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Thèse de doctorat

Essays on the Determinants of Job Search Behavior and Employment

Thèse de doctorat de l'Université Paris-Saclay
préparée à Ecole nationale de la statistique et de l'administration économique

Ecole doctorale n°578: Sciences de l'Homme et de la Société (SHS)
Spécialité de doctorat: Sciences économiques

Thèse présentée et soutenue à Palaiseau, le 7 septembre 2018, par

Daphné Skandalis

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Résumé substantiel

Ma thèse explore différents déterminants du comportement de recherche d'emploi, dans le but de comprendre certains des obstacles au retour à l'emploi en particulier pour les travailleurs les plus défavorisés.

Dans mon premier chapitre de thèse, écrit avec Gerard J. van den Berg, Sylvie Blasco, Bruno Crépon et Arne Uhlendorff, nous évaluons l'impact d'un programme d'accompagnement pour les jeunes chômeurs de zones urbaines sensibles. Des "clubs de recherche d'emploi" ciblés pour l'accompagnement des demandeurs d'emploi de cette population défavorisée ont été expérimentés récemment en France. Le principe d'un club est que chaque participant est assigné à un groupe avec une douzaine d'autres demandeurs d'emploi pour réaliser les activités de recherche d'emploi collectivement. Grâce à une évaluation randomisée, nous montrons que le fait d'être affecté à un club plutôt qu'à un programme d'accompagnement traditionnel a un effet positif sur la probabilité de trouver un emploi stable. De plus, nous établissons que les chômeurs assignés à un club profitent le plus de ce programme lorsqu'ils sont dans un groupe constitué de participants ayant une relativement faible probabilité a priori de trouver un emploi. Ces effets de pairs négatifs semblent provenir d'un effet sur la confiance en soi: être entouré de "mauvais" pairs semble avoir un effet positif sur la confiance en soi et stimuler les efforts de recherche d'emploi tout en rendant les individus plus sélectifs vis-à-vis des offres d'emploi. Finalement, nos résultats suggèrent que les programmes d'accompagnement pourraient exploiter les effets de pairs afin d'activer certains mécanismes comportementaux de nature à favoriser leur efficacité.

Dans le second chapitre, j'étudie l'impact d'un choc d'information sur la recherche d'emploi et la probabilité de retour à l'emploi des chômeurs. Je mesure l'impact de nouvelles médiatiques portant sur des créations d'emploi dans une entreprise sur le nombre de candidatures que reçoit l'entreprise et les embauches qu'elle effectue. Je trouve que ces nouvelles médiatiques conduisent à une augmentation des candidatures de 60% au cours du mois qui suit. Les chômeurs qui postulent à la suite des nouvelles médiatiques sont en moyenne relativement détachés du marché du travail et tendent à habiter plus loin de l'entreprise que les candidats habituels. Cela suggère qu'il existe des frictions d'information qui sont accentuées par la distance. Ces chômeurs ont un profil qui semble adapté aux besoins de l'entreprise et une fraction importante d'entre eux est finalement recrutée. Dans leur ensemble, les chômeurs bénéficient de l'information apportée par les médias car elle leur permet d'orienter leurs candidatures vers des entreprises qui ont une relativement grande chance de les recruter même s'il faut noter que certains chômeurs habitant près de l'entreprise voient leur probabilité d'être embauchés diminuer par effet de ricochet. Ces résultats suggèrent donc qu'aider les chômeurs à orienter leurs candidatures vers les entreprises qui ont le plus de chance de faire des recrutements à court-terme peut aider à corriger certaines inégalités dans l'accès à l'emploi et stimuler la mobilité géographique.

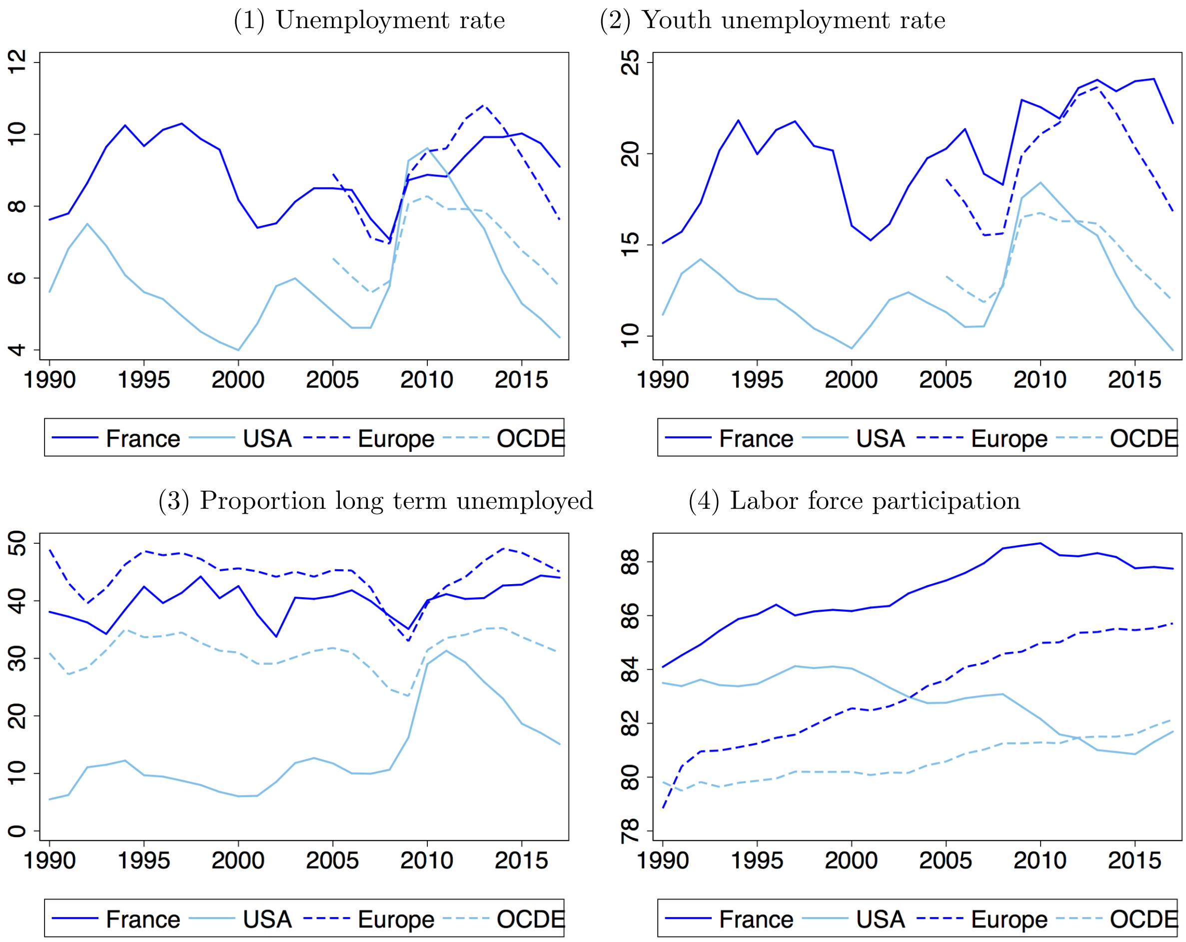
Dans le troisième chapitre, écrit avec Ioana Marinescu, nous explorons les mécanismes sous-jacents derrière l'effet négatif—largement documenté—de la durée d'assurance chômage sur le taux de retour à l'emploi. Nous utilisons des données françaises individuelles longitudinales retraçant le comportement de recherche d'emploi au cours d'un épisode de chômage. Nous montrons que les efforts de recherche augmentent de 25 % dans les mois qui entourent la date de fin des droits à l'assurance chômage, même lorsqu'on neutralise l'impact de la sélection dynamique. Nous mesurons également une légère baisse dans le salaire indiqué dans les offres d'emploi auxquelles les chômeurs postulent dans cette période. De plus, nous utilisons une régression sur discontinuité afin de montrer que la durée des droits à l'assurance chômage ne modifie pas le comportement des chômeurs au début de leur épisode de chômage, mais commence à avoir un impact dans les mois qui précèdent l'épuisement des droits. Ces résultats suggèrent donc qu'une extension de l'assurance chômage affecte les comportements de recherche d'emploi principalement par un recul du pic dans l'intensité de la recherche d'emploi observé autour de la date de l'épuisement des droits. En termes de politique publique, cela semble indiquer que l'aléa moral associé à l'extension de l'assurance chômage n'apparaît que tard dans les épisodes de chômage individuels et ne concerne donc pas les individus qui sortent rapidement du chômage.

Chapter 0

General introduction

0.1 Contemporary labor market challenges

Figure 0.1: Evolution of labor market indicators over time



Notes: Data come from Insee and OECD, where unemployment is measured by national labour force surveys and refers to people reporting that they have worked in gainful employment for less than one hour in the previous week, who are available for work and who have sought employment in the past four weeks. Europe refers the OECD definition including 28 countries. Youth unemployment rate refers to unemployment rate among 15-24 years old individuals. The proportion long term unemployed shows the proportion of people who have been unemployed for 12 months or more among all unemployed.

The Great recession was characterized by a strong increase in the unemployment rate in most developed countries, which has put the topic of unemployment at the center of the public and academic debate. While in the U.S., the unemployment rate went down to a relatively low level after some years, for other countries like France, the unemployment rate has remained high (see Graph (1) in Figure 0.1). Beyond the aggregate rate of unemployment, the difficulties of particular sub-groups represent unsolved problems in many labor markets.

One major issue is the very high rate of youth unemployment, which amounts to about 2 to 3 times the average unemployment rate in the population (Graph (2). This issue is more severe in Europe, where the situation for young workers has deteriorated strongly with the economic crisis and did not recover (17% in 2017) and in France in particular (22%). Young people from a disadvantaged social background or with low education were hit particularly strongly. Another burning issue is the situation of the long-term unemployed. A large body of evidence suggests that it is more and more difficult to find a job after a long unemployment spell, and that job seekers get more and more discouraged over time. The proportion of long-term unemployed is especially high in Europe, and amounts to around 45% on average (Graph (3)). Long term unemployment represents a concern in the U.S. as well: while it seems that the proportion of long-term unemployed decreased in the aftermath of the Great recession, this was partly caused by their exit from the labor force (see Graph (4) and Krueger (2015)).

The economic literature has long documented a very high and multi-dimensional cost of unemployment both for individuals and for society as a whole. For adults, involuntary job loss has been shown to permanently decrease earnings by 10–25% (Ruhm (1991), Jacobson et al. (1993), Stevens (1997)). Sullivan and von Wachter (2009) highlight a substantial increase in mortality among those who have lost their jobs in plant closings, while many studies show an negative impact on various other measures of health. Charles and Stephens (2004) show that marriage and family structure may be affected negatively by lay-offs. Intergenerational effects of job loss on children have also been documented (Oreopoulos et al. (2008), Rege et al. (2011)). In the case of young individuals, entering the labor market for the first time during a period of high unemployment has been shown to have long-lasting negative effects on earnings (Kahn (2010), Oreopoulos et al. (2012)) and to deteriorate family outcomes such as divorce and single motherhood (Engdahl et al. (2018)). The literature has also highlighted that youth unemployment is associated with higher crime (Fougère et al. (2009), Fella and Gallipoli (2014)). Overall, many evidence contribute to draw a picture of unemployment as a painful individual experience and a severe society problem.

0.2 Why study job search behavior?

Since the 1960s, economic theory has proposed models of the behavior of individuals searching for jobs based on the observation that labor markets are made of heterogeneous jobs and workers which cannot match without a complex search process. Rogerson et al. (2005) summarize: "It takes time and other resources for a worker to land a job, especially a good job at a good wage, and for a firm to fill a vacancy. There is simply no such thing as a centralized market where buyers and sellers of labor meet and trade at a single price, as assumed in classical equilibrium theory." In these models, based on imperfect information about vacancies and job characteristics, job seekers choose a level of their search effort and their reservation wage. This strategy ultimately determines how fast they find a job.

This theoretical framework hence posits search behavior as an important individual determi-

Table 0.1: Estimation of returns to search

	Duration before re-employment (log)				
	(1)	(2)	(3)	(4)	(5)
Monthly number of applications (log)	-0.319***	-0.288***	-0.248***	-0.248***	-0.269***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.005)
Monthly number of applications (log) interacted with individual characteristics:					
Woman					0.023***
					(0.002)
Young					0.037***
					(0.002)
Single					0.007***
					(0.002)
Wants full-time job					-0.017***
					(0.004)
Low education					0.011***
					(0.002)
Blue collar					-0.006*
					(0.003)
Low skill level					0.016***
					(0.003)
Wage of job applied for (log)	No	No	No	Yes	Yes
Individual characteristics	No	No	Yes	Yes	Yes
PBD FE	No	No	Yes	Yes	Yes
City FE	No	Yes	Yes	Yes	Yes
Calendar month of registration FE	No	Yes	Yes	Yes	Yes
No. of Obs.	807853	803782	803782	803099	803099

Notes: The data come from the online applications on the search platform from the French public employment agency, merged with administrative employment data (DPAE) and unemployment register (FH). The sample is made of individuals who have an unemployment spell between 2013 and 2017, have at least one online application during their unemployment spell and get unemployment benefits. The data construction is explained in more details in the third chapter of this thesis. Individual characteristics include gender, family situation, age, education level, qualification level, whether the person looks for a full-time job. Robust SE in parentheses. *** p<0.01, ** p<0.05, * p<0.1

nant of the probability to find a job. In order to illustrate this idea, I present some suggestive evidence of large positive individual returns to search in Table 0.1. In this exercise, I measure search effort by the count of applications made on the online search platform of the French public agency "Pôle Emploi". I estimate the elasticity of the duration before re-employment with respect to the number of applications over the unemployment spell. In the simplest model presented in column (1), the estimate I obtain for this elasticity amounts to 0.3, which suggests that an increase in individual search efforts by 10%, decreases the duration before the next job by about 3%. But search effort might be correlated with characteristics that lead job seekers to find a job faster, which would bias the naive estimate of the return to search. To address this concern, I include in columns (2) to (4) a number of control variables: city and time fixed effects, potential benefits duration, individual characteristics and a measure of job seekers' selectivity. The estimates I obtain are slightly lower but of a very similar magnitude around 0.25. Although this empirical strategy imperfectly addresses the endogeneity of search effort, the estimates suggest that search intensity has a large positive impact on the probability to find a job. Additionally, column (5) reveals important heterogeneity in the returns to search. Returns to search appear positive for all subgroups considered, but they appear to be lower for some groups that are disadvantaged on the labor market: women, young workers, workers with low education, and low-skill workers. The estimates suggest that understanding the determinants of returns to search can contribute to explain some of the difficulties that disadvantaged workers are facing.

Despite the important role of search behavior in the literature, very little was known until recently about the process of job search due to limitations of the available data. Time use surveys have revealed important disparities in the time spent looking for a job in various countries (Krueger and Mueller (2010)). A repeated survey on a panel of job seekers in New Jersey has shed some light on the individual dynamic of job search: job seekers seem to decrease their search effort and reservation wage over the unemployment spell (Krueger and Mueller (2011), Krueger and Mueller (2016)). Search behavior seems to depend on individual personality traits, as demonstrated using German linked survey and administrative data (Caliendo et al. (2015)). Evidence suggests that more generous unemployment insurance decreases aggregate search effort (Marinescu (2017), Lichter (2017), Fradkin and Baker (2017)), but it does not seem to impact reservation wages (Barbanchon et al. (2017)). In a randomized experiment, Belot et al. (2017) show that some job seekers spontaneously search in a too narrow set of occupations, but broaden their search if they receive external suggestions. The empirical literature on job search has therefore grown rapidly in recent years, but many questions remain open. In particular, understanding how external interventions affect job search behavior could help design policy interventions to order to improve the employment prospects of disadvantaged workers.

0.3 My dissertation

In my dissertation, I explore various determinants of search behavior in order to shed some light on the obstacles in the access to jobs, in particular among disadvantaged workers. For this purpose, I collected new sources of survey and administrative data. The three chapters of this dissertation address each a different question around this theme.

What mechanisms should a counseling program trigger to be most effective?

In many countries, young workers from deprived neighborhoods are particularly exposed to unemployment. “Search clubs” have been recently set up in France to help the unemployed from this disadvantaged population find a stable job. These clubs are a collective job search assistance program in which each participant is teamed up with a dozen other job seekers in order to search for jobs together. Based on a randomized control trial in France, we find a large positive impact of being assigned to a search club rather than a standard JSA program on the probability of finding a long-term job.

Moreover, we find that, within clubs, individuals assigned to groups of job seekers who have on average a low probability of finding a job perform better. Our experiment was set up to create a control group perfectly balanced in space and time and therefore allows us to disentangle the role of peers from the labor market context. The negative peer effects that we find are at odd with most findings in peer effects literature, but consistent with the frame of reference model, which states that it is better to be a “big fish in a little pond”. Using survey data on job seekers’ behavior and perceptions, we find that having “bad” peers increase search effort, and also seems to positively affect self-confidence as well as the minimum acceptable quality of a job offer. We therefore conclude that self-confidence seems to be an important factor of success for young disadvantaged job seekers, which suggests that JSA programs can be more effective by addressing this dimension in their design.

How do job seekers react to additional information on job openings?

This chapter focuses on information frictions on the labor market. Recent evidence suggests that posted job vacancies provide a poor signal of firms' hiring needs. For job seekers, this translates into uncertainty about their probability of being hired when they apply to a vacancy. I study how job seekers react when media outlets cover the expansion of a specific plant and thereby provide information about the hiring needs of the plant. I use new data about online job applications matched with administrative micro data about job seekers and firms' vacancies and hires in France during 2014-2016. My empirical strategy exploits the quasi-random timing of media news in the short run. I estimate the impact of news on the applications sent to mentioned plants and the subsequent hiring in an event study design. I find that news coverage of the expansion of a plant increases the number of applicants by 60% in the following month.

Job seekers who apply in reaction to the coverage have a good match quality and live further away than usual applicants. The average distance between applicants' residence and plants' location therefore increases by 40%–corresponding to 30km. Plants benefit from the diffusion of information as they hire a sizeable share of their additional applicants. Job seekers use the information to direct their search towards plants which have a relatively high hiring rate. I also find evidence for displacement effects among job seekers. The inflow of applicants from further away decreases the hiring rate of local job seekers. Overall, these results suggest that low-cost interventions providing information about hiring needs could both improve the matching process and increase geographical mobility.

How does unemployment insurance affects job search behavior?

The final chapter explores the impact of the potential duration of unemployment insurance on job search. The negative impact of potential UI duration on the job finding rate has been well documented, however, little is known about the underlying mechanism. Using French administrative longitudinal data on job search and unemployment benefits at the individual level, we estimate directly how potential benefits duration affects job search over the unemployment spell, both on the intensity and selectivity margin. We first highlight a 25% spike in job search intensity in the months surrounding benefits exhaustion, when controlling for dynamic selection. On the selectivity margin, we document a very small and typically insignificant drop in the target wage around benefits exhaustion (around 0.5%).

Additionally, exploiting a discontinuity in French unemployment rules in around age 50, we show in a regression discontinuity design that the duration of unemployment insurance only starts affecting search behavior in the months preceding benefits exhaustion. This suggests that the moral hazard associated with an unemployment insurance extension appears in the periods leading up to benefits exhaustion but not from the start of the unemployment spell. Overall, these results suggest that a benefits extension increases unemployment duration mostly by postponing the spike in search intensity associated with benefits exhaustion. This search behavior is not consistent with standard search models, but with alternative models that feature a shift in incentives or perception at benefit exhaustion, such as the reference-dependent model.

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Chapter 1

Peer Effects of Job Search Assistance Group Treatments: Evidence from a Randomized Field Experiment among Disadvantaged Youths

This chapter is joint with Gerard J. van den Berg, Sylvie Blasco, Bruno Crépon, Arne Uhlendorff

In many countries, young workers from deprived neighborhoods are particularly exposed to unemployment. "Search clubs" have been recently set up in France to help the unemployed from this disadvantaged population to find stable jobs. It is a collective job search assistance (JSA) program in which a group of 10-14 job seekers meets together with one caseworker several times a week and the participants are supposed to work together on their job search. The program intends to foster the interaction among the participants and to improve the relationship between participants and caseworkers. Based on a randomized control trial in France, we find a positive impact of being assigned to a search club rather than a standard JSA program on the probability of finding a long-term job. Moreover, we find evidence for peer effects in search clubs. Individuals assigned to groups of job seekers who have on average a low probability of finding a job perform better. These findings are in line with the frame of reference model: having "bad" peers might positively affect the self-confidence of job seekers, which might increase search effort as well as the minimum acceptable quality of a job offer.

Keywords: youth unemployment, deprived neighborhoods, counseling, randomized control trial, peer effects.

JEL codes: J64, J68

1.1 Introduction

Youth unemployment is regarded to be one of the most important labor market problems in many developed countries (see for example OECD, 2016 and 2017). The unemployment rate among the youth is especially high in deprived neighborhoods. For example, in France in 2012 the unemployment rate of 15-24 years old individuals was around 45% in deprived areas – almost twice as high as the national average for this age category (24%) (Insee and Onzus, 2014). This population faces great difficulties to leave unemployment and to enter long-term jobs. While there exist numerous studies on the effectiveness of JSA programs in general, little is know about the effectiveness of such programs for young job seekers in deprived neighborhoods. In this paper, we analyze the effectiveness of an intense JSA group treatment which has been specifically designed for particularly disadvantaged populations.

Our analysis is based on a field experiment which was conducted in 30 local labor agencies in deprived neighborhoods in France from February 2013 to March 2014. Around 3,600 young job seekers were randomly assigned to a 3-month intense JSA program dedicated to young unemployed individuals. This program could be either a standard JSA program or a "search club". In contrast to standard programs based on weekly one-to-one meetings with one caseworker, search clubs are groups of 10 to 14 job seekers supposed to search for jobs together as a team with the support of one dedicated caseworker. The innovative aspects of clubs are the team work and the active role left to participants who are supposed to search for job offers with their caseworkers rather than only receiving advice. The program therefore intends to favor exchange of information about the labor market and the job search process among participants, and to improve the relationship between participants and caseworkers. Each agency has set up one search club. We estimate the causal impact of assignment to the club instead of a standard program on two sets of outcomes: employment outcomes, that we track using administrative data, and job seekers' behaviors and perceptions that we measure through a survey.

In addition to analyzing average effects, our research design allows us to investigate the impact of the characteristics of the peers in the clubs on the effectiveness of the program. Whenever a participant in a search club leaves the program, a new participant is randomly allocated to the program. The continuous inflow of individuals into search clubs creates variation in the composition of clubs over time and across agencies, depending on variation in the pool of potential participants. However, the characteristics of this pool might be correlated with the probability of finding a job. For the identification of peer effects, it is crucial to control for this type of relationship between group level characteristics and outcome variables in the absence of peer effects. Our empirical strategy is to use the group assigned to the standard JSA program to control for the correlation between the characteristics of the pool of participants in the experiment and the individual employment outcomes. For this purpose, the random assignment is performed on pairs of participants in each agency. This ensures that the groups assigned to the two programs are perfectly balanced in space and time: each time someone is randomly assigned to the club, someone in the same agency is randomly assigned the control program. We focus on exogenous peer effects, i.e., we investigate to which extend the effectiveness of the search clubs varies with the *ex ante* characteristics of the peers in the clubs (Manski, 1993). For this, we construct an index of individual "employability" based on administrative data observed before randomization. The individual employability is defined as the probability of leaving unemployment for a job within a specific time period. For each participant in the club, we calculate the average employability of all other individuals who have been assigned to the club in the same time period. This index reflects the average probability of finding a job in the club in the absence of the group treatment.

We find that the assignment to a search club leads to a significantly higher probability of leaving unemployment for a job. While we see no significant difference of signing a short-term working contract, the probability of signing a contract with an employment duration of at least 6 months increases by around 5(4)% within the first 6(12) months after randomization. Next, we estimate a model allowing for effect heterogeneity with respect to individual employability. In this model, we do not find any evidence for effect heterogeneity. In contrast to that, our findings suggest that the average employability in the club has a significantly negative impact on the effectiveness of the club. Moreover, once we allow for effect heterogeneity with respect to the average employability in the club, we observe that the treatment effect positively depends on the individual employability.

Our results indicate that search clubs with relatively low average employability lead to a significantly larger increase in the employment probability. Moreover, the relative position within these clubs matters: everybody benefits from being assigned to this program except individuals who are in the lower part of the employability distribution within a club (but they do not suffer from it). These findings are in line with the frame of reference model (Marsh and Parker, 1984). In our context, this model would predict that being in a group which is composed by job seekers who have a relatively high probability of finding a long-term job negatively affects the self-concept and therewith the aspirations of the job seekers. This would translate into a lower level of search effort and a lower level of minimum job quality—e.g. in terms of contract duration—which defines whether or not a job is acceptable. The results based on the survey support the implications of this framework: we find that job seekers tend to send more applications when assigned to groups with bad peers, while they tend to increase their selectivity with respect to potential job offers when they are good in comparison to their peers.

Our paper contributes to the literature on the evaluation of JSA programs for unemployed workers. A vast body of evidence suggests a positive impact of such programs on the job finding probability.[1] However, the literature refers to JSA for all kind of counseling programs which encompass a large heterogeneity of format. In the US, a similar program to our search clubs - so-called "job clubs" - have been used since the 1970s. There is some limited evidence that these job clubs increase employment and reduce welfare receipt (Rothstein and von Watcher, 2016).[2] In general, little is known on the effectiveness of JSA for the most disadvantaged workers. The scarce studies on the impact of JSA programs on young job seekers suggest positive effects on employment outcomes (Caliendo and Schmidl, 2016) but they usually do not focus on youth living in deprived neighborhoods or having a disadvantageous social background. One exception is the study of Aeberhardt et al., 2014, which suggests standard JSA are rather ineffective for young unskilled unemployed workers: cash payments conditional on attendance increase participation in a JSA program but have no effects on search efforts and employment outcomes. The present paper

[1] See in particular Behaghel et al., 2014 and Crépon et al., 2013 in the case of France and for a more general overview Card et al., 2010 and Card et al., 2018.

[2] While there exist no studies on the effectiveness of this kind of programs in the economic literature, there is some evidence of positive effects of search clubs reported in policy reports (summarized in Rothstein and von Watcher, 2016) and the psychological literature (Azrin et al., 1980 and Azrin et al., 1983).

therefore contributes to the literature on JSA by studying JSA programs targeted on young job seekers form deprived neighborhoods, highlighting the important role of the format of counseling programs in this context, and shedding light on the underlying mechanisms.

Doing so, we also contribute to the understanding of peer effects in the context of job search. While there is no empirical evidence on the role of peer effects in the context of a JSA program, a large literature shows that peer effects are relevant for decisions and outcomes in a number of areas including education, worker productivity and criminal activities (see e.g. Sacerdote, 2011 ; Falk and Ichino, 2006 ; Bayer et al., 2009). Studies on peer effects in education usually find that the average ability of the peers has a positive impact on the individual outcomes, and there exists some evidence that the relative position within the peer group matters (Sacerdote, 2011). In contrast, Antecol et al., 2016 find that the average past achievement of the peers in the classroom negatively affects own student achievement based on a randomized experiment in schools in disadvantaged neighborhoods. Our findings are consistent with these results, and our study very notably focuses on the same population group. In line with this, Pop-Eleches and Urquiola, 2013 show that being with good or better peers can negatively affect self-concept and cause marginalization.

Third, we add to an increasing empirical literature showing evidence that behavioral mechanisms are relevant for job seekers' decisions and outcomes. Our results suggest that job seekers' behaviors might be importantly influenced by their sself-confidence. This factor has been emphasized in recent literature. The decrease in search over the unemployment spell is often explained by the notion of discouragement, which attributes an important role to job seekers' self-confidence (Krueger and Mueller, 2010, Krueger and Mueller, 2011). In a laboratory experiment designed to test the existence of such a discouragement effect, Falk et al., 2006 show that searchers are uncertain about their ability, adjust their self-confidence when they receive new information, and correct their search decisions accordingly: the more confident subjects are of being a high type the more frequently they engage in costly search. Other recent articles suggest that job seekers' subjective perception of their job finding probability, their return to search or their re-employment wage might affect their search behaviors and ultimately their employment outcomes (Caliendo et al., 2015, Spinnewijn, 2015, Drahs et al., 2018). This is part of a broader set of evidence that behavioral mechanisms play a large role in job search behaviors (DellaVigna and Paserman, 2005, DellaVigna et al., 2017). Our paper brings additional evidence that self-confidence might affect job seekers' decisions, and demonstrates how an active labor market policy program can trigger this mechanism through peer effects.

The paper is organized as follows. Section 2 presents the experimental design and the two programs. Section 3 describes the data and the empirical approach. In Section 4 we present average effects and effect heterogeneity with respect to individual employability. In Section 5 we present evidence for peer effects, and Section 6 discusses the mechanisms which might explain the peer effects. Last, Section 5 concludes.

1.2 The experiment

The experiment was conducted from February 2013 to March 2014 in 30 local agencies across 15 regions of France. 3604 individuals were selected in total to participate in the experiment.

1.2.1 The two intensive counseling programs

We compare the effect of being assigned to a "search club" instead of a standard counseling program. Both programs last for three months, involve at least one weekly meeting with one entitled caseworker and aim to help job seekers find a stable job. The programs consist of advice to job seekers on how to search for suitable vacancies and how to apply for jobs. The treatment and control programs however differ in how the support is provided.

The search club The search club relies on the idea of making job seekers and their caseworker work together as a team to find jobs. It has been developed in France since 2008 with the idea to provide a more effective form of counseling to the most disadvantaged job seekers. It has been used for older workers and young job seekers from deprived neighborhoods in particular. The first trace of this type of program dates from 1970s in the U.S. and the concept has been described in Azrin et al., 1975: "The new program was conducted in a group and stressed such distinctive techniques as mutual-assistance among job-seekers, a "buddy" system, family support and sharing of job leads. In addition, the program arranged special ways of using such common practices as searching want-ads, role-playing, telephoning, motivating the job-seeker, constructing a resume and contacting friends."

In our experiment, in each club, a caseworker is dedicated to a group of 10 to 14 job seekers and animates group activities with the goal to find to each participant a long-term job. Job seekers are invited to meet in a dedicated room reserved for them at the unemployment agency several times a week in order to do prospecting activities collectively with the support of their caseworker. They are also encouraged to meet outside of the agency and do "field activities" together such as visiting potential recruiters. The rhythm and format of meetings are much more flexible than in a standard counseling, to favor more intense and personal relations between participants and between the caseworkers and participants.

The control group We compare the effect of the experimental program with a standard intensive counseling program (*Objectif Emploi*), which is provided by a private operator located close to the individual's local employment agency. Participants in this program meet with their caseworker once a week in one-to-one meetings. Typically, caseworkers provide advice and suggestions of job offers during meetings and prospect for jobs matching with the job seekers in their portfolio between meetings. The scheme of the payment to the private operator includes an incentive for the caseworker to help job seekers find a stable job.

1.2.2 The random assignment

The selection process In each local agency, caseworkers selected participants for the experiment from the stock and the inflow of registered unemployed workers. Eligibility criteria included: being registered as unemployed at the public employment agency, being less than 30 years old, having a low level of education (at most a two-year college degree), living in a poor urban area (ZUS)[3], being immediately available for an intense counseling program, and not having any major

[3]The French urban policy identifies deprived neighborhoods, which are "characterized by the presence of large clusters or neighborhoods that are degraded and by a pronounced disequilibrium between housing and employment". It provides these areas with strong support to improve education, access, employment and security for the population living in these deprived areas to reduce territorial inequalities within urban areas. About 7% of the hexagonal

obstacle to employment—meaning that participants should not need a more long-term intervention such as a training or social support.

The experimental protocol is designed to assure a high compliance rate among eligible unemployed. The motivation for intensive job search counseling were therefore assessed by caseworkers in face-to-face meetings. During these meetings, job seekers were informed that the comparative evaluation of two counseling programs, and that they would be randomly assigned to one of them. Caseworkers were given precise guidelines to describe the common features of both intensive counseling programs and not describe their differences. When individuals agreed to join a counseling program and to participate in the experiment, they were required to give their consent to participate to an experiment and to commit to attend the counseling program they would be assigned to in an official form. However, this commitment could not be enforced and individuals did not face the risk of a sanction if they did not participate in the assigned program. New waves of recruitment were planned regularly for clubs to remain at a size of about 12 participants— as outflows occur continuously due to dropout and job finding.

The randomization process After enrolling in the experiment, participants were randomly assigned to the treatment or the control group. Randomization was performed pairwise by the research team, in order to guarantee that control and treatment groups are balanced in space and time: for each couple of selected participants, one is randomly assigned to the control group and the other to the treatment group. It should be noted that "pairs" were only observed by the caseworker and only relevant for the random assignment procedure. The objective was to assign individuals who enrolled in the experiment to one program and notify them of their first meeting within three days. In case caseworkers could not find an even number of job seekers willing to participate in the experiment, individual randomization was implemented on the "unmatched" individual in order to avoid long waiting times.

Table 1.1 compares the initial characteristics of the treatment and control groups. It shows that the distribution of observed characteristics is balanced between the group of individuals assigned to treatment and those assigned to the comparison group.

1.2.3 Compliance

Following the randomization, the participants were informed about their assigned program and a starting date was scheduled within a week. This short time frame was set to guarantee a high compliance rate. If they missed the enrollment meeting, caseworkers scheduled a new entry date as shortly as possible. In case of non compliance, job seekers could receive the regular support from the public employment agency, but they could not attend the programs from the experiment during 3 years.

Overall, the compliance rate is 84,9%, which is high compared to similar programs. However, we observe that the compliance rates for the two programs are different : 79.8% of individuals assigned to the control program effectively participated, compared to 90% of those assigned to a search club. Table 1.A.1 shows that observed characteristics of compliers (resp. non-compliers) are not balanced across assignment groups.

Several reasons may explain this difference in the compliance rates: the control program might

population live in one of the 751 existing ZUS (ONZUS, 2013).

Table 1.1: Average pre-assignment characteristics for treated and control groups

	Control	Treated (Club)	P-value: (1)=(2)
	(1)	(2)	(3)
Female	0.45	0.46	0.45
Age (years)	24.09	23.93	0.14
Single	0.83	0.83	0.67
French	0.85	0.86	0.42
Education: level of diploma			
No diploma	0.02	0.02	0.56
Middle school	0.13	0.12	0.29
Vocational high school	0.39	0.39	0.77
General high school	0.33	0.33	0.96
College	0.13	0.14	0.41
Receives means-tested benefit	0.25	0.25	0.77
No previous experience in desired occupation	0.39	0.37	0.31
Previous occupation			
Low qualification blue collar worker	0.07	0.07	0.79
High qualification blue collar worker	0.09	0.10	0.81
Low qualification white collar worker	0.30	0.30	0.95
High qualification white collar worker	0.46	0.45	0.82
Manager	0.05	0.05	0.90
Reason for registration			
Lay-off	0.08	0.07	0.29
Quit	0.02	0.02	0.77
End of contract	0.40	0.39	0.53
First entry on the LM	0.14	0.14	0.81
End of inactivity	0.03	0.04	0.69
Is immediately available for work	0.90	0.91	0.09
Declared reservation gross wage (euros)	1521.98	1527.91	0.68
Commuting distance accepted (km)	24.49	24.52	0.98
Commuting time accepted (min)	60.65	58.26	0.80
Labor market history			
Previous sickness spells	0.06	0.05	0.49
Previous subsidized employment	0.06	0.05	0.36
Previously on means-tested benefit	0.17	0.17	0.98
Cum. unempl. dur. at registration (in days)	354.88	338.84	0.30
Unempl. ancestry at randomisation (in days)	200.97	199.65	0.89
Average nb of previous registrations	2.11	2.03	0.29
No. of Obs.	1809	1795	

Notes: Column (3) reports the p-values associated with tests on the equality of means between the control and treatment groups.

have a bad reputation in some agencies due to information spillovers, the entry cost into the control program might be objectively or subjectively higher, or individuals might perceive a lower cost of defection if assigned to the control program. In Figure 1.A.1, we see no clear pattern in the evolution of the compliance rates gap over time (which would have been consistent with information spillover over time), but we can observe large heterogeneity in compliance rate gaps across agencies. In order to address this difference in compliance rate, we replicate our results in robustness checks excluding agencies with a large gap in compliance rates, and excluding pairs of job seekers with a different compliance status.

1.3 Data and empirical strategy

1.3.1 Data

The full sample is made of 3604 individuals who were selected to participate in the experiment. We use various sources of administrative data to track their trajectory for two years after randomization, as well as two surveys on search activities and perception of the labor market.

Administrative data First, we use experimental data collected by the caseworker to follow individuals through the experiment from the day of the selection. This source gives us information about the date of randomization and the date of (planned or effective) entry into the program, individual treatment and compliance states.

Second, we use the administrative records of the public employment agency. These records provide the individual's socio-demographic characteristics (age, gender, education level, family situation, living area) and labor market trajectory (previous registrations, date of registration for the current unemployment spell, reason for registration, type of job looked for, reservation wage, etc.). This data source allows us to track all deregistrations and registrations from unemployment registry after randomization.

Third, we combine these datasets with administrative data on employment contracts, the *Déclaration préalable à l'embauche*. Prior to hiring each new employee, any employer from the private sector has to fill a form indicating the day of start of the contract, the type of contract (indefinite-term, fixed-term and temporary) and the duration of the contract. This allows us to have information about the re-employment date of all randomized individuals.

Survey data To explore the mechanisms of the JSA group treatment, we conducted two follow-up surveys among the participants in the experiment. These data contain information like job search strategies, subjective expectations about future events, the reservation wage and the minimum contract duration individuals are willing to accept, and personality traits. We also asked the interviewees about their current labor market situation. This allows us to have more details about current jobs than available in administrative data, and in particular to know the current wage.

All participants from the experiment were contacted for an interview at both waves of the survey, independent of their compliance status. Participants were first interviewed around 2 months after the randomization date, while the counseling program was still going on. To evaluate medium-run effects of the program, randomized individuals were surveyed again around 6 months after randomization.

Surveys could be answered either by phone or online. The response rates for the first and second surveys are respectively 82% and 65%. Response rates to the survey are the same for people assigned to the club and to the control program. However, the probability of answering the surveys depends on the participation status. Response rates tend to be higher for participants. Responses to phone interviews are especially over-represented among participants. However, it does not seem that the survey mode affects answers.

1.3.2 Estimation strategy

Participants are either assigned to a search club or to the individual counseling program. Given the presence of non compliance in both treatments, we cannot identify the average treatment effect on the treated or a local average treatment effect. Instead, we estimate intention to treat (ITT) effects:

$$\Delta^{ITT} = E(Y \mid D = 1) - E(Y \mid D = 0)$$

where Y is the outcome variable (e.g. being employed) and D is the exogenous assignment status ($D = 1$ if the individual is assigned to the club and 0 otherwise). Exogeneity of D is given by randomization.

The ITT measures the effect of being assigned to a search club rather than to the control treatment. It captures the effects of the program, but also differences in the entry process. However, we argue that this is the relevant policy parameter since full compliance is difficult to enforce and the labor agency can only assign individuals to programs.

Predicting individual employability

We are interested in the effect heterogeneity of the search clubs with respect to the probability of leaving unemployment for a longer term job. We define this "employability" as the probability to find a long-term contract within the next 9 months. This probability is estimated with a logit model based on a large set of covariates determined before the randomization date. This measure has the advantage that it summarizes a large set of covariates in a meaningful baseline index.

We apply the leave-one-out approach suggested by Abadie et al., 2013 to avoid the bias induced by endogenous stratification based on an estimated baseline index. For this, we estimate the employment probability based on the group assigned to the control treatment. This model is used to predict the employability for all members of the group assigned to a search club. The prediction for each control group member i is based on the estimation with all control group members excluding i (leave-one-out). Table 1.A.2 shows results from the logit model estimated in the control group. Figure 1.A.2 shows the distributions of employability for controls and treated. The distributions are very similar, which is expected given the randomization of the assignment and the balanced observed characteristics reported in Table 1.1.

Measuring peer effects

Individual outcomes might depend on the group composition in the search club. A major challenge for the identification of the impact of the group composition is to disentangle the peer effects we are interested in from the correlation between characteristics of the group members and individual employment outcomes due to other reasons like the economic situation on the local labor market.

The continuous inflow of individuals into the search clubs creates variation in the group composition of clubs over time and across agencies, depending on variation in the pool of potential participants. Our empirical strategy is to use the control group which has been assigned to the standard counseling program to control for the correlation between the composition of peers and the individual employment outcomes. We use the fact that we observe the composition of the participants in the experiment and employment outcomes for a group for which there are no peer effects.

We can write the following potential outcomes model:

$$Y_{i,1} = \alpha_1 + \alpha_2 X_i + \alpha_3 f(X_{1i}, ..., X_{n_i i}) + \epsilon_i \tag{1.1}$$

$$Y_{i,0} = \beta_1 + \beta_2 X_i + \beta_3 f(X_{1i}, ..., X_{n_i i}) + \epsilon_i \tag{1.2}$$

where $Y_{i,1}$ is the potential outcome if individual i is assigned to a search club and $Y_{i,0}$ is the potential outcome if individual i is assigned to the control program. The outcomes depend on individual characteristics X_i and the group characteristics summarized in the function $f(X_{1i}, ..., X_{n_i i}) = f(X_{g(i),-i})$. The outcome additionally depends on the unobserved term ϵ_i. The assignment status is independent of those three elements. Combining equations (1) and (2) gives us the estimation equation:

$$\begin{aligned} Y_i &= \beta_1 + \underbrace{(\alpha_1 - \beta_1)}_{\delta_1} D_i + \beta_2 X_i + \underbrace{(\alpha_2 - \beta_2)}_{\delta_2} X_i \times D_i + \\ & \alpha_3 f(X_{1i}, ..., X_{n_i i}) + \underbrace{(\alpha_3 - \beta_3)}_{\delta_3} f(X_{1i}, ..., X_{n_i i}) \times D_i + \epsilon_i \end{aligned} \tag{1.3}$$

We can interpret δ_1 as the average impact of being assigned to clubs, δ_2 captures effect heterogeneity with respect to individual characteristics, and δ_3 can be interpreted as the causal effect of the group composition. In our main empirical analysis, we will use the individual and employability and the average employability at the group level as the main dimensions of effect heterogeneity.[4]

The main individual and group characteristic we consider is employability, denoted as Emp. Following the literature about peer effects, we consider the linear-in-means model (model (4)), in which the outcome of individual i in group g in agency a is allowed to depend linearly on her peers' average characteristics (i.e. $f(X_{g(i),-i}) = Emp_{g(i),-i}$).

$$Y_i = \beta_1 + \delta_1 D_i + \beta_2 Emp_i + \delta_2 Emp_i \times D_i + \alpha_3 Emp_{g(i),-i} + \delta_3 Emp_{g(i),-i} \times D_i + \eta_a + \epsilon_i \tag{1.4}$$

with η_a agency fixed effects. In this framework, we have to take into account that the employability is estimated. This is ignored in the current version of the paper. In the future, we will bootstrap the standard errors.

To allow for possible non linearities in peer effects, we consider more flexible specifications and allow for different effects depending on where the group mean stands relative to quantile q of the group mean employability distribution (q being the median, the first or second tercile, or the first, second or third quartile, depending on the specification). Last, we also allow for heterogeneous peer effects depending on individual employability relative the mean employability of the group, that is on where individual employability ranges relative to quantile r of the employability distribution within each group.

Group definition To implement the model, we need to define the groups and to build the $f(X_{g(i),-i})$ variables. We chose a "theoretical" definition for groups: we consider as members of someone's group people assigned to the same program (club or control) who were randomized in the same agency within a time window around own assignment day. More precisely, to each individual we attribute a group made of all the individuals randomly assigned to the same program in the 90 days before or after his own assignment date (oneself being excluded). We exclude oneself to disentangle the effect of one's own characteristics from the effect of the group characteristics. Individuals are weighted according to the "theoretical" number of days they overlap with individual i. Doing so, we account for the intensity of possible exposure to the peer.

We select this definition of group for three reasons: first, it permits to calculate what would

[4]An alternative way to flexibly control for the correlation between the composition of the pool of participants and the individual employment outcomes is to include fixed effects for the day of randomization x agency. We get very similar results for this type of specification.

