A Field Experiment on Labor Market Speeddates for Unemployed Workers*

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Abstract

We conduct a randomized field experiment to evaluate the effectiveness of speeddate events at which unemployed workers get in contact with private employment agencies. Using administrative data, we find that participation in such an event has an immediate positive impact on job finding. One month after the speeddates, treated participants are 20 percent more likely to be employed. Effects last about one year, indicating that temporary jobs at private employment agencies do not serve as stepping stones toward subsequent employment. We find that the UI administration saves about 400 euros per invited job seeker on benefits payments, while the organization of the speeddate events costs only about 4 euros per invited job seeker. Additional survey evidence collected shortly after the events shows that treated job seekers report lower reservation wages and higher job search motivation. Our results point towards the presence of substantial search frictions in the labor market for unemployed benefit recipients.

Keywords: Matching events, job search assistance, active labor market policies, randomization, temporary employment

JEL Classification Numbers: J64, J65, C21, C93

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1 Introduction

Search frictions are an important source of inefficiency in the labor market. To reduce these frictions, many countries rely on various types of labor-market policies. In a recent meta analysis of more than 800 estimates from 207 studies, Card et al. (2015) find that job-search assistance programs are relatively effective in increasing job-finding rates. This holds especially in the short run and for disadvantaged job seekers.[1] However, many programs involve high costs as they include intensive counseling by caseworkers.[2]

This paper focuses on a novel policy instrument which consists of matching events organized by the unemployment insurance (UI) administration. At these meetings, referred to as *speeddates*, benefit recipients meet a large number of private employment agencies. The goal of the program is to stimulate unemployed workers to enter the job market via temporary employment, which should serve as a *stepping stone* toward subsequent employment. In addition, the private employment agencies provide feedback to job searchers on their CV and job-talk skills, which may also help to find employment.

These matching events differ from usual job-search assistance programs in various ways. First, they allow job seekers to directly interact with many employers in a time-effective way while no caseworkers is involved. Second, participation takes at most a few hours. Therefore, it is not likely that job seekers reduce search effort as a response to matching events (lock-in effect), which is a concern of intensive activation programs (Sianesi, 2004; Rosholm, 2008). An impact on job search behavior prior to the intervention due to a threat effect is unlikely either because attendance is voluntary and events are announced just two to three weeks in advance. Finally, the program involves low costs for the UI administration as caseworkers only host and organize the events but do not provide direct assistance to job seekers.

[1] Earlier reviews by Card et al. (2010) and Kluve (2010) find qualitatively similar results based on fewer estimates and a higher share of non-experimental studies.

[2] Activation programs are evaluated in a wide range of countries including the Netherlands (Gorter and Kalb, 1996; Van den Berg and Van der Klaauw, 2006), Sweden (Sianesi, 2004), Norway (Røed and Raaum, 2006), Denmark (Rosholm, 2008; Graversen and Van Ours, 2008), United Kingdom (Dolton and O'Neill, 1996, 2002; Blundell et al., 2004) and the US (Johnson and Klepinger, 1994; Black et al., 2003; Ashenfelter et al., 2005).

To evaluate the effectiveness of the program, we conduct a randomized experiment with more than 13,000 individuals. Invitations to the matching events are sent out to a random subsample of eligible benefit recipients. Because participation is not compulsory, this experiment exploits an encouragement design. Benefit recipients in the control group are not informed and do not participate in matching events. This allows us to interpret instrumental-variable estimates as average treatment effects on the treated (ATET). For the analysis, we use administrative data on employment and income complemented with survey data on job-search behavior.

The results of our study are informative about the presence of search frictions in the labor market. Because all job seekers can contact and register with employment agencies on their own initiative, meeting events do not provide additional job opportunities to benefit recipients. A positive treatment effect suggests that some job searchers are not aware of available vacancies. Furthermore, the experimental setting allows us to examine the impact of temporary work on subsequent labor-market prospects. If temporary employment serves as a stepping stone, there should also be a positive effect on the job-finding rate in the long run. Finally, the experiment is informative about changes in job-search behavior during the search process. We analyze whether the meetings with private employment agencies affect expectations and search effort of benefit recipients.

Our study relates to other experimental evaluations of counseling schemes.[3] Crépon et al. (2013b) conduct a randomized controlled trial to assess the effects of job-search support for long-term welfare recipients in France. Although job seekers are significantly more likely to find employment, the program is not cost effective. In another experiment, Crépon et al. (2013a) analyze a large-scale job-search assistance program for young, educated individuals with long unemployment spells. By varying the treatment intensity between regions, the study estimates positive effects on the job-finding rate but also negative externalities on both ineligible and eligible job seekers. Correcting for displacement effects, the overall impact on job creation is likewise small compared to the program costs. To contrast the effectiveness of private and public providers, Behaghel et al. (2014) conduct an experiment on counseling job searchers who are at risk of long-term unemploy-

[3]For non-experimental evidence, see Weber and Hofer (2004) and Crépon et al. (2005).

ment. They find that public programs have a lower take-up rate but are more cost effective due to higher job-finding rates among participating job seekers. In a recent study, Cottier et al. (2015) evaluate the performance of a large private job-search assistance provider in Switzerland and find a short-lived positive impact on job finding which even turns negative two years later.

Compared to intensive counseling, direct meetings with private employment agencies might be a more effective instrument to mediate vacancies. Katz et al. (1999) and Houseman et al. (2003) argue that the growth of temporary help agencies in the US since the 1990s has helped to improve job-matching efficiency and reduce unemployment. Other studies on the effects of temporary work, which mostly rely on descriptive evidence, come to similar conclusions. Temporary jobs pay often worse salaries but can serve as a stepping stone into regular employment or, at least, do not have adverse effects in the long run (Booth et al., 2002; Heinrich et al., 2005; Andersson et al., 2009; Kvasnicka, 2009). Using data from the Netherlands, De Graaf-Zijl et al. (2011) show in a duration analysis that temporary jobs shorten unemployment spells but do not increase job-finding rates for permanent work. Yet, workers who had a temporary contract before earn more when they get a permanent job. On the contrast, Autor and Houseman (2010), who exploit rotational assignment of welfare recipients to contractors in the US, find that temporary jobs do not improve and may even be detrimental to long-run labor-market outcomes of job seekers.

In our analysis, we find that participation in matching events has a substantial impact on job finding in the short run. Participants are about 10 percentage points more likely to work one month after the matching event. This effect diminishes in the medium run. One year after the matching event, program effects are small and not statistically significant. Yet, we find substantial effect heterogeneity with respect to type of matching event and characteristics of job seekers. Program effects persist in the long run for low-educated workers and for those who attended a sector-specific matching event. The overall employment effects are almost entirely driven by an increase in temporary work. There is neither evidence for crowding out of regular employment nor for a *stepping-stone* effect into regular employment. Furthermore, we estimate that the program is highly cost-effective for the UI administration. The costs of organizing matching events

are much lower than the reduction in benefit payments. Evidence from a survey which we conducted two weeks after the treatment shows that the matching events have a positive effect on job-search motivation and decrease reservation wages. Both mechanisms explain higher job-finding rates.

The paper is organized as follows. Section 2 discusses the institutional background and the experimental design. In Section 3, we describe the data and provide balancing tests. The estimation strategy and results are presented in Section 4 and 5. Section 6 discusses potential mechanisms. In Section 7, we analyze displacement effects and provide a cost-benefit analysis. Finally, Section 8 concludes.

2 Experimental setting

2.1 Unemployment insurance

Workers in the Netherlands are publicly insured against unemployment.[4] They are entitled to UI benefits when they lose at least five working hours per week or, if they worked less than 10 hours per week, 50 percent of their working hours. Moreover, they should have worked at least 26 out of the 36 weeks prior to unemployment. All eligible job searchers may receive UI benefits for at least three months. The entitlement period to UI benefits is based on the previous employment history. For each calendar year with at least 52 working days, a worker is entitled to one month of benefits, with 38 months being the maximum entitlement period.[5]

The amount of UI benefits is based on earnings in the 12 months prior to unemployment. Workers eligible for UI benefits receive 75 percent of their earnings in the first two months, and 70 percent thereafter. Benefits amount to at least 70 percent of the minimum wage and are in most cases capped at a maximum of about 4,400 euros per month before taxes.[6] If recipients are no longer eligible

[4] An exception are self-employed workers.

[5] Starting in January 2015, newly eligible UI recipients are entitled to one month of benefits for each of the first 10 working years and half a month for each additional year. The maximum entitlement period reduces to 24 months.

[6] The benefit cap is not binding in a few sectors where special arrangements apply.

for UI benefits, they can apply for welfare benefits. The means-tested benefits correspond to 50 percent of the minimum wage and are paid for an indefinite period.[7]

UI benefit recipients have the obligation to write at least one job application each week and to accept all job offers which match their skills. Furthermore, they are required to participate in active labor market policies.

2.2 Private employment agencies

Since February 2011 the UI administration organizes matching events between job searchers and private employment agencies which are referred to as *speed-dates*. These agencies form a considerable part of the Dutch labor market. In 2014, there were more than 6,000 agencies active in the Netherlands. Many of them are specialized in mediating employment for specific sectors, and mostly offer vacancies for low or medium educated workers. Over 30 percent of benefit recipients find work via a private employment agency.

Working contracts usually last for three or six months but can be renewed several times. The mediated firm can stop the employment relation without any costs at any time. After four years or at most six temporary contracts, workers are required to get a permanent contract with the employment agency which provides the same employment protection as contracts with regular employers. If agency workers wish to become regular employees of mediated firms, the firms may be required to pay a transfer fee. On average, about 30 percent of workers hired via private employment agencies will eventually get a contract with the employer. Dutch law requires that agency workers are paid the same wage as workers on a given job who have a contract with the employer. However, they have in most cases less fringe benefits such as leave days, pension plans or sick pay.

2.3 Matching events

The matching events are organized by the local offices of the UI administration and can be either general or targeted towards a specific sector. At the events, benefit recipients have the opportunity to talk to representatives of the private

[7]For more details, see De Groot and Van der Klaauw (2014).

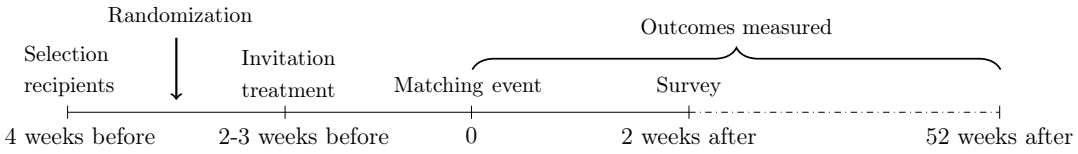


Figure 1: Timeline experiment

employment agencies and get information about vacancies. Some agencies further offer personal feedback and a CV check. The target group of most matching events are job searchers who became unemployed in the past three months. A few UI offices also consider job searchers who are up to 12 months unemployed. Three to four weeks before an event takes place, the UI administration selects eligible benefit recipients and sends out invitations either by mail, e-mail, or an electronic account. Invited job searchers are not required to attend the matching event but participation counts as a job application which all benefit recipients have to make each week.

