A Field Experiment on Labor Market Speeddates for Unemployed Workers*

Bas van der Klaauw**
VU University Amsterdam, Tinbergen Institute, CEPR, IZA

Lennart Ziegler***
University of Vienna, IZA

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Abstract

We conduct a field experiment to evaluate labor market speeddates where unemployed workers meet temporary employment agencies. Participation in such events increases immediate job finding by 6-7 percentage points. Afterwards employment effects diminish, suggesting that temporary employment has no long-lasting effect on employment prospects. While the intervention is cost effective for the UI administration, higher labor earnings of treated job seekers do not compensate for the decline in benefit payments. Survey evidence shows that speeddate participation increases job search motivation and reduces reservation wages. These findings concur with predictions of a model where job seekers update their labor market beliefs.

Keywords: Matching events, active labor market policies, randomized experiment, temporary work, job search behavior
JEL Classification Numbers: J64, J65, C21, C93

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**Van der Klaauw: VU University Amsterdam - Department of Economics, De Boelelaan 1105, 1081 HV Amsterdam, The Netherlands. Email: b.vander.klaauw@vu.nl.
***Ziegler: University of Vienna - Department of Economics, Oskar-Morgenstern-Platz 1, 1090 Vienna, Austria. Email: lennart.ziegler@univie.ac.at.

1 Introduction

Search frictions are an important source of inefficiency in the labor market, causing unemployment and limiting firms in the possibility to increase their labor input. To hire workers quickly, many firms rely on temporary employment agencies, which mediate workers to vacancies. According to a recent ILO report, the temporary employment sector has been expanding in many countries in the past decades (Countouris et al., 2016). It is often argued that also workers benefit from temporary work because it provides work experience and may improve future employment prospects. Therefore, benefit administrations often encourage unemployed workers who face difficulties in finding work to contact temporary employment agencies.

This paper focuses on a novel policy instrument which consists of matching events organized by the unemployment insurance (UI) administration. At these events, referred to as *speeddates*, unemployed workers meet a large number of temporary 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.

While the matching events have some similarities with job search assistance programs, there exist a few important differences. First, matching events allow unemployed workers to directly interact with many employers in a time-effective way while caseworkers are not involved. Second, participation takes at most a few hours. Therefore, it is unlikely that job seekers reduce search effort as a response to participating in matching events (lock-in effect), which is a concern of intensive activation programs (Sianesi, 2004; Rosholm, 2008).[1] Effects on job search behavior prior to the intervention, for example due to a threat effect, are also unlikely because attendance is voluntary and matching events are irregular and announced only 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 unemployed workers.

To evaluate the effectiveness of the program, we conduct a randomized experiment with more than 12,000 unemployed job seekers. Invitations to the matching events are

[1] 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 (Markussen and Røed, 2016; 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). See Kluve, 2010; Card et al., 2010, 2018 for comprehensive meta analyses of the effects of active labor market programs.

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, which allows us to interpret the instrumental variable estimates as average treatment effects on the treated (ATET). For the empirical 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 temporary employment agencies on their own initiative and would then receive the same support, matching events do not provide additional job opportunities to benefit recipients. A positive treatment effect would suggest that some unemployed workers are not aware of available vacancies. Furthermore, the experimental setting is informative on the impact of temporary work on subsequent labor market prospects. If temporary employment serves as a stepping stone, there should also be a positive treatment effect on employment in the long run. Finally, the experiment provides insights into changes in job search behavior during the search process. We analyze whether the meetings with temporary employment agencies affect expectations and search effort of benefit recipients.

Our study relates to several experimental evaluations of counseling schemes.[2] 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 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 seekers who are at risk of long-term unemployment. They find that public programs have a lower take-up rate but are more cost effective due to higher job finding rates among participants. 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

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

negative after two years.

Compared to intensive counseling, direct meetings with temporary 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 employment 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 often pay lower wages 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 contrary, 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.

We find that participation in matching events has a substantial impact on job finding in the short run. Participants are about 6-7 percentage points more likely to start working one month after the matching event. Estimated employment effects diminish in the following weeks. This shows that participation in matching events does not lead to an advantage in the long run. There exists substantial effect heterogeneity with respect to the type of matching event and characteristics of participants. In particular, the impact of matching events persists longer for job seekers with short unemployment spells. The positive employment effects are not entirely driven by additional work via temporary employment agencies. Treated participants also work somewhat more in regular employment, which suggests that the matching events not only result in immediate job offers by temporary employment agencies but also help job seekers to extent their network and improve their job search skills. 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. Despite a faster transition to work, treated participants do not experience significant income gains because their wages tend to be lower. One year after the event, differences in cumulative earnings between treatment and control group are close to zero. Due to the absence of stepping stone effects and unchanged income levels, the program does not benefit the average job searchers despite its positive impact on job finding in the short run.

Evidence from a survey which we conducted two weeks after the treatment shows that matching events have a positive effect on job search motivation and decrease reservation wages. Both mechanisms can explain higher job finding rates and are in line with our findings of increased regular employment and lower wages of participants. Overall, the results indicate that temporary work is not a stepping stone towards regular employment. To reconcile our findings, we provide a job search model in which matching-event participation temporarily reduces search frictions and provides information about the stock of vacancies in the labor market segment with temporary employment agencies. The latter allows job seekers to update their beliefs about their labor market opportunities. These findings are in line with experimental evidence on job search attitudes from a number of recent projects. Bandiera et al. (2020) show that offering job assistance can change the beliefs of workers about their job search prospects. Similarly, Abebe et al. (2021) find that meetings with firms at job fairs allow job seekers to correct their wrong beliefs about the labor market.

This 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 reconcile our findings using a job search model 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.[3] 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 need to have worked at least 26 out of the 36 weeks prior to unemployment. All eligible workers 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 a maximum entitlement period of 38 months.[4]

[3] An exception are self-employed workers.

[4] Since 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

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 capped at a maximum of about 4,400 euros per month before taxes.[5] If recipients are no longer eligible for UI benefits, they can apply for welfare benefits. These means-tested benefits correspond to 50 percent of the minimum wage and are paid for an unlimited period.[6]

UI benefit recipients have the obligation to make 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 programs.

2.2 Temporary employment agencies

Since February 2011, the UI administration organizes matching events between benefit recipients and temporary employment agencies, which are referred to as *speeddates*. These agencies form a considerable part of the Dutch labor market. In 2014, more than 6,000 agencies were 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 temporary employment agency.

Employment 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 temporary 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 temporary 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 and sick pay.

has been reduced to 24 months.

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

[6]For more details, see De Groot and Van der Klaauw (2019).

2.3 Matching events

The matching events take place at the local offices and can be either general or targeted towards a specific sector. They are organized by the department of the UI administration that maintains contact with employers. At the events, benefit recipients have the opportunity to talk to representatives of the temporary 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 individuals who became unemployed in the past three months. A few UI offices also consider individuals who are up to 12 months unemployed.[7] Two to three weeks before an event will take place, the UI administration selects eligible benefit recipients and sends out invitations either by regular mail, e-mail, or via an electronic account of the benefit recipient at the UI administration. Appendix C provides a translation of the invitation letter. Invited benefit recipients are not obliged to attend the matching event but participation counts as one job application, which all benefit recipients have to make every week.

Due to low costs and moderate organizational effort, matching events with temporary employment agencies have become an increasingly popular instrument of the UI administration in the Netherlands. On average, an organizer only invests about 13 hours of work in setting up and hosting a matching event. Although the temporary employment agencies are not compensated for their participation, previous survey evidence collected by the UI administration shows that most agencies consider matching events a useful tool. The events allow them to extend their database of job seekers even when the meetings do not result in an immediate hire.

It should be stressed that the temporary work agencies do not offer vacancies that are exclusively for participants of matching events. All individuals can register themselves as job seeker at the temporary work agencies.

2.4 The experiment

In 2014, the Dutch UI administration asked 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

[7] Table B.2 in the appendix provides a comparison of additional characteristics between the entire population of benefit recipients and those invited for matching events (labeled as control group).

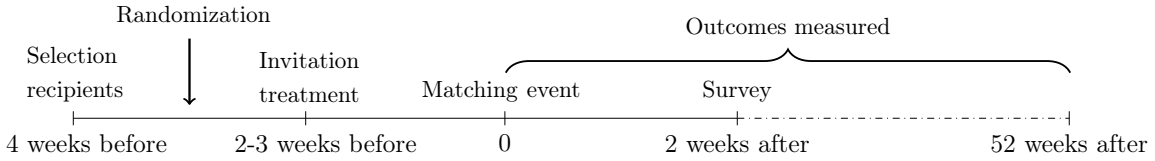


Figure 1: Timeline experiment

how many of them should be randomized in the treatment group.[8] We then randomly assigned the eligible workers to treatment and control group.

Figure 1 shows the timeline of the experiment. Treated individuals receive an invitation to a matching event about two to three weeks in advance, whereas individuals in the control group are not contacted. It is not known to participants that they take part in an experiment. The relative size of the treatment group is determined by the local offices and ranges from about 50 to 80 percent depending on the pool of potential invitees and the number of participating temporary employment agencies. During the matching event, the organizer compiles an attendance list, but this list is not recorded in the central database of the UI administration. Participants can register their attendance at the matching event as a job search activity. Otherwise, caseworkers of unemployed job seekers are normally not aware of the occurrence of matching events and they do not know which job seekers were invited and actually attended. Thus, the events neither affect the time constraint of caseworkers nor can cause substitution of their time between unemployed job seekers.

Two weeks after the matching event, we send out a short online questionnaire to individuals in both the treatment and control 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 do not complete the questionnaire receive email reminders one and two weeks later. The survey includes questions about job search behavior and, for individuals in the treatment group, questions about their experience with the matching event. Furthermore, participating temporary 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 locations between July 2014 and February 2016, of which five events are sector specific. The number of

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

participants varies considerably, ranging from small matching events with 15 participants to big events with more than 700 participants. At the events, participants could talk to between four and 11 temporary employment agencies. About 12,600 individuals take part in the experiment, of which 76 percent are assigned to the treatment group. The attendance 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 attendance rate is provided in the appendix (Table A.1).

3 Data

For the empirical analysis, we use both administrative data and survey data complemented with attendance lists of the matching events that are provided by the UI administration. Approximately 10 percent of individuals were twice entitled to participate. In the analysis, we measure treatment effects starting from the first time an individual became entitled to participate in a matching event.[9]

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. These variables are the outcomes of interest and they 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 limited set of individual characteristics.

