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Spatial Mismatch through Local Public Employment Agencies? Answers from a French Quasi- Experiment¹

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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 consistent with the spatial mismatch literature, we find no evidence of a worker/agency spatial mismatch, which supports a resizing of the French LPEA network. However, echoing the literature on the institutional determinants of the local public employment agencies' efficiency, we do find detrimental institutional transitory effects.

Mots-clef: spatial mismatch, chômage, service public de l'emploi, expérience quasi naturelle

Résumé

Dans cet article, nous nous interrogeons sur l'effet de la distance aux agences de Pôle Emploi sur la probabilité de sortir du chômage. Pour ce faire, nous utilisons le cadre d'une quasi-expérience naturelle issue de la création d'une nouvelle agence Pôle Emploi à Belleville (Rhônes-Alpes) et nous croisons plusieurs bases de données exhaustives géolocalisées sur les demandeurs d'emploi et les agences Pôle Emploi. Contrairement à des résultats précédents obtenus sur données agrégées et conformément à la littérature théorique sur le spatial mismatch, nous trouvons que la distance à Pôle Emploi n'a pas d'effet sur la probabilité de sortir du chômage, ce qui plaide pour le redimensionnement du réseau des agences Pôle Emploi. Cependant, en écho à la littérature sur les déterminants institutionnels de l'efficacité des agences de Pôle Emploi, nous mettons également en évidence des effets transitoires délétères associés à la création d'une nouvelle agence.

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

JEL codes: C218, J58, R53

1 Introduction

In many countries, the unemployment rates that soared after the 2008 financial crisis are still unprecedentedly high: 10.9% in the Eurozone, 22.2% in Spain, 9.5% in Ireland, 10.4% in France, 12.0% in Italy, and 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 jumped from 390 to 542 days and the proportion of long-term jobseekers increased from 30.3 to 43.3% (Cour des Comptes, 2015).

In parallel, an extensive literature shows the mostly positive effects of the active labour market public policies implemented since the 1990s in OECD countries².

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

First, some papers question caseworkers' marginal efficiency: using Dutch data, Koning (2009) finds that each additional marginal caseworker significantly increases the unemployment outflow rates for short-term jobseekers, reduces 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. Using Swedish data, Lagerstöm (2011) also shows that, when controlling for jobseekers' characteristics, caseworkers have a significant role in 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), managerial governance of agencies (Hill, 2006) or residual effects resulting from a combination of these factors (Suárez Cano *et al.* 2015), have a significant influence on jobseekers' employment prospects. Launoy and Wälde (2015) show that organizing the work of a LPEA in a more

² Active labour market policies focus on affecting the behaviour of jobseekers to improve their job search efficiency and/or employability. Additionally, they involve a "mutual obligations" principle, in which jobseeker benefits are keyed to their compliance with 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 Parent *et al.* 2013; Fontaine and Malherbet, 2013; or Biewen *et al.*, 2014.

efficient way has a much better result for unemployment than creating pecuniary incentives through unemployment assistance benefits.

In parallel, a growing number of papers question 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 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 900³ public employment agencies now exceeds 250 million euros (Cour des Comptes, 2015). Maintaining a dense local network is also a source of deleterious organizational effects, hampering, for example, the specialization of caseworkers. This is particularly the case in France, where 25.3% of the agencies have 15 or fewer caseworkers and 71.0% have 25 or fewer caseworkers (Le Monde, 2013). In public policy terms, examining whether distance to LPEAs affects jobseekers' employment prospects is relevant because it conditions the choice between two alternatives, equalitarian versus Rawlsian policy orientations. In the equalitarian 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; however, compensating schemes are put in place for jobseekers with less access to the agencies' network (e.g., payment of transportation costs, extra monitoring through Internet meetings).

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

In this paper, we rely on exhaustive French administrative geo-located data on both jobseekers' and LPEAs' location and characteristics to examine this issue. Measuring the effect of distance from LPEAs on unemployment has methodological pitfalls due to the potential endogeneity of the distance variable for two reasons. First, the agencies are not randomly distributed in space. Second, in most datasets, the true distance between agencies

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.

and jobseekers is affected by a measurement error bias: jobseekers are arbitrarily assigned to the centroid of their census tract. To address 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.

2 The Spatial Dimension of Public Intermediation in the Labour Market

2.1 Converging Empirical Evidence

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

This concern is typically 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 policy 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 literatures on the spatial distribution of local public goods produced by non-profit organizations, in which converging papers unearth spatial discrepancies between the spatial distribution of the non-profit agencies and the distribution of their clients. Many papers show that when relative needs are considered, the density of non-profit agencies is lower 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 DC 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 that experience limited mobility due to gender or race, such as black and female jobseekers.

Suárez Cano *et al.* (2012a, 2012b, 2015) study, in the Spanish context, the effect of the accessibility of local public employment offices on local unemployment rates varies according to the distribution of three different types of municipalities: large urban, small urban and non-urban. They also find that, at the municipal level, accessibility to employment offices significantly affects jobseekers' labour market outcomes 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 institutions formerly in charge of jobseeker monitoring and control (ANPE) and of the distribution of unemployment benefits (ASSEDIC).

