is that these exits are few (9.4% of all exits in March 2011), so that the overall impact on our estimations is bound to be weak (see part 4 for a further discussion in light of our results).

[9] The Pôle Emploi dataset does not provide a finer geolocation of jobseekers.

Another issue is the added value of the public intermediation service on the job search process: in 2011, only 14% of the job matches were directly organized by Pôle Emploi (Bernardi, 2013). Moreover, 28% of the new job matches were created through personal or professional relations and 22% through unsolicited applications, which underlines the increasing role of informal and decentralized search processes (such as Interned-based job search, Kuhn and Mansour, 2014) – for which no datasets exist. However, the mission of the public intermediation goes beyond the mere matching of jobs and workers: since the implementation of active labour market policies, it also focuses in helping the jobseekers in implementing efficient and diversified search strategies which has indirect positive effects on the employment prospects of jobseekers, as shown by the converging empirical evidence on the evaluation of active labour market policies. Caseworkers counsel jobseekers in writing resumes, in using the Internet in their job search, in devising an effective spontaneous application strategy, in identifying job opportunities and in the activating their personal and professional network….

Second, we use information on the LPEAs available through the Annual Declaration of Social Data[10] dataset, which provides *exhaustive* data on all establishments located in France, identified through a unique SIRET[11] number. Beyond the mere monitoring and control of jobseekers, LPEAs perform a varied set of tasks: benefits distribution, vacancies prospection, local public institutions and firms networking… We only take into account the LPEA staff members whose profession is a variation of 'caseworker'[12]. Using this information, the average congestion of the LPEA j is computed by measuring the average caseload of each caseworker, as in:

$$caseload_j^n = \frac{jobseekers_j^n}{caseworkers_j^n} \quad (1)$$

With:

- $jobseekers_j^n$ the number of jobseekers enrolled in the LPEA j during at least 3 months[13] during the quarter n
- $caseworkers_j^n$ the number of caseworkers of the LPEA j during the quarter n

Third, to measure directly the distance, we rely on the original Odomatrix© dataset (Hilal, 2010), that provides municipality-to-municipality transportation times (by car). The distance by time is a better measure of accessibility than Euclidian distance because it takes into account congestion and actual road networks.

3.2 Zoning Modifications as a Quasi Natural Experiment

Our study area (called 'Belleville zone' throughout this paper) is located in the suburban Northern part of the Greater Lyon area, France's third city in size. It is constituted of 399 municipalities located in six LPEA catchment areas: Roanne, Riorges, Tarare, Belleville, Villefranche, Bourg-en-Bresse and Trevoux (see Figure 1).

[10] Déclaration Annuelle de Données Sociales (DADS). Pôle Emploi also publishes, since 2013 and the successful legal action carried out by the reference newspaper *Le Monde*, detailed information on the staff composition of all local public employment agencies (staff structure, number of caseworkers and caseload per caseworker). Sadly, this information is not available for our study period (2006-2012).

[11] SIRET: Computer System of the Firm and Establishment Directory (Système Informatique du Répertoire des Entreprises et de leurs éTablissements).

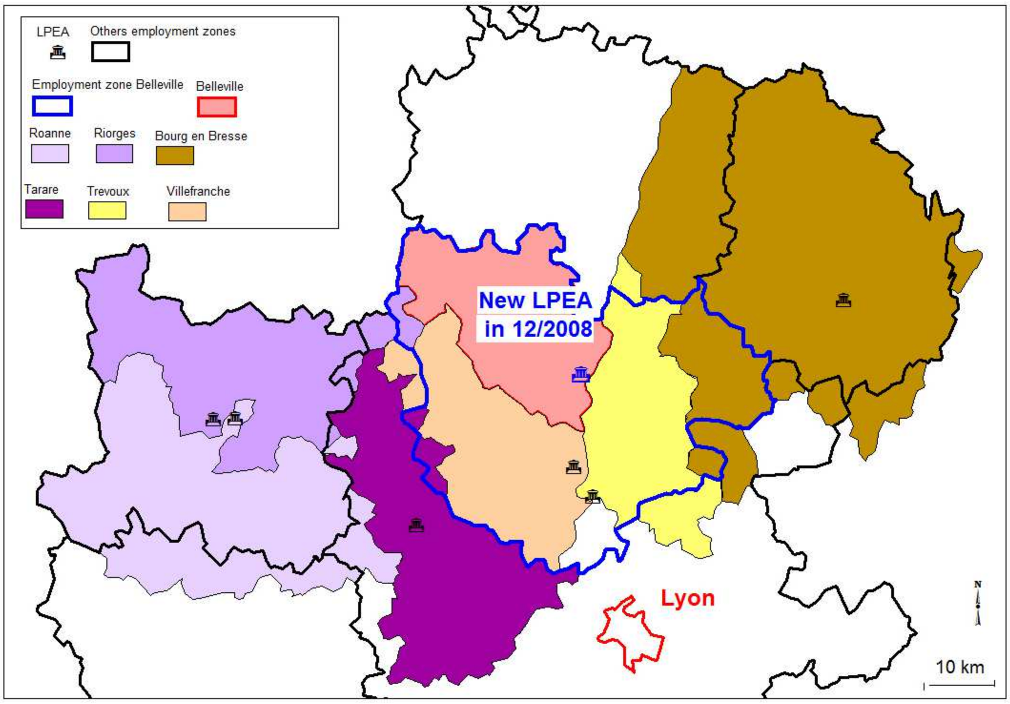
[12] We only keep the staff members whose profession is coded PCS 333E, PCS 343A or PCS 451C: senior advisor, advisor, deputy advisor and placement officer ('*conseiller principal*', '*conseiller*', '*conseiller adjoint de l'emploi*' and '*prospecteur placier*').

[13] Alternative results can be provided for a 6-month threshold, with no significant differences.

This area is interesting because of a modification of the spatial distribution of the LPEAs that took place in December 2008, when Pôle Emploi was created. Before December 31st, 2008, all the jobseekers who lived in the 104 municipalities situated in the Belleville zone were enrolled in the LPEA of Villefranche-sur-Saône. In January 1st, 2009 a new agency opened in Belleville (blue symbol on Figure 1), its catchment area comprising the 43 northern municipalities of the zone (area in pink in Figure 1). The catchment area of the Villefranche agency[14]was reduced to the 61 southern municipalities of its prior catchment area (area in salmon pink in Figure 1). The creation of the Belleville agency created a variation in the geographical distance between jobseekers and the placement agencies in the area, creating a quasi-natural experiment. Controlling for all individual jobseeker characteristics to the use of exhaustive individual datasets (see above) it is therefore possible to check whether 'pure' spatial effects affect the unemployment prospects of jobseekers.

Another thing is that this area is comprised of rural and semi-rural municipalities, which echoes with the literature. In France, evidence of job/workers spatial mismatch is more convincing for rural areas (Détang-Dessendre and Gaigné, 2009). Moreover, Suárez Cano *et al*. underline that detrimental effects of poor accessibility to LPEAs are more important for rural areas (Suárez Cano *et al*., 2012a).

Figure 1. The Belleville Zone Map



3.3 Identification strategy

3.3.1 Direct measure of the distance-to-agency effect on unemployment

This quasi-experimental settings allows us to use the difference-on-difference method to study the effect of distance to the LPEA on, Y_{it}, the labour market outcomes of jobseekers i at the period t. To do so, we use the difference-in-difference and the matching methodologies.

[14] The Villefranche agency also moved in 2013 but this change was implemented after our period of investigation; furthermore, it remained within such a small perimeter (less than 500m from its initial location) that we suppose that this move is trivial and will have no impact whatsoever in the future.

We start with a simple model:

$$Y_{it} = \beta X_i + \alpha D_{it} + \gamma_1 Dist_i + \gamma_2 (Dist_i)^2 + u_{it} \qquad (1)$$

with:

- X a set of individual explanatory variables (age, gender, diploma, years of professional experience, trimester of entry in unemployment, duration of unemployment spells in the last 30 months and a constant);
- D_i dummy for the years after the change;
- G a dummy for the residential location area of the jobseeker;
- $Dist$ the distance expressed in time between the centroid of the jobseeker's residential municipality and the agency location.

The key parameters of the equation are γ_1 and γ_2 which represent the effects of geographical distance on the labour market outcomes of jobseekers. In a discrete framework, the marginal effect associated to the distance variable is calculated by using the following formula:

$$\frac{\partial p_i}{\partial \mathrm{dist_i}} = \frac{\partial \Lambda(.)}{\partial \mathrm{dist_i}} = p_i(1 - p_i)(\gamma_1 + 2 \times \gamma_2 \mathrm{dist_i})$$

With p_i the estimated probability.

To define the labour market outcome Y_{it}, we use two measures based on different definitions of the jobseeker's unemployment spells.

- The jobseeker's unemployment spell duration
- A "durable exit to work" outcome, defined as the time need to find a job without entering a new unemployment spell during the 6 months after exiting unemployment.

As in Crépon *et al.* (2005), we only keep the first observed spell of unemployment to avoid the possible correlation of unobservable characteristics.