Table 1 shows descriptive statistics on characteristics of job seekers 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 assignment probability. Treatment and control group have a very similar composition of job seekers. The final column of Table 1 shows no statistically significant differences (at the 10-percent level) in characteristics measured before the matching events.

[9]For repeatedly assigned job seekers, the treatment effect in the long run should be interpreted as a combined effect of the first matching event and another potential participation.

Table 1: Descriptive statistics and balancing

	Control group	Treatment group	p-value
Female	0.36	0.35	0.56
	(0.48)	(0.48)	
Age	40.96	41.21	0.32
	(11.91)	(11.97)	
Married	0.43	0.43	0.81
	(0.50)	(0.49)	
Primary/lower secondary education	0.25	0.25	0.53
	(0.43)	(0.43)	
Higher secondary education	0.56	0.54	0.20
	(0.50)	(0.50)	
College/university education	0.19	0.20	0.36
	(0.40)	(0.40)	
Benefits (prev. 3 months)	1663.00	1623.74	0.34
	(1986.35)	(1982.75)	
Earnings (prev. 3 months)	3945.39	3880.03	0.42
	(3926.64)	(3839.04)	
Workdays (prev. 3 months)	30.53	30.25	0.53
	(21.32)	(21.45)	
Perm. contract (prev. 3 months)	0.23	0.22	0.18
	(0.42)	(0.41)	
Temp agency work (prev. 3 months)	0.36	0.37	0.41
	(0.48)	(0.48)	
Observations	3,054	9,556	

Note – All estimates are weighted by inverse treatment assignment probabilities. Columns (1) and (2) report means, with standard deviations in parentheses. Column (3) shows p-values of two-sided difference-in-means tests.

Around 35 percent of benefit recipients are female, and they are, on average, 41 years old. 43 percent are married and the majority completed higher secondary education as highest schooling level. One fifth obtained a college or university degree. In the three months prior to a matching event, individuals collected around 1,600 euros in UI benefits and earned 3,900 euros from work. On average, they worked about 30 days in these three months.[10] Approximately 22 percent of individuals had a permanent

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

contract in that period and about one third already worked at temporary employment agencies before becoming unemployed. The variables reported in Table 1 will serve as control variables in the analysis of treatment effects.

The upper panel of Table 2 provides descriptive statistics on matching event attendance and outcome variables four weeks after the intervention. Again, summary statistics are weighted to account for varying treatment shares among matching events.[11] Whereas the attendance rate in the treatment group is 24 percent, nobody in the control group attended the matching events. For most outcome variables, we observe significant differences in means between treatment and control group four weeks after the matching events. In the treatment group, fewer individuals collect benefits, and they are more likely to start a new job. Benefit payments are about four percent lower while earnings are four percent higher compared to the control group. Individuals in the treatment group have somewhat more working days and earn more but the means do not differ significantly.

3.2 Survey data

The administrative records are complemented with data from an online questionnaire that we sent out two weeks after the matching event. 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 and non-respondents, we find that individuals with higher levels of schooling are significantly more likely to answer the questionnaire (see Table A.2 in the appendix). To account for selective non-response, we apply inverse probability weighting based on gender, age and education in the analysis of survey outcomes.[13]

[11] Differences between the weighted outcome means are thus comparable to intention-to-treat (ITT) estimates obtained from regressions that only include matching event indicators as controls.

[12] Overall, about 30 percent of benefit recipients started the questionnaire but some did not provide answers to all questions.

[13] As an additional balancing check, we compare treatment and control group means of characteristics observed in the administrative records for the sample of survey respondents (see Table A.3 in the appendix). While the differences are somewhat larger compared to those reported for the whole sample, none of the estimates differ significantly at the 10-percent level.

Table 2: Descriptives statistics of outcomes

	Control group	Treatment group	p-value	Observations
Attendance	0.00	0.24	0.00	12,610
Administrative outcomes (4 weeks after matching event)				
Collecting benefits	0.71	0.69	0.06	12,610
	(0.46)	(0.46)		
Amount monthly benefits	922.43	883.76	0.04	12,610
	(902.20)	(897.46)		
New job after matching event	0.10	0.11	0.03	12,610
	(0.30)	(0.31)		
Working days	7.83	8.12	0.13	12,610
	(9.40)	(9.89)		
Amount monthly earnings	785.38	814.03	0.21	12,610
	(1090.76)	(1108.62)		
Survey response	0.23	0.23	0.70	12,610
Survey outcomes (2-3 weeks after matching event)				
# employment agencies registered	3.51	3.78	0.04	2,888
	(3.03)	(2.97)		
Job search motivation (1-5 scale)	3.91	4.00	0.05	2,888
	(1.08)	(1.02)		
# applications sent (last 4 weeks)	6.42	6.16	0.13	2,888
	(4.06)	(3.66)		
# job talk invitations (last 4 weeks)	0.80	0.80	0.99	2,888
	(1.19)	(1.13)		
Reservation wage (month, in euros)	2281.59	2154.14	0.01	2,888
	(1063.97)	(952.43)		

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 (1) and (2) report means, with standard deviations in parentheses. Column (3) shows p-values of two-sided difference-in-means tests.

The lower panel of Table 2 provides summary statistics of outcomes reported in the survey. These outcomes include a subjective measure of job search motivation, the number of recent job applications and the number of recent job talk invitations,

which we use to proxy search performance. Furthermore, survey respondents report the minimum monthly wage for which they are willing to accept work (reservation wage) and the number of temporary 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 makes about six applications but receives less than one invitation to a job interview per month. The respondents are registered at three to four temporary employment agencies and report a reservation wage of about 2,300 euros per month. A first comparison of means in treatment and control group shows significant differences in three survey outcomes. Job searchers in the treatment group are registered at more temporary 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

$$Y_{is} = \mu_s + \delta T_i + X_i^{'}\beta + U_{is}$$

where Y_{is} denotes the outcome of individual i, who is either in the treatment or the control group of matching event s. T_i indicates whether the individual attended the matching event. X_i is a vector that contains the individual characteristics described in Table 1. Matching event fixed effects, denoted by μ_s, account for the different treatment assignment probabilities of matching events. Whereas X_i is only included to increase the precision of estimates, we have to include μ_s to avoid biased estimates because size, composition, and assignment to the 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}, the coefficient δ does not identify the average impact of matching events. We focus on two alternative estimates to capture program effects. By regressing the outcome variable on treatment group assignment (Z_i) instead of matching event 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 (*never takers*) and the treatment effect for actual participants (*compliers*). The ITT is informative from a policy perspective because the UI administration invites benefit recipients to the events but cannot enforce participation. The estimate can also be used to evaluate the cost-effectiveness of matching events.

In addition 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 instrumental variable. 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 individuals. For most of the analysis, we report ATET estimates in the main text and ITT estimates in the appendix.

4.1 First-stage results and identification

Table 3 shows estimation results for the effect of assignment on actual participation in matching events, which is the first-stage regression of the instrumental variable approach. The coefficient on assignment corresponds to the attendance rate of 24 percent reported in Table 2. As shown in the second column, including individual characteristics as controls does not affect the attendance rate.

The last two columns of Table 3 test for differences in attendance rates by matching event size and matching event type. The estimates show that attendance rates are, on average, six percentage points lower at sector-specific matching events. Interacting treatment assignment with the number of job seekers at a matching event does not indicate significant differences by event size.

Randomization takes place at the moment of invitation, which is approximately two to three 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 (Black et al., 2003). Even though participation is not compulsory for invited benefit recipients

Table 3: First-stage estimates

Attendance	(1)	(2)	(3)	(4)
Treatment assignment	0.236***	0.235***	0.249***	0.236***
	(0.008)	(0.008)	(0.009)	(0.012)
Sector-specific × treatment			-0.059***	
			(0.018)	
Size ($\times 10^{-3}$) × treatment				-0.001
				(0.005)
Control group mean	0.000	0.000	0.000	0.000
Characteristics	No	Yes	Yes	Yes
F-statistic (on excl. instruments)	900.32	927.73	469.68	463.85

NOTE – $N = 12,610$. All regressions include matching event fixed effects. Standard errors are in parentheses; * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

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 temporary employment agencies. If these effects occur, the instrumental variable approach is invalid 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 regress outcomes measured in the period between invitation and matching event on treatment assignment. The difference between treatment and control group in the share of individuals who collect UI benefits is small ($\hat{\delta} = -0.004$) and not significant (p-value $= 0.67$). Similar results can be observed for monthly working days ($\hat{\delta} = -0.106$; p-value $= 0.53$).

Another threat for the treatment effects analysis are displacement effects. Participants of matching events may fill vacancies that would otherwise be filled outside the matching event by non-participating job seekers. In Appendix B, we examine potential displacement effects by comparing differences in treatment assignment probabilities and by considering workers in other local labor markets and time periods. Our findings do not provide evidence for the presence of displacement effects.

5 Results

This section analyzes the impact of matching events on various labor market outcomes. First, we provide graphical evidence and report estimates for average treatment effects. Afterwards, we consider heterogeneity in treatment effects.

5.1 Average treatment effects

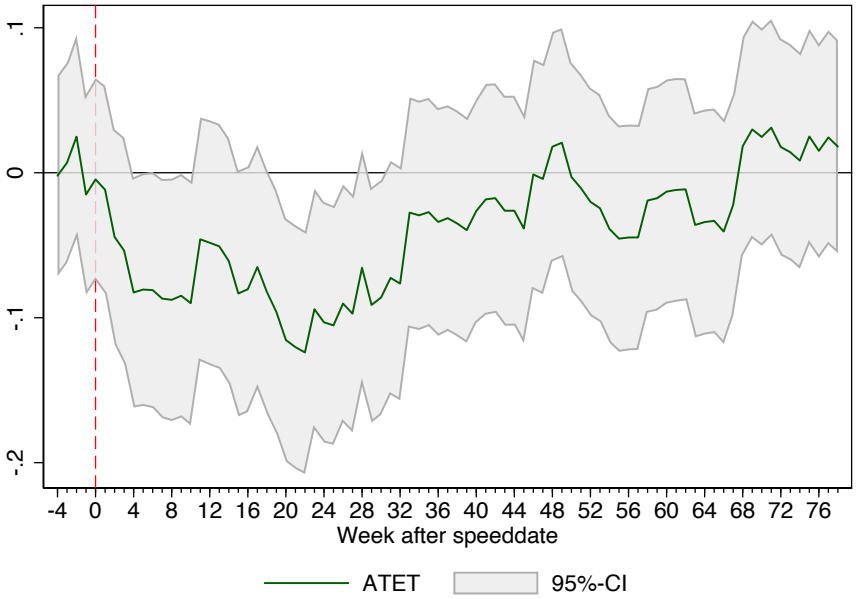
Figure 2 plots ATET estimates for the impact on benefits receipt and monthly working days by week after the matching event. The graphs illustrate a strong immediate effect of matching events. As shown in the upper graph, job seekers are significantly less likely to collect UI benefits shortly after participation. In the first four weeks after a matching event, the effect size increases steadily. In the subsequent 20 weeks, the ATET on collecting benefits remains constant at approximately 10 percentage points. Afterwards, the impact diminishes again, and 32 weeks after the matching event, differences are close to zero.

