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Bas van der Klaauw
Lennart Ziegler

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

Lennart Ziegler
University of Vienna

ABSTRACT

A Field Experiment on Labor Market Speeddates for Unemployed Workers*

We conduct a field experiment to evaluate the effectiveness of labor market speeddates where unemployed workers meet temporary employment agencies. Our analysis shows that participation in such events increases immediate job finding by 6-7 percentage points. In the subsequent months, employment effects diminish again, suggesting that vacancies mediated through temporary employment agencies have 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 fully compensate for the decline in benefit payments. Additional survey evidence shows that speeddate participation increases job search motivation and reduces reservation wages.

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

Corresponding author:
Bas van der Klaauw
VU University Amsterdam
Department of Economics
De Boelelaan 1105
1081 HV Amsterdam
The Netherlands

E-mail: b.vander.klaauw@vu.nl

1   Introduction

Search frictions are an important source of inefficiency in the labor market. To reduce these frictions, many countries rely on various types of labor market policies. In a recent meta analysis of more than 800 estimates from 207 studies, Card et al. (2018) find that job search assistance programs are relatively effective in increasing job finding rates. This holds especially in the short run and for disadvantaged job seekers.[1] However, many programs involve high costs as they include intensive counseling by caseworkers.[2]

This paper focuses on a novel policy instrument which consists of matching events organized by the unemployment insurance (UI) administration. At these 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. In addition, the temporary employment agencies sometimes provide feedback to job seekers on their CV and job talk skills, which may help to find work.

The matching events differ from usual job search assistance programs in various ways. First, they 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). An impact on job search behavior prior to the intervention, due to for example a threat effect, is also unlikely because attendance is voluntary and matching events are irregular and announced only

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

[2]Activation programs are evaluated in a wide range of countries including the Netherlands (Gorter and Kalb, 1996; Van den Berg and Van der Klaauw, 2006), Sweden (Sianesi, 2004), Norway (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).

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 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, meeting events do not provide additional job opportunities to benefit recipients. A positive treatment effect suggests 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 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.[3] Crépon et al. (2013b) conduct a randomized controlled trial to assess the effects of job search support for long-term welfare recipients in France. Although job seekers are significantly more likely to find employment, the program is not cost effective. In another experiment, Crépon et al. (2013a) analyze a large-scale job search assistance program for young, educated individuals with long unemployment spells. By varying the treatment intensity between re-

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

gions, the study estimates positive effects on the job finding rate but also negative externalities on both ineligible and eligible job seekers. Correcting for displacement effects, the overall impact on job creation is likewise small compared to the program costs. To contrast the effectiveness of private and public providers, Behaghel et al. (2014) conduct an experiment on counseling job 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. In a recent study, Cottier et al. (2015) evaluate the performance of a large private job search assistance provider in Switzerland and find a short-lived positive impact on job finding which turns negative two years later.

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 participa-

tion in matching events does not lead to an advantage in the long run. There is substantial effect heterogeneity with respect to type of matching event and characteristics of participants. In particular, the impact of matching events persists somewhat 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 slightly more in regular employment, which suggests that the meetings 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.

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 previous findings of increased regular employment and lower wages of participants in the matching events. Overall, our results indicate that temporary work is not a stepping stone towards regular employment.

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 analyze displacement effects and provide a cost-benefit analysis. Finally, Section 8 concludes.

2 Experimental setting

2.1 Unemployment insurance

Workers in the Netherlands are publicly insured against unemployment.[4] They are entitled to UI benefits when they lose at least five working hours per week, or if they worked less than 10 hours per week, 50 percent of their working hours. Moreover, they 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.[5]

The amount of UI benefits is based on earnings in the 12 months prior to unemployment. Workers eligible for UI benefits receive 75 percent of their earnings in the first two months, and 70 percent thereafter. Benefits amount to at least 70 percent of the minimum wage and are capped at a maximum of about 4,400 euros per month before taxes.[6] 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.[7]

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

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[4]An exception are self-employed workers.

[5]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 has been reduced to 24 months.

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

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

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.

2.3 Matching events

The matching events are organized by the local offices of the UI administration and can be either general or targeted towards a specific sector. At the events, benefit recipients have the opportunity to talk to representatives of the 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. 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. Invited benefit recipients are not obliged to attend the matching event but participation counts as one job application
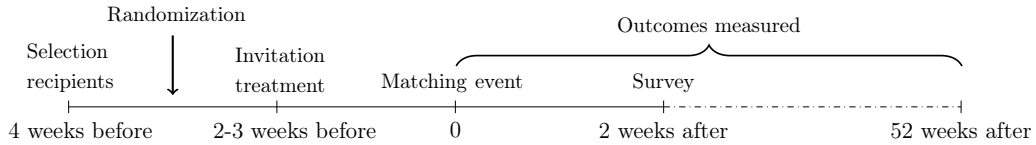
Figure 1: Timeline experiment

which all benefit recipients have to make each 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, a caseworker only invests about 13 hours of work in organizing 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 as a useful tool.

2.4 The experiment

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

Figure 1 shows a 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 affected. It is not known to participants that they take part in an experiment. The relative size of the treatment group is determined by the 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 UI offices

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

register attendance of every participant.

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 have not yet completed the questionnaire receive email reminders one and two weeks later. The survey includes questions about job search behavior and, in case of the treatment group, about their experience with the matching event. Furthermore, participating 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, out of which five events were sector specific. The number of participants varies considerably ranging from small matching events with 15 participants to big events with more than 700 participants, where they 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 and survey data complemented with attendance lists of the matching events provided by the UI administration. Approximately 10 percent of individuals were twice entitled to participation. In the analysis, we measure treatment effects starting from

the first time an individual was 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, which are the key outcomes of interest. These variables are observed for all individuals up to one year after the matching event. We define an individual as working if any labor earnings are registered for a given period. Furthermore, the administrative records contain a set of individual characteristics.

Table 1 shows descriptive statistics on characteristics of job searchers in the experiment as well as information on their previous employment and benefits spells measured up to three months before the matching event. Because the matching events differ in the share of individuals assigned to the treatment group, each observation is weighted by its inverse treatment assignment probability. The treatment and control group have a very similar composition of job searchers. The final column of Table 1 shows no statistically significant differences (at the 10-percent level) in characteristics measured before the matching events.

