Empirical Evaluation of Broader Job Search Requirements for Unemployed Workers*

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Abstract

This paper exploits a large-scale field experiment where unemployed workers were randomly assigned to an additional caseworker meeting focussing on broader job search. The meeting significantly increases job finding and is cost effective. However, caseworkers differ in the rate at which they impose broader job search. We exploit this heterogeneity in caseworker stringency and the random assignment of unemployed workers to caseworkers to evaluate the broader search requirement. Our results show that imposing the broader search requirements reduces job finding. We argue that restricting the job search opportunities forces unemployed workers to search sub-optimally which negatively affects labor market outcome.

JEL-codes: J22, J64, J65, J68, C93

Keywords: Unemployment insurance, broader job search, caseworker stringency, caseworker meetings, field experiment

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1 Introduction

In a recent study Belot et al. (2019) advocate that unemployed workers are searching for work too narrowly. This has drawn attention from both policy makers and researchers, who are considering policies to stimulate broader job search. Indeed, an increasing number of OECD countries require unemployed workers to search and accept jobs beyond the occupation of their previous employment. The underlying idea is that unemployed workers have biased beliefs about their labor market prospects. In particular, they anchor their reservation wage on their previous wage and search too often for work that resembles their previous job (Krueger & Mueller, 2016; Mueller et al., 2021). Stimulating unemployed workers to search more broadly may then positively affect labor market outcomes and this would yield low costs to benefits administrations. However, Moscarini (2001) argues that only workers without comparative advantages should apply broadly for jobs while specialized workers should search narrowly.

In this paper, we empirically evaluate a program that enforces the requirement that unemployed workers search broadly for work. Individuals who have been collecting unemployment insurance (UI) benefits for six months are invited for a caseworker meeting to discuss job search strategies. When the caseworker concludes that the unemployed worker applies mainly for a narrow set of vacancies, she can give the unemployed worker a task to broaden the job search.[1] The unemployed worker is obliged to complete this task and this is monitored by the caseworker. In practice, it means that the unemployed worker should actively apply for jobs that are in different sectors, may have a longer commuting distance, offer a lower wage and may require a lower level of education.

For the empirical evaluation we use data from a large-scale field experiment conducted at the Dutch UI administration. A random subsample of about 130,000 unemployed workers has been invited to the caseworker meeting on job search strategies. We use this random assignment to estimate the causal effects of having the

[1]The Dutch law allows benefits recipients to only apply for jobs that meet their qualifications during the first six months of UI. After these initial six months benefits recipients are obliged to broaden their search to jobs that have lower requirements than their qualifications.

additional caseworker meeting. During this meeting the caseworker has the discretion to impose the broader search task on the unemployed worker. To estimate the causal effect of this broader search task we exploit that within local UI offices unemployed workers are randomly assigned to caseworkers and that there is substantial variation between caseworkers in the rate of imposing the broader search task. The identifying assumption is that if caseworkers differ in other dimensions that are important for supporting unemployed workers, these dimensions are orthogonal to the rate at which they impose the broader search task.

Our identification of the broader search task relates to the literature using judge stringency as instrumental variable. Kling (2006), Aizer & Doyle Jr (2015), Doyle Jr (2007, 2008) and Bhuller et al. (2020) use the random assignment to judges to estimate the effects of judge decisions on various socioeconomic and crime outcomes. Maestas et al. (2013) and French & Song (2014) use the assignment to an examiner to show that receipt of disability insurance benefits reduces labor supply. Most closely related to our approach is Arni & Schiprowski (2019), who use caseworker assignment to evaluate the relevance of job search requirements for unemployed workers. They consider a setting where caseworkers meetings occur more frequently (monthly) and search requirements can change between meetings. We study a setting with much less interaction between the caseworker and unemployed worker, which increases the plausibility of the validity of the empirical design.

Our paper contributes to the recent literature on broader job search requirements and to the literature on search requirements and active labor market programs. Within an online environment Belot et al. (2019) have randomly provided job seekers with additional vacancies to stimulate them to search more broadly. They find that this broader search encouragement increases the incidence of job interviews particularly for job seekers who initially searched narrowly. Altmann et al. (2018) randomly distributed an information brochure – with information about job search strategies and consequences of unemployment – among unemployed workers who are at risk of long-term unemployment and find that recipients of the brochure are more likely to find work. Skandalis (2019) shows that when the media announces intended hiring by plants, the composition of job applicants changes to individuals living further away. These studies show that job seekers benefit from the broader search induced by the information provision. The program we study in this paper has the same goal of stimulating broader job search, but as a formal policy it makes broader job search compulsory. This may imply that unemployed workers are restricted in their job search behavior and, therefore, are forced to search sub-optimally. We show within a simple job search model the potential effects of imposing the broader search task. The model shows that if unemployed workers do not have biased belief, the broader search task may stimulate job finding if narrow and broad search are close complements or do not differ substantially in their effectiveness. This coincides with Moscarini (2001) who argues that broader search is mainly useful for workers without comparative advantage.

Our paper further relates to a relatively extensive literature on caseworker meetings, and imposing and monitoring job search requirements. Recent studies by Maibom et al. (2017) and Schiprowski (2020) show non-negligible effects of caseworker meetings. They consider regular caseworker meetings, while we study a single meeting focussing on broader job search. The literature shows that additional job search requirements shorten the period of unemployment (Johnson & Klepinger, 1994; Klepinger et al., 2002; Lammers et al., 2013; Arni & Schiprowski, 2019). In our case the number of required job applications remains unaffected, but unemployed workers should also apply to jobs that are less closely related to their previous job. Finally, the caseworker meeting evaluates if the unemployed worker makes enough job applications and if these are already sufficiently broad. The caseworker meeting thus also contains an element of monitoring. The evidence on the effectiveness of job search monitoring is mixed (Van den Berg & Van der Klaauw, 2006; Petrongolo, 2009).

In the empirical analysis we use administrative data provided by the Dutch UI administration on all participants in the randomized experiment. Our evaluation of the experiment shows that the broader search program shortens the unemployment duration. We next exploit that unemployed workers are randomly assigned to caseworkers and that the rate at which caseworkers impose the broader search task is unrelated to other types of assistance. We find that imposing the broader job search task reduces the effect of the program, i.e. job finding is reduced after the broader search task. Even though being imprecisely estimated, marginal treatment effects suggest that broader search task are most often imposed on unemployed workers for whom the adverse effects are largest. Finally, we provide a decomposition of the effect of the broader search program in an effect of the broader search task and an effect of the meeting. This decomposition takes into account that groups of compliers differ when evaluating the program and the task. Our results differ from previous studies that often found positive effects of stimulating broader search. This shows the limitations of incorporating a broader search requirement in a formal (low-cost) policy.

The remainder of the paper is organized as follows. In the next section, we describe the Dutch UI system, the broader search policy, and the design and implementation of the experiment. Section 3 contains a description of the data and shows an evaluation of the broader search program. In section 4 we provide our empirical framework to estimate the effects of imposing the broader search task and we justify the use of caseworker stringency as instrumental variable. Section 5 presents some theoretical predictions of imposing the broader search task and shows the estimated effects as well as a decomposition of the program effects in an effect of caseworker meeting and the broader search task. Finally, section 6 concludes.

