Effectiveness of a Job Vacancy Referral Scheme

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1. Introduction

The referral of job seekers to vacancies is a policy that is used by many countries, yet little is known about the effectiveness of this approach. In a meta-analysis of the effectiveness research regarding active labor market policies (ALMP's) (Card *et al.* 2010), the "Services and Sanctions"- type of ALMP is considered. These are policies that are aimed at enhancing the job search efficiency and effort. Examples are job search courses, job clubs, vocational guidance, counselling and monitoring, and sanctions in the case of non-compliance with job search requirements. The referral procedure clearly belongs to this category. As to their effectiveness, Card *et al.* (2010) conclude that this type of ALMP turns out to be particularly promising, as, on average, their effects on the probability to leave unemployment towards employment are positive, while at the same time this kind of policies are relatively inexpensive.

Specific analysis on the effectiveness of referrals is rare. On the basis of 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 Public Employment Service (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 the verification 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 more costly job search 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 case worker on the transition rate from unemployment to employment in Denmark. They find large positive effects of the meetings. Moreover, the transition rate strongly increases in the week the meeting is held and remains significantly higher up to eight weeks later. For women this effect even persists for a longer period, be it at a lower level. The effect size tends to increase with the number of meetings. Interestingly, they conclude that meeting effects appear to be driven by highly significant vacancy referral effects.

In Germany, a refusal to apply to a vacancy referral can be punished by an unemployment benefit sanction. Van den Berg *et al.* (2013) 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 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 case workers (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.

2. Institutional context

In general, if a Belgian worker loses his job, he will be entitled to unemployment benefits, provided that he contributed to the unemployment system while he was working. Eligibility depends on the length of the previous employment spell, and this length increases with age: whereas someone below 36 should have worked 12 months during the previous 18 months in order 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 in Belgium depends on the last wage, elapsed unemployment duration, on family status, and on age. The benefits are provided without time limit.

In order to remain eligible for unemployment benefits, once unemployed, unemployment has to be involuntary. This implies that the unemployed is not allowed to turn down what is called a 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 four hours in order to get to the job, or if accepting the job implies that one's income decreases. A third principle, which is only valid during the first six months of the unemployment spell, states that a job offer is not suitable if it does not relate to the professional skills acquired by the unemployed.

Unemployed persons who turn down suitable job offers, run a risk of obtaining an unemployment benefit sanction: a temporary or permanent reduction or withdrawal of their unemployment benefit. Unemployment benefit sanctions can also be obtained is case of a refusal to participate in vocational training, in case of fraud, and in case of undeclared work. Starting in 2004, the long term unemployed regularly have to prove their job search efforts. Non-compliance can give rise to an unemployment benefit sanction.

Belgium has 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 unemployment benefit system, including the sanctioning authority, is run by the RVA/ONEM, a federal institution, i.e. on the Belgian level. The Regions, on the other hand, also have wide powers regarding labor market issues such as active labor market policies and the matching of demand and supply on the labor market through the (regional) Public Employment Services (PES).

Given this division of tasks, noncompliance with eligibility requirements, such as a refusal to accept a suitable job or to participate in a vocational training, typically will be observed by the regional PES. In that case, they can report this to the federal RVA/ONEM, which accordingly will decide whether or not an unemployment sanction is in order.

In Flanders, the Northern part of Belgium, the regional PES is called VDAB. In the year 2007, the VDAB reported 32615 cases of noncompliance with eligibility requirements to the RVA/ONEM (to put this number into perspective, in 2007 there were on average 143035 unemployed persons receiving an unemployment benefit). In 44% of the reported cases, a sanction followed.

The VDAB keeps an unemployment register with information (age, education, place of residence, work experience, job preferences, etc.) of the persons who currently are unemployed. At the same time, the VDAB maintains a database with the job vacancies that are currently available. Both databases are regularly compared in order to find whether suitable matches can be found between an unemployed and a job vacancy. These matches are subsequently used in various ways. In the *notification procedure*, an unemployed person will be informed that a (potentially) adequate match has been found for him or her. The unemployed person, however, is not required to respond to the notification. In the so called *referral procedure* (which is the subject of this paper), more commitment is imposed. Here the matching between job characteristics and the unemployed is partly standardized and partly 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 unemployment benefit.