For both outcomes, we consider the probability that a jobseeker who has been unemployed more than 4 months[15] exited unemployment within the first M months of her unemployment spell:

$$Proba(exit \mid T < M\ months, T < 4\ months)$$

3.3.2 The difference-in-difference model

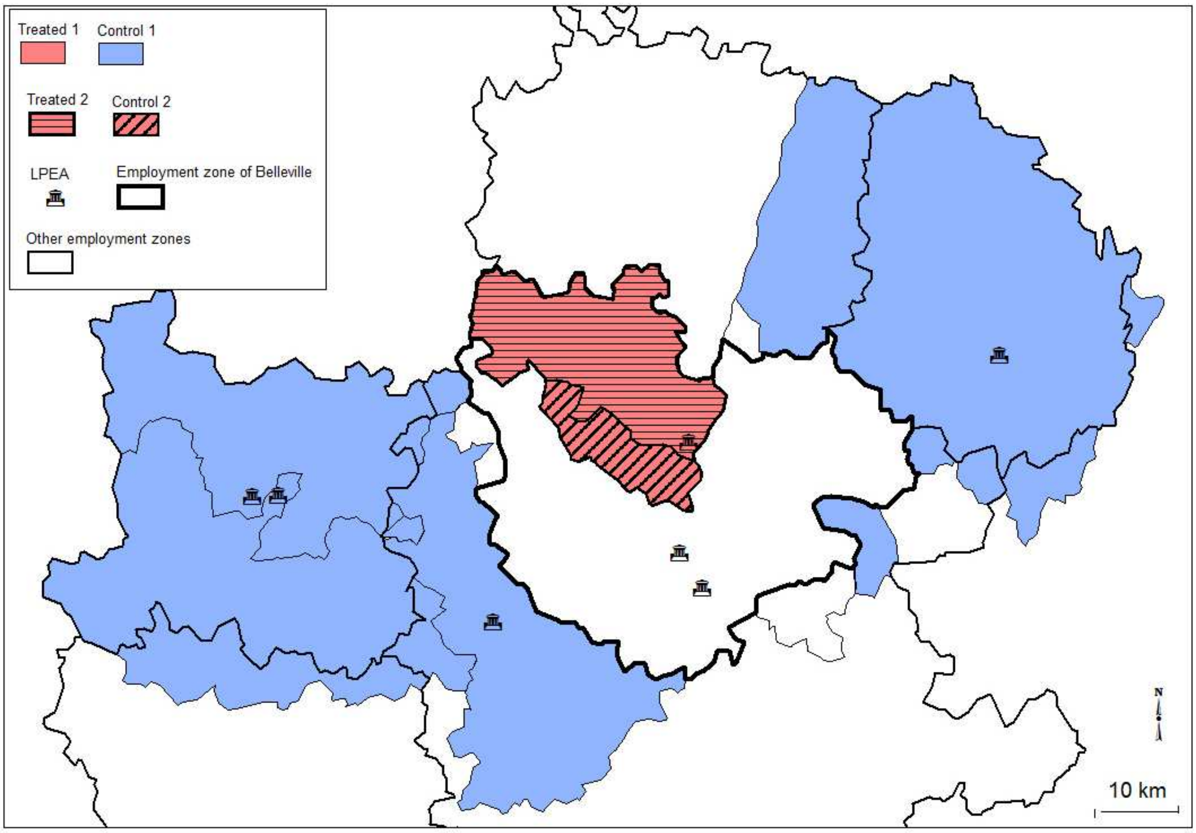
a) Control groups

We consider two pairs of treated and control groups.

The first treated group (in red in Figure 2) is the group of jobseekers 1) who live in the municipalities located in the catchment area of the new Belleville LPEA, 2) who were formerly enrolled in the Villefranche agency and 3) who benefited, with the creation of the Belleville agency, from a significant reduction of the travel time between their home and their Pôle Emploi agency (on average, almost a 50% decrease, dropping from 25 to 12 minutes for a one-way trip, see Figure 3).

[15] Since the implementation of active labour market public policies, an important landmark in the counselling is the compulsory second meeting with their caseworker (the first one takes place when the jobseekers is enrolled at the LPEA) where the search strategy of the worker is outlined. Fontaine and Le Barbanchon, 2012, established that 37% (25%) of the second caseworker/jobseeker interview take place after the 4th (5th) month in unemployment). With a 4-month threshold, we conservatively think that most jobseekers will have received the 'added' value counselling from their caseworker.

Figure 2. Control and treated groups

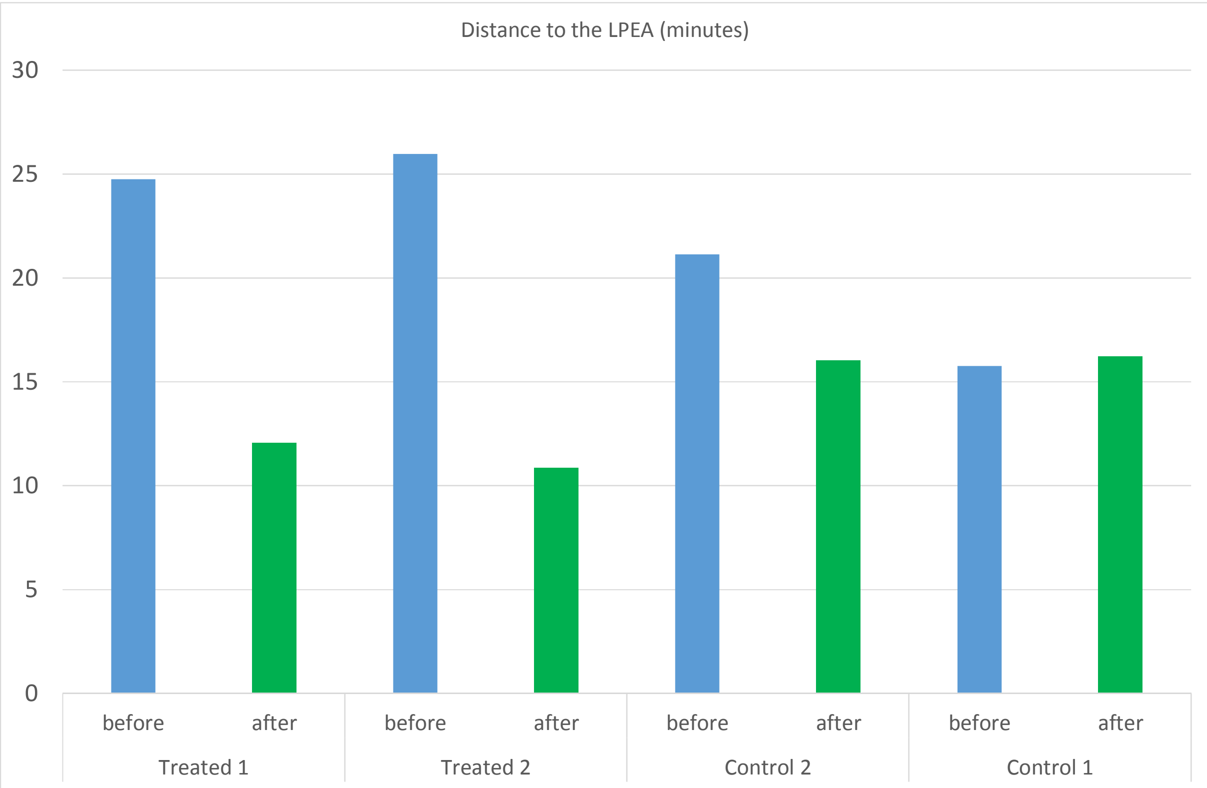


The first control group (in light blue in Figure 2) is a group of jobseekers who were not affected by the creation of the new Belleville agency, i.e. who live in the municipalities that are located 1) inside the catchment areas of nearby agencies 2) not including Villefranche and 3) outside the Belleville *Employment Zone*. In Figure 3, we can check that the travel times to these jobseeker's agencies was not significantly altered after the Belleville creation. First, the Roanne, Riorges, Tarare and Bourg-en-Bresse agencies were chosen because they are geographically close[16] to the Belleville-Villefranche areas and because, being rural or semi-rural areas, they share similar socio-economic characteristics (see Appendix 1 for descriptive statistics). Second, the jobseekers enrolled in the Villefranche agency are excluded because they can be directly affected by the creation of the Belleville agency in two opposite ways. On the one hand, the reduction of the catchment area of the Villefranche agency contributes to a reduction in the caseload of Villefranche caseworkers, which may lead to a greater efficiency and a better outcomes for the Villefranche jobseekers. On the other hand, the creation of a specific agency for the Belleville jobseekers could lead to a better placement perspectives for *them*, i.e. an increased competition for the Villefranche jobseekers leaving in the same Employment Zone. Third, as pointed out by RUBIN (1977) to identify causal effect it is important to be in a situation where we do not observe interactions between treated and control group. The well-known stable unit treatment value assumption (SUTVA) assumes that the treatment status of any unit does not affect the potential outcomes of the other units. To minimize the potential interactions between the treated and the control groups, we exclude from Control Group 1 all the jobseekers who live in the

[16] Other nearby areas north of the zone could also be included in the control group, but they are located outside the Rhône region, i.e. outside the perimeter of our datasets.

same Employment Zone than the Belleville treated group. Defined using Census data[17] by the French National Statistics Institute (INSEE), an employment zone is a homogeneous labour market zone, i.e. an area within which most of the labour force lives and works, and in which firms can find the main part of the labour force necessary to occupy the offered jobs. Restricting the control group to jobseekers who live outside the employment zone of the treated group should limit the interactions between the two groups. We also exclude the jobseekers who live in the catchment area of the Trevoux agency since most of them are also located inside the Belleville employment zone.

Figure 3. Time travel to the LPEA before and after the Belleville creation for the Treated and Control groups



Source: ODOMATRIX and FHS-Pôle Emploi, first spell per jobseeker

The second treated/control group pair is defined in order to disentangle institutional and distance effects. The second treated group is defined as the jobseekers who, inside the Belleville area, benefited from a substantial reduction of their travel time to their agency (more than 14 minutes on average, see Figure 3) (horizontal stripes in Figure 2): they were affected by both a distance and an institutional change. The second control group is formed by the jobseekers who were affected by the institutional change but who did not benefit from proximity effects after the creation of the Belleville agency, i.e. the jobseekers who live in the area that is close both to their former Villefranche and their new Belleville agencies, so that they gained less than 10 minutes (5 minutes on average, see Figure 3) in their time travel to their agency (diagonal stripes in Figure 2).