2 Background of the experiment

In this section we first provide a brief description of the Dutch UI system. Next, we discuss the content of the broader search program and finally we give some details on the experiment.

2.1 The Dutch UI system

In the Netherlands, the UI system insures workers against loss of working hours. If an individual worked 26 of the previous 36 weeks and loses at least five working hours, the individual becomes entitled to UI benefits. During the first two months of UI the benefits level is 75% of the previous wage (capped at a maximum) and after that it becomes 70%. All eligible individuals are entitled to at least three months of UI benefits. The entitlement period to UI benefits depends on the work history.[2]

While collecting UI benefits, workers are obliged to (*i*) attend meetings with caseworkers when being invited, (*ii*) make at least one job application each week, and (*iii*) accept suitable job offers. During the first six months of UI, a job is considered suitable when it is in line with the worker's educational level, experience and previous wage. After these six months all jobs are considered suitable. During the first year of UI workers have three meetings with caseworkers, in the fourth, the seventh and the tenth month.

The meeting in the seventh month is affected by the experiment described in this paper. The meeting is eliminated for untreated individuals, while for treated individuals this meeting focuses on broader job search. The untreated individuals have the same (broader) search obligation, but since they do not receive the invitation letter and do not have the meeting this is less actively communicated.

2.2 The treatment

In 2015 the UI administration introduced a program to stimulate broader job search of workers who were collecting benefits for six months and for whom thus all jobs are considered suitable. Towards the end of the sixth month of UI, individuals receive a letter inviting them for the meeting with a caseworker in the seventh month of UI. This letter explains that the UI spell is approaching six months and that, therefore, the worker should apply for a broad set of jobs, including jobs requiring lower levels of education, in other sectors, with longer commuting times and lower wages than the previous job. The letter states that the purpose of the mandatory caseworker meeting is to discuss future job search strategies. The unemployed worker should bring two suitable vacancies, a curriculum vitae, past applications and the reactions of employers on these applications to the meeting. The untreated individuals do not receive the invitation letter for the caseworker meeting.

During the meeting the caseworker reviews the recent job applications. If the

[2] De Groot & Van der Klaauw (2019) provide a more extensive discussion on the Dutch UI system.

caseworker assesses the recent job search as narrow, the caseworker should give the unemployed worker a task to search more broadly. As a start of the task, the caseworker often provides two vacancies that are considered broader to which the unemployed worker must apply. When the broader search task is imposed, this is registered. Fulfilling the task is then an obligation and compliance can be evaluated in the subsequent months.

To summarize, the broader search program involves an invitation letter emphasizing the broader search obligation after six months of unemployment, a meeting with the caseworker evaluating the past job search and possibly a task for the unemployed worker to apply for jobs more broadly. Unemployed workers who are not subject to the program do not receive the letter, do not have the meeting and, therefore, cannot get the broader search task.

2.3 The experiment

The UI administration organized a randomized experiment with the intention to evaluate the broader search program. Excluded from the experiment are individuals who were older than 50 years and individuals who were entitled to less than ten months of UI benefits. A random subsample of the eligible workers who are approaching six months of benefits receipt between April 2015 and March 2017 were invited for the caseworker meeting discussing the broader search requirement. The randomization was organised such that individuals with one specific final digit of their social security number were not receiving the invitation letter for the caseworker meeting. Therefore, 10% of the eligible unemployed workers are assigned to the control group and the other 90% to the treatment group. Individuals who attend the meeting are assigned to a caseworker in their local office of the UI administration. The assignment is based on the current caseload of the caseworker, i.e. each unemployed worker is assigned to a caseworker with a caseload below the maximum caseload. In practice this often means that unemployed workers are assigned to the caseworker in their local office with the lowest caseload.

Table 1 shows for the treatment and control group how often they meet their caseworker in the period before the experimental intervention (1-23 weeks), during

Table 1: Caseworker services received by the treatment and control group

	Treatment group	Control group	p-value
	(1)	(2)	(3)
Panel A: Services between 1-23 weeks			
Caseworker meeting	78.7%	79.1%	0.31
Contact by phone	0.9%	0.8%	0.80
Online contact	1.4%	1.7%	0.02
Panel B: Services between 24-36 weeks			
Caseworker meeting	62.4%	25.9%	0.00
Contact by phone	3.0%	9.0%	0.00
Online contact	12.0%	36.4%	0.00
Broader search task	43.1%	4.5%	0.00
Panel C: Services 37 weeks and later			
Caseworker meeting	17.6%	16.5%	0.00
Contact by phone	3.8%	4.0%	0.17
Online contact	24.7%	27.3%	0.00
Number of workers	118,697	13,420	

Note: The p-values in column (3) apply to t-tests of different means for the treatment and control group.

the experimental intervention (24-36 weeks) and after the experimental intervention (37 weeks and later). The population describes unemployed workers who have been collecting benefits for at least six months and thus entered the experiment. During the first 23 weeks of unemployment both in the treatment and control group about 80% of the individuals met their caseworker. Contact by phone or online contact is very rare in this period. After the randomization about 62% of the individuals in the treatment group and 26% of the individuals in the control group had a meeting with their caseworker. The individuals in the control group have much more often online contact or contact by phone. About 43% of the individuals in the treatment group and less than 5% of the individuals in the control group get a broader search task from their caseworker. This shows that the randomization actually affected the services provided to individuals, but compliance to the randomization is not perfect. The noncompliance is mainly caused by caseworkers not inviting individuals for a meeting, individuals that receive an invitation letter generally attend the caseworker meeting.[3] Panel C shows that after the period affected by the experiment, differences in services provided to the treatment and control group are modest.

3 Data and experimental evaluation

In this section we first provide a description of the data. Next, we consider the randomized experiment and show that participation in the broader search program significantly increases exit from UI.

3.1 Data description

For the empirical analysis we use administrative data available at the Dutch UI administration. Our sample contains all 132,177 individuals who participated in the randomized experiment. This means that they entered UI between October 2014 and September 2016 with a benefits entitlement period of at least ten months. In addition, they collected UI benefits for at least six consecutive months and were at that moment younger than 50 years. Individuals that previously worked as teacher or for the government are excluded as well as individuals participating in an entrepreneurship program.[4]

For each individual in the experiment sample, we observe if the individual was assigned to the treatment or control group, whether the individual attended the caseworker meeting during the seventh month of UI, the identity of the caseworker and whether a broader search task was imposed. In addition we observe for all individuals information on the UI spells (start and end date, monthly benefits payments and re-integration activities), employment contracts (start and end date, monthly earnings and working hours, type of contract and sector) and personal characteristics such as the date of birth, gender, nationality and level of education. We use the data to construct for all individuals a labor market history for the 32 months after

[3]Letters are imprecisely registered in our data, e.g. often there is no identifier of the content of the letter. So we cannot always determine exactly when the invitation letter for the caseworker meeting was send.