In Bollens and Heylen (2009) the effectiveness of the *notification procedure* for new entrants in unemployment was investigated. 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. From the literature we can deduce two possible explanations for this finding: (i) the high standardization of the notification procedure may lead to a low quality of the match between the requirements of the referred vacancy and the characteristics of the unemployed worker; (ii) the notification procedure is not compulsory, so that the positive ‘threat’ effect of a sanction 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; (ii) 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, one cannot speak of the referral approach as such, since it relates to a collection of several related but different approaches, as can be seen in Figure 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, the automatic referrals have become quantitatively less important, as the PES considered them to be less efficient.

In a second type of referral caseworkers intervene. We distinguish between the direct and the indirect approach. In the direct approach, the caseworker refers the unemployed to a given vacancy by mail or by phone. In the indirect approach, the caseworker again starts with a match between a vacancy and an unemployed, but instead of sending a referral, she invites the unemployed to the office in order to discuss the appropriateness of the match. Depending on show-up and, in case of show-up, on the outcome of this meeting, either a referral is given or not.

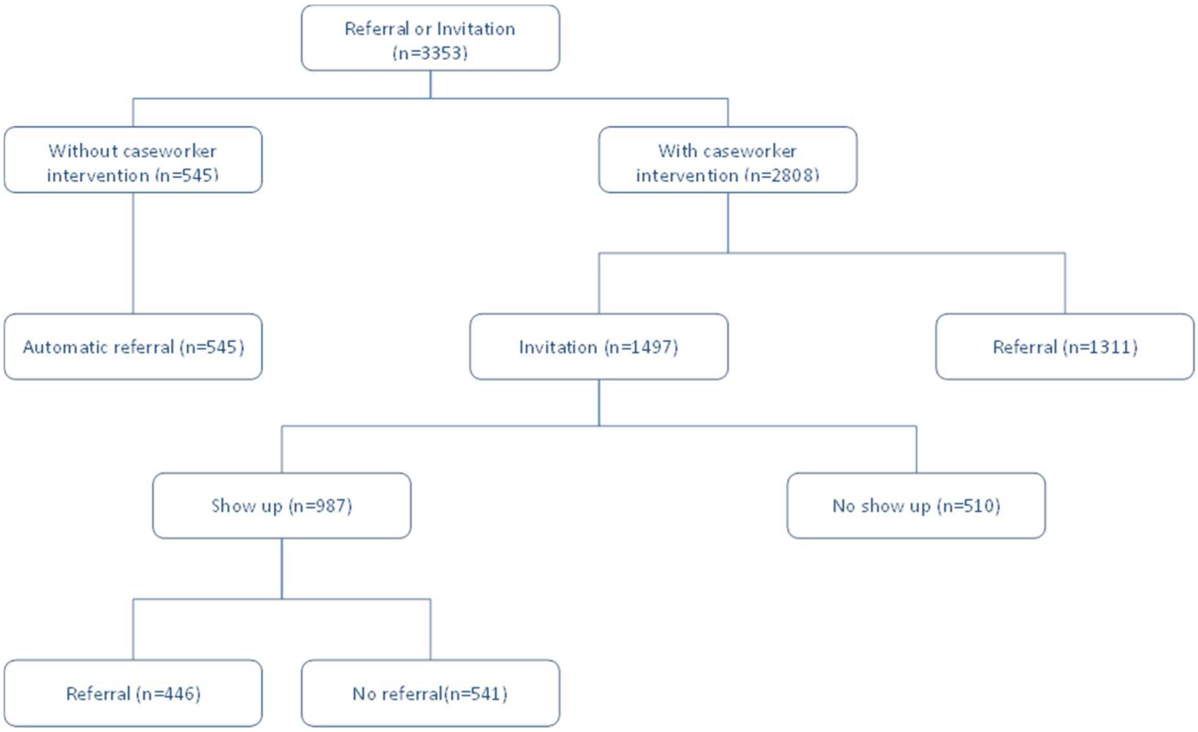

Figure 1 The referral approach

The fact that it is mandatory to apply after receiving a referral, and that it is mandatory to show up at the PES-meeting after receiving an invitation, obviously is an important characteristic of these policies. Therefore it is important to know whether all implied 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 done by the VDAB itself, one can conclude that several distinct referral procedures are used within the organisation, implying that this important message is communicated in more and in less formal ways to the referred unemployed. Generally, one may however assume that the referred unemployed indeed will be aware of the mandatory nature.

