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ORIGINAL ARTICLE

Effectiveness of a job vacancy referral scheme

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Abstract

The public employment service (PES) makes use in many countries of vacancy referrals as to facilitate the matching between unemployed workers and vacancies. Based on a "timing-of-events" approach to control for selective participation, this study evaluates whether this policy instrument enhanced the transition to employment in Flanders (region in northern Belgium). Three referral types are distinguished: (1) *referrals* actively matched by a caseworker by phone or by e-mail; (2) *automatic referrals*, in which the match is accomplished by a software without caseworker intervention; and (3) *invitations*, in which the referral is transmitted to the unemployed in a meeting with a caseworker. All three referral instruments are found to be effective, even many months after the transmission of the referral: the first and third referral types more than triples, respectively, double the transition rate to employment both in short- and long-run, while the automatic referrals enhance this rate by 50% in the first 2 months and double it in the long-run.

JEL Classification: C41, J63, J64, J65, J68

Keywords: Vacancy referral, Active labor market policy, Evaluation, Timing-of-events method, Unemployed, Transition to employment

1 Introduction

This research investigates to which extent the public employment service (PES) can raise the transition rate to employment by referring vacancies to unemployed job seekers. We study this question on a random sample of workers who became unemployed during 2007 in Flanders, Dutch speaking region in the North of Belgium. The referral of job seekers to vacancies is a policy that is used by many countries. The referral of vacancies is an active labor market policy (ALMP) that aims at enhancing search efficiency and effort and therefore belongs to so-called category of "services and sanctions." In a meta-analysis summarizing research on the effectiveness of active labor market policies (ALMPs), Card et al. (2010) conclude that services and sanctions is a particularly promising category of ALMPs, because, on the whole, they raise the transition rate to employment significantly, while being at the same time relatively inexpensive. Yet, this category is wide and comprises not only referrals but also many different actions, such as job search courses, job clubs, vocational guidance, counseling and monitoring, and sanctions in case of non-compliance with job search requirements. So, based on this meta-analysis, we cannot draw any firm conclusions regarding the effectiveness of referrals.

Specific analysis on the effectiveness of referrals is relatively rare. Based on a randomized experiment in Sweden, Engström et al. (2012) conclude that a large fraction (one third) of job referrals do not result in job applications. If the PES announces that it will contact the employer to verify whether referred vacancies have been applied to, the job application rate increases. However, the policy does not affect unemployment duration. Moreover, van den Berg and Vikström (2014) argue that verifying whether referred jobs have been applied to and are accepted or not can downgrade the quality of the job.

Fougère et al. (2009) study whether or not in France vacancy referral provided by the PES crowds out the personal job search effort of the unemployed worker. Such crowding out could explain why vacancy referrals do not automatically boost the job-finding rate. Van den Berg and van der Klaauw (2006), for instance, find that in The Netherlands, the monitoring of formal job search crowds out informal job search. By contrast, Fougère et al. find that in France, contacts brought about by the PES are more often transformed into a hiring proposal vacancy than private search, especially for the low-educated and low-skilled workers. Hence, in France, vacancy referrals enhance the exit rate from unemployment, especially for disadvantaged workers, even if application to these jobs was neither monitored nor, consequently, sanctioned.

Van den Berg et al. (2014) investigate the effects of repeated meetings between the unemployed and their caseworker on the transition rate from unemployment to employment in Denmark. They find large positive effects of the meetings. The authors argue that the strong increase of the job-finding rate right after the meetings is explained by the intensified referrals to vacancies during these meetings. However, the authors find that the positive effects of these meetings remain present up to 8 weeks later. For women, this effect even persists for a longer period, be it at a lower level. These long-run effects could not only be generated by more effective job search strategies that these interactions with caseworkers induce but also be related to the intensified monitoring after the meetings.

In Germany, a refusal to apply to a vacancy referral can be punished by an UB sanction. Van den Berg et al. (2016) analyze the effects of these sanctions and of the vacancy referrals on unemployment duration and job quality. Their results suggest that sanctions increase the probability of finding a job but that the wages of sanctioned individuals are lower in the subsequent jobs. Receiving a vacancy referral not only has a positive effect on the job finding probability but also leads to less stable employment spells and lower wages. Vacancy referrals have a stronger impact on the probability of finding a job if the local unemployment rate is high. However, the authors also find an increased sickness absence shortly after vacancy referrals by caseworkers (during sickness spells, the minimum requirements on job search do not apply).

Given that studies on this topic are scarce, additional research evidence on the topic is welcome. Moreover, the operational features of the referral procedures in other countries differ from those in Flanders. For instance, in France, the application to job referrals is not mandatory, whereas in Germany, this is mandatory and sanctioned. Since these operational features can affect the effectiveness of the scheme, it is important to gather more evidence on different schemes, so that the extent to which these features matter can be studied in a more systematic way. This study aims at providing a contribution to this evidence.

We organize the presentation of this research in the following way. We first describe the institutional context within which the Flemish PES transmits referrals to unemployed job seekers. In Section 3, we describe the data that we use in the empirical analysis. Section 4 presents the methodology which is used to identify the causal impact of referrals on the transition rate from unemployment to employment. We follow up by a presentation and a discussion of the results. We end by a conclusion which summarizes the findings and the implications that we can draw from them and which makes suggestions for future avenues of research.

2 Institutional context

In general, if a Belgian worker loses his job, he will be entitled to UBs, provided that he contributed to unemployment insurance (UI) while he was working. Eligibility depends on the length of the previous employment spell. This length increases with age: whereas someone below 36 should have worked 12 months during the previous 18 months to be eligible, unemployed between 36 and 49 should have worked 18 months in the previous 27 months, and someone who is 50 or older should have worked 24 months within the previous 36 months. The level of unemployment benefits (UB) in Belgium depends on the last wage, elapsed unemployment duration, on family status, and on age. The benefits are paid without time limit.

In order to remain eligible for UB, the unemployed may not turn down a so-called "suitable" job offer. According to the law, under some strict conditions, job offers are not suitable. This is, e.g., the case if one has to commute daily more than 4 h or if job acceptance leads to income loss. A third principle applies only during the first 6 months of the unemployment spell; it states that a job offer is not suitable if the requirements do not relate to the professional skills acquired by the unemployed.

Unemployed persons who turn down suitable job offers run a risk of obtaining an UBUB sanction: a temporary or permanent reduction, or a withdrawal of their UB. UB sanctions can also result from refusing participation in vocational training, fraud, and undeclared work. Since 2004, the long-term unemployed are regularly submitted to evaluations of their job search efforts. Non-compliance can give rise to an UB sanction.

Belgium is institutionally organized according to a multi-layered federal system. Over the course of several decades, a series of constitutional reforms have devolved ever more powers to the regional authorities (both regions and communities). The UBs are paid out by the RVA/ONEM, a federal institution, i.e., at the level of the country as a whole. This institution is also the sanctioning authority in case of non-compliance to the rules. On the other hand, the Regional PES is competent for ALMP's and the matching of labor demand and supply.

Given this division of tasks, non-compliance with eligibility requirements, such as a refusal to accept a suitable job or to participate in a vocational training, typically will be detected by the regional PES. In that case, the PES can report this to the federal RVA/ONEM which accordingly will decide whether or not an unemployment sanction is applicable. In Flanders, the regional PES is called VDAB. In the year 2007, the VDAB reported 32,615 cases of non-compliance with eligibility requirements to the RVA/ONEM. To put this number into perspective, in 2007, there were on average 143,035 unemployed persons entitled to UBs. In 44% of the reported cases, a sanction was imposed.

The VDAB keeps a register with information (age, education, place of residence, work experience, job preferences, etc.) about the unemployed persons. At the same time, the VDAB maintains a database with the available job vacancies. Both databases are regularly compared to find suitable matches between the competencies and job preferences of the unemployed individuals and the job requirements in the vacancies. These matches can subsequently be transmitted to the unemployed according to two procedures. In the *notification procedure*, an unemployed person is informed that a (potentially) adequate match has been found for him or her. The unemployed person is, however, not required to respond to the notification. In the *referral procedure* (which is the subject of this paper), more commitment is imposed. Here, the aforementioned standardized matching between job characteristics and the unemployed can be complemented or replaced by a matching that is based on the appreciation of caseworkers. Upon referral, application to the vacancy is compulsory. Non-compliance can result in a sanction, such as a reduction or temporary withdrawal of the UB.

Bollens and Heylen (2009) studied the effectiveness of the *notification procedure* for new entrants in unemployment. After controlling for selection on observables in a propensity score matching approach, the notification was found to have no effect on the transition rate from unemployment to employment. The literature provides two possible explanations for this finding: (i) the high standardization of the notification procedure may lead to a low-quality match and (ii) the notification procedure is not compulsory, so that the positive "threat" effect of a sanction, present in case of non-compliance in a mandatory scheme, is lacking.