Its creation, coincidental with the 2008 financial crisis, led to institutional dysfunction, without any real managerial reorganization 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 became 873 Pôle Emploi local generalist agencies and 41 agencies specializing in niche labour markets (e.g., entertainment, workers with disabilities). The result is a very dense agency network: in 2009, 80% of jobseekers could reach their LPEA in 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 Court⁴ criticizes the relative dispersion of caseworkers, their reduced specialization and the unnecessary duplication of tasks across agencies (e.g., 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 jobseekers. Moreover, the actual monitoring of jobseekers took up a maximum of only 37% of actual caseworkers' time, and the locating of vacancies up to 7% of their time. In terms of cost, the Court reports that the space occupied by LPEAs showed a 15.9% increase between 2009 and 2013; with 85% of this space 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. Consequently, the Court has mandated a reduction of the number of agencies in

⁴ Charged with conducting financial and legislative audits of French public institutions.

the years to come; in a Rawlsian fashion, this measure is to be offset by specific mechanisms for jobseekers who live far away from LPEAs.

This view is strongly opposed by Pôle Emploi, local public opinion⁵ 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. In addition to 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 in 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 in 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 contexts. Theoretically, different factors channel the spatial mismatch (see Gobillon *et al.*, 2007 or Zenou, 2009, for a literature review): 1) transportation costs limit the area where it will be profitable for jobseekers to search for jobs, so that they limit their search area; 2) information on jobs decreases with distance, whether because jobseekers are ignorant of the specificities of the labour market of unfamiliar parts of 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 jobseekers who live in neighbourhoods with bad reputations; 4) firms redline jobseekers because too-long commutes reduce their productivity and 5) cheap housing in the areas located the farthest from the jobs means fewer search incentives for 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 a partial solution to rather than a further cause of spatial mismatch between jobs and workers. Indeed, public placement agencies are dedicated precisely to acting as matching facilitators, collecting the best possible information on vacancies and working with firms to help them

⁵ For an example, see Merlin (2015).

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, friction between jobseekers and agencies. This friction could arise, in line with the theoretical Spatial Mismatch literature, if higher transportation costs discourage jobseekers from travelling to their agency, or if agencies discourage the enrolment of far-living, less employable jobseekers.

However, these effects do not occur in practice for three reasons. First, agencies are required by law to enrol all jobseekers within their catchment area; as a result, 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 prospects, 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, such as 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 accessibility to the local labour market. In other words, it is possible that the location of LPEAs is not exogenous relative 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, independent of jobseekers' individual characteristics 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.

3 Data and Empirical Strategy

3.1 The Data

We combine previously unexploited exhaustive individual datasets on jobseekers and LPEA 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 of all unemployed jobseekers (18-65 years old) during a long period of time (8 years). We focus on the June, 2006 to April, 2012 period to stay within the parameters of a 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 municipal level⁶). This information allows computing unemployment recurrence for the 2004 to 2012 period and controlling for jobseeker residential moves that could otherwise lead to underestimation of unemployment duration. To take into account unemployment recurrence, we first calculate the total duration of unemployment spells during the last 2 and a half years before each new entry of an unemployed jobseeker in the agency register. To measure the durability of the exits from unemployment, we also compute the gap between a jobseeker's last unemployment spell and her actual one.

To compute these two elements, we use the exit of a jobseeker from the individual Pôle Emploi dataset, where the motive for the exit is not noted. 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 (24.8% of the exits). Because 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 exits from unemployment). Jobseekers also transition to inactivity (0.9% of the exits) or temporarily suspend their job search for maternity, military, holidays or medical reasons (8.2% of all exits). In these cases, the nature of our database does not allow us to track and exclude these exits from the computation of our variables of interest; however, the impact

⁶ The Pôle Emploi dataset does not provide a finer geolocation of jobseekers.

of this shortcoming is lessened by the fact that there is virtually no link between these motives for exit and jobseeker/agency distance. In contrast, the remaining motives are more problematic for us because they can, at least partially, be caused by the demotivation of jobseekers, which in turn may be 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).

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 Internet-based job searches, 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 on helping jobseekers implement efficient and diversified search strategies, which has indirect positive effects on jobseekers’ employment prospects, as shown by the converging empirical evidence on the evaluation of active labour market policies. Caseworkers counsel jobseekers on writing resumes, using the Internet in their job search, devising an effective spontaneous application strategy, identifying job opportunities and activating their personal and professional networks (de Larquier and Rieucan, 2015).