Due to low costs and moderate organizational effort, matching events with employment agencies have become an increasingly popular instrument of the UI administration in the Netherlands. On average, a case worker only invests about 13 work hours to organize and host a matching event. Although the private employment agencies are not compensated for their participation, previous survey evidence collected by the UI administration shows that most of them consider matching events as a useful tool.

2.4 The experiment

In 2014, the UI administration asked the VU University Amsterdam to analyze the effectiveness of matching events and the decision was made to conduct a randomized experiment. All local UI offices were asked to enroll matching events in the experiment. For each enrolled event, the offices provided a list of eligible benefit recipients and stated how many of them should be randomized in the treatment group.[8]

Figure 1 shows a timeline of the experiment. Treated individuals receive an

[8]When job seekers are repeatedly eligible for a matching event, they remain in the same group as before.

invitation to a matching event about two to three weeks in advance, whereas job searchers in the control group are not affected. It is not known to participants that they take part in an experiment. The relative size of the treatment group is determined by the regional offices and ranges from 50 to 80 percent depending on the pool of potential invitees and the number of participating employment agencies. During the matching event, the UI offices register attendance of every participant.

Two weeks after the matching event, we send out a short online questionnaire to individuals in both control and treatment group. Participation in this survey is voluntary. The aim of the questionnaire is stated vaguely as collecting information to evaluate the services of the UI administration. We make no explicit reference to the evaluation of matching events. Those who have not yet completed the questionnaire receive email reminders one and two weeks later. The survey includes questions about job-search behavior and, in case of the treatment group, about their experience with the matching event. Furthermore, participating employment agencies and the UI administration fill in a short questionnaire about the required time investment and their opinion on the matching event.

In total, 18 matching events have been organized in 11 different regions between June 2014 and March 2016, out of which five events were sector-specific. The number of participants varies considerably ranging from small matching events with 15 to big events with more than 700 participants, where they could talk to between four and 11 private employment agencies. About 12,600 individuals take part in the experiment, of which 76 percent are assigned to the treatment group. The take-up rate among invited benefit recipients is approximately 24 percent. In the control group, nobody participated in matching events. A list of all events with information on size, treatment share and take-up rate is provided in the appendix (Table A.1).

3 Data

For the empirical analysis, we use both administrative and survey data complemented with attendance lists of the matching events provided by the UI administration. The current sample covers eight matching events for which the obser-

vation period following the intervention is sufficiently long.[9] In total, we observe about 8,700 UI benefit recipients in treatment and control group.[10]

3.1 Administrative data and balancing tests

The administrative records of the UI administration include (pre-tax) labor earnings, unemployment benefits, working days and type of work contract, which are the key outcomes of interest. These variables are observed for all individuals up to one year after the matching event. We define an individual as working if any labor earnings are registered for a given period. Furthermore, the administrative records contain a set of individual characteristics. We restrict the estimation sample to individuals for whom data on all variables are available ($N = 8,361$).

Table 1 shows descriptive statistics on characteristics of job searchers in the experiment as well as information on their previous employment and benefits spells measured up to three months before the matching event. Because the matching events differ in the share of individuals assigned to the treatment group, each observation is weighted by its inverse treatment probability. Comparing treatment and control group, we find that differences are very moderate. As shown in the last column, no difference is statistically significant at the 10-percent level. This confirms that the assignment of the treatment is random. Averages should effectively be the same because all characteristics in Table 1 are measured before the matching events.

Around 39 percent of benefit recipients are female and they are, on average, 40 years old. 42 percent are married and the majority completed higher secondary education. One fifth obtained a university degree. In the three months prior to the matching event, individuals collected approximately 1,400 euros in UI benefits and earned 2,400 euros from work. On average, they worked about 21 days in the three months prior to the matching event.[11] Only approximately nine percent of

[9] A future update of data will also cover the more recent events that enrolled in the experiment.

[10] Approximately 10 percent of individuals were twice entitled to participation. In the analysis, we measure treatment effects starting from the first intervention. Before the second event takes place, the effect for these cases corresponds to the impact of the initial event. After that, the treatment effect results from a combination of the first matching event and another potential participation.

[11] Full-time employment corresponds to 21.5 days per month.

Table 1: Descriptive statistics and balancing

	Control group	Treatment group	p-value
Female	0.39	0.38	0.34
	(0.49)	(0.49)	
Age	40.42	40.60	0.55
	(11.87)	(11.87)	
Married	0.42	0.42	0.53
	(0.49)	(0.49)	
Primary/lower secondary education	0.20	0.20	0.43
	(0.40)	(0.40)	
Higher secondary education	0.59	0.58	0.19
	(0.49)	(0.49)	
College/university education	0.21	0.22	0.42
	(0.41)	(0.41)	
Benefits (prev. 3 months)	1475.16	1380.71	0.10
	(2305.66)	(2142.68)	
Earnings (prev. 3 months)	2440.57	2457.96	0.86
	(3690.55)	(4290.46)	
Workdays (prev. 3 months)	21.31	21.72	0.47
	(22.38)	(22.51)	
Perm. contract (prev. 3 months)	0.09	0.08	0.09
	(0.29)	(0.27)	
Observations	2,124	6,237	

NOTE – All estimates are weighted by inverse treatment assignment probabilities. Columns (2) and (3) report means, with standard deviations in parentheses. Column (4) shows p-values of two-sided difference-in-means tests.

individuals had any permanent contract in that period. All variables in Table 1 serve as control variables in the treatment analysis.

The upper panel of Table 2 provides descriptive statistics on matching event attendance and outcome variables four weeks after the intervention. Again statistics are weighted to account for varying treatment shares among the matching events. Whereas the attendance rate in the treatment group is 24 percent, nobody in the control group went to the matching events. Next to UI benefit recipiency and the amount of benefits, we analyze treatment effects on working status and their monthly earnings. Here, we also distinguish between regular work and work for

private employment agencies. Whereas 63 percent of individuals in the control group still receive benefits one month after the matching event, 41 percent are working. More than half of them are employed via an employment agency. Some may have started working but still receive delayed benefits for their unemployment spell. Also, it can be that they work part time and top up their earnings with UI benefits. This is possible in the Dutch UI system if re-employment earnings are low or if they work less hours as compared to previous employment. As a result, average benefit payments are higher than income from work in the control group. Four weeks after the intervention, benefits and earnings (including zero income observations) amount to about 800 and 660 euros, respectively. Comparing the raw means in treatment and control group, we can observe an impact of matching events for all outcome variables. In the treatment group, less individuals collect benefits and the working rate is higher.

3.2 Survey data

The administrative records are complemented with data from an online questionnaire that we sent out two weeks after the intervention. About 23 percent of benefit recipients filled in the complete questionnaire.[12] As shown in Table 2, the response rate does not differ between treatment and control group. Comparing individual characteristics of respondents to the full population, we find that female and higher educated job seekers are significantly more likely to respond to the questionnaire (see Table A.4 in the appendix). To account for selective non-response, we apply inverse-probability weighting based on gender, age and education.

[12]For each matching event, approximately 30 percent of benefit recipients started the questionnaire but some of them did not provide answers to all questions.

Table 2: Descriptives statistics of outcomes

	Control group	Treatment group	p-value	Observations
Attendance	0.00	0.24	0.00	8,361
Administrative outcomes (4 weeks after matching event)				
Collecting benefits	0.63	0.60	0.02	8,361
	(0.48)	(0.49)		
Amount monthly benefits	802.56	744.93	0.01	8,361
	(889.93)	(885.95)		
Working	0.41	0.43	0.08	8,361
	(0.49)	(0.50)		
Amount monthly earnings	662.27	710.67	0.06	8,361
	(1029.34)	(1072.33)		
Work at employment agency	0.21	0.23	0.02	8,361
	(0.41)	(0.42)		
Survey response	0.23	0.23	0.99	8,361
Survey outcomes (2-3 weeks after matching event)				
# employment agencies registered	3.41	3.72	0.05	1,931
	(2.96)	(2.94)		
Job search motivation (1-5 scale)	3.88	4.02	0.01	1,931
	(1.11)	(1.02)		
# applications sent (last 4 weeks)	6.21	6.05	0.43	1,931
	(3.82)	(3.54)		
# job talk invitations (last 4 weeks)	0.67	0.70	0.57	1,931
	(1.04)	(1.01)		
Reservation wage (month, in euros)	2216.53	2092.15	0.02	1,931
	(1045.27)	(913.16)		

Note – All estimates are weighted by inverse treatment assignment probabilities. All survey outcomes are additionally weighted by inverse probability weights to account for selective response. Column (2) and (3) report means, with standard deviations in parentheses. Column (4) shows p-values of two-sided difference-in-means tests.

The lower panel of Table 2 shows summary statistics of outcomes reported in the survey. Next to a subjective question about the motivation to search for a job, we measure job search performance by the number of applications sent and job talk invitations received in the four weeks prior to the survey. Furthermore, respondents are asked to report the minimum monthly wage for which they are willing to accept work and the number of private employment agencies at which they are currently registered.

On a one-to-five scale, the average job search motivation is about 3.9 among respondents in the control group. We find that an average job searcher sends six applications per month but only receives 0.7 invitations for job interviews. They are registered at, on average, 3.4 employment agencies and report a reservation wage of about $2,200$ euros a month. A first comparison between means in control and treatment group shows significant differences in three survey outcomes. Benefit recipients in the treatment group are registered at more employment agencies, claim to be more motivated and are willing to work for a lower wage.

4 Estimation strategy

To estimate the impact of matching events on labor-market outcomes, we specify the regression model

$$Y_{is} = \mu_s + \delta T_i + X_{is}^{'}\beta + u_{is}$$

where Y_{is} denotes the outcome of individual i who was eligible for participation in matching event s. T_i indicates whether the individual attended a matching event. X_{is} is a vector of observed individual characteristics as described in Table 1. Matching event fixed effects, denoted by μ_s, account for different treatment assignment probabilities of matching events. Whereas X_{is} is included to increase the precision of estimates, we have to include μ_s to avoid biased estimates because size, composition, and assignment to treatment group differ between matching events. Finally, u_{is} denotes the error term.

Not all individuals who are assigned to the treatment group eventually attend the matching event. Because actual participation might depend on unobserved

characteristics that affect Y_{is}, coefficient δ does not identify the average impact of matching events. Instead, we focus on two alternative estimates to capture program effects. By regressing the outcome variable on matching event assignment (Z_i) instead of participation (T_i), we estimate the intention-to-treat effect (ITT). The coefficient on Z_i then corresponds to the average of a zero effect for invited job searchers who did not participate and the treatment effect for participants. If the take-up rate is low, it is likely that this estimate is lower than the actual treatment effect. It is informative from a policy perspective as it shows the impact per assigned benefit recipient and can be used to evaluate the cost-effectiveness of the program.