[17] The zoning used in the paper is based on the flows of movement from residence to work of active persons observed in the 2006 Census.

b) Parametric estimation

In the second model we do not introduce the distance variable but we deal with difference-in-difference strategy of identification. Note the labour market outcome ${Y_{itl}}^k$ of the treated (k=T) and non-treated groups (k=C) before (l=1) and after (l=0) the creation of Belleville LPEA.

$$\begin{cases} {Y_{it1}}^T = \beta X_i + \gamma_3 \mathrm{d}_i + u_{it} \quad if\ t > \bar{t} \\ {Y_{it0}}^T = \beta X_i + u_{it} \quad if\ t \leq \bar{t} \\ {Y_{it1}}^c = \beta X_i + u_{it} \quad if\ t > \bar{t} \\ {Y_{it0}}^c = \beta X_i + u_{it} \quad if\ t \leq \bar{t} \end{cases}$$

Where $\bar{t}$ is the period where the creation of the Belleville agency takes place.

Also, to account for learning effects by caseworkers in the new agency, we introduce three dummy variables for the 2009, 2010 and 2011 years.

The measure of the causal effect is represented by the coefficient on the interaction term γ_3 in a regression.

$$Y_{it} = \beta X_i + \delta_1 T_i + \delta_2 \mathrm{d}_i + \gamma_3 \mathrm{d}_i \times \mathrm{T}_{\mathrm{i}} + u_{it}$$

Note that in a logit model, the marginal effect associated to the treatment in the period where the treatment is implemented ($\mathrm{d}_i \times \mathrm{T}_{\mathrm{i}}$) is obtained by using:

$$\frac{\partial p_i}{\partial \mathrm{d}_i \times \mathrm{T}_{\mathrm{i}}} = \frac{\Delta p_i}{\Delta \mathrm{d}_i \times \mathrm{T}_{\mathrm{i}}} = F(X_i \beta + \gamma_3) - F(X_i \beta)$$

Where $\Delta(.)$ is the différential operator and $F(.) = \frac{\exp(.)}{1+\exp(.)}$

c) Non-parametric estimation

In the difference-in-difference method we assume that the Treated and the Control groups are subject to the same aggregated labour market trends. This methodology gives the effect of the treatment on the treated controlling for the individual-specific effect fixed over time and time-specific effect common for all agents.

However, if the expectation of the individual specific effects differ between the treated and the control groups differs over time, the difference-in-differences estimator is inconsistent.

To address this issue, we also implemented the matching method with differences in differences proposed by Blundell and Costa Dias (2000). In this framework the non-random treatment assignment bias is reduced by balancing the treated and the control groups on observed covariates.

The non-parametric method of propensity matching allows to select a control group on the basis of a single score. Find comparable treated before the introduction of the new LPEA:

$$\gamma_4 = \sum_{i \in T_1} \left[\left({Y_{it_1}}^T - \sum_{j \in T_0} {w_{ijt_0}}^T {Y_{it_0}}^T \right) - \left(\sum_{j \in C_1} {w_{ijt_1}}^C {Y_{it_1}}^C - \sum_{j \in C_0} {w_{ijt_0}}^C {Y_{it_0}}^C \right) \right]$$

Where ${w_{ijt}}^G$ represent the weights attributed to individual j in the group G (where G= C or T) at the period of time t when comparing with treated individual t.

To estimate γ_4 we compute the two propensity scores by using usual regression model for binary variables (i.e. the logit model):

$$P_{TX} = P(T = 1|X)$$

$$P_{dX} = P(d = 1|X)$$

Where T is a binary variable equal to 1 if the jobseeker leave in a commune in catchment area LPEA of Belleville and 0 otherwise and X is a vector of covariates. The selection of the control group based on P_{dX} and P_{TX} is possible if given those probabilities (or scores) exposition to the treatment is independent of the covariates (X). Formally this balancing of score condition can be written as:

$$T \perp X | P_{TX}$$

$$d \perp X | P_{dX}$$

See Appendix table B for the presentation of matching propensity score of the treated versus control groups and before versus after the creation of the LPEA.

The average treatment on the treated is obtained by using command psmatch2 in Stata© software (Becker and Ichino, 2002). The matching is restricted to the area of common support (see figure A in the appendix) and is based on the kernel matching procedure (for each treated all the controls are considered with a weight inversely proportional to the distance between the propensity score of treated individuals and control individuals). To take into account the discrete nature of the outcome variable, the impact of the treatment obtain with Stata© is modify by using the formula proposed by Blundell and Costa Dias (2000):

$$\gamma_4 = E(Y_{it}|X, T = 1, t = 1) - f\left[f^{-1}\left(E(Y_{it}|X, T = 1, t = 1)\right) - A\right]$$

Where

$$A = \left[f^{-1}\left(E(Y_{it}|X, T = 1, t = 1)\right) - f^{-1}\left(E(Y_{it}|X, T = 1, t = 0)\right)\right] \\ - \left[f^{-1}\left(E(Y_{it}|X, T = 0, t = 1)\right) - f^{-1}\left(E(Y_{it}|X, T = 0, t = 0)\right)\right]$$

Finally the standard error are obtain by bootstrap with 200 replications.

4 Results

4.1 Accessibility Differentials to Agencies

In our study area, we find that, on average, municipalities are located just under 30 minutes from their LPEA (see Table 1). Note that this result is measured at the municipality level, without accounting for population density disparities between municipalities. By contrast, individual travel times are, on average, inferior (17.8 minutes for a one-way trip), which highlights the potential bias that might arise when working on aggregated data.

In line with past empirical evidence, (Allard, 2004, 2009; Allard and Danzinger, 2003; Joassart-Marcelli and Giordano, 2006; Suárez Cano *et al*., 2012a, 2012b, 2015), we find notable *average* differentials in accessibility to LPEAs between municipalities: rich, educated and white collar municipalities are, on average, closer to LPEAs than poor, uneducated and blue collar municipalities. Also, supporting Suárez Cano *et al*. (2012a), we find that, on average, travelling to one's LPEA takes almost twice the time jobseekers who live in rural municipalities than for jobseekers who live in urban ones (38.8 minutes versus 21.0 minutes).

Interestingly, the municipalities with high unemployment also tend to be, on average, closest to LPEAs than municipalities with low unemployment rates (35.2 vs. 24.1 minutes): this result hints that the spatial distribution of agencies is not exogenous, but deliberately targets high-unemployment urban zones.

Further, we find that, controlling for individual characteristics, distance to LPEAs significantly affects the probability to exit unemployment for jobseekers whose unemployment spell lasted at least 4 months (see Table 2): for example, we find that an increase of 10 minutes of a jobseeker's home/agency travel time reduces by 0.12 points the probability of exiting unemployment after an unemployment spell of 12 months (see Table 2). We also find that the effect of distance is not linear, so that the effect of the marginal minute is stronger for jobseekers who live in distant municipalities than for jobseekers who live in closer ones.

These results are line with past empirical evidence: distance to LPEAs seems to affect poorly the jobseekers' employment perspectives, with an increased effect on vulnerable groups. However, keeping in mind that the usual job/worker spatial mismatch sources shouldn't work for agency/worker distances, this result could reflect the fact that distance to the LPEAs could act as a proxy for other factors, for example the distance to the central business (or administrative) district where most jobs are concentrated.

Table 1. Accessibility to Local Job Employment Agencies (minutes)		
Municipality profile (2012 data)	Mean	Std
Metropolitan status		
Rural	38.8	8.9
Suburban	29.0	10.4
Urban	21.0	11.8
Income		
Rich: top 10 municipal median income	28.6	13.2
Poor: bottom 10 municipal median income	38.4	6.5
Education		
High: top 10 with the highest % with a college degree	30.9	9.5
Low: top 10 with the highest % with a diploma inferior to the Bac*	34.7	9.2
Unemployment		
High: top 10 unemployment rate	24.1	17.3
Low: bottom 10 unemployment rates	35.2	9.3
Workforce		
Blue-collar: top 10 proportion of blue collar workers	36.4	8.3
White-collar: top 10 proportion of white collar workers	35.7	7.0
All agencies	29.1	11.2
Sources: Odomatrix, INSEE Census. (*) The Bac (Baccalauréat) is the French equivalent of the A-Levels		

Table 2. LOGIT model of the probability of exiting unemployment for jobseekers unemployed during at least 4 months.

	EXIT AFTER															
	5 months		**6 months**		**7 months**		**8 months**		**9 months**		**10 months**		**11 months**		**12 months**	
	Model 1	**Model 2**	**Model 1**	**Model 2**	**Model 1**	**Model 2**	**Model 1**	**Model 2**	**Model 1**	**Model 2**	**Model 1**	**Model 2**	**Model 1**	**Model 2**	**Model 1**	**Model 2**
Fixed effect of LPEA	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual covariates(*)	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Log Likelihood	-1,6514	-1,6245	-23,639	-23,148	-26,432	-27,244	-30,519	-29,621	-32,052	-30,900	-31,845	-30,726	-32,150	-30,919	-32,125	-31,565
Akaike's info. criterion	33,044	32,553	47,295	46,358	52,926	54,551	61,124	59,576	64,190	62,043	63,707	61,514	64,315	61,899	64,267	63,463
Pseudo-R2	0.15%	1.78%	0.20%	2.28%	2.94%	2.89%	0.33%	3.27%	0.30%	3.62%	0.54%	4.04%	0.54%	4.35%	0.57%	4.77%
Distance to LEPA (hrs)																
Dist	-0.091 (0.181)	0.052 (0.183)	-0.165 (0.142)	-0.009 (0.144)	-0.299** (0.130)	-0.138 (0.129)	-0.371*** (0.119)	-0.213* (0.121)	-0.443*** (0.115)	-0281** (0.118)	-0.589*** (0.116)	-0.288** (0.116)	-0.399*** (0.116)	-0.190* (0.112)	-0.398*** (0.116)	-0.205* (0.167)
Dist*dist	0.148 (0.260)	0.010 (0.263)	0.230 (0.205)	0.084 (0.207)	0.420** (0.188)	0.271 (0.185)	0.529*** (0.171)	0.380** (0.174)	0.592*** (0.166)	0.440*** (0.170)	0.705*** (0.169)	0.426*** (0.160)	0.484*** (0.168)	0.295* (0.162)	0.474*** (0.168)	0.290* (0.167)

Source: FHS-Pôle Emploi, first spell per individual. Number of observations: 46 672. (*) Covariates include: gender, diploma (3 levels), age, and time since the last unemployment spell, job experience and a dummy for the post 2009 period.