Second, we use information on the LPEAs available through the Annual Declaration of Social Data⁷ dataset, which provides *exhaustive* data on all establishments located in France, identified through a unique SIRET number. Beyond the mere monitoring and control of jobseekers, LPEAs perform a varied set of tasks: benefit distribution, vacancy prospecting, local public institutions and firm networking. We only take into account the LPEA staff members whose profession is a variation of “caseworker”. Using this information, the average congestion of LPEA j is computed by measuring the average caseload of each caseworker as follows:

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

with:

⁷ 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). Unfortunately, this information is not available for our study period (2006-2012).

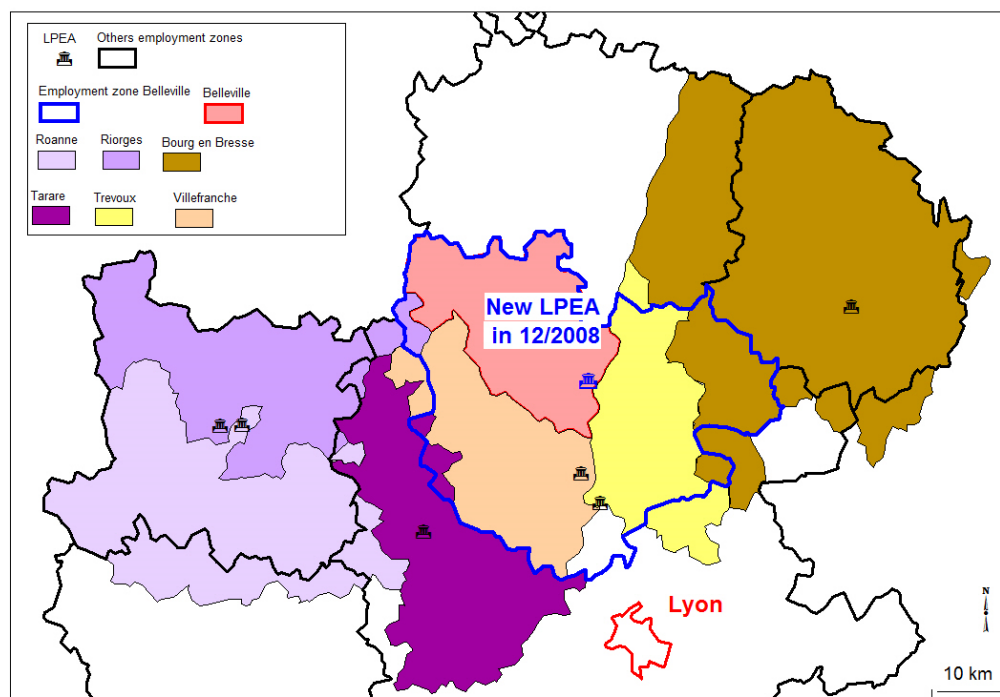
- $jobseekers_j^n$ the number of jobseekers enrolled in LPEA j during at least 3 months⁸ in quarter n
- $caseworkers_j^n$ the number of caseworkers at LPEA j during quarter n .

Third, to directly measure the distance, we rely on the original Odomatrix© dataset (Hilal, 2010), which 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-largest city in size. It is composed of 399 municipalities located in six LPEA catchment areas: Roanne, Riorges, Tarare, Belleville, Villefranche, Bourg-en-Bresse and Trevoux (Figure 1).

Figure 1. The Belleville Zone Map



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 in 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

⁸ Alternative results can be provided for a 6-month threshold, with no significant differences.

agency⁹ 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 in the use of exhaustive individual datasets (see above), it is therefore possible to check whether “pure” spatial effects affect jobseekers’ unemployment prospects.

Additionally, this area is comprised of rural and semi-rural municipalities, which echoes 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).

3.3 Identification strategy

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

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

We start with a simple model

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

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 , a dummy for the years after the change;
- G , a dummy for the residential location 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 jobseekers’ labour market outcomes. In a discrete framework, the marginal effect associated with the distance variable is calculated by using the following formula

⁹ The Villefranche agency also moved in 2013; however, 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.

$$\frac{\partial p_i}{\partial \text{dist}_i} = \frac{\partial \Lambda(\cdot)}{\partial \text{dist}_i} = p_i(1 - p_i)(\gamma_1 + 2 \times \gamma_2 \text{dist}_i)$$

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 needed to find a job without entering a new unemployment spell during the 6 months after exiting unemployment.

As in Crépon *et al.* (2005), we keep only 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 for more than 4 months¹⁰ exited unemployment within the first M months of her unemployment spell:

$$\text{Proba}(\text{exit} \mid T < M \text{ months}, T < 4 \text{ months}).$$

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 who 1) live in the municipalities located in the catchment area of the new Belleville LPEA, 2) were formerly enrolled in the Villefranche agency and 3) 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).