Next to the ITT, we estimate the treatment effect for individuals who comply with the assignment (*compliers*). Since attendance of matching events is not enforced, effects for this subgroup are more informative on the effectiveness of the program than the average effect across all benefit recipients. The impact on compliers corresponds to the local average treatment effect (LATE) which can be estimated by means of instrumental variable estimation (Angrist and Krueger, 1999). To obtain exogenous variation in participation, we use the randomized assignment as instrument. Because matching event attendance is only possible upon invitation, there are no job seekers who are always treated independent of assignment (*always takers*). Thus, the LATE estimate corresponds to the average treatment effect on the treated (ATET), which is the average impact on all treated job seekers.

4.1 First-stage results and identification

Table 3 shows estimation results for regressions of actual participation in the matching event on assignment to the treatment group, which is the first-stage regression of the IV approach. The coefficient on assignment corresponds to the attendance rate of 24 percent shown in Table 2. Including individual characteristics as controls in the second column does not affect the estimated take-up rate but slightly reduces the standard error.

The last two columns of Table 3 test for differences in take-up rates by matching event size and type. When we include an interaction term of the assignment

Table 3: First-stage estimates

Attendance	(1)	(2)	(3)	(4)
Treatment assignment	0.237***	0.237***	0.241***	0.238***
	(0.010)	(0.009)	(0.011)	(0.019)
Sector-specific × treatment			-0.018	
			(0.023)	
Size × treatment				-0.000
				(0.000)
Control group mean	0.000	0.000	0.000	0.000
Characteristics	No	Yes	Yes	Yes
F-statistic (on excl. instruments)	615.81	639.13	319.86	319.53

NOTE – $N = 8,361$. All regressions include matching event fixed effects. Standard errors are reported in parentheses; * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

dummy and an indicator variable for sector-specific matching events, we obtain a small and insignificant coefficient. Interacting treatment assignment with event size, i.e. the number of job seekers at a matching event, leads to similar results. This shows that attendance shares do not systematically vary between matching events.

Randomization takes place at the moment of invitation which is approximately two weeks before the matching event. To identify average treatment effects on the treated (ATET), the invitation itself should have no direct impact on outcomes. Previous studies have found evidence for threat effects of assistance programs due to job seekers who exit unemployment after the announcement to avoid the treatment. Even though participation is not compulsory for invited benefit recipients in our experiment, receiving an invitation may already affect their job search behavior. The invitation might put additional pressure on job seekers and thereby increase search effort. Some job seekers may also learn about or reconsider the possibility of working via private employment agencies. If these effects occur, the IV approach is violated because the invitation to a matching event then has itself a direct effect on outcomes.

To formally test for the presence of anticipation effects, we compare outcome

variables in control and treatment group in the period between invitation and matching event. The difference in the share of working individuals one week before a matching event is small and not significant ($p-value = 0.52$). Similar results can be observed for the share of individuals who collect UI benefits ($p-value = 0.17$).

4.2 Theoretical predictions

At the matching event, individuals meet many private employment agencies within a short period of time. Theses agencies offer temporary jobs at various employers. If the program is effective, attendance of matching events should lead to higher job-finding rates and less benefit recipients.

In the short run, we expect the positive employment effects to mainly be driven by higher job-finding rates at employment agencies. Attendees might directly get a job offer at the matching events or they register with the agencies and can be considered for vacancies in the future. Many matching events also offer a CV check and provide individuals with feedback on their application skills. If this service positively affects the job search process, we should also observe higher job-finding rates for regular work in the short run.

Furthermore, the program might change job search behavior of participants. Yet, effects on the job-finding rate are less clear in this case. On the one hand, matching events may increase motivation and search effort if job seekers are able to gather useful information about available vacancies on the labor market. On the other hand, the intervention could also have negative effects. Attending a matching event might crowd out other job search activities. Also, job seekers could get discouraged if they learn that job-finding prospects are worse than expected. Depending on the information they obtain, reservation wages may likewise be positively or negatively affected.

The overall increase in work does not need to coincide with a decrease of the same size in the share of individuals collecting benefits. As shown in the previous section, some workers top up their earnings with UI benefit payments. Because working hours at private employment agencies vary more strongly, more individuals may start working but keep a share of their benefits.

In the long run, the positive impact on agency work might become smaller

because most contracts with employment agencies are short term. In return, we should observe an increase in regular employment if agency work serves as a stepping stone. However, it is also possible that temporary work crowds out job search effort which in turn decreases the share of individuals finding regular work. Depending on which effect prevails, the long-run impact on regular employment may either be positive or negative.

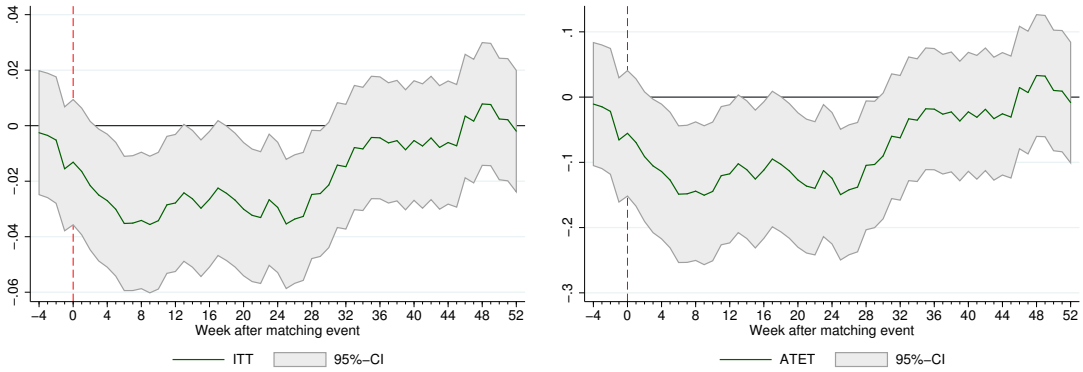
5 Results

5.1 Impact on labor-market outcomes

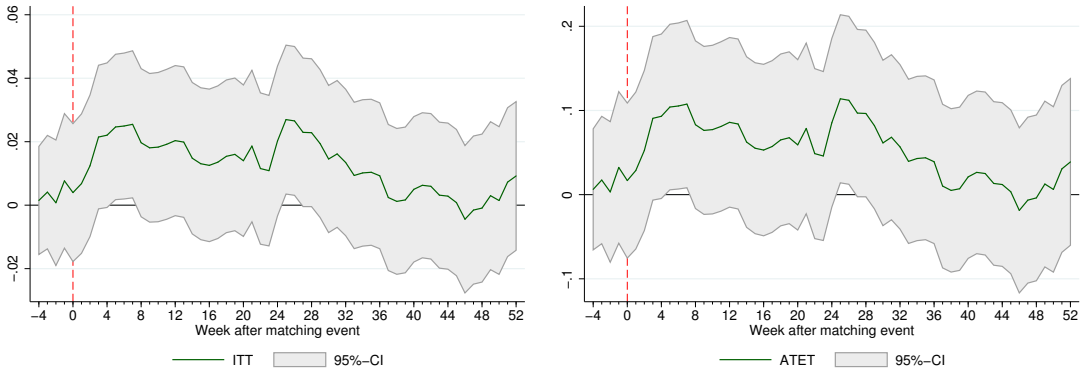
In this section, we present estimation results for the impact of matching events on unemployment benefits, work status, labor earnings, and other re-employment job characteristics using the administrative data. Figure 2 plots ITT and ATET estimates by week after the matching event for collecting benefits and being employed. Corresponding graphs for the amount of monthly benefits and earnings can be found in the appendix (Figure A.2a and A.2b). The graphs illustrate a clear impact of the program on both outcomes. ITT and ATET estimates show very similar patterns over time but the latter are larger by a factor of about four. This is not surprising as they capture the specific impact for benefit recipients who actually attended the matching event. The upper plots show that invited job seekers are significantly less likely to collect unemployment benefits in the short run. In the first eight weeks after a matching event, the effect size increases steadily. In the subsequent 20 weeks, differences in collecting benefits between treatment and control group are approximately three percentage points. Afterwards, the impact diminishes again, and 36 weeks after the matching event, differences are close to zero and not statistically significant.

The lower plots of Figure 2 illustrate the impact on being employed during a given week after the event. The pattern of estimated effects on employment mirror these of benefit recipiency. Again, there is a positive impact in the short run that diminishes after about 28 weeks. Compared to the effect on the share of benefit recipients, estimates are somewhat smaller and measured with larger standard errors. Also, effect sizes increase faster in the beginning and decrease

Figure 2: ITT and ATET estimates by week after matching event



(a) Impact on collecting UI benefits



(b) Impact on having work

more steadily later on.

To better quantify the impact of matching events, we summarize ITT and ATET point estimates as well as control group means and standard deviations for various labor-market outcomes in Tables 4 to 7. All outcomes are measured one, six and twelve months after the matching event to capture both short-term and medium-term effects.

We first consider benefit receipt. Column (1) to (3) of Table 4 show the impact on the probability to collect UI benefits. Compared to the share of individuals in the control group who remain collecting benefits after one month, the ATET estimate indicates that the average among participants of matching events is approximately 20 percent lower. Another five months later, the relative effect grows

Table 4: Impact on UI benefits

	Collecting benefits			Amount monthly benefits		
	+1m	+6m	+12m	+1m	+6m	+12m
Intention-to-Treat Estimates (ITT)						
Invited	-0.027**	-0.034***	-0.002	-46.568**	-62.045***	2.384
	(0.012)	(0.012)	(0.011)	(21.308)	(23.105)	(16.985)
Treatment-on-the-Treated Estimates (ATET)						
Attended	-0.114**	-0.142***	-0.008	-196.648**	-262.007***	10.066
	(0.053)	(0.051)	(0.047)	(91.847)	(99.167)	(71.699)
Control group mean and standard deviation of outcomes						
Mean	0.63	0.38	0.28	802.56	574.57	332.54
Standard deviation	(0.48)	(0.49)	(0.45)	(889.93)	(995.05)	(665.99)

NOTE – $N = 8,361$. ITT estimates are obtained through OLS regressions. ATET estimates involve IV regressions using treatment assignment as an instrument for attendance. Outcomes are measured one ("+1m"), six ("+6m") and twelve ("+12m") months after the matching event. All regressions control for matching event fixed effects as well as a set of individual characteristics (gender, age, marital status, education) and previous job characteristics (earnings, benefits, permanent contract, working days) measured in the three months before the matching event; * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

to a difference of more than 30 percent. As expected, the intention-to-treat estimates are considerably smaller but still amount to a 5-10 percent difference compared to those who were not invited to the matching event. Despite these comparatively strong effects, estimated coefficients drop to virtually zero again one year after the matching event. Column (4) to (6) of Table 4 show the impact on the amount of paid UI benefits. One month after the matching event, participants receive significantly lower benefits. Comparable to previous estimates for the benefit indicator, we observe that the effect size is slightly larger six months after the event. ITT estimates show a 7-12 percent difference to individuals who were not invited for a matching event. The decrease does not need to be solely driven by a lower share of recipients. Some job seekers might have found part-time work due to the matching event but still receive a share of the benefits if their earnings are low. However, we cannot distinguish both channels because those who still collect benefits are a selective sample of job seekers. As for the benefit indicator, estimated differences vanish again twelve months later. Given that

participants do not differ in the probability of benefit receipt after one year, we can, in this case, conclude that the amount of benefits conditional on collecting benefits is not affected either.