[4]Teachers and civil servants are covered by separate UI schemes.

starting collecting UI benefits.

Table 2: Descriptive statistics, balancing and compliance to the experiment

	Explanatory variables		Dependent Variables			
			Treatment group		**Meeting**	
	Mean	Standard Deviation	Coefficient Estimate	Standard Error	Coefficient Estimate	Standard Error
	(1)	(2)	(3)	(4)	(5)	(6)
Demographics						
Age	39.41	(6.39)	0.0000	(0.0002)	0.003***	(0.000)
Female	0.571	(0.495)	-0.0001	(0.0020)	0.032***	(0.003)
Dutch nationality	0.954	(0.210)	0.0085**	(0.0041)	0.016**	(0.007)
Low educated	0.176	(0.381)	–		–	
Middle educated	0.518	(0.500)	0.0025	(0.0023)	0.018***	(0.004)
High educated	0.307	(0.461)	-0.0014	(0.0028)	-0.001	(0.005)
Previous employment and benefit eligibility						
Monthly earnings (€)	2,325	(1,098)	0.0018	(0.0012)	0.008***	(0.002)
Hours per week	31.55	(9.10)	0.0000	(0.0001)	-0.001***	(0.000)
Maximum entitlement (in weeks)	87.63	(28.36)	0.0000	(0.0001)	-0.001***	(0.000)
Employed at 6 months UI	0.296	(0.457)	-0.0015	(0.0021)	-0.159***	(0.003)
Sector last job						
Financial	0.234	(0.424)	0.0037	(0.0034)	-0.002	(0.005)
Retail and trade	0.195	(0.396)	-0.0010	(0.0035)	0.020***	(0.006)
Health care	0.191	(0.393)	-0.0010	(0.0036)	-0.006	(0.006)
Temporary employment	0.088	(0.283)	0.0058	(0.0040)	-0.032***	(0.006)
Industrial	0.086	(0.281)	–		–	
Transport	0.057	(0.232)	-0.0022	(0.0045)	0.009	(0.007)
Other	0.149	(0.356)	0.0034	(0.0036)	-0.017***	(0.006)
F-statistic for joint significance			1.40		93.48	
[p-value]			[0.138]		[0.000]	
Number of workers = 132,177						

Note: OLS estimates of regressing assignment to the treatment group (column (3)) and attending the caseworker meeting (column (5)) on worker characteristics. Robust standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Columns (1) and (2) of Table 2 show for the different characteristics the sample means and the standard deviations, respectively. Individuals are, on average, slightly younger than 40 years, 57% are female and almost 95% have the Dutch nationality. The mean level of education is relatively high, 31% have a high education (university or college), 52% have a middle education (higher vocational or high school) and about 18% low education (lower vocational or primary education). Before entering UI, the average monthly earnings was 2,325 euros and individuals worked, on average, almost 32 hours per week. At the start of UI the average benefits entitlement period is almost 88 weeks. After six months of UI benefits almost 30% of the individuals have some employment and are thus collecting UI part-time.[5] Most individuals in the experiment entered UI after having worked in the financial sector, retail and trade, or the health care sector.

Table 2 shows that the treatment and control group are balanced. Column (3) presents the results of regressing assignment to the treatment group on the individual characteristics, and the standard errors are in column (4). There is only a significant effect of having the Dutch nationality on being assigned to the treatment group. Having another nationality is very rare in our sample and the size of the difference is small. All results are robust against using a sample of only individuals with the Dutch nationality. For all other characteristics we do not find any significant difference between the treatment and control group. The F-test at the bottom of the table shows that jointly all characteristics do not have a significant effect on the assignment to the treatment or control group.

Column (5) of Table 2 shows which characteristics predict the incidence of attending the caseworker meeting. This is informative on how compliance to the treatment differs between individuals.[6] The strongest effect is that individuals who have some part-time employment after six months of UI, so at the moment of the invitation, are less likely to attend the caseworker meeting. There are also some other characteristics that affect the likelihood that a worker will attend the caseworker meeting and all estimated covariate effects are jointly significant. This indicates that there is selection in which individuals meet the caseworker. The (limited) information available from the invitation letters seems to suggest that the selection is mainly induced by local offices not scheduling a meeting with all unemployed workers rather than the behavior of the worker, who may succeed in canceling the meeting.

[5]The Dutch UI system compensates loss of weekly working hours. A worker can enter UI when losing part of the working hours and remaining working for the other part. Furthermore, when a UI recipients finds a part-time job with fewer working hours than the UI entitlement, the workers remains collecting UI benefits for the remaining hours.

[6]The regression uses the full sample. The estimation results are unaffected when only considering the treatment group.

3.2 Evaluating the broader search program

The randomized experiment evaluates the broader search program that starts with the caseworker meeting. The program can also contain the broader search task (when imposed during the caseworker meeting) and the monitoring of compliance to this task. Above it was shown that there is partial compliance to the random assignment to the treatment and control group. To deal with the partial compliance we use instrumental variable estimation. We specify the following regression equation for outcome Y_i observed for worker i,

$$Y_i = \alpha + \delta M_i + X_i'\beta + \varepsilon_i \tag{1}$$

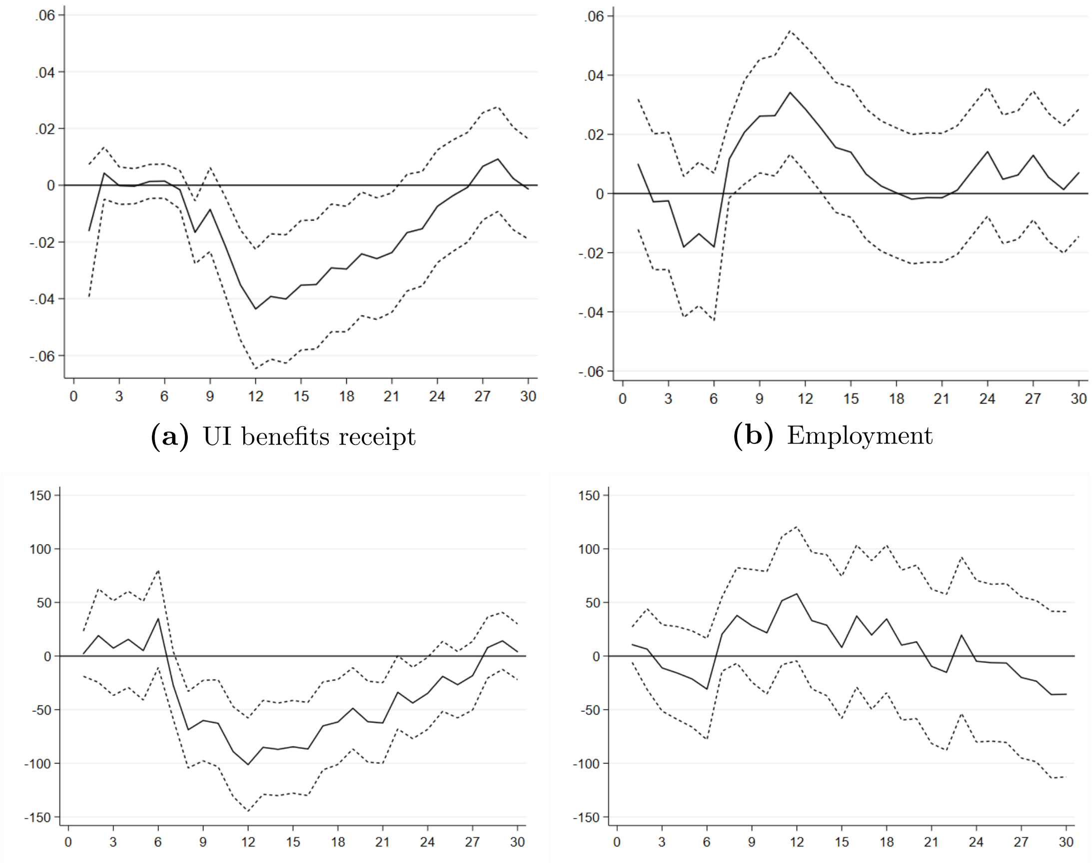
The variable M_i indicates attendance of the caseworker meeting, so our parameter of interest δ describes the effect of participating in the broader search program. The effect also includes that attending the caseworker meeting can result in a broader search task, which may change job search behavior. The vector X_i contains all characteristics described in Table 2.