The referral procedure is clearly different in the last two mentioned respects: (i) vacancy referrals are not completely standardized and automated, since caseworkers appreciate the adequacy of the match and (ii) the application to the referred vacancy is mandatory. This justifies investigating whether, in contrast to the notification procedure, the referral procedure does positively affect the transition rate from unemployment to employment.

In fact, the referral procedure is not one approach, but rather a collection of several related, but different approaches. This can be seen in Fig. 1. A first important distinction has to do with the question whether there is caseworker intervention or not. In the year 2007, some referrals were sent to the unemployed without any caseworker intervention. These so-called automatic referrals, based on matching software, are akin to the notification procedure. As with the notifications, one can expect a low quality of the match between the requirements of the referred vacancy and the characteristics of the unemployed worker. An obvious difference with the notifications, however, is that the unemployed who receives this referral, has to act on it. In recent years (after 2010), the automatic referrals have become quantitatively less important, as the PES considered them to be less efficient.

In a second type of referrals, caseworker intervene. We distinguish within this type between the direct and the indirect approach. In the direct approach, the caseworker refers the unemployed to a given vacancy by e-mail or by phone. In the indirect approach, the starting point of the caseworker is again the automatic match between a vacancy and an unemployed that was generated by the software, but instead of sending this referral, she invites the unemployed to the office in order to discuss the

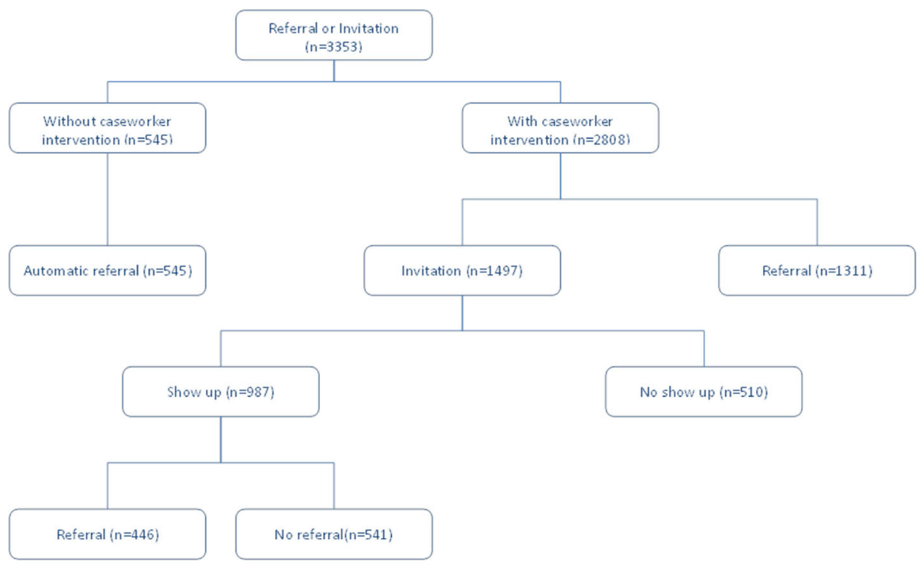

Fig. 1 The referral approach. Note: *n* refers to the number of individuals who are observed in the stated treatment within the 10% random sample of the population of individuals older than 25 years starting an unemployment spell in the year 2007 in Flanders. This sample corresponds to the one used in the econometric analysis

appropriateness of the match. Depending on show-up and, in case of show-up, depending on the outcome of this meeting, either a referral is transmitted or not.

The mandatory nature of the referral, i.e., the requirement to apply to the referred vacancy and to show up at the PES-meeting to which one is invited, is obviously an essential feature of these referral procedures. Therefore, it is important to know whether all concerned unemployed workers are fully aware of this mandatory character and whether compliance with these obligations is monitored and sanctioned. On the basis of an internal survey of the VDAB, one can conclude that the aforementioned obligations are communicated to the unemployed, although not systematically in a formal prescription. Generally, one may however presume that the referred unemployed indeed are aware of the mandatory nature. However, the PES does not closely follow up whether the unemployed effectively applies for the referred vacancy. In the year 2007, the VDAB gathered this information for barely 25% of all referrals. For the remaining 75%, this is not monitored. This is not actively followed up, because the caseworkers of the PES want to maintain a good relationship with employers and, hence, aim at minimizing the administrative burden for employers related to checking and informing the VDAB whether a particular individual applied for the vacancy. This lack of information also implies that the VDAB does not systematically report non-compliance to the RVA/ONEM. The aforementioned internal survey indicates that such reporting does occur, but rather occasionally than systematically.

3 Data

We use data from the unemployment register of the VDAB. The dataset contains monthly records of each individual as from their registration in unemployment. It reports the labor market state (unemployed, employed, or inactivity) occupied at the end of each calendar month. However, once unemployment is left, the labor market position after the month of exit remains unknown until the individual reenters unemployment.

We have data that cover the periods August 1995 to September 2010. Among these, we selected individuals who entered unemployment in 2007 and who were benefit recipient from the start of this spell. We chose 2007 as starting point both to have a sufficiently long observation window and because the referral system was different prior to 2007. From this selection, we excluded voluntary registered job seekers (e.g., those who were previously inactive and decided to start working again), as well as school-leavers, because school-leavers are only entitled to UBs after some initial waiting period.[1] In order to further enhance the homogeneity of the sample, we removed all individuals younger than 25 at the start of the unemployment spell.[2]

The unemployment duration is defined as the time until employment has been found. We observe transitions to employment but do not have any further information about this job. Unemployment spells that are still ongoing at the end of the period covered by the dataset, i.e., at the end of September 2010, are right censored at that point. There is also right censoring in case of a transition from unemployment to inactivity. The spell of someone who participates in training is right censored at the start of the training program.

In another database, the PES collects information with respect to the *treatments* (either a referral, an automatic referral, or an invitation). We know the exact date at which an individual was referred to a(n) (automatic) vacancy or was sent an invitation. For each unemployment spell that started in 2007, we checked whether a treatment had been administered before the end of that spell. When an individual receives more than one treatment during the course of the unemployment spell, only the first occurrence is selected. This can either be a referral, an invitation, or an automatic referral. If a second treatment occurs at a later duration, the unemployment duration is right censored at that point. The duration until the referral is offered is defined as the time from the start of the unemployment spell until the beginning of the first of the three possible treatments. For someone who does not receive a treatment, this duration is right censored when the person makes a transition to employment, inactivity or training, or at the end of the observation period, whichever comes first.

These selection criteria gave rise to a sample of 129,305 spells starting in 2007. To speed up the estimation, a random sample of 10% (12,983 individuals) was selected. Given the large population size, this did not induce an excessive loss in precision of the estimates. Table 1 provides some descriptive statistics of the explanatory variables. These are all measured at the beginning of the unemployment spell, except for the local unemployment rate (not included in Table 1), which varies on a monthly basis.[3] This time-varying local unemployment rate is not depersonalized so that it captures not only the business cycle but also the seasonal effects.

Nearly 26% of the unemployment spells starting in 2007 received a treatment. When comparing the spells with and without treatment, some selection on observables can be noticed, but in general, differences between the two groups tend to be small. Those who received a treatment are on average slightly older, and males have a slightly higher likelihood of being treated. With respect to the educational attainment, the differences are somewhat more marked: whereas the lower skilled (no secondary degree) are more likely to be treated, those with a tertiary degree are less likely to be included in the

Table 1 Summary statistics

Variables	All	Treated	Not-treated	Referral	Invitation	Automatic referrals
	$a = b + c$	$b = d + e + f$	c	d	e	f
N	12,983	3353	9630	1311	1497	545
Gender (woman = 1)	0.500	0.475	0.509	0.498	0.455	0.475
Age (in years)	36.9	37.5	36.7	37.5	37.7	37.2
25–40	0.626	0.599	0.635	0.593	0.609	0.589
40–50	0.257	0.271	0.252	0.296	0.234	0.314
50+	0.117	0.129	0.113	0.111	0.157	0.097
# months unempl. in the preceding 2 years	6.5	6.9	6.3	7.4	6.6	6.9
Education level						
No secondary degree	0.411	0.463	0.393	0.449	0.463	0.499
Secondary degree	0.361	0.359	0.362	0.349	0.374	0.343
Tertiary degree	0.228	0.177	0.245	0.202	0.163	0.158
Tertiary (outside university)	0.170	0.141	0.179	0.158	0.136	0.117
Tertiary (university)	0.058	0.036	0.066	0.044	0.027	0.040
Educational track[a]						
General track	0.090	0.090	0.090	0.087	0.088	0.106
Technical track	0.166	0.165	0.166	0.157	0.177	0.152
Vocational track[b]	0.278	0.298	0.271	0.294	0.311	0.273
Province of residence						
Antwerpen	0.296	0.306	0.293	0.240	0.359	0.316
Vlaams Brabant	0.150	0.154	0.148	0.146	0.169	0.130
West Vlaanderen	0.165	0.162	0.166	0.196	0.126	0.180
Oost Vlaanderen	0.245	0.218	0.254	0.285	0.168	0.194
Limburg	0.144	0.160	0.139	0.132	0.178	0.180
Driving license	0.795	0.781	0.799	0.755	0.813	0.754
Mother tongue = Dutch	0.797	0.782	0.802	0.770	0.819	0.712
Belgian	0.891	0.887	0.892	0.878	0.914	0.837

[a]The educational track is reported among pupils who started (but not necessarily completed) secondary school and who did not obtain any tertiary degree
[b]The small arts track is included in the vocational track

referral procedure. This observation may be related to the dynamic sorting process: high-educated unemployed workers will generally find employment sooner than low-educated unemployed and are therefore less likely to be treated.