The situation is quite different with respect to the follow up of the obligation to apply for the vacancy. In the year 2007, for barely 25% of all referrals the VDAB could say whether or not the unemployed had applied. For the remaining 75%, this is not monitored. This is not actively followed up, because one wants to minimise the administrative burden for the employers with vacancies. This lack of information also implies that one does not systematically report non-compliance to the RVA/ONEM. The internal survey done by the VDAB itself indicates that such reporting does occur, but rather occasionally than systematically.

3. Data

We use data from the unemployment register as collected by the VDAB, the public employment service in Flanders. The dataset consists of individual records. Anyone who enters unemployment is recorded in the month of entrance. The dataset moreover has information on the labor market position of the individual at the end of all calendar months (either unemployed or employed).

We selected the unemployment spells that started with unemployment benefit receipt in 2007. This excludes voluntary registered job seekers (e.g. those who were previously inactive, and decided to start working again). This also excludes school-leavers, who are not entitled to an unemployment benefit[1]. In order to make the sample more homogeneous, we removed all spells of unemployed who are younger than 25 at the beginning of the spell[2].

The duration of the unemployment spell is defined as the time until employment has been found. We observe transitions to employment, but do not have additional information about this employment situation. 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 a 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 (automatic) vacancy or obtained an invitation. For each unemployment spell in the sample of spells that started in 2007, we checked whether a treatment had been given 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 obtaining a referral is defined as the time from the start of the unemployment spell until the date of obtaining a (first) treatment. 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 period covered by the dataset, whichever comes first.

These selection criteria gave rise to a sample of 129305 spells that started in 2007. For computational reasons, a random sample of 10% was selected, leading to a sample size of 12,983 cases. Table 1 provides some descriptive statistics of the explanatory variables. These are all measured at the beginning of the unemployment spell, except the local unemployment rate (not included in table 1), which varies on a monthly basis[3]. This time varying local unemployment rate takes account of seasonal and business cycle effects.

Of the unemployment spells that started in 2007, almost 26% did get a treatment. When comparing the spells with and without treatment, it can be seen that there is some selection on observables, but in general differences between both groups tend to be small. Those who received a treatment on average are slightly older and males have a slightly higher probability to be treated, but differences related to educational attainment are somewhat more marked: whereas the lower skilled (no secondary degree) have a higher probability to be treated, those with a tertiary degree have a lower probability. This observation may be related to the dynamic sorting process: unemployed workers with a higher educational attainment in general will leave unemployment sooner as compared to unemployed with a lower educational level, and therefore have less chance to be treated.

[1] School leavers who acquire a minimal level of attainment are entitled to unemployment benefits after 9 months if they are younger than 26 and after one year if they are older. Since the waiting period has been increased to one year for those younger than 26.

[2] The other removed spells relate to spells of unemployed with a disability, spells of persons who are older than 65 and spells of persons who live outside Flanders (i.e. in Brussels or in Wallonia).

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

Table 1 Summary statistics

Variables	All a= b+c	Treated b= d+e+f	Not-treated c	Referral d	Invitation e	Autom. Referral f
N	12983	3353	9630	1311	1497	545
Sex (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						
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	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

The educational track refers to the objective of the subject chosen in secondary education, for those whose highest educational level is either a higher secondary degree or a lower secondary degree. The vocational track prepares for a direct transition to a profession, whereas the general and the technical tracks prepare for a transition to tertiary education.

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 e.a. (1997), and Blundell e.a. (2004), the recent labor market history can be a crucial component in an 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 realized through a variable measuring the number of months in unemployment within the 24 months that precede the current unemployment spell. 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 modelling

To estimate the impact of the treatment on the rate of transition to employment, the labor market outcomes of recipients (treated group) and of non-recipients (control group) will be compared. As participation possibly is selective, meaning that the observed and unobserved characteristics of both groups may be different, a different outcome for both groups may not only be a consequence of the treatment, but could also be due to these other differences.

Over and above this classical selection problem, we have to take into account a dynamic selection problem, since the more employable workers on average will leave unemployment sooner, and therefore will have a smaller probability to be treated.

To solve these problems, we control for differences between the treated group and the control group based on both observed and unobserved individual characteristics. Selection on observables is taken into account by conditioning the hazard rates 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 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 follow each other quickly, it is possible to distinguish between the treatment effect and the selection effect without imposing 'exclusion restrictions' on the observed explanatory variables. In what follows, we 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 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, $T_r|X$, 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 $\theta_e(t|t_r, x, V_e)$.

[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.