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 months.[10] Approximately 22 percent of individuals had a permanent contract in that period. The variables reported in Table 1 serve as control variables in the empirical analysis.

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

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

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

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)	
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.

among matching events. Whereas the attendance rate in the treatment group is 24 percent, nobody in the control group attended the matching events. In addition to UI benefit recipience and the amount of monthly benefits, we will analyze treatment effects on the probability of starting a new work spell after the matching event, working days and monthly earnings. Comparing the raw means in the treatment and control group four weeks after the intervention, we observe a significant impact of matching events for most outcome variables. In the treatment group, fewer individuals collect benefits, and they are more likely to start a new job. Benefits payments are about four percent lower and earnings are approximately four percent higher compared to the control group.

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.[11] As shown in Table 2, the response rate does not differ between the treatment and control group. Comparing individual characteristics of respondents to the full sample of the experiment, we find that individuals with higher levels of schooling are significantly more likely to respond to the questionnaire (see Table A.5 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.

The lower panel of Table 2 shows summary statistics of outcomes reported in the survey. These outcomes include a subjective measure of the motivation to search for a new job as well as the number of applications and the number of job talk invitations, which proxy job 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. On average, they are registered at three to four temporary employment agencies and report a reservation wage of about 2,300 euros per month. A first comparison between means in the treatment and control group shows significant differences in three survey outcomes. Benefit recipients 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.

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

Table 2: Descriptives statistics of outcomes

	Control group	Treatment group	p-value	Observations
Attendance	0.00	0.24	0.00	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 work spell since 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.

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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 control group of matching event s. T_i indicates whether the individual attended the matching event. X_i is a vector containing the observed individual characteristics described in Table 1. Matching event fixed effects, denoted by μ_s, account for different treatment assignment probabilities of matching events. Whereas X_{is} is included only 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. Instead, we focus on two alternative estimates to capture program effects. By regressing the outcome variable on matching event assignment (Z_i) instead of participation (T_i), we estimate the intention-to-treat effect (ITT). The coefficient on Z_i then corresponds to the average of a zero effect for invited job searchers who did not participate and the treatment effect for actual participants. The ITT is informative from a policy perspective as the UI administration can only assign benefit recipients to the matching events but cannot enforce participation. Therefore, the ITT can 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 instrument. Because matching event attendance is only possible upon invitation, there are no job seekers who are always treated independent of assignment (*always takers*). Thus, the LATE estimate corresponds to the average treatment effect on the treated (ATET), which is the average impact on all treated individuals.

4.1 First-stage results and identification

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

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 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

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.

working via temporary employment agencies. If these effects occur, the IV 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 working days ($\hat{\delta} = -0.106$; p-value $= 0.53$).

4.2 Theoretical predictions

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

In the short run, we expect the positive employment effects to be mainly driven by higher job finding rates at temporary employment agencies. At-

tendees might directly get a job offer at the matching events or they register with the agencies and can be considered for vacancies in the future. Many employment agencies also offer a CV check at matching events and provide individuals with feedback on their application skills, a service which is also provided to individuals who register themselves at a temporary employment agency. If this feedback positively affects the job search process, we should as well observe higher job finding rates for regular work in the short run.

The matching events might change job search behavior of participants. In this case effects on the job finding rate are less clear. On the one hand, the events may increase motivation and search effort if job seekers are able to collect useful information about available vacancies on the labor market. On the other hand, negative effects are possible when attendance crowds out other job search activities. Job seekers can also get discouraged if they learn that job finding prospects are worse than expected. Depending on the information they obtain, reservation wages may likewise be positively or negatively affected.

The overall increase in work does not need to coincide with a decrease of the same size in the share of individuals collecting benefits. As shown in the previous section, earnings of some workers are low and can then be supplemented with UI benefit payments. Because working hours at temporary employment agencies vary, some individuals may start working again but keep a share of their benefits.

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

5 Results

5.1 Impact on labor market outcomes

In this section, we present estimation results for the impact of matching events on unemployment benefits, work spells, labor earnings, and other job characteristics using the administrative data. Figure 2 plots ATET estimates by week after the matching event for collecting benefits and working days.[12] Graphs for the amount of monthly benefits and earnings can be found in the appendix (Figure A.2a and A.2b).

The graphs illustrate a clear immediate impact of participating in the matching events on both outcomes. The upper plot shows that invited job seekers are significantly less likely to collect unemployment benefits in the short run. 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 and not statistically significant.

The lower plot of Figure 2 illustrates the impact on working days during a given week after the event. 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. Two months after the matching event, the ATET fades to be statistically significant and remains at a low level.

To better quantify the impact of matching events, we summarize ITT and ATET estimates as well as control group means and standard deviations for various labor market outcomes in Tables 4 to 6. All outcomes are measured one, six and 12 months after the matching event to capture both short-term and long-term effects.

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[12]Corresponding ITT estimates are equivalent to multiplying ATET estimates by the attendance share (0.24).