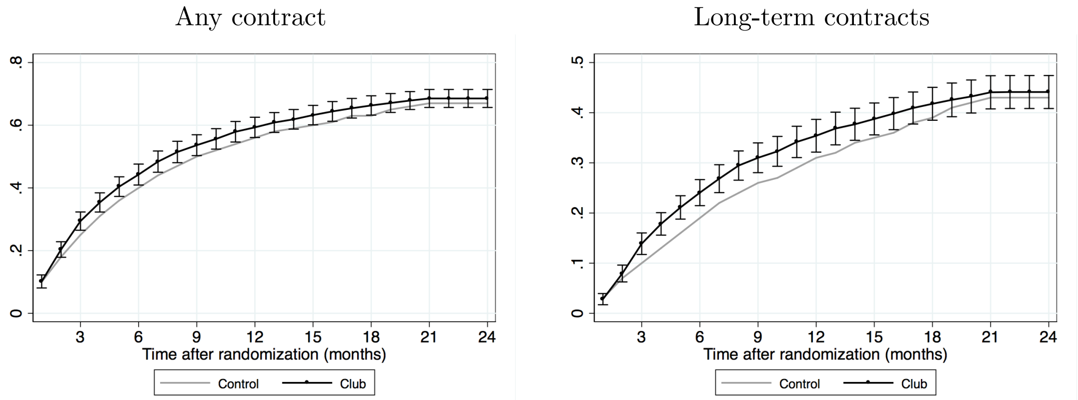
have been the group of someone assigned to the control program, had he be assigned to the treatment program. We need to have this fictive variable in order to be able to identify an effect in the comparison of control and treated groups. Second, it permits to have a definition that is not based on participation. As we focus on ITT, we need to be able to have the same variables for non-participants too. This corresponds to the club they would have been to, had they been participating. Third, a measure based on the actual group composition would suffer from potential endogeneity related to peers' participation and exit from the program. The theoretical and actual group composition variables are highly correlated and have a very similar distribution (see Figure 1.A.3).

1.4 Employment impact of club vs standard counseling

We first estimate the effect of being assigned to club rather than a standard counseling program on employment outcomes (ITT effect) in linear probability models. We consistently find a positive effect, driven by a better access to long-term jobs.

Average effects

Figure 1.1: ITT impact of club on cumulated probability to find a job



Notes: This figure presents the cumulated probability to find a job over time, for 2 years starting from the randomization date. The cumulated probability for job seekers in the two programs are presented separately. The vertical lines denote the confidence intervals at a 5% level for the gap in outcomes between the two groups, when controlling for individual covariates, time and agencies fixed effects. The gap between the two plots and the corresponding confidence intervals therefore provide a visual representation of the employment ITT effect. SE are clustered at the time∗agency level. Long-term contracts include fixed-duration contracts with a term longer than 6 months and indefinite duration contracts.

Table 1.2 and Figure 1.1 present the ITT impact of club vs standard counseling on the cumulative probability of having signed a contract after randomization, using administrative employment data for a two years window. We observe in the left panel of Figure 1.1 that a shift in the probability to have found a job starts to appear after 2 months, and remains for about one year. On the right panel, we see that this shift is relatively larger and more persistent in the case of long-term contracts only. Table 1.2 presents the corresponding magnitudes. Being assigned to the club rather than a standard program increases the probability to have found a job after 3 months by 4.2 ppt, which corresponds to a relative increase by 10%. This is driven by long-term contracts, as being assigned to a club increases the probability to have found a long-term job by 3.4 ppt, which corresponds to a 28% increase. The short-term effect on the job finding probability is therefore rather large, as the comparison group is also assigned to an intensive program.

Table 1.2: ITT impact of club on cumulated probability to find a job

	Finding a job before ... months							
	3	6	9	12	15	18	21	24
All jobs								
D	0.042***	0.045***	0.040**	0.033**	0.031*	0.029*	0.015	0.015
	(0.015)	(0.017)	(0.017)	(0.016)	(0.016)	(0.016)	(0.015)	(0.015)
Constant	0.386***	0.510***	0.770***	0.879***	0.852***	0.841***	0.934***	0.934***
	(0.079)	(0.053)	(0.078)	(0.066)	(0.064)	(0.076)	(0.051)	(0.051)
Short-term jobs								
D	0.005	-0.007	-0.009	-0.009	-0.004	-0.008	-0.017	-0.017
	(0.013)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.014)	(0.014)
Constant	0.301***	0.339***	0.520***	0.585***	0.563***	0.562***	0.610***	0.609***
	(0.046)	(0.064)	(0.074)	(0.060)	(0.058)	(0.068)	(0.061)	(0.061)
Long-term jobs								
D	0.034***	0.048***	0.051***	0.044***	0.040**	0.030*	0.016	0.015
	(0.011)	(0.013)	(0.015)	(0.017)	(0.016)	(0.017)	(0.017)	(0.017)
Constant	0.123	0.325***	0.445***	0.525***	0.522***	0.514***	0.549***	0.549***
	(0.079)	(0.067)	(0.091)	(0.082)	(0.078)	(0.069)	(0.052)	(0.053)
Time fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Agency fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	3604	3604	3604	3604	3604	3604	3604	3604

Notes: Long-term contracts include fixed-duration contracts with a term longer than 6 months and indefinite duration contracts. Short-term contracts include all other temporary contracts. We include agency fixed-effect corresponding to the agency of assignment and time fixed-effect corresponding to the month of assignment. SE clustered at the time∗agency level are presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

The administrative source of data does not allow us to measure the employment rate—which is an usual employment outcome in the literature. Therefore, we present estimates of the impact of club on employment rates using information collected in the survey in Table 1.B.1. The upper panel of Table 1.B.1 shows that individuals assigned to a club have a higher probability to have worked and to be working at the time of the first survey. Being assigned to a club increases the employment probability by 7 ppt (32% instead of 26%), which represents a 25% increase. In particular, we estimate an increase in the probability to have a long-term job by 4ppt. In the bottom panel of table 1.B.1, we see that the effect on the employment rate remains positive after 6 months and is around 4ppt. This estimate appears to be rather large if we compare it with the 1.2 ppt average effect of job search assistance programs on employment rate rate found in the literature during the first year (Card et al., 2018). In addition to that, the assignment to a club increases the probability of being employed full-time in the short run, but does not allow workers to earn more than the monthly full-time minimum wage. Assignment to the club therefore does not seem to importantly affect re-employment hourly wages.

Effect heterogeneity

We then investigate whether the relative efficiency of club in comparison to a standard program depends on individual characteristics. We show that the club outperforms the standard counseling for all job seekers, irrespective of their baseline employability level.

In Table 1.3, column (1) shows that employability does not affect the treatment effect. The coefficient associated with the continuous measure of employability interacted with the treatment

Table 1.3: Heterogeneous impact of club on employment outcomes

Long contract before	6 months			12 months		
	(1)	(2)	(3)	(1)	(2)	(3)
D	0.048***	0.060***	0.048**	0.043***	0.060***	0.049*
	(0.013)	(0.018)	(0.022)	(0.017)	(0.021)	(0.026)
D*Emp_i	0.001			-0.008		
	(0.014)			(0.016)		
Emp_i	0.022*			0.034**		
	(0.012)			(0.014)		
D*$\mathbb{1}\{\text{Emp}_i \geq m\}$		-0.023			-0.033	
		(0.029)			(0.033)	
$\mathbb{1}\{\text{Emp}_i \geq m\}$		0.041*			0.065***	
		(0.021)			(0.025)	
D*$\mathbb{1}\{\text{Emp}_i \in [t_1, t_2]\}$			0.010			0.011
			(0.032)			(0.037)
D*$\mathbb{1}\{\text{Emp}_i \geq t_2\}$			-0.011			-0.027
			(0.035)			(0.039)
$\mathbb{1}\{\text{Emp}_i \in [t_1, t_2]\}$			0.013			0.034
			(0.021)			(0.026)
$\mathbb{1}\{\text{Emp}_i \geq t_2\}$			0.051*			0.090***
			(0.028)			(0.032)
Constant	0.193***	0.173***	0.172***	0.311***	0.278***	0.269***
	(0.009)	(0.013)	(0.016)	(0.011)	(0.016)	(0.020)
Agency fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	3604	3604	3604	3604	3604	3604

Notes: Long-term contracts include fixed-duration contracts with a term longer than 6 months and indefinite duration contracts. In columns (1) the model includes a continuous measure of individual employability, while employability is discretized in columns (2) and (3): dummies represent the individual position in the experimental sample distribution. In column (2), the position relative to the median is indicated while in column (3), the position relative to terciles is presented. Emp_i is the *ex ante* employability of individual i. SE clustered at the time∗agency level are presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

dummy is very close to zero. In columns (2) and (3), we replicate this results using a discrete employability measure: we find that having an employability level above the median of the individual employability distribution does not affect the effect of being assigned to the club (if anything, it diminishes it), nor being in the second or third decile.

Additionally, we present the results in terms of other heterogeneity dimension in Appendix (Table 1.B.4). Consistent with the absence of heterogeneity in terms of employability, we find that variables related to skills, education and labor market history do not affect the efficiency of the program. However, the program is more than three times more efficient for women (it increases the probability to find a job within 6 months by 38% for women and 12% for men). This gender heterogeneity in treatment effect is in line with what is usually found in active labor market policies (Card et al., 2018).

Sensitivity analysis

In Table 1.B.2, we present estimates of the ITT impact of club on the probability to find a job before 6 and 12 months (measured in administrative data) obtained in different specifications. The empirical setting was designed so that treated and control groups are balanced across agencies and over time, and including agencies or time fixed effect should therefore be unnecessary. This is confirmed by the comparison of columns (1), (2), (3) and (4). In columns (5), we exploit the pairwise randomization and compare the outcome of job seekers within randomization pairs. The results are similar in all these different specifications, confirming the validity of our empirical design.

The gap in compliance rate across treatment groups however raises the concern that the ITT estimates reflect the effect of compliance rather than the effect of the programs themselves. We suspect that some of the gap in compliance between the programs come from some failure in the implementation of the empirical setting at the agency level (longer delays to enter the control program, lower follow-up of job seekers assigned to the control program, bias in the presentation of the two programs during the selection procedure). In Table 1.B.3, in Part I.A and I.B, we therefore estimate the ITT impact of club on the probability to find a job before 6 and 12 months excluding agencies where the gap is superior to 10% and 5%. The magnitude of the estimates in these subsamples is very similar to the one in the full sample, if anything slightly larger. In part I.C, we include the average compliance gap interacted with the treatment directly in the covariates and observe that the associated estimated parameter is not significant (and if anything negative). Additionally, in part II.A we find similar estimates when excluding all pairs with a different compliance status. This table provides compelling evidence that the ITT is not driven by the gap in compliance rate.

1.5 Peer effects

We now turn to the role of the group composition in terms of employability. We first consider peer effects in the linear-in-means-model and test for the effect of the group's mean employability. Column (1) of Table 1.4 shows that independent of the participant's own employability, having low employability peers increases the probability of getting a durable job within 6 and 12 months after randomization. A one-standard-deviation increase in group mean employability (corresponding on an increase in average employability probability by 8.5 ppt) significantly reduces reemployment by about 6.6 ppt. This is equivalent to being assigned to a group in the 80th percentile of the distribution of group employability in our sample instead of the median. This result suggests that peer effects in our context are negative and substantial. The estimate of the impact of one's own employability is significantly positive when controlling for the group quality: one-standard-deviation increase in individual employability (corresponding to an increase in individual employability probability by 13 ppt) increases re-employment probability by about 4 ppt.

This result of a negative impact of the average quality of peers is robust to the inclusion of the within-group standard deviation of employability along with the group mean employability[5], and to the inclusion of agency-time fixed effects (Table 1.C.2). Our result is also not sensitive to the specification used to predict employability (Table 1.C.1).

To have a clearer picture of the anatomy of the estimated peer effects and to allow for non linearities, which are found to be important in the literature of peer effects in education (Hoxby et al., 2005), we consider more flexible specifications. We split the groups into categories, according to their position to different quantiles of the group mean employability distribution. Columns (2) of Table 1.4 shows that clubs significantly increase the short and medium run reemployment probabilities, but only when the individual is assigned to groups belonging to the bottom half of the group mean employability distribution. No significant effects are found for groups of mean employability above the median. The fact that the effect is concentrated in low average employability groups

[5]Note that the dispersion of employability with the club has no significant effect on the reemployment probabilities, indicating that given the group mean, the degree of homogeneity in terms of employability does not matter for the effectiveness of the program.

Table 1.4: Impact of club depending on the mean employability of the group

Long contract before	6 months			12 months		
	(1)	(2)	(3)	(1)	(2)	(3)
D	0.047***	0.087***	0.084***	0.042**	0.074***	0.086***
	(0.013)	(0.017)	(0.020)	(0.017)	(0.022)	(0.027)
D*Emp_i	0.040**			0.029		
	(0.018)			(0.019)		
Emp_i	0.004			0.017		
	(0.013)			(0.015)		
D*$\text{Emp}_{g(i),-i}$	-0.066***			-0.064***		
	(0.017)			(0.020)		
$\text{Emp}_{g(i),-i}$	0.042*			0.059*		
	(0.025)			(0.032)		
D*$\mathbb{1}\{\text{Emp}_{g(i),-i} \geq m\}$		-0.077***			-0.059*	
		(0.025)			(0.033)	
$\mathbb{1}\{\text{Emp}_{g(i),-i} \geq m\}$		0.028			-0.010	
		(0.029)			(0.034)	
D*$\mathbb{1}\{\text{Emp}_{g(i),-i} \in [t_1, t_2]\}$			-0.034			-0.045
			(0.032)			(0.041)
D*$\mathbb{1}\{\text{Emp}_{g(i),-i} \geq t_2\}$			-0.079**			-0.087**
			(0.033)			(0.041)
$\mathbb{1}\{\text{Emp}_{g(i),-i} \in [t_1, t_2]\}$			0.007			-0.021
			(0.033)			(0.036)
$\mathbb{1}\{\text{Emp}_{g(i),-i} \geq t_2\}$			0.072			0.053
			(0.048)			(0.055)
Constant	0.194***	0.178***	0.168***	0.312***	0.315***	0.301***
	(0.009)	(0.017)	(0.025)	(0.011)	(0.021)	(0.029)
Agency fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	3604	3604	3604	3604	3604	3604

Notes: Long-term contracts include fixed-duration contracts with a term longer than 6 months and indefinite duration contracts. Emp_i is the employability of individual i, $\text{Emp}_{g(i),-i}$ is the mean employability within the group g of job seeker i. In columns (1) the model includes a continuous measure of group mean employability, while group mean employability is discretized in columns (2) and (3): dummies represent the individual position in the group mean employability distribution. In column (2), the position relative to the median is indicated while in column (3), the position relative to terciles is presented. SE clustered at the time$*$agency level are presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.5: Impact of club depending on the interaction of individual and group employability

Long contract before	6 months		12 months	
	(1)	(2)	(1)	(2)
D × Ind. low & Group low	0.095***		0.093***	
	(0.020)		(0.025)	
D × Ind. high & Group low	0.070**		0.031	
	(0.033)		(0.040)	
D × Ind. low & Group high	-0.024		-0.019	
	(0.034)		(0.039)	
D × Ind. high & Group high	0.022		0.026	
	(0.025)		(0.031)	
D × Ind. low within group & Group low		0.032		0.065
		(0.035)		(0.046)
D × Ind. medium within group & Group low		0.086**		0.097**
		(0.038)		(0.045)
D × Ind. high within group & Group low		0.127***		0.095**
		(0.034)		(0.042)
D × Ind. low within group & Group medium		0.091**		0.062
		(0.039)		(0.041)
D × Ind. medium within group & Group medium		0.009		0.019
		(0.042)		(0.049)
D × Ind. high within group & Group medium		0.047		0.045
		(0.044)		(0.051)
D × Ind. low within group & Group high		-0.031		0.010
		(0.041)		(0.046)
D × Ind. medium within group & Group high		0.006		-0.056
		(0.056)		(0.062)
D × Ind. high within group & Group high		0.046		0.036
		(0.048)		(0.052)
Constant	0.202***	0.165***	0.293***	0.305***
	(0.027)	(0.031)	(0.031)	(0.034)
Agency fixed effect	Yes	Yes	Yes	Yes
No. of Obs.	3604	3604	3604	3604

Notes: Long-term contracts include fixed-duration contracts with a term longer than 6 months and indefinite duration contracts. In columns (1) individual employability is defined as low (resp. high) if it falls below (resp. above) the median of the distribution of individual employability. Symmetrically, the mean employability of the group is considered low (resp. high) if it falls below (resp. above) the median of the distribution of group mean employability. In columns (2), individual employability is considered low within her group if her employability is below the first tercile of her group, medium if it falls in the second tercile and high if it is above the third tercile. The mean employability of the group is also compared to terciles of the distribution of group mean employability. SE clustered at the time∗agency level are presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

is confirmed when we categorize groups according to their position relative to terciles[6]: treated individuals have a reemployment probability that increases by 8.4ppt (significant at 1%) when assigned to a group belonging to the first tercile. The effect is almost divided by two (significant at 5%) when the individual is assigned to a group from the second tercile. It is not significant in case of an assignment to a group of the top tercile – compared to controls, the effect of treatment in high ability groups is close to 0. We find similar results when we consider the probability of reemployment within 12 months after randomization.

Heterogeneous peer effects

We finally turn to the analysis of the interaction between one's own employability and the employability of peers. In the context of education, there is a strong evidence that the effect of the ability of peers may be different depending on the individual own ability, though the salience and mechanisms differ strongly depending on the context (Sacerdote, 2011). Do we observe heterogeneous effects in the context of job search assistance programs?

[6]We also find similar results when we consider the relative position to quartiles. Results are not shown for sake of space.

The top panel of Table 1.5 reveals that the estimated peer effects are homogeneous for low and high employability job seekers. Individuals assigned to groups with low average employability do not differently benefit from the treatment when they are of low or high employability (i.e. in the bottom half or top half of the overall individual employability distribution). Similarly, being assigned to groups with high average employability has no significant effect on the reemployment probability for both low and high employability job seekers. Within each group level (low or high), the coefficients associated to the individual level (low or high) are not significantly different, both in the short and in medium runs.

The bottom panel of Table 1.5 shows that on the contrary the relative position within the group matters, but only in groups with low mean employability. We classify here individuals and groups relative to the first and third terciles of the within-group and group employability distributions respectively. We find that clubs are more effective than the control program for individuals who are assigned to low-average groups and who fall in the second and top terciles of the group-specific employability distribution. Individuals who are assigned with low-employability peers and fall in the bottom third of the group employability distribution do not suffer from being less employable than their peers, they do not perform differently than those assigned to the control program. On the contrary, the relative position within the group has no significant impact on performance when assigned to groups of intermediate or high mean employability. Last, we find that the treatment is significantly more effective for individuals in the upper part (top tercile of within group distribution) of low-employability groups than for individuals in the lower part (bottom and middle terciles of within group distribution) of high-employability groups. This set of results is the same whether we look at the effect on the reemployment probability within 6 or 12 months.

1.6 Discussion on potential mechanisms

There exists a number of models of peer effects (Hoxby et al., 2005) and the literature has highlighted several possible channels through which the quality of peers might affect an individual behaviors and outcomes. Most channels however predict *positive* peer effects and seem therefore irrelevant in our context: peer learning, peer-pressure. The main channel that could predict *negative* peer effects is that peers induce a shift in self-perception, and seems the most credible in our context. In this part, we will first discuss evidence supporting this channel and then report evidence against some other potential channels using intermediary outcomes measured in surveys.

1.6.1 Frame of reference and self-confidence

Theoretical framework The role of social comparison in the construct of self-perception is at the core of the frame of reference model, introduced by Marsh and Parker, 1984. According to this model initially developed to analyze academic performance, the self-concept of an individual is positively influenced by his own ability, but negatively affected by the peers average achievement: an individual of a given ability will have a lower self-concept when the average ability of peers is high than when it is low. In other words, it is better to be of a high type in a low achieving group than of a high type in a high achieving group, which has been summarized as the "Big fish in a little pond" effect. Hoxby et al., 2005 describe similar mechanisms and insist on the role of the relative position in the group: Interacting with better (lower-ability) peers would lower (improve) the individual self-esteem and self-confidence for instance.[7] The relevance of this mechanism has been demonstrated in education (Pop-Eleches and Urquiola, 2013 ; Antecol et al., 2016).

This theory predicts that in our context, individuals assigned to more employable job seekers would suffer from the comparison, be discouraged and exert lower search efforts or revise downwards their job requirements, leading to poorer performance on the labor market. On the contrary, treated assigned to peers of lower employability would benefit from the comparison and gain in self-esteem and be more optimistic about their job search, which would encourage them to exert greater search effort or would allow them to better perform in job interviews, making them eventually more successful in their job search. Our main group composition results are therefore consistent with this line of explanation. Although having good and better peers do not effectively hurt job seekers, having peers that are on average of lower employability benefit job seekers in bad groups.

Intermediary outcomes To investigate further whether this mechanism is plausible in our case, we turn to the analysis of the impacts of club on job seekers' behaviors and perceptions based on our survey data. Table 1.6 presents the average and heterogeneous impact of club on the most relevant intermediary outcomes obtained: in part I, we estimate the ITT impact of club; in part II, we include the interaction of the treatment with the group mean employability to assess the role of peers' quality; and in part III, we estimate in a non linear model the role of interactions of individual employability and group mean employability. Note that for survey variables, we use the subsample of 2120 respondents. In column (1), we replicate on this subsample the main results from previous sections concerning the impact of club on employment outcome. Estimates on this subsample are very similar to the ones obtained on the full sample.

[7] Hoxby et al., 2005 call it the invidious comparison model and propose the following definition: "advent of a higher achieving peer depresses the performance of everyone who is pushed to a lower rank in the local distribution (presumably by depressing their self-esteem). The advent of a lower achieving peer has the opposite effect: boosting the performance of all those who are pushed to a higher local rank."

Table 1.6: Average and group composition effects on survey outcomes

	Employment outcome	**Reported behaviors**				**Reservation value**		**Subj. proba. of being in 3 months**	
	LT Contract before 6 months (dummy)	Talk to peers (dummy)	Exchange job offers (dummy)	Applications overall (Number)	Applications via caseworker (Number)	Res. contract term > 6 month (dummy)	Res. wage > Min Wage (dummy)	on permanent contract (probability)	Unemployed (probability)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Part I: Average impact of club									
D	0.062***	0.070***	0.018	0.518	0.207***	0.038***	-0.045**	2.965**	-3.309**
	(0.016)	(0.021)	(0.022)	(0.910)	(0.021)	(0.012)	(0.021)	(1.329)	(1.406)
Constant	0.186***	0.567***	0.492***	16.689***	0.383***	0.067***	0.439***	34.739***	43.855***
	(0.011)	(0.015)	(0.015)	(0.581)	(0.013)	(0.007)	(0.015)	(0.886)	(1.001)
Part II: Role of individual employability									
D	0.059***	0.069***	0.018	0.315	0.208***	0.037***	-0.044**	3.092**	-3.186**
	(0.016)	(0.021)	(0.022)	(0.911)	(0.021)	(0.011)	(0.021)	(1.333)	(1.419)
D*Emp$_{g(i),-i}$	-0.052**	0.015	-0.002	-2.888***	0.001	-0.019	-0.016	-2.289	1.006
	(0.022)	(0.025)	(0.026)	(1.077)	(0.025)	(0.014)	(0.026)	(1.555)	(1.605)
Constant	0.189***	0.568***	0.491***	16.874***	0.382***	0.067***	0.437***	34.638***	43.765***
	(0.011)	(0.015)	(0.015)	(0.595)	(0.013)	(0.007)	(0.016)	(0.898)	(1.013)
Emp$_{g(i),-i}$, Emp$_i$, D*Emp$_i$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Part III: Role of individual and group employability									
D×Ind. low in group & Group low	0.086***	-0.024	-0.029	0.337	0.136***	0.010	-0.010	4.667	0.623
	(0.033)	(0.040)	(0.040)	(1.628)	(0.046)	(0.021)	(0.042)	(2.843)	(2.913)
D×Ind. high in group & Group low	0.116***	0.089**	0.031	2.813*	0.278***	0.055**	-0.083**	4.862*	-8.075***
	(0.031)	(0.041)	(0.045)	(1.560)	(0.041)	(0.022)	(0.042)	(2.676)	(2.512)
D×Ind. low in group & Group high	-0.007	0.109**	-0.013	-2.248	0.237***	0.022	-0.079*	0.986	-1.636
	(0.036)	(0.044)	(0.042)	(1.829)	(0.039)	(0.024)	(0.041)	(2.413)	(2.698)
D×Ind. high in group & Group high	0.051	0.107**	0.085*	1.317	0.169***	0.068**	0.008	0.626	-3.228
	(0.039)	(0.042)	(0.049)	(1.738)	(0.045)	(0.027)	(0.043)	(2.598)	(2.909)
Constant	0.225***	0.562***	0.481***	17.107***	0.379***	0.072***	0.486***	34.459***	43.896***
	(0.033)	(0.035)	(0.032)	(1.435)	(0.032)	(0.020)	(0.032)	(1.880)	(2.606)
Emp$_{g(i),-i}$, Emp$_i$, D*Emp$_i$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Agency fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	2120	2120	2120	2120	2120	2120	2120	2120	2120

Notes: This table presents the impact of being assigned to a club on various intermediary outcomes measured in our survey. The sample size therefore corresponds to the subsample of respondents among participants to the experiments. SE clustered at the time∗agency level are presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

In columns (2) to (5), we report the estimates concerning job seekers' behaviors. Strikingly, although clubs do not increase search intensity—as measured by the number of applications that job seekers declare (column (4))—, within clubs, being assigned to bad peers clearly does: being assigned to a group with a group mean employability lower by 1 standard deviation induce job seekers to make 2.8 additional applications. This is consistent with bad peers fostering search efforts. In contrast, direct exchange of information with other peers (columns (2) and (3)) and the intervention of caseworkers (column (5)) seem independent from the group mean employability.

Columns (6) and (7) present the results concerning job seekers' search strategy. Job seekers are asked about the minimum duration of a contract that they would accept and the minimum wage that they would accept. The club causes job seekers to increase their reservation contract duration while decrease their reservation wage. Having low peers seem to rather encourage the shift in reservation contract duration although the estimate is imprecise, while part III suggests that being employable in comparison to one's group is what matters the most for this adjustment. However the absolute nor the relative employability of peers seem to affect the reservation wage strategy.

Last, columns (7) and (8) report the effect of assignment to club on the subjective expectations of an individual about her status in three months. One limitation of this measure is that it partly captures changes in actual employment status 2 months after the start of the experiment, however it still provide insight on job seekers self-confidence regarding future labor market status. When assigned to club, a job seeker reports higher subjective probability of being on an open-ended contract and a lower subjective probability of being unemployed in 3 months. There is no significant linear effect of peers' average employability on such expectations, although the signs are consistent with the frame of reference model: good peers have a negative impact on the subjective probability of being in a permanent contract in 3 months, and a positive impact on the subjective probability to stay unemployed. In the non linear model in part III, we find that clubs shift significantly these perception when individuals are relatively more employable than their low-employability peers

Overall, intermediary outcomes provide a rather consistent picture: it seems that "bad" peers indeed foster search effort, and that being "good" in comparison with one's peers favor a shift towards more selectivity in terms of contract duration which is the main selectivity margin for this population group, given that their reservation wage is bounded by minimum wage. Being better than the peers also seems to boost the individual's positive expectations about the outcomes of his job search. The relation we highlight between peers' ability and individual perception and search effort confirms the prediction from frame of reference mechanism. Besides, the relation that we find between subjective perceptions and search efforts are in line with the literature using behavioral insights to explain search behaviors (Falk et al., 2006, Spinnewijn, 2015, Caliendo et al., 2015).

1.6.2 Alternative mechanisms

Other potential mechanisms discussed in the literature could have seemed relevant as well in the context of JSA, however most of them should generate a positive effect of peer quality, which is inconsistent with our results.

Peer learning Clubs allow for information transmission in a different format than usual JSA: people can learn from their peers by direct information exchange, or by observing the payoffs a peer gets from his actions, which could increase job offer arrival rates or efforts and ultimately their reemployment probability. Evidence of such peer learning are found in lab experiments (Kimbrough et al., 2017), in a field experiment in the context of on-the-job training (Grip and Sauermann, 2012) and in the workplace (Jackson and Bruegmann, 2009 ; Cornelissen et al., 2017). The literature in education sciences shows that peer learning can be a successful learning technique to improve students' achievements when it is carefully structured and supervised (Fuchs et al., 1997 ; Topping, 2005). However, since knowledge is most likely disseminated by good or better peers, all individuals should benefit from having high achieving peers in case of peer learning. Therefore our results do not support the idea that peer learning is the main driver here. Additionally, although our survey data clearly indicate that young job seekers assigned to clubs report more interactions with peers (Table 1.6), treated job seekers do not report exchanging more job offers. Moreover, we do not observe that the intensity of interactions significantly differs depending on the group employability.

Peer pressure effects Peer effects can also act through peer-pressure effects: individuals would tend to behave as the others because there is a cost at deviating from the norm or group mean. Evidence of peer pressure effects in the workplace among coworkers is found in a number of experimental and non experimental studies (see for instance Falk and Ichino, 2006 ; Mas and Moretti, 2009 ; Bandiera et al., 2010 ; Cornelissen et al., 2017). However, the general theoretical and empirical findings of this literature is that having higher-ability and more productive peers increases the one's productivity. Hence in our context, we should observe positive peer effects on employment and also on search efforts, assuming that more employable job seekers exert more search efforts. The peer effects we estimate go in the opposite direction both in terms of employment outcome and search efforts.

Endogenous caseworker's response Empirical studies about classroom peer effects are usually concerned by the idea that the observed effect of the group composition may be due to changes in the teacher's practices. Duflo et al., 2011 show that teachers adapt their teaching to the level of their students and that student ability-tracking may also be beneficial to low-achieving students due to this change in teacher behaviors. Lavy et al., 2011 use non experimental data to show that low-ability peers are detrimental to high-ability students because they deteriorate relations among students, between students and teachers and ultimately teaching quality. Overall, this literature suggests that being in a more homogeneous group has a positive impact on outcomes. In our context, it would imply that high (resp. low) employability job seekers are disadvantaged by being with low (resp. high) employability peers because the caseworker would offer a support less suitable to their needs. It is inconsistent with our findings. Additionally, in Table 1.6, although we find that youths assigned to clubs rely significantly more on the caseworker to look for a job, sending applications to job offers found thanks to the caseworker do not depend on the group mean employability, nor on the relative position within the group.[8]

[8]It should be noted that our estimates are based on variation of group employability within agency, and therefore within caseworker, so our results can not be driven by the fact that high-quality caseworker would be assigned to low-employability groups.

1.7 Conclusions

In this article, we show that there is room to improve the efficiency of counseling program for job seekers, and in particular for the job seekers who are the most detached from the labor market. We evaluate the impact of an innovative program targeted for young unemployed workers living in deprived neighborhoods in France: "Search club". This program is based on team work in a group of about 12 job seekers with one entitled caseworker. We find a large positive effect on the access to long-term jobs of being assigned to a search club rather than a standard intense counseling program.

Additionally, we show that job seekers assigned to clubs with less employable peers are more likely to find a job. This negative peer effect is in contradiction with most of the literature on peer effects, although it is consistent with the frame of reference mechanism. In this model, being a "big fish in a little pond" has a positive effect on self-confidence and therefore increases efforts and improves outcomes. Consistent with this model, we find suggestive evidence of a stronger shift in job seekers' perceptions about their future labor market status in "bad groups", a higher selectivity, and an increased search effort. We review alternative mechanisms through which peers might affect search behaviors and outcomes, and conclude that either these mechanisms are not relevant in this context or they are dominated by the frame of reference mechanism.

Overall, our results suggest that social programs can use peer effects to activate some behavioral mechanisms which improve their effectiveness. In the case of young disadvantaged job seekers, we find that self-confidence seems to be an important factor of success, which suggests that JSA programs can be more effective by addressing this dimension in their design. This finding is consistent with evidence from Antecol et al., 2016 on the same population but in the context of education. The frame of reference mechanism could therefore be particularly relevant for this population, which is known to be very exposed to motivational, mental health and self-esteem issues (IGAS, 2010 and ONZUS, 2013). It could also be more relevant in the context of job search than in education, as unsuccessful job search has been shown to be very discouraging and depressing (Mueller and Krueger, 2012). More research is needed to determine under which conditions the frame of reference mechanism dominates.

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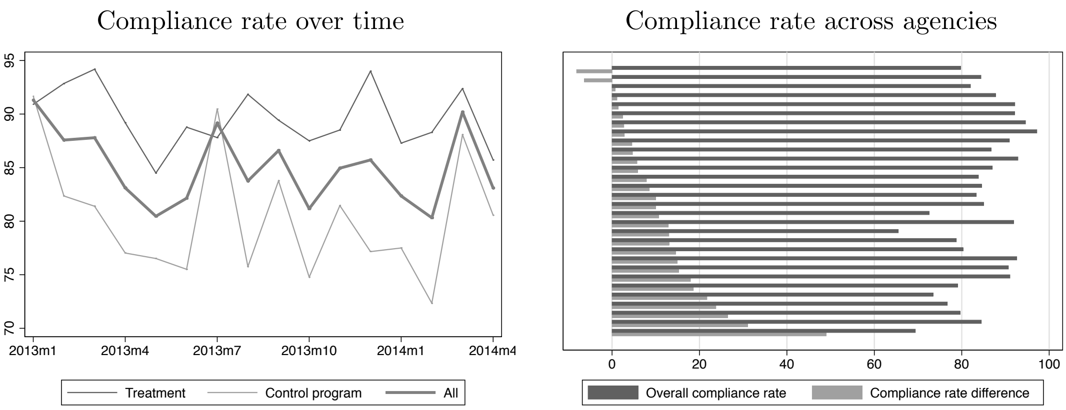
Appendix

1.A.1 Empirical setting

Figure 1.A.1: Randomized individuals and compliance rates over time and across agencies



Notes: This figure presents the compliance rate in each treatment group over (calendar) time during all the experiment on the left hand side. The right hand side presents the average compliance rate during all the experiment for each of the 30 unemployment agencies. The gap in compliance between the two groups is also presented.

Table 1.A.1: Average pre-assignment characteristics for treated and control groups, by compliance status

	Compliers			Non-compliers		
	Control	Treated (Club)	P-value: (1)=(2)	Control	Treated (Club)	P-value: (1)=(2)
	(1)	(2)	(3)	(4)	(5)	(6)
Female	0.45	0.46	0.66	0.44	0.47	0.42
Age (years)	24.19	23.94	0.04	23.71	23.83	0.69
Single	0.82	0.84	0.33	0.84	0.78	0.14
French	0.85	0.86	0.22	0.86	0.83	0.36
Education: level of diploma						
No diploma	0.02	0.02	0.68	0.02	0.02	0.86
Middle school	0.13	0.12	0.48	0.16	0.15	0.79
Vocational high school	0.38	0.38	0.81	0.41	0.46	0.35
General high school	0.33	0.33	0.87	0.32	0.27	0.28
College	0.14	0.15	0.87	0.08	0.09	0.63
Receives means-tested benefit	0.26	0.25	0.50	0.23	0.26	0.51
No previous experience in desired occupation	0.40	0.36	0.06	0.35	0.43	0.06
Previous occupation						
Low qualification blue collar worker	0.07	0.07	0.91	0.08	0.06	0.44
High qualification blue collar worker	0.09	0.09	0.99	0.10	0.13	0.30
Low qualification white collar worker	0.30	0.30	0.90	0.31	0.30	0.77
High qualification white collar worker	0.47	0.46	0.62	0.42	0.41	0.91
Manager	0.04	0.04	0.75	0.06	0.07	0.77
Reason for registration						
Lay-off	0.08	0.07	0.73	0.10	0.07	0.15
Quit	0.01	0.02	0.54	0.02	0.01	0.49
End of contract	0.40	0.40	0.95	0.41	0.31	0.02
First entry on the LM	0.15	0.14	0.52	0.12	0.13	0.67
End of inactivity	0.04	0.03	0.85	0.03	0.06	0.08
Is immediately available for work	0.90	0.92	0.06	0.90	0.89	0.80
Declared reservation gross wage (euros)	1524.35	1525.68	0.93	1511.85	1549.28	0.29
Declared maximim commuting distance (km)	25.25	24.62	0.60	21.38	23.71	0.28
Declared maximum commuting time (min)	57.53	59.64	0.83	72.95	44.38	0.32
Labour market history						
Previous sickness spells	0.05	0.05	0.30	0.06	0.09	0.19
Previous subsidized employment	0.05	0.05	0.57	0.06	0.04	0.38
Previously on means-tested benefit	0.18	0.17	0.58	0.15	0.18	0.29
Cum. unempl. dur. at registration (in days)	354.37	332.05	0.19	356.90	399.74	0.31
Unempl. ancestry at randomisation (in days)	205.40	202.99	0.82	183.43	169.66	0.59
Average nb of previous registrations	2.01	1.99	0.82	2.53	2.37	0.52
No. of Obs.	1444	1615		365	180	

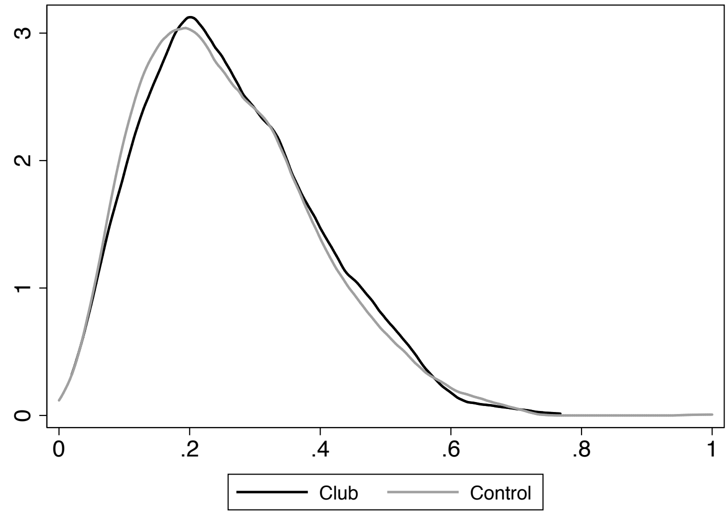
Notes: Column (3) reports the p-values associated with tests on the equality of means between the control and treatment groups, among compliers and non compliers.

Table 1.A.2: Logit model for the employability index

	Specification 1	Specification 2	Specification 3
	(1)	(2)	(3)
Female	-0.025	-0.639	-0.967
	(0.143)	(0.899)	(1.141)
Age			
21-24	0.206	0.312	0.229
	(0.186)	(0.249)	(0.258)
25-27	-0.046	0.267	0.157
	(0.222)	(0.291)	(0.309)
28-32	0.103	0.532	0.390
	(0.253)	(0.331)	(0.356)
Single	0.224	0.049	0.090
	(0.172)	(0.260)	(0.267)
French	0.314*	0.251	0.240
	(0.180)	(0.234)	(0.243)
Education			
Low diploma	-0.819	-0.885	-0.897
	(1.460)	(1.502)	(1.529)
General high school	-0.666	-0.705	-0.604
	(1.452)	(1.490)	(1.516)
Vocational high school	-0.825	-0.753	-0.705
	(1.453)	(1.492)	(1.519)
College degree	-0.289	-0.315	-0.202
	(1.455)	(1.469)	(1.492)
Gets means-tested benefits	-0.209	-0.459**	-0.552**
	(0.167)	(0.232)	(0.247)
Work experience			
Low	0.055	0.019	-0.113
	(0.190)	(0.255)	(0.271)
Intermediary	-0.222	-0.106	-0.240
	(0.192)	(0.256)	(0.271)
Long	-0.266	-0.239	-0.389
	(0.188)	(0.253)	(0.264)
Very long	-0.035	0.126	-0.019
	(0.197)	(0.257)	(0.272)
Qualification			
High skills, blue collar	0.068	0.168	0.165
	(0.270)	(0.283)	(0.292)
Low skills, white collar	-0.068	0.234	0.196
	(0.229)	(0.273)	(0.284)
High skills, white collar	0.203	0.220	0.212
	(0.214)	(0.247)	(0.257)
Manager	-0.584	-0.379	-0.494
	(0.405)	(0.414)	(0.421)
Is immediately available for work	0.004	-0.309	-0.345
	(0.192)	(0.296)	(0.310)
Previous employment spells $\in [4, 12]$ (months)	-0.027	-0.053	0.158
	(0.204)	(0.206)	(0.289)
Previous employment spells > 12 (months)	-0.103	-0.098	0.094
	(0.204)	(0.206)	(0.300)
# Previous registration $= 1$	0.297	0.319*	0.373
	(0.189)	(0.190)	(0.261)
# Previous registration > 1	0.226	0.220	0.081
	(0.216)	(0.219)	(0.310)
Past U spell $\in [2, 4]$ (months)	-0.121	-0.116	0.002
	(0.163)	(0.165)	(0.227)
Past U spell > 4 (months)	0.028	0.021	-0.017
	(0.140)	(0.142)	(0.198)
Reason for registration	Yes	Yes	Yes
Past admin. records	Yes	Yes	Yes
Agency fixed effect	Yes	Yes	Yes
Socio-demographic variables $*$ Female	No	Yes	Yes
Qualification variables $*$ Female	No	Yes	Yes
Labor market history variables $*$ Female	No	No	Yes
Past admin. records variables $*$ Female	No	No	Yes
Log likelihood	-964.240	-954.747	-921.818
Pseudo R^2	0.068	0.077	0.109
No. of Obs.	1809	1809	1809

Notes: We estimate the parameters used in the employability index using the subsample of job seekers assigned to the control group.

Figure 1.A.2: Density of individual employability in treatment and control groups

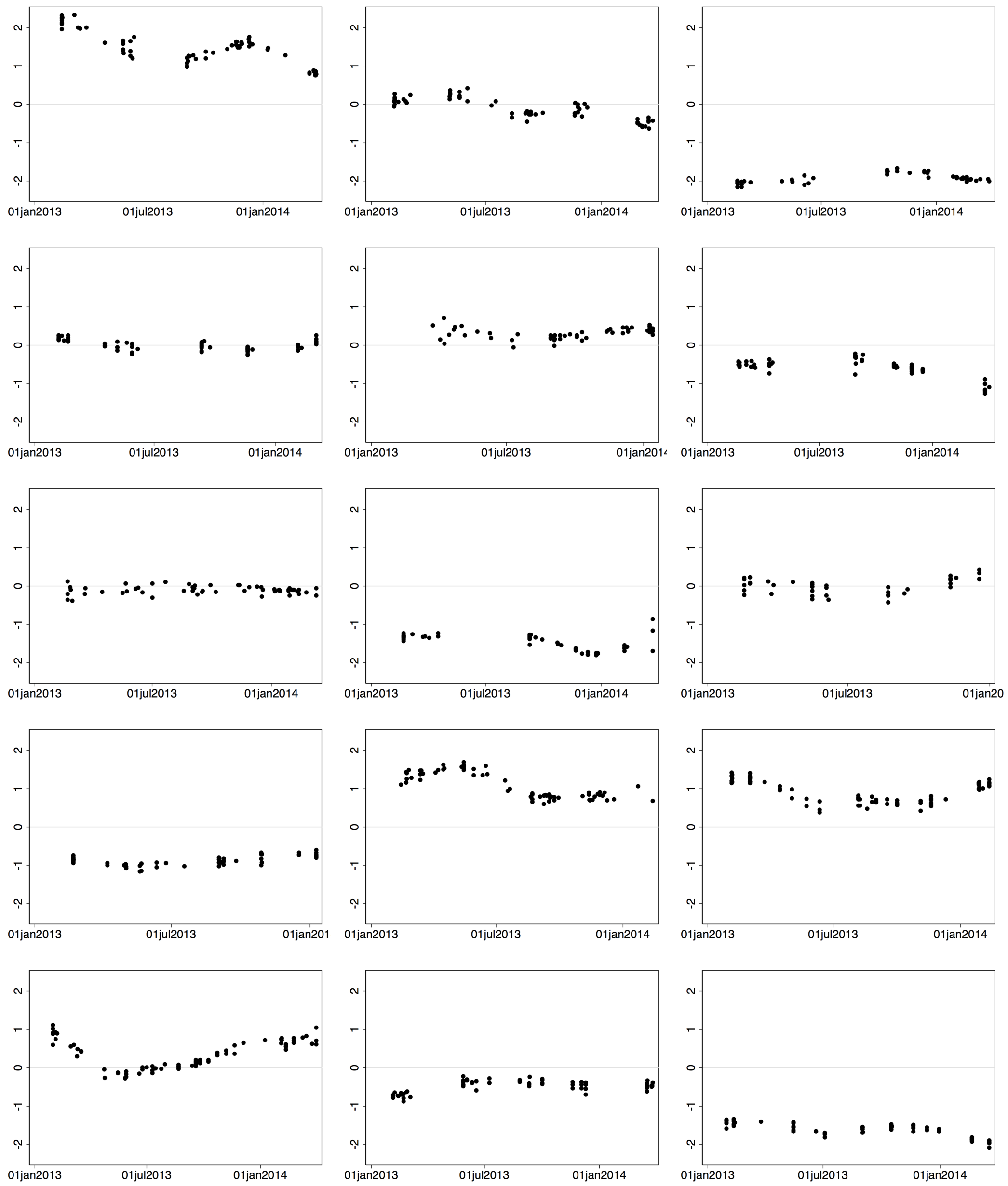


Notes: We present here the density of the employability index before normalization, separately for treatment and control groups.