The educational track is chosen at the start of secondary education. Table 1 only reports the shares of the three most important tracks, irrespectively of whether the student graduates from secondary school or not. The general track provides a primarily theoretical and general preparation for tertiary education. The technical track consists of a mix of theoretical and practical classes aiming at both direct labor market entry or entry in primarily technical tertiary education. The vocational track teaches practical skills that prepare for particular professions. In the data, it also includes a small arts track that combines general education with active arts practice.

The dataset covers the period August 1995 until September 2010. This implies that it is possible to control for the recent labor market history (before the current unemployment spell), at least if the person has been in unemployment recently. As suggested by

Heckman et al. (1997), and Blundell et al. (2004), the recent labor market history can be a crucial component in a non-experimental evaluation, as it is possibly correlated with non-observed characteristics that are driving the employability of the person (assuming that this relation is stable over time). This is approximated by a variable measuring the number of months in unemployment within 2 years preceding the entry in the unemployment spell that is retained for analysis. Table 1 indicates that there is a difference between the treated and the non-treated, but the difference is rather small. The right hand side of Table 1 compares the three different treatment types. The automatic referrals are quantitatively less important than referrals and invitations.

4 Econometric modeling

To estimate the impact of the treatment on the transition rate to employment, the labor market outcomes of the treated group and the control group are compared. As participation is possibly selective, meaning that the composition of treatment and control groups may differ both in terms of observed and unobserved characteristics, different observed outcomes for these groups do not necessarily reflect a causal impact of the treatment but could also reflect these compositional differences. Over and above this classical selection problem, we have to take into account a dynamic selection problem. Since the more employable workers leave unemployment on average sooner, it less likely that they enter treatment.

To solve these problems, we control for both observed and unobserved fixed differences between the treated and control group. Selection on observables is taken into account by conditioning the hazard rates, within a "proportional hazard specification," on the explanatory variables mentioned in Table 1. Selection on unobserved characteristics is taken into account by making use of the timing-of-events approach (Abbring and van den Berg 2003, 2004). This method exploits the fact that the unobserved heterogeneity affects the transition to regular employment throughout the unemployment spell, whereas the treatment may only influence this transition from the moment at which the treatment occurs. Since the treatment and the outcome typically succeed each other quickly, it is possible to distinguish between the treatment effect and the selection effect without imposing any "exclusion restrictions" on the observed explanatory variables. In what follows, we formally specify the econometric model and discuss the identification of the treatment effect.

4.1 The econometric model

The timing-of-events approach involves estimating a competing-risks duration model in which transition rates are proportional to the observed and unobserved explanatory variables, denoted X and $V = (V_r, V_e)$, respectively. In what follows, the index r refers to the treatment and the index e refers to regular employment.[4] The observed explanatory variables X and the unobserved variable V are independently distributed. In this model, transitions to the treatment and to regular employment are represented by two random latent continuous durations, T_r and T_e, with t_r and t_e denoting their realizations. The joint distribution of T_e, $T_r|X$, and V is expressed as the product of the following conditional distributions: $T_r|X = x, V_r$ and $T_e|T_r = t_r, X = x, V_e$. These distributions are in turn completely determined by the corresponding hazard rates $\theta_r(t|x, V_r)$ and $\theta_e(t|t_r, x, V_e)$, where t is the elapsed duration in unemployment ($t = 0$ at the start of the unemployment spell). We are interested in the causal effect of t_r on the transition rate to regular employment $\theta_e(t|t_r, x, V_e)$.

Since we cannot observe V, further assumptions are required for the identification of the causal impact of the treatment. The main identification problem arises because treated individuals are not randomly selected from the population. If the unobserved determinants of the transition to the treatment and to regular employment, V_r and V_e, are dependent, then the distribution of V_e among the treated group cannot be equal to the population distribution. Participants will on average have high values of V_r and, given the dependence, have values of V_e that differ from those of the nonparticipating population. When the correlation is positive, participants with a high value for V_e, i.e., persons with a high propensity to leave unemployment, will on average have a high value for V_r, meaning they will tend to obtain a treatment rather early in their unemployment spell, whereas person with a low value for V_e, whom we expect to remain longer in unemployment, will on average have less chances to obtain a treatment. A positive or negative correlation therefore implies that participation will be selective and that the treatment effect will be, respectively, over- or underestimated.

A second reason for selection on V_e is dynamic sorting: in order to get treated, individuals may not have left unemployment for a regular job before t_r and must therefore have relatively low values of V_e in comparison to the sampled population. Abbring and van den Berg (2003) show under which assumptions one can identify the true causal effect of the treatment from the spurious effect induced by the aforementioned selection effects. We discuss these assumptions in Section 4.2.

We now turn to the specification and derivation of the likelihood function. The hazards are specified in the following mixed proportional (MPH) form:

$$\theta_e(t|t_r, x, V_e) = \lambda_e(t).\exp[x'\beta_e + \delta(t|t_r, x).I(t > t_r) + V_e] \tag{1}$$

$$\theta_r(t|x, V_r) = \lambda_r(t).\exp(x'\beta_r + V_r) \tag{2}$$

where $\lambda_r(t)$ and $\lambda_e(t)$ represent the baseline hazard for transitions to the treatment and to regular employment, respectively, and $I(.)$ is an indicator function, equal to 1 if the argument is true and to 0 otherwise. Consequently, $\delta(t|t_r, x)$ measures the impact of a transition to the treatment on the transition to regular employment. This impact may vary with the elapsed unemployment duration t, with the starting time of the treatment t_r and with x. Consequently, the treatment effect may also depend on the elapsed time since the treatment. However, $\delta(t|t_r, x)$ cannot depend on an unobserved covariate. We will discuss the consequence of this restriction in Section 4.2.

In our basic model, we distinguish between three different treatment types: a referral, an invitation, and an automatic referral. It is assumed that these treatment types are the outcome of a similar selection process. Therefore, only one selection equation has to be specified. The three treatments enter the employment hazard as follows:

$$\theta_e(t|t_r, x, V_e) = \lambda_e(t).\exp[x'\beta_e + \delta_k(t|t_r, x).I(t > t_r) + V_e] \text{ with k} = 1, \ldots, 3 \tag{1'}$$

When an individual receives more than one treatment during the course of the unemployment spell, only the first occurrence will be selected. This can either be a referral, an invitation, or an automatic referral. If a second treatment occurs at a later moment, the unemployment duration is right censored at that point. For each of the three treatment types, we distinguish between the immediate effect and the long-term effect (van den Berg et al. 2014). The immediate effect relates to the month during which the treatment was obtained and the subsequent month. The long-term effect relates to all later months in the unemployment spell.

In order to examine whether the treatment effect is heterogeneous, we also present a more elaborate model in which we interact the treatment indicator with a limited number of the observed explanatory variables x. We allow the treatment to depend on (1) the elapsed unemployment duration at the start of the treatment,[5] (2) the level of education (having a tertiary degree or not), (3) the age[6], (4) the gender, and (5) the local unemployment rate at the moment of the treatment. These interactions are interesting from a policy perspective.

In our data, we do not measure time continuously, but on a monthly basis. This time grouping has consequences for identification, which we discuss in Section 4.2. The time grouping is explicitly taken into account in the specification of the baseline hazard and of the likelihood function. We exploit the fact that the exact date of treatment is known: in a month in which an individual is treated, the fraction of the month before the treatment and the remaining fraction of the month, starting on the day of the treatment, can be observed (see the Appendix section for details).

To take the time grouping into account, the baseline hazard is specified as piecewise constant. For both hazards, the time line is divided in 12 intervals of different length: month 2 (the first month is not observed), month 3, month 4, month 5, month 6, month 7, month 8, months 9–10, months 11–12, months 13–16, months 17–28, and months 28–45.

As very short spells of persons who enter and leave unemployment in the same month (either with or without treatment) are not observed, we have to take into account that all persons in the observed sample survived the inflow month. Therefore, the likelihood must be written conditional on surviving the first month, i.e., conditional on neither treatment nor exit to employment in the first month (see the Appendix section for details).