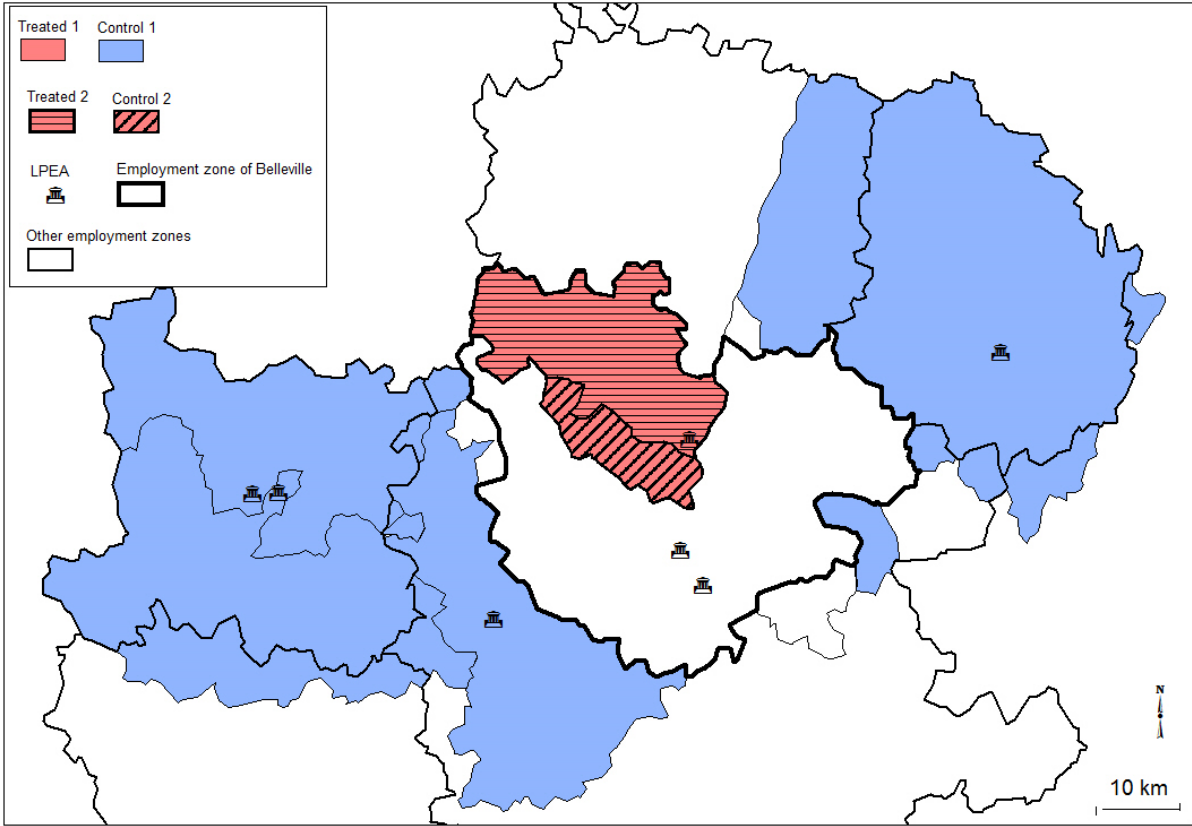
The first control group (in light blue in Figure 2) is a group of jobseekers who were not affected by the creation of the new Belleville agency, i.e., who live in the municipalities that are located 1) inside the catchment areas of nearby agencies, 2) not including Villefranche and 3) outside the Belleville *Employment Zone*. In Figure 3, we can observe that the travel times to these jobseekers' agencies were not significantly altered after the Belleville creation. First, the Roanne, Riorges, Tarare and Bourg-en-Bresse agencies were chosen because they are geographically close¹¹ to the Belleville-Villefranche areas and because, being in rural or semi-

¹⁰ Since the implementation of active labour market public policies, an important landmark in counselling is the compulsory second meeting with one's caseworker (the first takes place when the jobseeker is enrolled at the LPEA), in which the worker's search strategy is outlined. Fontaine and Le Barbanchon, 2012, established that 37% (25%) of the second caseworker/jobseeker interview should take place after the 4th (5th) month of unemployment). By 4 months in, we conservatively think that most jobseekers will have received the "added" value counselling from their caseworker.

¹¹ Other nearby areas north of the zone could also be included in the control group but are located outside the Rhône region, i.e., outside the perimeter of our datasets.

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 may 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 caseloads of Villefranche caseworkers, which may lead to greater efficiency and better outcomes for Villefranche jobseekers. On the other hand, the creation of a specific agency for Belleville jobseekers could lead to better placement prospects for *them*, i.e., increased competition for Villefranche jobseekers living in the same Employment Zone.