Figure 2: ATET estimates by week after matching event
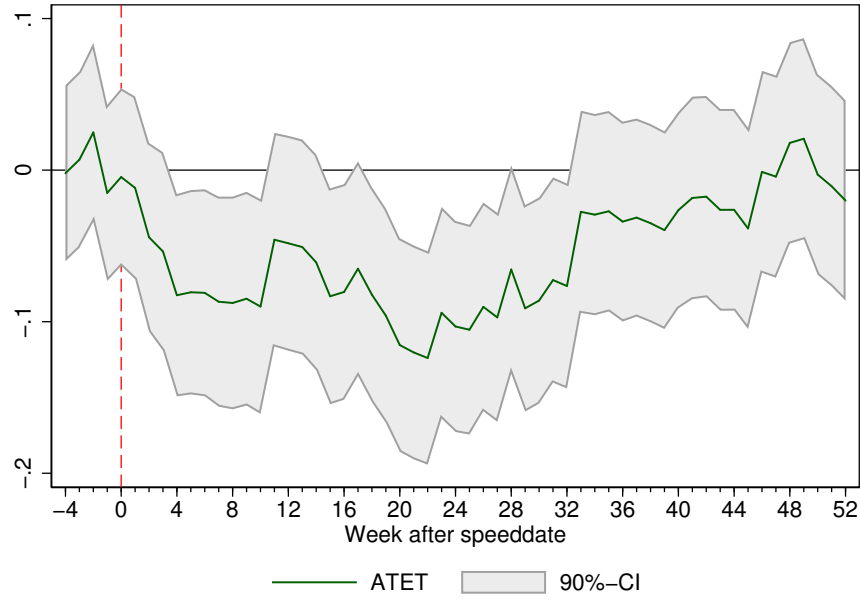
(a) Impact on collecting UI benefits
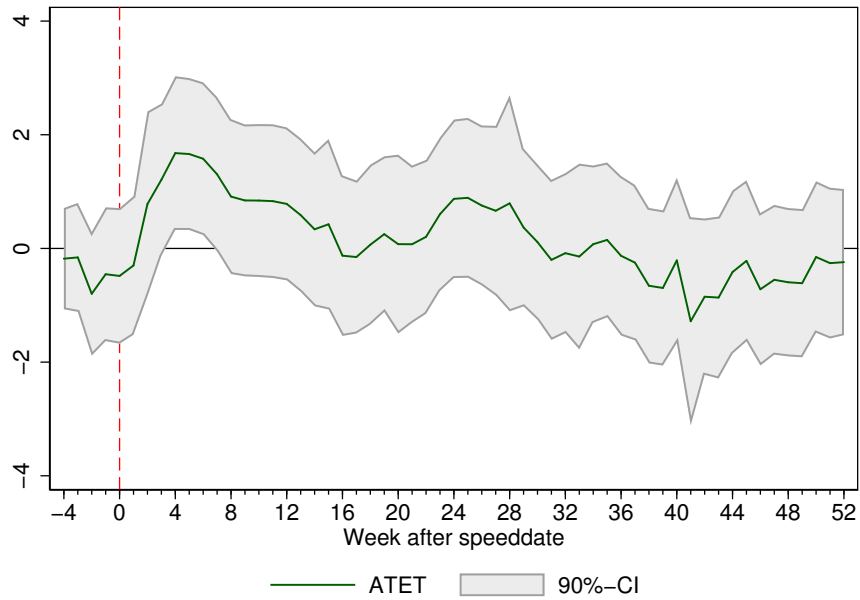
(b) Impact on working days

Table 4: Impact on UI benefits

	Collecting benefits			Amount monthly benefits		
	+1m	+6m	+12m	+1m	+6m	+12m
Intention-to-Treat Estimates (ITT)						
Invited	-0.019**	-0.021**	-0.005	-36.15**	-39.56**	0.67
	(0.009)	(0.010)	(0.009)	(17.14)	(17.35)	(14.37)
Treatment-on-the-Treated Estimates (ATET)						
Attended	-0.083**	-0.090**	-0.020	-154.17**	-168.69**	2.84
	(0.041)	(0.042)	(0.040)	(74.34)	(74.80)	(61.25)
Control group mean and standard deviation of outcomes						
Mean	0.71	0.42	0.34	922.43	589.94	399.71
Standard deviation	(0.46)	(0.49)	(0.47)	(902.20)	(921.02)	(717.43)

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.

We first consider benefit receipt. The first three columns of Table 4 show the impact on the probability of collecting UI benefits. One and six months after the matching event, the treatment effect on the treated amounts to eight and nine percentage points, respectively. Due to partial compliance, intention-to-treat estimates are smaller but still sizeable when compared to the share of benefit recipients in the control group. Despite the comparatively strong impact in the short run, treatment effects are virtually zero one year after the matching event. The final three columns of Table 4 show the impact on the amount of monthly UI benefits. After one and six months, treated job seekers receive, on average, 150 to 170 euros less in benefits. The ITT estimate is just below 40 euros after one month and six months. This is a non-ignorable effect given that average benefit payments in the control group are about 922 euros and 590 euros in the first and sixth months, respectively. 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. We cannot distinguish both channels because those who still collect benefits are a selective sample of job seekers. As for the benefit indicator, estimated differences vanish again 12 months later. Given that participants do not differ in the probability of benefit receipt after one year, we can, in this case, assume that the amount of benefits conditional on collecting benefits is not affected either.

Next, we examine the impact of matching events on work-related outcomes.[13] The first column of Table 5 shows that treated job seekers are 6.5 percentage points more likely to start a new job within the first four weeks. The ITT effect in the first months is about 1.5 percentage point, which is substantial given that only about 10 percent of individuals in the control group find a new job within the first month. As shown in column four to six, individuals in the treatment group also work more days in the short run. The ATET after one month indicates that treated individuals have worked, on average, 1.7 additional days. The ITT effect on working days is around 0.4, while average working days in the control group are 7.8. This shows that the impact on working days is smaller in relative terms than the effect on finding new work, which may arise because workers at temporary employment agencies often work fewer days compared to workers in regular employment. Again we find that effects are temporary. After six months, the estimated impact decreases to less than half of the initial effect and becomes insignificant. One year after the matching event, point estimates for both outcomes are close to zero. This confirms that the positive impact of matching events eventually disappears.

In the final three columns of Table 5, we report the impact on monthly earnings. The ATET coefficients show that 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 estimates on earnings are even negative but still insignificant. The absence

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[13]Recall that there is no one-to-one relation between leaving the benefits system and finding work due to the possibility of part-time UI benefits. Some individuals may find work with low pay or few working hours and then remain entitled to some UI benefits.