4.1 Difference-in-difference results

Using to the quasi-experimental framework created by the creation of the Belleville agency, we are able to test our working hypothesis of no jobseeker/agency distance effects on the jobseekers' job market outcomes (see Figure 4 for gross differences and Table 3 for the results of the difference-in-difference and matching estimations[18]).

First, let's focus on the first part of Table 3 (Treated and Control groups 1). If our working hypothesis is right, the difference of the marginal effect of distance on the probability to leave unemployment between the treated and control group should be insignificant. On the other hand, if the distance to LPEAs is a new channel for spatial mismatch, the coefficients should be significantly positive.

For *durable long-term exits* from unemployment (probability of not having been unemployed again during the 12 months that followed the exit from unemployment), our hypothesis is validated: neither the difference-in-difference nor the matching models show a significant difference of the effect of distance between the jobseekers of the Belleville area and those of the control group 1, who were not affected by the creation of the new agency. This is also the case for durable short-term exists from unemployment (6 months) and for the effect of distance on gross exits from unemployment (i.e. the probability of exiting unemployment after a 6 and 12 months unemployment spell) for the 2010-2011 period[19]. This is interesting since Card *et al*. (2105) have recently established that the effects of active labour market public policies had the greater positive effects in the medium and longer run (two years and more).

This result does not hold for short-term exists from unemployment and for gross exits from unemployment if we also take into account the year 2009. For the 2009 and the 2009-2011 period as a whole, distance to LPEAs has a significant impact job matching outcomes. However, this impact is negative, not positive: we find that the Belleville area jobseekers are worse off than the jobseekers of the control group, which invalidates both our working hypothesis *and* the "new spatial mismatch channel" hypothesis.

An intuitive solution to this apparent conundrum, in line with seminal papers on the institutional determinants of LPEAs efficiency, suggests that the poor efficiency of the Belleville agency could be due to transitory institutional dysfunctions during the agency's running-in period. To test this explanation, we compare the outcomes of the Treated and Control groups 2, who share the institutional effects of the creation of the new agency but who differ in the 'pure' distance effect, since the control group's home/agency distance was, by construction, not drastically affected by the creation of the new agency.

Whatever the estimation period, the definition of exit from unemployment or the difference-in-difference methodology, we that no coefficients are significant anymore (see Part 2 of Table 3), which validates our working hypothesis of no evidence of a worker/agency spatial mismatch.

[18] More detailed results are available upon request.
[19] We focus on the three years that follow the creation of the Belleville agency: 2009, 2010 and 2011. Extending the estimation to the year 2012 (which is the last year for which we have available data) does not change the results.

Table 3. Difference-in-difference results

	Parametric						Matching		
	All years		2009		2010-2011		All years	2009	2010-2011
	Coef (std)	*Marginal effect*	*Coef (std)*	*Marginal effect*	*Coef (std)*	*Marginal effect*	*Marginal treatment effect on treated*	*Marginal treatment effect on treated*	*Marginal treatment effect on treated*
Part 1 – Control 1 / Treated 1									
Gross exit (difference of the effect of distance on the probability of exiting unemployment after having been unemployed for 6 and 12 months)									
6 months	-0.472*** (0.098)	-0.071	-1.934*** (0.201)	-0.290	-0.107 (0.104)	-0.017	-0.068*** (0.014)	-0.118*** (0.042)	-0.007 (0.051)
12 months	-0.404*** (0.082)	-0.095	-1.081*** (0.107)	-0.252	-0.082 (0.09)	-0.019	-0.105*** (0.020)	-0.277*** (0.090)	-0.001 (0.091)
Durable exit (difference of the effect of distance on the probability of not having been unemployed the 6 and 12 months that followed an exit from unemployment)									
6 months	-0.449*** (0.153)	-0.029	-1.487*** (0.229)	-0162	-0.113 (0.160)	0.002	-0.033 (0.074)	-0.179*** (0.056)	-0.001 (0.021)
12 months	-0.124 (0.083)	-0.027	-0.684*** (0.115)	-0.149	0.100 (0.09)	0.022	-0.038 (0.024)	-0.279*** (0.089)	-0.001 (0.021)
Part2 – Control 2 / Treated 2									
Gross exit (difference of the effect of distance on the probability of exiting unemployment after having been unemployed for 6 and 12 months)									
6 months	0.015 (0.206)	0.002	-0.526 (0.520)	-0.065	0.006 (0.213)	0.001	0.013 (0.026)	-0.022 (0.031)	0.021 (0.034)
12 months	0.079 (0.172)	0.018	-0.010 (0.239)	-0.002	0.053 (0.185)	0.012	0.027 (0.045)	0.004 (0.010)	0.0150 (0.048)
Durable exit (difference of the effect of distance on the probability of not having been unemployed the 6 and 12 months that followed an exit from unemployment)									
6 months	-0.114 (0.246)	-0.011	-1.106. (0.767)	-0.007	-0.075 (0.252)	-0.009	0.004 (0.010)	-0.024 (0.025)	0.014 (0.034)
12 months	-0.022 (0.173)	-0.005	-0.246 (0.265)	-0.047	-0.007 (0.184)	-0.002	-0.002 (0.005)	-0.022 (0.028)	-0.002 (0.009)

Source: FHS-Pôle Emploi, first spell per individual. Number of observations: Covariates include: gender, diploma (3 levels), age, and time since the last unemployment spell, job experience and quarter of entrance in unemployment, LPEA fixed effect

**** significant at 1%, ** at 5%, * at 10%. Standard errors are given in parenthesis below the estimate. For the propensity score matching standard errors are obtain by using bootstrapping with 200 replications.*

To sum up, we find that controlling for individual characteristics of jobseekers and for institutional effects, there is evidence of a worker/agency spatial mismatch. However, we find evidence of short-term institutional detrimental effects of the

5 Conclusion

In this paper, we try to provide evidence on an apparent contradiction between the spatial mismatch theory and past empirical evidence on the negative effect of distance to LPEAs on the employment prospects of jobseekers.

To do so, we combine exhaustive individual datasets on jobseekers, agencies and caseworkers, which allows us to work on actual individual unemployment durations (and not on aggregated unemployment rates computed at the census tract level) and to control for an extensive set of variables. We also take advantage of a quasi-experiment created by a zoning modification in the catchment area of a LPEA in the French region of Lyon. We use two different econometric strategies (difference in difference and matching by propensity score) that allows us to assess 'pure' distance effects on the probability of exiting unemployment. We find evidence that when controlling for individual characteristics, and institutional effects, distance to agencies does not affect the matching process efficiency.

In terms of public policy, our results suggest that accessibility to LPEAs has no or little effects on the probability to exit unemployment. An explanation consistent with the spatial mismatch literature could be that travelling to one's LPEA is compulsory due to the activation of labour market public policies: since jobseekers cannot de facto arbitrate between transportation costs and benefits from a travel to their LPEA, distance does not create added frictions in the matching process. The expensive maintenance of a very dense network of LPEAs does not appear to be a very efficient public policy, which gives credit to the position of the Cour des Comptes on the re-sizing of the French public employment agencies network (Cour des Comptes, 2015). On the contrary, echoing Launoy and Wälde (2015), a re-sizing of the public employment agencies network could have a positive effect on unemployment.

This being said, three important issues immediately arise.

First, we have found that the creation of the Belleville agency did have a transitory detrimental impact on the employment prospects of jobseekers: this means that the long-term benefits from a re-sizing of the French public employment agency network should be balanced with the short-term adverse institutional effects. In any case, this suggests that any reform should be timed with a reduction of the caseworkers' caseload, i.e. should not take place in times of high unemployment.

Second, welfare issues could be problematic since jobseekers must endure the travel cost to their agency, however far it is located from their home. Any loosening of the LPEA network should come with a compensation for the jobseekers who will incur the greatest cost (financial compensation, creation of specific shuttles to distant locations, reinforced phone or Internet meetings, etc.).

Second, our evidence suggests that distance to LPEA does not create and added source of friction in the job search process. This goes against the idea of a spatial mismatch between agencies and jobseekers. However, it is important to stress that this does not mean that the spatial distribution of agencies has no spatial mismatch effects whatsoever. In the jobseeker/agency/jobs relationship, our paper examined the effect of the jobseeker/agency distance, but we say nothing about possible harmful effects of a too great agency –to-jobs distance. In fact, from the spatial mismatch theory's perspective, distance to jobs should be problematic for the agencies' efficiency, since it would lessen the quality of the information on jobs collected by the agencies. In terms of public policy, further research on this matter could provide evidence in favour of a dense network of agencies – density being measured relatively to jobs and not jobseekers.

6 References

Allard SW (2004) *Access to social services: The changing urban geography of poverty and service provision. Metropolitan policy program, survey series*. The Brookings Institution, Washington DC

Allard SW (2009) *Out of reach: place, poverty, and the new American welfare state*. Yale University Press, New Haven

Allard SW, Danziger S (2003) Proximity and opportunity: How residence and race affect the employment of welfare recipients. *Housing Policy Debate*13: 675–700

Altavilla C, Caroleo FE (2013) Asymmetric Effects of National-based Active Labour Market Policies. *Regional Studies* 47 (9): 1482-1506.