The initial assignment to the treatment group T_i is used as instrumental variable for attending the caseworker meeting. This provides the first-stage equation

$$M_i = \kappa + \gamma T_i + X_i'\phi + \nu_i \tag{2}$$

The randomization ensures that initial assignment is orthogonal to (unobserved) individual characteristics. To use initial assignment as instrumental variable it is also required that there are no other pathways in which initial assignment can affect outcomes. In practice, this requires that the invitation to the caseworker meeting should not directly affect outcomes. Recall that this is not the first meeting with a caseworker during the UI spell and it is a single not very time-consuming meeting. Therefore, we argue that behavioral responses to the invitation letter are unlikely. We also do not find differences in outcomes between the treatment and control group in the month at which the invitation letters were sent. The first-stage regression shows that the estimate for γ is 0.366, and the F-test statistic equals 8,213. The instrumental variable is very strong and when assuming monotonicity about 36.6% of the population in the experiment are compliers.[7] The parameter δ should be interpreted as the causal effect for these compliers.

Figure 1: Effects of participating in the broader search program - instrumental variable estimates



(a) UI benefits receipt
(b) Employment
(c) Amount of UI benefits
(d) Earnings

Note: The estimated effects are based on regressions including controls for age, gender, nationality, education, previous wage, sector and working hours, and UI benefits eligibility. $N = 132,117$ for all estimated effects in all panels. The 95% confidence interval are based on robust standard errors. $t = 0$ is the start of collecting UI, the broader search program starts with a caseworker meeting in the seventh month of UI.

Figure 1 presents the effects of enrolling in the broader search program for four outcomes for each month since starting collecting UI. Recall that the program starts with a meeting in the seventh month of UI and none of the outcomes shows any significant effect for the earlier period. Enrolling in the broader search program

[7] In the control group 25.9% of the individuals attend a caseworker meeting (always takers), and in the treatment group 37.6% do not attend the caseworker meeting (never takers).

Table 3: Effects of participating in the broader search program on cumulative outcomes - instrumental variable estimates

Dependent variable:	Weeks of collecting UI	UI Benefits	Weeks of employment	Earnings
	(1)	(2)	(3)	(4)
18 months after start UI	-1.41***	-879***	0.90**	379
	(0.34)	(174)	(0.43)	(291)
Dependent mean	36.76	10,357	28.24	11,635
30 months after start UI	-1.84***	-1,202***	1.15	265
	(0.60)	(264)	(0.85)	(630)
Dependent mean	51.11	13,893	64.06	29,050
Number of workers	132,117			

Note: All regressions include controls for age, gender, nationality, education, previous wage, sector and working hours, and UI benefits eligibility. Robust standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

significantly reduces UI benefits receipt, after 12 months the fraction of individuals collecting UI benefits is reduced by about four percentage points (graph (a)). The effect on employment mirrors the effect on receiving UI, but after 12 months the effect declines slightly faster (graph (b)). After 18 months of UI the program effect disappears, so the program stimulates individuals to find work faster but the nonparticipants catch up later.

The bottom two graphs of Figure 1 show that the increased exit from UI also reduces the average monthly UI benefits payments. This effect is already significant in the eighth month and increases to about €100 less in benefits payments in the twelfth month. The effect on earnings is about half the effect on benefits payments and is never significant. This means that the reduced benefits receipt is, on average, not compensated by increased earnings from work.[8]

Table 3 shows the estimated effects on cumulative outcomes 18 and 30 months after starting collecting UI, so about one and two years after the start of the program.

[8]Our data do not contain information on self-employment and other social insurance or welfare schemes. However, our target population is not eligible for other benefits schemes and exit to self-employment is minor and often not affected by labor market policies (De Groot & Van der Klaauw, 2019).

These estimates confirm the earlier findings. The largest part of the program effects are in the year after the start of the program. In that period the UI period is reduced by, on average, 1.4 weeks and total benefits payments are 880 euros lower. The program is very cost effective for the UI administration, who estimate that the costs of offering the program are about €169 per invited individual.[9] The additional earnings for the participants are almost half of the reduction in UI benefits payments and insignificant. The increase in weeks of employment is also less than the reduction in weeks of UI.

4 Broader search task

In the previous section we showed that participating in the broader search program stimulates the exit from UI and increases job finding. The two key elements of the program are a meeting with a caseworker and the broader search task. Caseworker meetings not always result in a broader search task. There is ample empirical evidence that attending a caseworker meeting positively affects job finding (Card et al., 2010; Schiprowski, 2020; Maibom et al., 2017). There is much less evidence on mandating unemployed workers to search more broadly for work. In this section we present our empirical approach to estimate the effects of the broader search task.

4.1 Empirical approach and data

During the meeting the caseworker assesses the job search behavior of the unemployed worker. If the caseworker considers the search behavior as too narrow, the caseworker can give the unemployed worker a task to search more broadly. We exploit that within local offices of the UI administration unemployed workers are randomly assigned to caseworkers and that caseworkers differ in the rate at which they impose the broader search task. Our instrument variable approach is inspired by Bhuller et al. (2020), who use judge stringency as instrumental variable for incarceration and Arni & Schiprowski (2019) who use caseworker stringency as in-

[9] Figure A.1 in appendix A provides a back-of-the-envelope calculation showing how the program costs compare to intention-to-treat effects of the program.

strumental variable for required job search effort. For the empirical analysis, we use unemployed workers that attended the caseworker meeting, because only these individuals have a caseworker and only for them it is observed whether or not they received a broader search task. Furthermore, we restrict the sample to individuals who were assigned to the treatment group. This implies that we do not rely on variation induced by the randomized experiment. Individuals in the control group who attended a caseworker meeting are always takers in the randomized experiment. As will be discussed below excluding these individuals allows for a more straightforward interpretation and is necessary for the decomposition in subsection 5.4.