Table 5: Impact on employment and earnings

	New work spell			Working days			Monthly earnings		
	+1m	+6m	+12m	+1m	+6m	+12m	+1m	+6m	+12m
Intention-to-Treat Estimates (ITT)									
Invited	0.015**	0.006	0.005	0.393**	0.177	-0.057	38.508*	26.497	-21.871
	(0.007)	(0.010)	(0.010)	(0.190)	(0.200)	(0.183)	(21.713)	(25.172)	(25.855)
Treatment-on-the-Treated Estimates (ATET)									
Attended	0.065**	0.026	0.021	1.678**	0.755	-0.241	164.208*	112.991	-93.261
	(0.028)	(0.044)	(0.043)	(0.822)	(0.856)	(0.781)	(93.874)	(107.667)	(110.124)
Control group mean and standard deviation of outcomes									
Mean	0.10	0.44	0.60	7.83	11.23	10.23	785.38	1187.23	1298.27
Standard deviation	(0.30)	(0.50)	(0.49)	(9.40)	(10.00)	(9.91)	(1090.76)	(1241.75)	(1308.37)

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.

of positive long-run employment effects concurs with the findings of Cottier et al. (2015). Comparing income gains and income losses, we find that the sum of earnings and benefits are never significantly different from zero. To get a better picture of income changes, we analyze cumulative differences in earnings and benefit payments in Section 7.2.

Above, we mentioned that if unemployed workers find a new job with low earnings or few working hours compared to previous employment, then they may remain entitled to UI benefits. 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 status. Table 6 shows that the impact on working days is clearly driven by those who stop receiving benefits. The effect on working days with benefit receipt is three times smaller and not statistically significant. This shows that the additional working days due to participation in a matching event are often sufficient for unemployed workers to end the UI benefits spell.

Because unemployed workers meet with temporary employment agencies during the matching events, the most straightforward channel for job finding are vacancies mediated by the agencies. This may crowd out regular employment, but it can also act as stepping stone. Furthermore, matching events

Table 6: Impact on working days by benefit status

	Working days (with benefits)			Working days (without 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.

may activate unemployed workers, which can stimulate finding regular work, too. Table 7 summarizes the effects on the probability of finding a new job via a temporary employment agency and on the probability of finding regular employment. The estimation results show that the positive effects on employment can be explained to a similar extent by agency work and regular work arrangements. These results do not indicate any crowding out of regular employment due to participation in the matching events. Likewise there is no evidence that jobs mediated through temporary employment agencies serve in the longer run as stepping stone into regular employment.[14] This concurs with

[14]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 latest job before becoming unemployed (ITT $= -0.004$, *s.e.* $= 0.014$). In Table A.3 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.

the findings of De Graaf-Zijl et al. (2011) for the Netherlands.

Because treated job seekers are also more likely to start a regular employment spell, network-building and improved job search skills due to CV checks or feedback provided during the matching events might be other relevant mechanisms here.

To shed more light on differences between regular work and work via temporary employment agencies, we estimate treatment effects on working days and earnings separately for both types (see Table A.2 in the appendix). The results reveal that the increase in working days of treated individuals is mostly driven by regular employment. We also estimate lower positive effects on earnings for agency workers which cannot be fully attributed to the difference in working days. This confirms that the extent of work at temporary work agencies is more uncertain, and that wage rates are lower than in regular employment.

5.2   Heterogenous effects

Effects on job finding may differ between matching events and by characteristics of participants. The two main characteristics of matching events are the number of participants in an event and whether an event is general or sector specific. In Subsection 4.1, we showed that attendance rates are unaffected by the size of an event, but that attendance rates in the five sector-specific matching events are lower than in the 13 general matching events. As for attendance, we do not find evidence for a systematic relation between the size of a matching event and its effectiveness but we do observe differences by event type. The first column of the upper panel of Table 8 shows the impact on working days when we split the sample in general and sector-specific events. General matching events have a significant short-run impact, but this effect 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 effects never become significant.

The other panels in Table 8 show effects for different subgroups stratified by a number of individual characteristics. Previous research shows that the

Table 7: Impact on type of work

| | New work spell | | | | | |
| | Employment agency | | | Regular employment | | |
	+1m	+6m	+12m	+1m	+6m	+12m
Intention-to-Treat Estimates (ITT)						
Invited	0.009**	0.010	0.004	0.010*	0.003	0.010
	(0.004)	(0.008)	(0.009)	(0.006)	(0.010)	(0.010)
Treatment-on-the-Treated Estimates (ATET)						
Attended	0.037**	0.044	0.016	0.043*	0.013	0.045
	(0.017)	(0.032)	(0.037)	(0.024)	(0.042)	(0.044)
Control group mean and standard deviation of outcomes						
Mean	0.03	0.16	0.23	0.07	0.34	0.49
Standard deviation	(0.18)	(0.36)	(0.42)	(0.25)	(0.47)	(0.50)

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.

effectiveness of active labor market policies often differs between individuals (see Card et al., 2018). We find that men, younger individuals and those with short unemployment spells benefit in the short-run more from participating in a matching event. Individuals who have already been longer unemployed by the time of the event experience negative effects which even become marginally significant in the medium- and long-run. For education and remaining entitlement duration of UI benefits, we do not find any clear pattern of heterogeneous effects.

To check whether some of the obtained effect differences are driven by correlations between characteristics, we estimate their joint impact by including

Table 8: Heterogeneous effects - Working days (ATET estimates)

	Type of event		Gender		Age	
	General	Sector	Female	Male	< 40	≥ 40
After 1 month	1.913**	0.597	0.618	2.077*	5.224***	-0.212
	(0.913)	(1.810)	(1.228)	(1.082)	(1.768)	(0.867)
After 6 months	0.520	1.788	-1.872	2.360**	0.618	0.692
	(0.935)	(2.054)	(1.307)	(1.130)	(1.738)	(0.940)
After 12 months	-0.559	0.886	-2.014	0.858	0.064	-0.513
	(0.862)	(1.810)	(1.231)	(1.011)	(1.553)	(0.871)
Observations	9,877	2,733	4,468	8,142	5,857	6,753
	Unemployed		Education			Remaining UI duration	
	< 3 mo.	≥ 3 mo.	Low	Medium	High	< 3 mo.	≥ 3 mo.
After 1 month	2.804***	-1.284	0.772	1.473	2.882*	1.218	1.203*
	(0.992)	(1.384)	(1.838)	(1.140)	(1.535)	(4.034)	(0.672)
After 6 months	2.347**	-3.148*	1.836	-0.159	1.317	5.177	-0.078
	(1.005)	(1.629)	(1.991)	(1.187)	(1.553)	(4.283)	(0.763)
After 12 months	0.682	-2.426*	1.381	-0.604	-1.146	-3.044	-0.016
	(0.921)	(1.465)	(1.825)	(1.101)	(1.360)	(3.900)	(0.696)
Observations	8,804	3,806	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); * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

interaction terms of attendance and all characteristics in the ATET regressions. Even though effect sizes tend to be somewhat less pronounced, the estimation results, presented in the appendix (Table A.4), largely confirm the results above.