Next, we provide estimates for the impact on being employed and earnings in Table 5. As illustrated in Figure 2, individuals in the treatment group are more likely to work in the short run. The ATET estimate after one month shows that the difference between treated and non-treated participants equals nine percentage points, or 22 percent. When we use the earnings indicator six months after a matching event as dependent variable, we find similar estimates. Compared to the decrease in benefit receipt after six months, the positive impact on the working rate is slightly less pronounced. Some individuals who started working due to the program might be unemployed again but have not returned to benefit receipt yet. This interpretation is consistent with the observed long-term impact of matching effects. After one year, ITT and ATET estimates decline again and turn insignificant. The last three columns of Table 5 examine the impact on the amount of earnings. The ATET coefficients translate into a difference of 25-30 percent in monthly earnings one and six month after the matching event before diminishing again to zero. This confirms that the positive impact of the program eventually disappears. The increase in earnings during the period of observation is comparable to the decrease in UI benefits. We estimate that effects on the sum of earnings and benefits are never significantly different from zero (see Figure A.2c in the appendix).

Point estimates of Table 4 and 5 suggest that matching events decrease benefit receipt and increase job finding. It is also possible that some individuals start working but still receive a share of their benefits if they do not find sufficient work. To shed more light on this, we re-estimate treatment effects for the combination of collecting UI benefits and working. Table A.2 in the appendix shows that the impact on working and receiving benefits is clearly driven by job searchers switching from benefits without work to working without benefits. Individuals who neither collect benefits nor work, or do both, are not significantly affected.

Table 6 compares the impact on work through an employment agency contract to effects on work under regular contracts. The regression results show that the positive employment effects reported in Table 5 are mostly driven by agency

Table 5: Impact on employment and earnings

	Working			Amount monthly earnings		
	+1m	+6m	+12m	+1m	+6m	+12m
Intention-to-Treat Estimates (ITT)						
Invited	0.022*	0.026**	0.009	48.857*	62.239**	-1.593
	(0.012)	(0.012)	(0.012)	(25.229)	(29.916)	(29.132)
Treatment-on-the-Treated Estimates (ATET)						
Attended	0.092*	0.111**	0.038	206.315*	262.823**	-6.726
	(0.050)	(0.051)	(0.051)	(108.438)	(127.457)	(123.011)
Control group mean and standard deviation of outcomes						
Mean	0.41	0.57	0.63	662.27	1033.86	1194.82
Standard deviation	(0.49)	(0.49)	(0.48)	(1029.34)	(1193.43)	(1250.27)

NOTE – $N = 8,361$. ITT estimates are obtained through OLS regressions. ATET estimates involve IV regressions using treatment assignment as an instrument for attendance. Outcomes are measured one ("+1m"), six ("+6m") and twelve ("+12m") months after the matching event. All regressions control for matching event fixed effects as well as a set of individual characteristics (gender, age, marital status, education) and previous job characteristics (earnings, benefits, permanent contract, working days) measured in the three months before the matching event; * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

work. ITT and ATET coefficients for this type of work are similar to the overall impact on employment whereas effects on working under regular contracts are insignificant and much smaller. On the one hand, the latter result suggests that the treatment does not lead to crowding out of regular employment. On the other hand, we do not find evidence that jobs through employment agencies serve as stepping stone into regular employment. This concurs with the findings by De Graaf-Zijl et al. (2011) for the Netherlands.

Finally, we examine how matching events affect working days and the type of contract. The first three columns of Table 7 report effects on the number of monthly working days. As expected, we find positive differences between treatment and control group. One and six months after the matching event, participants have worked, on average, 2 days more. Compared to average working days in the control group, the relative increase amounts to 20-30 percent, and is, thus, slightly smaller than effects on the overall working rate.

Table 6: Impact on employment agency work

	Work at employment agency			Other work		
	+1m	+6m	+12m	+1m	+6m	+12m
Intention-to-Treat Estimates (ITT)						
Invited	0.018*	0.030***	0.006	0.007	0.002	0.005
	(0.010)	(0.011)	(0.011)	(0.010)	(0.012)	(0.012)
Treatment-on-the-Treated Estimates (ATET)						
Attended	0.074*	0.125***	0.026	0.028	0.008	0.021
	(0.042)	(0.046)	(0.046)	(0.043)	(0.050)	(0.052)
Control group mean and standard deviation of outcomes						
Mean	0.21	0.24	0.26	0.21	0.35	0.39
Standard deviation	(0.41)	(0.42)	(0.44)	(0.41)	(0.48)	(0.49)

NOTE – $N = 8,361$. ITT estimates are obtained through OLS regressions. ATET estimates involve IV regressions using treatment assignment as an instrument for attendance. Outcomes are measured one ("+1m"), six ("+6m") and twelve ("+12m") months after the matching event. All regressions control for matching event fixed effects as well as a set of individual characteristics (gender, age, marital status, education) and previous job characteristics (earnings, benefits, permanent contract, working days) measured in the three months before the matching event; * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

To check whether matching events lead to different contract types, we estimate the impact on working days separately for jobs with temporary and permanent contracts. Whereas contracts with private employment agencies are almost exclusively temporary, regular working contracts can be of both types. Yet, about 75 percent of benefit recipients who find regular work start with a temporary contract, too. The last six columns of Table 7 show that matching events increase temporary and permanent contract working days to a different extent. As expected, most of the difference in working days can be explained by temporary contracts.

The estimation results of this section show that matching events increase job-finding rates in the short run. The effect size is relatively large and mostly driven by additional temporary work at private employment agencies. This suggests that the underlying mechanism is a reduction in search frictions due to mediation of

vacancies during or after the matching events. Because regular work is hardly affected, improved job search skills due to CV checks or feedback provided at the matching events does not seem to be a relevant mechanism here.

Table 7: Impact on working days

Monthly working days	Total			w/ temp. contract			w/ perm. contract		
	+1m	+6m	+12m	+1m	+6m	+12m	+1m	+6m	+12m
Intention-to-Treat Estimates (ITT)									
Invited	0.466**	0.448*	0.077	0.335	0.452*	-0.058	0.130	-0.003	0.135
	(0.230)	(0.246)	(0.247)	(0.214)	(0.236)	(0.240)	(0.109)	(0.130)	(0.147)
Treatment-on-the-Treated Estimates (ATET)									
Attended	1.967**	1.892*	0.324	1.417	1.907*	-0.245	0.550	-0.014	0.569
	(0.989)	(1.046)	(1.043)	(0.915)	(1.002)	(1.015)	(0.460)	(0.548)	(0.622)
Control group mean and standard deviation of outcomes									
Mean	6.74	10.01	11.35	6.05	8.72	9.81	0.70	1.29	1.54
Standard deviation	(9.23)	(10.12)	(9.93)	(8.91)	(10.00)	(9.98)	(3.62)	(4.82)	(5.24)

NOTE – $N = 8,361$. ITT estimates are obtained through OLS regressions. ATET estimates involve IV regressions using treatment assignment as an instrument for attendance. Outcomes are measured one ("+1m"), six ("+6m") and twelve ("+12m") months after the matching event. All regressions control for matching event fixed effects as well as a set of individual characteristics (gender, age, marital status, education) and previous job characteristics (earnings, benefits, permanent contract, working days) measured in the three months before the matching event; * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

5.2 Heterogenous effects

Effects on job finding may differ between matching events and by individual characteristics of benefit recipients. To check whether events with a smaller number of participants are more effective, we interact the treatment dummy with event size and re-estimate the ATET. The resulting coefficient on the interaction term is insignificant, indicating no systematic relationship between effectiveness and the number of participants. As described in Section 2, matching events also differ by target group and are either general or sector specific. In the current sample, five events were general matching events whereas the remaining events were tailored to job searchers seeking employment in the technology, construction, or business service sector. To examine differences between both types, we split the sample by general and sector-specific matching events and estimate ATET coefficients using the working indicator as dependent variable. As shown in the first column of the upper panel of Table 8, coefficients for general matching events are small and

insignificant. After one year, the estimate even turns negative. Industry-specific matching events have much larger effects, suggesting that more specialized events lead to more matches between job searchers and employment agencies. Even after 12 months, large and significant estimates persist. Because most benefit recipients are invited to general matching events, these long-run effects average out to zero when we estimate effects for the full sample.

In the remainder of this section, we compare matching event effects for different subgroups of job searchers. Evaluations of active labor-market policies find frequently that the effectiveness of interventions can differ by observed individual characteristics (Card et al., 2015). ATET estimates by gender, age, duration of unemployment, education and desired work hours are reported in the remaining columns of Table 8. The coefficients show that women exhibit a larger effect one month after the treatment but, another five months later, the positive impact is fully explained by men. One year after the matching event, no coefficient is statistically significant. Furthermore, we find that positive effects of matching events are in all months driven by individuals who are younger than 40. To analyze differences by duration of unemployment, we split the sample into job seekers who have been unemployed less than three months before the matching event and those with longer unemployment spells. The estimates show that effects are somewhat larger for job searchers who are less than three months unemployed. These results suggest that private employment agencies find it more difficult to mediate older or long-term unemployed workers on the labor market.

Columns three to five of the lower panel of Table 8 show ATET estimates by educational attainment. The coefficients indicate that positive employment effects are driven by workers with, at most, a secondary vocational school degree. For the lowest educated, the impact even persists after one year. On the contrary, we find no effect for college and university educated job searchers. This is consistent with the notion that most private employment agencies in the Netherlands offer only few vacancies for higher educated workers. Finally, we consider effect sizes by desired scope of work. Comparing job searchers who seek full-time work to those who look for part-time work, we obtain ATET estimates that are larger for individuals seeking part-time work in the short run. After one year, effects are stronger for full-time job searchers. However, the coefficients are in most cases

too small to be statistically significant.

To check whether some of these effects are driven by correlations between characteristics, we estimate their joint impact by including interaction terms of attendance and characteristics in the ATET regressions. Estimation results, presented in the appendix (Table A.3), confirm that effect sizes differ in education and type of matching event whereas estimates for age, scope of work and duration of previous employment spell are small and insignificant.

Overall, the analysis of heterogeneous effects shows that treatment effects do not consistently diminish to zero one year after the intervention. Significant ATET estimates persist for sector-specific matching events as well as for low-educated individuals.