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 chance to obtain a treatment. A positive correlation therefore implies that participation will be selective, and δ will be a biased estimator of the true impact (an overestimation in this case). In the case of a negative correlation, δ will be underestimate the true impact.

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 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:

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

$$\theta_r(t \mid x, V_r) = \lambda_r(t).exp\,(x'\beta_r + V_r) \qquad (2)$$

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. Note, however, that $\delta(t|t_r, x)$ 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:

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

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 duration, 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 e.a. 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 where 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 treatment[5], (2) the level of education (having a tertiary degree or not) (4) the age[6], (5) the sex and (6) the local unemployment rate at the moment of getting the treatment. These interactions seem 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 a treatment is obtained, one can distinguish the fraction of the month before the treatment, and the fraction of the month, starting at the day of the treatment (see Appendix 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, 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 1st month (see Appendix 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 (m-1) [7]; (2) l_2 for individuals who leave for employment within [m-1, m), with m>1, without having been treated; (3) l_3 for individuals who are treated within [k-1,k), but who remain in unemployment and are right censored at (m-1), (4) l_4 for individuals who are treated within [k-1,k), and leave towards employment in [m-1, m), with m > k, and (5) l_5 for individuals who are treated within [k-1,k), and leave towards employment in [m-1, 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 Appendix for the details of this derivation). Subsequently, we derive the unconditional likelihood contributions by integrating V out:

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

[5] In order to allow for non-linear effects, also the square of the unemployment duration at the point of 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 at the end of September 2010.

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 1st 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 (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-criterion 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 $\delta(t|tp, x)$ 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 don't 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 unobserved characteristics of participants.

Assumption (5): There are at least two non-linearly 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 they change their behavior. Considering these workers as members of the control group would bias the treatment effect. 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 of treatment, as also invitations arrive unannounced.

It is important to distinguish anticipation effects from ex ante effects (Abbring and van den Berg 2004; Richardson and van den Berg 2008; van den Berg *et al.* 2009). An ex ante effect occurs if the transition rate to regular employment of non-participants 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 being invited for a meeting at the PES, he may change his search strategy by accepting job offers that he otherwise would not have accepted. Note that, since the ex ante effect concerns a spillover effect of the treatment on non-participants, it can be regarded as a specific general equilibrium effect of the treatment. In any case, given the relatively small burden imposed on unemployed workers by referrals and invitations, we expect these general equilibrium effects to be negligible. The analysis at hand 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 (2003)’s method is extremely reliable for time-grouped data as long 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 2008). 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 correlated with 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 (2008) prove non-parametric 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 (2008) point out that this time profile may be biased downwards by a dynamic sorting effect: Treated individuals with unobserved characteristics such that their treatment effect is high (holding every other characteristic 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). For a closer look, we first consider Chamberlain (1980)'s 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:

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

where υ_{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 (1980)'s 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

The estimation results are reported in Tables 2, 3 and 4. Table 2 reports the estimates of the transition to treatment and Table 3 reports the estimates of the transition towards employment. Table 4 gives some general model characteristics. Information about unobserved heterogeneity 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 enhanced with a correction for unobserved heterogeneity; and (c) the previous specification, enhanced with several interaction effects between the treatment effect and specific explanatory variables. These interactions allow checking whether there are heterogeneous treatment effects.

We always report four elements: (1) “b”, the estimated coefficients; (2) “exp(b)-1”, which gives the change (as compared to the reference category) in the exit to respectively treatment (table 2) and 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 direct effect gives the change in the exit rate to employment at the end of the month in which the treatment was obtained (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, on average are less employable than persons who do not get the treatment. This implies that a specification that does not control for unobserved heterogeneity will underestimate the treatment effect. In what follows, we will therefore focus on the results of the specifications that do correct for unobserved heterogeneity.

The results of both specifications that do correct for unobserved heterogeneity suggest that the direct 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 smallest for the automatic referrals. The estimated effects appear to be very high: a referral increases the exit to employment (in the second specification) with 207%, getting an invitation changes the exit rate to employment with 123%, and an automatic referral still increases this exit rate with 51%. One should however take into account that (1) these are short term effects; and (2) that this effect is relative to what the exit would have been in the counterfactual of no participation. As the analysis of the unobserved heterogeneity terms indicates, treated participants on average are less employable than persons who do not get the treatment, suggesting that their exit probabilities in the absence of treatment would have been relatively small.

[8] In the left hand side model of table 2, the coefficient for sex 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.