Figure 2. Control and treated groups

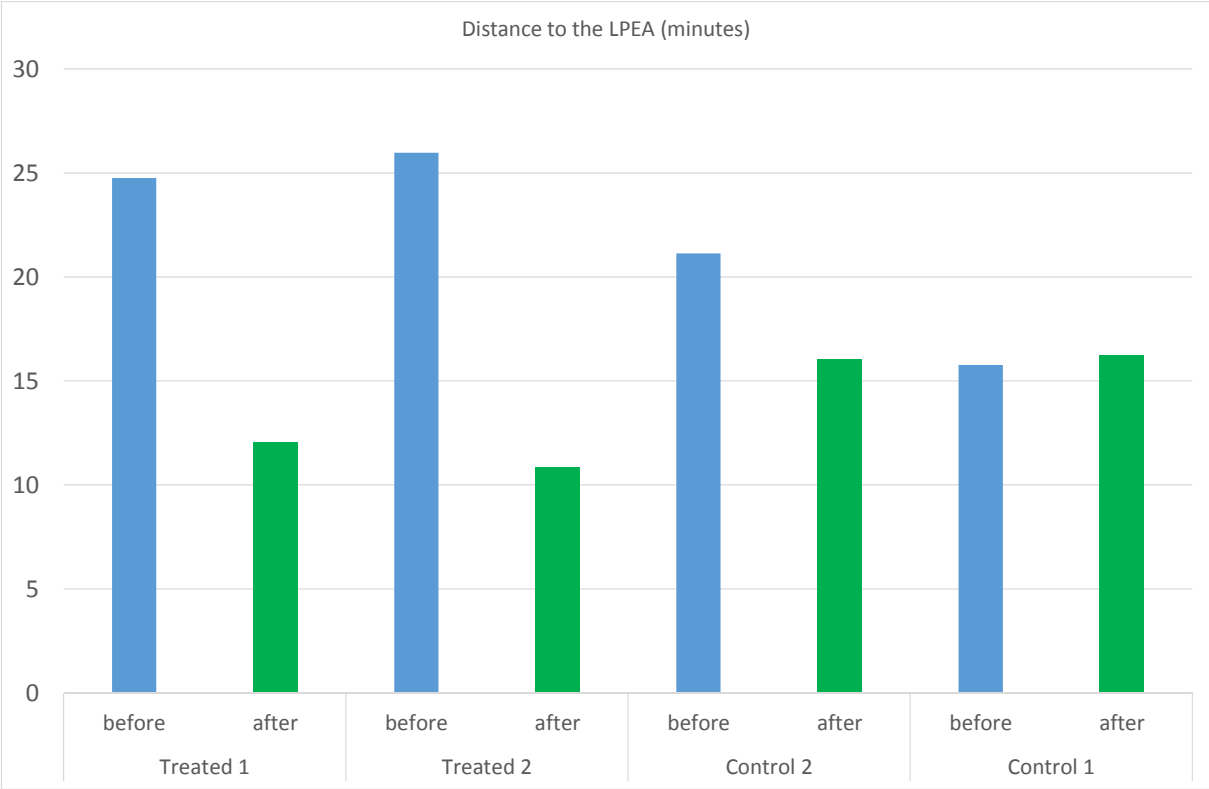


Third, as noted by RUBIN (1977), to identify causal effect, it is important to be in a situation where we do not observe interactions between the treated and control groups. The well-known stable unit treatment value assumption (SUTVA) assumes that the treatment status of any unit does not affect the 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 same Employment Zone as the Belleville treated group. Defined using Census data¹² by the French National Statistics Institute (INSEE), an employment zone is a homogeneous labour market zone, i.e., an area within which most of

¹²The zoning used in the paper is based on the flows of movement from residence to work of active persons observed in the 2006 Census.

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 because most of them are also located in 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 to disentangle institutional and distance effects. The second treated group is defined as jobseekers who, within 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 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., jobseekers who live near both 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 travel time to their agency (diagonal stripes in Figure 2).

b) Parametric estimation

In the second model, we do not introduce the distance variable; however, we address the difference-in-difference strategy of identification. Note the labour market outcomes 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.

$$\begin{cases} Y_{it1}^T = \beta X_i + \gamma_3 d_i + u_{it} & \text{if } t > \bar{t} \\ Y_{it0}^T = \beta X_i + u_{it} & \text{if } t \leq \bar{t} \\ Y_{it1}^C = \beta X_i + u_{it} & \text{if } t > \bar{t} \\ Y_{it0}^C = \beta X_i + u_{it} & \text{if } t \leq \bar{t} \end{cases}$$

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

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

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

$$Y_{it} = \beta X_i + \delta_1 T_i + \delta_2 d_i + \gamma_3 d_i \times T_i + u_{it}.$$

Note that in a logit model, the marginal effect associated with the treatment in the period in which the treatment is implemented ($d_i \times T_i$) is obtained by using

$$\frac{\partial p_i}{\partial d_i \times T_i} = \frac{\Delta p_i}{\Delta d_i \times T_i} = F(X_i \beta + \gamma_3) - F(X_i \beta)$$

where $\Delta(\cdot)$ is the differential operator and $F(\cdot) = \frac{\exp(\cdot)}{1+\exp(\cdot)}$.

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 to all agents.

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

To address this issue, we also implemented the matching method with the difference-in-difference method 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 the observed covariates.