The lower graph of Figure 2 shows the impact on monthly working days in each week. Again, we observe a sizeable impact in the short run, which grows to almost two working days after four weeks. In the following weeks, the effect size diminishes again. Two months after the matching event, the ATET fades to be statistically insignificant and remains at a low level. Similar trends can be observed for monthly benefits, monthly earnings and the probability to start a new job. The corresponding graphs can be found in the appendix (Figures A.1a and A.1b).

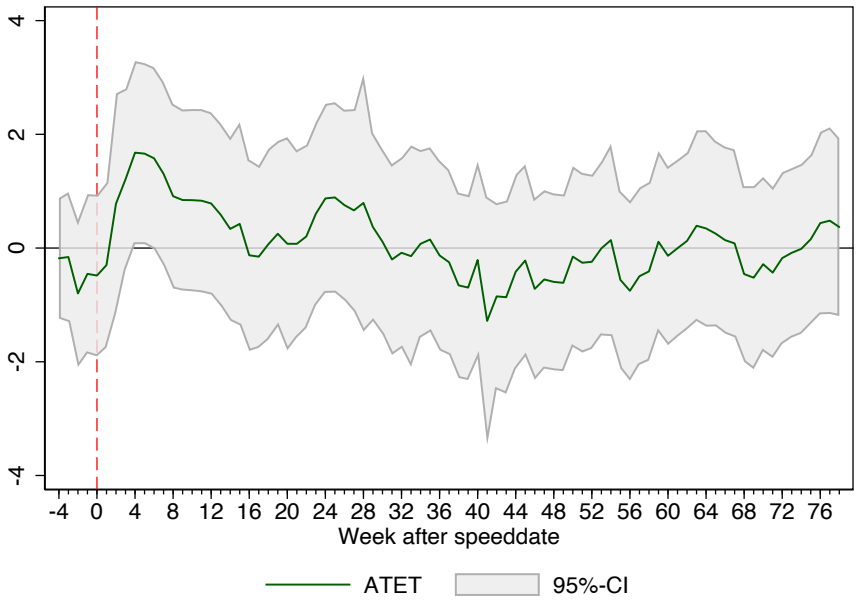
Table 4 reports ATET estimates as well as control group means of compliers for our main labor market outcomes. All outcomes are measured one, six and 12 months after the matching event to capture both short-term and long-term effects.[14] The upper left columns show that, in the first months after participating in an matching event, UI benefits receipt declines substantially by around eight to nine percentage points. This impact disappears in the longer run and, after 12 months, the estimate is small and insignificant. A similar pattern can be observed for the amount of monthly UI benefits. After one and six months, treated job seekers receive, on average, 150 to 170 euros less in benefits.

[14] Table A.4 in the appendix reports the corresponding ITT estimates which are equivalent to the ATET estimates multiplied by the attendance share.

Figure 2: ATET estimates by week after matching event



(a) Impact on collecting UI benefits



(b) Impact on monthly working days

This is a non-ignorable effect given that average benefit payments of compliers in the control group are about 1,500 euros and 500 euros in the first and sixth month, respectively.[15] Again, estimated differences vanish in the following months.

Matching events intensify contacts between unemployed workers and temporary employment agencies. A likely channel to explain the fast exit from UI benefits is that job seekers are more likely to find vacancies mediated by participating agencies at the matching event. ATET estimates on the probability to find new work show both increased job finding in the temporary employment sector and of regular jobs. As shown in Table 4, only about half of the increase in job finding can be explained by temporary employment. These results indicate that the matching events do not crowd out search for regular jobs.

Table 4: ATET estimates for main outcomes

	Collecting benefits			**Monthly benefits**			**New work - Temp. agency**		
	+1m	+6m	+12m	+1m	+6m	+12m	+1m	+6m	+12m
ATET	-0.083**	-0.090**	-0.020	-154.17**	-168.69**	2.84	0.037**	0.044	0.016
	(0.041)	(0.042)	(0.040)	(74.34)	(74.80)	(61.25)	(0.017)	(0.032)	(0.037)
Complier control group mean and standard deviation of outcomes									
Mean	0.992	0.633	0.441	1511.67	1011.98	551.87	0.022	0.147	0.241
SD	(0.340)	(0.543)	(0.511)	(1029.21)	(1199.48)	(840.29)	(0.146)	(0.354)	(0.428)
	New work - Regular			**Monthly working days**			**Monthly earnings**		
	+1m	+6m	+12m	+1m	+6m	+12m	+1m	+6m	+12m
ATET	0.043*	0.013	0.045	1.678**	0.755	-0.241	164.21*	112.99	-93.26
	(0.024)	(0.042)	(0.044)	(0.822)	(0.856)	(0.781)	(93.87)	(107.67)	(110.12)
Complier control group mean and standard deviation of outcomes									
Mean	0.031	0.384	0.522	2.040	9.110	9.043	168.210	934.30	1244.60
SD	(0.179)	(0.489)	(0.501)	(6.435)	(9.957)	(9.262)	(808.84)	(1162.63)	(1215.82)

Note – $N = 12,610$. ATET estimates are obtained through 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. Following Imbens and Rubin (1997), we compute control group means and standard deviations of outcomes for the compliers in our experiment.

[15] The decrease does not need to be solely driven by a lower number 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. There is also no one-to-one relation between leaving the benefits system and finding work.

Instead, participation in such events seems to activate job seekers to also search more for regular jobs. Network building and improved job search skills due to CV checks and feedback provided during the matching events may be relevant mechanisms. In the longer run, there remains some increase in regular work but this effect is not significant. Thus, we find at most weak evidence that temporary employment can be a stepping stone into regular employment, which concurs with the findings of De Graaf-Zijl et al. (2011) for the Netherlands.[16]

Because the job finding rate increases, we also expect positive effects on working days and earnings. Indeed, we observe a significant increase of about 1.7 working days per month after four weeks, which is quite substantial in comparison to the initially low control group mean of compliers. The effect can be almost fully attributed to regular employment (see Table A.5 in the appendix). This is in line with the notion that working hours in the temporary employment sector are more uncertain than in regular jobs. The positive effect on working days declines shortly after the matching event. As mentioned above, unemployed workers remain entitled to UI benefits when they find a new job with low earnings or few working hours compared to previous employment. Thus, leaving the UI benefits system and finding new work does not always concur. To examine how both interact, we estimate treatment effects on working days separately by UI benefit receipt. Table A.6 in the appendix shows that the impact is clearly driven by those who stop collecting benefits. The effect on working days during benefits receipt is three times smaller and not statistically significant. This suggests that working days of jobs mediated at matching events are often sufficient to end the UI benefits spell of unemployed workers.

Earnings increase by 164 euros after one month, which is very similar to the estimated reduction in UI benefits. After six months, the effects are smaller and insignificant, and after one year, the estimated impact on earnings remains insignificant and even turns negative. This concurs with the findings of Cottier et al. (2015), who neither estimate positive long-run employment or earnings effects. Comparing income gains and losses, we find that the sum of earnings and UI benefits is never significantly different from zero. To better understand income changes, we examine cumulative differences in earnings and benefit payments as part of a cost-benefit analysis in Section

[16] We also estimated whether individuals change sectors more often after having participated in a matching event. Our results show that participants are not more likely to take a job in a sector other than the sector of their last job before becoming unemployed (ITT $= -0.004$, $s.e. = 0.014$). In Table A.8 in the appendix, we show that, one year after the matching event, the distribution of jobs over sectors is the same in treatment and control group.

7.2.[17]

The estimated treatment effects show an immediate positive effect on job finding, which can be attributed to both temporary jobs and regular jobs. The latter effect indicates that network building and improved job search skills may be relevant mechanisms. In Section 6, we provide additional evidence on changes in job search behavior. The absence of long-run effects, however, suggests that temporary jobs mediated at the matching events do not serve as stepping stones to regular or more permanent employment.

5.2 Heterogenous treatment effects

Effects on job finding may differ between matching events and by characteristics of participants. To examine these differences, we next consider heterogeneous treatment effects. Our analysis focuses on the number of working days per month because it is the most precise measure of employment. Heterogeneous effects estimated for other outcome variables concur with our findings for working days.

Table 5: Heterogeneous effects - Monthly working days (ATET estimates)

	Type of event		**Unemployed**		**Prev. temp work**	
	General	Sector	< 3 mo.	≥ 3 mo.	No	Yes
After 1 month	1.913**	0.597	2.804***	-1.284	1.088	3.983*
	(0.913)	(1.810)	(0.992)	(1.384)	(0.844)	(2.325)
After 6 months	0.520	1.788	2.347**	-3.148*	0.427	2.474
	(0.935)	(2.054)	(1.005)	(1.629)	(0.900)	(2.366)
After 12 months	-0.559	0.886	0.682	-2.426*	-0.339	-0.112
	(0.862)	(1.810)	(0.921)	(1.465)	(0.836)	(2.027)
Observations	9,877	2,733	8,804	3,806	8,531	4,079

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; previous temporary employment agency spells are observed in the six months prior to the speeddate; * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

[17]Given that wages are only observed for individuals who have found work, the causal impact on re-employment wages cannot be estimated. Applying the trimming procedure developed by Lee (2009), we instead calculate upper and lower bounds for wage effects. Unfortunately, due to large standard errors of our ATET estimates, the obtained bounds are very wide and cannot be interpreted conclusively.