The model is estimated by maximum likelihood. We distinguish between five types of likelihood contributions: (1) l_1 for individuals who neither got treated nor exited to employment. These observations are right censored in both durations at $t_{(m-1)}$[7]; (2) l_2 for individuals who leave for employment within $[t_{(m-1)}, t_m)$, with $m > 1$, without having been treated; (3) l_3 for individuals who are treated within $[t_{(k-1)}, t_k)$, but who remain in unemployment and are right censored at $t_{(m-1)}$; (4) l_4 for individuals who are treated within $[t_{(k-1)}, t_k)$ and leave towards employment in $[t_{(m-1)}, t_m)$, with $m > k$; and (5) l_5 for individuals who are treated within $[t_{(k-1)}, t_k)$ and leave towards employment in $[t_{(m-1)}, t_m)$, with $m = k$. We derive these likelihood contributions by explicitly taking the monthly grouping of the data into account. In a first step, we derive these likelihood contributions conditional on the unobserved covariates V (see the Appendix section for the details of this derivation). Subsequently, we derive the unconditional likelihood contributions by integrating V out:

$$l_s = \int_V [l_s(V)/D_0(V)]\mathrm{d}G(V) \qquad \text{for } s = 1, \ldots, 5 \tag{3}$$

where $G(V)$ is the joint distribution function of the unobserved heterogeneity terms and D_0 is the conditioning event taking into account that there is neither treatment nor exit to employment in the first month. Gaure et al. (2007) show that in order to get unbiased estimates, one has to specify the heterogeneity distribution correctly. In order to do so, we implement a non-parametric approximation of the heterogeneity distribution (Lindsay 1983; Heckman and Singer 1984). The distribution of unobservables is approximated by a discrete mixture distribution with an unknown number of mass points. We assume that the vectors of unobserved attributes (ν_{ri}, ν_{ei}) are jointly discretely distributed. The number of mass points is determined by adding consecutively mass points as long as the AIC decreases (Gaure et al. 2007). We used the BHHH algorithm to maximize the likelihood.

4.2 Identification of the treatment effect

Abbring and van den Berg (2003) showed that $\delta(t|t_p, x)$ is non-parametrically identified for single-spell data provided that:

Assumption (1): Agents do not anticipate the starting date of the treatment. They may however know the distribution of this moment, implying that the unemployed workers are allowed to know in advance that a referral or invitation can arrive at each moment, as long as they do not know the exact timing of the future arrival.

Assumption (2): The econometrician has sufficiently precise information concerning the timing of transitions.

Assumption (3): Observed and unobserved individual characteristics influence the rates of transitions (to treatment and to regular employment) of untreated individuals proportionally.

Assumption (4): The treatment effect may not be heterogeneous in the unobserved characteristics of participants.

Assumption (5): There are at least two nonlinearly dependent continuous explanatory variables.

Assumption (6): Variables X and V are independently distributed.

Assumption (7): There are no unobserved random shocks correlated with the timing of the treatment.

Let us discuss these assumptions in turn.

Assumption (1). If workers anticipate the starting date of the treatment, then they could use this information to modify their behavior accordingly. If this was the case, then these individuals should be considered as treated from the moment that they change their behavior. Considering these workers as members of the control group would bias the treatment effect, because anticipation could occur, e.g., if a worker knows that she will receive a referral in the near future and therefore reduces here present job search intensity. As both referrals and automatic referrals arrive unannounced, no anticipation bias is to be expected. For invitations, the situation is more complex. With an invitation, the unemployed worker is invited to attend to a meeting at the PES at a later date. These meetings can result in referrals. For these referrals, obviously, there can be an anticipation problem. In order to avoid this problem, we chose the date at which the invitation itself was sent as the point at which the treatment was administered.

It is important to distinguish anticipation effects from ex-ante effects (Abbring and van den Berg 2004; Richardson and van den Berg 2013; van den Berg et al. 2009). An ex-ante effect occurs if the transition rate to regular employment of nonparticipants is affected by the mere existence of a treatment. The ex-ante knowledge of the existence of referrals and invitations may affect the distribution of transitions to work. For instance, if an unemployed worker wants to prevent to be invited for a meeting at the PES, he may change his search strategy by accepting job offers that he otherwise would not have accepted. Since the ex-ante effect concerns a spillover effect of the treatment on nonparticipants, it can be regarded as a specific general equilibrium effect of the treatment. In any case, given the relatively low burden imposed on unemployed workers by referrals and invitations, we expect these general equilibrium effects to be negligible. The analysis that we implement here identifies an ex-post effect. The ex-post effect measures, for a given environment with the policy in place, the effect of a referral or invitation on the individual transition rate to a regular job. This effect is identified even in the presence of ex-ante effects, as long as there is no anticipation.

Assumption (2). One could argue that this condition is not satisfied, since the duration data are grouped into months. However, using an extensive Monte Carlo analysis, Gaure et al. (2007) have shown that Abbring and van den Berg's (2003) method is extremely reliable for time-grouped data as long as the time grouping is explicitly taken into account in the formulation of the likelihood function. Since we implement a grouped duration version of the timing-of-events approach, we satisfy this requirement. The results of Gaure et al. (2007) suggest that the observed effects can be identified with time-grouped data. This means that the model is able to disentangle selection effects from treatment effects and will be able to predict the observed grouped duration outcomes correctly.

Assumption (3). The assumption of proportionality is fundamental. Gaure et al. (2007) have shown that strong departures from non-proportionality can induce serious biases. In principle, we could test for departures from the MPH assumption, since in the presence of a time-varying exogenous covariate, such as the unemployment rate in the current application, this assumption is no longer required for identification (Brinch 2007; Richardson and Van den Berg 2013). Testing for such specification problems is, however, beyond the scope of the current paper. Note that the MPH assumption is not required for the specification of the treatment effect $\delta(t|t_r, x)$: x may be related to the unemployment duration t or the elapsed duration since the start of the treatment $(t - t_r)$. This holds only, however, if the treatment effect does not vary with unobservable characteristics.

Assumption (4). In principle, we can allow for unobserved heterogeneity in the treatment effect if the transition rate of treated participants to regular employment is proportional in all three arguments (unemployment duration, observed and unobserved characteristics). This holds as long as this transition rate depends neither on the moment of entry into treatment nor on the period of time elapsed since that moment. Alternatively, Richardson and Van den Berg (2013) prove nonparametric identification of a model that allows for unobserved heterogeneity in the treatment effect if the last mentioned transition (i) is proportional in the period of time elapsed since entry into the program $(t - t_r)$, and in observed and unobserved characteristics, but (ii) does not depend on unemployment duration (t) nor on the moment since entry (t_r). Allowing for unobserved heterogeneity in the treatment effect would complicate the analysis drastically. We therefore maintain the assumption that the treatment effect is homogeneous with respect to unobservables. Consequently, we must take care in interpreting the time profile of the treatment effect with the time since the start of the treatment. Richardson and Van den Berg (2013) point out that this time profile may be biased downwards by a dynamic sorting effect: treated individuals with an unobserved high return to the treatment (holding other characteristics constant) are more likely to leave unemployment quickly.

Assumption (5). This is a technically sufficient condition for identification if there are no time-varying explanatory variables. We meet this requirement here, since age and the unemployment rate are two continuous explanatory variables. Note, however, that in our empirical application, this condition is not essential, since the model is overidentified by including the unemployment rate as a time-varying covariate. Using an extensive Monte Carlo analysis, Gaure et al. (2007, p. 1186) show that with "some exogenous variation in hazard rates over calendar time, no subject-specific covariates are required in order to identify treatment and spell-duration effects."

Assumption (6). It is unlikely that unobservable and observable covariates are independent of each other. However, a violation of this assumption need not affect the consistency of our main parameter of interest, δ. In this case, it only means that we can no longer give a structural interpretation to the coefficients of the observed covariates, x (see Chamberlain 1980; Wooldridge 2002, p. 487; Crépon et al. 2005, p. 14; for a similar argumentation in the context of transition models). To clarify this, we first consider Chamberlain's (1980) random-effects probit model in a panel setup. This model allows for correlation between the unobserved effect and the explanatory variables by assuming that the conditional distribution of the unobserved effect is normal with a conditional expectation that is a linear index in the observed explanatory variables. With these assumptions, we can identify the structural parameters associated with the time-varying covariates. The parameters associated with the time-constant covariates, however, cannot be identified from the linear conditional expectation of the unobserved covariate. In the context of transition models, one can make a similar assumption. For instance, assume that the unobserved heterogeneity terms conditional on observed covariates x can be written as follows:

$$v_{xjk} = v_{jk}\exp(x^{'}\gamma_j) \text{ for } j = e, r \text{ and } k = 1, 2 \tag{4}$$

where v_{jk} does not depend on x. With this assumption, it is clear that γ_j ($j = e$, r) cannot be disentangled from the structural parameters β_j. However, this does not affect the consistency of the parameters of interest characterizing the treatment effect $\delta(t|t_r, x)$. In principle, this treatment effect may even depend on x, as long as the treatment effect itself does not depend on unobservables—as discussed under assumption (4). Finally, this argument holds only to the extent that the unobserved terms are related to x as expressed in Eq. 4. Such an assumption is, however, not stronger than the one required for the consistency of the widely used Chamberlain's (1980) random-effects probit model.