We use the following regression equation to model how the outcome Y_{ic} of unemployed worker i at local office c of the UI administration depends on whether or not a broader search task B_{ic} has been imposed,

$$Y_{ic} = \alpha_c + \delta B_{ic} + X_i'\beta + \varepsilon_{ic} \tag{3}$$

The parameters α_c are the fixed effects for the local office at the moment that the unemployed worker attends the caseworker meeting.[10] The vector X_i includes the characteristics discussed in Table 2. The parameter of interest δ describes the effect of the broader search task in addition to the caseworker meeting.

Caseworkers impose the broader search task when they believe that the unemployed worker focuses her job search activities too narrow. The broader search task is thus imposed on a selective subsample of unemployed workers who attend a caseworker meeting and the selection depends on unobserved job search behavior. However, caseworkers may assess job search behavior differently, which introduces exogenous variation in imposing the broader search task. An unemployed worker who receives a broader search task during the caseworker meeting, might not have received this task if she would have been assigned to another caseworker in the same local office (or vise versa). In accordance with earlier studies we refer to the rate at which a caseworker imposes the broader search task as the stringency of the caseworker (e.g. Kling, 2006; Maestas et al., 2013; Aizer & Doyle Jr, 2015; Bhuller et al.,

[10] In the estimation we interact fixed effects for the local offices with calendar month to take account that the pool of caseworkers within a local office may change over time.

2020).

Because within a local office unemployed workers are randomly assigned to a caseworker, whether or not an unemployed worker receives a broader search task depends on the stringency of her caseworker. This provides the first-stage regression equation

$$B_{ic} = \gamma_c + \lambda Z_{j(i)c} + X_i'\theta + \nu_{ic} \tag{4}$$

The instrumental variable $Z_{j(i)c}$ describes the stringency of caseworker $j(i)$ who is assigned to unemployed worker i. To compute the caseworker stringency faced by the unemployed worker, we use the leave-out mean. This implies that we consider all other unemployed workers assigned to caseworker $j(i)$ (excluding unemployed worker i) and take the fraction that received a broader search task in this group. The leave-out mean is also used by Maestas et al. (2013); Aizer & Doyle Jr (2015); Bhuller et al. (2020).[11]

To estimate the model, we require information on the identity of the caseworker and whether or not a broader search task has been imposed. We can thus only use the sample of unemployed workers that actually attended the caseworker meeting. To keep the sample representative for the usual unemployed workers that would attend the caseworker meeting, we exclude workers in the control group of the experiment that attended the caseworker meeting.[12] In addition, we restrict the sample further to workers who attended a meeting with a caseworker who met with at least 50 and at most 400 unemployed workers during the experiment period.[13] After applying the sample selection criteria, the data include 42,605 workers and 461 caseworkers. Each of the 36 local UI offices has, on average, 13 caseworkers over the experimental period and each caseworker in our sample met about 92 unemployed workers participating

[11] Bhuller et al. (2020) argue that both the leave-out mean and the split-sample estimator perform well when the number of cases per judge is large enough. However, the split-sample estimator substantially reduces the sample. We perform the split-sample estimator as robustness check.

[12] In the control group only the always takers in the randomized experiment attend the caseworker meeting, while in the treatment group both the always takers and the compliers attend the caseworker meeting. Including the attendants in the control group would bias the sample towards always takers.

[13] The minimum of 50 is imposed to obtain a reliable estimate for caseworker stringency. The maximum of 400 is used to exclude a few managers who register workers to themselves before assigning them to caseworkers.

Table 4: Descriptive statistics, assignment of caseworker stringency and the broader search task

	Explanatory variables		Dependent variables			
			Caseworker stringency		**Broader search task**	
	Mean	Standard Deviation	Coefficient Estimate	Standard Error	Coefficient Estimate	Standard Error
	(1)	(2)	(3)	(4)	(5)	(6)
Demographics						
Age	39.47	(6.33)	0.0002	(0.0002)	0.002***	(0.001)
Female	0.592	(0.492)	0.0020	(0.0016)	0.051***	(0.005)
Native	0.960	(0.196)	-0.0010	(0.0031)	0.038***	(0.012)
Low educated	0.166	(0.372)	–		–	
Middle educated	0.520	(0.500)	0.0023	(0.0016)	0.044***	(0.006)
High educated	0.314	(0.464)	0.0040*	(0.0022)	0.064***	(0.007)
Previous employment and benefit eligibility						
Wage (€)	2,337	(1,094)	-0.0005	(0.0007)	0.001	(0.003)
Hours per week	31.28	(9.09)	0.0000	(0.0001)	0.000	(0.000)
Maximum entitlement	87.83	(28.03)	0.0000	(0.0000)	0.001***	(0.000)
Employed at 6 months	0.237	(0.425)	-0.0026*	(0.0016)	-0.160***	(0.006)
Sector last job						
Financial	0.244	(0.430)	0.0021	(0.0028)	0.012	(0.008)
Retail and trade	0.202	(0.402)	0.0011	(0.0027)	0.026***	(0.009)
Health care	0.194	(0.396)	0.0016	(0.0027)	-0.005	(0.009)
Industrial	0.087	(0.282)	–		–	
Temporary employment	0.079	(0.266)	0.0056*	(0.0031)	-0.040***	(0.011)
Transport	0.054	(0.227)	0.0007	(0.0035)	-0.023**	(0.012)
Other	0.139	(0.346)	0.0026	(0.0026)	-0.006	(0.009)
F-statistic for joint significance			1.32		89.35	
[p-value]			[.181]		[.000]	
	Number of workers = 42,605			Number of caseworkers = 461		

Note: OLS estimates of caseworker stringency (column (3)) and imposing the broader search task (column (5)) on individual characteristics. All regressions include controls for local office fixed effects interacted with month fixed effect. Standard errors are robust and clustered at the caseworker level. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

in the randomized experiment. Column (1) of Table 4 shows the summary statistics of the subsample that we use to evaluate the broader search task. In terms of observed characteristics this sample is very similar to the full sample of participants in the experiment (see for comparison column (1) of Table 2).

4.2 Justification of the assumptions

The instrumental variable approach relies on three key assumptions, i.e. independence, exclusion restriction and relevance. Furthermore, for the interpretation of the estimates often a monotonicity assumption is made. Below we discuss the validity of these assumptions in our setting.

The validity of the instrumental variable approach relies on both an independence assumption and an exclusion restriction. The (conditional) independence of the instrumental variable is guaranteed by the random assignment of unemployed workers to caseworkers within local offices of the UI administration. An unemployed worker thus gets randomly assigned a risk of receiving a broader search task. Since the unemployed worker can only be matched to a caseworker within the local office, it is essential to include fixed effects for local offices in the regression equations. To justify the random assignment of unemployed workers to caseworkers, we regress the caseworker stringency on the worker characteristics and fixed effects for the local offices (interacted with calendar time). The parameter estimates for this regression are shown in column (3) of Table 4. A joint test shows that worker characteristics do not predict stringency of the caseworker (p-value equals 0.181).