The non-parametric method of propensity matching allows selecting a control group on the basis of a single score. To find a comparably treated group before the introduction of the new LPEA, we use

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

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

To estimate γ_4 , we compute the two propensity scores by using the 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 lives in a community in the LPEA catchment area 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), exposure to the treatment is independent of the covariates (X). This balancing of score condition can be written formally as follows:

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

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

See Appendix for the presentation of matching propensity scores 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 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 obtained with Stata© is modified using the formula proposed by Blundell and Costa Dias (2000):

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

where

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

Finally, the standard error is obtained by bootstrapping 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 LPEAs (Table 1). Note that this result is measured at the municipality level, without accounting for population density disparities between municipalities. In 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 with aggregated data.

Consistent 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. Additionally, supporting Suárez Cano *et al.* (2012a), we find that, on average, travelling to one's LPEA takes almost twice the time for jobseekers who live in rural municipalities as for jobseekers who live in urban ones (38.8 minutes versus 21.0 minutes).

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

Furthermore, we find that, controlling for individual characteristics, distance to LPEAs significantly affects the probability of exiting unemployment for jobseekers whose unemployment spell lasted at least 4 months (Table 2); for example, we find that an increase of 10 minutes in a jobseeker's home/agency travel time reduces by 0.12 points the probability of exiting unemployment after an unemployment spell of 12 months (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 negatively affect jobseekers' employment prospects, with an increased effect on vulnerable groups. However, keeping in mind that the usual job/worker spatial mismatch sources should not work for agency/worker distances, this result could reflect the fact that distance to LPEAs could act as a proxy for other factors, such as 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)	-0.281** (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 jobseekers' job market outcomes (see Figure 4 for gross differences and Table 3 for the results of the difference-in-difference and matching estimations¹³).

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)	-0.162	-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)
Part 2 – 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.									

¹³ More detailed results are available upon request.

First, we will focus on the first part of Table 3 (Treated and Control groups 1). If our working hypothesis is correct, then the differences in the marginal effect of distance on the probability of leaving unemployment between the treated and control groups should be insignificant. However, if the distance to LPEAs is a new channel for spatial mismatch, then 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 in the effect of distance between the jobseekers in the Belleville area and those in control group 1, who were not affected by the creation of the new agency. This is also the case for durable short-term exits from unemployment (6 months) and for the effect of distance on gross exits from unemployment (i.e., the probability of exiting unemployment after 6- and 12-month unemployment spells) for the 2010-2011 period¹⁴. This finding is notable because Card *et al.* (2015) recently established that the effects of active labour market public policies had greater positive effects in the medium and long runs (two or more years).

This result does not hold for short-term exits from unemployment or for gross exits from unemployment if we also take into account the year 2009. For the 2009 and the 2009-2011 periods as a whole, distance to LPEAs had a significant impact on job matching outcomes. However, this impact was negative, not positive: we find that the Belleville-area jobseekers are worse off than the jobseekers in 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 LPEA efficiency, suggests that the poor efficiency of the Belleville agency could be due to transitory institutional dysfunction during the agency’s start-up period. To test this explanation, we compare the outcomes of the Treated and Control groups 2, which share the institutional effects of the creation of the new agency but differ in the “pure” distance effect because 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 find that no coefficients are significant at this point (see Part 2 of Table 3), which validates our working hypothesis of no evidence of a worker/agency spatial mismatch.

¹⁴ 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.

To summarize, 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 worker/agency spatial mismatch.

5 Conclusion

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

To do so, we combine exhaustive individual datasets on jobseekers, agencies and caseworkers, which allows us to use actual individual unemployment durations (rather than 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 allow 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 little or no effect on the probability of exiting 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: because jobseekers cannot de facto arbitrate between transportation costs and benefits from travel to their LPEA, distance does not create added friction in the matching process. The expensive maintenance of a very dense network of LPEAs does not appear to be a very efficient public policy, supporting the position of the Cour des Comptes on the re-sizing of the French public employment agency network (Cour des Comptes, 2015). In contrast, 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 had a transitory detrimental impact on jobseekers' employment prospects: this means that the long-term benefits of a re-sizing of the French public employment agency network should be balanced with the short-term adverse institutional effects. In any case, our findings suggest that any reform should be timed with a reduction of caseworkers' caseloads, i.e., should not take place in times of high unemployment.

Second, welfare issues could be problematic because jobseekers must absorb the costs of travel to their agency, however far from their home. Any loosening of the LPEA network should come with compensation for the jobseekers who will incur the greatest cost (e.g., financial compensation, creation of specific shuttles to distant locations, increased telephone or Internet meetings).

Third, our evidence suggests that distance from LPEA does not create an added source of friction in the job search process. This finding challenges the idea of a spatial mismatch between agencies and jobseekers. However, we stress that this result 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; however, we do not address possible harmful effects of a too-great agency-to-jobs distance. In fact, from the spatial mismatch theory perspective, distance to jobs should be problematic for the agencies' efficiency because 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 relative to jobs and not jobseekers.