Assumption (7). This assumption is not explicitly imposed in Abbring and Van den Berg (2003, 2004) but is implicit in the model. We try to avoid seasonal or business cycle shocks that could be correlated with the start of the treatment by conditioning on a time-varying indicator of the local unemployment rate.

5 Results and discussion

The estimation results are reported in Tables 2, 3, and 4. Table 2 reports the estimates of the transition to treatment and Table 3 those of the transition towards employment.

Table 2 Duration model estimates: transition to treatment

Variables	No unobserved heterogeneity				Unobserved heterogeneity: base model				Unobserved heterogeneity: model with interactions			
	b	e^b-1	s.e.	p val.	b	e^b-1	s.e.	p val.	b	e^b-1	s.e.	p val.
Constant	− 2.83	− 0.94	0.12	0.000	− 2.94	− 0.95	0.18	0.000	− 2.98	− 0.95	0.20	0.000
Gender (reference man)	− 0.18	− 0.16	0.04	0.000	− 0.17	− 0.16	0.04	0.000	− 0.18	− 0.16	0.04	0.000
Age	− 0.01	− 0.01	0.00	0.005	0.00	0.00	0.00	0.133	− 0.01	− 0.01	0.00	0.106
Age squared/100	− 0.09	− 0.08	0.02	0.000	− 0.09	− 0.09	0.02	0.000	− 0.09	− 0.08	0.02	0.000
# months unempl. in the preceding 2 years	0.01	0.01	0.00	0.000	0.01	0.01	0.00	0.000	0.01	0.01	0.00	0.000
Educational level												
No secondary	0.54	0.72	0.10	0.000	0.56	0.75	0.10	0.000	0.56	0.75	0.10	0.000
Secondary	0.41	0.51	0.11	0.000	0.43	0.53	0.11	0.000	0.43	0.53	0.11	0.000
Tertiary education (outside university)	0.33	0.39	0.10	0.001	0.34	0.40	0.10	0.001	0.34	0.41	0.10	0.001
Tertiary (university) (reference)												
Educational track (if secondary level)												
General track	− 0.09	− 0.08	0.08	0.275	− 0.08	− 0.08	0.08	0.325	− 0.08	− 0.08	0.08	0.306
Technical track	0.02	0.02	0.06	0.769	0.01	0.01	0.07	0.856	0.02	0.02	0.07	0.808
Vocational track	0.11	0.11	0.06	0.052	0.12	0.13	0.06	0.034	0.12	0.12	0.06	0.037
Province of residence												
Antwerp (reference)												
Vlaams Brabant	− 0.16	− 0.15	0.07	0.022	− 0.13	− 0.12	0.08	0.098	− 0.14	− 0.13	0.08	0.075
West Vlaanderen	− 0.12	− 0.11	0.07	0.070	− 0.11	− 0.11	0.07	0.093	− 0.12	− 0.11	0.07	0.084
Oost Vlaanderen	− 0.26	− 0.23	0.05	0.000	− 0.25	− 0.22	0.05	0.000	− 0.26	− 0.23	0.05	0.000
Limburg	0.19	0.20	0.05	0.001	0.19	0.21	0.05	0.001	0.19	0.21	0.05	0.001
Driving license	0.04	0.04	0.04	0.327	0.03	0.03	0.05	0.592	0.03	0.03	0.05	0.485
Mother tongue = Dutch	0.14	0.15	0.05	0.005	0.12	0.13	0.05	0.019	0.13	0.13	0.05	0.021
Belgian	0.11	0.12	0.06	0.070	0.11	0.12	0.06	0.068	0.11	0.12	0.06	0.070
Regional unemployment rate (time varying)	− 0.10	− 0.09	0.02	0.000	− 0.09	− 0.08	0.02	0.000	− 0.09	− 0.09	0.02	0.000
Baseline hazard												
Months 28–45	− 1.90	− 0.85	0.24	0.000	− 2.04	− 0.87	0.25	0.000	− 2.01	− 0.87	0.26	0.000
Months 17–28	− 1.35	− 0.74	0.12	0.000	− 1.49	− 0.77	0.14	0.000	− 1.46	− 0.77	0.15	0.000
Months 13–16	− 0.71	− 0.51	0.10	0.000	− 0.83	− 0.56	0.12	0.000	− 0.81	− 0.56	0.13	0.000
Months 11–12	− 0.36	− 0.30	0.10	0.000	− 0.46	− 0.37	0.11	0.000	− 0.45	− 0.36	0.11	0.000
Months 9–10	− 0.59	− 0.44	0.09	0.000	− 0.69	− 0.50	0.10	0.000	− 0.68	− 0.49	0.11	0.000
8th month	− 0.22	− 0.20	0.09	0.014	− 0.31	− 0.27	0.10	0.002	− 0.30	− 0.26	0.10	0.004
7th month	− 0.59	− 0.45	0.09	0.000	− 0.67	− 0.49	0.10	0.000	− 0.66	− 0.49	0.10	0.000
6th month	− 0.43	− 0.35	0.08	0.000	− 0.51	− 0.40	0.09	0.000	− 0.50	− 0.39	0.09	0.000
5th month	− 0.38	− 0.31	0.07	0.000	− 0.44	− 0.36	0.07	0.000	− 0.44	− 0.35	0.08	0.000
4th month	− 0.18	− 0.17	0.06	0.002	− 0.24	− 0.21	0.06	0.000	− 0.23	− 0.21	0.06	0.000
3rd month	− 0.08	− 0.07	0.05	0.127	− 0.11	− 0.11	0.05	0.029	− 0.11	− 0.10	0.05	0.035
2nd month (reference)												

The variables age and the regional unemployment rate are centered around their mean