The independence assumption allows to give a causal interpretation to the estimate for λ in the first-stage regression. However, for the validity of caseworker stringency as instrumental variable also an exclusion restriction is required. The exclusion restriction imposes that caseworker stringency only affects the (labor market) outcomes of the unemployed worker via the broader search task. This rules out that caseworker stringency is correlated to assistance provided by the caseworker who may help the unemployed workers in finding work. Recall that the caseworker meeting is part of a new program that is evaluated using a randomized experiment. The program focuses solely on broader search and, therefore, the caseworker meeting has a clear agenda. This reduces the discretion for caseworkers to consider other interventions. Furthermore, the program only contains a single caseworker meeting, which limits the scope for caseworkers to provide additional support.

To provide some justification for the exclusion restriction we follow Arni & Schiprowski (2019) and consider other policy choices made by the caseworker. Col-

Table 5: Use of other policy tools related to caseworker stringency and the broader search task

	Explanatory variables		Dependent variables			
	Mean	Standard Deviation	**Caseworker stringency**		**Broader search task**	
	(1)	(2)	(3)	(4)	(5)	(6)
Workshop participation	0.033	(0.178)	-0.006	0.003	0.082***	0.105***
			(0.006)	(0.005)	(0.012)	(0.012)
Benefits sanction	0.074	(0.263)	0.000	0.001	0.042***	0.046***
			(0.003)	(0.002)	(0.008)	(0.008)
Job search exemption	0.189	(0.392)	0.003	0.000	0.067***	0.061***
			(0.003)	(0.001)	(0.006)	(0.005)
F-statistic for joint test			2.34	0.34	83.33	84.10
[p-value]			[0.071]	[0.795]	[0.000]	[0.000]
Office x month FEs			—	✓	—	✓
Number of workers = 42,605			Number of caseworkers = 461			

Note: OLS estimates of caseworker stringency (columns (3) and (4)) and the broader search task (columns (5) and (6)) on other caseworker behavior. Regressions include controls for age, gender, nationality, education, previous wage, sector and working hours and UI benefits eligibility. Standard errors in parenthesis are robust and clustered at the caseworker level. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

umn (1) of Table 5 shows summary statistics on the most frequent other interventions in the period after the caseworker meeting. Almost 19% of the unemployed workers are at some point in time after the meeting exempted from job search, while 7.4% get a punitive benefits reduction for not complying to the guidelines on the UI administration and 3.3% are assigned to participating in a (job search training) workshop. Column (3) shows that the use of these interventions is uncorrelated to caseworker stringency. Even without correcting for the local office fixed effects all estimated coefficients are already insignificant. So, caseworkers who are less likely to impose the broader search task do not compensate this with more frequent use of other interventions.[14] To provide further support for the exclusion restriction we

[14] Caseworker added-value may be heterogeneous and related to caseworker stringency. To test this we consider unemployed workers that were to old to participate in the experiment (above 50)

include caseworker behavior as additional regressors in our empirical model. This validity check is discussed in subsection 5.2 and shows that the estimated effects of the broader search task are robust to including these additional regressors.

The instrumental variable approach requires that caseworker stringency is relevant, which means that it has sufficient explanatory power on assigning the broader search task. The explanatory power is expressed in the first-stage regression, and in particular in the parameter λ. Table 6 shows the estimate for λ in the full sample and also for individuals with different characteristics. In the full sample the estimate is 0.826, which implies that when an unemployed workers is assigned to a caseworker who imposed the broader search task during 90% of the meetings instead of 50%, the probability that this worker receives is broader search task is about 0.33 higher. The estimate for λ is highly significant and does not differ much between individuals with different characteristics. From this we conclude that caseworker stringency is a relevant and a very strong instrumental variable (F-statistic equals 1,778).[15]

To interpret the instrumental variable estimates the monotonicity assumption is helpful. This assumption states that when the stringency of the caseworker increases, the treatment status of the unemployed worker can not switch from receiving broader search task to not receiving this task. Since caseworker stringency is a continuous instrumental variable, our estimate for the broader search task is a weighted average of marginal treatment effects (see subsection 5.3 for an analysis of marginal treatment effects). Figure 2 displays the distribution of caseworker stringency unconditional and conditional on local office-month interactions. In the unconditional distribution (left panel), the caseworker at the 5th percentile imposes the broader search task in 46% of the meetings, while this is 91% for the caseworker in the 95th percentile. Roughly speaking under the monotonicity assumption about 46% of the unemployed workers are always takers (receive the broader search task from all

and who entered UI in the six months before the experimental sample. OLS estimates for this sample do not show any significant relation between the stringency of the caseworkers they met and job finding or exit from UI.

[15] In appendix B we show the robustness by including additional controls (Table B.1), the split-sample approach (Table B.2) and the reverse-sample approach (Table B.3). In the split-sample approach the sample is randomly split in two. The first sample is used to compute the caseworker stringency, while the second sample is used for the regression. In the reverse-sampling approach, the caseworker stringency is computed using opposite types, e.g. for women we use in the regression the caseworker stringency computed on men.

Table 6: First-stage estimates by demographics

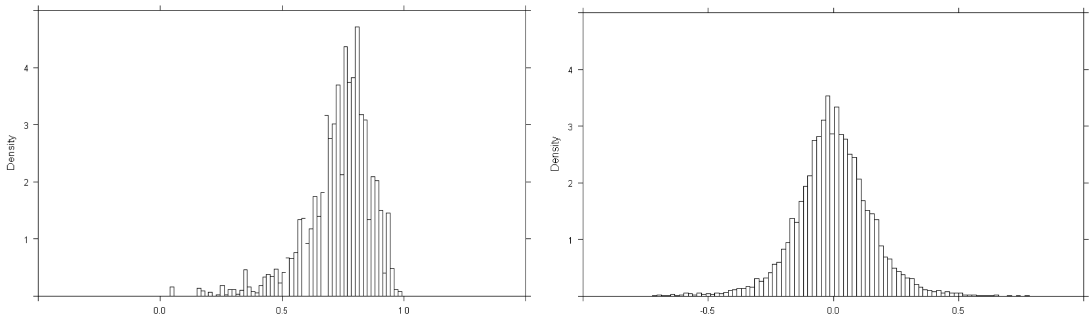
	Coefficient	S.e.	F-stat	N	Dependent Mean
	(1)	(2)	(3)	(4)	(5)
Full sample					
Full sample	0.826***	(0.020)	1,778	42,605	0.730
Gender					
Female	0.827***	(0.027)	921	25,207	0.752
Male	0.820***	(0.025)	1,036	17,398	0.698
Nationality					
Native	0.824***	(0.020)	1,644	40,899	0.733
Non-native	0.973***	(0.123)	63	1,706	0.663
Educational level					
Low educated	0.862***	(0.047)	336	7,063	0.674
Middle educated	0.816***	(0.028)	877	22,156	0.764
High Educated	0.823***	(0.034)	571	13,386	0.709
Age					
Younger than 40	0.845***	(0.027)	949	20,323	0.709
Older than 40	0.813***	(0.028)	857	22,282	0.750
Employment status					
Not employed at 6 months	0.818***	(0.024)	1,207	32,504	0.769
Employed at 6 months	0.850***	(0.044)	369	10,101	0.605