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
Sex (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 unemp. 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

Table 2 Continued

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.
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 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	-1.28	-0.72	0.06	0.000	0.25	0.29	0.32	0.433	0.23	0.26	0.59	0.697
Sex (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 unemp. 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

Table 3 Continued

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.
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
Effect of automatic referral												
Month of aut. refer. 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

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

Table 3 Continued

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.
Interactions (continued)												
Age									0.00	0.00	0.01	0.915
Age squared /100									-0.02	-0.02	0.07	0.719
Sex (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			

Table 4 Model characteristics

	No unobserved heterogeneity	Unobserved heterogeneity: base model	Unobserved heterogeneity: model with interactions
Log-likelihood	-35836.69	-35703.985	-35692.939
Number of variables	66	84	91
Number of observations	12983	12983	12983
Akaike Information criterion	71805.38	71575.97	71567.88

At first sight somewhat more surprising is the magnitude of the estimated coefficients for the long term effects. One would expect that the effect of specific instruments such as a referral and an invitation, if any, would be concentrated in the period immediately after their application. Someone who gets treated and does remain in unemployment, possibly will receive another treatment. As such, these subsequent treatments could be an explanation for positive long term effects, but we right censored all spells at the moment where they obtain a second treatment, and therefore this explanation is excluded. Note that van den Berg. e.a. (2014) also report sizable long effects for meetings between the unemployed and their caseworker, which in case of women between 30 and 49 are even larger than the reported direct effect (as is the case in our specification for the automatic referrals). A possible explanation for a positive long term effect could be that obtaining the treatment, even if it does not immediately affects the transition to employment for everybody concerned, will in a sense alert the participants, as it signals that the PES is following them and is expecting them to keep investing in their job search. Moreover, also in the case of the long term effects it should be noted that the effect is relative to what the exit would have been in the counterfactual of no participation.

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 indicates that treatment effects are higher if the local unemployment rate is higher, which is comparable to the effect reported by van den Berg e.a. 2013 for Germany. The absence of a significant effect in the interaction with the unemployment duration suggests that the effectiveness of the treatment does not depend on the unemployment duration. The same observation can be made with respect to the age and the sex of the participants.

6. Conclusion

As in many other countries, also in Flanders, the northern part of Belgium, the public employment service 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 supervised 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; (3) invitations, where the unemployed worker is invited for a meeting at the PES. As result of this meeting either or not a referral will follow. Here we look at the effect of obtaining an invitation as such, whether it is followed by a referral or not.

We use a sample of 12983 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 hand.

We find large and significant direct effects of the exit to employment in the month in which the treatment is given and in the subsequent month. These effects are positive for the three treatment types, although the effect for a referral is the largest, and the effect for an automatic referral is smallest, while the effect of invitations is situated in between. Also the long term effects on the exit to employment are substantial for the three treatment types. An explanation could be that the treatments serve as a job search monitor device, alert the unemployed workers that the PES is following them and is expecting them to keep investing in their job search.

These results are interesting, especially given the fact that the cost of this treatments is relatively small when compared with e.g. (vocational) training programs for the unemployed.

There are some avenues for further research. One interesting extension would be to remove the right censoring when a second treatment occurs, and modelling the effect of a second treatment and of subsequent treatments.

Appendix

The likelihood function for time grouped data

The exit from unemployment towards employment can only be observed on a monthly basis. Therefore we have time grouped data. Gaure e.a. 2007 show that interval-censoring is unproblematic, as long as this is taken into account in the likelihood function. In this appendix, we give 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 fraction of the month, starting at 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 a likelihood function can be obtained that is unconditional on the unobservables.

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, 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 (m-1), and their likelihood contribution is given by the survivor probability:

$$l_1(V) = \Pr\ (T_e > t_{(m-1)},\ T_r > t_{(m-1)} \mid x, t_r, V)$$

$$= \exp\left[-\sum_{j=2}^{m-1}\left[\theta_e\left(t_j \mid x, t_r, V_e\right) + \theta_r\left(t_j \mid x, V_r\right)\right]\right]$$