Table 3 Duration model estimates: transition to employment

Variables	No heterogeneity				Unobserved heterogeneity: base model				Unobserved heterogeneity: model with interactions			
	b	e^b-1	s.e.	*p* val.	*b*	e^b-1	s.e.	*p* val.	*b*	e^b-1	s.e.	*p* val.
Constant	− 1.28	− 0.72	0.06	0.000	0.25	0.29	0.32	0.433	0.23	0.26	0.59	0.697
Gender (reference man)	− 0.09	− 0.09	0.02	0.000	− 0.54	− 0.41	0.11	0.000	− 0.45	− 0.36	0.17	0.009
Age	− 0.02	− 0.02	0.00	0.000	− 0.13	− 0.12	0.01	0.000	− 0.12	− 0.11	0.01	0.000
Age squared/100	− 0.05	− 0.05	0.01	0.000	− 0.24	− 0.21	0.06	0.000	− 0.22	− 0.20	0.07	0.001
# months unempl. in the preceding 2 years	0.00	0.00	0.00	0.235	− 0.01	− 0.01	0.01	0.149	− 0.01	− 0.01	0.01	0.429
Educational level												
No secondary	− 0.25	− 0.22	0.05	0.000	− 1.09	− 0.66	0.24	0.000	− 1.24	− 0.71	0.31	0.000
Secondary	− 0.19	− 0.17	0.06	0.001	− 0.89	− 0.59	0.28	0.001	− 0.97	− 0.62	0.31	0.002
Tertiary education (outside university)	− 0.06	− 0.06	0.05	0.180	− 0.32	− 0.27	0.23	0.162	− 0.41	− 0.34	0.24	0.083
Tertiary (university) (reference)												
Educational track (if secondary level)												
General track	− 0.08	− 0.07	0.05	0.130	− 0.50	− 0.39	0.26	0.052	− 0.45	− 0.36	0.33	0.172
Technical track	0.02	0.02	0.04	0.648	0.16	0.18	0.19	0.381	0.06	0.06	0.24	0.802
Vocational track	0.05	0.05	0.04	0.195	− 0.09	− 0.09	0.18	0.629	− 0.04	− 0.04	0.23	0.853
Province of residence												
Antwerp (reference)												
Vlaams Brabant	− 0.21	− 0.19	0.04	0.000	− 1.47	− 0.77	0.22	0.000	− 1.30	− 0.73	0.25	0.000
West Vlaanderen	− 0.03	− 0.03	0.04	0.413	− 0.31	− 0.27	0.14	0.031	− 0.23	− 0.21	0.17	0.162
Oost Vlaanderen	− 0.06	− 0.06	0.03	0.037	− 0.36	− 0.30	0.15	0.014	− 0.26	− 0.23	0.15	0.090
Limburg	0.02	0.02	0.03	0.466	0.15	0.16	0.17	0.386	0.13	0.14	0.18	0.471
Driving license	0.22	0.24	0.03	0.000	1.50	3.50	0.19	0.000	1.29	2.64	0.25	0.000
Mother tongue = Dutch	0.25	0.29	0.03	0.000	1.22	2.39	0.14	0.000	1.27	2.56	0.18	0.000
Belgian	0.07	0.07	0.04	0.098	0.43	0.54	0.19	0.026	0.48	0.61	0.42	0.253
Regional unemployment rate (time varying)	− 0.08	− 0.08	0.01	0.000	− 0.50	− 0.39	0.03	0.000	− 0.51	− 0.40	0.03	0.000
Baseline hazard												
Months 28–45	− 2.70	− 0.93	0.16	0.000	2.29	8.85	0.29	0.000	1.96	6.09	0.31	0.000
Months 17–28	− 2.31	− 0.90	0.09	0.000	2.02	6.56	0.22	0.000	1.86	5.45	0.23	0.000
Months 13–16	− 1.63	− 0.80	0.07	0.000	2.02	6.56	0.19	0.000	1.90	5.71	0.19	0.000
Months 11–12	− 1.29	− 0.73	0.07	0.000	2.04	6.70	0.17	0.000	1.95	6.03	0.17	0.000
Months 9–10	− 1.17	− 0.69	0.06	0.000	1.86	5.41	0.14	0.000	1.78	4.93	0.15	0.000
8th month	− 1.02	− 0.64	0.06	0.000	1.75	4.78	0.13	0.000	1.68	4.39	0.13	0.000
7th month	− 0.99	− 0.63	0.06	0.000	1.56	3.77	0.12	0.000	1.50	3.50	0.12	0.000
6th month	− 0.92	− 0.60	0.05	0.000	1.36	2.88	0.10	0.000	1.31	2.70	0.10	0.000
5th month	− 0.84	− 0.57	0.04	0.000	1.14	2.11	0.08	0.000	1.10	2.00	0.09	0.000
4th month	− 0.60	− 0.45	0.04	0.000	0.99	1.69	0.07	0.000	0.96	1.62	0.07	0.000
3rd month	− 0.32	− 0.27	0.03	0.000	0.69	0.99	0.05	0.000	0.67	0.95	0.05	0.000
2nd month (reference)												
Effect of referral												
Month of referral and next month	0.55	0.74	0.05	0.000	1.12	2.07	0.11	0.000	1.14	2.11	0.21	0.000
Afterwards	0.30	0.35	0.07	0.000	1.12	2.08	0.16	0.000	1.12	2.05	0.25	0.000
Effect of invitation												
Month of invitation and next month	0.31	0.37	0.05	0.000	0.80	1.23	0.12	0.000	0.80	1.23	0.20	0.000
Afterwards	0.13	0.14	0.07	0.050	0.82	1.28	0.17	0.000	0.77	1.17	0.25	0.000

Table 3 Duration model estimates: transition to employment *(Continued)*

Variables	No heterogeneity				Unobserved heterogeneity: base model				Unobserved heterogeneity: model with interactions			
	b	$e^{b}-1$	s.e.	*p* val.	*b*	$e^{b}-1$	s.e.	*p* val.	*b*	$e^{b}-1$	s.e.	*p* val.
Effect of automatic referral												
Month of automatic referral and next month	0.06	0.06	0.09	0.521	0.41	0.51	0.15	0.006	0.42	0.53	0.22	0.059
Afterwards	0.24	0.28	0.09	0.007	0.70	1.01	0.19	0.000	0.71	1.03	0.26	0.007
Interaction with												
Unemployment duration when treated									− 0.01	− 0.01	0.03	0.806
Unemployment duration squared/100									0.14	0.15	0.10	0.156
Tertiary educational level									− 0.11	− 0.10	0.13	0.386
Age									0.00	0.00	0.01	0.915
Age squared/100									− 0.02	− 0.02	0.07	0.719
Gender (reference = man)									− 0.03	− 0.03	0.11	0.765
Unemployment rate in month of treatment									0.12	0.13	0.04	0.001
Unobserved heterogeneity												
Treatment 2					0.46	0.59	0.30	0.127	0.52	0.69	0.33	0.109
Employment 2					2.71	14.05	0.16	0.000	2.57	12.11	0.19	0.000
Masspoint 2					0.48	0.62	0.10	0.000	0.49	0.64	0.13	0.000
Treatment 3					0.33	0.39	0.24	0.161	0.31	0.36	0.29	0.290
Employment 3					− 8.14	− 1.00	0.36	0.000	− 7.84	− 1.00	0.38	0.000
Masspoint 3					− 1.66	− 0.81	0.18	0.000	− 1.61	− 0.80	0.22	0.000
Treatment 4					0.20	0.22	0.17	0.261	0.24	0.28	0.20	0.221
Employment 4					− 2.13	− 0.88	0.13	0.000	− 2.00	− 0.87	0.18	0.000
Masspoint 4					− 0.44	− 0.36	0.09	0.000	− 0.39	− 0.32	0.12	0.001
Treatment 5					0.01	0.01	0.15	0.954	0.06	0.06	0.18	0.750
Employment 5					− 4.05	− 0.98	0.20	0.000	− 3.89	− 0.98	0.23	0.000
Masspoint 5					− 0.55	− 0.42	0.10	0.000	− 0.44	− 0.36	0.15	0.003
Treatment 6					0.22	0.24	0.17	0.213	0.25	0.28	0.21	0.236
Employment 6					− 6.10	− 1.00	0.29	0.000	− 5.92	− 1.00	0.32	0.000
Masspoint 6					− 1.06	− 0.65	0.12	0.000	− 0.96	− 0.62	0.15	0.000
Probability 1					0.23				0.22			
Probability 2					0.37				0.36			
Probability 3					0.04				0.04			
Probability 4					0.15				0.15			
Probability 5					0.13				0.14			
Probability 6					0.08				0.08			

The variables age and the regional unemployment rate are centered around their mean

Table 4 contains summary information regarding some general model characteristics. Information about the unobserved heterogeneity distribution is included at the bottom of Table 3.

In each table information, about three different specifications can be found: (a) a base specification where no correction for unobserved heterogeneity is applied; (b) the same specification, but allowing for unobserved heterogeneity; and (c) the previous specification, allowing for several interaction effects between the treatment effect and specific

Table 4 Model characteristics

	No unobserved heterogeneity	Unobserved heterogeneity: base model	Unobserved heterogeneity: model with interactions
Log-likelihood	− 35,836.69	− 35,703.985	− 35,692.939
Number of variables	66	84	91
Number of observations	12,983	12,983	12,983
Akaike information criterion	71,805.38	71,575.97	71,567.88

explanatory variables. These interactions allow to check whether the treatment effects are heterogeneous in these selected observed dimensions.

We always report four elements: (1) "b," the estimated coefficients; (2) "exp(b) – 1," which measures the proportional change (relative to the reference category) in the exit rate to the treatment (Table 2) and to employment (Table 3)[8]; (3) "s.e.," the standard error of the estimated coefficient; and (4) "p val," the corresponding p value.

For each of the three different treatment types (i.e., referral, invitation, and automatic referral), two effects are estimated, an immediate effect and a long-term effect. The immediate effect is the change in the exit rate to employment at the end of the month in which the treatment was imposed (and in the following month). The results at the left hand side of Table 3 indicate that the immediate effect for the three different treatment types are respectively 0.55, 0.31, and 0.06, suggesting that the treatment effect is positive for all treatment types (albeit very small for the automatic referral). However, in this specification, selection on unobservables has not yet been taken into account, and as mentioned in the previous section, this can cause a bias with an a priori unknown sign. The unobserved heterogeneity terms at the bottom of Table 3 indicate that there is a strong negative correlation between the unobserved terms of both hazards, suggesting that persons who get treated are on average less employable than persons who do not get the treatment. This implies that a specification that does not control for unobserved heterogeneity underestimates the treatment effect. In what follows, we will therefore focus on the results of the specifications that do correct for selection on unobservables.

The results of both specifications that do correct for unobserved heterogeneity suggest that the immediate effects for the three treatment types are large and statistically significant. The immediate effect on the transition towards employment is consistently the largest for referrals, somewhat smaller for getting an invitation, and the smallest for the automatic referrals. The estimated effects appear to be very high: a referral increases the exit to employment (in the second specification) by 207%, getting an invitation changes the exit rate to employment by 123%, and an automatic referral still increases this exit rate by 51%. The fact that the treatment selects individuals who have a very low job-finding rate in the absence of treatment[9] explains why these effects are so large in proportional terms. More surprising is that these treatment effects persist afterwards at this high level and even double from 51 to 101% in case of the automatic referrals. In the following paragraphs, we try to provide an interpretation for these findings.