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).[15] 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.

Table 9 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*).[16] 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, more often married and better educated. Compliers worked fewer days in the three months before the matching event, but had higher earnings due to higher hourly wages. Furthermore, compliers more often had a permanent contract, but they did not collect more benefits in this period.

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. 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 coefficient 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 working days separately for both subsamples.

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[15]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.

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

Table 9: 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)	
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.

The estimation results in Table 10 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 to find new work. This suggests that the effectiveness of matching events can be increased by convincing unemployed workers who would usually not attend to start participating.

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

	After 1 month	After 6 months	After 12 months
Low propensity group (0.021 ≤ prop. score < 0.220)			
Attendance	5.762***	2.037	0.805
	(2.172)	(2.133)	(1.934)
Intercept	6.754	6.695	11.284
	(1.137)	(1.117)	(1.102)
$N = 6,305$			
High propensity group (0.220 ≤ prop. score < 1)			
Attendance	0.080	0.150	-0.710
	(0.772)	(0.844)	(0.773)
Intercept	9.352	10.884	14.547
	(1.554)	(1.699)	(1.557)
$N = 6,305$			

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

Our results show that unemployed workers are more likely to start a new job in the first weeks after participating in a matching event. This can be explained by a decrease in search frictions. If job seekers learn at the matching event about vacancies that they were not aware of, the job finding rate rises. Another possible explanation is that participants update their beliefs about temporary employment agencies and their prospects on the labor market, which can have an indirect effect on employment. We next use survey data to examine changes in job search behavior, which allows us to study the importance of both 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.[17] The five outcomes which we consider here correspond to those reported in Table 2.

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[17]Unweighted estimates are presented for comparison in the appendix (see Table A.6).

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

	# employment agencies registered	Job search motivation (1-5 scale)	# applications sent (last 4 weeks)	# job talk invitations (last 4 weeks)	Reservation wage (month, in euros)
Intention-to-Treat Estimates (ITT)					
Treatment	0.265*	0.114**	-0.235	0.034	-88.202**
	(0.150)	(0.055)	(0.203)	(0.057)	(37.295)
Treatment-on-the-Treated Estimates (ATET)					
Attendance	0.748*	0.322**	-0.664	0.096	-249.261**
	(0.422)	(0.156)	(0.575)	(0.162)	(105.528)
Control group mean and standard deviation of outcomes					
Mean	3.51	3.91	6.42	0.80	2281.59
SD	(3.03)	(1.08)	(4.06)	(1.19)	(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. ATET estimates involve IV regressions using treatment assignment as an instrument for attendance. All outcomes are measured 2-3 weeks after the matching event. If individuals already found work, all outcomes except for column (2) refer to the previous job search period. All regressions control for matching event fixed effects as well as a set of individual characteristics (gender, age, education); * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

As shown in the first column of Table 11, 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. 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.

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

ing 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. This can cause that they lower their requirements for accepting a job offer, which is reflected in reduced reservation wages.[18]

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 is smaller than 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 distribution between 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.[19] The results show substantial differences in the lower tail of the earnings distribution up to about 1,500 euros per month.[20] In particular, one month after the matching event, there is a higher share of workers with low earnings among the treated compliers. After six and 12 months, the lower tails of the earnings distributions of treated and non-treated compliers become similar.

Our survey results suggest that participants in the matching events update their beliefs about their labor market opportunities. This is consistent with the labor market effects obtained in the analysis of administrative data, which

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[18]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.

[19]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.

[20]Figure A.4 in the appendix shows the estimated earnings distributions of compliers one, six, and 12 months after the matching event (excluding the mass points for zero earnings).

showed that participating in a matching event increases job finding in the short-run but lacks medium- and long-run effects. Reducing the reservation wage makes unemployed workers less selective which has a positive effect on job finding in the short-run. Due to the increased job finding, there is a short-lived positive effect on monthly earnings. Individuals in the control group have higher reservation wages which leads to longer unemployment spells but somewhat higher earnings in the long-run.

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 the meetings of participants during a matching event, which allows to test for differences in performance by individual characteristics. We observe how many temporary employment agencies they met, and how long the meetings took. As shown in Table 12, participants 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. However, there is substantial variation between participants in the number and duration of meetings with temporary employment agencies.

Table 12 reports how these outcomes differ by observable characteristics of participants. The regressions show that female, older and medium and high-educated unemployed workers have, on average, significantly shorter meetings. Furthermore, women talk to significantly fewer temporary employment agencies during the meetings than men, and the probability of further contact with the agency declines with age. These differences concur to a large extent with our earlier findings on heterogeneous treatment effects, which showed that matching events are less effective for women and older unemployed workers.

Table 12: 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 Displacement effects

A concern in the evaluation of active labor market policies are potential displacement effects. Participants in the matching events may fill vacancies that would otherwise be filled outside the matching events, which would decrease job finding rates of other workers. On the other hand, matching events may reduce labor market frictions and either fill vacancies that would otherwise remain empty or fill these vacancies faster and at lower costs.