Bech LS (2015) *The Relationship between Caseworkers and Unemployed Workers.* PhD Thesis, School of Business and Social Sciences, Aarhus University.

Becker SO, Ichino A (2002) Estimation of average treatment effects based on propensity scores. *Stata Journal* 2 (4): 358–377.

Behaghel L, Crépon B, Gurgand M (2009) Évaluation d'impact de l'accompagnement des demandeurs d'emploi par les opérateurs privés de placement et le programme Cap vers l'entreprise. *Document de travail de la DARES* (october)

Behncke S, Frölich M, Lechner M (2010a) Unemployed and their caseworkers: should they be friends or foes?. *Journal of the Royal Statistical Society Series A* 173 (1): 67-92

Behncke S, Frölich M, Lechner M (2010b) A Caseworker Like Me. Does The Similarity Between The Unemployed and Their Caseworkers Increase Job Placements?. *Economic Journal* 120 (549): 1430-1459

Bell S, Orr L (2002) Screening (and creaming?) applicants to job training programs: the AFDC homemaker-home health aide demonstrations. *Labour Economics* 9 (2): 279-301

Bernardi V (2013) Les sortants des listes de Pôle Emploi en 2011: stabilisation sur un an du taux de sortie pour reprise d'emploi. *DARES Analyses* n°2013-04

Bernardi V, Poujouly C (2011) Les sortants des listes de demandeurs d'emploi inscrits à pôle emploi, en Mars 2011. *DARES Analyse* n°2011-071

Bielefeld W, Murdoch J (2004) The locations of nonprofit organizations and their for-profit counterparts: An exploratory analysis. *Nonprofit and Voluntary Sector Quarterly* 33: 221–246

Biewen M, Fitzenberger B, Osikominu A, Paul M (2014) The Effectiveness of Public-Sponsored Training Revisited: The Importance of Data and Methodological Choices. *Journal of Labor Economics* 32 (4): 837-897

Blundell R, Costa Dias M (2000) Evaluation Methods for Non-Experimental Data. *Fiscal Studies* 21 (4): 427-468

Blundell R, Costa Dias M, Costas M, Van Reenen J (2004) Evaluating the Employment Impact of a Mandatory Job Search Program. *Journal of the European Economic Association* 2 (4): 569-606

Boone J, van Ours JC (2009) Bringing Unemployed Back to Work: Effective Active Labor Market Policies. *De Economist* 157 (3): 293-313

Caliendo M, Cobb-Clark DA, Uhlendorff A (2105) Locus of Control and Job Search Strategies. *Review of Economics and Statistics* 97 (1): 88-103

Card D, Kluve J, Weber A (2015) What Works? A Meta Analysis of Recent Active Labor Market Program Evaluations. *IZA Working paper* (# 9236)

Card D, Kluve J, Weber A (2015) What Works? A Meta Analysis of Recent Active Labor Market Program Evaluations. *Ruhr Economic Papers* n°572

Cour des Comptes (2015) *Pôle Emploi à l'épreuve du chômage de masse*, Rapport Public Thématique, 176 p. [link]

Crépon B, Dejemeppe M, Gurgand M (2005) Counseling the Unemployed: Does It Lower Unemployment Duration and Recurrence?. *IZA Working Papers* n°1796

Delattre E, Choffel P (2003). Effets locaux et urbains sur les parcours de chômage Une analyse microéconométrique sur le panel de chômeurs TDE-MLT. *DARES Premières Synthèses* n°43.1

Détang-Dessandre C, Gaigné C (2009) Unemployment duration, city size, and the tightness of the labor market. *Regional Science and Urban Economics* 39: 266–276

Duguet E, L'Horty Y, Sari F (2009) Sortir du chômage en Île-de-France. Disparités territoriales, spatial mismatch et ségrégation résidentielle. *Revue Économique* 60 : 979-1010

Dujardin C, Selod H, Thomas I (2008) Residential Segregation and Unemployment: The Case of Brussels. *Urban Studies* 45(1): 89-113

Ferracci M, Jolivet G, van den Berg G (2014) Evidence of Treatment Spillovers within Markets. *Review of Economics & Statistics* 96 (5): 812-823

Fontaine F, Malherbet F (2013) *Accompagner les demandeurs d'emploi.* Les presses de Sciences Po, Paris

Fontaine M, Le Barbanchon T (2012) Evaluation du suivi mensuel personnalisé mis en place par l'ANPE en 2006, *Document d'Etudes DARES* (n°175, december)

Fougère D, Kamionka T, Prieto A (2010) L'efficacité des mesures d'accompagnement sur le retour à l'emploi. *Revue Economique* 61 (3): 599-612

Fougère D, Pradel J, Roger M (2009) Does the public employment service affect search effort and outcomes?. *European Economic Review 5*3 (7): 846-869

Frölich M, Lechner M (2010) Exploiting Regional Treatment Intensity for the Evaluation of Labor Market Policies. *Journal of the American Statistical Association* 105 (491): 1014-1029

Gobillon L, Selod H., Zenou Y (2007) The Mechanisms of Spatial Mismatch. *Urban Studies* 44 (12) :2401–2427

Hainmueller J, Hofman B, Krug G, Wolf K (2011) Do Lower Caseloads Improve the Effectiveness of Active Labor Market Policies? New Evidence from German Employment Offices. *LASER Discussion Paper* (2011/52)

Hilal M (2010) ODOMATRIX. Calcul de distances routières intercommunales. *Cahier des Techniques de l'INRA* 2010: 41-63

Hill CJ (2006) Casework Job Design and Client Outcomes in Welfare-to-Work Offices. *Journal of Public Administration Research and Theory* 16 (2): 263–288

Houston D (2005) Employability, Skills Mismatch and Spatial Mismatch in Metropolitan Labour Markets. *Urban Studies* 42(2): 221-243.

Iborra M (2013) Pôle emploi et le service public de l'emploi. *Rapport de la Commission Des Affaires Sociales de l'Assemblée Nationale* n°1107

Joassart-Marcelli P, Giordano A (2006) Does local access to employment services reduce unemployment? A GIS analysis of One-Stop Career Centers. *Policy Sciences* 39 (4): 335-359

Joassart-Marcelli P, Wolch J (2003) The intra metropolitan geography of poverty and the non profit sector in Southern California. *Non profit and Voluntary Sector Quarterly* 32: 70–96

Kain JF (1968) Housing segregation, Negro employment and metropolitan decentralization. *Quarterly Journal of Economics* 82 (2): 175–197

Kluve J (2010) The effectiveness of European active labor market programs. *Labour Economics* 17 (6): 904-918

Koning P (2009) The effectiveness of Public Employment Service workers in the Netherlands. *Empirical Economics* 37 (2): 393-409

Korsu E, Wenglenski S (2010) Job Accessibility, Residential Segregation, and Risk of Long-term Unemployment in the Paris Region. *Urban Studies* 47(11): 2279-2324

Kuhn P, Mansour H (2014) Is Internet Job Search Still Ineffective? *The Economic Journal* 124 (581): 1213-1233

Lagerstrom J (2011) How important and why are caseworkers? New evidence from Swedish employment offices. *Institute for Labor market Policy Evaluation Working Paper* n°2011-10

Launov A, Wälde K (2015) The Employment Effect of Reforming a Public Employment Agency. *European Economic Review*, In Press, Available online 5 September 2015

Le Monde (2013) Quel portefeuille pour les conseillers dans votre agence Pôle emploi?. *Le Monde* n°21.09.2013

Martin JP, Grubb D (2001) What works and for whom: a review of OECD countries' experiences with active labour market policies. *Office of Labour Market Policy EvaluationWorking Paper* n°2001-14

Matas A, Raymond JL, Roig JL (2009) Car ownership and access to jobs in Spain. *Transportation Research Part A* 43: 607-617

Merlin O (2015) Montreuillois: l'éloignement des Pôles emploi complique la vie des chômeurs. *La Voix du Nord* n°12/05/2015

OECD (2007) Activating the Unemployed: What Countries Do. In: OECD (ed) *OECD Employment Outlook 2007, Chapter* 5

Parent G, Sautory O, Desplatz R (2013) L'accompagnement des demandeurs d'emploi: enseignements et évaluations. *Document d'études DARES* (n°178, december).