Table 8: Heterogeneous effects - Working (ATET estimates)

	Type of event		**Gender**		**Age**	
	General	Sector	Female	Male	< 40	≥ 40
- after 1 month	0.077	0.149	0.133*	0.054	0.259**	-0.011
	(0.055)	(0.110)	(0.078)	(0.064)	(0.107)	(0.051)
- after 6 months	0.056	0.336***	-0.022	0.191***	0.180*	0.054
	(0.056)	(0.122)	(0.080)	(0.066)	(0.102)	(0.055)
- after 12 months	-0.023	0.282**	-0.075	0.104	0.084	-0.004
	(0.056)	(0.120)	(0.081)	(0.065)	(0.100)	(0.055)
Observations	6,982	1,379	3,204	5,157	4,082	4,279
	Unemployed		**Education**			**Scope of work**
	< 3 mo.	≥ 3 mo.	Low	Medium	High	Part-time	Full-time
- after 1 month	0.106*	0.027	0.094	0.104	0.068	0.117	0.076
	(0.057)	(0.100)	(0.125)	(0.068)	(0.087)	(0.088)	(0.060)
- after 6 months	0.115**	0.106	0.287**	0.129*	-0.045	0.124	0.107*
	(0.058)	(0.109)	(0.136)	(0.069)	(0.089)	(0.092)	(0.061)
- after 12 months	0.042	0.027	0.281**	-0.002	-0.035	-0.011	0.059
	(0.057)	(0.107)	(0.137)	(0.069)	(0.088)	(0.093)	(0.060)
Observations	6,769	1,592	1,683	4,872	1,806	2,669	5,692

NOTE – Estimates are obtained through IV regressions using treatment assignment as an instrument for attendance. All regressions control for matching event fixed effects as well as a set of individual characteristics (gender, age, marital status, education) and previous job characteristics (earnings, benefits, permanent contract, working days) measured in the three months before the matching event; educational levels are defined as follows: elementary school or less (low), high school or/and secondary vocational school (medium), college or university (high); * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

6 Mechanisms

6.1 Who are the compliers?

In the survey, we ask benefit recipients who were invited but did not attend a matching invent about the reason for their absence. Most respondents claim that they either did not notice the invitation (36%) or did not have time when the matching event took place (27%).[13] About 16 percent already found work by the time of the event. Another 14 percent was absent because they do not expect to find work at matching events or do not want to work via private employment agencies. The remaining non-compliers report to be absent for other reasons such as illness or childcare.

Given the low take-up rate, it is informative to know whether and how participating job seekers (*compliers*) and those who did not attend (*never-takers*) differ in terms of observable characteristics. Analogous to the balancing tests in Section 3.1, a comparison of never-takers and compliers is provided in Table 9. As types in the control group are not revealed, we focus only on *compliers* and *never-takers* in the treatment group ($N = 6,237$).[14] p-values in the third column of Table 9 show that both types differ significantly in most of the observed characteristics. Compliers are somewhat more likely to be women and are on average more than five years older. Furthermore, we observe that participating job seekers are more often married and better educated. The share of university educated among the compliers is 40 percent higher. When we compare labor-market characteristics of compliers and never-takers in the three months before the intervention, we observe further significant differences. Compliers work less days, have lower earnings and collect more benefits in the three months before the matching events. The share of workers with a permanent contract during that period is similar for both groups.

The previous analysis of heterogeneous effects suggests that the potential impact of matching events might be different for complier and never-takers because

[13] Comparing the matching events, we find that invitations sent by letter are the most likely to be noticed by benefit recipients whereas those sent to the electronic account are the least likely.

[14] Due to random assignment, the two types in the treatment group are representative for all job seekers.

Table 9: Comparison of never-takers and compliers

	Never-takers	Compliers	p-value
Female	0.37	0.40	0.04
	(0.48)	(0.49)	
Age	39.23	44.88	0.00
	(11.65)	(11.54)	
Married	0.39	0.50	0.00
	(0.49)	(0.50)	
Primary/lower secondary education	0.21	0.17	0.00
	(0.41)	(0.38)	
Higher secondary education	0.59	0.55	0.03
	(0.49)	(0.50)	
College/university education	0.20	0.28	0.00
	(0.40)	(0.45)	
Benefits (prev. 3 months)	1342.67	1499.89	0.02
	(2083.56)	(2314.86)	
Earnings (prev. 3 months)	2593.76	2032.48	0.00
	(4619.07)	(3000.71)	
Workdays (prev. 3 months)	23.23	16.98	0.00
	(22.52)	(21.84)	
Perm. contract (prev. 3 months)	0.08	0.08	0.92
	(0.27)	(0.27)	
Observations	4,715	1,522	

NOTE – $N = 6,237$. All estimates are weighted by inverse treatment assignment probabilities. Column (1) and (2) report means, with standard deviations in parentheses.

both groups also differ in most observable characteristics. To further investigate this, we use probit regressions to estimate the individual propensity to comply with the treatment based on observables. As in the treatment analysis, all observable characteristics are included as explaining variables. The estimated propensities to be a complier range from 2 to 66 percent. To examine heterogenous responses to the treatment, we again estimate ATET regressions on the working indicator but include additionally the complier propensity as well as its interaction term with attendance. Regression results in Table 10 show that job searchers with complier characteristics are significantly less likely to work but only after 12 months. Furthermore, we observe that the interaction terms with attendance are

always negative, indicating that those with lower propensity scores are somewhat more likely to benefit from matching events. This is also illustrated in Figure 3, which plots the share of employed individuals after six months by propensity score deciles. Benefit recipients with a lower complier propensity are more likely to be employed if they were invited to a matching event. For higher propensity deciles, we mostly observe similar shares in both groups.

Table 10: Impact by complier propensity - Working (ATET estimates)

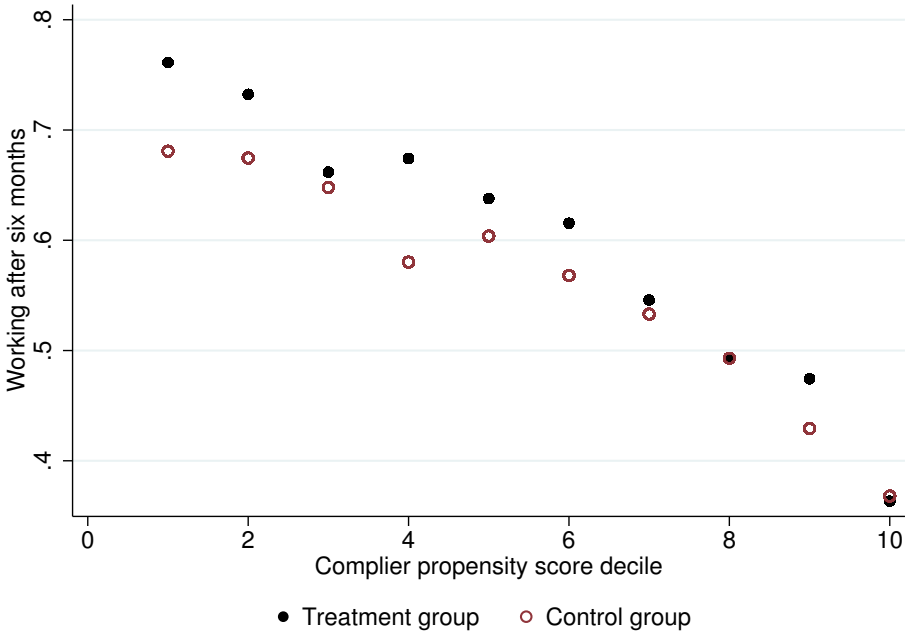
	After 1 month	After 6 months	After 12 months
Attended	0.245	0.403**	0.206
	(0.158)	(0.161)	(0.159)
Compl. prop. × attended	-0.518	-0.984**	-0.565
	(0.439)	(0.448)	(0.443)
Complier propensity	0.002	-0.307	-0.834**
	(0.420)	(0.429)	(0.424)
Constant	0.352***	0.579***	0.689***
	(0.074)	(0.076)	(0.075)

NOTE – $N = 8,361$. Estimates are obtained through IV regressions using treatment assignment as an instrument for attendance. All regressions control for matching event fixed effects as well as a set of individual characteristics (gender, age, marital status, education) and previous job characteristics (earnings, benefits, permanent contract, working days) measured in the three months before the matching event; * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

6.2 Job search behavior

We found that invited benefit recipients are more likely to work via private employment agencies in the first months after the matching events. This could be explained by a decrease in search frictions. If job seekers learn about vacancies that they were not aware of, the job-finding rate rises. Another possibility is that participants update their beliefs about private employment agencies and prospects on the labor market, which could have an indirect effect on employment. To distinguish both channels, we use survey data to examine changes in job search behavior. Following the same format as in the previous Section, Table 11 reports ITT and ATET estimates for six different survey outcomes measured

Figure 3: Working rate after six months by complier propensity decile



two to three weeks after the matching event.[15]

As shown in the first column, matching events significantly increase the number of private employment agencies at which job searchers are registered. On average, treated benefit recipients report one additional agency. This shows that the program widens the scope of search which can help to find work even when the matching event does not directly result in a job offer.

Next, we analyze whether the program changes the motivation of job seekers. Getting in touch with potential employers and receiving feedback might foster motivation but the program could also have negative effects when offered vacancies do not match with prior expectations. Estimation results in the second column show that participants are significantly more motivated. Compared to a control group mean of 3.9, treated benefit recipients report a 0.4 points higher motivation

[15]Unweighted estimates are presented for comparison in the appendix (see Table A.5). If job searchers have already found work by the time of the survey, the outcomes refer to job-search behavior during the last job-search period. One exception is the question about expectations to find work within the next three months (Column (2) in Table 11). Here, we only asked respondents who were still unemployed when the survey took place.

Table 11: Impact on job search behavior (weighted survey data)

	# employment agencies registered	Job search motivation (1-5 scale)	# applications sent (last 4 weeks)	# job talk invitations (last 4 weeks)	Reservation wage (month, in euros)
Intention-to-Treat Estimates (ITT)					
Treatment	0.376**	0.139**	-0.153	0.052	-92.18*
	(0.179)	(0.069)	(0.232)	(0.064)	(47.17)
*Treatment-on-the-Treated Estimates (*ATET*)*					
Attendance	1.022**	0.378**	-0.415	0.142	-250.64*
	(0.485)	(0.190)	(0.630)	(0.173)	(128.26)
Control group mean and standard deviation of outcomes					
Mean	3.41	3.88	6.21	0.67	2216.53
SD	(2.96)	(1.11)	(3.82)	(1.04)	(1045.27)

NOTE – $N = 1,931$. Observations are weighted by inverse probability weights to account for selective response. ITT estimates are obtained through OLS regressions. ATET estimates involve IV regressions using treatment assignment as an instrument for attendance. All outcomes are measured 2-3 weeks after the matching event. If individuals already found work, all outcomes except for column (2) refer to the previous job search period. All regressions control for matching event fixed effects as well as a set of individual characteristics (gender, age, education); * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

on a one-to-five scale. Yet, we do not find significant differences for other proxies of job search effort. Participants do not write more applications. The number of job interviews is about 25 percent higher for treated job searchers but the standard error is large too. This holds as well when we additionally control for the number of applications.