The two main characteristics of matching events that we consider are the number of invited job seekers and whether an event is general or sector specific. As reported in Section 4.1, attendance rates are unaffected by the size of an event but invited job seekers are more likely to attend events targeted at a general audience. Estimation results in Table 5 show that general matching events have a significant short-run impact which disappears in the medium and long run. Sector-specific matching events have more substantial effects in the medium run, but due to large standard errors, the estimates are never significant. Effect heterogeneity by size of matching event tends to be smaller and is often driven by sector-specific events with few invitees (see Table A.7 in the appendix).

Earlier studies show that the effectiveness of active labor market policies often differs by observable characteristics of individuals (see Card et al., 2018). We find that the labor market history of participants is most relevant for the success of matching events. Table 5 shows that only job seekers with short unemployment spells work more days after attending a matching event. At the start of the unemployment spell, individuals might often be too optimistic and have not yet received many signals about their prospects on the labor market. Talking to several potential employers and obtaining feedback at matching events should thus be more informative for those with short unemployment spells. For job searchers who have already been unemployed for more than three months at the time of the matching event, we find a negative effect, which becomes more pronounced in later months. While these coefficients are estimated with large standard errors, they suggest that short-lived temporary employment can crowd out more stable work spells and even reduce working days for this subgroup. Furthermore, we find that treatment effects are larger for participants who already worked previously at a temporary employment agency. These workers may be better suited for available vacancies. Having already gained experience with such jobs, they might also be more likely to accept job offers from agencies.

Table A.7 in the appendix summarizes treatment effects stratified by a number of additional individual characteristics. While the estimated impact is somewhat stronger for men and for job seekers below age 40, we do not find any clear pattern of heterogeneous effects by education and remaining entitlement duration of UI benefits. Taken together, these differences suggest that the effectiveness of matching events could be considerably improved by targeting specific subgroups of benefit recipients.

6 Mechanisms

6.1 Who are the compliers?

Because attendance is not enforced, only about 24 percent of invited unemployed workers attend a matching event. In our online survey, individuals who were invited but did not attend a matching event are asked to report the reason for their absence. Most respondents state that they either did not notice the invitation (36 percent) or did not have time when the matching event took place (25 percent).[18] About 17 percent already found work by the time of the event. Another 11 percent were absent because they did not expect to find work at matching events or did not want to work via temporary employment agencies. The remaining non-compliers report to be absent for other reasons such as illness or childcare.

Since temporary employment agencies usually offer vacancies with relative low earnings, we expect that matching events mainly attract job seekers with low reservation wages, who are either already unemployed for a longer period or have less favorable characteristics. Table 6 compares the characteristics of participating unemployed workers (*compliers*) to the characteristics of those in the treatment group who did not attend the event (*never-takers*).[19] The p-values in the final column show that compliers and never-takers differ significantly in most observed characteristics. Compliers are more likely to be female, on average more than five years older and better educated. They worked fewer days in the three months before the matching event, but had higher earnings due to higher wages. While we do not observe any difference in the amount of UI benefits, job seekers working via a temporary employment agency before becoming unemployed rejected the invitation much more often. These unemployed may not expect to benefit from attending a matching event because they are already aware of job opportunities at temporary employment agencies.

The previous section showed the presence of heterogeneous treatment effects. Given the significant selectivity in the decision to attend a matching event, it is informative to study whether unemployed workers who are most likely to benefit from attending a matching event actually participate when being invited.

[18] 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.

[19] Due to random assignment, the $9,556$ individuals in the treatment group are representative for the full eligible population.

Table 6: Comparison of never-takers and compliers

	Never-takers	Compliers	p-value
Female	0.34	0.40	0.00
	(0.47)	(0.49)	
Age	39.95	45.17	0.00
	(11.87)	(11.37)	
Married	0.40	0.50	0.00
	(0.49)	(0.50)	
Primary/lower secondary education	0.27	0.21	0.00
	(0.44)	(0.41)	
Higher secondary education	0.55	0.53	0.17
	(0.50)	(0.50)	
College/university education	0.19	0.26	0.00
	(0.39)	(0.44)	
Benefits (prev. 3 months)	1624.62	1620.98	0.94
	(1963.23)	(2043.27)	
Earnings (prev. 3 months)	3775.34	4208.91	0.00
	(3592.13)	(4513.05)	
Workdays (prev. 3 months)	30.85	28.38	0.00
	(21.23)	(22.02)	
Perm. contract (prev. 3 months)	0.20	0.29	0.00
	(0.40)	(0.45)	
Temp agency work (prev. 3 months)	0.41	0.26	0.00
	(0.49)	(0.44)	
Observations	7,228	2,328	

NOTE – $N = 9,556$. All estimates are weighted by inverse treatment assignment probabilities. Column (1) and (2) report means, with standard deviations in parentheses. Column (3) shows p-values of two-sided difference-in-means tests.

To investigate this, we first use a probit regression to estimate the relation between observables and compliance in the treatment group. We then use the estimated coefficients to predict for all individuals in the experiment the propensity to comply. These estimated propensities range from slightly above zero to almost one. Next, we split the sample into individuals below and above the propensity-score median and re-estimate

the ATET on monthly working days separately for both subsamples. The estimation results in Table 7 show that participation in a matching event is mainly beneficial for unemployed workers who are less likely to attend when being invited. Judging from the observed characteristics, these are the job searchers who usually experience more difficulties in finding new work. This suggests that the effectiveness of matching events can be increased by convincing reluctant unemployed workers to start participating.[20]

Table 7: Impact by complier propensity - Monthly working days (ATET estimates)

	Low propensity group ($0.021 \leq$ prop. score < 0.220)			**High propensity group** ($0.220 \leq$ prop. score < 1)		
	+1m	+6m	+12m	+1m	+6m	+12m
Attendance	5.762***	2.037	0.805	0.080	0.150	-0.710
	(2.172)	(2.133)	(1.934)	(0.772)	(0.844)	(0.773)
Observations		6,305			6,305	
F-statistic (First stage)		222.83			738.91	

NOTE – $N = 12,610$. 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

While matching events can lead to direct job offers from temporary employment agencies, it is also possible that participation improves the job search performance of unemployed workers. Participants can collect personal feedback and valuable information about the labor market at the events. We next use survey data to examine changes in job search behavior, which allows us to study the importance of these channels. To analyze the survey data, we use the same empirical model as for the administrative data, but we weigh observations to account for non-response in the survey. The five

[20] We can use the estimates of heterogeneous treatment effects and the participation decision of invited job seekers to show how the ATET changes if we change the composition of invited job seekers. This could be used to optimize the matching events. Alternatively, we can investigate how the ATET changes if the UI administration would be able to change the participation decision. However, the lack of positive long-term effects does not provide a clear guideline what ATET should be optimized.

outcomes which we consider here correspond to those reported in Table 2. We divide reported job search motivation by its standard deviation in the control group to better gauge the magnitude of the corresponding treatment effect.[21]

As shown in the first column of Table 8, unemployed workers who participate in a matching event register, on average, at 0.75 additional temporary employment agencies. This registration allows agencies to contact job seekers when suitable vacancies become available. Estimation results in the remaining columns show that unemployed workers become more motivated to find work, and that they lower their reservation wage as a response to the matching events.

Table 8: Impact on job search behavior (ATET estimates)

	# employment agencies registered	Job search motivation (standardized)	# applications sent (last 4 weeks)	# job talk invitations (last 4 weeks)	Reservation wage (month, in euros)
*Treatment-on-the-Treated Estimates (*ATET*)*					
Attendance	0.748*	0.297**	-0.664	0.096	-249.26**
	(0.422)	(0.144)	(0.575)	(0.162)	(105.53)
Complier control group mean and standard deviation of outcomes					
Mean	3.549	3.567	7.248	0.715	2569.27
SD	(3.096)	(1.039)	(4.632)	(1.205)	(1230.74)

NOTE – $N = 2,888$. Observations are weighted by inverse probability weights to account for selective response. 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. Job search motivation is normalized to have a standard deviation of 1 in the control group. 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.

Increased motivation, reduced reservations wages and registering at temporary employment agencies can stimulate job finding both in the short-run and long-run. However, we do not find significant effects on the number of job applications and job interviews of participants in the weeks after an event.

Our interpretation of these results is that meeting temporary employment agencies helps unemployed workers to update their beliefs about their labor market prospects. During the events, they quickly learn which job opportunities are available to them, and they can register at additional temporary employment agencies to get updates in

[21]The corresponding ITT estimates are again reported in the appendix (Table A.9).

the future. Seeing job opportunities may make unemployed workers more optimistic about finding work, which can increase their job search motivation. But, at the same time, they might mainly learn about lower paying jobs in the temporary work sector. As a result, participants may lower their requirements for accepting a job offer, which is reflected in reduced reservation wages.[22]

A decrease in reservation wages should lead to lower accepted wages. The analysis of administrative data has shown that matching events increase earnings, but that this increase merely makes up for the decrease in benefit payments. Our data suggest that, on average, unemployed workers are willing to accept an earnings cut of about 25 percent compared to post-unemployment earnings. To further examine effects on earnings, we compare the earnings distributions of treated and non-treated compliers. Since there are no always-takers in the experiment, participants in the treatment group represent the treated compliers. We follow Imbens and Rubin (1997) to estimate the earnings distribution of non-treated compliers, which is the counterfactual.[23] Figure A.3 in the appendix shows the computed earnings distributions (excluding the mass points of zero earnings). One month after a matching event, the share of workers with earnings below 1,500 euros per month is substantially higher among the treated compliers. After six and 12 months, observed differences in the lower tail become smaller again.

In the previous section, we documented significant heterogeneous effects of participating in a matching event. In particular, men, younger individuals and those who recently became unemployed benefit in the short-run more from matching events. Our survey collects further information on actual meetings of participants during a matching event, which allows us to test for differences in performance by individual characteristics. As shown in Table 9, job searchers meet about three to four temporary employment agencies at a matching event, and each talk lasts, on average, eight minutes. About 43 percent of participants report to remain in contact with at least one agency after the matching event. Estimated standard deviations show that there is substantial variation between participants in the number and duration of meetings with temporary

[22]Figure A.2 in the appendix shows the distribution of reservation wages in treatment and control group. The graph illustrates a shift over the full distribution, which is slightly larger below the median.

[23]The earnings distribution of non-treated compliers is estimated from the earnings distribution of all individuals in the control group and the earnings distribution of non-compliers in the treatment group. The earnings distribution of non-treated compliers is given by $g_{c0} = \frac{1}{\phi_c} f_{00} - \frac{1-\phi_c}{\phi_c} f_{10}$, where ϕ_c denotes the share of compliers, f_{00} the earnings distribution of individuals in the control group and f_{10} the earnings distribution of never-takers in the treatment group. All complier distributions are estimated separately for each matching event and weighted by size to account for different shares of compliers and never-takers in the matching events.

employment agencies.