Figure 1.A.3: Density of mean group employability based on empirical and theoretical groups

Notes: We present here the group mean employability index before normalization, exclusively for the subsample of individuals assigned to a club and comply. We can only observe an actual group of peers for these individuals, which we define as "empirical groups". By contrast, the theoretical definition of groups is based on the randomization date, irrespective of the complying status and the duration of attendance in the program. The correlation between the two measures of group mean employability is 0.967.

Figure 1.A.4: Within agency variation in leave-one-out group mean employability in clubs



Notes: Each graph displays, for one agency, the mean employability of peers that new participants face when they are randomly assigned to the club. Mean employability of peers is presented over randomization dates. In the interest of space, we present within agency variation for half of the agencies.

1.B.2 Main employment effect

Table 1.B.1: Labor market situation at the first and second survey wave

PANEL I: First survey wave (≈ 2 months after randomization)				
Variable	Control mean	Coefficient	SE	N
Activity in past 6 weeks				
Has worked	0.34	0.03**	0.02	2949
Employment status at survey 1				
Is employed	0.26	0.07***	0.02	2941
Works full-time	0.17	0.05***	0.01	2919
Net monthly wage $>$ 590 euros	0.19	0.06***	0.02	2907
Net monthly wage $\geq$ 1120 euros	0.14	0.03**	0.01	2907
Net monthly wage $>$ 1120 euros	0.09	0.00	0.01	2907
Type of contract				
Permanent	0.05	0.01	0.01	2924
Fixed-term	0.12	0.05***	0.01	2924
Temp agency work	0.06	-0.01	0.01	2924
Internship	0.02	0.01	0.01	2924
Self employment	0.00	0.00	0.00	2924
Subsidized contract	0.04	0.03***	0.01	2919
Non Subsidized contract	0.21	0.03*	0.02	2919
Contract duration				
$\geq$ 3 months	0.14	0.05***	0.01	2897
$\geq$ 6 months	0.11	0.04***	0.01	2897
$\geq$ 12 months	0.07	0.03***	0.01	2897
PANEL II: Second survey wave (≈ 6 months after randomization)				
Variable	Control mean	Coefficient	SE	N
Activity in past 4 weeks				
Has worked	0.46	0.04**	0.02	2442
Employment status at survey 1				
Is employed	0.52	0.04*	0.02	2442
Works full-time	0.35	0.02	0.02	2425
Net monthly wage $>$ 590 euros	0.42	0.03	0.02	2408
Net monthly wage $\geq$ 1120 euros	0.29	0.03	0.02	2408
Net monthly wage $>$ 1120 euros	0.19	0.01	0.02	2408
Type of contract				
Permanent	0.10	0.01	0.01	2432
Fixed-term	0.27	0.02	0.02	2432
Temp agency work	0.12	-0.01	0.01	2432
Internship	0.02	0.01	0.01	2432
Self employment	0.00	-0.00	0.00	2432
Subsidized contract	0.12	0.03	0.01	2394
Non Subsidized contract	0.39	0.01**	0.02	2394
Contract duration				
$\geq$ 3 months	0.33	0.03	0.02	2410
$\geq$ 6 months	0.28	0.02	0.02	2410
$\geq$ 12 months	0.18	0.02	0.02	2410

Source: Experiment surveys.
Notes: The models include agency fixed-effect. SE clustered at the time∗agency level are presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.B.2: Robustness - Estimation of employment main effect in different specifications

	Long contract before 6 months					Long contract before 12 months				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
D	0.049***	0.048***	0.049***	0.048***	0.050***	0.045***	0.045***	0.045***	0.044***	0.045*
	(0.013)	(0.013)	(0.013)	(0.013)	(0.019)	(0.016)	(0.017)	(0.016)	(0.017)	(0.024)
Agency FE	No	Yes	No	Yes	No	No	Yes	No	Yes	No
Time FE	No	No	Yes	Yes	No	No	No	Yes	Yes	No
Pair FE	No	No	No	No	Yes	No	No	No	No	Yes
No. of Obs.	3604	3604	3604	3604	3528	3604	3604	3604	3604	3528

Notes: Long-term contracts include fixed-duration contracts with a term longer than 6 months and indefinite duration contracts. Sample size in columns (5) is slightly smaller because individual randomization (outside of a pair) was marginally implemented when an odd number of new participants was selected. SE clustered at the time∗agency level are presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.B.3: Robustness - ITT effect on cumulated employment probability

Long-term job before (months)	6	12
PART I.A: Excluding agencies with gap in compliance rate >10%		
D	0.052***	0.057**
	(0.018)	(0.024)
Constant	0.193**	0.288***
	(0.080)	(0.079)
Time fixed effect	Yes	Yes
Agency fixed effect	Yes	Yes
Covariates	Yes	Yes
No. of Obs.	2071	2071
PART I.B: Excluding agencies with gap in compliance rate >5%		
D	0.056**	0.055*
	(0.023)	(0.030)
Constant	0.156**	0.250***
	(0.078)	(0.087)
Time fixed effect	Yes	Yes
Agency fixed effect	Yes	Yes
Covariates	Yes	Yes
No. of Obs.	1342	1342
PART I.C: All agencies		
D	0.056***	0.059**
	(0.018)	(0.023)
D∗Compliance rate gap	-0.080	-0.147
	(0.127)	(0.147)
Constant	0.329***	0.532***
	(0.067)	(0.083)
Time fixed effect	Yes	Yes
Agency fixed effect	Yes	Yes
Covariates	Yes	Yes
No. of Obs.	3604	3604
PART II.A: Excluding pairs with gap in compliance ≠0		
D	0.062***	0.053***
	(0.016)	(0.020)
Constant	-0.052	0.863
	(0.364)	(0.567)
Time fixed effect	Yes	Yes
Agency fixed effect	Yes	Yes
Covariates	Yes	Yes
No. of Obs.	2600	2600

Notes: Long-term contracts include fixed-duration contracts with a term longer than 6 months and indefinite duration contracts. We include agency fixed-effect corresponding to the agency of assignment and time fixed-effect corresponding to the month of assignment. SE clustered at the time∗agency level are presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.B.4: Heterogeneous effect of club

	Long contract before 6 months					Long contract before 12 months				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
D	0.021	0.023	0.054**	0.054***	0.044***	0.028	0.008	0.035	0.054***	0.044**
	(0.017)	(0.099)	(0.022)	(0.017)	(0.016)	(0.020)	(0.111)	(0.025)	(0.020)	(0.021)
D∗Female	0.058**					0.036				
	(0.029)					(0.033)				
Female	0.025					0.025				
	(0.019)					(0.023)				
D∗age		0.001					0.002			
		(0.004)					(0.005)			
age		-0.003					-0.006**			
		(0.003)					(0.003)			
D∗LowDiploma			-0.012					0.017		
			(0.029)					(0.032)		
LowDiploma			-0.033					-0.075***		
			(0.021)					(0.024)		
D∗NoExperience				-0.016					-0.027	
				(0.029)					(0.031)	
NoExperience				-0.013					-0.023	
				(0.021)					(0.022)	
D∗PastUspell					0.000					0.000
					(0.000)					(0.000)
PastUspell					-0.000					-0.000
					(0.000)					(0.000)
Constant	0.181***	0.265***	0.210***	0.198***	0.196***	0.299***	0.465***	0.350***	0.319***	0.314***
	(0.012)	(0.066)	(0.015)	(0.012)	(0.011)	(0.015)	(0.078)	(0.018)	(0.015)	(0.014)
Agency fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	3604	3604	3604	3604	3604	3604	3604	3604	3604	3604

Notes: Long-term contracts include fixed-duration contracts with a term longer than 6 months and indefinite duration contracts. SE clustered at the time∗agency level are presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

1.C.3 Impact of peers

Table 1.C.1: Robustness - Impact of group composition on employment, with different logit models for the prediction of employability

Outcome:	Long contract before 6 months			Long contract before 12 months		
Specification of logit model:	Spec 1	Spec 2	Spec 3	Spec 1	Spec 2	Spec 3
	(1)	(2)	(3)	(1)	(2)	(3)
D	0.048***	0.047***	0.047***	0.043***	0.042**	0.043***
	(0.013)	(0.013)	(0.013)	(0.017)	(0.017)	(0.016)
D*Emp_i	0.045**	0.040**	0.027	0.039**	0.029	0.009
	(0.018)	(0.018)	(0.018)	(0.020)	(0.019)	(0.018)
Emp_i	0.004	0.004	0.015	0.016	0.017	0.030**
	(0.014)	(0.013)	(0.012)	(0.016)	(0.015)	(0.014)
D*$\text{Emp}_{g(i),-i}$	-0.070***	-0.066***	-0.052***	-0.070***	-0.064***	-0.045**
	(0.017)	(0.017)	(0.014)	(0.021)	(0.020)	(0.018)
$\text{Emp}_{g(i),-i}$	0.034	0.042*	0.051**	0.063*	0.059*	0.055**
	(0.028)	(0.025)	(0.020)	(0.035)	(0.032)	(0.025)
Constant	0.193***	0.194***	0.194***	0.311***	0.312***	0.311***
	(0.009)	(0.009)	(0.009)	(0.011)	(0.011)	(0.011)
Agency fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	3604	3604	3604	3604	3604	3604

Notes: Long-term contracts include fixed-duration contracts with a term longer than 6 months and indefinite duration contracts. Specifications 1, 2 and 3 are described in Table 1.A.2. Emp_i is the employability of individual i, $\text{Emp}_{g(i),-i}$ is the mean employability within the group g of job seeker i. SE clustered at the time$*$agency level are presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.C.2: Robustness - Impact of group composition on employment

Long contract before	6 months					12 months				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
D	0.047***	0.049***	0.047***	0.051***	0.050***	0.041**	0.043***	0.041**	0.045***	0.044***
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.016)	(0.017)	(0.016)	(0.017)	(0.017)
D*Emp_i	0.040**	0.040**	0.041**	0.038**	0.060**	0.029	0.029	0.029	0.024	0.023
	(0.018)	(0.018)	(0.018)	(0.019)	(0.025)	(0.019)	(0.019)	(0.019)	(0.020)	(0.028)
Emp_i	0.006	0.004	0.006	-0.000	-0.009	0.018	0.017	0.019	0.014	0.016
	(0.013)	(0.013)	(0.013)	(0.014)	(0.018)	(0.014)	(0.015)	(0.014)	(0.016)	(0.020)
D*$\text{Emp}_{g(i),-i}$	-0.076***	-0.068***	-0.074***	-0.069***	-0.086***	-0.065***	-0.048*	-0.061**	-0.045**	-0.047*
	(0.021)	(0.022)	(0.021)	(0.019)	(0.022)	(0.024)	(0.025)	(0.024)	(0.023)	(0.026)
$\text{Emp}_{g(i),-i}$	0.063***	0.035	0.062***			0.075***	0.048	0.071***		
	(0.015)	(0.027)	(0.015)			(0.017)	(0.033)	(0.017)		
D*$\text{StdEmp}_{g(i),-i}$	0.014	0.006	0.012	0.013	0.018	0.000	-0.021	-0.003	-0.014	-0.013
	(0.019)	(0.019)	(0.019)	(0.018)	(0.018)	(0.021)	(0.022)	(0.021)	(0.020)	(0.021)
$\text{StdEmp}_{g(i),-i}$	0.001	0.007	0.001			-0.000	0.019	0.003		
	(0.012)	(0.013)	(0.012)			(0.014)	(0.016)	(0.014)		
Agency fixed effect	No	Yes	No	No	No	No	Yes	No	No	No
Time fixed effect	No	No	Yes	No	No	No	No	Yes	No	No
Agency∗time fixed effect	No	No	No	Yes	No	No	No	No	Yes	No
Pair fixed effect	No	No	No	No	Yes	No	No	No	No	Yes
No. of Obs.	3604	3604	3604	3601	3512	3604	3604	3604	3601	3512

Notes: Long-term contracts include fixed-duration contracts with a term longer than 6 months and indefinite duration contracts. Emp_i is the employability of individual i, $\text{Emp}_{g(i),-i}$ is the mean employability within the group g of job seeker i. SE clustered at the time∗agency level are presented in parentheses. $\text{StdEmp}_{g(i),-i}$ is the standard deviation of employability in group g. *** p<0.01, ** p<0.05, * p<0.1.

Chapter 2

Breaking News: The Role of Information in Job Search and Matching

Although the idea of information frictions is behind many policy interventions, little is known about the nature of such frictions and how they affect different categories of job seekers. In this paper, I study how an information shock affects the job search of unemployed workers and their access to employment. I estimate the impact of media news spreading the information that a plant needs to hire many workers on job applications sent to the plant and subsequent hiring. I use data on job search activity from a large public online search platform in France, combined with unemployment register and administrative data on hires. I link 612 news stories with this data at the plant level. My empirical strategy exploits the quasi-random timing of news in the short run. I find that news of a plant expansion lead to a 60% increase in job applications in the following month. Job seekers who apply in reaction to the news tend to be more detached from the labor market in several dimensions, and in particular, they tend to live relatively far away from the growing plant, suggesting that information frictions increase with geographical distance. However, these job seekers also appear to be good matches for the plant's needs, and plants hire a substantial fraction of them. Job seekers as a whole benefit from the additional information by being able to direct their search towards plants that are more likely to hire, though I find some evidence of displacement effects concentrated among local job seekers. My findings suggest that helping disadvantaged job seekers detect which firms have large hiring needs could contribute to correct inequalities in job access and increase geographic mobility.

2.1 Introduction

With the recent advent of online job search platforms, it may be tempting to think that information frictions have disappeared, since job seekers can learn about many thousands of vacancies at virtually no cost.[1] For the fraction of vacancies advertised online, one could expect job seekers to detect all job offers within the scope of their criteria and apply to them. In this paper, I show that, on the contrary, job seekers are not informed about all aspects of posted job offers relevant to their application decisions. Furthermore, I show that information frictions affect particularly job seekers who are more detached from the labor market, and might hence reinforce inequalitiy in employment access.

To highlight this information friction, I estimate the impact of a shock of information about plants' planned hires on search behavior. I show that when job seekers learn that a plant plans to hire a particularly high number of new workers, they become much more likely to apply to its posted job offers. Specifically, I use a commercial data base of media news stories collected for local forecasting purposes[2] to identify media news of intended plant expansions. I link these news to high frequency measures of search activity for job seekers using the French Public Employment Service (PES) online search platform and to administrative data on plants' subsequent hiring activity. The search platform of the French PES is the most popular search platform in France, especially for low-skilled jobs. Since 2014, PES offers the possibility for job seekers to apply directly via its platform to specific job vacancies—which is how a fraction of job applications can be tracked. These combined data allow me to track for each plant the vacancies that it advertises on the platform, the applications that it receives via the platform, and *any hire* it ultimately makes[3].

My identification strategy relies on the assumption that the exact timing of news publication is as good as random. I document that—although plants tend to post more vacancies in the year surrounding publication of the news of an intended expansion—they do not change their recruiting activity in a short window of several weeks around the publication date. At the week-to-week level, the news announcement therefore represents new information for job seekers about the firm, with no change in a firm's actual labor demand. In order to control for any simultaneous shock in the economy, I construct a set of control plants with similar characteristics regarding the number of hires and posted vacancies in the months prior to the news through a dynamic matching procedure. I then estimate the effect of the news in a differences-in-differences design (similar to Azoulay et al. (2010) and Jäger (2016)) that compares outcomes between treated plants and control plants in the weeks around the news coverage.

I find that media coverage of plants' expansion strongly increases the number of applications sent to these plants. The number of applications increases by 75% in the week of the news and by about 60% over the following month. At the same time, the number of posted vacancies remains constant, which means that the number of applications per vacancy increases. Consistent with job seekers reacting because of the information content of the news, the reaction is stronger when the reported hiring needs are larger.

[1] Direct empirical studies on the impact of online search on the effectiveness of job search find contrasting results. Kuhn and Skuterud (2004) find that the use of internet for job search seems to slower the access to employment in 1998–2000 while Kuhn and Mansour (2014) observe a rather opposite effect in 2005–2008; Kroft and Pope (2014) show that the expansion of the search platform Craigslist in the U.S. has not reduced unemployment.

[2] This data base was previously used for research in Galbiati et al. (2015).

[3] I can observe any hire—whether or not there was a vacancy posted on the search platform.

Moreover, my setting allows me to identify the characteristics of job seekers who are affected by the information shock, and therefore shed some lights on individual determinants of information frictions. I find that job seekers who apply in response to an information shock (i.e., the "compliers") tend to live far away from the plant. The average distance between the city of residence of an applicant and the location of the plant increases by 30 km (about 18 miles) in the weeks following the publication of the news. As most job seekers tend to search very locally in baseline, this implies a 40% increase in the typical distance from home to work for applicants, mostly driven by an increase in the number of applications from outside the plant's departement[4]. This is consistent with the idea that job seekers may have an easier access to information on plants around them than on plants located further away.

The third key finding is that these information shocks have a positive impact on hiring at the plant in the short run. Based on observable characteristics of applicants and vacancies, I estimate that information compliers are about as well-matched as those who would apply in the absence of news. I then estimate that this inflow of additional well-matched applicants translates into an increase in hiring at the plant by about 50%. The increase in the number of hired applicants suggests that, in order to meet their hiring target, growing plants need more applicants than they typically attract.

Finally, I show that the information shocks are followed by a change in the composition of newly hired applicants in a way that suggests that some information compliers are hired instead of the usual applicant. Among newly hired applicants, those who applied in the month following the news are based much further away from the plant's location. On the flip side, I detect a large decrease in the hiring rate of job seekers living in the same city as the plant itself. This result points toward some crowding-out for locally-based job seekers. Overall, my findings suggest that providing tailored information to job seekers about the hiring needs of plants—irrespective of their geographical location—could therefore contribute to both improve the matching process and foster labor mobility.

My paper contributes to several important literatures on job search and the efficiency of labor markets. It first relates to the articles that have collected direct measures of the behavior of job seekers in order to test predictions of various search models. Krueger and Mueller (2010) and Krueger and Mueller (2011)) have challenged the standard model of the relationship between unemployment insurance and search intensity (Mortensen (1977)) using direct evidence on job search behavior measured in surveys. However surveys cannot measure search activities over time at a high frequency for a large set of job seekers. Recently, internet data—such as social media indexes (Dolan Antenucci et al. (2014)), activities on online search platforms (Marinescu (2017)), google trends (Baker and Fradkin (forthcoming))—have also been used to further document the impact on UI on job search at the aggregate level. The availability of micro-data for online search have allowed to start empirically exploring more complex aspects of search behaviours related to directed search models (Kudlyak et al. (2014) and Faberman and Kudlyak (2016)). The micro-data collected for this paper push even further the possibilities to learn about the direction of job search because of the relatively large coverage of the French labor market by the public search platform and because of the possibility to combine them with various administrative data.

Second, my paper speaks to the literature on the impact of information interventions on the la-

[4]There are 94 departements in France when excluding Corsica and oversee territories

bor market. A large body of papers has established that the impact of counseling programs on the employment outcomes of beneficiaries is positive although limited[5]. However, it is difficult to disentangle the role of information provision from monitoring, and even more to pin down the impact of a specific type of information. To fill this gap, some recent articles have directly evaluated information policies in experiments. Altmann et al. (forthcoming) emphasize the impact of a brochure containing general information about job search together with motivation-boosting messages on the access to employment. Consistent with my findings, their results indicate that information interventions are particularly effective for job seekers at risk of long-term unemployment. But they cannot conclude whether a specific type of information is lacking because their treatment mixes together several interventions. My results relate more closely to Belot et al. (2017) who find that information about job prospects affect the direction of job search. In their experimental design, the authors expose individuals searching for a job on an online platform to algorithm-based suggestions of job offers in different occupations. The authors find that this intervention makes job seekers search for jobs in a broader set of occupations, especially for those who were initially searching narrowly. As in my paper, they conclude that job seekers would be willing to look for a job beyond the initial scope of their search (occupational in their case and geographic in mine) if they received additional information. My data contain a larger sample of job seekers as well as more precise measures of hires, which allows me to also document the impact on matching.

Third, my paper is related to the literature on inefficiencies in search equilibrium. In particular, my findings are consistent with the information frictions associated with phantom vacancies modelled in Cheron and Decreuse (2017) and Albrecht et al. (2017). In these papers, the authors argue that a substantial amount of vacancies remain posted on search platforms after they have been filled because firms do not internalize the costs of these "phantoms" on other users of the platform. Therefore, job seekers are uncertain about the pay-off from applying to a vacancy and direct their applications to newer vacancies in order to minimize the risk of applying to a phantom. Albrecht et al. (2017) claim that phantoms can account for 85% of frictional unemployment and more than one fourth of overall unemployment. My results provide an empirical validation for their two fundamental assumptions: that job seekers direct their search according to the hiring needs associated with a vacancy, and that they imperfectly observe these hiring needs. Other empirical evidence suggest that the lack of information on the current availability of suppliers or demanders is a problem common to various online markets. For instance, Fradkin (2017) shows that availability tracking contributes very importantly to the efficiency of search on the platform Airbnb.

Finally, my results shed new light on the local dimension of job search. Consistent with recent evidence (Marinescu and Rathelot (2016), Manning and Petrongolo (forthcoming)), I document that workers search for jobs very locally. Generally, this is interpreted as a consequence of mobility costs. In contrast, in a structural model estimated on French employer-employee data, Schmutz and Sidibé (2016) show that information frictions also have a very large impact on labor mobility. Estimating directly the role of information in a setting closer to the one in my paper, Wilson (2018) finds that media news covering employment opportunities associated with the boom in fracking in the U.S. have had a positive impact on migration decisions to fracking areas. My results confirm

[5] See the meta-analysis by Card et al. (forthcoming).

that information frictions are an overlooked but significant determinant of labor mobility.[6]

The rest of my paper proceeds as follows. In Section 2, I describe the institutional background and the data collected for this study. Section 3 turns to my identification strategy. In section 4, I present my estimation of the impact of information shocks on the number of applications sent to growing plants. In Section 5, I investigate the characteristics of job seekers who react to information shocks. Section 6 presents how these changes in job search translate into changes in hires. Section 7 explores potential mechanisms behind the reaction of job seekers while Section 8 offers a conclusion.

2.2 Institutional setting and Data

In this paper, I use data about online applications and posted vacancies on the search website administrated by the French Public Employment Service (PES) which have not been used in research before. It is the most popular search platform in France as it gathers information about one third of all existing vacancies. On the recruitment side, I can therefore observe a large share of overall vacancies, especially for low-skill jobs. Similar sources in other countries have been exploited by Berman (1997) or more recently Mueller et al. (2017). However, to my knowledge, the possibility to connect them with micro-level applications is unique.

2.2.1 Institutional setting

The French PES is in charge of providing both financial support and job search assistance to job seekers. Unemployed workers have to be registered at the PES in order to be eligible for unemployment benefits.[7] For their first registration, job seekers are required to give information about their professional experience, education, family status and about the job they look for. They receive personal assistance that can include meetings with a caseworker, collective job search trainings, etc. In order to remain registered, job seekers have to update their registration once a month.

The PES is also in contact with firms in order to advertize their vacancies and provide recruitment assistance services. Its website is the main source of vacancies in France: it covers approximatively one third of the French labor market. In Table 2.A.1, I compare the number of hires during a given year with the number of vacancies on the website in the same sector and geographic zone, and find that the the ratio ranges from 18% to 41%. Services and Retail sectors are by far the economic sector in which there are the most vacancies posted on the PES website. Vacancies posted on the website are mostly for low-skilled positions but also tend to offer more permanent contracts and relatively better-paid jobs than in the whole economy.

Since 2014, recruiters have the possibility to have their vacancies open to online applications on the website. In this case, job seekers are not given the contact information of the recruiter and have to fill an application form online instead of directly sending their application to the employer. For recruiters, this online standardized application form can be combined with a pre-selection of

[6]These new insights on the determinants of workers' mobility decisions contribute to the literature on the incidence of local labor demand shocks (Manning and Petrongolo (forthcoming), Notowidigdo (2013)), local-based policies (Neumark and Kolko (2010), Busso and Kline (2013), Kline and Moretti (2014), Briant et al. (2015)) and spatial mismatch (Sahin et al. (2014) and Marinescu and Rathelot (2016)).

[7]Unemployment insurance can go up to 24 months for workers under 50 (with a minimum of 4 months) and the replacement rate corresponds roughly to 57%.

applicants by caseworkers according to predefined criteria. In this case, this pre-selection is only the first stage of the recruitment procedure and the firm has no obligation to hire one of the applicants.

2.2.2 Data on search, recruitment and hires

I combine several administrative data sets of the French PES for this study. In this section, I present my data sources and discuss their representativeness for the French labor market.

- Job vacancies: I observe vacancies posted on the French PES search platform. The data set includes many characteristics such as the date on which a vacancy is posted and removed from the platform,[8] a very precise category for the occupation, requirements in terms of skills, education, professional experience and posted wages. A subsample of those vacancies is open for online applications. I observe at least one online application for 12% of all posted vacancies, which represents about 700,000 vacancies in 2014–2016. Descriptive statistics are presented in Table 2.A.2.
- Hires: I use employers' administration declarations which are mandatory in the week before the hire a new worker (*"Déclarations préalables à l'embauche"*). They cover almost all contracts in the private sector. This data source can be linked with unemployment register at the individual level and with vacancies at the plant level since 2012. It contains a very small set of variables, essentially the date of start of the job and the planned duration of the contract.
- Unemployment register matched with online applications: I am the first to exploit data containing online applications made on the PES search platform. I match this data set with unemployment register data in order to collect rich information about online applicants.

For the purpose of this study, I match vacancies, online applications and hires at the plant level. My analysis will focus on plants which have at least one vacancy in the online platform and one hire in 2012–2016. I present descriptive statistics for this sample in Table 2.A.3. Large plants are more likely to recruit through the PES search platform. Plants which posted vacancies on the platform therefore represent 32% of all plants but account for about 65% of employees in the private sector. Similarly, plants which received at least one online application account for 12% of plants and about 40% of employees in the private sector.

2.2.3 Data on news announcement of plants' expansions

In order to detect shocks in the information about plants, I use a data base comprising a large set of media news about plants' expansion and downsizing in France. In this section, I present this data source. I then highlight the accuracy of the information reported in media news using administrative data about hires and vacancies.

The data base on media news about plants' expansion in France is collected for commercial purposes by a private firm[9] in order to help local economic forecasting. It is compiled from about 4,000 Internet sources, in particular local newspapers websites, national newspapers website, and firms' websites.[10] It contains information on the date and sources of the news, number of jobs

[8] The date when one vacancy is removed does not however correspond to the vacancy filling date in all cases. One vacancy can be removed automatically if the employer is not responding to caseworkers from the PES for a given period.

[9] "Observatoire de l'Emploi et l'Investissement."

[10] A web crawler is programmed to detect news about the growth of one plant and variables describing the events

supposed to be created during the expansion, city where the plant is located, name and the sector of the plant, plant identifiers.[11] It is noteworthy that, the data collection process is not designed to include exhaustively all sources that mention each event. When several articles in different media outlets report about the same plant in a short time period, they are pooled together in one observation in the data set. This happens frequently, consistent with the empirical evidence that newspapers tend to reproduce each other's content (Hervé et al. (2017)). The data set therefore includes the first sources detected and up to four different sources for each event. However, the original source might not be the one that the web crawler detects, which means that the date of publication from the data base can be a few days posterior to the public disclosure of the information.

Table 2.1: Descriptive statistics about media news

	Mean	Standard deviation
Nb of jobs to be created	32.4	50.0
Size of plant	159.1	255.2
Nb of jobs relative to size of plant	3.0	14.6
Nb of media sources	1.5	0.6
	Proportion (%)	
National media	39.1	
Events concerning the same plant		
First event	94.6	
Second event	4.4	
Third event	0.8	
Fourth event	0.2	
Size plant		
0-4 employees	12.5	
5-19 employees	15.3	
20-49 employees	20.4	
50-99 employees	14.7	
More 100 employees	37.1	
Sector of plant		
Transport	3.4	
Scientific	9.2	
Manufacturing	46.5	
Information, communication	7.2	
Hostel, restaurants	2.5	
Health	5.1	
Bank, insurance	2.5	
Car retail	13.1	
Clerical	4.3	
Nb observations	612	

Notes: This table presents the characteristics of the media news from my study sample. They were selected from the data base collected by the private firm "Observatoire de l'Emploi et de l'investissement" based on the possibility to link them at the plant level with administrative data bases of the French PES.

The period in which the expansion takes place can be very long, but my data only contains one date. Most of the expansion dates are identical to news' publication dates, which indicates that the expansion is contemporaneous to the media coverage. I do not include news about past or future expansion episodes because they represent a very small share of the data set and because the precision of the timing is very limited. The news I include should therefore be understood as articles about an expansion happening in the plant during a large time period around the day

are filled after additional manual investigation.

[11] I complemented plant identifiers from the original data base when missing with plant identifiers by crawling the website societe.com and am able to identify 80% of plants.

of publication. I will document in the next section the timing of the expansion relative to the date of media coverage. Additionally, I make two restrictions. First, I only include news that are reported in a general audience media because they have a larger diffusion. Second, I only include news corresponding to one clearly identified plant (75% of articles of the period). Thanks to plant identifiers, I am able match this data base with online applications, vacancies and hires. Figure 2.A.1 gives example of typical news including in this sample and descriptive statistics are presented in Table 2.1.

2.2.4 Fake news?

Are news consistent with recruitment and hires observed in other data sources? In order to check the accuracy of the information reported in media news, I compare the number of hires and posted vacancies in plants mentioned in media news with all other plants. Figure 2.A.2 shows that plants mentioned in media news have a much more intense recruiting activity than other plants, even when controlling for plants' size, sector, and location. Panel (1) shows that the average number of vacancies is around 5 in plants with news while it is around 3 for similar plants with no news. In Panel (2), I see that the average number of hires is around 55 in plants with news while it is around 45 for similar plants with no news.

Additionally, I analyze the evolution of posted vacancies and hires within treated plants in Figure 2.A.3. The number of posted vacancies starts increasing from its baseline quarterly level approximatively two quarters before media news, keeps increasing until three quarters after media news, and seems to decrease then. The time window does not allow to observe all of the plants' expansion events as posted vacancies remain at a level higher than normal 4 quarters after media news. However, I infer from the observable pattern that the expansion lasts on average more than 6 quarters, starts before media news and continues long after. The vertical red lines delimit the time window in which I conduct my empirical analysis on the impact of media news. Panel (2) displays the evolution of hiring for a shorter period of time due to data limitation. The pattern looks quite similar to the one in vacancies posting for the period considered. Overall, from this figure, it is clear that this time window is much shorter than the expansion event covered in media news. In the section dedicated to my empirical strategy, I document that recruiting activities are constant in this short interval.

The takeaway from this analysis is that media coverage of an expansion happens for plants which have particularly high hiring activities. The news also happen during quarters in which the hiring activities are particularly high within those plants. Job seekers may therefore rightfully infer from the news that these plants have large hiring needs. However, I show in the identification strategy that media news do not correlate with specific hiring activities within plants in the short run.

2.3 Empirical strategy

The primary objective of my empirical analysis is to estimate how job applications are affected by the arrival of new information coming from media news. I consider such media news as a treatment affecting plants that are covered and study their impact on the applications sent to these plants. The expansion goes along with an increase in recruiting activity which is the most important potential cofounder. However, my identification strategy makes use of the fact that at the week-level, the timing of news is as good as random. I identify the effect of the news using a differences-in-differences (DiD) design in which I compare the applications sent to treated plants with the applications sent to a set of control plants. I select the set of control plants through a matching procedure with the objective that they look similar to treated plants for potential job applicants, but that they are not affected themselves by media news. This approach was previously used by Azoulay et al. (2010) and Jäger (2016). In this section, I discuss my identification strategy, then present my matched sampling procedure, and finally describe my estimation equations.

2.3.1 Identification strategy

My identification strategy relies on the assumption that the timing of news is as good as random in the short run. In the absence of media coverage, outcomes in plants mentioned in media news should have followed the same trend as in other plants in the weeks following media news. In this section, I first explain in more details the implications of this assumption and then present the various tests I implement to establish its credibility.

Identifying assumption

My estimation strategy includes plant and time fixed effects in order to control for potential plant selection, time trend and seasonality as in a standard DiD. Moreover, in my specific setting, even within treated plants, recruiting and hiring activities are atypical during the quarters around media news. I therefore only exploit short-term variation at the week level during a 15 weeks window centred around the news. In this narrow time window, plants' expansions that are covered in the news are absorbed in plant fixed effects. This is why, the hypothesis I need to identify the causal effect of media news is that the timing of news are as good as random in the short run.

This identifying assumption implies in particular that the timing of the news is independent from plants' recruiting activities in the short run. It means that firms should not strategically use media coverage simultaneous to a hiring campaign. This hypothesis is convincing as, although firms can seek media coverage, the delay between a press release and the date of publication of news in large outlets can be long and uncertain. Additionally, the objective of firms' press releases is typically to advertise a brand among potential consumers rather than a recruiting tool. However, to assess the credibility of this assumption, I show that media news do not correlate with any within plant variation in observable recruiting activities in the short run.

Tests of the identifying assumption

Parallel trend during the pre-event period As in Azoulay et al. (2010) and Jäger (2016), I present the pre-event coefficients in order to provide a visual test of the plausibility of the parallel trend assumption. I take one period just before the news as a reference period and systematically present the coefficients associated with both the pre- and pos-event period. Being able to observe the pre-event pattern of the outcome variables offers a convenient visual test of the parallel trend assumption. But this test does not address potential confounders that would happen exactly in the same time as media news.

Recruiting activities around media news In this setting, the main concern is that plants might change their recruiting activities *simultaneously with media news*. In order to address this potential threat for identification, I will, on the one hand, show that my results are not affected by the inclusion of these variable as controls in robustness checks, and, on the other hand, show that observable recruiting activities does not change around media news.[12] These various tests show that observable recruiting activities do not represent a confounding factor for the estimation of the impact of media news on job search. But there could still remain changes in other recruiting activities that are unobservable to me in this context but observable to job seekers. It does not seem likely that plants would exclusively change their recruiting activities outside of the search platform simultaneous to media news. However, if media news are simultaneous to information campaigns implemented by the firm itself outside of the search platform, the conclusions about the role of information on search activities of job seekers remain the same.

In this paragraph, I present the evolution of the number and the characteristics of vacancies posted in treated plants during 15 weeks around media news. Following the presentation that I will use in the rest of the paper, the coefficients are presented in tables and graphically in order to be able to carefully observe the timing of changes. The period $d-2$ is taken as a reference: all the coefficients corresponding to pre-treatment and post-treatment periods represent the level of the outcome variable compared with its level in $d-2$. I conduct this analysis for all media news, and for the subset of media news reporting hiring needs above the median. In the result section, I expose that the effect of media news on applications is the largest for this subset of media news, so if recruiting activity was a confounder, I should detect the largest variation there. Overall, there is no change in observable recruiting activities after media news.

First, I estimate the evolution in the number of posted vacancies. In Table 2.2, I present the evolution in the number of vacancies after the news, relative to $d-2$, looking both at all vacancies posted on the website (columns (1) to (3)) and at the subsample of vacancies open to online applications (columns (4) to (6)).[13] The evolution of the number of vacancies of any type and for any duration of contract appears to be very flat both for control and treated plants.This table shows that, if anything, the number of vacancies posted in the month following media news is below its previous level (however not significantly different).

Moreover, firms could attract more applicants by changing the information contained in job ads. Indeed, the literature suggests that firms might attract applicants by using other margins than the quantity of posted vacancies, such as the posted wage (Kaas and Kircher (2015)). Therefore, I present how characteristics of posted vacancies evolve around media news in Table 2.B.3 and Table 2.B.4. I use different characteristics of job ads: the posted wage and the required professional experience, qualification and education levels. Not all vacancies contain a posted wage (about 75% of them) so the sample size is reduced when I use this outcome variable. All tests reject that the content of job ads is significantly different after media news. Finally, firms may also signal higher hiring needs by indicating a large number of unfilled jobs per vacancy. For most vacancies, this variable takes its default value of one, only few plants signal that they want to hire several workers for the same position. I observe that this variable is not significantly higher in the week of the news, but that there is a significant increase over the following month in the case of media news reporting large expansion plans (in column (1)). It seems unlikely that the increase in the applications sent

[12] Both methods offer very similar tests of my identifying assumption but Pei et al. (2017)) show that the latter test is more powerful in the case that the potential confounder is imprecisely measured.

[13] The coefficients are estimated in the way I estimate the impact of media news on applications in the results sections.

Table 2.2: Evolution of the number of vacancies around media news

Dep variable:	**Nb of posted vacancies**			**Nb of posted vacancies open for online applications**		
	All	**Short-term contract**	**Long-term contract**	**All**	**Short-term contract**	**Long-term contract**
	(1)	(2)	(3)	(4)	(5)	(6)
Pre period	-0.010	-0.017	0.007	-0.013	-0.008	-0.005
	(0.052)	(0.029)	(0.041)	(0.018)	(0.012)	(0.012)
Over 1 week	-0.045	-0.027	-0.018	-0.023	-0.018	-0.005
	(0.064)	(0.032)	(0.047)	(0.026)	(0.017)	(0.014)
Over 1 month	-0.044	-0.038	-0.006	-0.007	-0.017	0.010
	(0.045)	(0.028)	(0.034)	(0.019)	(0.011)	(0.015)
Time to event fixed effets	Yes	Yes	Yes	Yes	Yes	Yes
Plant fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	4113750	4113750	4113750	4113750	4113750	4113750
Baseline	0.307	0.102	0.205	0.072	0.034	0.038
Relative effect: Over 1 week	-14.685	-26.727	-8.720	-32.807	-53.670	-13.909
Relative effect: Over 1 month	-14.403	-37.899	-2.765	-9.335	-49.023	26.615

Notes: SE clustered at the plant level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The data base is a panel at the plant and week-to-news level. The sample corresponds to 15 weeks centered around the date of the news for treated and control plants. The table displays the effect of media coverage on the number of vacancies posted by the mentioned plant based on DiD regressions.

to vacancies open to online applications could be driven by a change in the information contained in other vacancies. However, in order to check that this is not a confounding factor, I will also include this variable as a covariate in some robustness checks and the results are not affected.

In conclusion to these various tests, I cannot detect any sizeable change in the recruiting activities of the plants on the search platform so the identifying assumption seems credible.

2.3.2 Matched sampling procedure

My identification strategy requires to estimate counterfactual outcomes in the post-news periods. I use a matching procedure to identify a relevant control group among the sample of never-treated plants. The objective of the matching procedure is to identify plants which would be similarly affected in case of global shocks in job search, in order to make the parallel trend assumption credible. There are several motivations for using a matching procedure in my setting. Another common approach in the literature is to conduct an event study, in which the control group consists in plants which are treated at a different point in time or never treated. This is a very similar strategy but this method allows to have a group control plants more similar to treated plants. Additionally, as I have 612 events, the matching procedure allows me to gain power by using a much larger group of control.

Media news occur at different points in time therefore I implement a matching procedure separately for each media news event. The relevant timing is therefore the week relative to media news and not calendar dates. In the rest of the article, denote d the dates of the publication of media news and k the number of periods relative to media news. In order to be able to test the parallel trend assumption, I only match based on observations up to 7 weeks before media news. To be valid, the control group should not include plants that could also be treated in the same period. As plants located nearby could be indirectly treated through geographic spillovers, I exclude plants located in a departement where there is a treated plant in the same period. For each event's date d, I therefore match the plants treated in d to plants which are never treated, and which are located in a departement with no information shock in d. I implement an exact matching on categorical variables representing the following characteristics:

- Size of the plant measured the year prior to $k = -7$
- The qualification that the plant is asking the most frequently in job ads in the period 2010–2016.
- Number of hires, number of posted vacancies, number of posted vacancies open for online applications in the plant during the year prior to $k = -7$

From this procedure, I obtain a large set of control plants associated with each treated plant. The number of control plants associated with each treated unit varies, I chose to keep a large set of matched control plants as I am focusing on highly volatile outcomes at the week level.

As I can observe job applications in 2014–2016, I include in the set of treated plants, plants that are mentioned in media news between February 2014 and November 2016 (in order to be able to observe applications from 2 months before until 2 months after the news). I will focus for the rest of this analysis on a set of 614 of such events, corresponding to 588 different treated plants. Through my exact matching procedure, I am able to find controls for 612 events (corresponding to 587 different plants). I form a control group made of observations from 182,968 plants. For plants that are covered in media news at several dates during the study period, I keep all news events in my main specification. I will also show that my estimates are robust to including only the first news event of each plant and to excluding all news from plants with multiple coverage. In the notations in the rest of the article, for simplicity, I will treat one plant around two different media news at different times as different plants. Table 2.B.1 compares treated plants with matched control plants and with all the plants from the sample.

2.3.3 Estimation equations

I create a panel with treated and control plants centered around the date of the actual/potential event. I consider 15 weeks around the news to be a relevant period for the analysis, therefore I keep observations in the weeks relative to each event $k \in [-7; +7]$.

$$Y_{j,k} = \sum_{\substack{k=-7 \\ k \neq -2}}^{+7} \beta_k^{Treated} \mathbb{1}\{period_k\} \cdot Treated_j + \sum_{\substack{k=-7 \\ k \neq -2}}^{+7} \beta_k \mathbb{1}\{period_k\} + \gamma_j + \epsilon_{j,k} \tag{2.1}$$

In this expression, $Y_{j,k}$ presents the number of online applications sent to plant j during calendar week t. $\mathbb{1}\{period_k\}$ is a dummy variable indicating that the period is k weeks before/after the date at which there were news about plant j and γ_j controls for average differences between plants. $\beta_k^{Treated}$ gives the impact of the news k periods after the news. I take $k = -2$ as the reference period, as the week immediately before the newspapers article might already be affected if information starts spreading before the date of publication of the article. The estimated coefficients $\beta_k^{Treated}$ for $k \in [-7; -2]$ allow to evaluate the plausibility of the common trends assumption. I cluster standard errors at the plant level in order to address potential concerns of serial correlation of outcomes across periods raised in Bertrand et al. (2004).

$\beta_0^{Treated}$ gives the immediate effect. I also present in every result table the effect over the month following the news, given by: $\frac{1}{4} \cdot \sum_{k=0}^{+3} \beta_k^{Treated}$. In tables, symmetrically, I present a pre-event estimate corresponding to the average of coefficients in the month prior to the reference period: $\frac{1}{4} \cdot \sum_{k=-6}^{-3} \beta_k^{Treated}$. Since the increase in the weekly number of applications is difficult to interpret in absolute terms, I systematically convert my results in the relative effect of the news, by dividing the estimates of absolute effects by the baseline level of the outcome in treated plants: $\frac{1}{6} \sum_{k=-7}^{-2} y_j^{Treated}$.

My baseline model is linear however non linear models seem better suited for my data since my dependent variables are count variables and contain many zeros. The Poisson regression is the best candidate among nonlinear models, as it does not suffer from the incidental parameter problem, and allows for convenient inclusion of fixed effects. I will therefore also systematically estimate my results in Poisson regressions and present these results as robustness checks. In order to allow for misspecification of the Poisson distribution, I will present coefficients estimated using a quasi-maximum likelihood method (Wooldridge (2010)). In the Poisson model, the observed number of applications in each period is considered as a realization of a Poisson random variable. The log of the mean weekly number of applications that a plant receives is modelled as a function of the covariates. The estimated coefficients from this model can be interpreted as semi-elasticities.