Patacchini E, Zenou Y (2005) Spatial mismatch, transport mode and search decisions in England. *Journal of Urban Economics* 58: 62-90

Plesca M (2006) *A General Equilibrium Evaluation of the Employment Service*. Mimeo, University of Guelph

Pôle Emploi, Ministère de l'Emploi, du Travail, de la Formation Professionnelle et du Dialogue Social and Unédic (2014) *Convention tripartite Etat – Unédic – Pôle emploi 2015*-2018

Rosholm M (2014) Do case workers help the unemployed? Evidence for making a cheap and effective twist to labor market policies for unemployed workers. *IZA World of Labor n°*2014 : 72

Rubin DB (1977) Assignment to treatment group on the basis of a Covariate. *Journal of Educational Statistics* 2: 1-26

Suárez Cano P, Cueto Iglesias B, Mayor Fernández M, Henar Salas-Olmedo M (2015) People-and Place-Sensitive Perspective for Studying the Potential for the Role of Job centers. *SSRN Working Paper* (n°2554128)

Suárez Cano P, Mayor Fernández M, Cueto Iglesias B (2012a) How important is access to employment offices in Spain? An urban and non-urban perspective. *Investigaciones Regionales* 21: 119-140

Suárez Cano P, Mayor Fernández M, Cueto Iglesias B (2012b) The accessibility to employment offices in the Spanish labour market. *Papers in Regional Science* 91 (4): 823-848

van der Klaauw B, van Ours JC (2003) From welfare to work: does the neighborhood matter?. *Journal of Public Economics* 87 (5-6): 957-985

Wathen MV, Allard SW (2014) Local Nonprofit Welfare Provision: the United States and Russia. *Public Administration Issues*, Special Issue: 7-28

Zenou Y. (2009) Search in cities. *European Economic Review* 53: 607-624

7 Appendixes

7.1 Descriptive Statistics

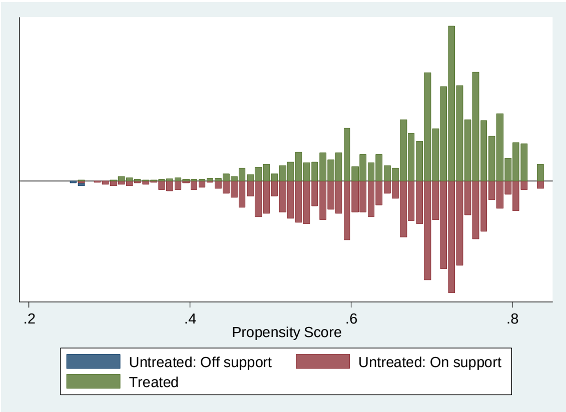
Table A. Descriptive Statistics												
	Treated 1			Control 1			Treated 2			Control 2		
	All	**Before**	**After**	**All**	**Before**	**After**	**All**	**Before**	**After**	**All**	**Before**	**After**
Dist. to LPEA (min)	16.3	24.8	12.1	18.0	17.6	18.2	15.8	26.0	10.9	17.8	21.1	16.0
5 months	10.0%	15.5%	7.2%	10.7%	12.6%	9.6%	9.9%	15.8%	6.9%	10.3%	14.7%	8.0%
6 months	18.1%	26.3%	14.0%	19.1%	22.3%	17.4%	18.2%	26.6%	14.1%	17.9%	25.4%	13.9%
7 months	25.2%	35.6%	19.9%	26.5%	31.3%	24.1%	25.1%	36.1%	19.7%	25.4%	34.2%	20.7%
8 months	31.0%	43.4%	24.8%	33.0%	39.0%	29.9%	31.1%	44.1%	24.7%	30.9%	41.4%	25.3%
9 months	36.9%	51.7%	29.6%	38.8%	45.5%	35.4%	36.9%	51.8%	29.6%	37.0%	51.1%	29.4%
10 months	41.3%	57.0%	33.5%	43.7%	50.8%	39.9%	41.1%	56.8%	33.4%	42.2%	57.7%	34.0%
11 months	45.7%	61.4%	37.8%	47.9%	55.4%	44.0%	45.5%	61.7%	37.6%	46.1%	60.3%	38.5%
12 months	49.3%	64.6%	41.7%	51.6%	59.5%	47.5%	49.2%	65.1%	41.5%	49.6%	63.2%	42.3%
Mean unemployment spell (months)	15.302	11.353	17.275	14.955	12.700	16.126	15.313	11.247	17.301	15.266	11.670	17.189
Long run unemployment	50.7%	35.4%	58.4%	48.6%	40.7%	52.7%	50.8%	34.9%	58.6%	50.5%	36.8%	57.8%
No unemployment days in the previous 2 years	80.5%	72.5%	84.6%	79.9%	73.8%	83.1%	80.6%	73.1%	84.3%	80.4%	70.7%	85.5%
Unemployment months in the previous 2 years	1.442	2.145	1.091	1.839	2.613	1.437	1.426	2.101	1.096	1.494	2.278	1.074
[20-25[years old	21.8%	20.0%	22.6%	23.8%	23.4%	24.1%	21.8%	19.8%	22.7%	21.7%	20.5%	22.3%
[25-35[years old	29.5%	31.8%	28.4%	29.2%	29.8%	28.9%	29.6%	32.5%	28.2%	29.3%	29.6%	29.1%
[35-45[years old	26.0%	27.0%	25.5%	23.8%	24.8%	23.3%	25.9%	26.0%	25.8%	26.3%	30.0%	24.4%
[45-55[years old	22.7%	21.2%	23.5%	23.2%	22.1%	23.8%	22.7%	21.6%	23.2%	22.7%	19.9%	24.2%
Males	42.2%	38.7%	43.9%	45.4%	41.7%	47.3%	43.3%	40.3%	44.8%	38.5%	33.9%	40.9%
Education : Superior	18.3%	19.3%	17.8%	19.7%	18.9%	20.1%	17.9%	18.1%	17.8%	19.8%	22.8%	18.1%
Education : A-level	20.6%	19.9%	20.9%	19.6%	19.3%	19.8%	19.5%	19.7%	19.4%	24.1%	20.2%	26.1%
Education: < A-level	61.1%	60.9%	61.2%	60.7%	61.7%	60.1%	62.6%	62.1%	62.9%	56.2%	57.0%	55.7%
Entrance in unemployment in												
Sept. –Oct. –Nov.	26.0%	27.4%	25.3%	27.5%	28.5%	26.9%	26.0%	27.2%	25.3%	26.2%	28.0%	25.3%
Dec. –Jan. –Feb.	27.4%	27.1%	27.6%	27.5%	28.6%	27.0%	27.5%	28.0%	27.2%	27.4%	24.4%	28.9%
March- April -May	22.6%	23.8%	22.0%	22.6%	22.6%	22.6%	22.7%	23.2%	22.4%	22.4%	25.4%	20.7%
Jun- July -August	23.9%	21.7%	25.0%	22.4%	20.3%	23.5%	23.9%	21.6%	25.0%	24.1%	22.1%	25.1%
No experience in the researched job (Nexpe)	21.8%	29.8%	17.8%	23.3%	29.4%	20.2%	22.7%	31.3%	18.5%	19.0%	25.1%	15.7%
Relative caseload ratio variation (%)	1.137	1.003	1.205	1.031	1.000	1.047	1.138	1.004	1.204	1.135	0.999	1.207
N obs.	3,689	1,229	2,46	37,755	12,911	24,844	2,808	922	1,886	881	307	574
Source: FHS-Pôle Emploi, first spell per individual.												

7.2 Ex-post controls for matching methodology

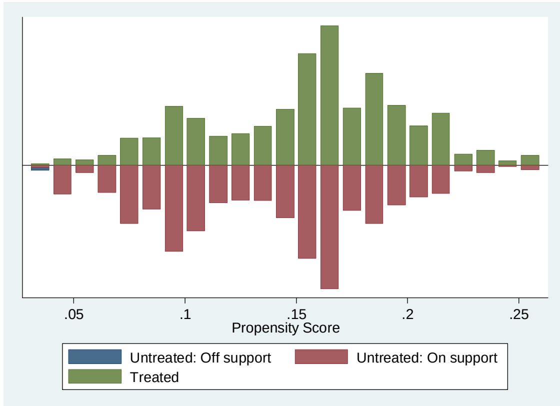
The assumptions of overlap and covariate balance can be check after the estimations. Figure A presents the overlap charts used to assess whether propensity scores met the overlap assumption.

Figure A. Histograms of propensity score for the treated and control groups before and after the creation of the Belleville LPEA

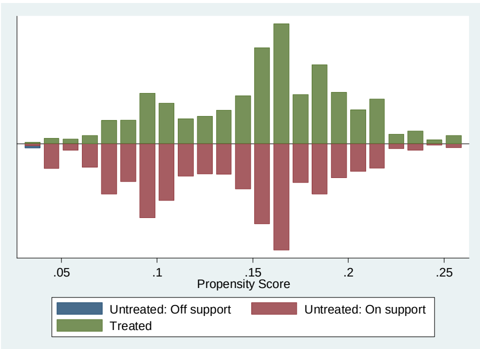
Treated after versus treated before



Treated after versus control before



Treated after versus control after



Source: FHS-Pôle Emploi, first spell per individual.

Table B. Means of the covariates after matching for the three control groups

	T=1 and t=1	T=1 and t=0			T=0 and t=0			T=0 and t=1		
	mean	mean	Difference	(P value)	mean	Difference	(P value)	mean	Difference	(P value)
[25-35[years old	0.284	0.290	-0,006	0.639	0.292	-0,008	0.518	0.289	-0,005	0.699
[35-45[years old	0.255	0.268	-0,013	0.313	0.264	-0,009	0.468	0.237	0,018	0.140
[45-55[years old	0.235	0.242	-0,007	0.537	0.238	-0,004	0.749	0.240	-0,006	0.627
Men	0.439	0.430	0,009	0.530	0.423	0,016	0.267	0.469	-0,030	0.035
Education : Superior	0.178	0.181	-0,003	0.799	0.188	-0,010	0.383	0.196	-0,018	0.110
Superior x [25-35[years old	0.077	0.075	0,002	0.785	0.082	-0,005	0.524	0.081	-0,004	0.640
Superior x [35-45[years old	0.047	0.050	-0,003	0.578	0.048	-0,002	0.781	0.049	-0,002	0.734
Superior x [45-55[years old	0.020	0.021	0,000	0.944	0.022	-0,002	0.600	0.025	-0,005	0.277
Education : A-level	0.209	0.208	0,002	0.895	0.202	0,008	0.513	0.204	0,005	0.648
A-level x [25-35[years old	0.072	0.069	0,002	0.767	0.073	-0,001	0.891	0.069	0,003	0.708
A-level x [35-45[years old	0.042	0.041	0,000	0.933	0.040	0,002	0.778	0.039	0,003	0.610
A-level x [45-55[years old	0.032	0.035	-0,003	0.520	0.030	0,001	0.775	0.031	0,001	0.890
Never Unemployment in previous 2 years	0.081	0.080	0,001	0.911	0.088	-0,007	0.391	0.079	0,002	0.845
Unemployment in the previous 2 years less than 6 months	0.074	0.074	-0,001	0.942	0.076	-0,002	0.767	0.084	-0,011	0.171
Inter in unemployment in December -January-February	0.253	0.258	-0,005	0.700	0.269	-0,016	0.208	0.269	-0,016	0.212
Inter in unemployment in March- April -May	0.276	0.278	-0,002	0.862	0.287	-0,011	0.382	0.271	0,005	0.702
Inter in unemployment in Jun- July -August	0.220	0.220	0,000	0.972	0.229	-0,009	0.469	0.228	-0,008	0.521
No experience in the researched job (Nexpe)	0.178	0.177	0,001	0.929	0.186	-0,007	0.495	0.190	-0,011	0.316
[25-35[*Nexpe	0.048	0.049	-0,001	0.834	0.049	-0,002	0.792	0.050	-0,003	0.667
[35-45[*Nexpe	0.034	0.036	-0,002	0.758	0.036	-0,002	0.754	0.031	0,003	0.570
[45-55[*Nexpe	0.025	0.022	0,003	0.511	0.026	-0,001	0.813	0.027	-0,002	0.670
N obs	2,460	1,229			15,174			29,038		

Source: FHS-Pôle Emploi, first spell per individual.