The last column shows treatment effects on reported reservation wages. If job searchers obtain additional information about their labor-market prospects during the matching event, some might adjust their reservation wage accordingly. This is in fact what we observe in the experiment. The ATET estimate indicates that matching events result in significantly lower reservation wages with a difference of 250 euros, which corresponds to 12 percent of the control group mean.

6.3 Earnings distributions

Matching events might decrease reservation wages because some job seekers have too high earnings expectations and learn during the matching event about actual opportunities on the labor market. If reported reservation wages decline, we should also observe lower accepted wages in the administrative data. This could be another explanation why matching events lead to higher job-finding rates. To examine changes in the wage structure, we estimate earnings distributions of treated and non-treated compliers.

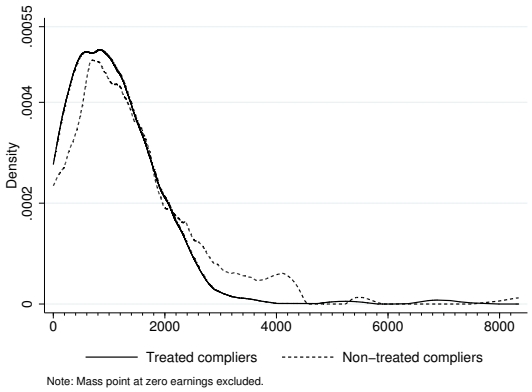
Because there are no always-takers in the experiment, treated compliers are all individuals in the treatment group who attended a matching event. Following Imbens and Rubin (1997), we recover the earnings distribution of non-treated compliers from the distribution of all individuals in the control group and the distribution of non-compliers in the treatment group.[16] All complier distributions are estimated separately for each matching event and weighted by size to account for different shares of compliers and never-takers across matching events.

Figure 4 shows the estimated earnings distributions of compliers one, six, and twelve months after the matching event.[17] Comparing both types, we indeed find differences for the lower tail of the distributions up to around 2000 euros. One month after the treatment, the graph depicts a higher share of workers with low earnings among treated compliers. About 90 percent earn 2000 euros or less compared to 85 percent of non-treated compliers. After six months, the lower tails of both distributions look almost identical. This changes again in the subsequent months. One year after the matching event, we estimate a somewhat higher share of non-treated compliers with earnings below 2000 euros. These patterns suggest that, in the short run, treated job searchers are more likely to work in low-paid jobs. However, comparable to the employment estimates, the effect does not persist.

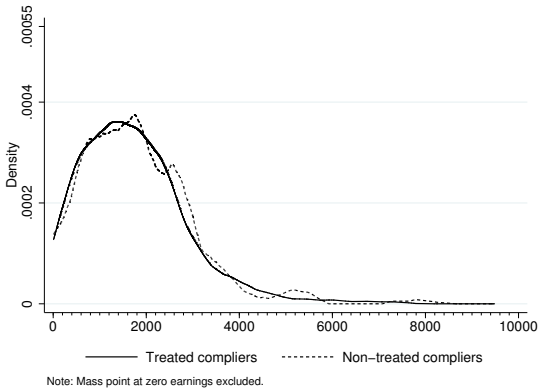
[16] The distribution of non-treated compliers is given by $g_0 c = \frac{1}{\phi_c} f_{00} - \frac{1-\phi_c}{\phi_c} f_{10}$, where ϕ_c denotes the share of compliers, f_{00} the distribution of individuals in the control group and f_{10} the distribution of non-compliers in the treatment group.

[17] Mass points for zero earnings are excluded.

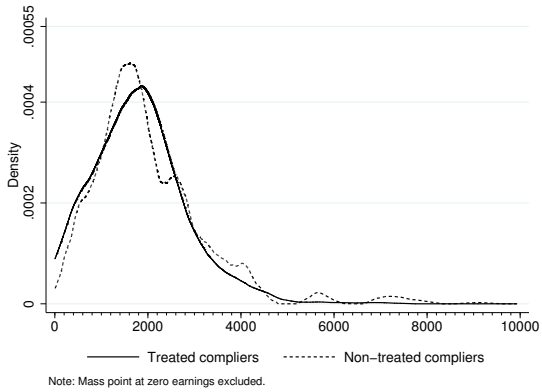
Figure 4: Earnings distributions of compliers (Epanechnikov kernel)



(a) Earnings (in euros) after one month



(b) Earnings (in euros) after six months



(c) Earnings (in euros) after twelve months

6.4 Matching event performance

The analysis of heterogenous effects has shown that not all job seekers benefit from matching events to the same extent. To examine matching event performance more closely, we provide statistics on the number and duration of individual meetings and show how these differ by observed characteristics of compliers. The data are taken from the online survey collected two weeks after each matching event. As shown in Table 12, attendees meet, on average, three to four private employment agencies at an event, and each talk lasts about seven minutes. Both number and duration vary considerably between job searchers. About 60 percent of participants report to remain in contact with at least one agency after the

matching event.

To examine differences in performance, we regress these outcomes on observed characteristics of participants. Results are reported in Table 12. Whereas the number of spoken employment agencies cannot be explained by included variables, the duration of job talks varies by gender, age and education. Older, female and university educated job searchers have, on average, significantly shorter meetings during the events. As shown in the last column, patterns are somewhat different when we examine further contact with agencies after the matching event. Here, older workers and those with a medium level of education are more successful.

These differences concur to some extent with our findings from the heterogeneous effects analysis. For low and medium educated, we find higher employment effects, which are reflected in their performance during the matching events.

Table 12: Performance differences by individual characteristics

	# agencies spoken	Average duration talk	Further contact with agency
Female	-0.131	-0.737*	-0.065
	(0.227)	(0.381)	(0.041)
Age	-0.015	-0.038*	0.004*
	(0.012)	(0.021)	(0.002)
Higher secondary education	-0.136	-0.052	0.104**
	(0.246)	(0.409)	(0.043)
College/university education	-0.268	-1.847***	0.020
	(0.396)	(0.701)	(0.099)
Constant	4.383***	9.421***	0.416***
	(0.655)	(1.097)	(0.102)
Population mean	3.54	7.26	0.60
and standard deviation	(2.29)	(3.97)	(0.49)
Observations	669	672	700

NOTE – Observations are weighted by inverse probability weights to account for selective response. * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

7 Discussion

7.1 Displacement effects

A major concern in the evaluation of active labor-market policies are displacement effects. Matching events might be effective because employers can fill vacancies which would otherwise remain unoccupied. However, it is also possible that participants are more likely to be considered for vacancies which would also be filled without the program, thereby decreasing the job-finding rate of benefit recipients in the control group.

To examine potential displacement effects, we compare job-finding rates in the control groups of matching events with different treatment assignment probabilities. If displacement effects exist, job seekers should do, on average, worse when the size of the treatment group increases. Because the local UI offices choose the share of benefit recipients in the treatment group, we have to assume that the chosen size does not correlate with unobserved characteristics of the location or the job searchers. Figure A.2 in the appendix shows the share of individuals who work six months after the matching events in control and treatment group sorted by treatment assignment probability. Comparing the eight events so far, the graph does not suggest that a higher share of treated individuals drives down the job-finding rate in the control group.

To shed more light on displacement effects, we extend our sample by additional job seekers. For each regional UI office in our sample, we use administrative data on benefit recipients during any of the months on which a matching event took place at another UI office. Adding these individuals to the control groups in our sample, the resulting panel includes benefit recipients at six offices in six different months. This allows us to compare the control groups to individuals at other locations where no matching event took place at a given date. Furthermore, we can compare the control group to benefit recipients at the same location but at different points in time. Exploiting this panel structure, we estimate displacement effects in a difference-in-difference framework. The corresponding regression equation is given by

$$Y_{ilt} = \kappa_l + \lambda_t + \pi D_i + v_{ilt}$$

where Y_{ilt} denotes the labor market outcome, κ_l are location fixed effects and λ_t are month fixed effects. D_i is an indicator that equals one if there was a matching event at location l and month t. Finally, v_{ilt} represents the error term. Parameter π then denotes the difference-in-difference estimator. To interpret the coefficient as causal effect, we have to assume that labor market outcomes follow a common trend over time at all locations. Furthermore, we need to assume that job seekers only search at there own location and thus do not affect labor market outcomes at other locations.

When comparing job searchers in the experiment to other benefit recipients, we have to account for the fact that the entire population of recipients might differ from the subset that was selected by the UI offices as eligible for a matching event. As shown in the appendix (Table A.6), individuals indeed differ in terms of observable characteristics between both samples.[18] Benefit recipients eligible for matching events are more likely to be male, are somewhat younger and higher educated, and have longer unemployment spells.

Using probit regressions, we estimate the individual propensity of being eligible. Next to the characteristics shown in Table A.6, we include occupational sector indicators as explaining variables. Figure A.3 in the appendix shows densities of the estimated propensity scores separately for the control group of the experiment and the added sample of benefit recipients. Both densities have a large area of common support, indicating a sufficient match in observable characteristics of job searchers in the two samples. Based on the propensity scores, we construct inverse probability of treatment weights (IPTW) for the displacement effect regression. Given that some matching events took place at the same location within three months, we focus on the short run impact to avoid confounding effects. Using the working indicator after one month as outcome, we find a small difference-in-difference coefficient of -0.009, which is not significantly different from zero ($SE = 0.019$). This suggests that the program did not affect the job-finding rate of benefit recipients who were not invited to matching events.

[18] Data on the full population come from a different source and do not contain all characteristics that we observe in our experimental sample.

7.2 Cost-benefit analysis

To evaluate the cost effectiveness of matching events, we calculate the cumulative gains of the treatment and contrast them with the costs of organizing a matching event.

The costs consist of the time investment made by caseworkers to set up a matching event, invite job searchers and employment agencies, and host the event. Matching events take, in most cases, place at the local offices of the Dutch UI administration. The hourly personnel costs of a UI caseworker amount to 55 euros. Participating employment agencies do not receive any compensation. Based on survey data from the local UI offices, we are able to calculate the average time investment per invited job searcher. Depending on the size of the matching event, this number ranges from 0.5 to 10 minutes. The average time investment is four minutes, which translates into costs of about four euros per job searcher assigned to the treatment group.