Pooling weekly coefficients In the second part of my empirical analysis, I estimate the impact of media news on the composition of applications in terms or various dimensions. These outcomes are not precisely measured in each period as I can only observe them in periods in which there is at least application. Therefore, in order to gain power, instead of estimating changes in the outcome variable for each week relative to $k-2$, I pool periods at the month level and compare the month just after the news with the month just before. I keep observations in the two months surrounding the event and construct a dummy indicating that an application is sent in the month following the news. I estimate the following DiD model at the application level:

$$X_{i,j,k} = \beta Treated_j \cdot After_k + \alpha Treated_j + \mu After_k + \delta_j + \epsilon_{i,j,k} \tag{2.2}$$

$X_{i,j,k}$ is a characteristic of the job application i to plant j in period k. I will explore several dimensions of heterogeneity, using dummies as well as continuous outcome variables. In some specifications, I also include characteristics of vacancies to which job seekers apply in the covariates.

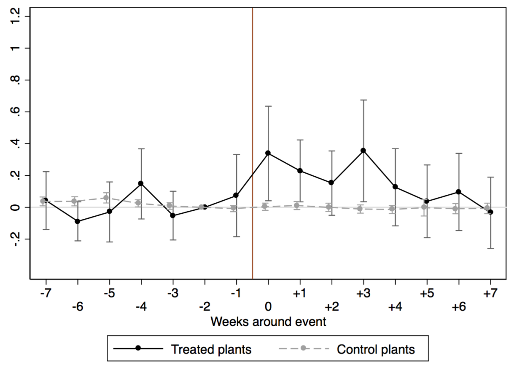
2.4 Impact of information shocks on job search

I analyze how information about hiring needs of specific plants affects job search. I show that growing plants get considerably more applicants than usual after being covered in media news. This effect indicates that more job seekers would choose to direct their applications to growing plants if they were informed about their expansion. Additionally, I find that the job seekers who react the most to this information are the ones living far from the plants. This suggests that job seekers have a worse access to information about hiring needs of plants located further away.

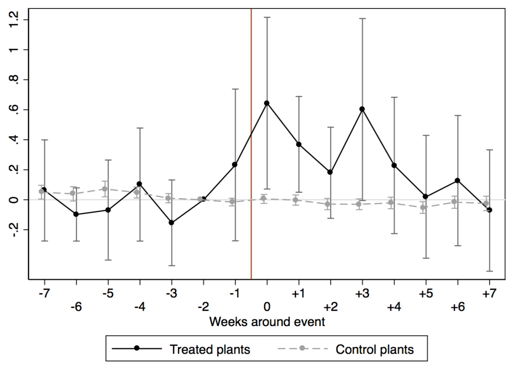
2.4.1 Main result

Figure 2.1: Impact of media news on the number of applications sent to mentioned plants

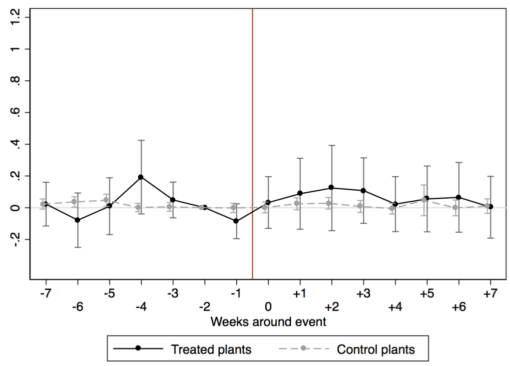
(1) All news



(2) News reporting large hiring needs:



(3) News reporting small hiring needs:



Notes: These figures display time-to-event fixed effects for treated and control plants separately. The model includes plant fixed effects, is fully saturated and period $k = -2$ is normalized to 0. Each coefficient represents the difference between the level of the outcome variable each week around the event and in the reference period $k = -2$ within the treated or the control group. The vertical lines denote 90% confidence intervals based on standard errors clustered at the plant level.

I estimate the increase in applications to growing plants mentioned in media news after the publication of news using equation (1). Results are presented graphically in Figure 2.1 and summarized in Table 2.3. Figure 2.1 presents the evolution of the number of applications sent to plants from the treated and control groups separately in the left panel and the DiD coefficients on the right panel. In the simple before/after comparison for treated plants, we observe a clear and large positive effect of the news. Applications sent to control plants exhibit a flat pattern.

In Table 2.3, the DiD estimate in column (1) indicates that media news cause an increase equivalent to 1 additional applicant over 3 weeks (0.33 additional applicant per week). When compared with the baseline number of applications in treated plants, this effect corresponds to a relative increase of approximatively 75%. Over one month, I estimate an increase of about 60%. The absence of pre-period differences in the outcome variable between the treated and control group shows the plausibility of the parallel trend assumption. As a robustness check, I estimate the same effect in a Poisson count model. The pattern looks very similar: The largest increase happens in the week of the news but the effect persists for one month; the effect is larger for news corresponding to large hiring needs; and coefficients corresponding to the pre-event period are not significant. We cannot directly compare the magnitude of the coefficients as they correspond to different measures:

Table 2.3: Impact of news on applications sent to mentioned firms

Model:	**OLS**			**Poisson**		
Sample:	**All News**	**News about large hiring needs**	**News about small hiring needs**	**All News**	**News about large hiring needs**	**News about small hiring needs**
	(1)	(2)	(3)	(4)	(5)	(6)
Pre period	0.001	-0.038	0.037	-0.013	-0.083	0.107
	(0.087)	(0.154)	(0.079)	(0.187)	(0.235)	(0.352)
Over 1 week	0.336*	0.646*	0.027	0.542***	0.706***	0.062
	(0.180)	(0.345)	(0.099)	(0.193)	(0.204)	(0.371)
Over 1 month	0.264**	0.457**	0.076	0.427***	0.562***	0.182
	(0.114)	(0.203)	(0.101)	(0.148)	(0.148)	(0.357)
Time to event FE	Yes	Yes	Yes	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes	Yes	Yes	Yes
Nb observations	4113750	1527075	2586675	4113750	1527075	2586675
Nb events	612	306	306	612	306	306
Baseline level of outcome	0.453	0.632	0.272	0.453	0.632	0.272
Relative effect: Over 1 week	0.742*	1.022*	0.098	0.719***	1.025***	0.064
Relative effect: Over 1 month	0.584**	0.723**	0.280	0.544***	0.774***	0.204

Notes: SE clustered at the plant level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The data base is a panel at the plant and week-to-news level. The sample corresponds to 15 weeks centered around the date of the news for treated and control plants. The table displays the effect of media coverage on the number of applications sent to the mentioned plant based on DiD regressions. The pre-event presents the average of coefficients corresponding to the months prior to $k-2$ and should not be significant if the parallel trend assumption is verified. I present separately the immediate effect the week of the news and the effect in the following month (from k to $k+3$). In columns (1) to (3), I present the coefficients estimated in a linear model. In the upper part, I present the standard coefficients which represent an absolute increase in the outcome. The bottom part of the table presents the ratio of the absolute effects to the baseline level of the outcome, corresponding to the relative effect. In columns (4) to (6), I present the coefficients estimated in the Poisson model. In the upper part, I present the standard coefficients assimilable to semi-elasticities. In the bottom part, I present the transformation of coefficients $exp(\beta)-1$ in order to interpret the point estimates as the relative effect on the outcome. SEs are computed using the delta method and the confidence intervals correspond to the transformed endpoints of the confidence intervals in the natural parameter space.

they represent an absolute increase in the linear model and semi-elasticities in the standard Poisson count model with a log link-function and I hence also present the corresponding relative increase in the lower part of the table[14]. The magnitude of the relative effect estimated in the two models is the same. Additionally, the graphical presentation of the coefficients estimated in a Poisson model in Figure 2.C.1 exhibit a pattern identical to the one estimated in the OLS model. The conclusions I derived from the linear model are therefore confirmed in the Poisson count model.

2.4.2 Treatment intensity

In panels (2) and (3) (Figure 2.1), I decompose the average effect into the effect of news reporting large hiring needs and small hiring needs. Some media news report a plant expansion associated with a very low number of new jobs, and one would expect a smaller effect for these events. I split the set of events into two equal groups based on the level of hiring needs reported in media news. As expected, I observe the same temporal pattern for large news as for all news together, but

[14] In the linear model, I compute the relative increase by taking the ratio of the absolute increase to the baseline of the outcome; in the Poisson model, it is obtained by calculating $exp(\beta)-1$.

the magnitude of the effect is larger. News reporting large hiring needs cause an increase of 0.65 applications in the week of the news, and a persistent increase of approximatively 0.45 applications per week over the month. This represents a very large relative increase by 100% in the week of the news and by 70% over the month. In contrast, news reporting small hiring needs are followed by a small and insignificant increase in the number of applications to mentioned plants. Additionally, I show that the pattern is very similar if I choose other measures for the size of the expansion reported in news. As alternatives, I use the number of new jobs over the size of the plant or over the average yearly number of hires in the departement (Figure 2.C.1 in Appendix). Overall, these results suggest that the information content of the news is what causes the observed reaction in job search.

Additional measures of treatment intensity In order to look into the mechanisms of the effect, I examine how its magnitude depends on the characteristics of the information shock. My data do not contain a very large set of media news, so the objective of this analysis is not to quantify precisely the magnitude of differences but rather to check that I obtain differences qualitatively consistent with my interpretations. These different results confirm the interpretation that the increase in applications is caused by an increase in the level of information about hiring needs among job seekers. Table 2.F.1 presents the relative effect estimated in a Poisson model for various subsample of events. In columns (1) to (4), I emphasize the role of information by using several proxy measures for how widely news are spread.[15] Columns (1) and (2) compare the effect of news when at least one media source is a national newspapers with all other news. I observe that the estimated coefficient is about twice larger for national news. In columns (3) and (4), I compare news for which my data set contains several media sources to news for which I observe only one media source. The effect is driven by news for which I observe several sources. For those news, he relative increase in applications in the following month corresponds to 80%. Overall those two comparisons confirm the interpretation that media news affects applications because they correspond to an inflow of information.

2.4.3 Robustness check

Finally, I show that the estimation of the impact of media news on the number of applications is robust to alternative specifications and sample restrictions. In Table 2.C.2, I estimate the effect of media news in various models controlling for recruiting activities. I include different combinations of covariates: the number of posted vacancies overall or open to online applications only, the number of jobs needed per vacancy—both for all vacancies or among those open to online applications only. The coefficients for the effect of media news over one week and over one month are of identical magnitude and significant in all models. As the pattern of the outcome in control plants is very flat, results from the DiD estimation do not rely heavily on the selection of control plants. In order to illustrate this, I compare results from a before/after estimation with result from DiD in Table 2.C.3. I compare the estimation of the immediate effect of information and the effect over one month in the case of all news and then large and small news separately. I observe that using only observations from the treated plants in a before/after design yields very similar results in all cases. This suggests that, in the very short run, there is no variation in the average search

[15]These measures are proxies as my data base does not report exhaustively all media sources.

nationwide around media news, and that controlling for time fixed effect is not crucial in my setting. Consequently, the choice of the matching procedure cannot importantly affect my results. Last, I compare the estimates I obtain when I restrict the sample to various subsets of media news events in Table 2.C.4. In my main specification, I am using all events, even when my data set contains several news corresponding to the same plant at several points in time. I compare my baseline results with the estimates I get when I keep only the first event for each plant in columns (2) and (6). The estimates of the effect of news exhibit a comparable pattern but are of a slightly smaller magnitude. This is easily explained as plants that are reported several time in the news must experience a particularly large expansion. The results remain qualitatively identical. The same conclusions are reached when I keep only plants which appear once in media news in my study period. Finally, I present in column (4) and (8) the effect estimated without including plants located in Paris in the sample. There are very few of them and results are not affected.

2.5 Impact of information shocks on the composition of applicants

After showing that media news attract a substantial number of new applicants to growing plants, I explore the characteristics of these additional applicants. Using the randomized trial terminology, this group of job seekers are labelled the "compliers", while job seekers who would apply to the plant even in the absence of media news are labelled "always-takers". I find that information compliers tend to live further away from growing plants than regular ones. This individual heterogeneity in the response to media news is consistent with the idea that job seekers who react the most are the ones who are the least informed in normal times. In order to assess the value of the additional information from media news for matching efficiency, I finally investigate the selection of compliers and the potential match quality of applicants. I predict match quality based on observable applicants' characteristics and find no evidence for a decline in the quality of applicants. On the contrary, compliers appear to be slightly better selected than always-takers.

In order to estimate directly changes in the composition of applicants, I turn to a DiD at the application level and pool week coefficients into two periods corresponding to the pre- and post-treatment (specification presented in equation (2)). I also estimate the heterogeneity in the reaction to news in the first specification as a robustness check.

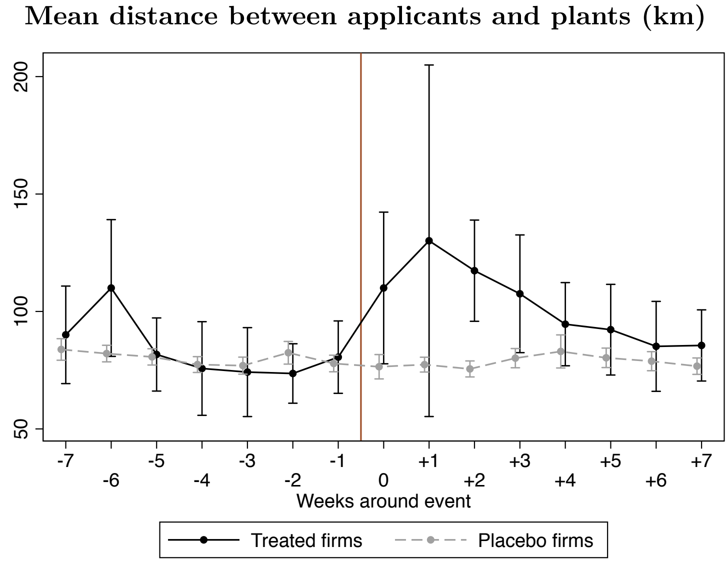
2.5.1 Geographic composition

I first present graphical evidence of the impact of media news on the average applicant-plant distance in Table 2.2. This figures show that the average distance starts increasing in the week of the news and remains at a high level for 4 weeks. The timing mirrors the timing of the increase in the number of applicants very closely.

Table 2.4 presents the effect of media news on the geographic composition of applications in a DiD model at the application-level. Columns (1) and (2) present changes in the average distance between applicants' city of residence and plants' location, using the control group to control for potential trends.[16] The first specification in column (1) suggests a very large increase in mean

[16] The applicant-plant distance is missing for approximatively 10 % of my sample, which slightly reduces my sample size.

Figure 2.2: Impact on news on the geographic composition of applicants



Notes: This figure represent the evolution of the geographic composition of applicants around media news in treated and control plants. Each coefficients corresponds to the average outcome k weeks before/after the week of the news. The vertical line denotes the period of media news. The outcome variable is the distance between applicants and plants.

distance by 30 km in absolute value (about 18 miles), which corresponds to a relative increase of almost 40%. In column (2) I include controls for the characteristics of job ads, in order to ensure that the change in the composition of applicants is not driven by a change in the composition of job ads job seekers apply to. The additional controls do not significantly affect the result but slightly increase the point estimate. In columns (3) to (8), I present similar results using a different measure of geographic distance. I use dummy variables indicating that applicants do not live in the geographic unit where the plant is located. I first use geographic units based on administrative borders (departements and city) which might represent more homogenous areas for information diffusion, as they correspond to well-identified units for job seekers. In particular, these geographic units are suggested as search criteria on PES search platform. Second I use commuting zones which are built to represent local labor markets. The proportion of applicants from a different geographic unit increases in all cases: The proportion of applicants from another city increases by 0.5 ppt, form another CZ, by 4 ppt and from a different departement, by 9 ppt. In particular, the increase in applications from another departement is particularly large and precisely estimated. Although the increase in the share of applicants from a different city is not significant, I show in the next section (Table 2.F.3) that the magnitude suggests that *almost all compliers* live in a different city.[17] Overall, these results points towards a very substantial increase in distance, driven especially by applications over large distances.

These results provide an interesting new explanation for the local dimension of job search. They suggest that job seekers might be less likely to apply to plants located far away because they lack information about their hiring needs. Recent papers have estimated how locally job seekers search

[17] It should be noted that, in a linear model, I detect more precisely the increase in the proportion of applicants living in a different geographic unit if the unit is broader—such as applicants from a different departement—because their baseline proportion is smaller.

Table 2.4: Impact of news on the composition of applicants

Dep variable:	Applicant-plant Distance		Applicants lives in a different (dummy):					
			City		Commuting zone		Departement	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
After∗Treated	29.271***	32.749***	0.005	0.008	0.036	0.046*	0.086**	0.098***
	(11.348)	(11.126)	(0.014)	(0.015)	(0.027)	(0.028)	(0.035)	(0.034)
After	0.350	0.615	-0.005	-0.005	-0.003	-0.003	-0.009*	-0.009*
	(1.725)	(1.727)	(0.003)	(0.003)	(0.005)	(0.005)	(0.005)	(0.005)
Treated	9.308	0.668	0.039	0.028	0.112*	0.085	0.060	0.034
	(7.371)	(6.341)	(0.028)	(0.025)	(0.057)	(0.052)	(0.043)	(0.037)
Job offer X	No	Yes	No	Yes	No	Yes	No	Yes
Event FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	274773	274773	306915	306915	306915	306915	306915	306915
Baseline level	76.435	76.435	0.885	0.885	0.583	0.583	0.377	0.377
Relative effect	0.383***	0.428***	0.005	0.009	0.061	0.080*	0.228**	0.261***

Notes: SE clustered at the plant level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. This table gives the coefficient estimated in a DiD at the application-level. The sample corresponds to applications sent to treated and control plants in the 15 weeks around the news. Each observation corresponds to one application and is characterized by the period in which it was sent (before/after the news) and the plant to which it was sent (treated/control plant). The model includes event fixed effect. The outcome variable is a variable describing the characteristics of applicants. When it is a dummy variable, the coefficients corresponding to AfterNews∗Treated can be interpreted as the effect of the news on the probability that an applicant has this characteristic; it when is a continuous variable, the coefficients can be interpreted as the effect of the news on the level of this characteristic among applicant. These coefficient therefore correspond to the estimated impact of news on the composition of applicants to the mentioned plant.

for a job when controlling for the allocation of job seekers and vacancies (Manning and Petrongolo (forthcoming), Marinescu and Rathelot (2016)). There are several potential explanations for the decision of job seekers to focus their search on nearby vacancies but Marinescu and Rathelot (2016) and Manning and Petrongolo (forthcoming) emphasize a distaste for distance which encompasses the costs (in time or money) for a job seeker to commute. In contrast, Schmutz and Sidibé (2016) argue that low labor mobility also result from job seekers being less informed about vacancies posted in distant cities.[18] In order to isolate the impact of information frictions from the impact of mobility costs, the authors estimate a structural search model in which wages after mobility allow to estimate mobility costs while the frequency of job transitions is determined by information frictions. My setting allows to disentangle the role of information from other factors because preferences or the composition of job seekers should not change in the very short time window in which I conduct my analysis. The only change between the period just before and just after the news should come from the additional information. Therefore I bring additional evidence that the local dimension of job search is also determined by information frictions in a reduced form analysis.

Robustness checks I finally estimate the impact of media news on the number of applications sent by a specific group, using the same plant-level DiD specification that I used for the main results (i.e., the specification presented in equation (1)). As the relevant coefficient for a comparison

[18] Other articles have also suggested that costs of getting information about plants located farther away could explain the local dimension of job search. See for instance Patacchini and Zenou (2005), Guglielminetti et al. (2017).

across subgroups is the relative increase, I directly present coefficients estimated in a Poisson count model in Table 2.C.5. The upper part of the table compares the relative increase in applications according to the distance between applicants' residence and the location of the plant. I approximate the distance by comparing applicants living inside or outside of the geographic unit of the plant, and systematically find that the relative increase in applications from job seekers outside of the geographic unit is larger. Applications increase by 60% over one month for job seekers from another city. Consistent with previous evidence, the estimate corresponding to the impact of media news on the number of job seekers from the same city is small and insignificant and standard errors are very large. We observe very similar pattern at the commuting zone level. At the departement level, the difference is even more striking: the number of applications from another departement *doubles* while the relative increase in the number of applications from the same departement amounts only to approximatively 30%, and is imprecisely estimated. The lower part of the table compares the reaction of job seekers with different education levels: I observe that the size of the reaction decreases with the education level. The size of the reaction also increases with the unemployment spell and in particular very long job seekers (job seekers who have been registered as unemployed for more than 2 years) have a larger relative increases corresponding to more than 100%.

2.5.2 Composition of applicants in other dimensions

I also estimate the impact of media news on the composition of applicants in terms of other dimensions in Table 2.F.2. This table indicates changes in other characteristics that can be associated with a low access to information, while other characteristics remain unchanged. I detect an increase in the share of applicants who have a low level of skill or education, are younger, or have been unemployed for longer. In contrast, I observe no change in the proportion of women, blue-collar workers, workers with little experience in the occupation they are searching for, or workers searching for a job with a relatively high wage. In particular, columns (7) and (8) indicate that the proportion of low educated job seekers increases by 8 ppt. This increase amounts to 3 ppt for the proportion of long-term unemployed (columns (15) and (16)). Overall, this table is consistent with the idea that job seekers with fewer ties to the labor market are more affected by the inflow of additional information. In particular, the relatively strong reaction of long-term unemployed provides an interesting insight concerning the determinants of the well-documented decrease in job search and the job finding rate along the unemployment spell (Krueger and Mueller (2010), Krueger and Mueller (2011), Faberman and Kudlyak (2016)). One determinant of this negative duration dependence could be a decreasing access to information over the unemployment spell.

2.5.3 Characteristics of information compliers

Another way to analyze the heterogeneity of the reaction of job seekers is to describe the characteristics of job seekers who reacted to media news. Media news can be considered as an instrument that affects the probability to apply to the mentioned firms. They may not affect everyone with the same intensity. Under the monotonicity assumption, I recover the characteristics of individuals who apply in reaction to the news following Abadie (2003). Table 2.F.3 presents the composition of compliers in terms of a large set of individual characteristics, one by one. I see that the proportion of job seekers with a low education level is much more important among compliers than among normal applicants (i.e., always-takers): 54% versus 31%. Long-term unemployed represent 30% of compliers but only 16% of always-takers. The distance between job seeker's residence and plant's location is almost 3 times larger for compliers (216 km on average for compliers versus 76 km for always-takers). This pattern is confirmed when I compare the proportion of job seekers from various geographic units. The proportion of job seekers coming from a different departement is twice as large among compliers. I estimate that the proportion of compliers coming from a different city amounts to 99.2%—this suggests that virtually all compliers live in a different city.

2.5.4 Impact of information shocks on the match quality of applicants

If the probability to react to media news is positively correlated with the probability to be a good match for the vacant job, information shocks might not only affect the number of applicants but also increase the average quality of the applicant pool. However, if having information at no cost about one firm induces workers who would be a bad match to apply, news might cause an inflow of low quality applications (Decreuse (2008) and Seabright and Sen (2015)). From a firm's perspective, bad applications induce a processing cost without increasing the expected productivity of hired workers. In order to evaluate the quality of additional applications, I examine how the composition of applicants in terms of predicted match quality changes after the news.

My data offer a simple way to assess the determinants of application's success. For vacancies open to online applications, I can detect who is hired among the pool of applicants. I can therefore study the importance of applicants' characteristics and their interactions with vacancies' characteristics. For each plant from the control group, I collect all online applications sent between 2014 and 2016. I estimate the probability to be hired in a logit model. I include a large set of covariates : adequacy of the applicant to the criteria of the job offer, education and skill level of the applicant, labor market history and socio-demographic characteristics. Table 2.D.1 presents results from the logit estimation. I observe that the most important predictors of a match are interaction terms between applicant characteristics and vacancy requirements: As expected, the probability to be hired is higher for applicants who meet or overpass requirements from job offers in terms of education, skill and professional experience. Moreover, candidates who are looking for a job corresponding to the occupation of the vacancy are more likely to be hired.[19] Conditional on these variables, management skills or a higher education is associated with a negative impact on the probability of a match. In contrast, professional experience has a positive impact even in a different occupation than the position applied for. Finally, being over 45 has a negative impact on chances of a match. More details are provided in Appendix. I use the coefficients from the logit model to predict the probability of success of each application. The predicted probability provides an empirical measure of applicants' match quality. I distinguish 4 different levels of match quality based on the distribution of predicted success. Figure 2.D.1 presents the distribution of this index.

Using this index, I investigate how media news affect applications' match quality in Table 2.5. In column (1), I present the impact of news on the average match quality. The point estimate is positive, but corresponds to a very small magnitude when compared with the baseline level of match quality (it corresponds to a 2% increase) and is insignificant. Columns (2) to (5) present how the proportion of applications from different quality levels is affected. I observe that the proportion of applications from the lowest quartile decreases significantly after the news (by 15%). Applications from other quality levels slightly increase. The table suggests that match quality of applications is not significantly affected by news. If anything, plants receive a lower share of

[19] Job seekers declare which occupation they search for to PES when they register as unemployed.

very bad applications. It should be noted that this result is not in contradiction with changes in composition in terms of education and unemployment duration highlighted earlier. Indeed, as the vacancies posted on the search platform correspond to low-skill jobs, one can be a good match (and meet the skill and education requirement in particular) with a low level of education. Besides, unemployment duration has only a small negative impact on the probability of a match. And other changes in composition such as younger age contributes positively to match quality.

Table 2.5: Impact of news on the predicted match quality of applicants

Dep variable:	**Predicted match quality**	**Application's predicted quality is (dummy)**			
		Very low	**Low**	**High**	**Very high**
	(1)	(2)	(3)	(4)	(5)
AfterNews∗Treated	0.370	-0.041**	0.023	0.008	0.010
	(0.349)	(0.020)	(0.021)	(0.018)	(0.020)
AfterNews	0.044	-0.002	-0.002	0.002	0.002
	(0.059)	(0.003)	(0.002)	(0.002)	(0.004)
TreatedPlant	-0.241	0.007	-0.009	0.010	-0.008
	(0.307)	(0.023)	(0.016)	(0.017)	(0.016)
Event FE	Yes	Yes	Yes	Yes	Yes
No. of Obs.	306915	306915	306915	306915	306915
Baseline level	17.772	0.254	0.226	0.259	0.261
Relative effect	2.079	-15.989**	10.023	3.153	3.718

Notes: SE clustered at the plant level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. This table gives the coefficient estimated in a DiD at the application-level. The sample corresponds to applications sent to treated and control plants in the 15 weeks around the news. Each observation corresponds to one application and is characterized by the period in which it was sent (before/after the news) and the plant to which it was sent (treated/control plant). The model includes event fixed effect. The outcome variable is a variable describing the characteristics of applicants. When it is a dummy variable, the coefficients corresponding to AfterNews∗Treated can be interpreted as the effect of the news on the probability that an applicant has this characteristic; it when is a continuous variable, the coefficients can be interpreted as the effect of the news on the level of this characteristic among applicant. These coefficient therefore correspond to the estimated impact of news on the composition of applicants to the mentioned plant.

The fit between applicants' and vacancies' characteristics offers another intuitive measure of match quality and I observed that it is a strong predictor of the probability of a match in Table 2.D.1. I therefore also investigate changes in match quality by estimating directly how well applicants meet the requirements of the vacancies they apply to before and after the news. In Table 2.F.4, I observe that there is no significant change in the proportion of applicants that meet or overpass skill requirements (columns (1) and (2)), education requirements (columns (3) and (4)), or professional experience requirements (column (8)). Additionally, the proportion of applicants who were looking for a job precisely in the same occupation as the vacancy they apply to does not change significantly either. These results confirm the previous finding that the increase in the number of applicants following media news does not go along with a significant change in application quality. Overall, my results show that plants mentioned in media news do not only receive more applications but also applications of a relatively good quality.

2.6 Consequences on the matching process

I find that the number of hired applicants increases and that the composition of hired workers changes. I finally discuss the consequences of more transparency on hiring needs for jobseekers and firms. The additional information contributes to a better allocation of applications across plants for job seekers. However, it has distributional effects on the access to employment. For firms, I argue that news are beneficial because they allow them to make a larger number of hires and potentially to also hire workers with a better match quality.

2.6.1 Hires in growing plants

Table 2.6: Impact of news on the number of successful applications

Dependent variable:	**Number of successful applications**					
Model:	**OLS**			**Poisson**		
Sample:	**All News**	**News about large hiring needs**	**News about small hiring needs**	**All News**	**News about large hiring needs**	**News about small hiring needs**
	(1)	(2)	(3)	(4)	(5)	(6)
Pre period	-0.000	-0.002	0.002	0.286	0.273	0.422
	(0.006)	(0.008)	(0.008)	(0.229)	(0.189)	(0.822)
Over 1 week	0.025*	0.054**	-0.005	0.808**	0.999***	-0.915
	(0.014)	(0.027)	(0.007)	(0.374)	(0.368)	(1.296)
Over 1 month	0.010*	0.022*	-0.001	0.236	0.408**	-0.462
	(0.006)	(0.011)	(0.002)	(0.185)	(0.192)	(0.682)
Nb of observations	4113750	1529085	2584665	4113750	1529085	2584665
Nb of events	612	306	306	612	306	306
Baseline level of outcome	0.022	0.032	0.011	0.022	0.032	0.011
Rel. effect: Over 1 week	1.125*	1.677**	-0.428	1.243**	1.717***	-0.599
Rel. effect: Over 1 month	0.472*	0.671*	-0.078	0.266	0.505**	-0.370

Notes: SE clustered at the plant level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The data base is a panel at the plant and week-to-news level. The sample corresponds to 15 weeks centered around the date of the news for treated and control plants. The table displays the effect of media coverage on the number of applications sent to the mentioned plant based on DiD regressions. The pre-event presents the average of coefficients corresponding to the months prior to $k-2$ and should not be significant if the parallel trend assumption is verified. I present separately the immediate effect the week of the news and the effect in the following month (from k to $k+3$).
In columns (1) to (3), I present the coefficients estimated in a linear model. In the upper part, I present the standard coefficients which represent an absolute increase in the outcome. The bottom part of the table presents the ratio of the absolute effects to the baseline level of the outcome, corresponding to the relative effect. In columns (4) to (6), I present the coefficients estimated in the Poisson model. In the upper part, I present the standard coefficients assimilable to semi-elasticities. In the bottom part, I present the transformation of coefficients $exp(\beta)-1$ in order to interpret the point estimates as the relative effect on the outcome. SEs are computed using the delta method and the confidence intervals correspond to the transformed endpoints of the confidence intervals in the natural parameter space.

The inflow of additional applicants of good quality after media news should result in some matches with the mentioned plants. First, I estimate that media news are associated with an increase in the number of hires. Second, I show that the composition of hired applicants changes importantly in several dimensions.

In order to explore how media news affect hiring in mentioned firms, I first estimate the effect of news on the number of *successful* applications. I consider that an application is successful if the applicant is hired in the plant in the 6 months following her application. In Table 2.6, I see that the number of successful applications increases, especially applications sent the week of the news itself. The estimated increase in the number of successful applications sent during the month following the news is lower, which suggests that there are fewer hires among applicants who react later to the news. Consistent with previous findings, the increase in successful applications is larger following media news that report large hiring needs: weekly hires increase by 0.05 applicants, corresponding to a number 2.7 times larger than usual (increase by 170%). The average effect over 1 month corresponds to an increase of about 47%. We observe no change in hiring for news corresponding to smaller hiring needs. An important takeaway from is table is that the impact of media news on hiring is positive, however the relative increase in successful applications is smaller than the relative increase in the number of applications (around 60%). As a result, the rate of applications' success should decline after media news. I will discuss this in the next subsection.

Another interesting question is the impact of media news on the number of all hired workers, not just online applicants. If online application constitutes a search channel representative of overall search, the relative increase in hired applicants should be comparable to the relative increase in all hired workers. However, as I do not observe the date of the application for applicants using other channels, it is more difficult to detect hires that result from the inflow of applicants reacting to media news. I discuss this limitation in more details in Appendix 2.E. and only briefly comment the results here. I obtain significantly positive estimates of the impact of news on overall hires, which suggests that there is a relative increase in hiring of about 35% over 3 months. Consistent with the estimate of the increase in hired online applicants, this additional result suggests that the increase in hiring is large but smaller than the relative increase in applications. This confirms qualitatively the findings from the previous analysis.

2.6.2 The composition of hired applicants

Additionally, I find sizeable changes in the composition of hired applicants around media news. In order to estimate the impact of news on the composition of hired applicants, I estimate a similar DiD at the application level on the restricted sample of applicants who get hired in treated or control plants during the 6 months following their application. It should be noted that the sample is therefore considerably smaller than when analysing the composition of applicants, and I have less power to detect changes.

In Table 2.7, I present changes in the geographic composition of hired applicants. In column (1), I observe that the applicant-plant distance is 50 km higher for hired workers who applied after media news. The coefficient is significant at the 5% level. In columns (2) to (4), I present the impact of news on the proportion of hired applicants coming from a different geographic unit. The coefficients are all large and positive but the estimation is imprecise. The effect is particularly large for job seekers who live in another departement. The increase amounts to 12 ppt which corresponds to an increase of 90%. Overall, I conclude from this table that there are sizeable changes in the geographic composition of hired applicants which mirror the changes in the composition of applicants.

Finally, Table 2.F.5 reports estimates of the change in composition of hired applicants in other dimensions. In contrast with the changes detected in the composition of applicants, there is no increase in the prevalence of workers with low skills or a low education level in the pool of hired

Table 2.7: Impact of news on the composition of hired applicants

Dependent variable:	Applicant-plant distance	Applicant does not live in plant's: Departement	CZ	City
	(1)	(2)	(3)	(4)
AfterNews*TreatedPlant	50.286**	0.126	0.083	0.118
	(20.841)	(0.078)	(0.079)	(0.073)
AfterNews	5.949*	0.013	0.008	0.004
	(3.382)	(0.012)	(0.016)	(0.011)
TreatedPlant	-6.220	-0.019	-0.050	-0.027
	(13.865)	(0.048)	(0.068)	(0.073)
Event FE	Yes	Yes	Yes	Yes
No. of Obs.	6592	7306	7306	7306
Baseline level	36.125	0.141	0.281	0.750
Relative effect	139.201**	89.716	29.443	15.762

Notes: SE clustered at the plant level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. This table gives the coefficient estimated in a DiD at the application-level. The sample corresponds to **successful applications**—applications of workers who will get hired in the plant—sent to treated and control plants in the 15 weeks around the news. Each observation corresponds to one application and is characterized by the period in which it was sent (before/after the news) and the plant to which it was sent (treated/control plant). The model includes event fixed effect. The outcome variable is a variable describing the characteristics of applicants. When it is a dummy variable, the coefficients corresponding to AfterNews*Treated can be interpreted as the effect of the news on the probability that an applicant has this characteristic; it when is a continuous variable, the coefficients can be interpreted as the effect of the news on the level of this characteristic among applicant. These coefficient therefore correspond to the estimated impact of news on the composition of applicants to the mentioned plant.

applicants. However, the proportion of young hired workers (below age 25) increases significantly by 18 ppt. The proportion of long-term unemployed also increases but coefficients are imprecise. This table confirms that news are followed by both an increase in hires and changes in the composition of hired workers.

2.6.3 Consequences for firms and for job seekers

Finally, I discuss the implications of these changes in hiring from the point of view of job seekers and firms. Media news attract additional applicants to plants with large hiring needs, and therefore contribute to improve the allocation of job search. The changes in the number and composition of hired applicants suggest that firms hire from the pool of compliers. In this section, I will provide some evidence that some compliers might even be hired in place of always-takers. The existence of displacement effects indicates that additional information has distributional effects on the access to employment for job seekers. It also suggests that firm might use the increase in their set of applicants to improve to match quality of their hires.

Consequences for job seekers

From the point of view of job seekers, news bring an additional information that allow to improve the overall allocation of applications across plants. In order to illustrate this view, I estimate the impact of news on the hiring rate of applicants in Table 2.8. Column (1), shows that there is an important initial difference between the hiring rate in treated and control plant of 2.3 ppt (given by the coefficient associated with "Treated"). This is consistent with the idea that these plants have larger hiring needs and offer better employment prospects to their applicants, although they

Table 2.8: Impact of news on hiring rate for different subgroups of job seekers

Dependent variable:	**Applicant is hired (dummy)**			
Sample:	**All Applicants**	**Applicants living relatively to the plant:** same city	same CZ	same departement
	(1)	(2)	(3)	(4)
AfterNews∗TreatedPlant	-0.007	-0.072**	-0.021	-0.013
	(0.007)	(0.034)	(0.016)	(0.011)
AfterNews	-0.001	-0.002	-0.002	-0.002*
	(0.001)	(0.002)	(0.001)	(0.001)
TreatedPlant	0.023***	0.096***	0.060***	0.039***
	(0.007)	(0.032)	(0.014)	(0.010)
Job seeker X	Yes	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes
No. of Obs.	306915	41511	155975	198424
P-value test: = (1)		0.033	0.149	0.131
Baseline level	0.050	0.108	0.086	0.069
Relative effect	-14.840	-66.537**	-24.437	-18.425

Notes: SE clustered at the plant level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.This table gives the coefficient estimated in a DiD at the application-level. The sample corresponds to applications sent to treated and control plants in the 15 weeks around the news. Each observation corresponds to one application and is characterized by the period in which it was sent (before/after the news) and the plant to which it was sent (treated/control plant). The model includes event fixed effect. The outcome variable is an indicator of application's success—applications of workers who will get hired in the plant. The coefficients After∗Treated therefore correspond to the estimated impact of news on the probability of being hired for applicants.

have the same recruiting activities. I observe that media news cause a decrease in the hiring rate in mentioned plant of 0.7 ppt, but the coefficient is not significant. This means that the probability of being hired is lower for job seekers who apply after the news than before. This results points to the existence of congestion due to the inflow of additional applicants. This suggests that media news favor a decrease in *pre-existing differences* in hiring rates between treated and placebo plants, hence contribute improve the overall allocation of application and to equalize congestion across plants.

From column (2) to (4), I compare the changes in hiring rates for job seekers living in different geographic units. The general pattern is that job seekers living in plants' geographic unit experience a stronger decrease in the hiring rate following media news than job seekers living outside this geographic unit. In particular, column (2) presents changes in the hiring rate for job seekers living in the city where the plant is located. The decrease in the hiring rare is significant at the 5% level and amounts to 7 ppt—which represents a decrease of 65%. This finding is interesting in light with the observed changes in the composition of applicants. I have shown that the magnitude in the change of composition of job seekers from the same city suggests that almost no compliers live in the city where the plant is located (in Table 2.F.3). The decrease in the hiring rate of this subgroup therefore provides evidence that media news have negative externalities on the hiring rate of job seekers who would have applied even in the absence of media news.

This result suggests that providing additional information causes displacement effects. Similar displacement effects associated with counseling programs for job seekers have been detected in the literature (see Crépon et al. (2013), Gautier et al. (2017), Laun et al. (2017)). In particular, in the context of France, Crépon et al. (2013) estimate a negative effect of counseling programs on the probability of being hired for non-treated job seekers in a large two stage randomized experiment. However, this is the first time that one can document displacement effects at the plant level and lay the emphasis on their geographic dimension. As predicted by Manning and Petrongolo (forthcoming), my results suggest that the effect of a local labor demand shock on local workers can be mitigated by the inflow of workers from other labor markets, to the extent they are informed about it. The welfare cost associated with this distributional effect on the access to employment depends on the objective function. If geographic mobility is an objective per se for instance, additional information could improve welfare.

Consequences for firms

Finally, I discuss the consequences of information about their hiring needs on firms. I argue that media news have a positive impact on firms both because they increase the quantity and also potentially the quality of their matches. First, the number of hires increases in mentioned plants by approximatively 50%. In itself, this confirms that media news increase the returns to recruiting activity for firms, as the benefit from finding additional matches should offset the cost of screening from a larger pool of applicants.

Second, plants also benefit from media news by getting a larger set of applicants among which they can chose their workers. In Table 2.8, the test comparing the coefficients from columns (1) and (2) shows that the decrease in the hiring rate of local applicants is significantly larger than the average decrease for all applicants. This means that local job seekers are negatively affected by the inflow of additional applicants even beyond the average effect of fiercer competition. One possible interpretation is that plants chose to hire compliers instead of always-takers because these applicants are preferred. Through revealed preferences, this suggests that the compliers represent a better match. Consequently, on top of the increase in hires, there could also be an increase in the match quality of new hired workers.

2.7 Conclusion

This paper highlights that job seekers cannot perfectly observe plants' hiring needs but that they integrate this dimension in their application decisions when they are informed. When media outlets cover plants' expansion, plants receive 60% more applications. The additional applications are not associated with a decrease in applicants' match quality but the reaction increase in the distance between job seekers' residence and plants' location. The impact of information on application translates to changes in matching between plants and workers. I estimate a substantial increase in the number of hires in the growing plants mentioned in media news. Moreover, my results indicate that job seekers living further away represent a larger proportion of newly hired workers after media news. To a certain extend, job seekers seem to search locally because they lack of information about the pay-off of applying to distant plants. More information could favor search in broader geographic area and increase geographic mobility. The counterpart of this effect is that media news appear to have negative externalities on local job seekers. Beyond the potential positive impact on aggregate unemployment and match quality, providing additional information has a distributional effect on the access to employment.

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Appendix

2.A Descriptive statistics

Figure 2.A.1: Example of news

Économie

ÉCONOMIE | Les données du « Monde » | Économie mondiale | Économie française | Entreprises | Emplo

General Electric détaille la teneur des 1 000 emplois bientôt créés en France

Les postes à haute valeur ajoutée seront répartis entre région parisienne et Belfort. Les premières embauches sont prévues en janvier.

Le Monde.fr | 07.12.2015 à 17h57 • Mis à jour le 07.12.2015 à 18h26 |

Par Jean-Michel Bezat

Abonnez vous à partir de 1 € | Réagir | Ajouter | Partager (535) | Tweeter

Accueil » Emploi » Emploi Jeune Diplômé » Les secteurs qui recrutent » Les entreprises qui recrutent | Retour

Renault Cléon annonce 100 embauches à l'occasion du lancement du nouveau moteur électrique de l'Alliance.

L'usine Renault Cléon va recruter 100 personnes à Contrats à Durée Indéterminée en 2015

Partager

Recevoir les alertes

Votre adresse e-mail | OK

RENAULT

Ces embauches s'inscrivent dans le cadre du plan annoncé par Carlos Ghosn le 12 février dernier, qui prévoit 1000 recrutements en 2015 dont 500 dans les usines françaises.

Notes: These are examples of news included in my data base.

Table 2.A.1: Vacancies posted on the PES search platform and hires in France in 2013

	Vacancies (Thousand)	Hires (Thousand)	Ratio (%)
Agriculture	8.28	26.40	31.40
Retail	168.32	487.56	34.50
Industry	95.14	241.12	39.50
Services	357.52	1692.45	21.10
Construction	43.03	286.67	15.00

Notes: Data come from the administrative declaration of hires (DPAE) and vacancies posted on PES search platform. Vacancies included are all vacancies for a permanent contract filled in 2013. Hires included are permanent contract in 2013. Both are from plants based in France, excluding Corse and oversee territories.

Table 2.A.2: Selection of vacancies with applications

	(1) All vacancies	(2) Vacancies with applications
Contract conditions		
Hourly wage (euros)	11.959	11.876
Weekly hours (hours)	30.663	32.071
Contract longer than 6 months	53.955	58.496
Contract longer than 1-6 months	38.401	38.273
Contract shorter than 1 months	7.645	3.231
Required diploma		
Superior education	14.607	18.548
High school level	6.449	12.201
Less than high school level	15.105	23.314
Required professional experience		
Require some professional experience	63.361	68.264
Required skill		
Blue collar, low skill	8.051	6.183
Blue collar, high skill	11.412	11.106
White collar, low skill	22.417	18.349
White collar, high skill	41.409	51.131
Management position	0.038	0.026
Nb observations	5,242,248	706,700
Proportion	88.121	11.879

Notes: This table presents the characteristics of vacancies posted on the French PES platform in 2014–2016 in plants from the study sample. Column (1) presents characteristics of all vacancies and column (2) presents characteristics of vacancies for which we observe at least one online application.