Table 9 reports how these outcomes differ by observable characteristics of participants. The regressions show that female, older, and medium- and high-educated job searchers have, on average, significantly shorter meetings. Furthermore, women talk to significantly fewer temporary employment agencies during the events, and the probability of further contact with an agency declines with age. These differences concur to a large extent with our earlier findings on heterogeneous treatment effects, which show that matching events tend to be less effective for women and older unemployed workers.

Table 9: Performance differences by individual characteristics

	# agencies spoken	Duration talk (min)	Further contact agency
Female	-0.316*	-0.772**	-0.004
	(0.171)	(0.339)	(0.034)
Age	-0.006	-0.051**	-0.003*
	(0.010)	(0.020)	(0.002)
Higher secondary education	0.253	-1.628***	0.050
	(0.190)	(0.415)	(0.037)
College/university education	0.399	-2.450***	0.085
	(0.310)	(0.626)	(0.079)
Population mean	3.66	7.69	0.43
and standard deviation	(2.35)	(4.60)	(0.50)
Observations	1,038	1,044	1,080

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 Reconciliation of findings

The goal of matching events is to stimulate job finding by reducing search frictions for benefits recipients. Our empirical results indeed show increased job finding in the short run but also a significant reduction in reservation wages. Lower reservation wages make unemployed workers less selective, which has a positive effect on job finding in

the short run. As a result, there is a short-lived positive effect on monthly earnings. Individuals in the control group have higher reservation wages that cause longer unemployment spells but somewhat higher earnings in the long run. The drop in reservation wages contradicts that matching events only reduce search frictions. Our survey results suggest that matching events may change the beliefs of participants about their labor market opportunities. Below we provide a job search model in which job seekers can update their beliefs during the search process. The predictions from this model are consistent with the drop in reservation wages and the effects on job finding and earnings estimated in the main analysis.

When becoming unemployed, a worker has initial beliefs λ_0 about the job offer arrival rate and $F_0(\cdot)$ about the wage offer distribution. Previous empirical evidence suggests that unemployed workers are usually too optimistic about the probability to find work (Mueller et al., 2018; Abebe et al., 2021). We assume that, while being unemployed, workers update their beliefs about the job offer arrival rate and the wage offer distribution (for example, using Bayesian learning). In each time period, three situations can arise. First, the worker does not receive a job offer in a particular time period t. In this case, the worker becomes more pessimistic about the job offer arrival rate ($\lambda_{t+1} < \lambda_t$) but, since no offer arrived, the wage offer distribution remains unaffected ($F_{t+1}(\cdot) = F_t(\cdot)$). Second, the worker receives a job offer with a wage above the reservation wage and starts working. And third, the worker receives a job offer but with a wage below the reservation wage. This causes the worker to become more optimistic about the job offer arrival rate ($\lambda_{t+1} > \lambda_t$) but less optimistic about the wage offer distribution. The latter means that $F_t(\cdot)$ first-order stochastically dominates $F_{t+1}(\cdot)$. While staying unemployed, workers update their beliefs and become more pessimistic about their earnings prospects.

In each period, job searchers can choose the level of job search effort, denoted by e_t. For sake of simplicity, we assume that, after t periods of unemployment, the probability of receiving a job offer equals $\lambda_t e_t$ and the costs of search effort are ce_t^2.[24] Following Mortensen (1986), the optimal search effort e_t^* in this period is thus given by

[24]Following Van den Berg and Van der Klaauw (2006), the model could be extended to distinguish between a search channel for regular jobs and a search channel for jobs via temporary work agencies. Both search channels are linked through the reservation wage and the substitutability of search effort for regular jobs and temporary jobs. If workers believe that one channel becomes less effective, they reduce their reservation wage and shift search effort to the other channel.

$$e_t^* = \frac{\lambda_t}{2c} \int_{\phi_t^*}^{\infty} \frac{w - \phi_t^*}{\rho} dF_t(w),$$

where ρ is the discount rate. The optimal reservation wage ϕ_t^* can be derived from the corresponding Bellman equation and follows from solving

$$\phi_t^* = (1+\rho)b + \frac{\lambda_t^2}{4c} \left(\int_{\phi_t^*}^{\infty} \frac{w - \phi_t^*}{\rho} dF_t(w) \right)^2,$$

where b is the unemployment benefits level. This framework imposes that the job search environment is stationary in each period. As a consequence, unemployed workers do not take uncertainty about their beliefs into account.

While staying unemployed, workers can only become more pessimistic about their job opportunities because each period without finding work is a bad signal. As a result, the reservation wage declines over time ($\phi_{t+1}^* < \phi_t^*$). Search effort of workers who remain unemployed will likely also decline. Because searching is a costly investment, unemployed workers will search less when they become more pessimistic about the returns to this investment.

At a matching event, individuals meet several temporary employment agencies within a short period of time. These agencies offer temporary jobs at various employers. The matching event thus provides information about the stock of vacancies in the labor market segment with temporary employment agencies. The direct effect of attending a matching event is that the probability to generate one or multiple job offers increases, which positively affects the probability to find work. However, earnings in temporary jobs are usually lower, either because wages are lower or the number of working hours is uncertain. Attending the matching event requires some effort from unemployed workers, which may crowd out regular job search effort and dampen the direct effect on job finding. Recall that, by attending a matching event, an unemployed worker already fulfills the weekly search requirement imposed by the UI administration.

Attendance may also accelerate the updating of job offer arrival rate and wage offer distribution. The events allow unemployed workers to get a better idea about how many suitable vacancies are available. Furthermore, when job searchers cannot receive a wage offer above the reservation wage at the matching event, they have to adjust their beliefs about the wage offer distribution and decrease their reservation wage. Krueger

and Mueller (2016) show that unemployed workers tend to set their reservation wage too high. Learning about one's actual labor market prospects may thus reduce it substantially. As a consequence, more jobs become acceptable but earnings will be lower.

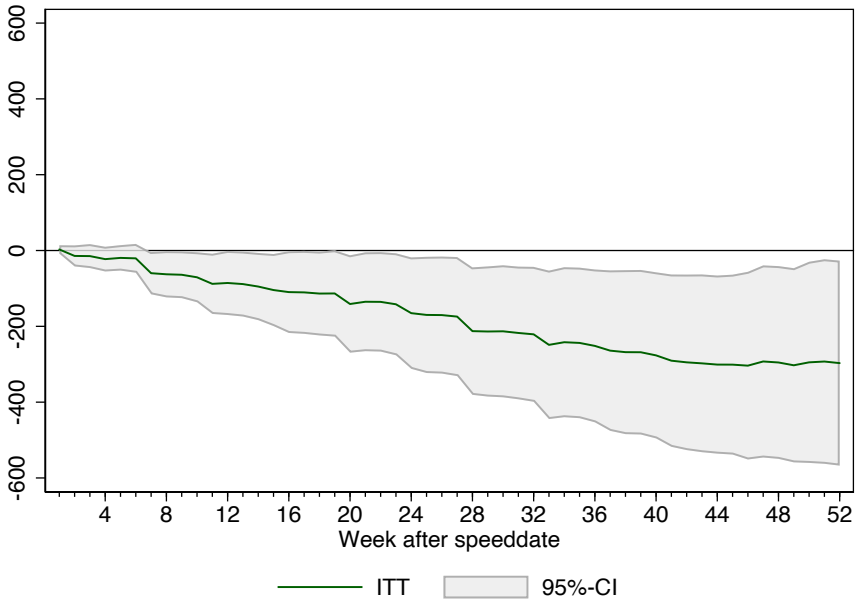
Taking all of these channels into account, the job search model predicts an immediate positive effect on job finding in the short run. First, matching events increase the job offer arrival rate by reducing search frictions. Second, unemployed workers with too high wage expectations reduce their reservation wage, which amplifies the positive effect on job finding but yields lower earnings. Learning about the availability of vacancies at a matching event may also make job seekers more optimistic. Most of our empirical findings are in line with these predictions. Matching events are found to increase job finding but the weak effect on earnings suggests that wages are lower. Furthermore, treated job seekers report lower reservation wages and higher job search motivation.

CV checks and individual feedback provided during the events can also help to increase the job offer arrival rate. This could explain why we observe higher job finding rates for both agency work and regular work. The absence of a longer run impact suggests that temporary jobs do not serve as stepping stones to regular work. While agency workers have the chance to get a permanent contract with the actual employer in the future, they might also search less for regular work while being employed by an agency.

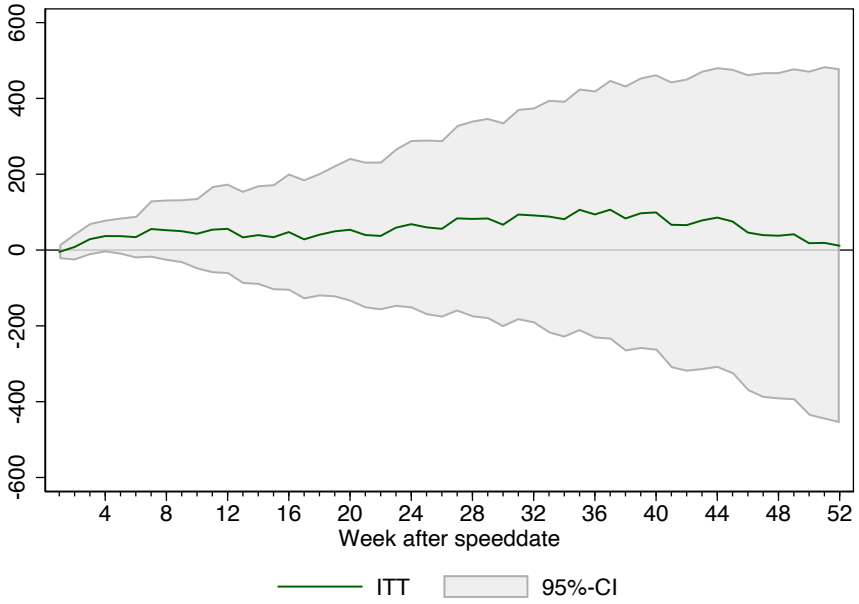
7.2 Cost-benefit analysis

To evaluate the cost effectiveness of matching events, we calculate the cumulative gains of the treatment and contrast them with the costs of organizing an event. The costs consist of the time investment made by caseworkers of the Dutch UI administration to set up a matching event, invite job searchers and temporary employment agencies, and host the event. In most cases, they take place at the local UI offices. The hourly personnel costs of a caseworker are approximately 55 euros. Participating temporary 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 across all events is four minutes, which translates into costs of about four euros per unemployed worker assigned to the treatment group.