Note: Regressions include controls for age, gender, nationality, education, previous wage, sector and working hours, UI benefits eligibility, and local office fixed effects interacted with month fixed effect. Standard errors are robust and clustered at the caseworker level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

caseworkers), 9% are never takers (none of the caseworkers assigns them a broader search task) and the remaining 45% are compliers (depends on which caseworker they are assigned to if they receive the broader search task).[16] Our estimates for the effect of the broader search task are informative on the compliers. When we condition on the local office-month fixed effects (right panel), the standard deviation of the distribution increases, 0.151 compared to 0.135 unconditional. This implies that

[16] Maestas et al. (2013) consider the caseworker with the lowest (0.06) and highest (0.98) stringency and multiply the difference with the first stage coefficient to obtain the share of compliers $0.826 \times 0.92 = 76\%$. This computation is sensitive to outliers, which may be caseworkers that only meet few unemployed workers.

Figure 2: Distribution of caseworker stringency (left graph) and demeaned by local UI office and month (right graph)



(a) Caseworker stringency **(b)** Caseworker stringency (demeaned)

variation in caseworker stringency is not driven by variation between local offices.

The monotonicity assumption is violated when different caseworkers are strict to different groups of unemployed workers (De Chaisemartin, 2017). For example, younger caseworkers may impose the broader search task more often to younger unemployed workers and less often to older unemployed workers, while an older caseworker does the opposite. If there are more younger than older unemployed workers, the younger caseworker is stricter but monotonicity is violated. Although this is not concluding evidence, recall from Table 6 that there are hardly any differences in how likely individuals in different groups receive the broader search task. Imbens & Angrist (1994) consider this as support in favor of the monotonicity assumption.

5 Effects of the broader job search task

In this section we first show using a theoretical job search model how imposing the broader search task can affect the job search behavior of unemployed workers and their labor market outcomes. Next, in subsection 5.2 we present the estimated effects, which show that imposing the broader search task reduced exit from UI. In this subsection we also discuss the robustness of the estimated treatment effects and heterogeneous treatment effects. In subsection 5.3 we explore that there is substantial variation in the stringency of caseworkers and show marginal treatment effects. Finally, in subsection 5.4 we decompose the estimated effect of the program in an effect of the broader search task and an effect of the caseworker meeting.

5.1 Theoretical predictions

To get an idea about the expected effects from the broader search requirement, we consider a job search model with two search channels following Van den Berg & Van der Klaauw (2006). We refer to the two search channels as narrow search n and broader search b. A worker has to decide how much effort she devotes to narrow search s_n and to broader search s_b. The rate at which narrow and broader search effort result in job offer is given by $\lambda_n s_n$ and $\lambda_b s_b$, respectively. Search is costly to the unemployed worker and the costs are described by $c(s_n, s_b)$. Since broader search implies in practice searching for jobs that pay a lower wage, we assume that the wage offer distribution from narrow search $F_n(\cdot)$ first-order stochastically dominates the wage offer distribution from broader search $F_b(\cdot)$ and that broader search effort is more likely to result in a job offer $\lambda_b > \lambda_n$. The unemployed worker accepts all wage offers that exceed the reservation wage ϕ. As shown in Van den Berg & Van der Klaauw (2006) the optimal reservation wage follows from solving the Bellman's equation

$$\phi = \max_{s_n, s_b \geq 0} \omega - c(s_n, s_b) + \sum_{j=\{n,b\}} \frac{\lambda_j s_j}{\rho} \int_\phi^\infty w dF_j(w)$$

where ρ is the discount rate and ω the level of UI benefits.

Van den Berg & Van der Klaauw (2019) discuss two specifications for the search costs function. First, substitution between both search channels $c(s_n, s_b) = (s_n + s_b)^2$ and second effort devoted to both channels are complements $c(s_n, s_b) = s_n^2 + s_b^2$.[17] We consider both specifications below and discuss the consequence of imposing a broader search requirement. The broader search requirement means $s_b \geq \bar{s}_b$, so broader search effort should exceed a minimum $\bar{s}_b$ set by the UI administration.

If both search channels are substitutes, then the optimal behavior of the unemployed worker is to devote only search effort to the channel with the highest return. If the broader search channel is the channel with the highest return, then the worker

[17] Van den Berg & Van der Klaauw (2019) allow for different marginal costs of effort to both channels. However, for our purpose this is not necessary since we can always scale s_n, s_b, λ_n and λ_b, such that the marginal costs are similar.

only searches broadly and should satisfy the broader search requirement already.[18] This is typically the case where caseworker does not impose the broader search task and the unemployed worker should be considered as a never taker. The more interesting case is when the narrow search channel yields the highest returns. In that case the unrestricted optimal search behavior is $s_b^* = 0$ and

$$s_n^* = \frac{\lambda_n}{2\rho} \int_{\phi^*}^{\infty} w dF_n(w)$$

and the job finding rate equals $\lambda_n s_n^* (1 - F_n(\phi^*))$ where ϕ^* is the (unrestricted) reservation wage. The broader search task sets $\tilde{s}_b = \bar{s}_b$, which changes the optimal narrow search effort to

$$\tilde{s}_n = \frac{\lambda_n}{2\rho} \int_{\tilde{\phi}}^{\infty} w dF_n(w) - \tilde{s}_b$$

Since unemployed workers are restricted in their job search behavior $\tilde{\phi} < \phi^*$. Furthermore, the total search effort increases, $\tilde{s}_b + \tilde{s}_n > s_n^*$. Because $\lambda_b > \lambda_n$, the job offer arrival rate increases, i.e. $\lambda_b \tilde{s}_b + \lambda_n \tilde{s}_n > \lambda_n s_n^*$. Accepted wages decline for two reasons, first the reservation wage declines and second a share of the job offers is now drawn from $F_b(\cdot)$ rather than $F_n(\cdot)$. Finally, the effect on the job finding rate is ambiguous. If there is a large difference between $F_n(\cdot)$ and $F_b(\cdot)$, then many job offers obtained via broader search will be declined by the unemployed worker, while fewer job offers are generated via narrow search.