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7 Appendixes

7.1 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%
Nb 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%
Caseload 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 checked after the estimations. Figure A presents the overlap charts used to assess whether propensity scores met the overlap assumption.

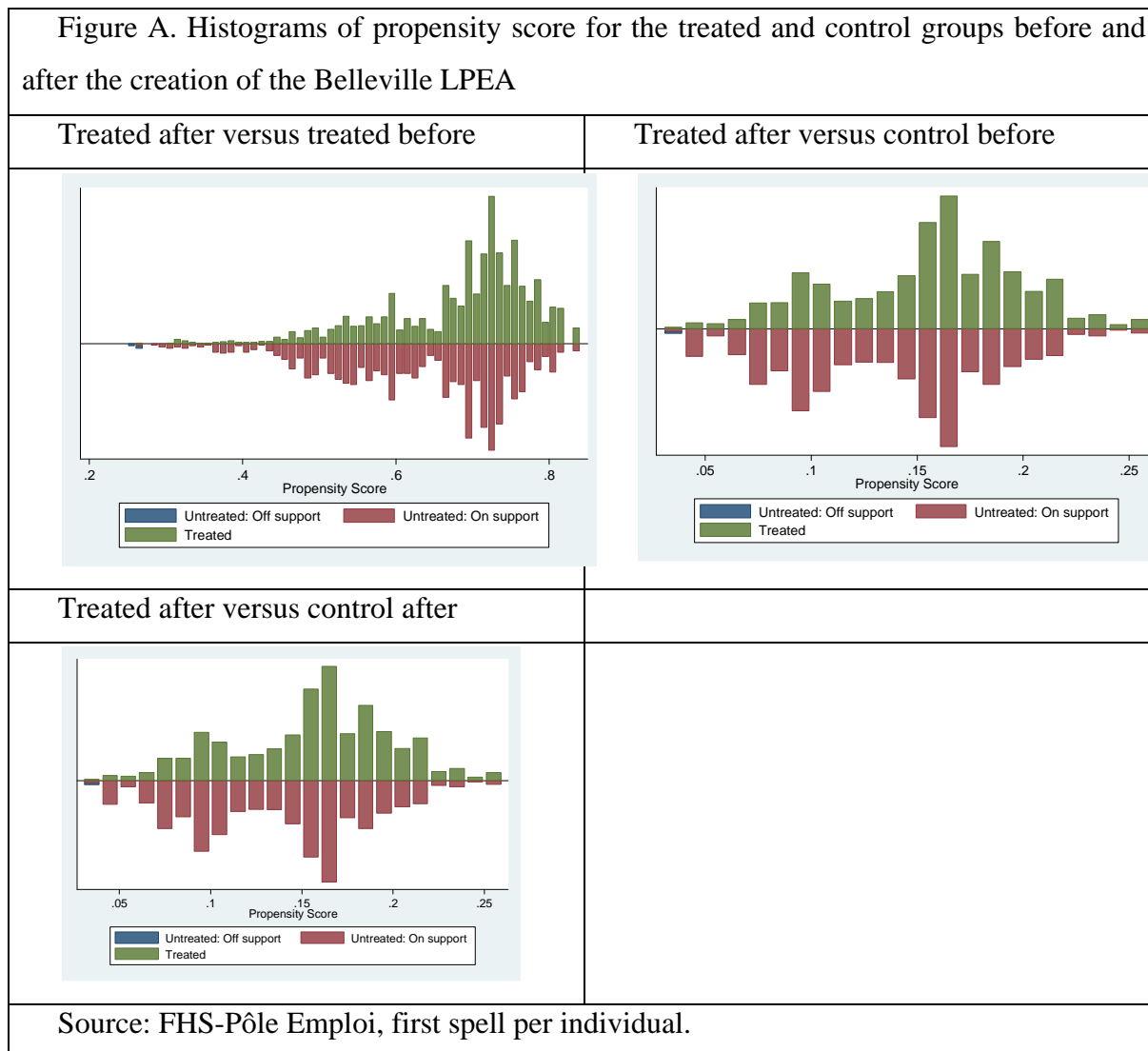


Table B. Means of the covariates after matching for the three control groups

	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
> [25-35[years old	0.077	0.075	0,002	0.785	0.082	-0,005	0.524	0.081	-0,004	0.640
> [35-45[years old	0.047	0.050	-0,003	0.578	0.048	-0,002	0.781	0.049	-0,002	0.734
> [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 $\alpha=0$ 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 $e^\alpha = 1$ the case with no unobserved bias the treatment effect are significant for the three control groups. When e^α 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 e^α 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.

e^{α}	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			
	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

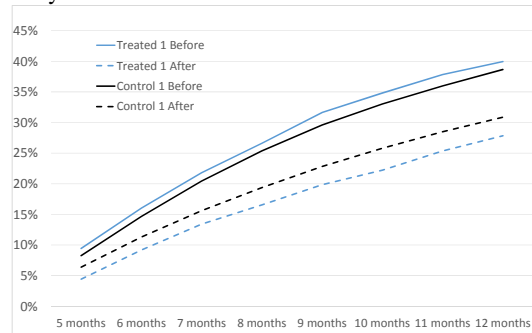
Source: FHS-Pôle Emploi, first spell per individual.