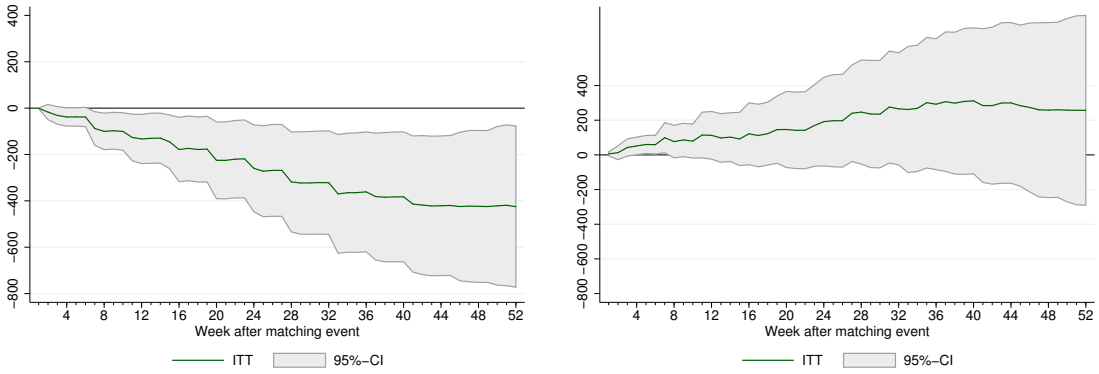
Gains are measured by means of changes in cumulative UI benefits and earnings up to one year after the intervention, for which data are currently available. In Figure 5, we plot ITT estimates for the cumulative outcomes by week after matching event. Both variables show a clear trend over time as the effect on the sum of paid benefits steadily decreases and the sum of earnings increases. After 12 months, individuals in the treatment group have collected, on average, 425 euros less in benefits and earned about 255 euros more compared to those in the control group. Yet, the difference in cumulative earnings is not statistically different from zero due to comparatively large standard errors.

Comparing benefits and costs up to one year later, matching events appear to be a cost-effective policy instrument for the UI administration. When we only focus on the reduction in benefit payments and assume a discount rate of 10 percent, the net present value amounts to 405 euros per invited benefit recipient.[19] Considering additionally the net change in cumulative income of job seekers, the net present value decreases to 245 euros because the decline in cumulative benefits

[19]The net present value is defined as $\sum_{n=1}^{52}(1+\delta)^{-w}ITT_w - C$, where δ denotes the discount factor, C the costs per invited benefit recipient, and ITT_w the intention-to-treat effect on UI benefits in week w after the matching event.

is larger than the rise in cumulative earnings. This estimate is, however, not statistically significant.

Figure 5: ITT estimates on cumulative outcomes by week after matching event



(a) Cumulative UI benefits

(b) Cumulative earnings

8 Conclusion

In this paper, we provide empirical evidence for the effectiveness of organized job talks between unemployed workers and privates employment agencies. Given their low costs and moderate organizational effort, matching events are a valuable instrument in the area of active labor market policies.

To obtain causal estimates on their impact, we conduct a randomized controlled trial in cooperation with several local offices of the UI administration in the Netherlands. Our empirical results suggest that matching events help to raise the job-finding rate. Using administrative data on unemployment and job characteristics up to one year after the matching event, we find that that job searchers are about 20 percent more likely to work in the short run. The increase in employment can be almost entirely explained by additional work via private employment agencies. Working under regular contracts is not significantly affected, which indicates that matching events neither crowd out regular employment nor help individuals to get a regular contract. Employment effect diminishes again 9-12 months after the matching event, suggesting that most of the temporary contracts are not extended.

Further analysis shows that treatment effects expose considerable heterogeneity with respect to type of matching event and individual characteristics of job searchers. The positive impact on the job-finding rate seems to be mainly driven by matching events that specialize in a specific sector. Also, we find stronger estimates for the low and medium educated. Surprisingly, benefit recipients with these characteristics are less likely to attend a matching event. Given the large effects on the job-finding rate, the costs of organizing matching events are much lower compared to forgone benefit payments.

Using survey data that we collected shortly after the meetings, we further find that the program significantly affects job search behavior. Job seekers become more motivated and decrease their reservation wage. The latter effect suggests that talks with private employment agencies allow job seekers to update their beliefs about current prospects on the labor market. This can contribute to higher job-finding rates. Overall, our results reveal the presence of substantial search frictions in the labor market for which matching events with private employment agencies can serve as a remedy in the short run.

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Appendix

Table A.1: Overview matching events

#	Location	Date	Type	Size	Treatment	Show up
1	Doetinchem	4-Jul-14	General	188	51%	19%
2	Doetinchem	5-Sep-14	Technical	170	48%	17%
3	Leeuwarden	17-Sep-14	General	4132	76%	21%
4	Eindhoven	18-Sep-14	Technical, Transport, Logistics, Industry, Security, Construction, ICT	936	50%	24%
5	Leeuwarden	12-Nov-14	General	2942	82%	29%
6	Venlo	22-Jan-15	General	314	80%	38%
7	Zwolle	4-Feb-15	General	350	80%	13%
8	Groningen	19-Mar-15	Commercial services	478	80%	19%
9	*Tiel*	*11-Jun-15*	*General*	*297*	*80%*	*11%*
10	*Veghel*	*10-Jun-15*	*General*	*691*	*75%*	*23%*
11	*Steenwijk*	*28-Aug-15*	*General*	*420*	*70%*	*16%*
12	*Groningen*	*17-Sep-15*	*Technical, Engineering, Construction*	*456*	*80%*	*14%*
13	*Venray*	*5-Nov-15*	*General*	*179*	*80%*	*40%*
14	*Venray*	*14-Jan-16*	*General*	*185*	*80%*	*44%*
15	*Venlo*	*21-Jan-16*	*General*	*436*	*80%*	*38%*
16	*Groningen*	*18-Feb-16*	*Technical, Engineering, Construction*	*908*	*80%*	*15%*
17	*'s Hertogenbosch*	*25-Feb-16*	*General*	*390*	*80%*	*33%*
18	*Venray*	*25-Feb-16*	*General*	*174*	*80%*	*41%*

NOTE – Matching events 9 to 18 (in *italic*) are not yet included in the empirical analysis because data are not available for the full period of observation.

Table A.2: Impact on working and collecting UI benefits

	Neither working nor benefits			Only benefits			Only working			Working and benefits		
	+1m	+6m	+12m	+1m	+6m	+12m	+1m	+6m	+12m	+1m	+6m	+12m
Intention-to-Treat Estimates (ITT)												
Invited	0.005	0.005	-0.001	-0.027**	-0.032***	-0.008	0.022**	0.028**	0.003	-0.000	-0.002	0.006
	(0.008)	(0.010)	(0.010)	(0.012)	(0.010)	(0.009)	(0.011)	(0.012)	(0.012)	(0.009)	(0.008)	(0.008)
Treatment-on-the-Treated Estimates (ATET)												
Attended	0.021	0.022	-0.005	-0.114**	-0.133***	-0.032	0.093**	0.120**	0.014	-0.000	-0.009	0.024
	(0.035)	(0.041)	(0.043)	(0.053)	(0.045)	(0.040)	(0.046)	(0.052)	(0.052)	(0.039)	(0.035)	(0.034)
Control group mean and standard deviation of outcomes												
Mean	0.12	0.16	0.19	0.47	0.26	0.17	0.25	0.46	0.52	0.16	0.12	0.11
Standard deviation	(0.32)	(0.37)	(0.39)	(0.50)	(0.44)	(0.38)	(0.43)	(0.50)	(0.50)	(0.37)	(0.32)	(0.31)

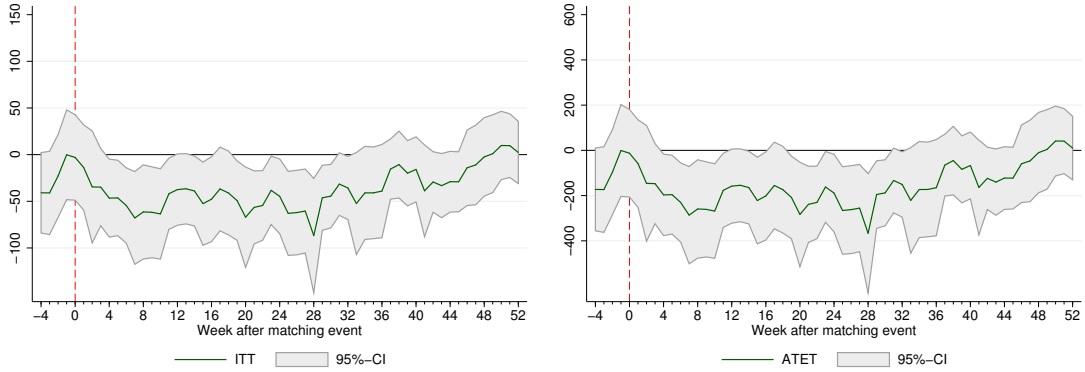
NOTE – $N = 8,361$. ITT estimates are obtained through OLS regressions. ATET estimates involve IV regressions using treatment assignment as an instrument for attendance. Outcomes are measured one ("+1m"), six ("+6m") and twelve ("+12m") months after the matching event. All regressions control for matching event fixed effects as well as a set of individual characteristics (gender, age, marital status, education) and previous job characteristics (earnings, benefits, permanent contract, working days) measured in the three months before the matching event; * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

Table A.3: Heterogeneous treatment effect regressions (ATET)

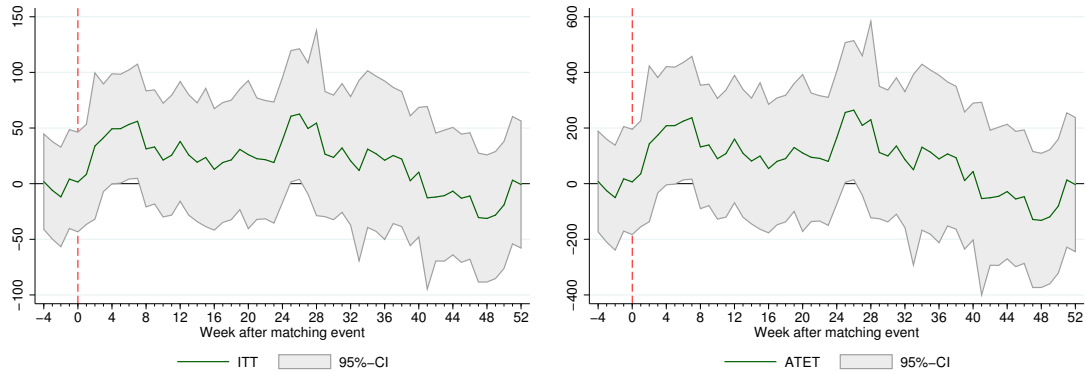
Outcome: Working	After 1 month	After 3 months	After 12 months
Show up	0.214	0.724	0.632
	(0.462)	(0.474)	(0.471)
× female	0.146	-0.133	-0.123
	(0.125)	(0.128)	(0.127)
× age	-0.004	-0.005	-0.002
	(0.005)	(0.005)	(0.005)
× level of education	-0.002	-0.037*	-0.042**
	(0.019)	(0.020)	(0.020)
× desired weekly hours	-0.001	-0.004	-0.006
	(0.008)	(0.008)	(0.008)
× unemployed three months ago	-0.003	-0.096	-0.080
	(0.126)	(0.129)	(0.129)
× specific matching event	0.123	0.270*	0.297**
	(0.140)	(0.144)	(0.143)
F-statistic	73.58	49.77	36.83