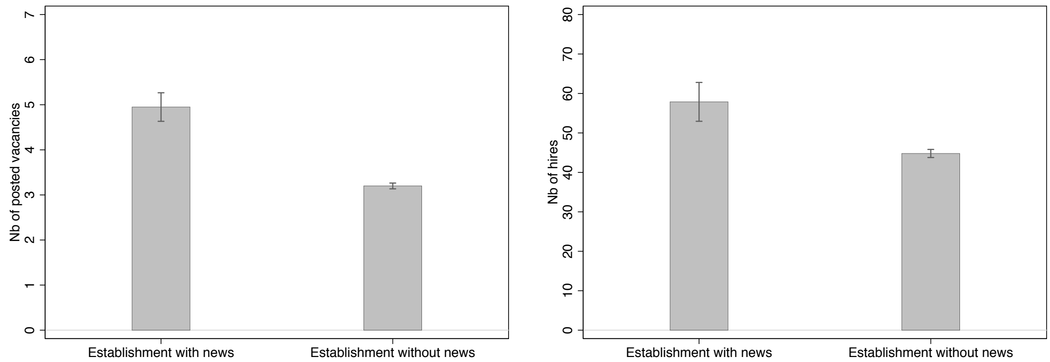
Table 2.A.3: Study sample of plants

Plants sample:	All		With vacancies		With online applications	
Weight by plant size:	No weight	Weight	No weight	Weight	No weight	Weight
	(1)	(2)	(3)	(4)	(5)	(6)
Size establishiment:						
0-5	78.3	7.1	63.7	3.8	54.1	2.1
5-20	16.8	17.0	25.4	13.5	28.9	9.7
20-50	4.2	22.3	9.2	24.6	13.9	23.2
More than 50	2.0	65.4	4.8	71.6	8.1	78.3
Nb hires per year:						
Less than 0.25	22.0	5.0	7.2	1.3	4.4	1.1
0.25–1	26.8	10.8	17.6	6.1	12.1	2.5
1–3	26.2	12.1	30.1	8.7	25.8	5.7
3–10	15.3	23.7	24.8	19.3	27.8	17.4
More than 10	9.7	48.4	20.2	64.6	30.0	73.3
Sector or activity:						
Agriculture	1.5	0.4	1.2	0.3	1.1	0.3
Extractive ind	0.1	0.1	0.1	0.1	0.1	0.0
Manufacturing ind	6.9	13.5	9.3	15.1	9.9	14.8
Energy	0.1	0.5	0.1	0.5	0.1	0.5
Water	0.4	0.9	0.5	0.7	0.5	0.7
Construction	14.4	12.8	10.9	7.3	8.6	7.5
Car retail	21.5	14.2	20.5	11.7	20.4	10.6
Transport	3.3	4.4	3.1	4.3	3.2	4.0
Hostel	11.2	5.5	13.2	6.3	13.9	2.9
Information	2.6	3.5	2.1	3.7	1.5	3.4
Finance	3.1	3.8	1.9	3.6	1.8	3.4
House	2.2	0.9	1.7	0.9	1.6	0.9
Sciences	8.5	5.9	7.2	5.4	6.1	4.6
Administration	6.0	8.3	5.7	9.9	6.4	10.3
Public	1.6	7.6	2.9	10.3	3.4	12.7
Teaching	2.1	4.3	3.2	4.5	3.9	4.9
Health	4.7	10.0	6.3	12.7	9.3	16.0
Art	3.4	1.5	2.8	1.0	2.0	0.9
Services	6.1	1.8	7.2	1.7	6.3	1.4
Nb observations	2,047,169	20,788,280	666,088	13,632,913	242,622	8,196,692
Proportion	100.0	100.0	32.5	65.5	11.9	39.4

Notes: This table describes the characteristics of plants selected in my study sample, i.e., that have at least one vacancy posted in 2014–2016. Comparing columns (1) and (2) with (3) and (4) allows to document the representativeness my study sample. Columns (5) and (6) presents characteristics of plants for which I can detect online applications in 2014–2016.

Figure 2.A.2: Differences in recruiting activities across plants

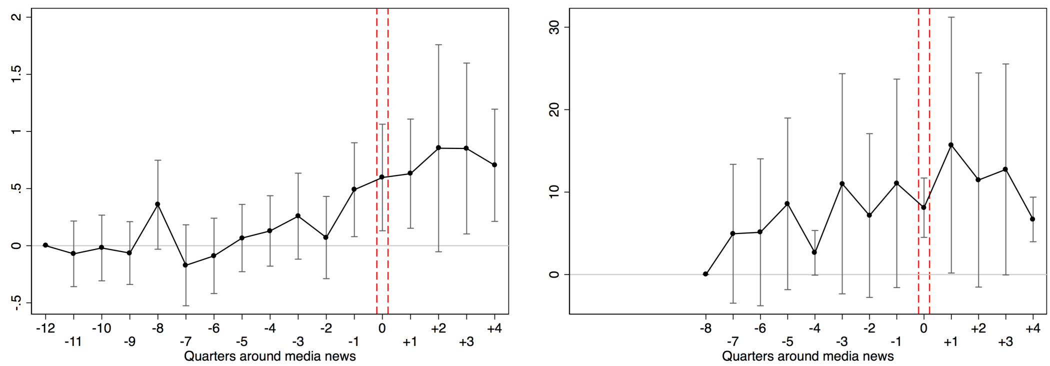
(1) Number of posted vacancies **(2) Number of hires**



Notes: These figures present the average number of posted vacancies (panel (1)) and hires (panel (2)) in all plants and in plants mentioned in media news during the year of the news, controlling for the plants' size, sector, location and for year fixed effect. The vertical lines denote 95% confidence intervals.

Figure 2.A.3: Within plant evolution of posted vacancies and hires around the news

(1) Posted vacancies **(2) Hires**



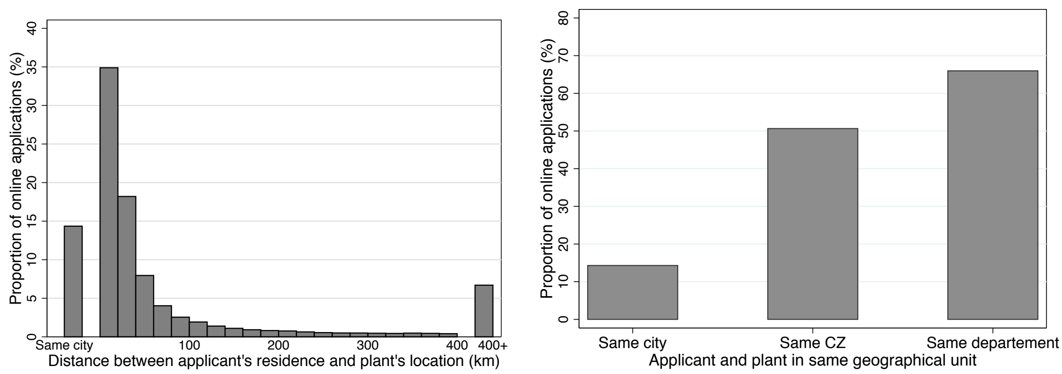
Notes: The figures present the long-term evolution of posted vacancies in (1) and hires in (2) in plants mentioned by news outlets. The time dimension correspond to quarter relative to the quarter of media news. The vertical dashed line denote the 15 weeks time window in which I focus to estimate the impact of media news. In (1), I include all news from 2014–2015 (and therefore leave out news from 2016) in order to observe the year posterior to media news. The first period is taken as a reference, it corresponds to 12 quarters before media news. In (2) I can only observe hires for 8 quarters before news because hires data are only available from 2012. The first period is taken as a reference, it corresponds to 8 periods before media news. The vertical lines denote 90% confidence intervals based on standard errors clustered at the plant level.

The geography of job search

The linked data contains zipcodes of plants' location and of applicants' residence. For each application, I therefore compute the Euclidian distance between the city of residence of the applicant and the city of the plant she applies to. I also build alternative measures using three types of geographic units of various size: city, commuting zone and departement. There are approximatively 30.000 cities in France, 380 commuting zones and 94 departements (when excluding oversee territories and Corsica). Cities and departements are administrative units and they do not provide a consistent measure of local labor markets. However, on the PES search platform, they are suggested as search criteria for job seekers who want to narrow down their search. They might therefore be a relevant unit of analysis for search activities. Commuting zones in contrast are built by the ministry of labor to capture the geographic dimensions of labor markets. In Figure 2.A.4, in panel (1), I present the distribution of geographic distance in online applications. I separately present applications made in the same city, which represent 15% of applications. 35% of applications are sent to surrounding cities located a distance of less than 20 km. Overall, job search appears to be very local as more than 80% of applications are sent to a city located less than 100 km away. However, 10% of applications are also sent to very large distances over 400 km, which suggest that these job seekers would be ready to move to a different location. In panel (2), we see that more than 50% of applications are sent within a commuting zone and more than 65% within a departement. Those geographic units are therefore meaningful for job seekers but a sizeable portion of search also takes place outside of them. In comparison, Marinescu and Rathelot (2016) observe that, on the U.S. search platform CareerBuilder, the average share of applications sent to plants within a state is 89%, within a commuting zone 81%, and within a zipcode only 4%.

Figure 2.A.4: Distribution of distance in online applications

(1) (2)



Notes: This figure presents the distribution of distance between applicant's city of residence and the city of the plant where they apply. The sample contains all online applications made during 2014 and contains 3 millions observations. Panel (1) shows the distribution of distance between the cities centers as the crow flies. In panel (2), I present the proportion of applications taking place within various geographic units.

2.B. Empirical strategy

Table 2.B.1: Comparison of treated and matched plants

	(1) All plants	(2) Treated plants	(3) Matched control plants
Nb of hires per month	3.41	16.51	11.64
Nb of posted vacancies per month	0.26	2.44	1.16
Size: 0 employee	30.63	4.86	3.71
Size: 1 employees	15.59	3.13	3.62
Size: 1-5 employees	20.04	4.23	5.69
Size: 5-10 employees	16.04	11.44	11.16
Size: 10-20 employees	7.71	13.95	15.56
Size: 20-50 employees	5.58	16.61	16.93
Size: 50-100 employees	2.19	12.54	11.60
Size: >100 employees	2.21	33.23	31.73
Sector: administration	5.82	4.23	7.03
Sector: agriculture	3.76	0.63	1.22
Sector: art	2.91	1.72	1.29
Sector: car retail	19.98	13.32	14.79
Sector: construction	11.09	0.94	6.58
Sector: energy	0.09	0.31	0.30
Sector: extractive ind	0.05	0.00	0.07
Sector: finance	1.86	2.35	1.63
Sector: health	5.71	5.17	13.22
Sector: hostel	12.56	2.66	6.04
Sector: house	1.77	0.31	1.05
Sector: information	2.07	7.21	3.36
Sector: manufacturing ind	8.89	46.55	18.38
Sector: personal service	0.00	0.00	0.00
Sector: public	2.63	0.31	5.79
Sector: science	7.05	9.25	6.04
Sector: services	7.21	0.78	3.27
Sector: teaching	3.12	0.31	4.32
Sector: transport	2.98	3.13	4.69
Sector: water	0.45	0.78	0.95
Qualification: Blue collar, low skill	9.19	9.72	9.21
Qualification: Blue collar, high skill	13.92	14.73	13.98
Qualification: White collar, low skill	19.85	12.70	12.83
Qualification: White collar, high skill	47.35	34.01	35.03
Qualification: Managing position	9.69	28.84	28.95
NbObs (Thousands)	666.09	0.61	90.19

Notes: This table compares the characteristics of plants mentioned in media news with plants (column (1)) and from the matched sample (column (3)). It should be noted that the statistics are computed in the period 2010–2016, and therefore not only on the period used for the matching of treated and control plants.

Evolution of the number of vacancies around media news

Table 2.B.2: Evolution of the number of vacancies around media news **reporting LARGE hiring needs**

Dependent variable:	**Nb of posted vacancies**			**Nb of posted vacancies open for online applications**		
	All	**Short-term contract**	**Long-term contract**	**All**	**Short-term contract**	**Long-term contract**
	(1)	(2)	(3)	(4)	(5)	(6)
Pre period	-0.073	-0.035	-0.038	-0.033	-0.026	-0.007
	(0.080)	(0.053)	(0.055)	(0.031)	(0.022)	(0.019)
Over 1 week	-0.030	-0.035	0.005	-0.024	-0.032	0.008
	(0.118)	(0.061)	(0.085)	(0.047)	(0.032)	(0.023)
Over 1 month	-0.042	-0.054	0.012	-0.020	-0.035*	0.014
	(0.076)	(0.054)	(0.053)	(0.029)	(0.019)	(0.020)
Time to event fixed effets	Yes	Yes	Yes	Yes	Yes	Yes
Plant fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	1527075	1527075	1527075	1527075	1527075	1527075
Baseline	0.333	0.147	0.186	0.090	0.045	0.045
Relative effect: Over 1 week	-9.519	-21.479	-0.078	-17.658	-50.690	15.373
Relative effect: Over 1 month	-9.974	-32.709	7.974	-3.260	-56.378	49.858

Notes: SE clustered at the plant level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The data base is a panel at the plant and week-to-news level. The sample corresponds to 15 weeks centered around the date of the news for treated and control plants. The table displays the effect of media coverage on the number of vacancies posted by the mentioned plant based on DiD regressions.

2.B. Evolution of the composition of vacancies

Table 2.B.3: Evolution of the characteristics of vacancies posted around media news

	AMONG ALL VACANCIES									
Dependent variable:	**Mean nb of unfilled jobs**	**Mean hourly wage**	**Proportion of posted vacancies with the following characteristics:**							
			Hours:	**Experience:**	**Type of qualification:**			**Required diploma:**		
			Full time	Required	Blue collar	White collar	Management position	Lower level	high school level	higher level
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Pre period	0.015	-0.095	-1.798	-6.057	-2.306	2.364	-11.415	15.541***	-4.704	-10.836**
	(0.272)	(0.351)	(3.321)	(5.977)	(3.292)	(5.994)	(8.223)	(5.955)	(4.962)	(4.936)
Over 1 week	-0.097	0.082	-0.732	-4.120	-2.137	-0.789	-5.772	8.185	-0.333	-7.852
	(0.291)	(0.342)	(3.506)	(6.774)	(4.148)	(6.607)	(10.302)	(7.468)	(5.391)	(6.169)
Over 1 month	0.288	0.140	-2.969	-7.341	-2.579	3.377	-10.917	11.446*	-2.592	-8.855*
No. of Obs.	3850987	3735855	3850987	3850987	3850987	3850987	3850987	3850987	3850987	3850987
Baseline	1.861	12.170	88.245	59.001	17.147	54.739	53.388	47.850	21.869	30.281
Dependent variable:	**AMONG VACANCIES OPEN FOR ONLINE APPLICATIONS**									
	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
Pre period	-0.289	0.194	3.370	-4.355	-5.232	-1.159	2.533	-3.454	5.056	-1.602
	(0.434)	(0.496)	(10.531)	(9.099)	(7.536)	(10.731)	(16.455)	(11.732)	(9.441)	(9.941)
Over 1 week	-1.106	0.647	-2.223	-6.152	-15.130	-1.518	22.141	-9.071	-5.572	14.643
	(1.104)	(0.772)	(10.088)	(14.221)	(12.205)	(15.259)	(18.128)	(16.251)	(14.434)	(11.562)
Over 1 month	-0.607	0.162	0.071	-6.032	-10.061	1.098	3.465	3.499	-3.374	-0.125
	(0.702)	(0.389)	(9.267)	(8.428)	(8.165)	(10.745)	(14.558)	(12.041)	(11.448)	(8.261)
No. of Obs.	3668162	3250793	3668162	3668162	3668162	3668162	3668162	3668162	3668162	3668162
Baseline	1.958	11.987	85.471	62.468	24.598	53.946	46.791	43.266	26.801	29.933

Notes: SE clustered at the plant level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The data base is a panel at the plant and week-to-news level. The sample corresponds to 15 weeks centered around the date of the news for treated and control plants. The table displays the effect of media coverage on the characteristics of vacancies posted by the mentioned plant based on DiD regressions.

Table 2.B.4: Evolution of the characteristics of vacancies posted around media news **reporting LARGE hiring needs**

Dependent variable:	**AMONG ALL VACANCIES**									
	Mean nb of unfilled jobs	**Mean hourly wage**	**Proportion of posted vacancies with the following characteristics:**							
			Hours:	**Experience:**	**Type of qualification:**			**Required diploma:**		
			Full time	Required	Blue collar	White collar	Management position	Lower level	high school level	higher level
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Pre period	0.379	-0.329	-5.205	-6.561	-0.236	2.563	-7.254	19.131**	-9.975	-9.156*
	(0.384)	(0.413)	(3.952)	(7.286)	(3.056)	(6.497)	(9.299)	(7.821)	(6.942)	(5.421)
Over 1 week	-0.019	0.102	-2.707	0.574	0.995	-7.449	-1.466	8.322	-0.760	-7.562
	(0.372)	(0.439)	(4.315)	(8.508)	(3.840)	(7.723)	(13.119)	(9.533)	(7.339)	(7.423)
Over 1 month	0.806**	0.063	-4.296	-6.489	-0.496	0.012	-8.458	14.454*	-5.630	-8.824
	(0.396)	(0.404)	(4.061)	(6.541)	(3.253)	(6.700)	(10.068)	(8.047)	(6.328)	(5.566)
No. of Obs.	1403455	1351862	1403455	1403455	1403455	1403455	1403455	1403455	1403455	1403455
Baseline	2.235	12.113	91.337	57.712	18.582	53.222	56.564	46.356	21.902	31.742
Relative effect: Over 1 week	-11.499	-0.012	-1.144	-0.167	-11.930	-5.680	-4.956	21.824	-18.209	-20.884
Relative effect: Over 1 month	39.973**	1.059	-3.377	-12.747	-9.596	3.265	-13.981	32.778*	-42.018	-21.487
Dependent variable:	**AMONG VACANCIES OPEN FOR ONLINE APPLICATIONS**									
	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
Pre period	0.023	0.650	-3.729	-4.840	-7.219	11.167	-3.260	-7.161	6.251	0.910
	(0.557)	(0.531)	(15.246)	(11.355)	(8.553)	(7.804)	(16.743)	(18.453)	(14.024)	(13.779)
Over 1 week	-1.505	0.460	-4.398	-5.504	-17.254	3.063	20.212	-22.779	7.990	14.789
	(1.175)	(1.069)	(13.761)	(14.432)	(12.609)	(15.875)	(18.514)	(18.415)	(17.092)	(13.224)
Over 1 month	0.315	-0.051	-2.011	0.898	-11.764	9.309	-3.105	-3.414	4.574	-1.160
	(0.769)	(0.460)	(13.307)	(9.847)	(9.613)	(10.767)	(12.022)	(17.698)	(15.641)	(8.383)
No. of Obs.	1311323	1143223	1311323	1311323	1311323	1311323	1311323	1311323	1311323	1311323
Baseline	2.403	11.888	85.229	59.979	28.333	51.736	43.559	44.358	29.514	26.128
Relative effect: Over 1 week	-62.640	3.871	-5.160	-9.176	-60.896	5.920	46.402	-51.354	27.072	56.603
Relative effect: Over 1 month	13.104	-0.433	-2.360	1.497	-41.520	17.994	-7.127	-7.696	15.498	-4.441

Notes: SE clustered at the plant level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The data base is a panel at the plant and week-to-news level. The sample corresponds to 15 weeks centered around the date of the news for treated and control plants. The table displays the effect of media coverage on the characteristics of vacancies posted by the mentioned plant based on DiD regressions.

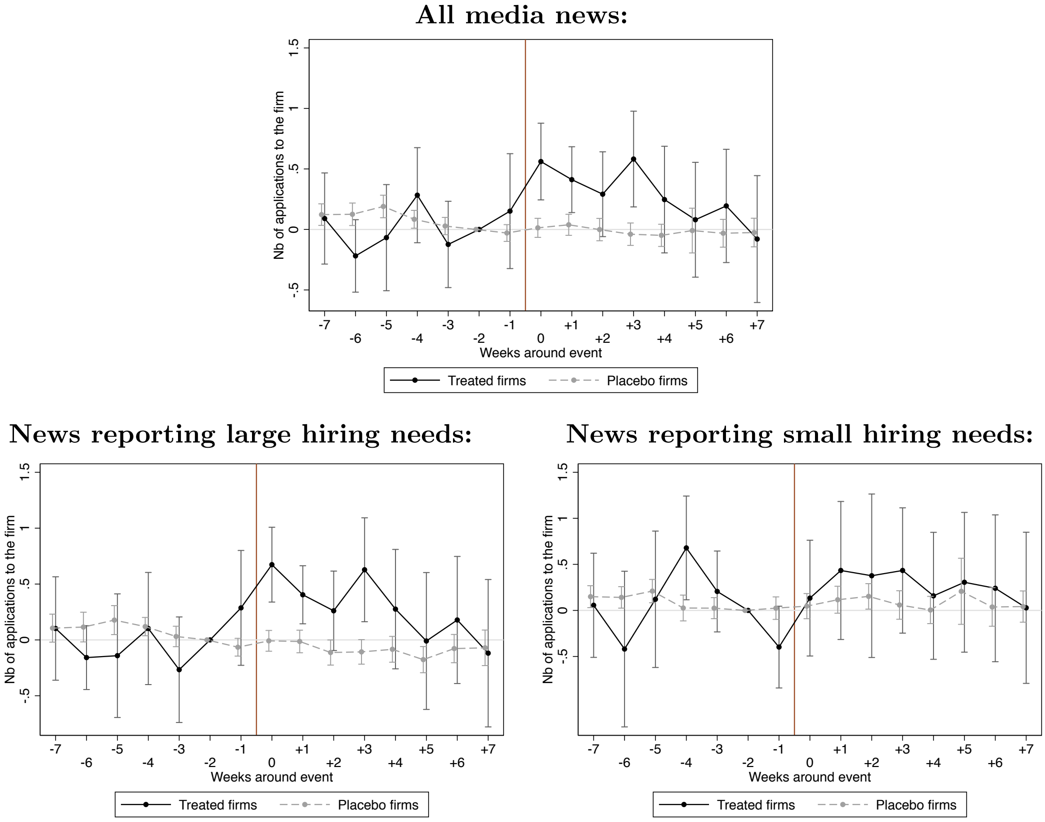
2.C. Robustness checks

Table 2.C.1: Heterogeneity of the impact media news on the number of applications depending on the content of the news

Sample of events:	**Number of jobs**		**Number of jobs relative to hires in dpt**		**Number of jobs relative to plant's size**	
	High	Low	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)
Pre event	0.021	0.126	0.002	0.181	0.237*	-0.119
	(0.003)	(0.033)	(0.000)	(0.049)	(0.030)	(0.026)
Effect over 1 week	1.025***	0.064	1.032***	0.053	1.311***	0.110
	(0.209)	(0.024)	(0.210)	(0.020)	(0.280)	(0.026)
Effect over 1 month	0.780***	0.205	0.759***	0.239	0.806***	0.271
	(0.117)	(0.074)	(0.114)	(0.089)	(0.111)	(0.070)
No. of Observations	1527075	2586675	1493610	2620140	2841675	1272075
No. of News events	306	306	308	304	319	293

Notes: SE clustered at the plant level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The data base is a panel at the plant and week-to-news level. The sample corresponds to 15 weeks centered around the date of the news for treated and control plants. I estimate a Poisson model, and present in the table $exp(\beta) - 1$ in order to interpret the coefficients as the relative impact on the outcome. SEs are computed using the delta method and the confidence intervals correspond to the transformed endpoints of the confidence intervals in the natural parameter space.

Figure 2.C.1: Impact of media news on applications to firms estimated in a Poisson model



Notes: These figures display time-to-event fixed effects for treated and control plants separately. The model includes plant fixed effects, is fully saturated and period $k = -2$ is normalized to 0. Each coefficient represents the difference between the level of the outcome variable each week around the event and in the reference period $k = -2$ within the treated or the control group. The vertical lines denote 90% confidence intervals based on standard errors clustered at the plant level.

Table 2.C.2: Robustness : Estimation of the impact of media news on applications, controlling for vacancies

Dependant variable:	Number of online applications						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Pre period	0.011	0.039	0.025	0.039	0.055	0.048	0.049
	(0.080)	(0.086)	(0.073)	(0.077)	(0.083)	(0.079)	(0.074)
Over 1 week	0.336*	0.389***	0.373**	0.395***	0.396***	0.351**	0.402***
	(0.180)	(0.148)	(0.170)	(0.150)	(0.144)	(0.159)	(0.151)
Over 1 month	0.264**	0.289***	0.307**	0.303***	0.297***	0.292**	0.320***
	(0.114)	(0.106)	(0.120)	(0.111)	(0.103)	(0.117)	(0.115)
Nb ST and LT vacancies for online applications	No	Yes	No	Yes	Yes	No	Yes
Nb ST and LT vacancies (all type)	No	No	Yes	Yes	No	Yes	Yes
Nb unfilled jobs in vacancies for online applications	No	No	No	No	Yes	No	Yes
Nb unfilled jobs in vacancies (all type)	No	No	No	No	No	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	4113750	4113750	4113750	4113750	4113750	4113750	4113750

Notes: SE clustered at the plant level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The data base is a panel at the plant and week-to-news level. The sample corresponds to 15 weeks centered around the date of the news for treated and control plants. The table displays the effect of media coverage on the number of applications sent to the mentioned plant based on DiD regressions. The pre-event presents the average of coefficients corresponding to the months prior to $k-2$ and should not be significant if the parallel trend assumption is verified. I present separately the immediate effect the week of the news and the effect in the following month (from k to $k+3$). In the upper part of the table, I present the standard coefficients which represent an absolute increase in the outcome. The bottom part of the table presents the ratio of the absolute effects to the baseline level of the outcome, corresponding to the relative effect.

Table 2.C.3: Robustness : Comparison of before/after and DiD estimators

Sample	All news		News about large hiring needs		News about small hiring needs	
Estimator	before/after	DiD	before/after	DiD	before/after	DiD
	(1)	(2)	(3)	(4)	(5)	(6)
Pre period	0.008	0.011	-0.026	-0.029	0.043	0.048
	(0.080)	(0.080)	(0.142)	(0.142)	(0.073)	(0.073)
Over 1 week	0.338*	0.336*	0.644*	0.650*	0.033	0.025
	(0.181)	(0.180)	(0.347)	(0.346)	(0.098)	(0.098)
Over 1 month	0.268**	0.264**	0.440**	0.451**	0.096	0.084
	(0.114)	(0.114)	(0.204)	(0.204)	(0.100)	(0.100)
Time FE	No	Yes	No	Yes	No	Yes
Plant FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	9180	4113750	4590	1491735	4590	2622015
Baseline	0.453	0.453	0.637	0.637	0.269	0.269
Relative difference (%) : Over 1 week	74.729*	74.217*	101.112*	102.108*	12.170	9.475
Relative difference (%) : Over 1 month	59.296**	58.379**	69.162**	70.791**	35.903	31.284

Notes: SE clustered at the plant level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The data base is a panel at the plant and week-to-news level. The sample corresponds to 15 weeks centered around the date of the news for treated and control plants. The table displays the effect of media coverage on the number of applications sent to the mentioned plant based on DiD regressions. The pre-event presents the average of coefficients corresponding to the months prior to $k-2$ and should not be significant if the parallel trend assumption is verified. I present separately the immediate effect the week of the news and the effect in the following month (from k to $k+3$). In the upper part of the table, I present the standard coefficients which represent an absolute increase in the outcome. The bottom part of the table presents the ratio of the absolute effects to the baseline level of the outcome, corresponding to the relative effect.

Table 2.C.4: Heterogeneity of the impact on the number of applications depending on the set of events

Sample of events:	**All news**				**News about large hiring needs**			
	All events	**First events**	**Without plants with multiple events**	**Without Paris**	**All events**	**First events**	**Without plants with multiple events**	**Without Paris**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre event	0.057	-0.011	0.094	0.044	0.021	-0.075	-0.047	0.011
	(0.007)	(0.002)	(0.014)	(0.006)	(0.003)	(0.014)	(0.009)	(0.002)
Following week	0.719***	0.562**	0.693***	0.717***	1.025***	0.887***	0.887***	1.039***
	(0.139)	(0.112)	(0.134)	(0.138)	(0.209)	(0.193)	(0.211)	(0.212)
Following month	0.544***	0.373*	0.531***	0.540***	0.780***	0.520**	0.573***	0.795***
	(0.080)	(0.062)	(0.086)	(0.080)	(0.117)	(0.085)	(0.101)	(0.119)
No. of Obs.	4113750	3873900	3762810	3822990	1527075	1498920	1447980	1403850
No. of events	612	581	559	590	306	291	279	294

*Notes:*SE clustered at the plant level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The data base is a panel at the plant and week-to-news level. The sample corresponds to 15 weeks centered around the date of the news for treated and control plants. I estimate a Poisson model, and present in the table $exp(\beta) - 1$ in order to interpret the coefficients as the relative impact on the outcome. SEs are computed using the delta method and the confidence intervals correspond to the transformed endpoints of the confidence intervals in the natural parameter space.

Table 2.C.5: Plant-level estimation of the heterogeneous effect of news on applications sent to mentioned firms using a Poisson count model

Dependent variable:	**Nb of applications of job seekers living ... relatively to the plant**					
	= city	**≠ city**	**= commut. z.**	**≠ commut. z.**	**= departement**	**≠ departement**
Pre period	-0.168	0.093	-0.027	0.132	0.012	0.141
	(0.034)	(0.012)	(0.004)	(0.017)	(0.002)	(0.026)
Over 1 week	0.311	0.785***	0.524**	0.904***	0.508**	1.135***
	(0.064)	(0.162)	(0.102)	(0.221)	(0.105)	(0.268)
Over 1 month	0.129	0.613***	0.310*	0.761***	0.297	1.034***
	(0.020)	(0.094)	(0.048)	(0.121)	(0.049)	(0.220)
No. of Obs.	4113750	4113750	4113750	4113750	4113750	4113750

Notes: SE clustered at the departement level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The data base is a panel at the plant and week-to-news level. The sample corresponds to 15 weeks centered around the date of the news for treated and control plants. I estimate a Poisson model, and present in the table $exp(\beta) - 1$ in order to interpret the coefficients as the relative impact on the outcome. SEs are computed using the delta method and the confidence intervals correspond to the transformed endpoints of the confidence intervals in the natural parameter space.

2.D. Prediction of match:

I keep all applications sent to control plants in 2014–2016. I exclude applications from job seekers who have made several applications to the same plant in the period. For each plant, I therefore identify a pool of unique applicants. Among them, I finally detect who is hired in the plant in the 6 months following the application. In Table 2.D.1, column (1) I only include covariates representing the adequacy between one candidate and one match. From column (2) to (4), I broaden the set of covariates by including successively different types of applicants' characteristics. I will use the most complete specification from column (4) to predict the probability of success of applications made to treated and control plants and build the match quality index.

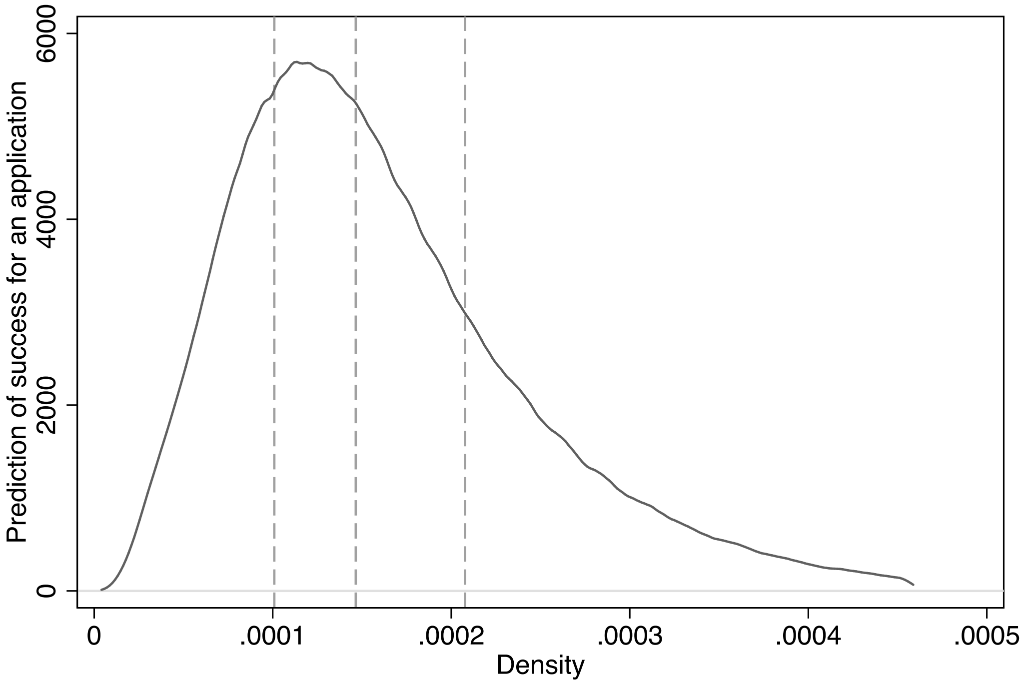
Table 2.D.1: Logit model of probability of success of an application depending on applicants' characteristics

	(1)	(2)	(3)	(4)
Adequacy between applicants and job offer				
> required education level	0.597***	0.692***	0.700***	0.735***
	(0.026)	(0.030)	(0.030)	(0.030)
= required education level	0.410***	0.449***	0.456***	0.489***
	(0.024)	(0.026)	(0.026)	(0.026)
> skill level	0.152***	0.272***	0.282***	0.250***
	(0.022)	(0.032)	(0.031)	(0.032)
= skill level	0.095***	0.106***	0.108***	0.081***
	(0.014)	(0.018)	(0.018)	(0.018)
Searching in vacancy's specific occupation	0.207***	0.224***	0.212***	0.228***
	(0.020)	(0.020)	(0.020)	(0.020)
≥ Required prof experience in specific occupation	0.354***	0.329***	0.321***	0.326***
	(0.023)	(0.022)	(0.023)	(0.023)
Education level (reference : Inferior to high school)				
HS Technical track		0.166***	0.156***	0.145***
		(0.023)	(0.023)	(0.023)
HS general track		0.107***	0.102***	0.057**
		(0.024)	(0.025)	(0.025)
Superior education		-0.060**	-0.068**	-0.138***
		(0.028)	(0.028)	(0.028)
Skills level (reference : blue collar, low skill)				
White collar, low skill		0.098***	0.101***	0.016
		(0.028)	(0.028)	(0.028)
Blue collar, high skill		0.115***	0.077**	0.118***
		(0.031)	(0.031)	(0.031)
White collar, high skill		0.105***	0.067**	-0.000
		(0.030)	(0.030)	(0.030)
Management level		-0.109***	-0.160***	-0.161***
		(0.041)	(0.041)	(0.042)

	(1)	(2)	(3)	(4)
Labor market history				
Professional experience : above 1-2 years			0.139***	0.122***
			(0.020)	(0.021)
Professional experience : above 2-3 years			0.154***	0.135***
			(0.020)	(0.020)
Professional experience : above 3-5 years			0.195***	0.172***
			(0.018)	(0.018)
Professional experience : above 5 years			0.157***	0.165***
			(0.017)	(0.018)
Unemployed since 6 months to 1 year			0.008	0.009
			(0.014)	(0.014)
Unemployed since 1 to 2 years			0.015	0.021
			(0.015)	(0.014)
Unemployed since more than 2 years			-0.096***	-0.072***
			(0.017)	(0.017)
Number of previous unemployment episodes			0.000	0.000
			(0.003)	(0.003)
Characteristics of job looked for				
Permanent duration contract			-0.138***	-0.150***
			(0.022)	(0.022)
Full time position			-0.088***	-0.023
			(0.023)	(0.023)
Hourly wage > 9.67 €/h (≈ minimum wage)			0.069***	0.101***
			(0.012)	(0.012)
Socio-demographic characteristics				
Male				-0.268***
				(0.017)
Nb of children				0.013**
				(0.005)
Single				-0.135***
				(0.012)
Age : 25-35				0.041***
				(0.016)
Age : 35-45				-0.030
				(0.019)
Age : above 45				-0.143***
				(0.021)
Constant	-4.758***	-4.951***	-4.858***	-4.657***
	(0.027)	(0.038)	(0.049)	(0.051)
No. of Obs.	2275970	2275970	2275970	2275970
Deviance	402850	402220	401902	400987
Degrees of freedom of dev	2275962	2275956	2275945	2275939
Wald X2	3282	3912	4229	5144
Degrees of freedom of X2	7	13	24	30
Prob > Wald X2	0.000	0.000	0.000	0.000
McFadden R2	0.008	0.010	0.010	0.013

Notes: SE clustered at the plant level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Firms characteristics include firms' size and sector of activity.

Figure 2.D.1: Density of job applications' estimated probability of success



Notes: This graph presents the distribution of the probability of success of applications in the sample of applications used for the logit model. Dashed lines indicates limits between quartiles.

2.E. Evolution in the number of all hires

I estimate a plant-level DiD model with the number of hires as dependent variable. When estimating the impact of information shocks on the number of successful applicants, I use the date of the application in order to attribute a hire to an application made after the news. However, as I only observe this variable for online applicants, I can only use the date of the hire. However, as there should be a delay between the application and the hire, the impact of media news on hire should happen with a lag. I therefore have to study the number of hires around the news during a larger time window. I therefore present the evolution in hiring in the same model as for applications but with a longer time period. However, over a longer time period, the identification strategy is less convincing as it is difficult to disentangle a potential impact of media news from the general upward trend the hiring of plants. Therefore, the estimates of the impact of media news on all hire are likely to be upward biased.

In Table 2.E.1, column (1) presents the evolution of the number of hires around media news. It suggests that news are associated with approximatively 0.7 additional hires per week in the two following months. The increase is more visible as I broaden the time window: over 3 months, the increase amounts to 1 additional hire per week. This represents a relative increase of 35% in the number of hires. Comparing columns (2) and (3), I can see that this increase is driven by hiring in plants for which media news mentioned large hiring needs. This hiring data source does not include wages, but it includes the planned duration of contracts at the moment they are signed. I can therefore examine whether contracts offered to applicants have a long duration (from more than 6 months to no planned end of contract) or a short duration (planned term within less than 6 months). Comparing columns (4) to (6) with columns (7) to (9), I can see that the increase in hires is mainly driven by short-term contracts. Results are presented graphically in Figure 2.E.1. I observe in panel (1), (3) and (5) that the evolution of hiring previous to the news is similar in treated and control plants for all types of news. After the news, I see clearly that the number of hires per week increases even if estimates are noisy at such a high frequency. Coefficients associated with the difference between treated and control group relative to the reference period $d-2$ are all positive after the news and many of them are significant.

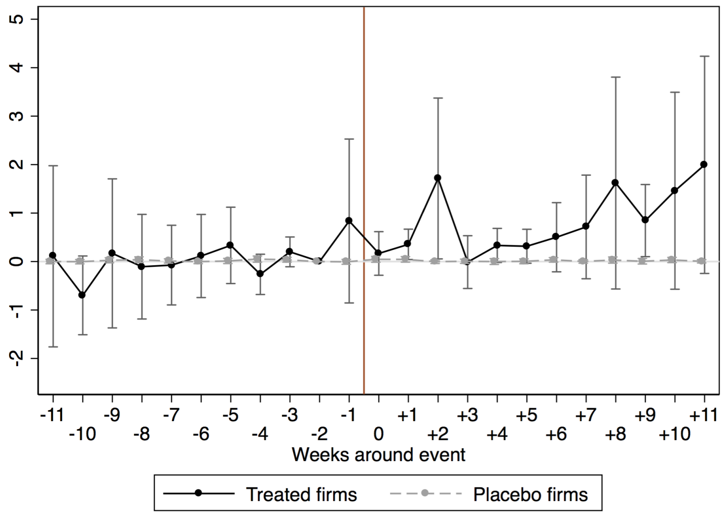
Table 2.E.1: Plant-level estimation of changes in hires following news

Dependent variable	**Number of hires all contract durations**			**Number of hires long-term contract**			**Number of hires short-term contract**		
Sample	All news	News about large job creation	News about small job creation	All news	News about large job creation	News about small job creation	All news	News about large job creation	News about small job creation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Pre period	-0.043	-0.102	0.015	-0.194	-0.351	-0.034	0.151	0.249	0.049
	(0.497)	(0.984)	(0.142)	(0.158)	(0.307)	(0.079)	(0.471)	(0.935)	(0.118)
Following month	0.532**	0.696	0.380**	0.011	-0.060	0.090	0.521***	0.757**	0.289**
	(0.240)	(0.455)	(0.158)	(0.226)	(0.447)	(0.072)	(0.165)	(0.301)	(0.138)
Following 2months	0.494***	0.768**	0.225	-0.096	-0.233	0.049	0.590**	1.001*	0.176
	(0.169)	(0.303)	(0.152)	(0.206)	(0.405)	(0.070)	(0.289)	(0.563)	(0.130)
Following 3months	0.817**	1.503**	0.129	-0.133	-0.281	0.023	0.949**	1.784**	0.106
	(0.356)	(0.693)	(0.155)	(0.214)	(0.423)	(0.069)	(0.457)	(0.901)	(0.133)
Time to event FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	6307750	2287327	4020423	6307750	2287327	4020423	6307750	2287327	4020423
Baseline	2.877	4.868	0.886	0.789	1.278	0.300	2.087	3.589	0.586
Rel. diff. (%): 1 month	18.509**	14.307	42.841**	1.427	-4.702	30.057	24.969***	21.077**	49.397**
Rel. diff. (%): 2 months	17.172***	15.777**	25.416	-12.131	-18.253	16.435	28.253**	27.898*	30.021
Rel. diff. (%): 3 months	28.382**	30.879**	14.535	-16.808	-21.997	7.694	45.471**	49.712**	18.043

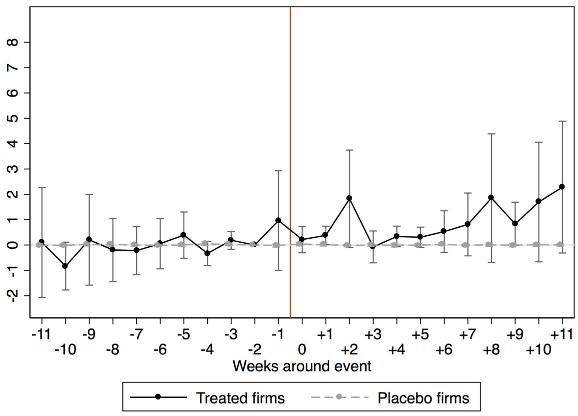
Notes: SE clustered at the departement level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The sample corresponds to treated plants and the average of control plants for each news. The data base is a panel at the plant, week-to-news level. The panel is centered around the date of the news and goes from $d-11$ to $d+11$. The table displays the evolution of hires in plants mentioned in media news based on DiD regressions.

Figure 2.E.1: Number of hires around the news

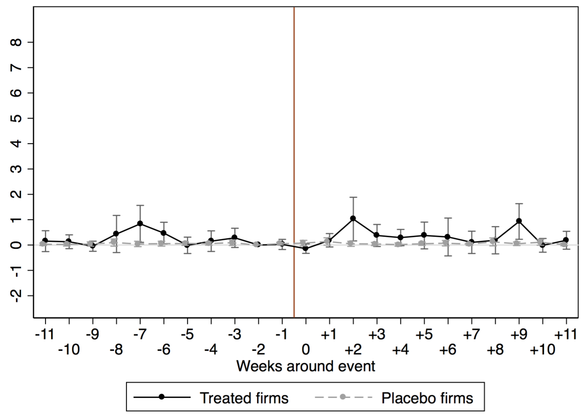
(1) All media news:



(2) News about large hiring needs:



(3) News about small hiring needs:



Notes: Figures show the coefficient for fixed effects of time relative to the event D_t^k for treated plants and control plants separately. The model includes plant fixed effects and is fully saturated. Period $k = -2$ is normalized to 0. Each coefficient represents the difference between the number of persons hired each week around the event and in the reference period $k = -2$ in the treated or control group. The vertical lines denote 90% confidence intervals based on standard errors clustered at the plant level.