7.3 Gross exits from unemployment

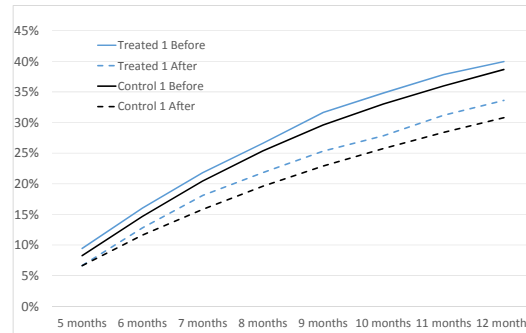
Figure B. % of workers who have not been unemployed in the 5 to 12 months after exiting unemployment (durable exits)

Part 1 – Control 1 versus Treated 1

All years

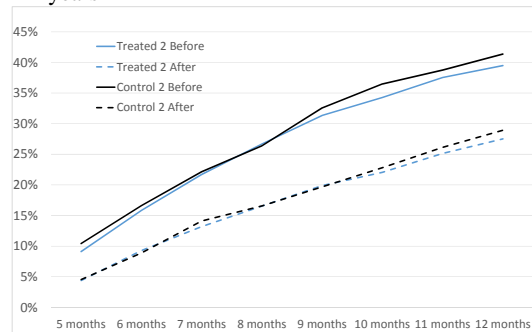


2010-2011

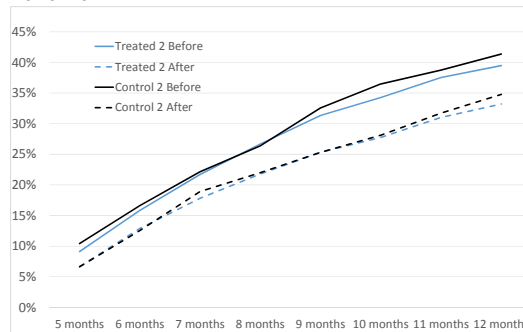


Part 2 – Control 2 versus Treated 2

All years



2010-2011

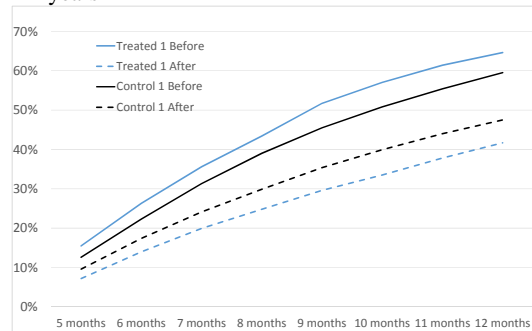


Source: FHS-Pôle Emploi, first spell per individual.

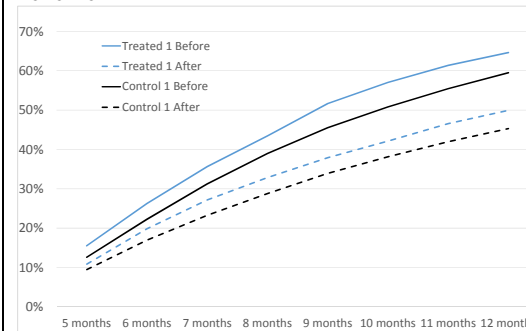
Figure C. % of workers who have exited unemployment after 5 to 12 months long unemployment spells (gross exits)

Part 1 – Control 1 versus Treated 1

All years

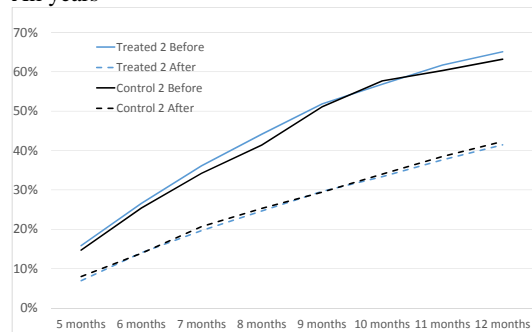


2010-2011

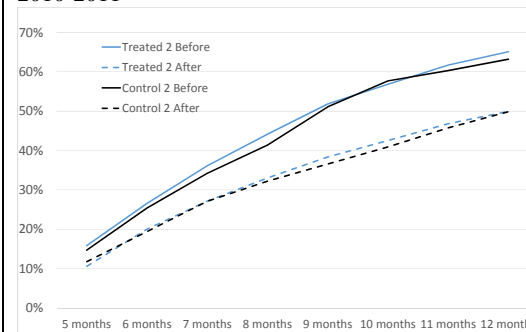


Part 2 – Control 2 versus Treated 2

All years



2010-2011



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