NOTE – $N = 8,361$. Estimates involve IV regressions using treatment assignment as an instrument for attendance. All characteristics above are included in the regressions along with their interaction terms. Furthermore, we control for matching event fixed effects, marital status and previous job characteristics (earnings, benefits, permanent contract, working days) measured in the three months before the matching event; * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

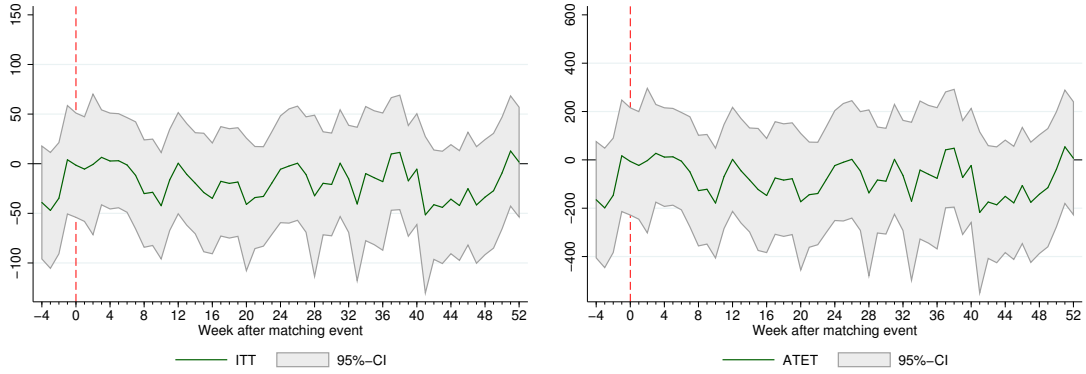
Figure A.1: ITT and ATET estimates by week after matching event



(a) Impact on monthly UI benefits (in euros)



(b) Impact on monthly earnings (in euros)



(c) Impact on the sum of monthly earnings and UI benefits (in euros)

Figure A.2: Share of benefits recipients by treatment share

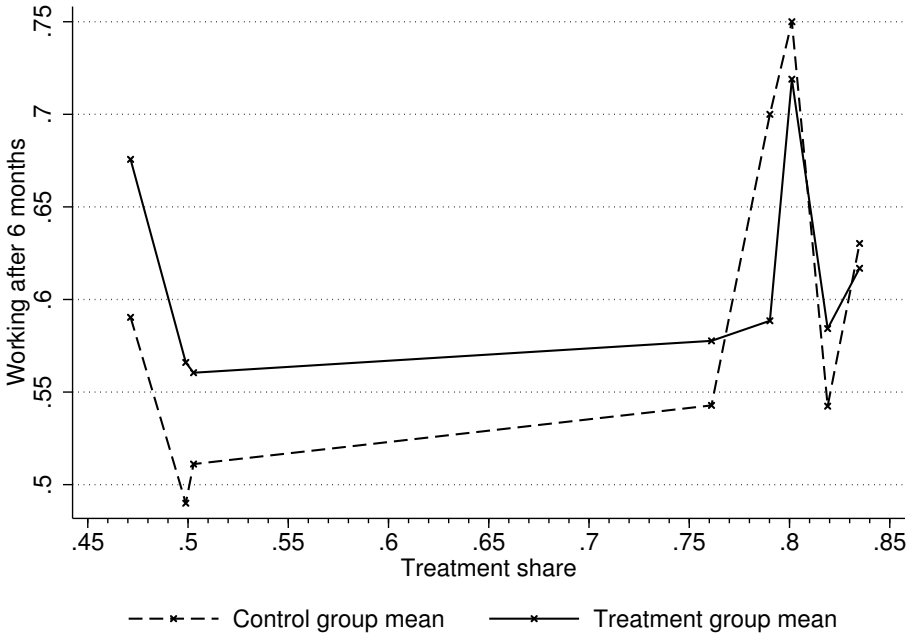


Figure A.3: Common support of propensity score estimates

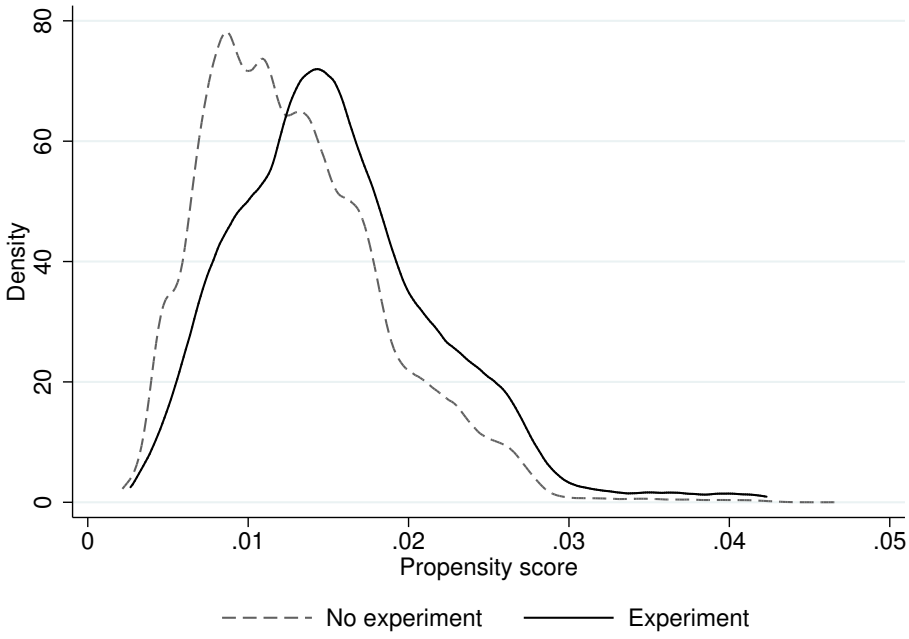


Table A.4: Comparison of survey respondents to full sample

	Full sample	Survey respondents	t-statistic
Female	0.39	0.37	1.33
	(0.49)	(0.48)	
Age	40.51	41.18	-2.26
	(11.87)	(11.76)	
Primary/lower secondary education	0.20	0.50	-24.29
	(0.40)	(0.50)	
Higher secondary education	0.59	0.45	10.71
	(0.49)	(0.50)	
College/university education	0.21	0.05	23.90
	(0.41)	(0.22)	
Observations	8,361	1,931	

NOTE – Column (1) and (2) report means, with standard deviations in parentheses. Column (3) shows t-statistics of difference in means tests.

Table A.5: Impact on job search behavior (unweighted survey data)

	# employment agencies registered	Job search motivation (1-5 scale)	# applications sent (last 4 weeks)	# job talk invitations (last 4 weeks)	Reservation wage (month, in euros)
Intention-to-Treat Estimates (ITT)					
Treatment	0.362**	0.069	-0.296	0.008	-95.55**
	(0.156)	(0.055)	(0.189)	(0.051)	(42.809)
*Treatment-on-the-Treated Estimates (*ATET*)*					
Attendance	0.929**	0.176	-0.760	0.021	-245.46**
	(0.395)	(0.142)	(0.491)	(0.131)	(110.718)
Control group mean and standard deviation of outcomes					
Mean	3.31	3.95	6.24	0.64	2289.47
SD	(2.91)	(1.04)	(3.65)	(1.02)	(1085.81)

NOTE – $N = 1,931$. ITT estimates are obtained through OLS regressions. ATET estimates involve IV regressions using treatment assignment as an instrument for attendance. All outcomes are measured 2-3 weeks after the matching event. If individuals already found work, all outcomes except for column (2) refer to the previous job search period. All regressions control for matching event fixed effects as well as a set of individual characteristics (gender, age, education); * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

Table A.6: Comparison of control group and entire population

	Population	Control group	p-value
Female	0.46	0.39	5.94
	0.50	0.49	
Age	41.61	40.42	4.59
	12.11	11.87	
Married	0.43	0.42	0.73
	0.50	0.49	
Level of education	5.77	6.02	-4.39
	2.86	2.61	
Working (one month before)	0.39	0.39	-0.18
	0.49	0.49	
Working (two months before)	0.51	0.43	7.15
	0.50	0.50	
Working (three months before)	0.62	0.52	9.63
	0.48	0.50	
Observations	162,101	2,124	

Note – Column (1) and (2) report means, with standard deviations in parentheses. Column (3) shows t-statistics of difference in means tests.

Extract online questionnaire (English translation)

Introduction

On behalf of the [Dutch] UI administration, VU University Amsterdam is conducting research on the effectiveness of services provided by the UI Administration and the chances of UI benefit recipients to find employment. For this purpose, we would like to ask you a few questions. Your answers are directly sent to researchers of VU University Amsterdam and will not be shared with the UI administration. The answers will be treated confidentially, will not be shared with third parties, and will not be used for other purposes than this specific research. It will not be possible to identify persons. We kindly ask you to fill in the complete questionnaire. For a successful evaluation, it is important to obtain a high response rate. Filling in the questionnaire takes just 10 minutes. If you have any questions about the questionnaire or the research, please contact the responsible researchers at VU University Amsterdam: [List of three researchers with contact details: name, email address, telephone number]. We thank you for your cooperation.

(1) Basic characteristics

- Gender, year of birth, highest completed level of education

(2) Current situation and last employment

- At how many private agencies are you currently registered? (Possible answers: 0, 1, 2, 3, 4-6, 7-10, more than 10)
- What do you think are your chances to find employment within three months? (Slider on a 0-100 [unlikely - very likely] percentage scale)

(3) Job search behavior (If already found work, asked retrospectively for last period of unemployment)

- How many application letters do/did you write in four weeks' time? (Possible answers: 0, 1-3, 4-7, 8-11, 12-15, 16-19, 20 or more)

- How often do/did you receive invitations for a job talk in four weeks' time? (Possible answers: 0, 1, 2, 3, 4, 5 or more)
- What is/was the minimal monthly pre-tax salary that you want/wanted to earn? (Fill in amount)
- How motivated are/were you to write applications? (Slider on a 1-5 [not motivated - very motivated] scale)

(4) Matching events (Questions only asked if applicable)

- Have you been invited to a matching event in the previous two months?
- With how many private employment agencies did you talk during the matching event. (Possible answers: 1-20)
- How long did a talk last on average? (Slider 0-30 minutes)
- Did you stay in contact with one or more private employment agencies after the matching event? (Possible answers: Yes, one ore more agencies contacted me; yes, I contacted one ore more agencies; no)