2.F. Additional results

Table 2.F.1: Heterogeneity of the impact of media news on the number of applications, estimated in a Poisson model

	Heterogeneity in terms of news' diffusions			
Sample of events:	**Diffusion**		**Number of sources**	
	National	Local	Several	One
	(1)	(2)	(3)	(4)
Pre event	0.338	-0.034	0.264*	-0.138
	(0.076)	(0.005)	(0.035)	(0.033)
Effect over 1 week	1.308***	0.545**	0.975***	0.415
	(0.421)	(0.108)	(0.221)	(0.133)
Effect over 1 month	0.908***	0.442**	0.790***	0.312
	(0.219)	(0.073)	(0.132)	(0.082)
No. of Observations	1854435	2259315	2157510	1956240
No. of News events	240	372	290	322

Notes: SE clustered at the plant level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The data base is a panel at the plant and week-to-news level. The sample corresponds to 15 weeks centered around the date of the news for treated and control plants. I estimate a Poisson model, and present in the table $exp(\beta) - 1$ in order to interpret the coefficients as the relative impact on the outcome. For example, column (1) indicates national news cause an increase in the number of applications by 90.8% in the following month. SEs are computed using the delta method and the confidence intervals correspond to the transformed endpoints of the confidence intervals in the natural parameter space.

Table 2.F.2: Application-level estimation of the impact of news on the composition of applicants

Dep variable:	**Applicant has a specific characteristic**							
	Female		Low skill		Blue collar		Low education	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effect	-0.009	-0.006	0.048**	0.035*	0.009	0.000	0.076***	0.054***
	(0.021)	(0.020)	(0.019)	(0.018)	(0.024)	(0.006)	(0.024)	(0.018)
Job offer X	No	Yes	No	Yes	No	Yes	No	Yes
No. of Obs.	306915	306915	306915	306915	306915	274773	306915	306915
Baseline level	0.541	0.541	0.204	0.204	0.209	0.209	0.311	0.311
Relative effect	-0.016	-0.011	0.233**	0.172*	0.045	0.001	0.244***	0.173***

	Low experience		Age < 25		High desired wage		U spell > 2 years	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Effect	-0.021	-0.023	0.043**	0.041**	-0.019	0.000	0.037***	0.031***
	(0.016)	(0.015)	(0.019)	(0.020)	(0.031)	(0.034)	(0.010)	(0.009)
Job offer X	No	Yes	No	Yes	No	Yes	No	Yes
No. of Obs.	306915	306915	306915	306915	306915	306915	306915	306915
Baseline level	0.375	0.375	0.283	0.283	0.595	0.595	0.166	0.166
Relative effect	-0.057	-0.062	0.152**	0.144**	-0.031	0.001	0.223***	0.190***

Notes: SE clustered at the plant level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. This table gives the coefficient estimated in a DiD at the application-level. The sample corresponds to applications sent to treated and control plants in the 15 weeks around the news. Each observation corresponds to one application and is characterized by the period in which it was sent (before/after the news) and the plant to which it was sent (treated/control plant). The model includes event fixed effect. The outcome variable is a variable describing the characteristics of applicants. When it is a dummy variable, the coefficients corresponding to AfterNews∗Treated can be interpreted as the effect of the news on the probability that an applicant has this characteristic; it when is a continuous variable, the coefficients can be interpreted as the effect of the news on the level of this characteristic among applicant. These coefficient therefore correspond to the estimated impact of news on the composition of applicants to the mentioned plant.

Table 2.F.3: Characteristics of compliers

Characteristics of applications to treated plant	Mean composition Before news (Always Takers) (1)	After news (AT + compliers) (2)	Estimated composition for compliers (3)	Ratio 100∗AT/compliers (4)
Education level				
Low diploma	0.311	0.392	0.542	174.428
	(0.013)	(0.010)	(0.096)	
Bac level	0.287	0.280	0.281	98.012
	(0.013)	(0.009)	(0.045)	
Higher Bac	0.392	0.321	0.172	43.886
	(0.014)	(0.010)	(0.094)	
Unemployment history				
U spell : Less 6months	0.447	0.433	0.430	96.249
	(0.014)	(0.010)	(0.062)	
U spell : 6-24 months	0.388	0.357	0.271	69.908
	(0.014)	(0.010)	(0.064)	
U spell : more 24 months	0.166	0.209	0.299	180.465
	(0.010)	(0.008)	(0.051)	
Distance				
Distance	76.435	112.066	216.132	282.764
	(4.010)	(4.045)	(68.890)	
Different city	0.885	0.902	0.992	112.111
	(0.009)	(0.006)	(0.042)	
Different commuting zone	0.583	0.630	0.835	143.332
	(0.014)	(0.010)	(0.117)	
Different departement	0.377	0.478	0.773	205.137
	(0.014)	(0.010)	(0.164)	
Demographic				
Male	0.459	0.486	0.567	123.516
	(0.014)	(0.010)	(0.060)	
Age : 25–35	0.283	0.325	0.441	155.625
	(0.013)	(0.010)	(0.081)	
Age : 35–45	0.362	0.354	0.332	91.682
	(0.013)	(0.010)	(0.044)	
Age : 45–55	0.207	0.184	0.131	63.089
	(0.011)	(0.008)	(0.042)	
Age : over 55	0.148	0.137	0.097	65.466
	(0.010)	(0.007)	(0.041)	
Estimated match quality				
Low quality	0.480	0.472	0.453	94.396
	(0.014)	(0.010)	(0.076)	
High quality	0.520	0.528	0.547	105.176
	(0.014)	(0.010)	(0.076)	

Notes: This table presents descriptive statistics for applications to treated plants in column (1) and (2). Column (3) presents the characteristics of compliers recovered via the IV method from Abadie (2003): I use the period after the news (After) as an instrument for directing the applications to the mentioned firm (Treated). This method relies on the hypothesis that the number of defiers is negligible, which means that there should not be job seekers who react to the news by applying less to the mentioned firm.

Table 2.F.4: Application-level estimation of the impact of news on the adequacy between applicants and job ads

Dep variable:	**Applicant corresponds to the job ad in a specific dimension (dummy)**							
					Occupation type			
	Skill level		Education level		(1 digit)	(3 digits)	(5 digits)	Experience
	Equal	Superior	Equal	Superior	Equal	Equal	Equal	Equal
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
After News∗Treated	0.002	-0.012	-0.039	0.027	-0.003	-0.014	-0.004	0.012
	(0.026)	(0.026)	(0.029)	(0.020)	(0.026)	(0.030)	(0.022)	(0.037)
AfterNews	-0.003	-0.002	-0.000	-0.000	0.005	0.004	0.004	0.004
	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.003)	(0.007)
TreatedPlant	0.051**	-0.005	0.078***	-0.060***	0.018	0.003	0.020	0.133***
	(0.024)	(0.021)	(0.027)	(0.013)	(0.020)	(0.024)	(0.020)	(0.044)
Event FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	306915	306915	306915	306915	306915	306915	306915	306915

Notes: SE clustered at the plant level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. This table gives the coefficient estimated in a DiD at the application-level. The sample corresponds to applications sent to treated and control plants in the 15 weeks around the news. Each observation corresponds to one application and is characterized by the period in which it was sent (before/after the news) and the plant to which it was sent (treated/control plant). The model includes event fixed effect. The outcome variable is a variable describing the characteristics of applicants. When it is a dummy variable, the coefficients corresponding to AfterNews∗Treated can be interpreted as the effect of the news on the probability that an applicant has this characteristic; it when is a continuous variable, the coefficients can be interpreted as the effect of the news on the level of this characteristic among applicant. These coefficient therefore correspond to the estimated impact of news on the composition of applicants to the mentioned plant.

Table 2.F.5: Application-level estimation of the impact of news on the composition of hired applicants

Dep variable:	**Hired applicant has a specific characteristic:**					
	Female	Low skill	Low diploma	Low experience	Age <25	U spell >2 years
	(1)	(2)	(3)	(4)	(5)	(6)
After∗Treated	0.087	-0.047	-0.008	0.071	0.186**	0.044
	(0.073)	(0.075)	(0.069)	(0.051)	(0.073)	(0.053)
Event FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	7306	7306	7306	7306	7306	7306
Baseline level	0.656	0.297	0.344	0.516	0.312	0.125
Relative effect	13.297	-15.848	-2.236	13.853	59.501**	35.516

Notes: SE clustered at the plant level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. This table gives the coefficient estimated in a DiD at the application-level. The sample corresponds to **successful applications**—applications of workers who will get hired in the plant—sent to treated and control plants in the 15 weeks around the news. Each observation corresponds to one application and is characterized by the period in which it was sent (before/after the news) and the plant to which it was sent (treated/control plant). The model includes event fixed effect. The outcome variable is a variable describing the characteristics of applicants. When it is a dummy variable, the coefficients corresponding to AfterNews∗Treated can be interpreted as the effect of the news on the probability that an applicant has this characteristic; it when is a continuous variable, the coefficients can be interpreted as the effect of the news on the level of this characteristic among applicant. These coefficient therefore correspond to the estimated impact of news on the composition of applicants to the mentioned plant.

Chapter 3

Direct Evidence on the Impact of Unemployment Insurance Duration on Job Search

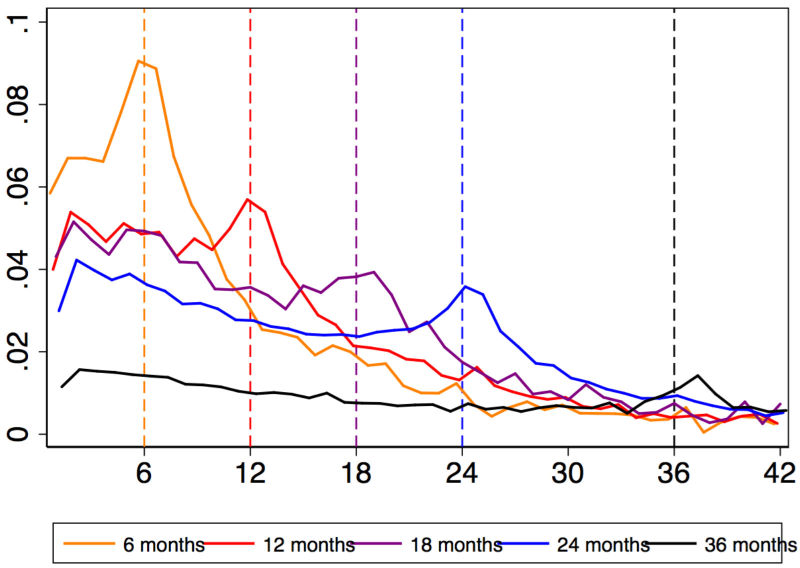
This chapter is joint with Ioana Marinescu

Why do people who receive unemployment benefits for longer stay unemployed for longer? Do they search less hard, do they demand higher wages? Using French administrative longitudinal data on job search and unemployment benefits at the individual level, we estimate directly how potential benefits duration affects job search over the unemployment spell, both on the intensity and selectivity margin. We first highlight a 25% spike in job search intensity in the months surrounding benefits exhaustion, when controlling for dynamic selection. On the selectivity margin, we document a very small and typically insignificant drop in the target wage around benefits exhaustion (around 0.5%). Additionally, exploiting a discontinuity in unemployment rules in France around age 50, we show in a regression discontinuity design that the duration of unemployment insurance only starts affecting search behavior in the months preceding benefits exhaustion. This suggests that the moral hazard associated with an unemployment insurance extension appears in the periods leading up to benefits exhaustion but not from the start of the unemployment spell. Overall, these results suggest that a benefits extension increases unemployment duration mostly by postponing the spike in search intensity associated with benefits exhaustion. This search behavior is not consistent with standard search models but is consistent with alternative models featuring a shift in incentives or perceptions at benefits exhaustion, such as the reference-dependent model.

3.1 Introduction

It is a very well established fact that the potential duration of unemployment benefits increases unemployment duration and to a lesser extend non employment duration, and many articles have documented a spike in the hazard of finding a job around benefits exhaustion. In France, we clearly observe spikes in the employment hazard measured in administrative data around benefits exhaustion, for all potential benefits duration (PBD) (see Figure 3.1). This empirical pattern has generated many discussions in the literature, both because it suggests that unemployment insurance (UI) distorts incentives to search, and because it suggests that job seekers' behavior is inconsistent with the standard search model (Mortensen (1977)): while this model predicts an increase in search intensity in the period leading up to benefits exhaustion, it cannot account for a decrease in search after. These discussions implicitly rely on the assumption that search intensity follows the same pattern as job finding rate, but there exists no clear evidence of this so far.

Figure 3.1: Empirical hazard of finding a job for job seekers eligible to different PBD



Notes: These rates correspond to the number of exit to employment in month t divided by the size of the risk set at the beginning of the month. This graph is based on our main sample. We present similar graphs corresponding to the hazard of getting a long term job, and the hazard of exiting unemployment registry in Figure 3.A.1.

Our paper is the first to directly estimate the impact of PBD on job search at the individual level over time and therefore to directly test predictions of various search models. We connect data on unemployment benefits and very detailed data on job search at the individual level. We are able to measure job search by tracking applications that are made on the online search platform from the French public employment agency. This data source—previously used for research by Skandalis (2018)—allows us to measure unemployed workers' search on this channel over time, together with a rich set of individual variables on employment history and UI eligibility. Because we observe the information contained in the job openings that individuals apply to (in particular the posted wage for about half of openings), we also have new measures of the evolution of job seekers' selectivity over time. This allows us to document the evolution in job search intensity and selectivity over time, using within individual variation and thereby avoiding selection issues. In a second part of the analysis, we exploit a discontinuity in PBD around age 50 (Barbanchon et al., 2017) in order to directly estimate the impact of a PBD extension on job search.

The main finding of this paper is the existence of a spike in job search intensity around benefits exhaustion in individual behavior. Controlling for dynamic selection, we show that search intensity exhibits a steep increase during the 4 months leading to benefits exhaustion, and a strong *decrease* afterwards, such that it reaches the level it had prior to the increase about 4 months after benefits exhaustion. The behavioral response to benefits exhaustion amounts to a 40% increase in job search in the month of benefits exhaustion and a 25% increase in a larger time window around exhaustion. This magnitude suggests that search intensity is a key determinant of the roughly 40% spike in job finding rate around benefits exhaustion.

Second, we find some evidence that individuals might also slightly lower their reservation wages as they approach benefits exhaustion. Our different estimates suggest that this decrease is very small, around 0.5%, and typically not statistically significant. Additionally, our results confirm the negative duration dependence in job seekers' selectivity. Over an unemployment spell, individuals send fewer and fewer applications on the online search platform and to lower and lower hourly wages.

Finally, we find that an extension in unemployment benefits duration from 2 years to 3 years only starts affecting search intensity and selectivity in the months preceding benefits exhaustion and not from the start of the unemployment spell. This suggests that benefits that are due late in the unemployment spell do not affect job search behavior from the start. Differences in job search behavior between unemployed people with different PBD only emerge around the earliest benefits exhaustion.

Overall, these results together paint a comprehensive picture of the impact of PBD on re-employment outcomes: when benefits exhaustion approaches, applications increase and exits to employment increase. PBD has very small and typically insignificant effects on target wages. The effect of PBD on applications persists for a short time after benefits exhaustion, and then fades away. Longer PBD increases unemployment duration by postponing the spike in job applications and exits to employment.

Our first contribution is to shed light on the mechanisms behind the impact of UI on employment outcomes. We speak to the large empirical literature evaluating the impact of UI on unemployment and non-employment durations at the micro level, summarized in the meta-analysis of Schmierder and von Watcher (2016).[1]. In particular, a large debate has focused on the existence of a spike in the hazard of finding a job around benefits exhaustion: evidence in that sense can be found in Katz (1986), Katz and Meyer (1990a), Katz and Meyer (1990b), Fallick (1991), McCall (1997), van Ours and Vodopivec (2006) and DellaVigna et al. (2017) while the hazard of finding a job is found to be rather smooth in Bratberg and Vaage. (2000), Carling and Jansson. (1996) and Card et al. (2007b).[2] Our findings point towards a small effect of PBD on unemployment,

[1]Various evidence of a positive elasticity unemployment and non-employment durations with respect to PBD ranging between in 0.1 and 0.7 can be found Katz and Meyer (1990a), Card and Levine (2000), Lalive (2007), Lalive et al. (2006), Card et al. (2007a), van Ours and Vodopivec (2006), Schmieder et al. (2012) Johnston and Mas (2017), Barbanchon et al. (2017))

[2]We mention only here articles looking at the duration before re-employment as duration of unemployment is a measure exposed to classification problems, as pointed out by Card et al. (2007b).

driven by a large spike in the exit rate around benefits duration and provide direct evidence on the mechanisms. Additionally, our findings contribute to the discussion about the impact of UI on re-employment wages. We find that, throughout the unemployment spell, PBD hardly affects individuals' target wage. This is consistent with the very small estimates for the impact of UI on re-employment wage generally found in the literature (mixed evidence can be found in Lalive (2007), Card et al. (2007a), van Ours and Vodopivec (2006), Schmieder et al. (2016), Nekoei and Weber (2017)). Overall, we find that UI chiefly affects unemployment duration through job search intensity.

Our focus on search behavior also connects our paper to the empirical literature investigating the interaction between UI and various measures of search behavior. Krueger and Mueller (2010) and Krueger and Mueller (2011) report mixed evidence of a potential shift in search intensity around benefits exhaustion, but our large longitudinal data for job search behavior around benefits exhaustion offer a better setting to investigate this question. Other recent contributions have established the existence of a negative impact of UI on search intensity at the macro level, using surveys or online search data (Marinescu (2017), Lichter (2017), Fradkin and Baker (2017)). In an apparent contradiction with the evidence of an impact of UI on search behavior, Barbanchon et al. (2017) establish that PBD has no effect on job seekers' selectivity at the start of their unemployment spell, using French administrative data in a setting very similar to ours. Our paper allows to reconcile this last finding with the literature: both in terms of search intensity and selectivity, job seekers do not react to PBD from the start of the unemployment spell, but only around benefits exhaustion.

Moreover, our empirical findings allow us to discriminate among various search models suggested in the literature. By directly observing individual search behavior, we can back up the implicit assumption that search intensity and re-employment hazard are correlated over the unemployment spell, which is crucial for the empirical motivation of a large strand of search models. This allows us to rule out that storable job offers (Boone and van Ours (2012)) or recalls (Katz and Meyer (1990c)) are the only causes of the spike in benefits exhaustion. As pointed in the literature, a temporary shift in search intensity around benefits exhaustion is not consistent with the standard search model with unemployment insurance (Mortensen (1977)): if the increase in search in the periods leading up to benefits exhaustion is consistent, the subsequent decrease in search intensity for job seekers who remain unemployed represents a puzzle. Our finding of a spike in search intensity within individual advocates for models including a shift in incentives or perceptions at benefits exhaustion. Such a feature can be found in existing models, such as worker learning (Chetty (2003)), consumption commitments (Chetty and Szeidl (2016)), habit formation (Constantinides (1990), Campbell and Cochrane (1999)), and reference dependence (DellaVigna et al. (2017)). A worker may learn over time that finding jobs is harder than expected, and this learning may happen around benefits exhaustion. A worker with committed consumption would increase search effort to avoid paying a fixed cost of adjustment; but if she fails, she would decrease search once she has paid the cost. A worker with loss aversion relative to recent income (the reference point) could search hard around benefits exhaustion, but then reduce search effort over time as she gets used to lower income. This last model was recently developed in DellaVigna et al. (2017) precisely to address the puzzle of the spike in the hazard of finding a job around benefits exhaustion. The authors however cannot directly observe search intensity, assume away wage effects, and have to calibrate different models based on employment outcomes rather than evidence on job search behavior.

Finally, we contribute to the understanding of the dynamics of moral hazard over the unemployment spell. Our results show that unemployment insurance indeed affects unemployment duration through job seekers' behavior— "moral hazard". Beyond the discussion on the generosity of UI, recent papers have highlighted the importance of the time profile of benefits during an unemployment spell. DellaVigna et al. (2017) show that discrete drops in unemployment benefits generate spikes in exit rates. By exploring how various UI schemes affect unemployment duration, Kolsrud et al. (2018) show that unemployment benefits due late after the start of the unemployment spell have less impact on search incentives than benefits due early during the unemployment spell. Our findings imply that the moral hazard associated with the extension of unemployment insurance is focused around benefits exhaustion.

The paper is organized as follows. Section 2 presents the institutional setting and data. Section 3 describes the empirical approach. Section 4 presents the results concerning the shift in search intensity around benefits exhaustion, while Section 5 focuses on the selectivity margin. In Section 6, we use a regression discontinuity design (RDD) to directly estimate the dynamic impact of a benefits extension on search outcomes. Section 7 discusses the mechanisms behind the impact of unemployment insurance on employment outcomes at the light of our results. Finally, Section 8 concludes.

3.2 Institutional setting and data

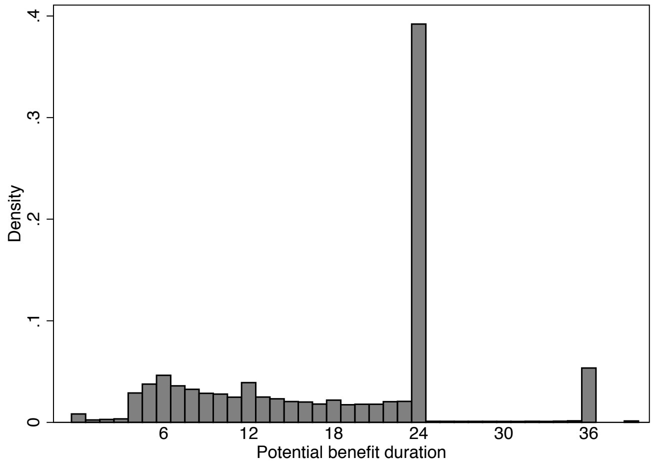
3.2.1 Institutional setting

UI eligibility rules

In France, workers who lose their job can claim unemployment benefits after registering at the employment agency. When the claim has been processed, workers are informed about the date of the start of their benefits, as well as the amount and potential duration. Workers are eligible to UI if they have worked more than 4 months during the 28 months preceding job loss (36 for workers who become unemployed after 50). The potential benefits duration (PBD) depends on the number of days worked during this reference period. During our study period, the PBD cannot exceed 2 years for workers who become unemployed below 50 years old and 3 years for workers who become unemployed after. The amount of benefits depends on the wages perceived during the last 12 months, and the replacement rate is 57% in most cases (with a minimal and maximal cap). The replacement rate is therefore lower than in most European countries but the PBD is longer. The average unemployment duration is 14 months in 2016.

As illustrated in Figure 3.2, the most frequent PBD are 2 and 3 years, corresponding to the upper bounds below and above 50 years old. Besides, PBD are dispersed between 4 and 24 months, with some over-representation of PBD at 6, 12 and (to a lesser extend) 18 months. It most likely comes from the fact that these round periods correspond to common contract durations for limited-term contracts in France.

Figure 3.2: Density of PBD among job seekers eligible to benefits



Notes: This figure displays density of PBD in our main sample.

Search activity on the public platform

The unemployment agency administers the most popular search platform in France. In 2013, the platform introduced the possibility for job seekers to apply online for certain job offers, so that employer receive standardized applications and spend less time selecting among them. The online applications have been tracked on the information system and can be merged with other sources of administrative data collected by the unemployment agency. A new tool was progressively introduced to replace online applications in 2017. As a consequence, the aggregate number of applications diminished and tracking this search channel is more and more difficult. We can see a decrease in the aggregate number of applications since 2017.

In Table 3.A.1, we document the selection of unemployed workers who are using the online search platform, using data on all unemployment spells starting in 2014. We can detect an online application for about one fifth of these spells, which suggests that this tool is widely used. But these unemployment spell are specific in several dimensions: they correspond to job seekers who are more likely to be women (55% versus 49% in total), tend to be younger (on average 31 years old versus 34), more educated (27 % of them have a higher education degree versus 25%), tend to be white collars (72% versus 66%), tend to stay unemployed longer and have unemployment spells more frequently.

3.2.2 Data

Measures of search behavior

Search intensity The first outcome variable that we analyze is the number of applications sent each month of the unemployment spell. This provides a measure of search intensity. This measure only covers one search channel, and therefore its scope is limited in comparison with survey data about the time spent searching for a job. However, it allows us to have a non-declarative measure of search over time for a very large sample without attrition, and is therefore particularly well suited for the analysis of search behavior over time and around benefits exhaustion. We discuss possible consequences of channel substitution for our results in the last part of this paper.

Selectivity in search The online application data set contains a wide range of information on the job offers: job category, type of contract, weekly hours of work, establishment identifier, employer's requirement in terms of experience, qualification and diploma, and posted hourly wage. However, the wage is missing for about 50% of the vacancies in the sample, because the information is not disclosed in the job offer. In order to make use of all the information contained in the offer, and to assess the quality of the job even when no wage is posted, we use a linear prediction of the hourly wage based on all the observable characteristics of the job. Table 3.A.2 shows the linear regression on the characteristics of vacancies in our study sample. We see that observable characteristics predict 50% of the posted wage (the R^2 is 0.49). In particular, the hourly wage is positively correlated with the length of the contract, the size of the establishment, as well as the stated requirements: professional experience, education and qualification level. Figure 3.A.2 displays the predicted wage and the actual wage in our individual panel sample. The two variables have a very similar distribution. We see that the predicted wage is more dispersed that the hourly wage, as the latter has a lower bound at minimum wage. In our empirical analysis, we will use the average predicted wage from job offers to which job seekers apply as a measure of their selectivity. In robustness checks, we will also use variables highly correlated with wage as a proxy.

Sample construction

We select all individuals who have made at least one online application tracked in the information system. Within this sample, we select all individuals who register as unemployed in 2013-2017 and are eligible to UI. We exploit online applications sent during the period January 2013 to December 2017. To measure the search activity, we track the online applications that individuals make while unemployed.

While they are eligible to unemployment benefits, job seekers can experience interruption periods during which they do not perceive their unemployment benefits, because they are receiving a training, working for a few hours or having holidays for instance. When the interruption ends, job seekers are still eligible to the same remaining UI as they had before the interruption (i.e. their entitled UI was not consumed during the period). In the main analysis, we exclude these interruption periods from the unemployment spell and append periods when job seekers actually receive their UI together. That way, for job seekers who exhaust their benefits, the time of the spell in which benefits exhaust in our data coincide with the PBD.

We ultimately create two distinct data sets:

(i). Individual panel: in order to track within individual search, we create a panel at the individual and month of the unemployment spell level.

(ii). Cross section for individuals entering unemployment around 50: to exploit the discontinuity in eligibility at age 50, we focus on individuals, who start an unemployment spell between January 2013 and September 2017[3] while they are between 45 and 50.

The characteristics of the samples are presented in Table 3.1. The last part of the Table presents statistics for our main outcome variables: the average number of applications sent, and the average predicted hourly wage at different moment of the unemployment spell. We can note that the number of online applications made on the platform is very low on average, around 3.6 per

[3] A reform was introduced in November 2017 postponing the threshold from 50 to 53 and 55 years.

unemployment spell, with a lot of heterogeneity among job seekers. One should keep in mind that the online applications on the platform constitute only one search channel among many others.

Table 3.1: Descriptive statistics of the two study samples

	Main sample		**Sample RDD**	
	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)
Female	0.5817	0.4933	0.5621	0.4961
Age	33.4424	10.8342	49.5168	3.1189
Single	0.5828	0.4931	0.3867	0.4870
Look for full time job	0.9237	0.2655	0.8719	0.3342
Education				
No diploma	0.0147	0.1204	0.0354	0.1847
Middle school	0.0659	0.2481	0.0985	0.2980
Vocational high school	0.3495	0.4768	0.4342	0.4957
General high school	0.2612	0.4393	0.1989	0.3992
Higher education	0.3078	0.4616	0.2322	0.4223
Qualification				
Blue collar, low skill	0.0475	0.2127	0.0396	0.1951
Blue collar, high skill	0.0870	0.2819	0.0961	0.2947
White collar, low skill	0.1640	0.3703	0.1305	0.3369
White collar, high skill	0.5562	0.4968	0.5218	0.4995
Intermediary	0.0966	0.2954	0.1154	0.3195
Management	0.0474	0.2126	0.0954	0.2938
Unemployment insurance				
PBD	551.5035	263.0837	804.2864	270.4311
Reference monthly wage	1588.9746	699.4774	2028.6124	992.8178
Unemployment/non employment durations				
Duration of benefits	354.5463	239.7988	514.5143	299.1047
Duration of unemployment spell	688.9858	433.4882	880.2187	457.9341
Duration before 1st job	361.6204	320.9317	447.4564	354.7124
Job applications				
Number during first 2 months	0.6265	1.4297	0.5107	1.3597
Hourly wage during first 2 months	11.9058	2.2285	12.2988	2.5587
Number during 1st year	2.4761	4.0634	2.2509	4.3008
Hourly wage during 1st year	11.8300	2.1222	12.1823	2.4462
Number during U spell	3.6331	6.1659	3.7842	6.7024
Hourly wage during U spell	11.7826	2.0674	12.0877	2.3794
Number of spells	807,853		59,037	

3.3 Empirical strategy

3.3.1 Search over the unemployment spell

Our longitudinal data of job search allows us to identify the evolution of search over the unemployment spell by looking at variation within spell.

Panel with spell fixed effects

We first study how individuals shift their search over time by estimating the following fully saturated model for each PBD group separately. The first month of unemployment is taken as the reference period:

$$Y_{i,t} = \sum_{t=2}^{N} \tau_{t,B(i)} + \mu_i + \epsilon_{i,t} \tag{3.1}$$

With:

- i and t referring to spell and time of the unemployment spell. It should be noted that one individual can enter several times in our sample if he has several spell episodes, and each will represent one distinct observation, which is why the level of observation is the spell.
- $Y_{i,t}$: Search outcomes
- τ_t: Period of the spell fixed effects
- μ_i: Spell fixed effects

Second, we focus on the behavior around benefits exhaustion, by estimating the following fully saturated model. The period corresponding to 6 months before benefits exhaustion is taken as a reference:

$$Y_{i,t} = \sum_{\substack{k=-11 \\ k \neq -6}}^{11} \kappa_k D^k_{B(i),t} + \mu_i + \epsilon_{i,t} \tag{3.2}$$

where variables are defined as in the first model, and $D^k_{B(i),t} = \mathbb{1}\{t = B(i) + k\}$ is an indicator for the period t being k months before/after the period of benefits exhaustion for that spell i, B_i.

We cluster standard errors at the spell level (Bertrand et al. (2004)).

The inclusion of the spell fixed effects in the two models allows us to control for dynamic selection. The coefficients $\tau_{t,B(i)}$ and κ_k can hence be interpreted as shifts in individual behavior over time and around benefits exhaustion. We systematically compare this estimate of the corresponding shift in aggregate search obtained when we do not controlling for dynamic selection, estimating the same models without spell fixed effects μ_i.

For the estimation of the second model, we restrict our sample to individuals who have a PBD between 12 and 24 months, in order to be potentially able to observe up to 11 months before and after their benefits exhaustion (we chose to present 11 periods around in order to include in our sample people with a PBD of 12 months as they are many) and who are still unemployed 6 months before their benefits exhaustion.

For both models, we also reproduce our results in robustness checks when we restrict our sample to individuals who remain unemployed at least 12 months after their benefits exhaustion. This brings an additional and more restrictive control for dynamic selection: for example, people who exit earlier may have levels of search effort that decline more slowly over time.

Differences-in-differences approach

In order to quantify the shift in search intensity around benefits exhaustion, we can exploit the fact that we observe trajectories of job seekers with different PBD. This allows us to disentangle between the impact of the time of the unemployment spell and of the time to benefits exhaustion. We include individual fixed effects or PBD fixed effects to control for the average difference in search between job seekers eligible to different PBD.

This strategy relies on the assumption that the evolution of job search along the unemployment spell among those different groups is parallel outside of benefits exhaustion. In our setting, the main concern is not that there is a confounding factor simultaneous to benefits exhaustion, but rather that differences in the search pattern over the unemployment spell outside of benefits exhaustion across job seekers with different PBD is not constant. A simple additive model would not correct them and our estimates could be biased.

We estimate the two following fully saturated models:

$$Y_{i,t} = \sum_{\substack{k=-11 \\ k\neq -6}}^{11} \beta_k D^k_{B(i),t} + \nu_t + \phi_{B(i)} + \epsilon_{i,t} \tag{3.3}$$

$$Y_{i,t} = \sum_{\substack{i=-11 \\ i\neq -6}}^{11} \beta_k D^k_{B(i),t} + \nu_t + \mu_i + \epsilon_{i,t} \tag{3.4}$$

With ν_t, $\phi_{B(i)}$, μ_i respectively time of the spell, individual and PBD group fixed effects.

In the first model, we control for differences across individuals with different PBD by including $\phi_{B(i)}$. However, we do not include spell fixed effects and therefore do not correct for dynamic selection. The estimates β_k in the first model should be interpreted as the shift in average search outcomes among individuals who are still unemployed at each point of their unemployment spell, while controlling for average differences in search effort across individuals with different PBD. In the second model, we include spell fixed effects and the estimates β_k should be interpreted as the shift in individual behavior around benefits exhaustion.

We estimate this model on a subsample of individuals who have a PBD between 12 and 24 months and who are still unemployed 6 months before their benefits exhaustion.

Fitted polynomial approach

Because the evolution of search outcomes over the unemployment spell does not seem perfectly parallel across different groups of job seekers outside of benefits exhaustion, the search pattern of other groups of job seekers might not be the ideal counter-factual for search behavior in the absence of a spike. This might slightly bias our measure of a spike around benefits exhaustion. Therefore, we also implement another strategy to measure the shift in search outcomes around benefits exhaustion.

As an alternative, we use the pattern of search over the unemployment spell for different PBD group separately to infer what would be the search behavior in the absence of a spike. it corresponds to the following model:

$$Y_{i,t} = \sum_{k=-4}^{4} \beta_k D^k_{B(i),t} + f_{B(i)}(t) + \epsilon_{i,t} \tag{3.5}$$

In order to recover the parameters β_k, we fit a PBD-specific polynomial function of time of the unemployment spell outside of the benefits exhaustion period $f_{B(i)}(t)$. The period of benefits exhaustion is defined as $[T-4;T+4]$. The residuals from the prediction for the period $[T-4;T+4]$ provide an estimation of the shift around benefits exhaustion.

In order to recover estimates for the shift in individual behavior, we do the same exercise for individuals who remain unemployed at least 12 months after their benefits exhaustion.

3.3.2 RDD strategy

In a second part of the empirical analysis, we implement a RDD to investigate the impact of the extension of UI on search behavior.

Discontinuity in PBD schedule We exploit a discontinuity in PBD depending on the age at the time of job separation:

- After 50 years old: Workers are entitled to as many days of benefits as days worked within the last 36 months before job separation, the maximum PBD is thus de facto 36 months.
- Before 50 years old: Workers are eligible to as many days of benefits as days worked within the last 28 months before job separation, with a maximum PBD fixed by UI rules to 24 months.

Besides this difference in PBD schedule, UI rules are the same for claimants above and below 50 years old.

The same discontinuity has been exploited in Barbanchon et al. (2017) in order to estimate the impact of PBD on reservation wage at the start of the unemployment spell. The authors have documented the existence of some selection around the 50 years old threshold, suggesting that job seekers can to some extend manipulate the moment at which they become unemployed. Like the authors, we therefore trim our sample to exclude job seekers who become eligible just around 50 (we present the results with various trimming procedures). It can be noted that this selection would probably create a positive bias in the estimated impact of PBD on search intensity (resp. negative on selectivity), if we consider that job seekers who manipulate their unemployment start in order to be eligible to longer unemployment benefits are probably more subject to moral hazard.

Treatment variables Senior workers tend to be eligible for longer benefits, depending on their work history over the last 3 years. In Figure 3.A.3, we estimate that PBD is on average around 25% higher for claimants above 50 than below 50 in our sample, similar as the estimate found on a larger sample by Barbanchon et al. (2017). We show that claimants above 50 have an increase in their probability to get a 2 years PBD instead of 3 between 55-70% depending on the trimming procedure.

In order to estimate the impact of a benefits extension on job search, we estimate a fuzzy RDD and take as a treatment variable an indicator for receiving 3 years of unemployment benefits. The probability or receiving 3 years UB is zero for job seekers below 50 while it is high (55-70%) for senior job seekers. As senior workers who are eligible to 3 years of benefits have necessarily worked continuously during the 3 years preceding their job loss, they would have been eligible to 2 years of benefits if they were below the 50 years old cutoff. The estimate we obtain in this fuzzy RD can be interpreted as the impact on job search of getting 3 years of unemployment benefits instead of 2. This specification is particularly relevant when we study the dynamic impact of PBD on search outcomes over the unemployment spell. Additionally, in some specifications, in order to have estimates comparable with the literature, we also run a fuzzy RD using the log PBD as a treatment variable. The estimates we obtain in that case can be interpreted as the impact of being having a 1% longer PBD.

Timing of search outcomes We first estimate in a fuzzy RD the impact of longer PBD on search outcomes measured at the start of the unemployment spell, and on non-employment duration. In the second part of the analysis, we investigate whether longer benefits affect search behavior over the unemployment spell. For this purpose, we separately estimate a fuzzy RDD using as an outcome variable search behavior measured at a different times of the unemployment spell, in the sample of individuals who are still unemployed. We estimate the parameters δ_t in the following model, where we instrument having a 3 years PBD ($\mathbb{1}\{PBD = 3\}$) with the dummy $\mathbb{1}(age_i \geq 50)$ indicating that the claimant is over 50 years old (at job separation):

$$Y_{i,t} = \alpha + \delta_t \mathbb{1}\{PBD = 3\} + P_0(age_i - 50) * \mathbb{1}(age_i < 50) + P_1(age_i - 50) * \mathbb{1}(age_i \geq 50) + \epsilon_{i,t} \quad (3.6)$$

withe $P_0(.)$ and $P_1(.)$ two polynomials whose coefficients are estimated (without constant). The objective is in particular to detect a potential spike in aggregate search intensity around benefits exhaustion, which would translate into a negative δ_t for t around 24 months. However, because of dynamic selection, we cannot interpret the estimated δ_t as the impact of PBD on individual behavior when t is far away from 0.

3.4 Search intensity around benefits exhaustion

We exploit the longitudinal nature of our data and explore the profile of search intensity over the unemployment spell.

3.4.1 Search intensity over the unemployment spell

In order to illustrate how search intensity shifts close to benefits exhaustion, Figure 3.3 presents the evolution of search intensity along the unemployment spell for job seekers with different PBD in Panel A, while Panel B shows search intensity as a function of time relative to benefits exhaustion. All graphs clearly show a spike in search intensity around benefits exhaustion.

The first specification includes PBD fixed effects, and therefore controls for differences in job applications level across job seekers eligible to different PBD. For each different PBD, the temporary increase in search happens approximately in the 10 months surrounding the date of potential exhaustion, which makes it very likely that this shift is caused by benefits exhaustion and not another change in the environment of job seekers over the unemployment spell. Given how closely the maximum search intensity is aligned with the month of benefits expiration, it is hard to believe that there is another factor that varies in the same way across individuals with different potential benefits durations.

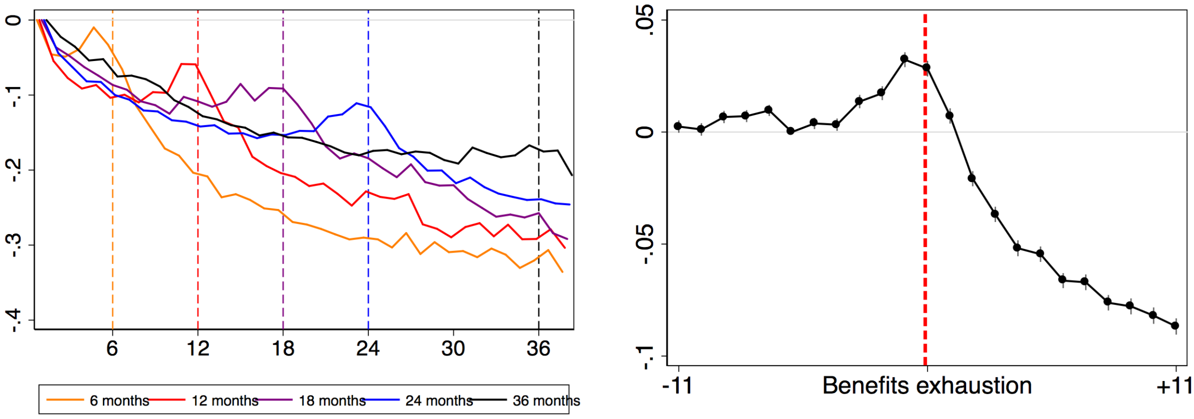
Apart from the spike around benefits exhaustion, search intensity exhibits the same decreasing profile for job seekers with different PBD, although the decrease in search intensity in steeper for shorter PBD. In Panel B, the graph pools observations of all job seekers with PBD between 12 and 24 months and exhibits the profile of search intensity between 12 months before benefits exhaustion to 12 months after. We can see that search intensity is first stable, and then increases from about 6 months before benefits exhaustion and until UB exhaustion. The 5 months following benefits exhaustion correspond to a very steep decrease. About 5 months after benefits exhaustion, the number of applications declines below levels of search at period zero. This finding is at odds with the standard search model, which predicts stable and high job search effort after benefits exhaustion.

In order to explore the causes of this spike in aggregate job search, specification (2) and (3) control in various ways for dynamic selection. Although it is unlikely that selection would explain the increase in search intensity in the periods leading up to UB exhaustion, it could have explained the subsequent decrease. However, when including spell fixed effects in specification (2), the spikes

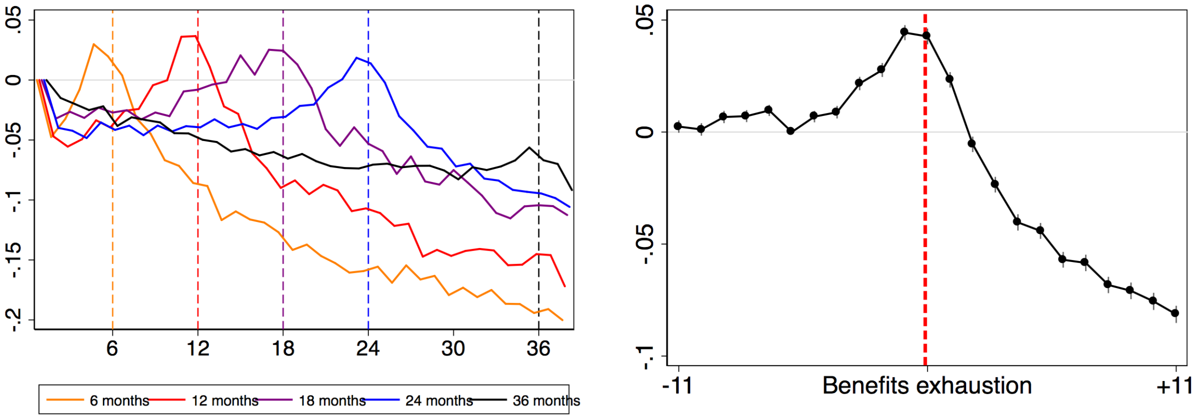
Figure 3.3: Search intensity over the unemployment spell and relative to benefits exhaustion

PANEL A: Search intensity over the unemployment spell **PANEL B: Search intensity over time relative to benefits exhaustion**

(1) With PBD fixed effects, for all spells



(2) With spell fixed effects, for all spells



Notes: In Panel A, the graphs present results from regressions of search intensity on month of unemployment spell dummies with various specification. The x-axis denotes time of the unemployment spell in month. The graphs display separately the search pattern for job seekers who have various duration of PBD (6, 12, 18, 24 and 36 months). The 1st month of the spell is used as the reference period. In Panel B, the graphs present results from regressions of search intensity on time to benefits exhaustion. The x-axis denotes time relative to benefits exhaustion in months. Observations from job seekers with various PBD between 12 and 24 months are pooled together. The vertical lines denote 90% confidence intervals based on standard errors clustered at the spell level. The period 6 months before benefits exhaustion is used as the reference period. Search intensity is measured by the monthly number of applications made on the online search platform.

are even more visible. This means that the spike in search is not caused by the dynamic selection, but by individual behavior. Besides, the overall decreasing trend is attenuated, which indicates part of the aggregate decrease in monthly applications come from compositional changes. This is consistent with the idea that job seekers searching more intensively exit unemployment faster. Specification (2) controls for changes in the composition of job seekers with a different average level of search intensity.