Figure 3: ITT estimates on cumulative outcomes by week after matching event



(a) Cumulative UI benefits



(b) Cumulative earnings

For the cost-benefit analysis, we examine UI benefits and earnings up to one year after the matching events. Figure 3 plots ITT estimates for the cumulative outcomes

by week and Table 10 reports the corresponding point estimates measured after 12 months. The difference in UI benefits between control and treatment group steadily grows to about 300 euros during the first nine months and remains constant afterwards. This shows that matching events are a cost-effective policy instrument for the UI administration. When we discount the reduction in benefit payments at a rate of 10 percent, the net present value per invited unemployed worker amounts to 285 euros.[25] Cumulative earnings increase only modestly in the nine months after a matching event and the increase diminishes completely afterwards. The effect on cumulative earnings also never becomes significant. Combined with the reduction in UI benefits, this implies that participants in matching events experience, on average, a reduction in total income.

Table 10: Cumulative benefits, earnings and total income after one year (ITT estimates)

	UI Benefits	Earnings	Total income
Treatment group	-297.055**	11.344	-291.132
	(137.955)	(238.766)	(201.001)
Control group mean and standard deviation of outcomes			
Mean	6,966.73	14,695.82	22,457.90
Standard deviation	(7,770.21)	(12,669.23)	(11,698.54)

NOTE – $N = 12,610$. 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.

8 Conclusion

In this paper, we provide empirical evidence for the effectiveness of a novel job matching program in which unemployed workers get the opportunity to meet temporary employment agencies. Whereas other active labor market programs often rely on individual

[25] The net present value is defined as $\sum_{w=1}^{52}(1+\delta)^{-w}ITT_w - C$, 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 a matching event.

guidance or counseling in small groups, the scale of matching events is much larger, which implies very low organizational costs.

To evaluate the effectiveness of these events, we conduct a randomized experiment in cooperation with several local offices of the UI administration in the Netherlands. Using administrative data on unemployment and work spells up to one year after the matching event, we find that immediately after a matching event participating unemployed workers are six to seven percentage points more likely to start a new job. Approximately half of this increase can be explained by work spells at temporary employment agencies whereas the other half is due to regular employment contracts. This shows that the meetings with temporary employment agencies do not crowd out regular employment. The finding also supports the idea that matching events help unemployed workers to extend their network and improve their job search skills. Despite these strong short-run effects, differences between treatment and control group diminish in the subsequent months, suggesting that non-treated unemployed workers catch up in the medium run.

Our treatment effect estimates expose heterogeneity with respect to the type of matching event and individual characteristics of job searchers. The positive impact on job finding seems to be mainly driven by individuals who have been unemployed for at most three months at the time of the matching event. Also, we find stronger estimates for men and younger individuals. However, unemployed workers with these characteristics are less likely to attend an event when being invited. This suggests that the effectiveness of matching events can be increased by convincing these individuals to participate.

Due to the large effects on job finding in the first weeks, cumulative UI benefit payments decrease substantially. A cost-benefit analysis shows that the reduction in benefits payments after one year is much higher than the costs of organizing matching events. However, individuals cannot compensate the reduced benefit payments with higher earnings from work, causing a decline in total income. Such an impact is also observed by Markussen and Røed (2016) who show that, after an activation program, income effects are absent because unemployed workers accept marginal jobs.

Analyzing survey data collected shortly after the matching events, we also find that participation significantly affects job search behavior. Consistent with predictions that we derived from a job search model, participants become more motivated and decrease their reservation wage. These indirect effects of reduced search frictions suggest that meetings with temporary employment agencies allow job seekers to update their beliefs about current prospects on the labor market and readjust too high wage expectations.

It is also possible that some unemployed workers accept job offers with initially low pay because they expect wage growth or better chances of more stable employment in the future. This can contribute to higher job finding rates and is consistent with our finding of low earnings effects. It may be that participants widen the scope of search too much and accept work for which they are not well suited. This would explain the negative long-run effects on income, which are also observed in other studies (e.g. Cottier et al. (2015)).

Overall, our findings suggest the presence of considerable search and information frictions in the labor market for which matching events with temporary employment agencies can serve as a remedy in the short run. However, the results also indicate that temporary work does not serve as a stepping stone towards regular employment or better paying jobs (see Autor and Houseman (2010) for a similar finding). While the matching events increase job finding in the first weeks, the absence of positive effects on income and on working in the long run shows that the program does not provide a clear benefit to the average unemployed worker.

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Appendix (for online publication)

Table A.1: Overview matching events

#	Location	Date	Type	Size	Treatment	Attendance
1	Doetinchem	4-Jul-14	General	187	51%	18%
2	Doetinchem	5-Sep-14	Technical	166	48%	18%
3	Leeuwarden	17-Sep-14	General	4,091	76%	22%
4	Eindhoven	18-Sep-14	Technical, Transport, Logistics, Industry, Security, Construction, ICT	932	50%	24%
5	Leeuwarden	12-Nov-14	General	2,222	83%	31%
6	Venlo	22-Jan-15	General	312	80%	38%
7	Zwolle	4-Feb-15	General	345	80%	13%
8	Groningen	19-Mar-15	Commercial services	478	80%	19%
9	Tiel	11-Jun-15	General	296	80%	11%
10	Veghel	10-Jun-15	General	680	75%	24%
11	Steenwijk	28-Aug-15	General	412	70%	16%
12	Groningen	17-Sep-15	Technical, Engineering, Construction	446	80%	14%
13	Venray	5-Nov-15	General	162	80%	42%
14	Venray	14-Jan-16	General	183	80%	44%
15	Venlo	21-Jan-16	General	427	80%	38%
16	Groningen	18-Feb-16	Technical, Engineering, Construction	711	81%	15%
17	Venray	25-Feb-16	General	390	80%	33%
18	's Hertogenbosch	25-Feb-16	General	170	82%	41%

Table A.2: Comparison of survey respondents to full sample

	Full sample	Survey respondents	p-value
Female	0.36	0.35	0.50
	(0.48)	(0.48)	
Age	41.09	41.40	0.20
	(11.94)	(11.69)	
Primary/lower secondary education	0.25	0.28	0.00
	(0.43)	(0.45)	
Higher secondary education	0.55	0.66	0.00
	(0.50)	(0.48)	
College/university education	0.20	0.06	0.00
	(0.40)	(0.24)	
Observations	12,610	2,888	

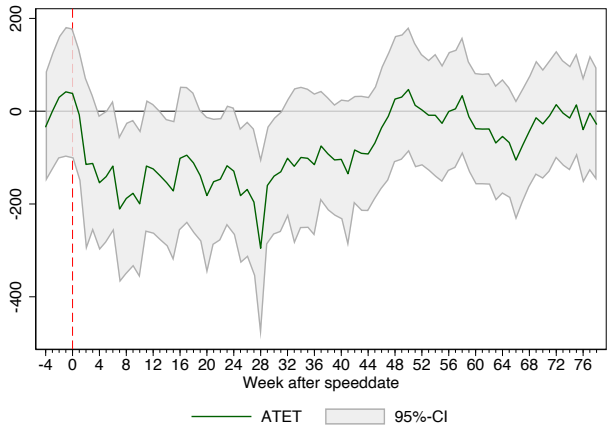
Note – Column (1) and (2) report means, with standard deviations in parentheses. Column (3) shows p-values of two-sided difference-in-means tests.

Table A.3: Descriptive statistics and balancing (Sample of survey respondents)

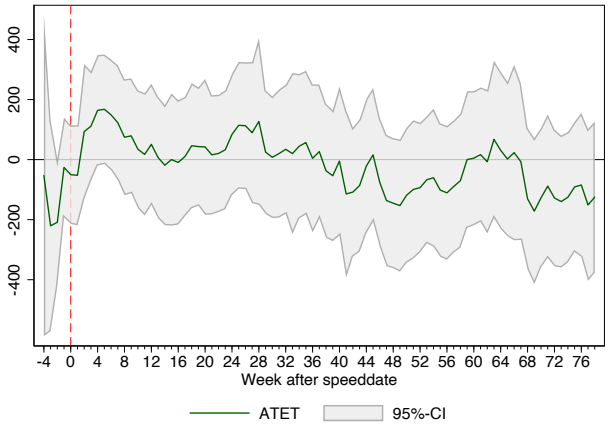
	Control group	Treatment group	p-value
Female	0.40	0.42	0.33
	(0.49)	(0.49)	
Age	45.82	46.04	0.67
	(11.17)	(10.97)	
Married	0.52	0.52	0.81
	(0.50)	(0.50)	
Primary/lower secondary education	0.19	0.19	0.86
	(0.39)	(0.39)	
Higher secondary education	0.50	0.52	0.31
	(0.50)	(0.50)	
College/university education	0.31	0.29	0.34
	(0.46)	(0.45)	
Benefits (prev. 3 months)	1963.89	1870.07	0.35
	(2295.66)	(2256.54)	
Earnings (prev. 3 months)	4729.62	4311.85	0.05
	(4915.27)	(4518.73)	
Workdays (prev. 3 months)	30.24	29.51	0.45
	(21.83)	(21.96)	
Perm. contract (prev. 3 months)	0.30	0.28	0.23
	(0.46)	(0.45)	
Temp agency work (prev. 3 months)	0.23	0.27	0.06
	(0.42)	(0.44)	
Observations	675	2,213	

Note – All estimates are weighted by inverse treatment assignment probabilities. Columns (1) and (2) report means, with standard deviations in parentheses. Column (3) shows p-values of two-sided difference-in-means tests.

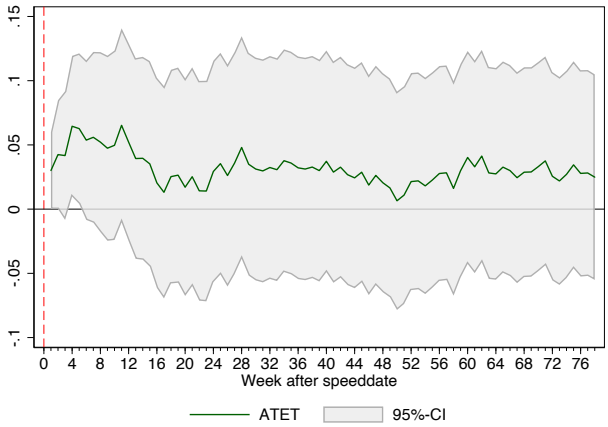
Figure A.1: ATET estimates by week after matching event



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



(b) Impact on monthly earnings (in euros)



(c) Impact on starting a new job

Note: For starting a new job after the matching event no pre-treatment effects can be defined.