These large immediate effects of the referrals are not consistent with the crowding out of informal search methods by formal ones (Engström 2012; van den Berg and van der Klaauw 2006) and seem to suggest that both the automated and the caseworker-induced referrals can enhance the matching of job seekers to vacancies (Fougère et al. 2009). The finding that the automated referrals generate lower effects than the caseworker induced referrals is consistent with the hypothesis that automated referrals are of lower quality than the ones with a caseworker intervention. In this line of reasoning, one may expect that referrals that are transmitted to the unemployed after a personal meeting are more effective in raising the job-finding rate than referrals that are sent by the caseworker without this personal contact. This is not what we find, but this may be explained by the fact that less individuals receive a referral in case of an invitation than when they are directly transmitted this referral by e-mail or telephone contact: 34% of the invited job seekers do not show up at the meetings (see Fig. 1); when meetings take place, the job seeker receives a referral in only 45% of the cases (see Fig. 1), presumably because the personal contact reveals to the caseworker that a referral to a vacancy is not the best strategy to bring the unemployed back to work. This implies that only 30% of the invited individuals receive a referral. If the short-run treatment effect would be completely induced by the referrals, the employment rate would be enhanced by as much as 410% (=123/0.30) per referral, which is about twice the magnitude of the effect of referrals that were sent out by caseworkers without these personal meetings. But this is for sure an overestimation, because it is unlikely that the effect of the invitations is only induced by the referrals that are sent out in this case. In fact, in view of the persistence of the effects, we argue next that part of the immediate effects is likely related to the "wake-up call" that the treatment triggers among the unemployed: it makes them realize that the PES is indeed monitoring their search behavior and that if they do not intensify their search they might be sanctioned.

Before attributing the persistence of the effect largely to a threat effect that is triggered by the treatment, we argue why other explanations are less likely. A first potential reason of observing a long-term effect is that, if the first referral is not successful, caseworkers are likely to send out other referrals subsequently. However, since we right-censor the unemployment spell as soon as an individual receives a second treatment, this cannot explain the long-run effect. Second, caseworkers could propose other actions to the unemployed if they observe that the transmission of a referral is not successful. In particular, they could propose job seekers to participate in training. But by right-censoring spells as soon as individuals enter training, we also preclude this explanation.[10] However, we must admit that we cannot observe all actions that might be proposed to the unemployed upon unsuccessful referrals (e.g., attending a job fair or subsequent meetings in which with caseworkers provide further job search assistance). A third possible explanation for the persistent effect is that the referral may have induced the job seekers to widen the scope of search and to search for other jobs than the ones they were searching for prior to the intervention. Recently, Arni (2015) and Belot et al. (2016) indeed provide evidence that simple interventions that lead workers to widen the scope of their job search can raise the job-finding rate substantially. Again, we cannot completely exclude this explanation. However, we believe that it is unlikely that a "one-shot" intervention in which the job seeker just receives one referral to a vacancy to which he/she may not have applied without the intervention would be sufficient to reorient the job search strategy to that extent that it could generate the persistent treatment effects of the size that we report here.

We therefore believe that the most likely explanation for a positive long-term effect is that the referral alerts the participants that the PES is monitoring them and is expecting them to sustain their job search effort all along the unemployment spell. In Section 2, we already argued that the job seekers are well informed about their obligation to search and the obligation to react to the referrals. The fact that the PES does neither systematically (only in about 25% of the cases) check whether the unemployed apply to the referred vacancies nor systematically report noncompliance to the federal agency RVA/ONEM does not mean that the unemployed do not perceive the threat of a sanction. In fact, this interpretation is in line with that of van den Berg. et al. (2014). These authors also report for Denmark sizable long-term effects of meetings that often lead to vacancy referrals. In case of women between 30 and 49, the long-term effects are even larger than the reported immediate effects (as is the case in our specification for the automatic referrals). Similarly, van den Berg et al. (2016) argue that vacancy referrals are used in Germany to fight moral hazard in UI. As in this study, they find that referrals substantially raise the job-finding rate. However, they also demonstrated that this comes at a cost of job quality as measured by lower wages and less employment stability. Moreover, they find that a significant fraction of the unemployed transit to sickness insurance as to escape the obligation to apply for vacancies that the PES refers to them. In this study, we unfortunately do not have the data to check whether such adverse effects are also present in Belgium.

In the third specification, interaction effects between the treatment effect and specific explanatory variables are included. Of these, only the local unemployment rate in the treatment month is significant. The positive results indicate that treatment effects are positively related to the local unemployment rate. van den Berg et al. (2016) report a similar interaction effect for Germany. However, the findings suggest that the effectiveness of vacancy referrals does not depend on the elapsed unemployment duration, neither on the age nor on the gender of the participants.

Finally, an interesting question from a policy perspective is which of the three different treatments would be most cost-effective. The invitations clearly require more costly time investment of case workers than the vacancy referrals that are directly dispatched, while the latter are much more effective than the former in stimulating the transition to employment. Therefore, at first sight, either the vacancy referral sent out by the case workers by e-mail or phone or the automatic referrals are the most cost-effective. Which of these two are to be preferred is unclear. While automatic referrals are clearly much less effective, they are also much cheaper than the referrals that are sent out by the caseworkers. A choice between the two requires more information about these costs as well as about the returns to enhanced transitions to employment. The aforementioned findings of van den Berg et al. (2016) for Germany caution that these returns might eventually be lower than expected at first sight, because job quality is at stake and higher public expenditures in sickness insurance are to be expected. Furthermore, other researchers have pointed out that the higher job-finding rate of the treated individuals generally comes at the expense of a lower one for untreated individuals (Crépon et al. 2013; Gautier et al. 2017).

6 Conclusions

As in many other countries, also in Flanders, the northern part of Belgium, the public employment service (PES) makes use of vacancy referrals in order to facilitate the matching between unemployed workers and vacancies. In this article, we evaluate the effectiveness of this policy. We differentiate between three treatment types: (1) referrals, in which case the match is handled by a caseworker, who also contacts the unemployed worker by phone or by e-mail; (2) automatic referrals, where there is no caseworker intervention and matches are made by matching software; and (3) invitations, where the unemployed worker is invited for a meeting at the PES in which the caseworker may refer (but not necessarily so) the job seeker to a vacancy. In this research, we can only identify the effect of the invitation, irrespectively of whether it is followed by a referral or not.

We use a sample of 12,983 unemployment spells that started in 2007. In order to identify the treatment effect, we use a "timing of events" approach. This approach allows to distinguish between the treatment effect on the one hand and selection on (un-)observables on the other. We find large positive and significant effects of all three treatments on the transition rate to employment both in the short- and in the long run. The effect of the referrals that are directly dispatched by the caseworkers more than triples the transition rate to employment both in short- and long run and the invitations double it, while the automatic referrals enhance this rate by 50% in the first 2 months and double it in the longer run. These treatment effects are so large because they are measured in proportional terms: as the treatment is targeted at job seekers with a very low transition rate to employment in the absence of the treatment, the percentage point effects are much lower. While the short-term effects could be largely induced by the job referrals themselves, we argue that an explanation of the long-run effects could be that the referrals serve as a job search monitoring device, alerting the unemployed workers that the PES is following them and is expecting them to sustain their job search effort. Other researchers have found similar findings for Denmark and Germany (van den Berg et al. 2014, 2016).These results seem at first sight very promising, especially in view the low cost of this treatments compared with other ALMP for the unemployed, such as (vocational) training programs. In terms of cost-effectiveness, either the automatic referrals (very low cost, but lower effectiveness) or the referrals that are directly dispatched by the caseworkers (somewhat higher cost, but also much more effective) are to be preferred. However, since we lack data on costs, a final recommendation cannot be made. Besides, some caution is warranted. First, van den Berg et al. (2016), who also report similar positive effects of vacancy referrals on the transition rate to employment and put forward a similar interpretation as we do, find that these effects come at a cost of lower job quality and that they enhance the entry in sickness insurance, outcomes which we could not measure with our data. Furthermore, other researchers have pointed out that the higher job-finding rate of the treated individuals generally comes at the expense of a lower one for untreated individuals (Crépon et al. 2013; Gautier et al. 2017). Gathering data that allow studying these aspects is therefore certainly an interesting avenue for further research. Another worthwhile extension would consist in removing the right censoring when a second treatment occurs and to model the effect of this second and subsequent treatments.

Endnotes

[1]School-leavers who acquire a minimal level of educational attainment are entitled to UBs after 9 months if they are younger than 26 and after 1 year if they are older. Since 2012, the waiting period has been raised to 1 year for those younger than 26.

[2]We also removed unemployed individuals with a disability, persons older than 65, and individuals not living in Flanders (i.e., in Brussels or in Wallonia).

[3]This unemployment rate is measured at the district level ("arrondissement").