In order to also control for changes in the composition in terms of job seeker's search dynamic, we additionally exclude spells which are completed before the end of the year following benefits exhaustion in Figure 3.B.1. This sample restriction further strengthens the result.

3.4.2 Size of the spike

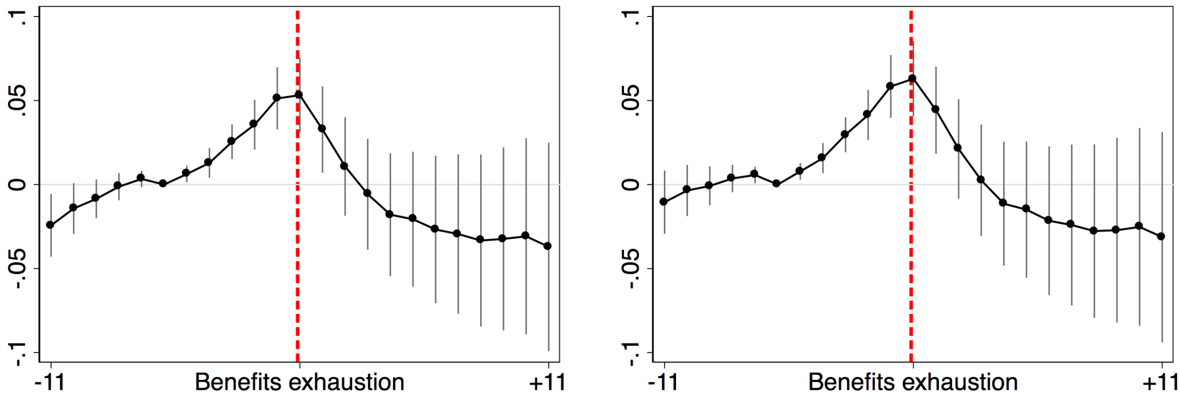
Differences-in-differences approach

In order to quantify the size of the spike around benefits exhaustion, we need to refer to a counterfactual behavior in the absence of benefits exhaustion. Under the assumption that PBD does not affect search intensity dynamic outside of the exhaustion period, one can use the behavior of job seekers eligible to other durations as a counterfactual. It should be noted that this assumption implies that search patterns should be parallel outside of benefits exhaustion, which does not seem perfectly verified from Figure 3.3.

Figure 3.4: Search intensity relative to benefits exhaustion, controlling for time of spell

(1) With PBD fixed effects, for all spells

(2) With spell fixed effects, for all spells



Notes: The graphs present results from regressions of search intensity on time to benefits exhaustion. The x-axis denotes time of the unemployment spell in month. Observations from job seekers with various PBD between 6 and 24 months are pooled together. The period 6 months before benefits exhaustion is used as the reference period. Search intensity is measured by the monthly number of applications made on the online search platform. The vertical lines denote 90% confidence intervals based on standard errors clustered at the spell level.

We present the results from this DiD approach in Figure 3.4. The graphs present the same specifications as in the Panel B from Figure 3.3 but we additionally include month of the spell fixed effects. These controls allows to disentangle the spike around benefits exhaustion from other factors affecting search along the unemployment spell, and in particular with the negative trend in search intensity. We see in Figure 3.4 that their inclusion makes the pattern of search look more flat outside of benefits exhaustion.

In specification (1), we include PBD fixed effects, which control for differences in level across PBD groups, but not for dynamic selection. Estimates reported in Table 3.2 indicate that the increase in search intensity amounts to 0.068 additional applications at benefits exhaustion and about 0.035 around benefits exhaustion—when taking 6 months before benefits exhaustion as a reference period. Comparing this increase to the outcome mean 6 months before benefits exhaustion, these coefficients correspond to a relative increase in search intensity by approximately 36%

Table 3.2: Shift in search intensity around benefits exhaustion

Outcome variable (Mean at $T=-6$: 0.1888)	Monthly number of applications			
	Aggregate		Within individual	
	(1)	(2)	(3)	(4)
$T=-4$	0.015***		0.020***	
	(0.003)		(0.003)	
$T=-3$	0.033***		0.041***	
	(0.003)		(0.003)	
$T=-2$	0.046***		0.056***	
	(0.003)		(0.003)	
$T=-1$	0.064***		0.076***	
	(0.003)		(0.003)	
$T=0$	0.068***		0.083***	
	(0.003)		(0.004)	
$T=1$	0.045***		0.064***	
	(0.003)		(0.003)	
$T=2$	0.023***		0.041***	
	(0.004)		(0.004)	
$T=3$	0.006*		0.022***	
	(0.004)		(0.004)	
$T=4$	-0.011***		0.003	
	(0.004)		(0.004)	
$[T-4;T+4]$		0.035***		0.046***
		(0.002)		(0.002)
All other T expect $T=-6$	Yes	Yes	Yes	Yes
Month of U spell FE	Yes	Yes	Yes	Yes
PBD group FE	Yes	Yes	No	No
Spell FE	Yes	Yes	No	No
No. of Obs.	4,409,314	4,409,314	4,409,314	4,409,314

Notes: Coefficients in this table are obtained in regressions of search intensity on time to benefits exhaustion dummies, time of the spell dummies as well as PBD group FE in columns (1) and (3) and spell FE in columns (2) and (4). This empirical strategy exploits the fact that different job seekers are "treated" by benefits exhaustion at different moment of their unemployment spell. SE are clustered at the spell level.

at benefits exhaustion and 17% around benefits exhaustion.

In specification (2), we include spell fixed effects in order to control for dynamic selection. The increase in search intensity within individual amounts to respectively 0.083 at benefits exhaustion and 0.046 around (Table 3.2, column (3) and (4)). This represents approximately a 38% increase at benefits exhaustion and a 20% increase around benefits exhaustion. These results show that spikes in search intensity are even larger within individual. Dynamic selection, if anything, rather attenuates the effect from individual reaction. This suggests that job seekers who are responding the most to benefits exhaustion in their individual search effort are more likely to exit unemployment around benefits exhaustion.

It should be noted that the estimated search pattern is not flat outside of benefits exhaustion in either of the two specifications. This suggests that the search profile of job seekers with another PBD—even when allowing for differences in the average level—does not offer a perfectly suited counterfactual. As a consequence, the estimates could be biased. We will therefore turn to another complementary approach.

Fitted polynomial approach

Because of the limitations of the DiD approach, we turn to another approach based on the extrapolation of the pattern of search over time outside of the benefits exhaustion period. We consider search as a polynomial function of time of the unemployment spell, and make the fit outside of the benefits exhaustion period, defined as going from 4 months before benefits exhaustion to 4 months after. This approach relies on the assumption that the dynamic of search over the unemployment spell is not affected by PBD outside of the benefits exhaustion period.

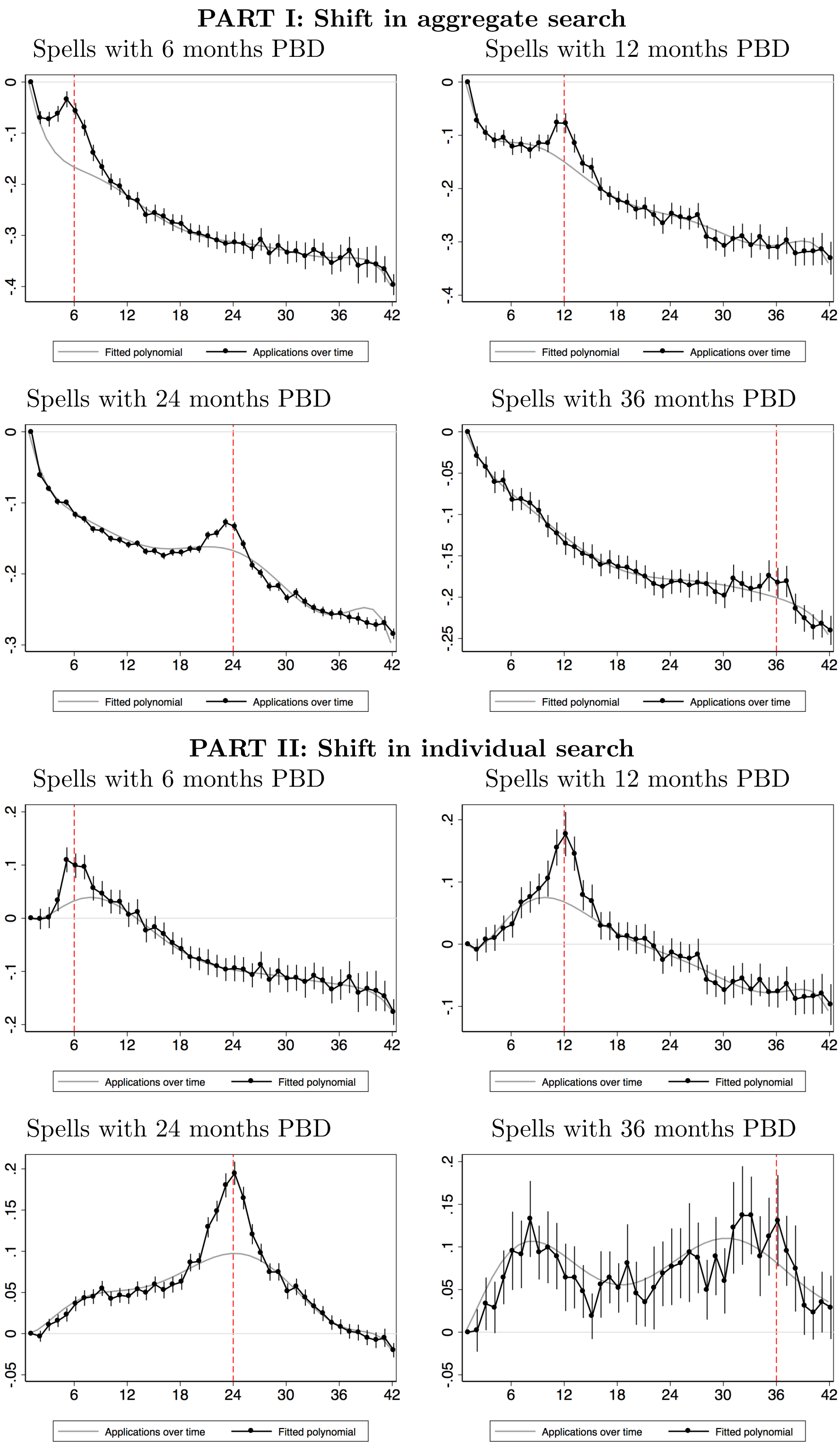
Table 3.3: Shift in aggregate search around benefits exhaustion

PART I: Shift in aggregate search						
Sample: PBD (month)	All	6	12	18	24	36
	Absolute increase in applications:					
At exhaustion	0.061***	0.110***	0.075***	0.048***	0.039***	0.021**
	(0.003)	(0.008)	(0.009)	(0.012)	(0.003)	(0.009)
Around exhaustion	0.037***	0.073***	0.038***	0.029***	0.022***	0.012**
	(0.001)	(0.004)	(0.004)	(0.006)	(0.002)	(0.005)
No. of Obs.	4,378,911	356,882	324,304	198,091	3,147,253	35,2381
	Relative increase in applications:					
At exhaustion	0.294***	0.411***	0.322***	0.252***	0.240***	0.254**
	(0.015)	(0.036)	(0.039)	(0.062)	(0.021)	(0.106)
Around exhaustion	0.171***	0.265***	0.166***	0.150***	0.133***	0.139**
	(0.008)	(0.019)	(0.019)	(0.033)	(0.011)	(0.058)
No. of Obs.	4,378,911	356,882	324,304	198,091	3,147,253	35,2381
PART II: Shift in individual search						
Sample: PBD (month)	All	6	12	18	24	36
	Absolute increase in applications:					
At exhaustion	0.091***	0.066***	0.105***	0.068***	0.106***	0.043
	(0.006)	(0.012)	(0.021)	(0.024)	(0.008)	(0.030)
Around exhaustion	0.049***	0.036***	0.049***	0.035***	0.060***	0.021
	(0.003)	(0.006)	(0.008)	(0.011)	(0.004)	(0.013)
No. of Obs.	768,366	168,719	106,984	47,102	428,719	16,842
	Relative increase in applications:					
At exhaustion	0.491***	0.268***	0.471***	0.368***	0.643***	0.392
	(0.034)	(0.053)	(0.096)	(0.134)	(0.051)	(0.283)
Around exhaustion	0.269***	0.146***	0.221***	0.188***	0.367***	0.193
	(0.017)	(0.031)	(0.040)	(0.064)	(0.026)	(0.135)
No. of Obs.	768,366	168,719	106,984	47,102	428,719	16,842

Notes: The table presents the estimated value of residuals at benefits exhaustion ($T = 0$) or around ($[T - 4; T + 4]$). Residuals come from the predicted search over the unemployment spell, based on a polynomial function of time of spell. We use a polynomial of degree 7. The fit is made outside of the benefits exhaustion period, which is defined as $[T - 4; T + 4]$. SE are clustered at the spell level.

In order to both have an estimate of the spike in aggregate search intensity, and of the spike in search intensity within individual, we repeat the same exercise for the pattern of aggregate search over the unemployment spell and within individual variation in job search over the spell. The fitting procedure is presented in Figure 3.5. The residuals from the prediction around the benefits exhaustion period give an estimate to the excess search intensity around benefits exhaustion. We report these estimates in Table 3.3.

Figure 3.5: Actual and predicted search intensity over unemployment spell, for different PBD



Notes: The x-axis denotes time of the unemployment spell in month. The prediction is based on a fitted polynomial function of time of spell. We use a polynomial of degree 7. The fit is made outside of the benefits exhaustion period, which is defined as $[T-4; T+4]$. The vertical lines denote 90% confidence intervals based on standard errors clustered at the spell level.

In Table 3.3, the first part reports estimates of the spike in aggregate search intensity. The first two lines present respectively the estimates of the increase during the month of benefits exhaustion, and during the period $[T-4; T+4]$. The first column presents the results for all job seekers, while column (2) to (6) present the results separately for job seekers eligible to different PBD. On average, the increase in search intensity amounts to 0.061 additional applications at exhaustion and to 0.037 in the broader period of $[T-4; T+4]$. These correspond respectively to a 29 % increase in search intensity at benefits exhaustion and 17% around benefits exhaustion. This increase is even higher for job seekers eligible to a short PBD of 6 months (41% and 26% respectively).

The second part of Table 3.3 presents the estimates corresponding to the increase in search within individual, when applying the most exhaustive controls for dynamic selection. We can see that the increase in individual search intensity is even larger: it represents 49%(i.e., 0.091 application) at exhaustion and 27% (i.e., 0.049 applications) in the broader time window around exhaustion. This confirms the conclusions from the visual examination of Figure 3.3. It can be noted that these magnitudes are very close to the one obtained in the DiD approach.

3.5 Target wage around benefits exhaustion

The second channel through which UI affects employment outcome in the literature is search selectivity. In this section, we therefore explore the dynamic of selectivity in search over the unemployment spell and in particular around benefits exhaustion, following the same steps as in the analysis of the dynamic of search intensity presented in previous section. Observing the (predicted) wage of jobs job seekers apply to over their unemployment spell allow us to have a longitudinal measure of selectivity in search.

3.5.1 Target wage over the unemployment spell

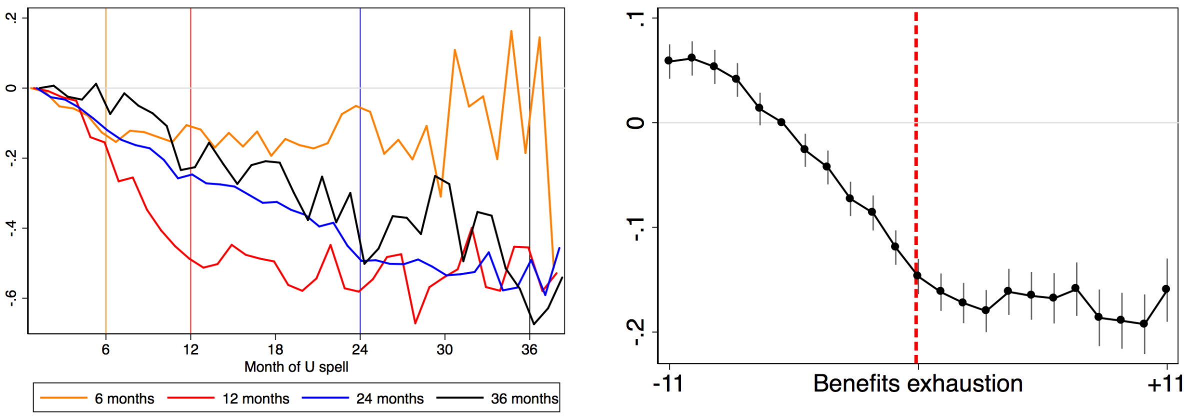
In Figure 3.6, we present the pattern of target wage over the unemployment spell. In Panel A, we highlight the shift around benefits exhaustion by presenting separately this pattern for job seekers with different PBD, while in Panel B, we pool the observations from spells associated with PBD between 6 and 24 months and present the target wage as a function of time relative to benefits exhaustion. The first specification shows the aggregate evolution in target wage controlling for differences in level across PBD groups, but not for dynamic selection. We observe a clear negative trend in target wage; which means that job seekers tend to apply to job with a lower posted wage as unemployment duration increases. However, after benefits exhaustion, the target wage seems to remain at a relatively constant level. This pattern was not visible in search intensity. Additionally some graphs suggest the existence of a small acceleration in the decline of target wage around benefits exhaustion, but the high variability of this measure does not allow for a clear conclusion.

In specification (2), we control for dynamic selection. The decreasing trend in target wage is attenuated. This means that job seekers who tend to have a higher reservation wage exit unemployment faster, which suggests that they have better labor market characteristics. However the decreasing trend in target wage is still visible, which shows that job seekers apply to jobs paying lower and lower wages over time. This finding is consistent with Krueger and Mueller (2011) results of based on longitudinal survey data. Benefits exhaustion still appears to be followed by a

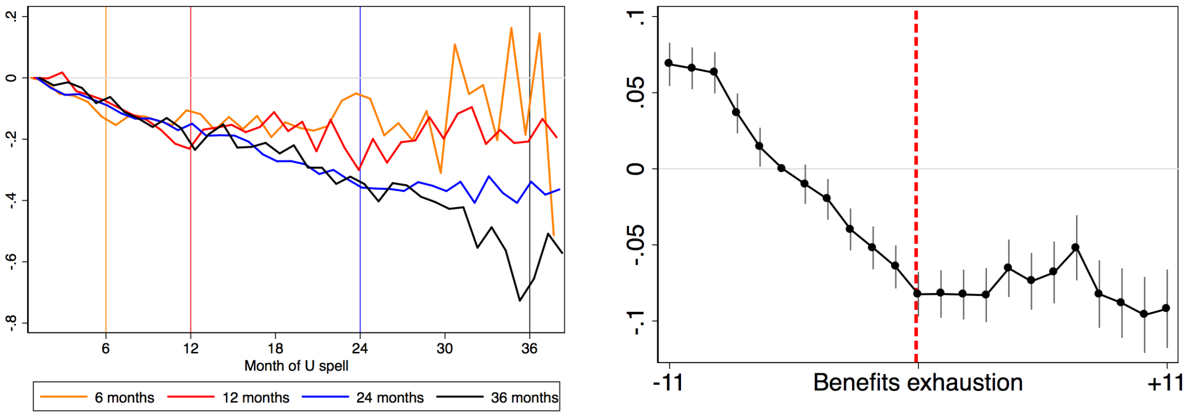
Figure 3.6: Target wage over the unemployment spell and relative to benefits exhaustion

PANEL A: Target wage over the unemployment spell **PANEL B: Target wage over time relative to benefits exhaustion**

(1) With PBD fixed effects, for all spells



(2) With spell fixed effects, for all spells



Notes: In Panel A, the graphs present results from regressions of target wage on month of unemployment spell dummies with various specification. The x-axis denotes time of the unemployment spell in month. The graphs display separately the target wage pattern for job seekers who have various duration of PBD (6, 12, 18, 24 and 36 months). The 1st month of the spell is used as the reference period. In Panel B, the graphs present results from regressions of target wage on time to benefits exhaustion. The x-axis denotes time to benefits exhaustion in month. Observations from job seekers with various PBD between 6 and 24 months are pooled together. The vertical lines denote 90% confidence intervals based on standard errors clustered at the spell level. The period 6 months before benefits exhaustion is used as the reference period.

stationary target wage at the individual level, similar to what was found at the aggregate level. Steeper decreases in target wage around benefits exhaustion are hardly visible. However the graphs are too noisy to draw a clear conclusion. These patterns are similar when restricting on spells that are completed more than 1 year after benefits exhaustion (Figure 3.C.1).

3.5.2 Size of the decrease in search selectivity at benefits exhaustion

Differences-in-differences approach

The differences-in-differences approach is not appropriate if PBD affects the target wage also outside of the benefits exhaustion period, which would be the case if benefits exhaustion goes together with a change in trend.

In Figure 3.C.2, we see that when adding month of the spell fixed effect in specifications (1) and (2), the target wage still exhibits a negative trend outside of benefits exhaustion, and coefficients are very imprecisely estimated. This downward trend could confound the estimation of the decline around benefits exhaustion and lead us to over-estimate the size of the decline. Corresponding estimates are reported in Table 3.C.1. These estimates should be seen as an upper bound. When comparing the target wage with a reference period defined as 6 months before benefits exhaustion, we find that the month of benefits exhaustion corresponds to a 0.087 euros decrease in the hourly wage job seekers apply to, which represent a 0.7% decrease. The period around benefits exhaustion corresponds to a decrease in wage by 0.05 euros (0.4%). Overall, we can not precisely estimate the shift in target wage, however, these results suggest that the magnitude of the additional decrease around benefits exhaustion is small.

Fitted polynomial approach

In order to have an alternative estimation of the additional decrease in target wage around benefits exhaustion, we turn to the fitted polynomial approach. The fitting exercise is presented graphically in Figure 3.C.3. Residuals from this prediction allow us to measure how much the target wage departs from its normal pattern in the surrounding of benefits exhaustion. Estimates are reported in Table 3.C.2. The decrease in target wage is below 1% at the aggregate and at the micro level, at benefits exhaustion and in the larger time window around benefits exhaustion. Estimates are consistent with the ones obtained in the differences-in-differences approach, and show that the additional decrease in target wage is of a much small order of magnitude that the shift in search intensity.

3.6 Dynamic impact of PBD on search outcomes in a RDD

We now estimate the impact of PBD on search outcomes in a regression discontinuity design. This additional analysis serves two purposes: first, the RDD approach allows us to estimate the impact of the PBD on the levels of various search measures. In previous analysis, we controlled for differences in levels accross different PBD groups. We could only interpret differences in *dynamics*, and not in *levels*. In the RDD setting, as job seekers with different PBD should be similar around the cutoff, we interpret differences in levels of search measures as caused by PBD. Second, the RDD provides an alternative strategy to estimate the shift in aggregate search around benefits exhaustion. We explore the dynamic impact of an extension of UI by estimating PBD impacts on outcomes measured at different moments of the unemployment spell. However, this strategy does not allow to disentangle shifts in aggregate job search due to shifts in individual behavior and shifts in aggregate job search due to dynamic selection.

The RDD is based on a discontinuity in eligibility rules around age 50: job seekers who become unemployed after 50 are eligible to up to 3 years of unemployment benefits, while they are only eligible to up to 2 years of unemployment benefits if they are younger than 50 when they become unemployed. We estimate a fuzzy RDD where the treatment variable is an indicator for having a PBD of 3 years. Estimates should therefore be interpreted as the impact for receiving 3 years of PBD instead of 2 years.

In this part, we first present in detail the RDD strategy, then we focus on the estimation of the impact of PBD on the search decisions at the start of the unemployment spell, and we finally explore the dynamic impact of PBD on job search over the unemployment spell.

3.6.1 Impact of PBD on search non-employment durations

We first present the estimated impact of PBD on unemployment and non-employment durations in Table 3.D.1, focusing only on the first year of the unemployment spell. In order to have estimates comparable to the literature, we first present estimates obtained in a fuzzy RDD with the log of PBD as a treatment variable in columns (1) to (3). In columns (4) to (6) we run our preferred specification, i.e. a fuzzy RDD with an indicator for having 3 years unemployment benefits as a treatment variable. We estimate in our sample an elasticity of benefits duration with respect to PBD between 0.29 and 0.41. These estimates are very close to the ones estimated by Barbanchon et al. (2017) using the same discontinuity, but on a larger sample (because not restricted on job seekers searching via the online search platform): the authors find estimates between 0.17 and 0.21. In columns (4) to (6), we see that the impact of an extension of PBD from 2 to 3 years causes an increase in the actual unemployment insurance between 12% and 18%.

The two panels below present the impact of PBD on unemployment duration and non employment duration. Estimates of the impact of PBD on unemployment durations are below the ones on actual benefits duration (elasticities range between 0.13 and 0.25). They are imprecisely estimated. Estimates for non-employment elasticity of PBD all range around 0, with high standard errors. However, because there can be very long duration before an exit from unemployment or before the exit to a new job, these elasticities are likely to be biased downward due to right censoring. We therefore also estimate the impact of PBD on the probability to stay unemployed/non-employed more than a certain duration in Figure 3.D.1. The graph displays the estimates obtained in separate RDD, with outcome variables respectively corresponding to the probability to stay unemployed/non-employed more than 6, 12, 18, 24, 30, 36 and 42 months. We observe that an increase of PBD from 2 to 3 years increase by 15 ppt the probability to remain unemployed more than 30 months and by 18 ppt the probability to remain non-employed more than 30 months. The graph suggests that the impact of the PBD on the hazard rate varies over the unemployment spell. We will now directly explore the dynamic impact of PBD on hazard rates and search outcomes.

3.6.2 Impact of PBD on search outcomes at the start of the unemployment spell

In the RD setting, as job seekers with different PBD should be similar around the cutoff *when they become unemployed*, we can test whether PBD affects job search decisions at the beginning of the unemployment spell. Table 3.4 reports fuzzy RDD estimates for hazard rates and search outcomes. In columns (4) to (6), in line with the pattern visible in Figure 3.D.1, we can see that having a PBD of 3 years instead of 2 slightly decreases the monthly hazard of leaving unemployment during the first year, by about 1 ppt. However, the rest of the table shows that the hazard of finding a job, or search behavior during the first year of the unemployment spell are not affected by the extension of UI. The table suggests that a benefits extension from 2 to 3 years does not affect how job seekers behave at the start of their unemployment spell. In order to explore when search behaviors actually start to be affected by this extension, we turn to a dynamic RDD.

Table 3.4: Impact of PBD on search during the first year of the unemployment spell

	Job finding rate			**Unemployment exit rate**		
Trimming	[49.9, 50.1]	[49.75, 50.25]	[49.5, 50.5]	[49.9, 50.1]	[49.75, 50.25]	[49.5, 50.5]
	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.003	0.005	0.008	-0.016**	-0.012*	-0.009*
	(0.011)	(0.010)	(0.010)	(0.008)	(0.007)	(0.005)
No. of Obs.	71577	70376	66837	71966	70761	67201
Outcome	**Number of applications**			**Target wage**		
Trimming	[49.9, 50.1]	[49.75, 50.25]	[49.5, 50.5]	[49.9, 50.1]	[49.75, 50.25]	[49.5, 50.5]
	(7)	(8)	(9)	(10)	(11)	(12)
RD Estimate	0.010	-0.015	-0.003	-0.046	-0.065	-0.023
	(0.033)	(0.037)	(0.036)	(0.118)	(0.123)	(0.144)
No. of Obs.	72371	71160	67583	42861	42145	40085

Note: Estimates correspond to a fuzzy RDD, where the treatment variable is an indicator for having a PBD of 3 years. Estimates should therefore be interpreted as the impact for receiving 3 years of PBD instead of 2. Each coefficient in this graph is estimated in a separate RDD. The estimation follows Calonico et al. (2014). The kernel used for local polynomial estimation is triangular.

3.6.3 Dynamic impact of PBD

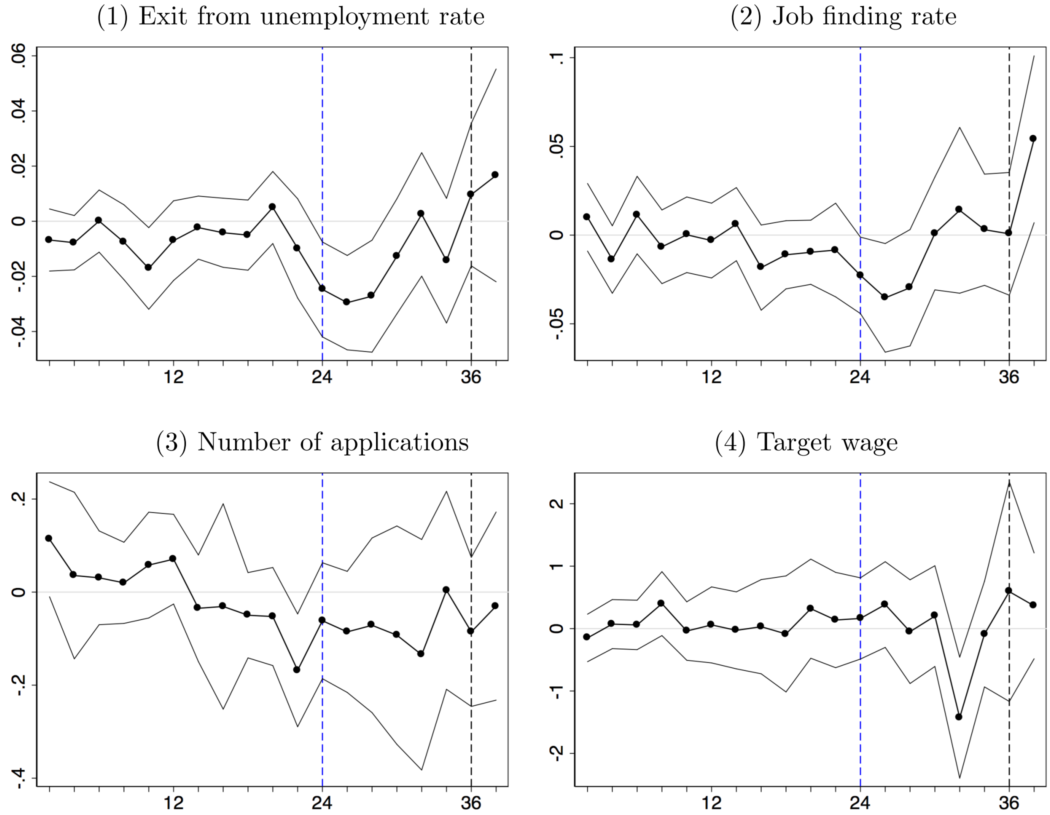
We then examine the evolution of the UI effect on job-finding rate and search behavior for different levels of unemployment duration. We present the estimates obtained in several RD estimations with outcome variables measured at different periods of the unemployment spell on the subsample of job seekers still unemployed. These estimates represent the impact of the shift in PBD from 2 to 3 years on these outcomes *without controlling for dynamic selection*. They should not be interpreted as the impact of PBD on individual hazards or search behavior, but rather as the impact on hazards or search behavior observed among job seekers still unemployed at each period.

Table 3.5: Impact of PBD on exit rates and search around 24 months

	Job finding rate			**Exit from unemployment rate**		
Trimming	[49.9, 50.1]	[49.75, 50.25]	[49.5, 50.5]	[49.9, 50.1]	[49.75, 50.25]	[49.5, 50.5]
	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.035*	-0.046**	-0.061**	-0.030***	-0.031**	-0.009
	(0.019)	(0.020)	(0.026)	(0.010)	(0.013)	(0.020)
No. of Obs.	16903	16611	15823	23300	22895	21739
Outcome	**Number of applications**			**Target wage**		
Trimming	[49.9, 50.1]	[49.75, 50.25]	[49.5, 50.5]	[49.9, 50.1]	[49.75, 50.25]	[49.5, 50.5]
	(7)	(8)	(9)	(10)	(11)	(12)
RD Estimate	-0.074**	-0.135***	-0.068	0.137	0.272	-0.342
	(0.037)	(0.052)	(0.058)	(0.464)	(0.647)	(0.565)
No. of Obs.	31142	30621	29095	4364	4275	4024

Notes: Estimates correspond to a fuzzy RDD, where the treatment variable is an indicator for having a PBD of 3 years. Estimates should therefore be interpreted as the impact for receiving 3 years of PBD instead of 2. Each coefficient in this graph is estimated in a separate RDD. The estimation follows Calonico et al. (2014). The kernel used for local polynomial estimation is triangular. The reported estimates corresponds to months 21-22 of the unemployment spell in this setting (where we observe the peak of the spike in Figure 3.7, (3)), and to 25-26 months for exit rates. Note that we collect data on individuals as long as they are in unemployment registers, which can be the case even after the start of a contract. The survival rates of leaving unemployment should not be the same as the survival of finding a job, and therefore the number of observations should not be the same in columns (1) to (3) and (4) to (6).

Figure 3.7: Dynamic effect of UI benefits extension



Notes: The x-axis denotes time of the unemployment spell. Each period corresponds to 2 months. Estimates correspond to a fuzzy RDD, where the treatment variable is an indicator for having a PBD of 3 years. Estimates should therefore be interpreted as the impact for receiving 3 years of PBD instead of 2. Each coefficient in this graph is estimated in a separate RDD. The estimation follows Calonico et al. (2014). The kernel used for local polynomial estimation is triangular. The trimming procedure excludes observations from job seekers aged in the interval $[49.9, 50.1]$.

Figure 3.7 displays the corresponding RD estimate for the exit from unemployment and job finding rates, search intensity, and the target wage among individuals who are still unemployed (the discontinuities in exit rates at the age cutoff at different durations of unemployment are presented in Figure 3.D.2). The first two graphs show that an extension of UI from 2 to 3 years causes a decrease in the probability of leaving unemployment and finding a job around 2 years. Figure (2) can be directly compared with the results from Nekoei and Weber (2017)[4]: using a discontinuity in Austria around 40 years old in PBD (from 30 to 39 weeks), the authors find a very similar dynamic impact of UI on job-finding hazards. They find that the earlier date of benefits exhaustion is associated with a negative gap in the job finding rate, while the later date is associated with a positive gap. Both gaps start to appears several periods before the dates of exhaustion, suggesting that job seekers are somewhat forward-looking. The similarities in the pattern are striking given the differences between the two institutional contexts considered. Figure (3) permits to highlight the underlying mechanism: we see a similar pattern in search intensity,

[4] See Figure 5, Panel B.

except that the gap appears earlier, in anticipation of benefits exhaustion. Finally, in Figure (4), we do not observe a significant gap around 2 years, however, we can observe a distinctive negative gap in the period preceding the later benefits exhaustion around 3 years. This pattern is consistent with the existence of a bump around benefits exhaustion, and consistent with findings of Nekoei and Weber (2017)[5].

In Table 3.5, we report the main corresponding estimates. Column (1) indicates that an increase in PBD from 3 to 2 years causes a decrease in job search by 0.074 applications around 24 months. If we compare it to the baseline number of applications around 24 months (that we approximate based on the average outcome for job seekers eligible to 3 years of PBD and above 50), it represents a decrease by 40.8%. This is equivalent to saying that job seekers experience a spike in their search intensity by 40.8% if they are eligible to 3 years of PBD instead of 2—conditional on being still unemployed after 2 years. The RDD estimate is even larger when adopting a more restrictive trimming procedure in column (2), and is of a similar order of magnitude (although imprecisely estimated) when applying the most restrictive procedure in column (3). In columns (4), (5) and (6), we see that the effect on the target wage is very imprecisely estimated. The order of magnitude of the estimate in column (4) suggests that job seekers eligible to 2 years of PBD experience a little decrease in their target wage of 0.137 euros per hour, conditional on still being unemployed. This corresponds to a 0.4% relative to the baseline level of target wage.

These results confirm previous findings concerning the spike in aggregate search intensity around benefits exhaustion. The slight differences in magnitude and in the timing of the shift could come from the fact that we focus on a different population here—job seekers around 50. In any case, this dynamic RDD brings additional evidence that there exists a temporary shift in search intensity around benefits exhaustion, of a very large magnitude. In contrast, the decrease in target wage consistently appears to be very small.

3.7 Discussion

3.7.1 Mechanisms behind the spike in job finding rate

Mechanisms highlighted by our results In Table 3.6, we present a summary of all estimates obtained through different approaches in the empirical analysis. The spike in search intensity consistently appears to represent a relative increase around 30% at its peak, while the target wage consistently seems to decrease by about 0.5%. We replicated the same analysis in Part II for the job finding rate. Although we cannot do this exercise within individual, we can measure changes in job finding rates among job seekers who are still unemployed. We find that the spike in job finding rate consistently amounts to a relative increase above 60%. These magnitudes seem consistent with search behavior accounting for most of the shift in job finding rate, although calibrating a search model would be required in order to test this more formally.

Returns to search Another way to assess non structurally the importance of search intensity in explaining the spike in the job finding rate around benefits exhaustion is to estimate the return to one application over the unemployment spell. The return to search is defined as the increase in the job finding rate associated with one extra application. Variations in return to search can

[5]See Figure 5, Panel D.

Table 3.6: Summary of estimates

Part I: Intermediary outcomes: Search behavior				
	Relative shift in search intensity around exhaustion			
	Aggregate level		Individual level	
	At the peak	Periods around	At the peak	Periods around
Diff-in-diff	0.360	0.169	0.381	0.207
Fitted polynomial	0.294	0.171	0.491	0.269
RDD	0.408	0.090		
	Relative shift in target wage			
	Aggregate level		Individual level	
	At the peak	Periods around	At the peak	Periods around
Diff-in-diff	-0.006	-0.004	-0.003	-0.001
Fitted polynomial	-0.005	-0.003	-0.006	-0.046
RDD	-0.004	-0.003		

Part II: Final outcome: Job finding rate		
	Relative shift in job finding rate	
	Aggregate level	
	At the peak	Periods around
Diff-in-diff	0.625	0.312
Fitted polynomial	0.672	0.400
RDD	0.898	0.860

Notes: In Part I, reported estimates are based on results presented in Table 3.2, Table 3.3, Table 3.C.1, Table 3.C.2 and Table 3.5. "At the peak" means the month of UB exhaustion, while "Periods around " corresponds to $[T-4; T+4]$ in the diff-in-diff and with the fitted polynomial approaches. In the RDD, "At the peak" corresponds to $[T-3; T-2]$ (i.e. from month 21 to 22 of the unemployment spell in this setting), and "Periods around", $[T-5; T+1]$ (i.e. from month 21 to 26 of the unemployment spell in this setting). For the RDD estimates, we take the coefficients obtained in the trimming procedure (1). In Part II, reported estimates are based on results presented in Appendix D.

be interpreted as the part of variation in job finding rate not explained by variations in search intensity. In Figure 3.F.1, one can observe a small increase in returns to search around benefits exhaustion, but it appears to be very small in comparison with the spike in job finding rate. This suggests that the spike in search intensity explains a large fraction of the spike in the job finding rate.

Other potential mechanisms Other mechanisms highlighted in previous literature might also contribute to the spike in job finding rate. Storable job offers, or agreements with previous employers in the context of recalls could also explain part of the spike in exits to job at benefits exhaustion. While we currently do not test this alternative mechanism, our data are well suited to do so. To test the storable offer mechanism, we can focus on job seekers who are hired in one establishment to which we observe that she made an application on the online search platform. For this subsample, we can assume that the date of the application is the date of contact between the worker and the firm, and therefore infer the duration between the contact and the start of the contract. If this duration is significantly longer for contract starting around benefits exhaustion, storable offers would prove to be also at play. In order to test whether recalls are also a determinant, we can replicate our analysis for the subsample of job seekers for which we observe a recall. If we observe that there is a spike in job finding rate but not in search intensity in this subsample, this would provide evidence supporting the additional role of recalls. We plan to examine these channels is future extensions. For now, we note that our evidence is inconsistent with storable job offers or recalls explaining all of the increase in the exit to employment around benefits exhaustion. Instead, we find that job search intensity plays an important role in explaining the spike in job finding around benefits exhaustion.

3.7.2 Potential search channel substitution

One limitation of our data is that we can only observe one search channel, and our results would be biased if there was endogenous search channel substitution. This might affect the interpretation of trends in our measure of search over the unemployment spell, however this is very unlikely to affect our results concerning the behavior around benefits exhaustion. We discuss these two points separately.

Trend over the unemployment spell Our results point toward a downward trend in search intensity and in target wage over the unemployment spell. Figure 3.F.1 also shows a downward trend in return to search which suggests that, although the number of applications decreases over the unemployment spell, the decrease in job finding rate is *even steeper*. But channel substitution is a concern for the interpretation of these trends: in particular, it could be that the decrease that we estimate in returns to search over a spell comes from an increase in the use of the online search platform relative to other search channels over time. This would imply that the overall decrease in search is even steeper than the one we observe. However, decreasing returns to search are consistent with the findings of a stigma associated with unemployment duration in the literature (Oberholzer-Gee (2008), Kroft et al. (2013), Eriksson and Rooth (2014)) and we find it unlikely that channel substitution would explain all the decrease in returns to search that we observe.

In the case of target wage, the estimated decrease is unlikely to come from search channel substitution. Assuming that job seekers have the same target wage on different search channels, this pattern provides compelling evidence that job seekers are less and less selective in their search over time.[6]

Spike around benefits exhaustion Finally, it is not credible that channel substitution drives the spike we observe in search intensity around benefits exhaustion for a couple of reasons: (1) it would require a temporary substitution of other search channels toward the online search platform, and we can not see any explanation for such a behavior (2) the magnitude of the spike in job search intensity that we observe is very consistent with the magnitude of the spike in the re-employment rate.

3.7.3 Implications of our findings and links with previous literature

Our data allows us to get under the hood of behavioral mechanisms that explain the impact of unemployment insurance on the duration before re-employment.

Mechanisms behind the UI effect on non-employment duration Overall, our results suggest that search intensity is the main factor in explaining the impact of unemployment insurance on non-employment duration. We find that the role of the reservation wage is limited, and that

[6] We actually even need a less restrictive assumption as it is enough to assume that, if job seekers have different target wages on different search channels, their ratio remains constant over time.

job search explains much of the behavioral effects of unemployment insurance. People decrease their target wage as their unemployment spell progresses, but there is little further decrease in target wages around benefits expiration. Furthermore, an increase in benefits duration does not affect target wages at the start of the unemployment spell. Most applied models of unemployment insurance assume away the reservation wage channel, and we provide compelling evidence that this simplifying assumption is justified.

We show a spike in search effort at exhaustion that is inconsistent with the basic search model (Mortensen (1977)): in this framework, job seekers should increase their search effort in the periods leading up to benefits exhaustion and then keep their level of effort constant at a high level. Instead, we find a steep decrease in search intensity following benefits exhaustion, which points toward a shift in the value of being unemployed, or the perceived return to search at benefits exhaustion. DellaVigna et al. (2017) provide a framework that can rationalize this individual behavior after benefits exhaustion: in their model, people get used to lower income and therefore search less and less over time. However, the reference dependence model also predicts a steep decrease in search intensity at the start of the spell—as job seekers also slowly get used to getting UI instead of their previous wage—which we do not observe clearly in our results (see Panel A, in the second row of Figure 3.3). This could come from the fact that we only track search activity from the time job seekers start to receive their unemployment benefits, and therefore do not observe search behavior right after their job loss (there might be a delay between job loss and the start of UI because job seekers might wait before claiming their UI and most importantly because there is a legal waiting period called "période de carence"). We plan to investigate this in the future.

Finally, we find suggestive evidence that there is a downward trend in returns to search over the unemployment spell. It might be useful to incorporate this component in a search model to fully account for the impact of UI on non-employment duration. This pattern suggests that the behavioral response to UI might not be the only reason why longer PBD increases non-employment duration. Longer benefits postpone the spike in search effort and therefore postpone the exit to employment: this is the behavioral response. However, there might be a second mechanism: because of decreasing returns to search, job seekers are less likely to find a job when they increase their search effort later in the unemployment spell. These two mechanisms reinforce each other to increase the effect of UI on non-employment duration.