To examine the presence of displacement effects, we compare job finding rates in the control groups of matching events with different treatment assignment probabilities. If displacement effects exist, job seekers should do, on average, worse when the 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 A.3 in the appendix shows the share of individuals who work six months after the

matching events 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 A.7 in the appendix 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 the appendix (Table A.8), individuals indeed differ in terms of observable characteristics.[21] Benefit recipients eligible

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[21]Data on the full population are not as rich as data on the individuals in the experiment,

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 weight our estimates accordingly using inverse-probability weights.[22]

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 the working days after one month as outcome, we find a small and insignificant difference-in-difference coefficient (p-value $= 0.46$). This finding supports our assumption that matching events do not affect the job finding rate of unemployed workers who are not invited to matching events.
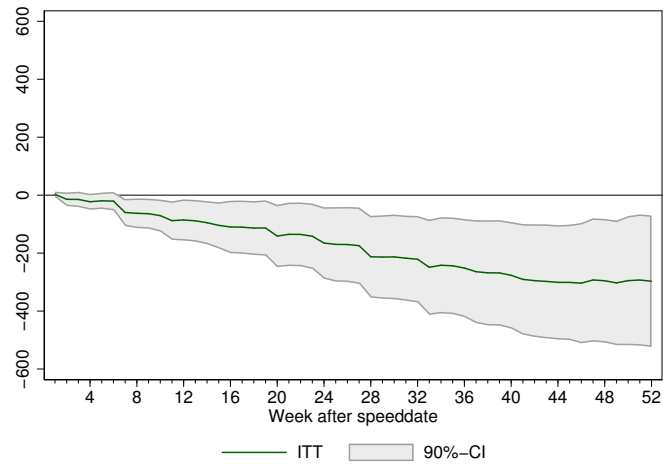
7.2 Cost-benefit analysis

To evaluate the cost effectiveness of matching events, we calculate the cumulative gains of the treatment and contrast these with the costs of organizing a matching event. The costs consist of the time investment made by caseworkers to set up a matching event, invite job searchers and temporary employment agencies, and host the event. Matching events take, in most cases, place at the local offices of the Dutch UI administration. The hourly personnel costs of a caseworker at the UI administration is about 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.

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and therefore contain fewer individual characteristics.

[22]The propensity score is estimated using the characteristics shown in Table A.8 and occupational-sector indicators. Figure A.5 in the appendix shows that the estimated propensity scores of the control groups and the added sample of benefit recipients have a large area of common support.

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

(a) Cumulative UI benefits
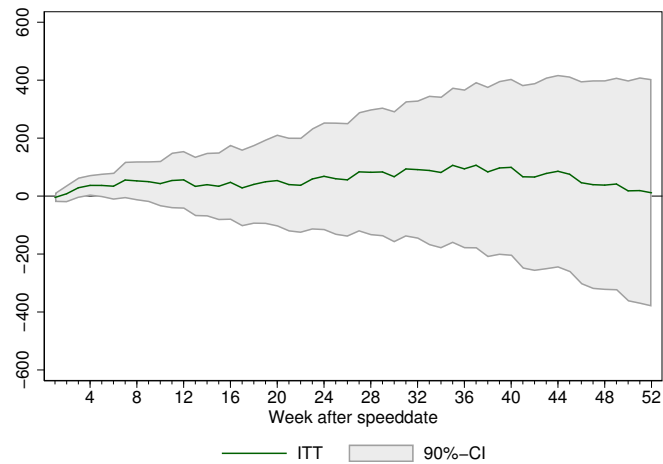
(b) Cumulative earnings

For the cost-benefit analysis, we use cumulative UI benefits and earnings up to one year after the matching events. Figure 3 plots ITT estimates for the cumulative outcomes by week after matching event. Corresponding point estimates measured after 12 months are reported in Table 13. The difference in UI benefits between treatment and control group steadily decreases to about

300 euros after nine to 10 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.[23]

Cumulative earnings increase only modestly in the nine months after the matching event, and the increase diminishes completely afterwards. The effect on 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 in the year after a matching event.

Table 13: Cumulative benefits, earnings and total income after one year (ITT)

	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 where unemployed workers get the opportunity to meet

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[23]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.

temporary employment agencies. Whereas other active labor market programs often rely on individual 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 job characteristics 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 organized 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 the strong short-run effects, differences between the 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 considerable heterogeneity with respect to the type of matching event and individual characteristics. The positive impact on the job finding rate 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 younger, male and higher educated individuals. However, unemployed workers with these characteristics are less likely to attend a matching event when being invited. This suggests that the effectiveness of matching events can be increased by convincing those individuals who are least likely to attend to actually participate in such an event.

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 do not compensate the reduced benefits 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.

Using survey data collected shortly after the matching events, we find that participating in an event significantly affects job search behavior. Participants become more motivated and decrease their reservation wage. The latter effect suggests that meetings with temporary employment agencies allow job seekers to update their beliefs about current prospects on the labor market. This can contribute to higher job finding rates and is consistent with our finding of lower re-employment wages. 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 results reveal the presence of substantial search frictions in the labor market for which matching events with temporary employment agencies can serve as a remedy in the short-run. However, our results 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).

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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%

Figure A.1: ATET estimates by week after matching event
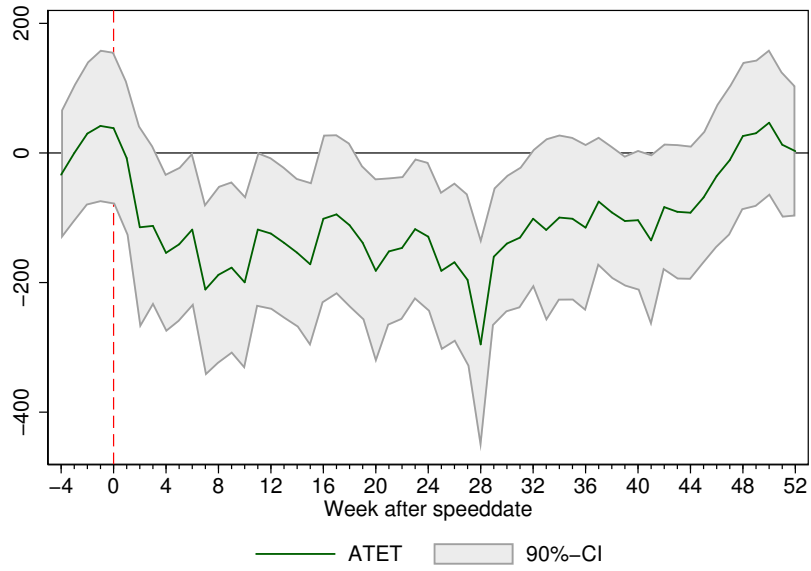
(a) Impact on monthly UI benefits (in euros)
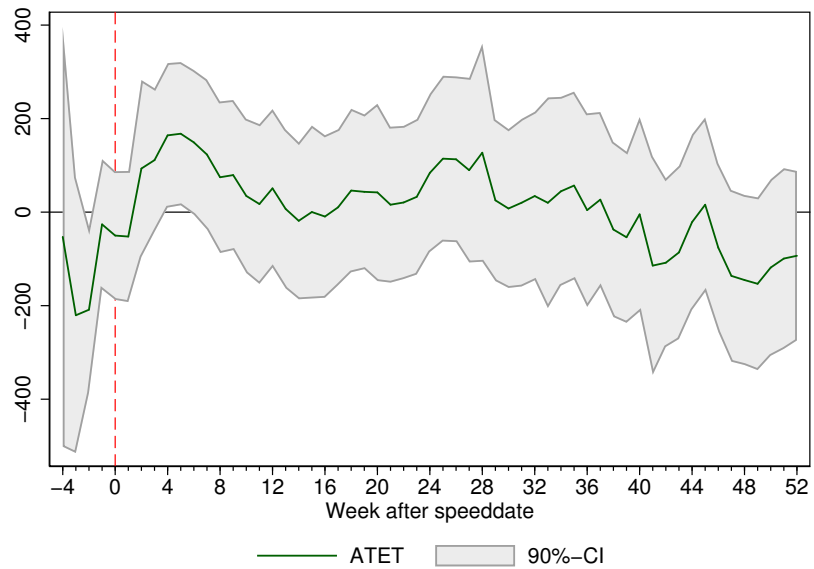
(b) Impact on monthly earnings (in euros)