Table B presents means of matching propensity score of control groups treated versus non-treated and before versus after. Our results reveal a high levels of covariate balance between treatment and matched comparison groups. All standardized differences produced coefficients with absolute values less than 0.1 and the p-values are all over the 0.15 threshold.

Finally to test the sensitivity of our results to possible unobserved variables we use the usual Mantel-Haenszel procedures (Mantel and Haenszel, 1959; Becker and Caliendo, 2007). In fact, propensity score matching gives biased estimates if unobserved characteristics influence either the probability to be treated or the probability to be observed before the arrival of the new LPEA and the outcome (the probability to exit unemployment).

If one assume that the unobserved covariate is a dummy variable and α the influence of this variable on the participation decision. If α=0 we have no selection bias. Conversely if $\alpha \neq 0$ we have either a positive unobserved selection or a negative one. Q+ is a test given that we overestimated the treatment effect and Q- is the case where we have underestimated the treatment effect.

Note for $\mathbf{e^{\alpha} = 1}$ the case with no unobserved bias the treatment effect are significant for the three control groups. When $\mathbf{e^{\alpha}}$increase similar individuals in terms of observable covariates could differ in their odds to be member of the treated group.

According to table C, even for a large value of e^{α} the treatment effect stay significant for the first group of control (T=1 and t=1 versus T=1 and t=0). For the second group (T=1 and t=1 versus T=0 and t=0) the treatment effect becomes insignificant when $\mathbf{e^{\alpha}}$reach 1.95. This threshold is 1.2 for the third group (T=1 and t=1 versus T=0 and t=1).

Table C. Mantel-Haenszel statistic indicating the significance of the treatment for different values.

	T=1 and t=1 versus T=1 and t=0				T=1 and t=1 versus T=0 and t=0				T=1 and t=1 versus T=0 and t=1			
e^{α}	Q+	Q-	p+	p-	Q+	Q-	p+	p-	Q+	Q-	p+	p-
1	13,02	13,02	0,00	0,00	16,29	16,29	0,00	0,00	5,458	5,458	0,000	0,000
1,05	13,72	12,32	0,00	0,00	17,43	15,16	0,00	0,00	6,605	4,314	0,000	0,000
1,1	14,39	11,66	0,00	0,00	18,52	14,09	0,00	0,00	7,701	3,225	0,000	0,001
1,15	15,03	11,03	0,00	0,00	19,57	13,07	0,00	0,00	8,751	2,185	0,000	0,014
1,2	15,65	10,42	0,00	0,00	20,58	12,09	0,00	0,00	9,759	1,191	0,000	0,117
1,25	16,25	9,85	0,00	0,00	21,55	11,16	0,00	0,00	10,730	0,238	0,000	0,406
1,3	16,82	9,29	0,00	0,00	22,49	10,27	0,00	0,00	11,665	0,635	0,000	0,263
1,35	17,37	8,76	0,00	0,00	23,40	9,41	0,00	0,00	12,569	1,517	0,000	0,065
1,4	17,91	8,25	0,00	0,00	24,28	8,58	0,00	0,00	13,442	2,366	0,000	0,009
1,45	18,43	7,76	0,00	0,00	25,14	7,79	0,00	0,00	14,289	3,187	0,000	0,001
1,5	18,93	7,29	0,00	0,00	25,97	7,03	0,00	0,00	15,109	3,980	0,000	0,000
1,55	19,42	6,83	0,00	0,00	26,78	6,29	0,00	0,00	15,906	4,748	0,000	0,000
1,6	19,90	6,38	0,00	0,00	27,56	5,57	0,00	0,00	16,681	5,493	0,000	0,000
1,65	20,36	5,96	0,00	0,00	28,33	4,88	0,00	0,00	17,435	6,216	0,000	0,000
1,7	20,81	5,54	0,00	0,00	29,08	4,21	0,00	0,00	18,170	6,918	0,000	0,000
1,75	21,25	5,14	0,00	0,00	29,81	3,56	0,00	0,00	18,886	7,602	0,000	0,000
1,8	21,67	4,75	0,00	0,00	30,52	2,93	0,00	0,00	19,585	8,267	0,000	0,000
1,85	22,09	4,36	0,00	0,00	31,22	2,31	0,00	0,01	20,267	8,915	0,000	0,000
1,9	22,50	3,99	0,00	0,00	31,91	1,72	0,00	0,04	20,934	9,547	0,000	0,000
1,95	22,90	3,63	0,00	0,00	32,58	1,13	0,00	0,13	21,587	10,164	0,000	0,000
2	23,29	3,28	0,00	0,00	33,23	0,57	0,00	0,29	22,226	10,767	0,000	0,000

Source: FHS-Pôle Emploi, first spell per individual.

Table D1. Difference in difference and matching results – Treated & Control 1						
	Difference-in-difference				**Matching**	
	All years		**2010-2011**		**All years**	**2010-2011**
	Coef (std)	*Marginal effect*	*Coef (std)*	*Marginal effect*	*Marginal treatment effect on treated*	*Marginal treatment effect on treated*
GROSS EXIT						
5 months	-0.537***	-0.050	-0.178	-0.018	-0.049***	-0.011
	(0.123)		(0.129)		(0.011)	(0.012)
6 months	-0.472***	-0.071	-0.107	-0.017	-0.068***	-0.007
	(0.098)		(0.104)		(0.014)	(0.017)
7 months	-0.418***	-0.079	-0.050	-0.010	-0.078***	0.003
	(0.088)		(0.095)		(0.016)	(0.019)
8 months	-0.402***	-0.085	-0.045	-0.010	-0.085***	0.010
	(0.084)		(0.09)		(0.016)	(0.019)
9 months	-0.456***	-0.103	-0.116	-0.027	-0.103***	-0.004
	(0.082)		(0.089)		(0.017)	(0.020)
10 months	-0.477***	-0.111	-0.127	-0.030	-0.115***	-0.009
	(0.081)		(0.089)		(0.018)	(0.021)
11 months	-0.448***	-0.106	-0.100	-0.024	-0.112***	-0.006
	(0.082)		(0.089)		(0.020)	(0.021)
12 months	-0.404***	-0.095	-0.082	-0.019	-0.105***	-0.001
	(0.082)		(0.09)		(0.020)	(0.022)
DURABLE EXIT						
5 months	-0,449***	-0,029	-0,113	-0,008	-0,030	-0,010
	(0,153)		(0,16)		(0.080)	(0.083)
6 months	-0,270***	-0,029	0,014	0,002	-0,033	-0,001
	(0,116)		(0,124)		(0.074)	(0.021)
7 months	-0,206**	-0,029	0,088	0,013	-0,033	0,011
	(0,102)		(0,109)		(0.075)	(0.033)
8 months	-0,194***	-0,032	0,072	0,012	-0,035	0,018
	(0,094)		(0,101)		(0095)	(0.044)
9 months	-0,198***	-0,036	0,052	0,010	-0,040	0,016
	(0,089)		(0,097)		(0.082)	(0.052)
10 months	-0,194***	-0,038	0,048	0,010	-0,046	0,012
	(0,086)		(0,094)		(0.072)	(0.033)
11 months	-0,158**	-0,033	0,084	0,018	-0,043	0,018
	(0,084)		(0,092)		(0.055)	(0.027)
12 months	-0,124	-0,027	0,100	0,022	-0,038	0,023
	(0,083)		(0,09)		(0.024)	(0.054)
Nb. obs	41,444		27,763		41,444	27,763

Source: FHS-Pole Emploi, first spell per individual.Covariates include: gender, diploma (3 levels), age, and time since the last unemployment spell, job experience and quarter of entrance in unemployment, LPEA fixed effect.
**** significant at 1%, ** at 5%, * at 10%. Standard errors are given in parenthesis below the estimate. For the propensity score matching standard errors are obtain by using bootstratpping with 200 replications. .*