Table A.4: ITT estimates for main outcomes

	Collecting benefits			**Monthly benefits**			**New work - Temp. agency**		
	+1m	+6m	+12m	+1m	+6m	+12m	+1m	+6m	+12m
ITT	-0.019**	-0.021**	-0.005	-36.15**	-39.56**	0.67	0.009**	0.010	0.004
	(0.009)	(0.010)	(0.009)	(17.14)	(17.35)	(14.37)	(0.004)	(0.008)	(0.009)
Control group mean and standard deviation of outcomes									
Mean	0.706	0.416	0.335	922.43	589.94	399.71	0.033	0.156	0.234
SD	(0.455)	(0.493)	(0.472)	(902.20)	(921.02)	(717.44)	(0.179)	(0.363)	(0.424)
	New work - Regular			**Monthly working days**			**Monthly earnings**		
	+1m	+6m	+12m	+1m	+6m	+12m	+1m	+6m	+12m
ITT	0.010*	0.003	0.010	0.393**	0.177	-0.057	38.508*	26.497	-21.871
	(0.006)	(0.010)	(0.010)	(0.190)	(0.200)	(0.183)	(21.713)	(25.172)	(25.855)
Control group mean and standard deviation of outcomes									
Mean	0.067	0.339	0.489	7.827	11.231	10.232	785.38	1187.23	1298.27
SD	(0.251)	(0.474)	(0.500)	(9.396)	(10.002)	(9.915)	(1090.76)	(1241.75)	(1308.37)

Note – $N = 12,610$. ITT estimates are obtained through OLS regressions. 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.5: Impact by type of contract

	Monthly working days						Monthly earnings					
	Employment agency			Regular employment			Employment agency			Regular employment		
	+1m	+6m	+12m	+1m	+6m	+12m	+1m	+6m	+12m	+1m	+6m	+12m
Intention-to-Treat Estimates (ITT)												
Invited	0.109	0.186	0.065	0.284	-0.009	-0.122	4.379	16.672	-4.832	34.129*	9.825	-17.038
	(0.135)	(0.142)	(0.129)	(0.179)	(0.206)	(0.191)	(14.885)	(16.043)	(16.098)	(19.925)	(25.088)	(25.746)
Treatment-on-the-Treated Estimates (ATET)												
Attended	0.466	0.792	0.278	1.212	-0.037	-0.519	18.673	71.094	-20.606	145.535*	41.897	-72.655
	(0.576)	(0.606)	(0.552)	(0.770)	(0.877)	(0.813)	(63.532)	(68.484)	(68.631)	(85.867)	(107.078)	(109.714)
Control group mean and standard deviation of outcomes												
Mean	3.00	3.14	2.58	4.82	8.10	7.65	318.70	337.19	329.49	466.68	850.04	968.77
Standard deviation	(6.85)	(6.94)	(6.44)	(8.36)	(10.07)	(9.75)	(767.31)	(782.80)	(783.61)	(946.04)	(1220.20)	(1308.81)

NOTE – $N = 12,610$. 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.6: Impact on monthly working days by benefit status

	Monthly working days (w/ benefits)			**Monthly working days (w/o benefits)**		
	+1m	+6m	+12m	+1m	+6m	+12m
Intention-to-Treat Estimates (ITT)						
Invited	0.091	0.019	-0.030	0.303*	0.158	-0.027
	(0.152)	(0.126)	(0.115)	(0.171)	(0.207)	(0.192)
Treatment-on-the-Treated Estimates (ATET)						
Attended	0.386	0.083	-0.127	1.291*	0.673	-0.114
	(0.651)	(0.536)	(0.490)	(0.737)	(0.883)	(0.820)
Control group mean and standard deviation of outcomes						
Mean	3.40	2.27	1.96	4.43	8.96	8.27
Standard deviation	(6.96)	(5.91)	(5.51)	(8.36)	(10.28)	(10.02)

NOTE – $N = 12,610$. 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.7: Additional heterogeneous effects - Monthly working days (ATET estimates)

	Size of event		**Gender**		**Age**	
	Small	Large	Female	Male	< 40	≥ 40
After 1 month	0.815	2.679**	0.618	2.077*	5.224***	-0.212
	(1.343)	(1.220)	(1.228)	(1.082)	(1.768)	(0.867)
After 6 months	-1.097	1.596	-1.872	2.360**	0.618	0.692
	(1.453)	(1.227)	(1.307)	(1.130)	(1.738)	(0.940)
After 12 months	-0.701	-0.606	-2.014	0.858	0.064	-0.513
	(1.152)	(1.202)	(1.231)	(1.011)	(1.553)	(0.871)
Observations	3,564	6,313	4,468	8,142	5,857	6,753

	Education			**Remaining UI duration**	
	Low	Medium	High	More than 3 months	At least 3 months
After 1 month	0.772	1.473	2.882*	1.218	1.203*
	(1.838)	(1.140)	(1.535)	(4.034)	(0.672)
After 6 months	1.836	-0.159	1.317	5.177	-0.078
	(1.991)	(1.187)	(1.553)	(4.283)	(0.763)
After 12 months	1.381	-0.604	-1.146	-3.044	-0.016
	(1.825)	(1.101)	(1.360)	(3.900)	(0.696)
Observations	3,167	6,916	2,527	4,489	8,121

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). Effects reported by size of event are for general speeddates only. Speeddates with at least 1,000 invitees are classified as large. * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

Table A.8: Sectors one year after treatment

	Control group	Treatment group	p-value
Temporary work	0.28	0.28	0.79
	(0.45)	(0.45)	
Commercial services	0.13	0.13	0.79
	(0.34)	(0.33)	
Health and social work	0.10	0.09	0.29
	(0.30)	(0.29)	
Metal- and technical industries	0.06	0.07	0.58
	(0.24)	(0.25)	
Port	0.04	0.04	0.76
	(0.20)	(0.20)	
Food services	0.04	0.04	0.86
	(0.19)	(0.19)	
Retail and crafts	0.03	0.04	0.06
	(0.17)	(0.19)	
Other sectors	0.55	0.56	0.70
	(0.50)	(0.50)	
Observations	3,054	9,556	

Note – All estimates are weighted by inverse treatment assignment probabilities. Columns (1) and (2) report means, with standard deviations in parentheses. Column (3) shows p-values of two-sided difference-in-means tests.

Table A.9: Impact on job search behavior (ITT estimates)

	# employment agencies registered	Job search motivation (standardized)	# applications sent (last 4 weeks)	# job talk invitations (last 4 weeks)	Reservation wage (month, in euros)
Intention-to-Treat Estimates (ITT)					
Treatment	0.265*	0.105**	-0.235	0.034	-88.20**
	(0.150)	(0.051)	(0.203)	(0.057)	(37.30)
Control group mean and standard deviation of outcomes					
Mean	3.510	3.610	6.421	0.800	2281.59
SD	(3.026)	(1.000)	(4.063)	(1.190)	(1063.97)

Note – $N = 2,888$. Observations are weighted by inverse probability weights to account for selective response. ITT estimates are obtained through OLS regressions. 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. Job search motivation is normalized to have a standard deviation of 1 in the control group. 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.

Figure A.2: Reported monthly reservation wage (weighted)

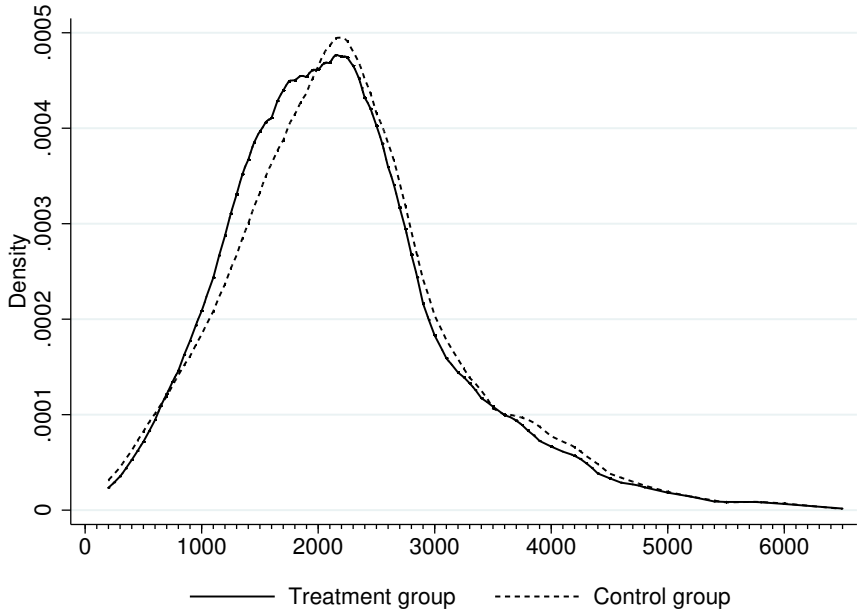
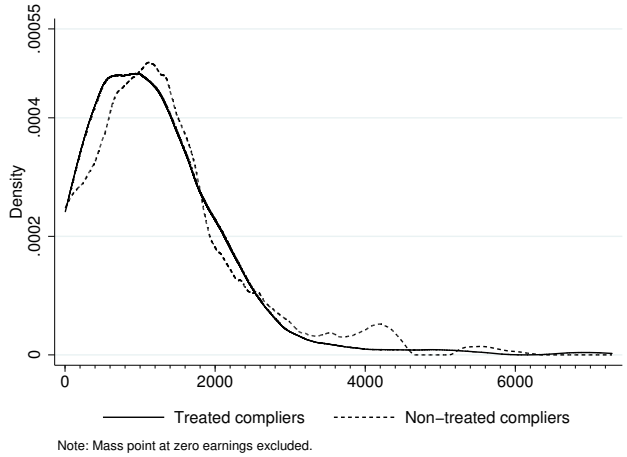
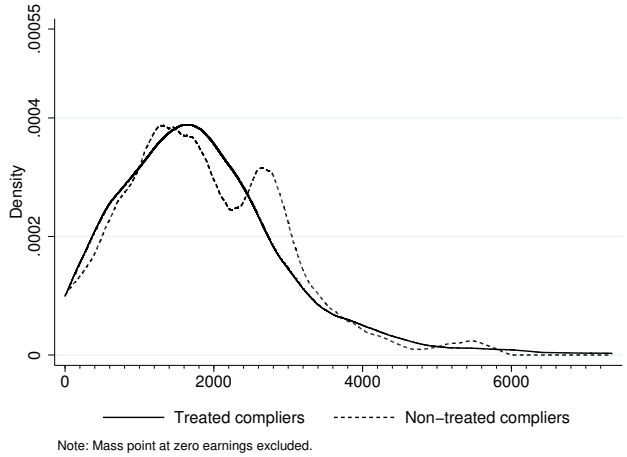