Table A.2: Impact by type of contract

| | 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.3: 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	

Table A.4: Heterogeneous treatment effect regressions (ATET)

Outcome: Working days	After 1 month	After 3 months	After 12 months
Show up	5.125**	3.595	4.053
	(2.504)	(2.764)	(2.514)
× I(Sector-specific)	0.127	1.638	2.016
	(2.316)	(2.557)	(2.325)
× I(Female)	0.015	-3.927**	-2.212
	(1.576)	(1.741)	(1.583)
× I(Age ≥ 40)	-4.638***	1.018	-1.157
	(1.759)	(1.942)	(1.766)
× I(Unemployed ≥ 3 months)	-2.948	-6.131***	-3.831**
	(1.808)	(1.997)	(1.815)
× I(Medium education)	0.465	-1.672	-1.819
	(2.049)	(2.262)	(2.057)
× I(High education)	1.380	-0.734	-2.315
	(2.245)	(2.479)	(2.254)
× I(Rem. UI dur. < 3 months)	-3.019	4.236	-3.398
	(3.544)	(3.913)	(3.558)

NOTE – $N = 12,610$. I() denotes an indicator function. Estimates involve IV regressions using treatment assignment as an instrument for attendance. All characteristics above are included in the regressions along with their interaction terms. Furthermore, we control for matching event fixed effects, marital status and previous job characteristics (earnings, benefits, permanent contract, working days) measured in the three months before the matching event; * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

Table A.5: 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.6: Impact on job search behavior (unweighted survey data)

	# employment agencies registered	Job search motivation (1-5 scale)	# applications sent (last 4 weeks)	# job talk invitations (last 4 weeks)	Reservation wage (month, in euros)
Intention-to-Treat Estimates (ITT)					
Treatment	0.267**	0.057	-0.264*	0.016	-61.528*
	(0.131)	(0.046)	(0.160)	(0.046)	(36.094)
Treatment-on-the-Treated Estimates (ATET)					
Attendance	0.709**	0.152	-0.702	0.041	-163.528*
	(0.344)	(0.121)	(0.428)	(0.122)	(96.182)
Control group mean and standard deviation of outcomes					
Mean	3.42	3.97	6.34	0.74	2352.21
SD	(2.99)	(1.04)	(3.87)	(1.16)	(1130.82)

NOTE – $N = 2,888$. ITT estimates are obtained through OLS regressions. ATET estimates involve IV regressions using treatment assignment as an instrument for attendance. All outcomes are measured 2-3 weeks after the matching event. If individuals already found work, all outcomes except for column (2) refer to the previous job search period. All regressions control for matching event fixed effects as well as a set of individual characteristics (gender, age, education); * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

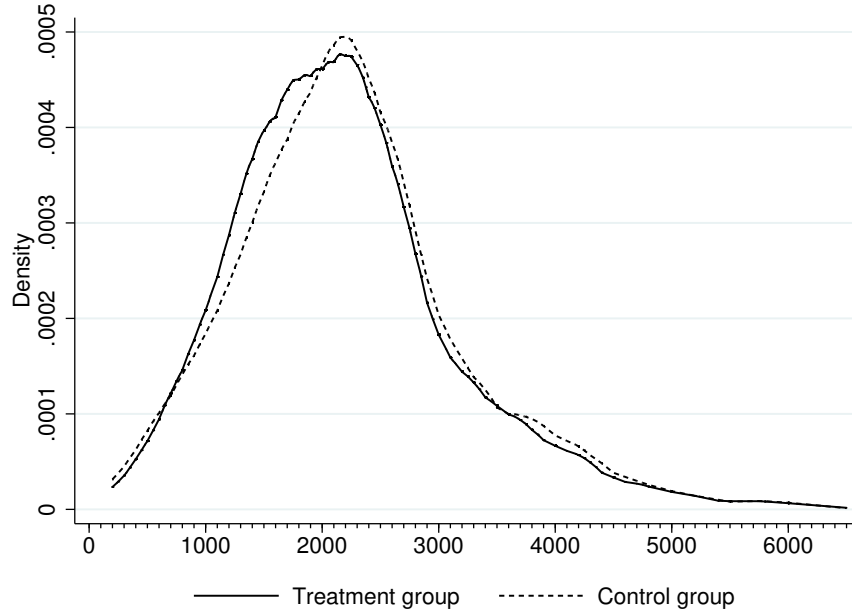Figure A.2: Reported monthly reservation wage (weighted)