Table D2. Difference in difference and matching results – Treated & Control 2						
	Difference-in-difference				**Matching**	
	All years		**2010-2011**		**All years**	**2010-2011**
	Coef (std)	*Marginal effect*	*Coef (std)*	*Marginal effect*	*Marginal treatment effect on treated*	*Marginal treatment effect on treated*
GROSS EXIT						
5 months	0.209	0.018	0.190	0.021	0.021	0.038
	(0.259)		(0.263)		(0.018)	(0.026)
6 months	0.015	0.002	0.006	0.001	0.013	0.021
	(0.206)		(0.213)		(0.026)	(0.034)
7 months	0.108	0.019	0.055	0.011	0.036	0.034
	(0.185)		(0.194)		(0.028)	(0.040)
8 months	0.101	0.020	0.042	0.009	0.028	0.016
	(0.176)		(0.186)		(0.032)	(0.043)
9 months	-0.017	-0.004	-0.073	-0.017	0.001	-0.015
	(0.172)		(0.183)		(0.039)	(0.044)
10 months	-0.040	-0.009	-0.123	-0.029	0.001	0.000
	(0.171)		(0.183)		(0.040)	(0.000)
11 months	0.066	0.015	-0.002	0.000	0.030	-0.020
	(0.171)		(0.183)		(0.042)	(0.047)
12 months	0.079	0.018	0.053	0.012	0.027	0.0150
	(0.172)		(0.185)		(0.045)	(0.048)
DURABLE EXIT						
5 months	-0,113	-0,006	-0,114	-0,008	0,003	0,009
	(0,32)		(0,323)		(0.010)	(0.310)
6 months	-0,114	-0,011	-0,075	-0,009	0,004	0,014
	(0,246)		(0,252)		(0.010)	(0.034)
7 months	0,028	0,004	0,047	0,007	0,020	0,025
	(0,213)		(0,221)		(0.089)	(0.044)
8 months	-0,010	-0,002	0,021	0,004	0,011	0,012
	(0,199)		(0,208)		(0.078)	(0.055)
9 months	-0,087	-0,015	-0,056	-0,011	-0,008	-0,009
	(0,187)		(0,197)		(0.012)	(0.078)
10 months	-0,070	-0,013	-0,073	-0,015	-0,001	-0,010
	(0,181)		(0,192)		(0.005)	(0.021)
11 months	-0,011	-0,002	-0,016	-0,004	0,007	0,001
	(0,176)		(0,187)		(0.012)	(0.010)
12 months	-0,022	-0,005	-0,007	-0,002	-0,002	-0,002
	(0,173)		(0,184)		(0.005)	(0.009)
Nb. obs	3,689		2,809		3,689	2, 809

Source: FHS-Pole Emploi, first spell per individual. Number of observations: Covariates include: gender, diploma (3 levels), age, and time since the last unemployment spell, job experience and quarter of entrance in unemployment, LPEA fixed effect.

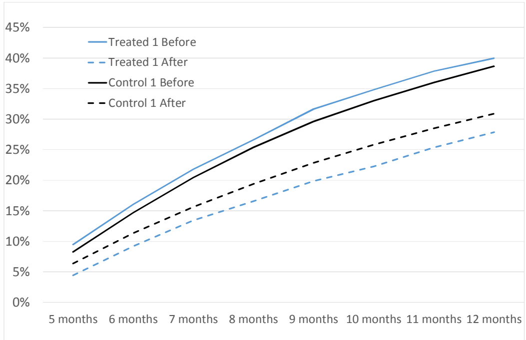
**** significant at 1%, ** at 5%, * at 10%. Standard errors are given in parenthesis below the estimate. For the propensity score matching standard errors are obtain by using bootstratpping with 200 replications. .*

7.3 Gross exits from unemployment

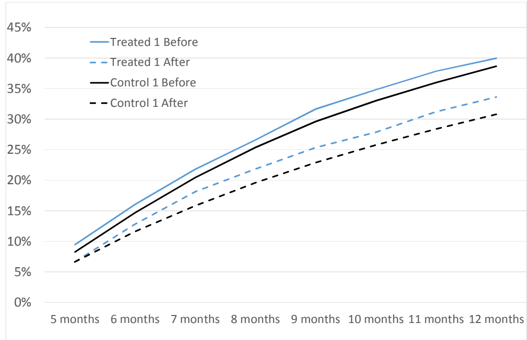
Figure B. % of workers who have not been unemployed in the 5 to 12 months after exiting unemployment (durable exits)

Part 1 – Control 1 versus Treated 1

All years

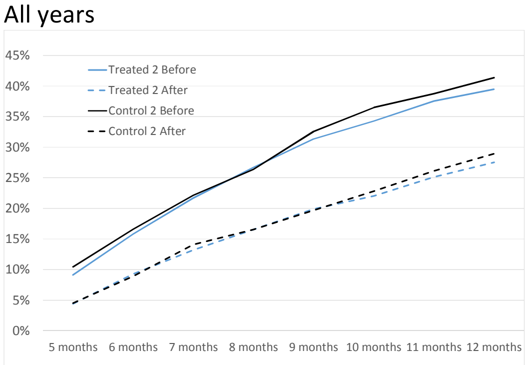


2010-2011



Part 2 – Control 2 versus Treated 2

All years



2010-2011

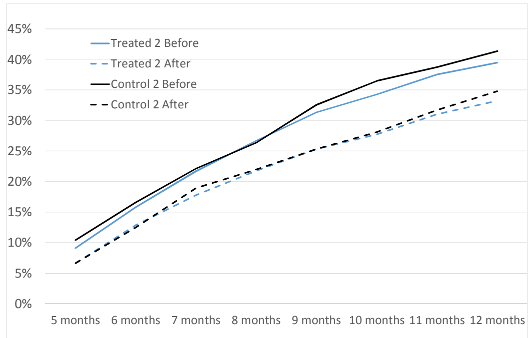
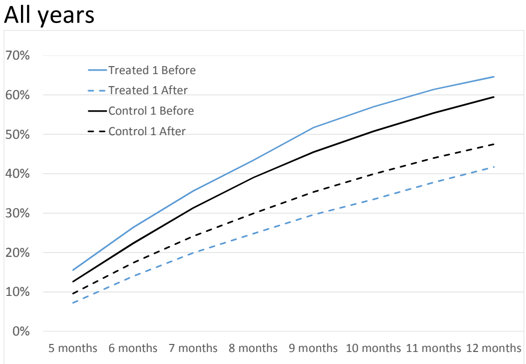


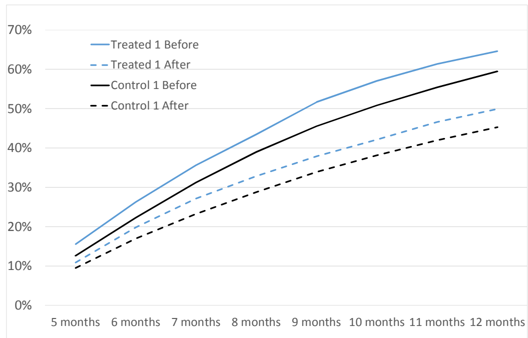
Figure C. % of workers who have exited unemployment after 5 to 12 months long unemployment spells (gross exits)

Part 1 – Control 1 versus Treated 1

All years

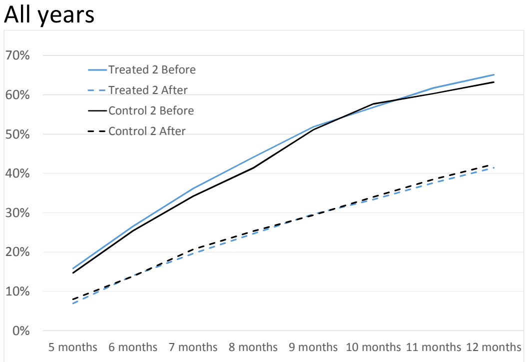


2010-2011

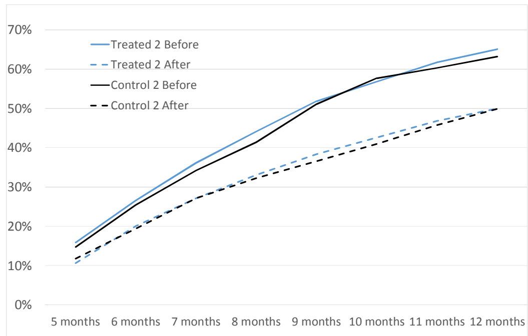


Part 2 – Control 2 versus Treated 2

All years



2010-2011



Source: FHS-Pôle Emploi, first spell per individual.

TEPP Working Papers 2015

15_5. Transmission of vocational skills at the end of career: horizon effect and technological of organisational change
Nathalie greenan, Pierre-Jean Messe

15_4. Protecting biodiversity by developing bio-jobs: A multi-branch analysis with and application on French data
Jean de Beir, Céline Emond, Yannick L'Horty, Laetitia Tuffery

15_3. Profit-sharing and wages: An empirical analysis using French data between 2000 and 2007
Noélie Delahaie, Richard Duhautois

15_2. A meta-regression analysis on intergenerational transmission of education: publication bias and genuine empirical effect
Nicolas Fleury, Fabrice Gilles

15_1. Why are there so many long-term unemployed in Paris
Yannick L'Horty, Florent Sari

The TEPP Institute

The CNRS **Institute for Labor Studies and Public Policies** (the TEPP Institute, FR n°3435 CNRS) gathers together research centres specializing in economics and sociology:

- l'**Equipe de Recherche sur les Marchés, l'Emploi et la Simulation** (Research Team on Markets, Employment and Simulation), **ERMES**, University of Paris II Panthéon-Assas
- the **Centre d'Etudes des Politiques Economiques de l'université d'Evry** (Research Centre focused on the analysis of economic policy and its foundations and implications), **EPEE**, University of Evry Val d'Essonne
- the **Centre Pierre Naville** (Research on Work and Urban Policies), **CPN**, University of Evry Val d'Essonne
- l'**Equipe de Recherche sur l'Utilisation des Données Temporelles en Economie** (Research Team on Use of Time Data in Economics), **ERUDITE**, University of Paris-Est Créteil and University of Paris-Est Marne-la-Vallée
- the **Groupe d'Analyse des Itinéraires et des Niveaux Salariaux** (The Group on Analysis of Wage Levels and Trajectories), **GAINS**, University of the Maine

The TEPP Institute brings together 147 researchers and research professors and 100 PhD students who study changes in work and employment in relation to the choices made by firms and analyse public policies using new evaluation methods.

www.tepp.eu