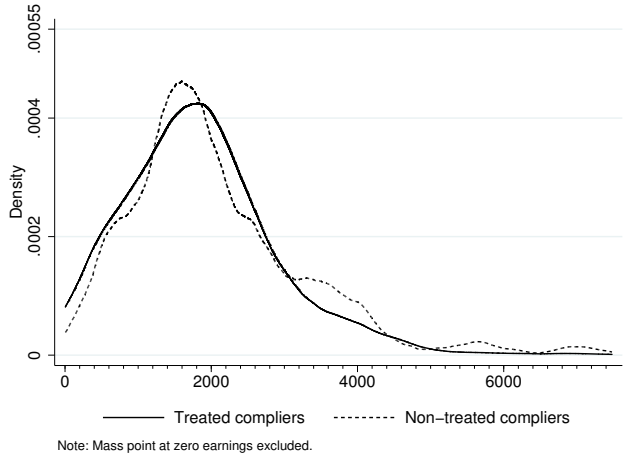
Figure A.3: Earnings distributions of compliers (Epanechnikov kernel)



(a) Earnings (in euros) after one month



(b) Earnings (in euros) after six months



(c) Earnings (in euros) after twelve months

B. Displacement effects

To examine the presence of displacement effects, we first 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 relative 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 B.1 shows the share of individuals who work six months after the matching event in treatment and control group, sorted by treatment assignment probability. The graphical evidence 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 potential displacement effects, we extend our sample by additional job seekers. For each local UI office in our sample, we use administrative data on benefit recipients during any of the months at 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 11 locations in 12 different months. Table B.1 illustrates the corresponding panel structure. 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 groups 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

$$Y_{ilt} = \kappa_l + \lambda_t + \pi D_{ilt} + V_{ilt}$$

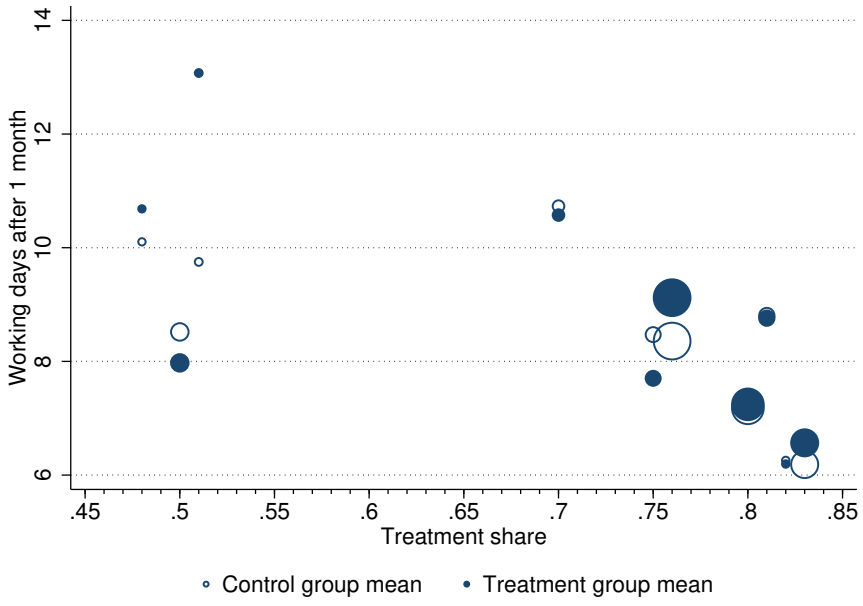
where Y_{ilt} denotes the labor market outcome of individual i in location l at time t. κ_l are location fixed effects and λ_t are month fixed effects. D_{ilt} is an indicator that equals one if there is a matching event at location l and month t. Finally, V_{ilt} represents the error term. The parameter π then denotes the difference-in-difference estimator of displacement effects. This approach requires two assumptions. First, labor market outcomes follow a common trend over time at all locations, and second, unemployed workers search for employment in their home region.

When comparing unemployed workers in the experiment to other benefit recipients, we have to account for the fact that the entire population of benefit recipients might

differ from the subset that was selected by the UI offices as eligible for a matching event. As shown in Table B.2, the two groups indeed differ in terms of observable characteristics.[26] Benefit recipients eligible for matching events are younger, more likely to be male and unmarried. They also have somewhat lower schooling levels and shorter unemployment spells. To account for these differences, we estimate for each job searcher the propensity to be eligible for a matching event and weigh our estimates accordingly using inverse-probability weights.[27]

Given that some matching events took place at the same location within a period of three months, we focus on the short-run impact to avoid confounding effects. Using monthly working days after one month as outcome, we find a small and insignificant difference-in-difference coefficient (p-value = 0.46). This supports our assumption that matching events do not affect the job finding rate of unemployed workers who are not invited to matching events.

Figure B.1: Monthly working days after one month by treatment share



[26]Compared to our dataset on individuals in the experiment, the data on the full population contain fewer individual characteristics.

[27]The propensity score is estimated using the characteristics shown in Table B.2 as well as sector indicators. Figure B.2 shows that the estimated propensity scores of the control groups and the added sample of benefit recipients have a large area of common support.

Table B.1: Locations and dates (**X**'s indicate matching events)

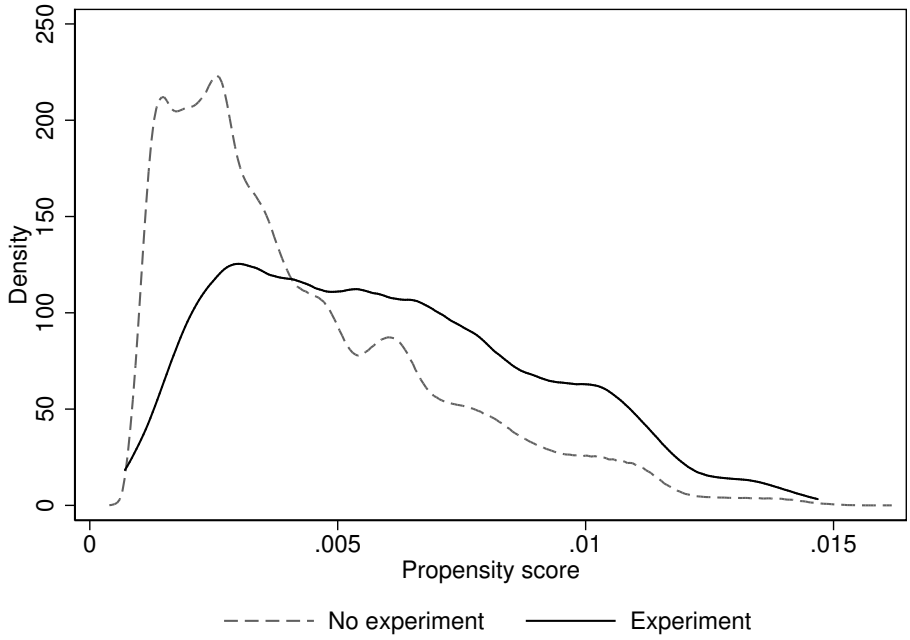
Location / **Month**	Doetinchem	Leeuwarden	Eindhoven	Venlo	Zwolle	Groningen	Tiel	Veghel	Steenwijk	Venray	's Hertogenbosch
Jul-14	X										
Sep-14	X	X	X								
Nov-14		X									
Jan-15				X							
Feb-15					X						
Mar-15						X					
Jun-15							X	X			
Aug-15									X		
Sep-15						X					
Nov-15										X	
Jan-16				X						X	
Feb-16						X				X	X

Table B.2: Comparison of control group and entire population

	Population	Control group	p-value
Female	0.49	0.36	0.00
	(0.50)	(0.48)	
Age	44.37	40.97	0.00
	(11.82)	(11.91)	
Married	0.48	0.43	0.00
	(0.50)	(0.50)	
Level of education	5.78	5.66	0.02
	(2.91)	(2.77)	
Working (one month before)	0.38	0.48	0.00
	(0.49)	(0.50)	
Working (two months before)	0.45	0.60	0.00
	(0.50)	(0.49)	
Working (three months before)	0.51	0.75	0.00
	(0.50)	(0.44)	
Observations	708,296	3,055	

Note – Column (1) and (2) report means, with standard deviations in parentheses. Column (3) shows p-values of two-sided difference-in-means tests.

Figure B.2: Common support of propensity score estimates



C. 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 temporary employment 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 temporary 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 temporary employment agencies after the matching event? (Possible answers: Yes, one ore more agencies contacted me; yes, I contacted one ore more agencies; no)

D. Invitation letter matching event

The exact phrasing of invitation letters differed somewhat between local offices of the UI administration but the content was very similar. Below we provide a translation of the invitation letter used in Leeuwarden.

English translation

Invitation - Speeddate with temporary work agencies

Dear Sir or Madam,

You receive UI benefits and you are searching for work. To support you, we organize a meeting with temporary work agencies for you: the Speeddate. You are cordially invited on

Date: XXX, Location: XXX, Time: XXX

What can you expect?

The goal of the Speeddate is to get you quickly in touch with multiple temporary work agencies offering jobs in various sectors. They would like to get to know you and find out whether there is work for you within their large network. Make sure that you are capable to present yourself briefly and powerful. It would be good to already think beforehand about what you want and to bring a sufficient number of CV printouts.

Your participation in the Speeddate can be registered as a job application activity. We count on your presence. If you cannot come, please send a message via your digital file.

Looking forward to seeing you at the Speeddate.

Please note: This letter is your entrance ticket, which you should hand in upon arrival. If you have to travel more than 15 km to the Speeddate location, you will receive a travel cost reimbursement.