Mechanisms behind the UI effect on re-employment wage Although we cannot estimate the impact of UI on re-employment wage in our data, it is worth noting that our results are consistent with the analysis of Nekoei and Weber (2017). The authors argue that the impact of benefits duration on re-employment wages is due to a conjunction of two opposite mechanisms: first, longer benefits reduce job search effort, and therefore decrease re-employment wage due to a decrease in target wage over the unemployment spell; second, longer benefits increase target wages. Related to the first mechanism, we show direct evidence that extending benefits delays the spike in search effort: this suggests that longer PBD decreases wages by delaying exit to a period where target wages are lower. Concerning the second mechanism, we find weak evidence that people drop their target wage more aggressively around benefits exhaustion: if this mechanism was important for some people and in some contexts, it could explain a positive effect of benefits extensions on wages, as in Nekoei and Weber (2017).

Policy implications From a policy perspective, our findings suggest that longer unemployment benefits mostly affect search behavior close to exhaustion, and hence unemployed workers with short durations are unaffected. Indeed, our RDD results suggest that individuals are only moderately forward-looking, which dampens the moral hazard associated with UI benefits due later in the spell. However, for less employable individuals, longer UI may increase their non employment duration due to a mix of moral hazard and negative duration dependence in returns to search. Hence our results contribute to the discussion of the optimal time path of UI in recent literature. Extending UI duration is equivalent to increasing the level of benefits late in the spell, and our findings therefore relate to the results in Kolsrud et al. (2018). The authors find that an increase in benefits has a lower moral hazard if it is due later in the unemployment spell and a higher insurance value, which provides arguments in favor of benefits increasing over the unemployment spell.

3.8 Conclusion

Why does unemployment insurance increase unemployment duration? We use unique longitudinal data on job search and unemployment insurance eligibility to show that most of the behavioral effect of unemployment insurance is due to changes in search effort, with reservation wages playing a minor role. We exploit rich variation in potential benefits duration and a regression discontinuity design for identification. We find that the unemployed strongly increase their number of applications as benefits exhaustion approaches, and decrease it after exhaustion. This job search pattern matches the spike in exits to jobs at benefits exhaustion. Extending benefits duration delays the spike in job applications, which leads to longer unemployment duration. The regression discontinuity design allows us to clearly establish that longer potential benefits duration does not affect job search effort nor reservation wages early on in the spell. The causal effect of longer potential benefits duration on job search effort is only apparent close to benefits exhaustion, while impacts on target wages are insignificant.

These findings support models of unemployment insurance where job search effort is the key behavioral channel. At the same time, the findings are inconsistent with the basic search and matching model: while the basic model predicts a constant job search effort after benefits exhaustion, we find that applications strongly decrease after benefits exhaustion. Job search models predicting a shift in perception of incentives at benefits exhaustion may account for the decrease after exhaustion, such as reference-dependent preferences.

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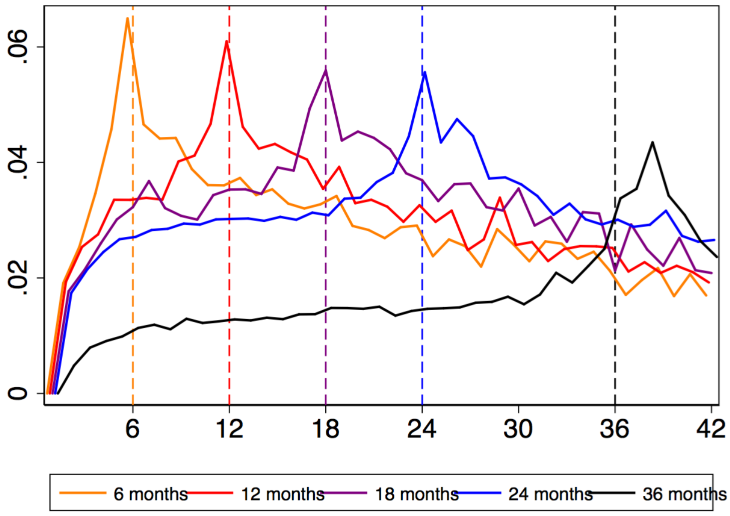
Appendix

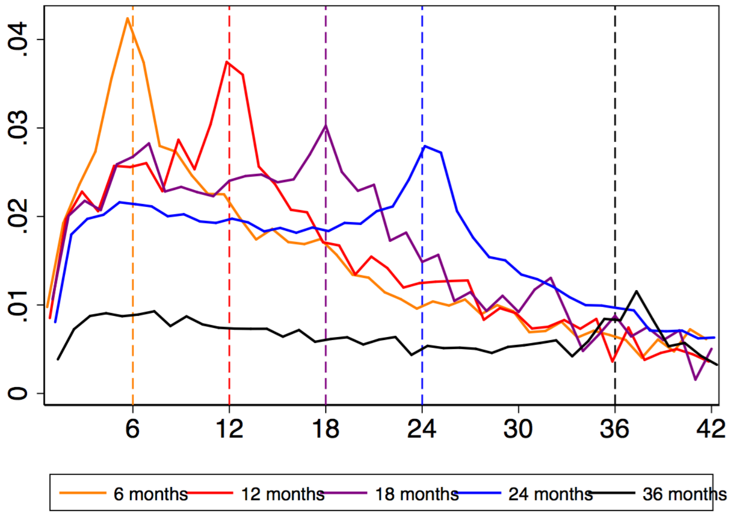
3.A.1 Descriptive statistics

Figure 3.A.1: Empirical hazard rate for job seekers eligible to different PBD

Exit from unemployment registry



Employment with a long term contract



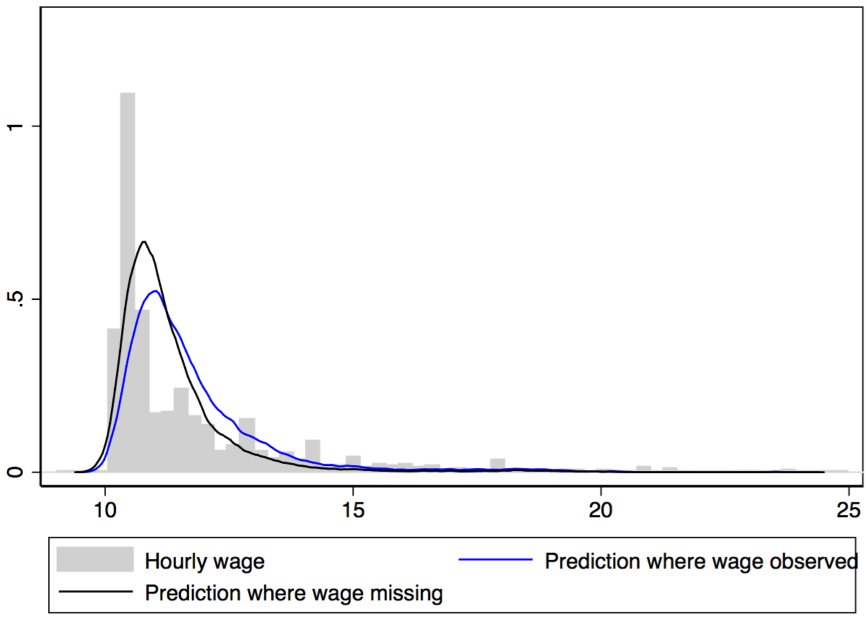
Notes: These rates correspond to the number of exit to employment in month t divided by the size of the risk set at the beginning of the month. This graph is based on our main sample.

Table 3.A.1: Selection of the unemployed workers applying on the online search platform

	Mean All population	Mean > 0 online application	Difference (2)-(1)	T stat (1)=(2)
	(1)	(2)	(3)	(4)
Female	0.49	0.55	0.07	123.82
Age	34.12	31.23	-3.63	-295.29
Single	0.62	0.65	0.04	76.32
Look for full time job	0.90	0.92	0.03	97.26
Education				
No diploma	0.04	0.02	-0.03	-143.65
Middle school	0.13	0.10	-0.04	-116.47
Vocational high school	0.34	0.34	0.01	17.80
General high school	0.24	0.27	0.04	96.78
Higher education	0.25	0.27	0.02	45.94
Qualification				
Blue collar, low skill	0.09	0.07	-0.03	-87.18
Blue collar, high skill	0.12	0.10	-0.02	-67.82
White collar, low skill	0.22	0.22	-0.00	-1.28
White collar, high skill	0.44	0.50	0.07	136.86
Intermediary	0.07	0.08	0.01	28.92
Management	0.06	0.04	-0.03	-112.84
Duration of unemployment spell	389.58	486.91	122.06	300.66
Number of previous registrations	0.49	2.05	1.96	1909.08
Number of spells	5,392,835	1,092,584		

Notes: Among all unemployment spells started in 2014, we compare those for which we observe at least one application during the spell on the online search platform with the population.

Figure 3.A.2: Predicted hourly wage



Notes: In this Figure, we present the distribution of the posted hourly wage contained in the job offers from our sample with the predicted hourly wage based on a linear prediction using all characteristics contained in the job offers. The posted wage is missing for 59% of job offers in our sample. We present separately the distribution of predicted hourly wage when the posted wage is observed and when it is missing.

Table 3.A.2: Linear model for the prediction of posted hourly wage

Outcome	Hourly wage
Full time job	0.116***
	(0.009)
Number of weekly hours	-0.026***
	(0.001)
Contract tern (ref: < 6 months long)	
Permanent contract	0.452***
	(0.005)
Long-term contract	0.111***
	(0.005)
Establishment size (ref: < 5)	
5 to 20 employees	0.048***
	(0.004)
20 to 50 employees	0.098***
	(0.004)
50 employees	0.218***
	(0.004)
Required experience (ref: No experience)	
Some work experience	0.362***
	(0.003)
Required qualification (ref: blue collar, low skill)	
blue collar, high skill	0.222***
	(0.009)
white collar, low skill	0.032***
	(0.008)
white collar, high skill	0.200***
	(0.008)
intermediary position	0.968***
	(0.009)
management position	4.221***
	(0.013)
Required education (ref: no diploma mentioned)	
vocational high school diploma	-0.083***
	(0.005)
general high school diploma	-0.034***
	(0.005)
Higher education diploma	0.450***
	(0.005)
Job category FE	YES
County FE	YES
N	1,200,061
F	14,007.577
R^2	0.485

Notes: In this Table, we present the regression of the posted hourly wage on all characteristics contained in the job offers. The sample is made of all job offers from our sample for which the posted wage is not missing (41% of job offers in our sample).

Table 3.A.3: First stage: Estimation of shift in PBD around 50 in a sharp RD

Outcome		**PBD (log)**			**1{PBD=3 years}**	
Trimming	$[49.9, 50.1]$	$[49.75, 50.25]$	$[49.5, 50.5]$	$[49.9, 50.1]$	$[49.75, 50.25]$	$[49.5, 50.5]$
	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.230***	0.274***	0.311***	0.539***	0.642***	0.696***
	(0.036)	(0.035)	(0.021)	(0.034)	(0.019)	(0.009)
No. of Obs.	72371	71160	67583	72371	71160	67583

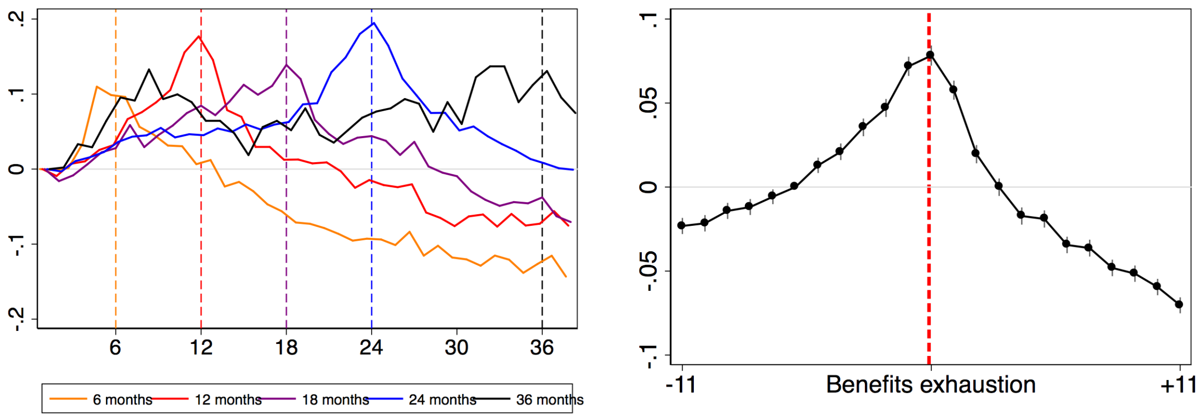
Notes: Table reports RDD estimates on samples trimmed in different ways.

3.B.2 Search intensity around benefits exhaustion

Figure 3.B.1: Robustness: Search intensity over the unemployment spell and relative to benefits exhaustion

PANEL A: Search intensity over the unemployment spell **PANEL B: Search intensity over time relative to benefits exhaustion**

With spell fixed effects, for spells $\geq$ 12 months longer than PBD



Notes: In Panel A, the graphs present results from regressions of search intensity on month of unemployment spell dummies with various specification. The x-axis denotes time of the unemployment spell in month. The graphs display separately the search pattern for job seekers who have various duration of PBD (6, 12, 18, 24 and 36 months). The 1st month of the spell is used as the reference period. In Panel B, the graphs present results from regressions of search intensity on time to benefits exhaustion. The x-axis denotes time relative to benefits exhaustion in months. Observations from job seekers with various PBD between 12 and 24 months are pooled together. The vertical lines denote 90% confidence intervals based on standard errors clustered at the spell level. The period 6 months before benefits exhaustion is used as the reference period. Search intensity is measured by the monthly number of applications made on the online search platform.

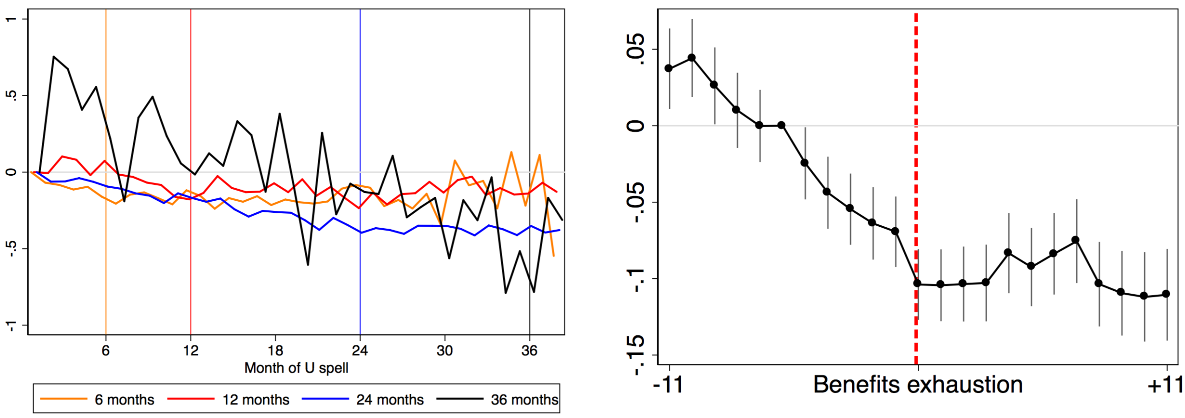
3.C.3 Target wage around benefits exhaustion

Figure 3.C.1: Target wage over the unemployment spell and relative to benefits exhaustion

PANEL A: Target wage over the unemployment spell

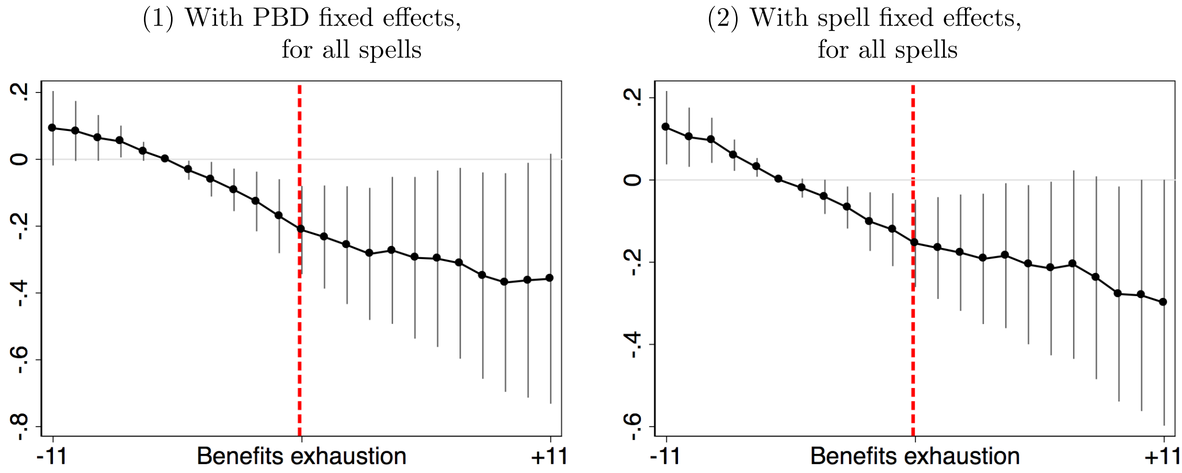
PANEL B: Target wage over time relative to benefits exhaustion

With spell fixed effects, for spells ≥ 12 months longer than PBD



Notes: In Panel A, the graphs present results from regressions of target wage on month of unemployment spell dummies with various specification. The x-axis denotes time of the unemployment spell in month. The graphs display separately the target wage pattern for job seekers who have various duration of PBD (6, 12, 18, 24 and 36 months). The 1st month of the spell is used as the reference period. In Panel B, the graphs present results from regressions of target wage on time to benefits exhaustion. The x-axis denotes time to benefits exhaustion in month. Observations from job seekers with various PBD between 6 and 24 months are pooled together. The vertical lines denote 90% confidence intervals based on standard errors clustered at the spell level. The period 6 months before benefits exhaustion is used as the reference period.

Figure 3.C.2: Target wage over time relative to benefits exhaustion, controlling for time of spell



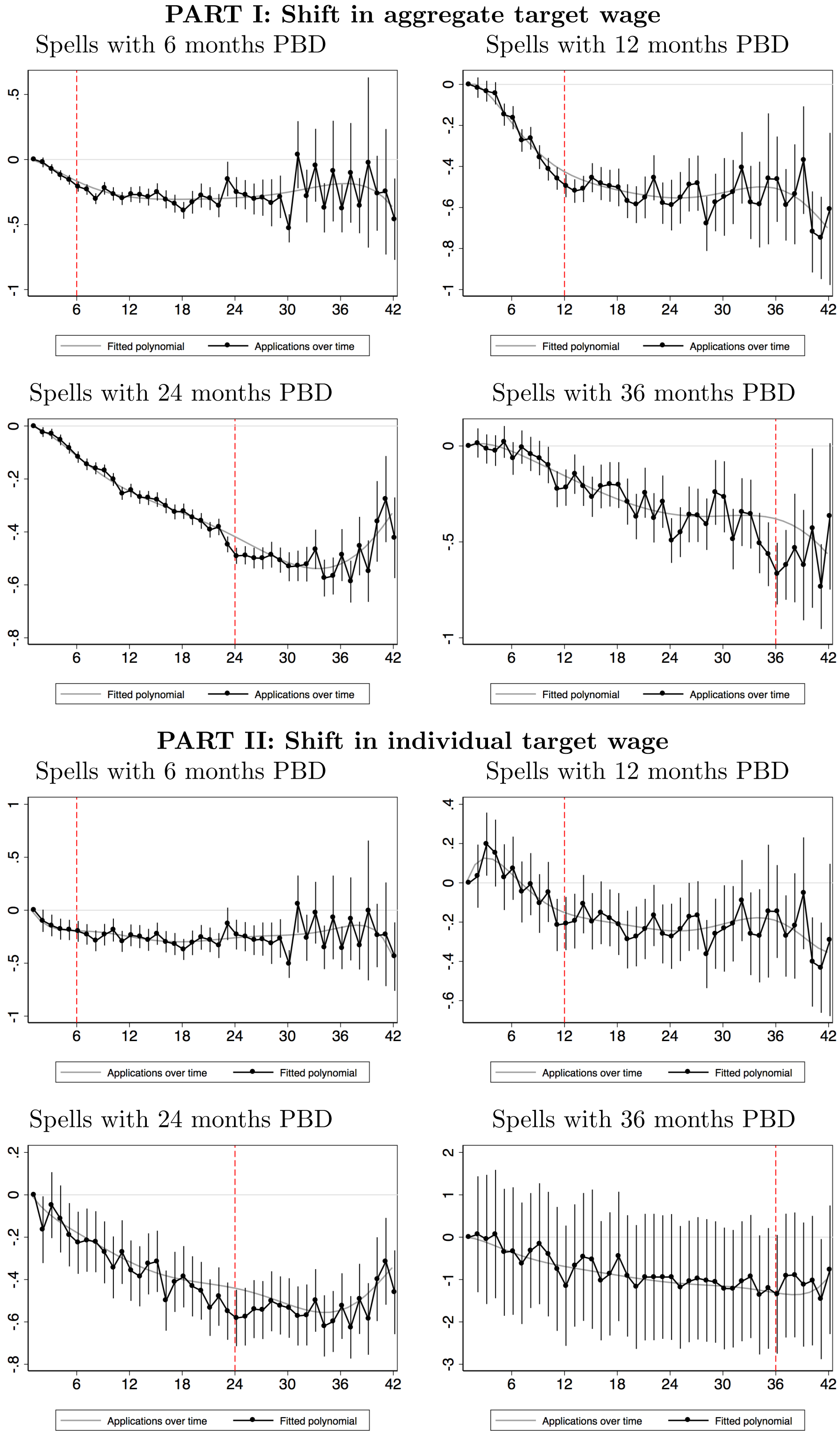
Notes: The graphs present results from regressions of search intensity on time to benefits exhaustion. Observations from job seekers with various PBD between 6 and 24 months are pooled together. The period 6 months before benefits exhaustion is used as the reference period. The vertical lines denote 90% confidence intervals based on standard errors clustered at the spell level.

Table 3.C.1: Shift in target wage around benefits exhaustion

Outcome variable (Mean at $T = -6$: 11.520)	Wage of job applied to			
	Aggregate		Within individual	
	(1)	(2)	(3)	(4)
$T = -4$	-0.007		0.002	
	(0.011)		(0.009)	
$T = -3$	-0.026**		-0.014	
	(0.011)		(0.009)	
$T = -2$	-0.033***		-0.025**	
	(0.012)		(0.010)	
$T = -1$	-0.054***		-0.026**	
	(0.012)		(0.010)	
$T = 0$	-0.072***		-0.040***	
	(0.013)		(0.011)	
$T = 1$	-0.069***		-0.032***	
	(0.013)		(0.011)	
$T = 2$	-0.071***		-0.024**	
	(0.014)		(0.011)	
$T = 3$	-0.074***		-0.020	
	(0.015)		(0.013)	
$T = 4$	-0.041**		0.010	
	(0.016)		(0.013)	
$[T-4; T+4]$		-0.041***		-0.016**
		(0.009)		(0.008)
All other T expect $T = -6$	Yes	Yes	Yes	Yes
Month of U spell FE	Yes	Yes	Yes	Yes
PBD group FE	Yes	Yes	No	No
Spell FE	Yes	Yes	No	No
No. of Obs.	486,473	486,473	375,477	375,477

Notes: Coefficients in this table are obtained in regressions of target wage on time to benefits exhaustion dummies, time of the spell dummies as well as PBD group FE in columns (1) and (3) and spell FE in columns (2) and (4). This empirical strategy exploits the fact that different job seekers are “treated” by benefits exhaustion at different moment of their unemployment spell. SE are clustered at the spell level.

Figure 3.C.3: Actual and predicted target wage over unemployment spell, for different PBD



Notes: The prediction is based on a fitted polynomial function of time of spell. We use a polynomial of degree 7. The fit is made outside of the benefits exhaustion period, which is defined as $[T-4; T+4]$. The vertical lines denote 90% confidence intervals based on standard errors clustered at the spell level.

Table 3.C.2: Shift in target wage around benefits exhaustion

PART I: Shift in aggregate target wage						
Sample: PBD (month)	All	6	12	18	24	36
			Absolute shift in target wage:			
At exhaustion	-0.063***	-0.049***	-0.069***	0.017	-0.073***	-0.283***
	(0.011)	(0.017)	(0.026)	(0.046)	(0.017)	(0.092)
Around exhaustion	-0.037***	-0.033***	-0.046***	-0.001	-0.036***	-0.153***
	(0.006)	(0.009)	(0.014)	(0.023)	(0.009)	(0.054)
No. of Obs.	573974	57035	48256	27067	403855	37761
			Relative shift in target wage:			
At exhaustion	-0.005***	-0.004***	-0.006***	0.001	-0.006***	-0.024***
	(0.001)	(0.002)	(0.002)	(0.004)	(0.001)	(0.008)
Around exhaustion	-0.003***	-0.003***	-0.004***	-0.000	-0.003***	-0.013***
	(0.000)	(0.001)	(0.001)	(0.002)	(0.001)	(0.005)
No. of Obs.	573,974	57,035	48,256	27,067	403,855	37,761
PART II: Shift in individual target wage						
Sample: PBD (month)	All	6	12	18	24	36
			Absolute shift in target wage:			
At exhaustion	-0.073***	-0.005	-0.056	-0.103	-0.119***	-0.095
	(0.019)	(0.034)	(0.044)	(0.070)	(0.029)	(0.128)
Around exhaustion	-0.046***	-0.019	-0.015	-0.043	-0.081***	0.131
	(0.011)	(0.019)	(0.028)	(0.043)	(0.016)	(0.142)
No. of Obs.	76,238	19,834	11,869	4,709	38,377	1,449
			Relative shift in target wage:			
At exhaustion	-0.006***	-0.000	-0.005	-0.009	-0.010***	-0.008
	(0.002)	(0.003)	(0.004)	(0.006)	(0.003)	(0.011)
Around exhaustion	-0.004***	-0.002	-0.001	-0.004	-0.007***	0.011
	(0.001)	(0.002)	(0.002)	(0.004)	(0.001)	(0.012)
No. of Obs.	76,238	19,834	11,869	4,709	38,377	1,449

Notes: The table presents the estimated value of residuals at benefits exhaustion ($T = 0$) or around ($[T-4; T+4]$). Residuals come from the predicted search over the unemployment spell, based on a polynomial function of time of spell. We use a polynomial of degree 7. The fit is made outside of the benefits exhaustion period, which is defined as $[T-4; T+4]$. SE are clustered at the spell level.

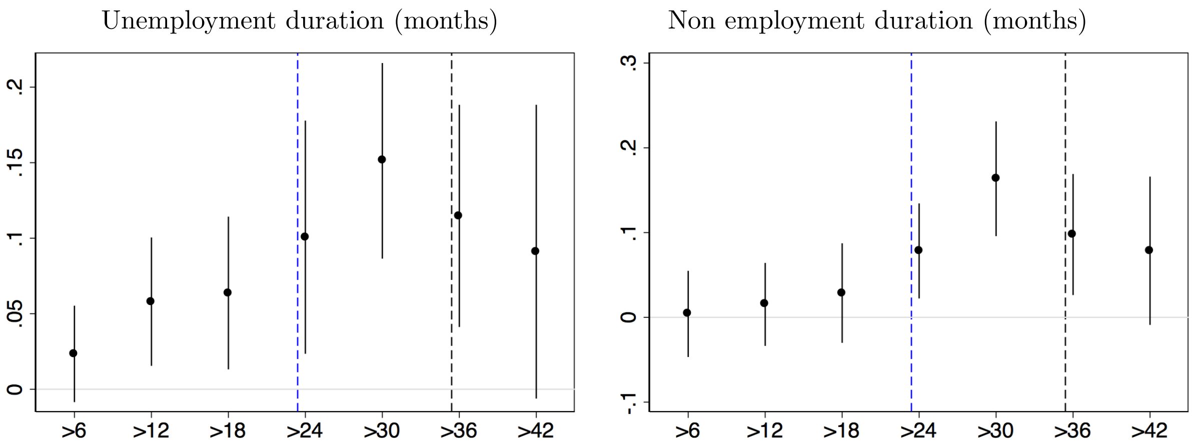
3.D.4 Regression discontinuity design

Table 3.D.1: Impact of PBD on unemployment and non employment durations

Outcome	Duration of actual benefits (log)					
Treatment		**PBD (log)**			**1{PBD=3 years}**	
Trimming	$[49.9, 50.1]$	$[49.75, 50.25]$	$[49.5, 50.5]$	$[49.9, 50.1]$	$[49.75, 50.25]$	$[49.5, 50.5]$
	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.290*	0.371**	0.415***	0.121*	0.179***	0.186***
	(0.171)	(0.153)	(0.105)	(0.070)	(0.054)	(0.046)
No. of Obs.	72371	71160	67583	72371	71160	67583
Outcome	**Duration of unemployment (log)**					
Treatment		**PBD (log)**			**1{PBD=3 years}**	
Trimming	$[49.9, 50.1]$	$[49.75, 50.25]$	$[49.5, 50.5]$	$[49.9, 50.1]$	$[49.75, 50.25]$	$[49.5, 50.5]$
	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.133	0.188	0.252**	0.043	0.093	0.108**
	(0.157)	(0.149)	(0.110)	(0.072)	(0.058)	(0.046)
No. of Obs.	72371	71160	67583	72371	71160	67583
Outcome	**Duration of non employment (log)**					
Treatment		**PBD (log)**			**1{PBD=3 years}**	
Trimming	$[49.9, 50.1]$	$[49.75, 50.25]$	$[49.5, 50.5]$	$[49.9, 50.1]$	$[49.75, 50.25]$	$[49.5, 50.5]$
	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.111	-0.010	0.096	-0.046	0.023	0.040
	(0.230)	(0.204)	(0.108)	(0.091)	(0.068)	(0.056)
No. of Obs.	72371	71160	67583	72371	71160	67583

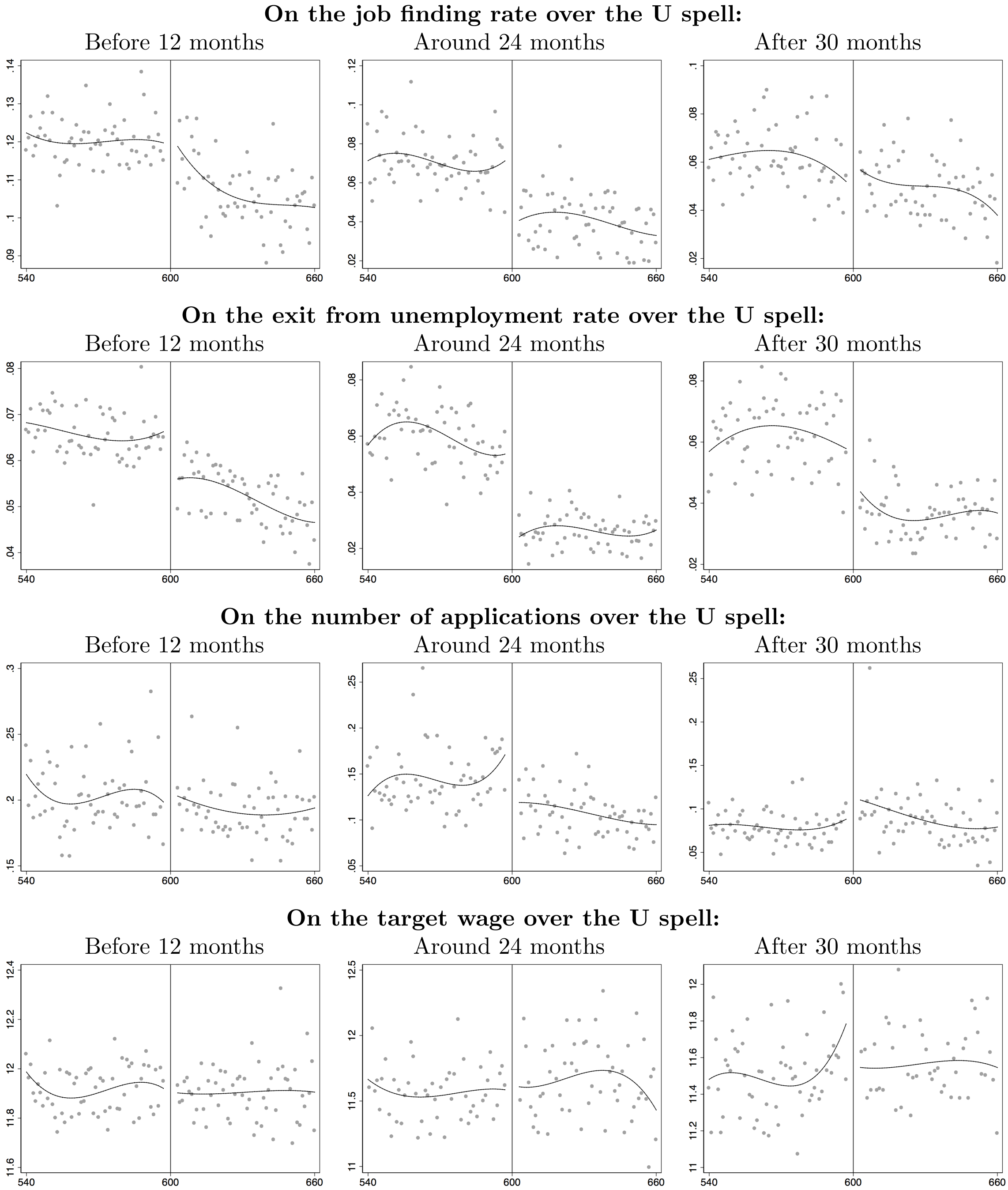
Notes: Table reports RDD estimates on samples trimmed in different ways.

Figure 3.D.1: Impact of PBD on unemployment and non employment durations



Notes: Each coefficient in this graph is estimated in a separate RDD. The graph shows the impact of PBD on the probability to remain unemployed more than various durations. The vertical lines denote 90% confidence intervals based on standard errors clustered at the spell level.

Figure 3.D.2: RDD Estimates of the impact of 3 vs 2 years of PBD



Notes: RDD plots are obtained with the stata command rdplot. The data is grouped in age bins, whose size is determined according to Calonico et al. (2014). For each age bin, we plot the averages of the outcome variable. Then a polynomial of degree 4 is fitted on these averages - the solid line.

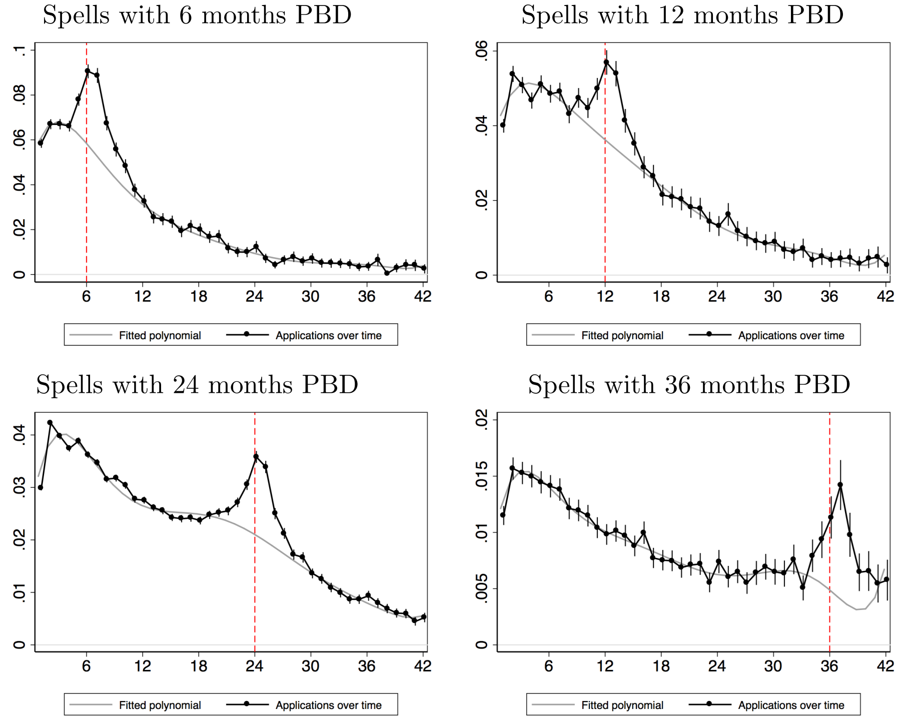
3.E.5 Spike in job finding rate

Table 3.E.1: Shift in job finding rate around benefits exhaustion

Outcome variable (Mean at $T = -6$: 0.0320)	Job finding rate	
$T = -4$	0.004*** (0.001)	
$T = -3$	0.006*** (0.001)	
$T = -2$	0.010*** (0.001)	
$T = -1$	0.013*** (0.001)	
$T = 0$	0.020*** (0.001)	
$T = 1$	0.017*** (0.001)	
$T = 2$	0.012*** (0.001)	
$T = 3$	0.007*** (0.001)	
$T = 4$	0.004*** (0.001)	
$[T - 4; T + 4]$		0.010*** (0.001)
All other T expect $T = -6$	Yes	Yes
Month of U spell FE	Yes	Yes
PBD group FE	Yes	Yes
No. of Obs.	3548632	3548632

Notes: Coefficients in this table are obtained in regressions of target wage on time to benefits exhaustion dummies, time of the spell dummies as well as PBD group FE in columns (1) and (3) and spell FE in columns (2) and (4). This empirical strategy exploits the fact that different job seekers are "treated" by benefits exhaustion at different moment of their unemployment spell. SE are clustered at the spell level.

Figure 3.E.1: Actual and predicted job finding rate over unemployment spell, for different PBD



Notes: The prediction is based on a fitted polynomial function of time of spell. We use a polynomial of degree 7. The fit is made outside of the benefits exhaustion period, which is defined as $[T-4;T+4]$. The vertical lines denote 90% confidence intervals based on standard errors clustered at the spell level.

Table 3.E.2: Shift in job finding rate around benefits exhaustion

Sample: PBD (month)	All	6	12	18	24	36
			Absolute shift in job finding rate:			
At exhaustion	0.018***	0.031***	0.020***	0.011***	0.015***	0.006***
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)	(0.001)
Around exhaustion	0.009***	0.017***	0.010***	0.008***	0.008***	0.005***
	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
No. of Obs.	6837407	403345	405681	250999	4844062	933320
			Relative shift in job finding rate:			
At exhaustion	0.672***	0.529***	0.556***	0.408***	0.689***	1.267***
	(0.027)	(0.032)	(0.055)	(0.095)	(0.033)	(0.229)
Around exhaustion	0.400***	0.305***	0.289***	0.314***	0.370***	1.140***
	(0.010)	(0.012)	(0.020)	(0.037)	(0.012)	(0.096)
No. of Obs.	6837407	403345	405681	250999	4844062	933320

Notes: The table presents the estimated value of residuals at benefits exhaustion ($T=0$) or around ($[T-4;T+4]$). Residuals come from the predicted search over the unemployment spell, based on a polynomial function of time of spell. We use a polynomial of degree 7. The fit is made outside of the benefits exhaustion period, which is defined as $[T-4;T+4]$. SE are clustered at the spell level.

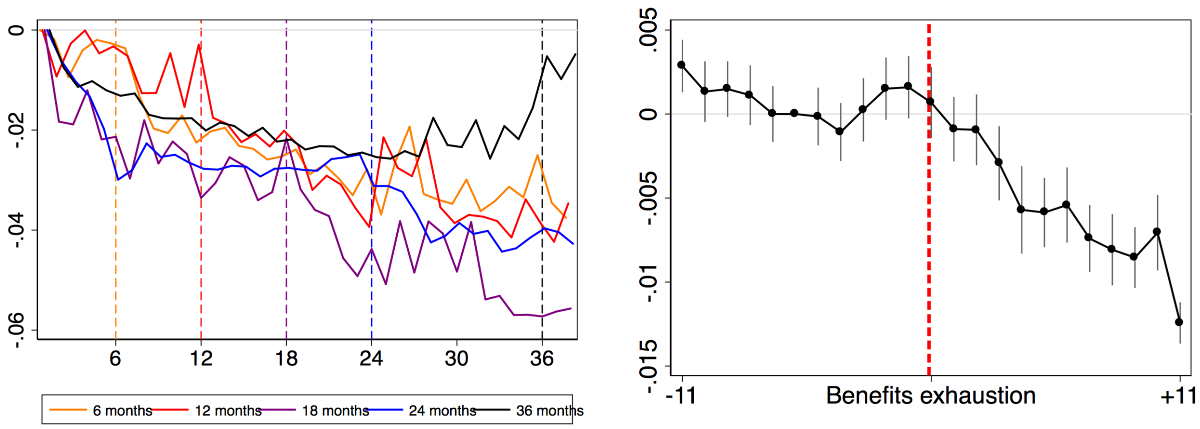
3.F.6 Return to search

We estimate the following fully saturated model for observations around benefits exhaustion ($k \in [-11; 11]$):

$$\mathbb{1}\{(E_i \in [t, t+1] | E_i \geq t) = \sum_{\substack{k=-11 \\ k \neq -6}}^{11} \alpha_k D^k_{B(i),t} * Y_{i,t} + \sum_{\substack{k=-11 \\ k \neq -6}}^{11} \beta_k D^k_{B(i),t} + \gamma Y_{i,t} + \mu_{B(i)} + \epsilon_{i,t} \quad (3.F.1)$$

where $\mathbb{1}\{(E_i \in [t, t+1] | E_i \geq t)$ is the probability to find a job between $[t, t+1]$ for job seekers still unemployed at t, $Y_{i,t}$ the number of applications, and $D^k_{B(i),t}$ periods around benefits exhaustion, $k = -6$ the reference period, $\mu_{B(i)}$ PBD group fixed effects.

Figure 3.F.1: Return to search over the unemployment spell and around benefits exhaustion



Notes: The vertical lines denote 90% confidence intervals based on standard errors clustered at the spell level.

ÉCOLE DOCTORALE
Sciences de l'homme
et de la société (SHS)

Titre: Essais sur les déterminants des comportements de recherche d'emploi et de l'accès à l'emploi

Mots clés: Recherche d'emploi, Marché du travail, Chômage, Evaluation de politiques publiques

Résumé: Cette thèse explore différents déterminants du comportement de recherche d'emploi, dans le but de comprendre certains des obstacles au retour à l'emploi pour les travailleurs les plus défavorisés. Le premier chapitre est consacré à l'évaluation d'impact d'un programme d'accompagnement collectif innovant pour les jeunes chômeurs des zones urbaines sensibles. Ce programme semble plus efficace qu'un programme classique pour permettre l'accès à un emploi stable. L'effet le plus large est détecté parmi les participants qui sont assignés à un groupe avec des chômeurs en grande difficulté. Dans le second chapitre, j'étudie l'impact d'un choc d'information sur la recherche d'emploi et la probabilité de retour à l'emploi des chômeurs. Mes résultats suggèrent qu'apporter de l'information permettant aux chômeurs d'orienter leurs candidatures vers les entreprises qui ont le plus de chance de faire des recrutements à court-terme peut permettre de corriger certaines inégalités dans l'accès à l'emploi et stimuler la mobilité géographique. Le troisième chapitre explore les mécanismes sous-jacents derrière l'effet négatif de la durée d'assurance chômage sur le taux de retour à l'emploi. Les efforts de recherche augmentent de 25 % dans les mois qui entourent la date de fin des droits à l'assurance chômage, même lorsqu'on neutralise l'impact de la sélection dynamique. Une extension de l'assurance chômage affecte les comportements de recherche d'emploi principalement par un recul du pic dans l'intensité de la recherche d'emploi observé autour de la date de l'épuisement des droits.

Title: Essays on the Individual Determinants of Job Search Behavior and Employment

Keywords: Job search, Labor Market, Unemployment, Policy evaluation

Abstract: My dissertation explores different determinants of job search behavior in order to highlight some obstacles in the access to jobs, in particular among disadvantaged workers. The first chapter evaluates a collective counselling program for young workers from deprived neighborhoods. This program seems more effective in helping participants access a stable job. The largest effect is found among participants assigned to groups with peers who have relatively bad employment prospects. In the second chapter, I study how an information shock affects the job search of unemployed workers and their access to employment. My findings suggest that providing information to help disadvantaged job seekers target firms which have large short-term hiring needs could contribute to correct inequalities in job access and increase geographic mobility. The third chapter explores the mechanisms behind the well-documented negative impact of unemployment insurance on re-employment rate. I highlight a 25% spike in job search intensity in the months surrounding benefits exhaustion, when controlling for dynamic selection. A benefits extension increases unemployment duration mostly by postponing the spike in search intensity associated with benefits exhaustion.

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