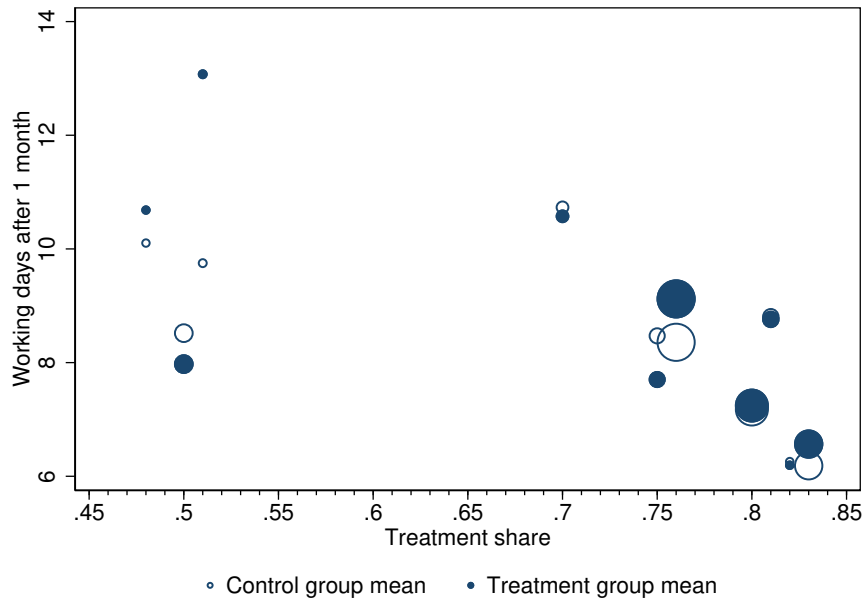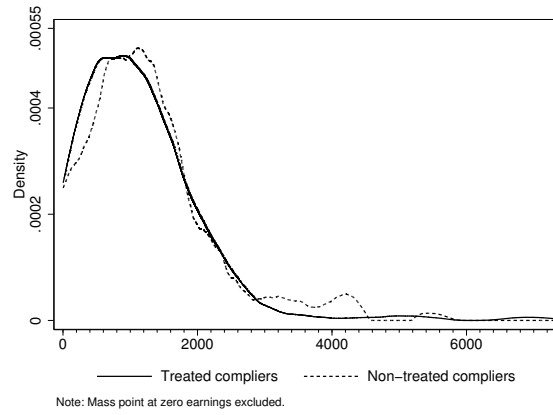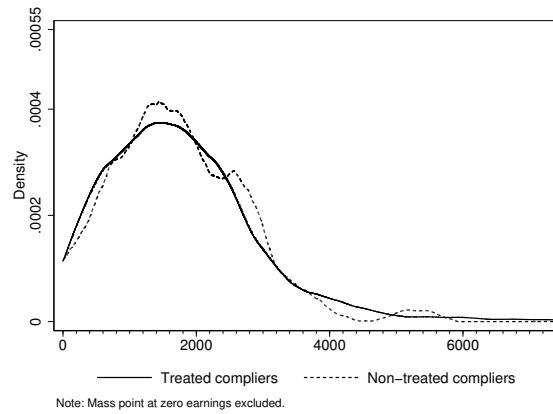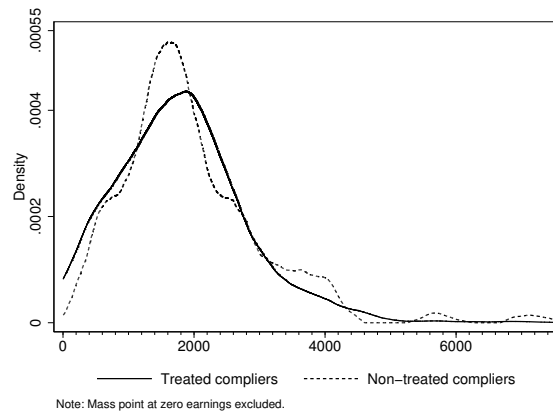Figure A.3: Working days after one month by treatment share

Figure A.4: Earnings distributions of compliers (Epanechnikov kernel)

(a) Earnings (in euros) after one month

(b) Earnings (in euros) after six months

(c) Earnings (in euros) after twelve months

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Table A.7: 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

Figure A.5: Common support of propensity score estimates
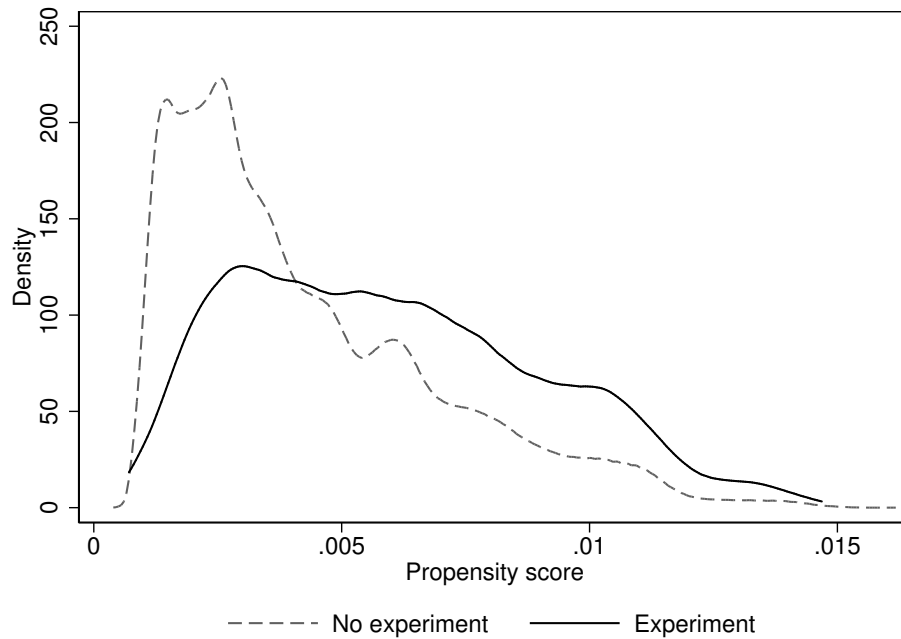
Table A.8: Comparison of control group and entire population

	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.

Extract online questionnaire (English translation)

<u>Introduction</u>

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 (<u>If already found work, asked retrospectively for last period of unemployment</u>)

- 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)