[4]One of the explanatory variables (the variation of the unemployment rate in the district of residence) is time-varying, but we do not make this explicit for notational convenience.

[5]In order to allow for non-linear effects, also, the square of the unemployment duration at treatment is included.

[6]Here, also, age squared is included.

[7]Unemployed persons experiencing a transition to inactivity or to training are censored when making this transition. Those who are unemployed during the whole observation period are censored by the end of September 2010.

[8]For example, in the left-hand-side model of Table 2, the coefficient for gender is − 0.18. When we take [exp(− 0.18) − 1], the result is − 0.16, indicating that the exit rate for women towards a treatment is 16% lower than the exit rate for men.

[9]The fact that the treatment effect increases so much once the selection on unobservables is taken into account, implies that the unobservables that determine participation in the treatment are strongly negatively related to unobservables that determine the transition rate to employment.

[10]Note, if the unemployed perceive participation in training as a threat, then the enhanced transition to employment that such a threat might induce prior to this participation is still measured in our data and should show up in the long-run treatment effects. This is in line with our interpretation that we develop further below.

Appendix

6.1 The likelihood function for time-grouped data

The exit from unemployment to employment can only be observed on a monthly basis. Therefore, we have time-grouped data. Gaure et al. 2007 show that interval censoring is unproblematic, as long as this is taken into account in the likelihood function. In this appendix, we derive the likelihood contributions for time-grouped data, conditional on observed and unobserved variables. We exploit the fact that the exact date of treatment is known: in a month in which a treatment is obtained, one can distinguish the fraction of the month before the treatment, and the subsequent fraction of the month, starting on the day of the treatment. Another element that will be taken into account relates to the fact that very short spells of persons who enter and leave unemployment in the same month (either with or without referral) are not observed in the data. Finally, we will show how the likelihood function unconditional on the unobservables can be obtained.

To take the time grouping into account, the baseline hazard is specified as piecewise constant. For both hazards, the time line is divided in 12 intervals of different length: month 2 (the first month is not observed), month 3, month 4, month 5, month 6, month 7, month 8, months 9–10, months 11–12, months 13–16, months 17–28, and months 28–45.

The first likelihood contribution relates to individuals who neither got treated nor exited to employment. These observations are right censored in both durations at $t_{(m-1)}$, and their likelihood contribution is given by the survivor probability:

$$\begin{aligned} l_1(V) &= \Pr(T_e > t_{(m-1)}\,, \quad T_r > t_{(m-1)}|x, t_r, V) \\ &= \exp\left[-\sum_{j=2}^{m-1}[\theta_e(t_j|x, t_r, V_e) + \theta_r(t_j|x, V_r)]\right] \end{aligned}$$

The second likelihood contribution relates to individuals who leave for employment within interval $[t_{(m-1)}, t_m)$, with $m > 1$, without having been treated:

$$\begin{aligned} l_2(V) = {} & \Pr\left(t_{(m-1)} < T_e \leq t_m, \quad T_r > t_m | x, t_r, V\right) \\ & = \left\{ \frac{\theta_e(t_m|x|t_r, V_e)}{\theta_e(t_m|x, t_r, V_e) + \theta_r(t_m|x, V_r)} \right. \\ & \quad \times \left[\exp\left[-\sum_{j=2}^{m-1} [\theta_e(t_j|x, t_r, V_e) + \theta_r(t_j|x, V_r)] \right] \right] \\ & \quad \times [1 - [\exp[-\theta_e(t_m|x, t_r, V_e) - \theta_r(t_m|x, V_r)]]]\} \end{aligned}$$

The third likelihood contribution relates to individuals who leave for program participation within interval $[t_{(k-1)}, t_k)$, but who remain in unemployment and are right censored at $t_{(m-1)}$:

$$\begin{aligned} l_3(V) = {} & \Pr\left(T_e > t_{(m-1)}, \quad t_{(k-1)} < T_r \leq t_k | x, t_r, V\right) \\ & = \{\theta_r(t_k|x, V_r) \\ & \times \left[\exp\left[-\sum_{j=2}^{k-1} [\theta_e(t_j|x, t_r, V_e) + \theta_r(t_j|x, V_r)] - [\theta_e(t_k|x, t_r, V_e) + \theta_r(t_k|x, V_r)](\mathrm{t} - \mathrm{k} + 1) \right] \right] \\ & \times \left[\exp\left[-[\theta_e(t_k|x, t_r, V_e)](\mathrm{k} - \mathrm{t}) - \sum_{j=k+1}^{m-1} [\theta_e(t_j|x, t_r, V_e)] \right] \right] \Bigg\} \end{aligned}$$

The fourth likelihood contribution relates to individuals who leave for program participation within $[t_{(k-1)}, t_k)$ and leave towards employment in $[t_{(m-1)}, t_m)$, with $m > k$:

$$\begin{aligned} l_4(V) = {} & \Pr\left(t_{(m-1)} < T_e \leq t_m, \quad t_{(k-1)} < T_r \leq t_k | x, t_r, V\right) \\ & = \{\theta_r(t_k|x, V_r) \\ & \times \left[\exp\left[-\sum_{j=2}^{k-1} [\theta_e(t_j|x, t_r, V_e) + \theta_r(t_j|x, V_r)] - [\theta_e(t_k|x, t_r, V_e) + \theta_r(t_k|x, V_r)](\mathrm{t} - \mathrm{k} + 1) \right] \right] \\ & \times \left[\exp\left[-[\theta_e(t_k|x, t_r, V_e)](\mathrm{k} - \mathrm{t}) - \sum_{j=k+1}^{m-1} [\theta_e(t_j|x, t_r, V_e)] \right] \right] \\ & \times [\exp[-\theta_e(t_m|x, t_r, V_e) - 1]]\} \end{aligned}$$

The fifth likelihood contribution relates to individuals who leave for program participation within $[t_{(k-1)}, t_k)$ and leave towards employment in $[t_{(m-1)}, t_m)$, with $m = k$:

$$\begin{aligned} l_5(V) = {} & \Pr\left(t_{(k-1)} < T_e \leq t_k, \quad t_{(k-1)} < T_r \leq t_k | x, t_r, V\right) \\ & = \{\theta_r(t_k|x, V_r) \\ & \times \left[\exp\left[-\sum_{j=2}^{k-1} [\theta_e(t_j|x, t_r|V_e) + \theta_r(t_j|x, V_r)] - [\theta_e(t_k|x, t_r, V_e) + \theta_r(t_k|x, V_r)](t - k + 1) \right] \right] \\ & \times [1 - \exp[-\theta_e(t_m|x, t_r, V_e)(k - t)]]\} \end{aligned}$$

As very short spells of persons who enter and leave unemployment in the same month (either with or without treatment) are not observed, we have to take into account that all persons in the observed sample survived the inflow month. Therefore, the likelihood must be written conditional on surviving the first month, i.e., conditional on neither treatment nor exit to employment in the first month. The conditioning event is given by $D_0(V)$:

$$D_0(V) = \int_0^1 \exp[-\int_{t_0}^1 [\theta_e(s-t_0|x, t_r, V_e) + \theta_r(s-t_0|x, V_r)]ds\Big] dt_0$$
$$= \{\frac{1}{\theta_e(t_1|x, t_r, V_e) + \theta_r(t_1|x, V_r)}$$
$$\times [1-[\exp[-\theta_e(t_1|x, t_r, V_e)-\theta_r(t_1|x, V_r)]]]\}$$

The first integral relates to the fact that the day of entering unemployment is unknown, and therefore any day of the month is given an equal probability.

Likelihood contributions l_1(V) until l_5(V) and the conditioning event D_0(V) are conditional on the unobservables V. The unconditional likelihood contributions are obtained by integrating V out:

$$l_s = \int_V [l_s(V)/D_0(V)]\mathrm{d}G(V) \qquad \text{for s} = 1, \ldots, 5$$

where $G(V)$ is the joint distribution of the unobserved heterogeneity terms. Unobserved heterogeneity is specified nonparametrically, using the approach of Heckman and Singer 1984. The distribution of unobservables is approximated by a discrete mixture distribution with an unknown number of mass points. We assume that the vectors of unobserved attributes (v_{ri}, v_{ei}) are jointly discretely distributed. The number of mass points is determined by adding consecutively mass points as long as the AIC decreases (Gaure et al. 2007).

Subsequently, the unconditional log-likelihood can be written as the sum of the individual log-likelihood contributions:

$$L = \sum_{i=1}^{N} \{c_{1i}\ \ln l_{1i} + c_{2i}\ \ln l_{2i} + c_{3i}\ \ln l_{3i} + c_{4i}\ \ln l_{4i} + c_{5i}\ \ln l_{5i} - \ \ln D_{0i}\ \}$$

where $c_{si} = 1$ if l_{si} is the contribution of individual i to the likelihood, and $c_{si} = 0$ otherwise.

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