The second likelihood contribution relates to individuals who leave for employment within [m-1, m), with m>1, without having been treated:

$$l_2(V) = \Pr\ (t_{(m-1)} < T_e \leq t_m,\ T_r > t_m \mid x, t_r, V)$$

$$= \left\{\frac{\theta_e(t_m \mid x, t_r, V_e)}{\theta_e(t_m \mid x, t_r, V_e) + \theta_r(t_m \mid x, V_r)}\right.$$

$$\times \left[\exp \left[-\sum_{j=2}^{m-1} \left[\theta_e\left(t_j \mid x, t_r, V_e\right) + \theta_r\left(t_j \mid x, V_r\right) \right] \right] \right]$$

$$\times \left[1 - \left[exp \left[-\theta_e(t_m \mid x, t_r, V_e) - \theta_r(t_m \mid x, V_r) \right] \right] \right] \Big\}$$

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

$$l_3(V) = \Pr \ (T_e > \ t_{(m-1)}, \ t_{(k-1)} < T_r \leq \ t_k \mid x, t_r, V)$$

$$= \{\theta_r(t_k \mid x, V_r)$$

$$\times \left[\exp \left[-\sum_{j=2}^{k-1} \left[\theta_e\left(t_j \mid x, t_r, V_e\right) + \theta_r\left(t_j \mid x, V_r\right) \right] \right. \right.$$

$$\left. \left. - \left[\theta_e(t_k \mid x, t_r, V_e) + \theta_r(t_k \mid x, V_r) \right] (\text{t-k+1}) \right] \right]$$

$$\times \left[\exp \left[-[\theta_e(t_k \mid x, t_r, V_e)](\text{k-t}) - \sum_{j=k+1}^{m-1} \left[\theta_e\left(t_j \mid x, t_r, V_e\right) \right] \right] \right] \Bigg\}$$

The fourth likelihood contribution relates to individuals who leave for program participation within [k-1,k), and leave towards employment in [m-1, m), with m > k:

$$l_4(V) = \Pr \ (t_{(m-1)} < T_e \leq \ t_m, \ t_{(k-1)} < T_r \leq \ t_k \mid x, t_r, V)$$

$$= \{\theta_r(t_k \mid x, V_r)$$

$$\times \left[\exp \left[-\sum_{j=2}^{k-1} \left[\theta_e\left(t_j \mid x, t_r, V_e\right) + \theta_r\left(t_j \mid x, V_r\right) \right] \right. \right.$$

$$\left. \left. - \left[\theta_e(t_k \mid x, t_r, V_e) + \theta_r(t_k \mid x, V_r) \right] (\text{t-k+1}) \right] \right]$$

$$\times \left[\exp\left[-[\theta_e(t_k \mid x, t_r, V_e)](\text{k-t}) - \sum_{j=k+1}^{m-1}\left[\theta_e\left(t_j \mid x, t_r, V_e\right)\right]\right]\right]$$

$$\times \left[\exp\left[-\theta_e(t_m \mid x, t_r, V_e) - 1\right]\right]\}$$

The fifth likelihood contribution relates to individuals who leave for program participation within [k-1,k), and leave towards employment in [m-1, m), with m = k:

$$l_5(V) = \Pr\ (t_{(k-1)} < T_e \leq\ t_k,\ t_{(k-1)} < T_r \leq\ t_k \mid x, t_r, V)$$

$$= \{\theta_r(t_k \mid x, V_r)$$

$$\times \left[\exp\left[-\sum_{j=2}^{k-1}\left[\theta_e\left(t_j \mid x, t_r, V_e\right) + \theta_r\left(t_j \mid x, V_r\right)\right]\right.\right.$$

$$\left.\left. - \left[\theta_e(t_k \mid x, t_r, V_e) + \theta_r(t_k \mid x, V_r)\right](\text{t-k+1})\right]\right]$$

$$\times \left[1 - \exp\left[-\theta_e(t_m \mid x, t_r, V_e)(\text{k-t})\right]\right]\}$$

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 1st month. The conditioning event is given by $D_0(V)$:

$$D_0(V) = \int_0^1 \exp\left[-\int_{t_0}^1 [\theta_e(s - t_0 \mid x, t_r, V_e) + \theta_r(s - t_0 \mid x, V_r)]ds\right]dt_0$$

$$= \left\{\frac{1}{\theta_e(t_1 \mid x, t_r, V_e) + \theta_r(t_1 \mid x, V_r)}\right.$$

$$\left.\times \left[1 - \left[exp\left[-\theta_e(t_1 \mid x, t_r, V_e) - \theta_r(t_1 \mid x, V_r)\right]\right]\right]\right\}$$

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:

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

where G(V) is the joint distribution of the unobserved heterogeneity terms. Unobserved heterogeneity is specified non-parametrically, using the approach of Heckman & 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-criterion decreases (Gaure e.a. 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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