Now consider the case that both channels are complements, i.e. $c(s_n, s_b) = s_n^2 + s_b^2$. In that case the optimal search is given by

$$s_j^* = \frac{\lambda_j}{2\rho} \int_{\phi^*}^{\infty} w dF_j(w) \qquad j = n, b$$

If the optimal effort to broader search s_b^* already exceed the minimum requirement $\bar{s}_b$, then the minimum broader search task does not affect job search behavior. The more interesting case is when the broader search requirement causes that unemployed workers have to devote more effort to broader search, so that $\tilde{s}_b = \bar{s}_b$. The optimal

[18] If the broader search requirement requires more effort than the optimal effort, the worker should increase the broader search effort and reduce the reservation wage. This increases job finding and reduces the average accepted wage.

amount of narrow search effort becomes

$$\tilde{s}_n = \frac{\lambda_n}{2\rho} \int_{\tilde{\phi}}^{\infty} w dF_n(w)$$

Since the unemployed worker is restricted in her behavior, the reservation wage decline $\tilde{\phi} < \phi^*$ and thus narrow job search effort also increases $\tilde{s}_n > s_n^*$. Because the broader search task increases both broad and narrow job search effort and reduces the reservation wage, the job finding rate increases. Furthermore, expected accepted wages will decline since the reservation wage decreases and because a larger share of the accepted jobs will be found broadly.

Van den Berg & Van der Klaauw (2019) estimate the cost function of effort and find that different channels are almost perfect substitutes.[19] In that case, a broader search requirement increases total job search effort, reduced mean accepted wages, but the effect on job finding is ambiguous and depends on how acceptable job offers from the broader search channel are. These effects only apply to unemployed workers who without the broader search task devote their effort mainly to narrow job search. Moscarini (2001) argues that in equilibrium specialized workers search narrow because they have a comparative advantage in narrow job search.

Our job search model assumes that unemployed workers are fully informed about the job search environment. This is a strong assumption, unemployed workers may be too optimistic about job finding. When unemployed workers overestimate the returns to search, they set their reservation wages too high (Krueger & Mueller, 2016; Mueller et al., 2021). Belot et al. (2019) argue that initially too optimistic unemployed workers target their job search towards better jobs and, therefore, search narrowly, but they get more pessimistic when search is unsuccessful. When they have updated their beliefs sufficiently, Information on broader search may induce unemployed workers to update their beliefs faster and consequently change their job search behavior. The theoretical prediction of Altmann et al. (2018) is that when the additional information makes beliefs of the unemployed workers more realistic, then

[19] Van den Berg & Van der Klaauw (2019) distinguish between formal and informal job search and parameterize the cost function as $c(s_1, s_2) = (s_1^\gamma + s_2^\gamma)^{2/\gamma}$. Their structural analysis shows that γ is close to 1.

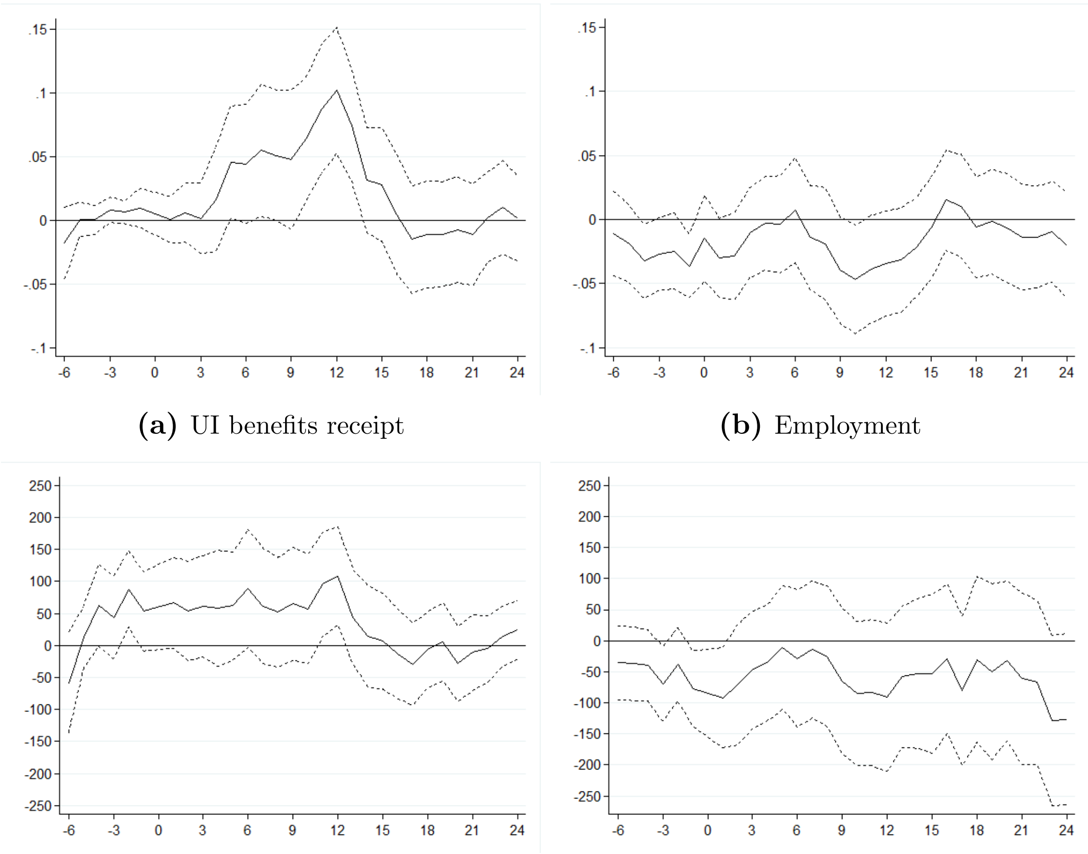
job search behavior becomes more efficient. Both Belot et al. (2019) and Altmann et al. (2018) state that providing information is particularly useful for individuals at risk of staying unemployed long-term. Recall that our broader search program targets individuals who have been collecting UI benefits for six consecutive months, which makes it likely that the target population is responsive.

5.2 Estimated effects

We use the empirical model discussed in the previous section to estimate the effects of imposing the broader search task for each month since the caseworker meeting. Figure 3 shows the estimated effects on the four main labor market outcomes, where time $t = 0$ is the moment of the caseworker meeting. Six months after the broader search task is imposed, the exit rate from UI significantly decreases (graph (a)). The broader search task causes that one year after the meeting the dependency on UI benefits is almost 10 percentage points higher. At that moment employment is about five percentage points lower and this is just significant (graph (b)). So the increased dependency on UI benefits is only partly explained by reduced job finding. One year after the meeting the effects diminish relatively fast, which might be partly due to unemployed workers reaching the end of the UI entitlement period. Recall that the individuals in our sample, on average, are entitled to 18 months of UI benefits, which is one year after the caseworker meeting. The average amount of UI benefits payments is somewhat higher when a broader search task is imposed (graph (c)). Again this peaks one year after the caseworker meeting and then the effect is just significant. The effects on earnings (graph (d)) show the opposite pattern with the exception that beyond one year after the caseworker meeting the effects remain negative rather than that they diminish. However, the effects on earnings are never significant.

Table 7 presents the estimated effects on cumulative outcomes one and two years after the caseworker meeting. The broader search task increases the period of collecting UI benefits with about 2.3 weeks. The majority of this effect is already present one year after the caseworker meeting. This also holds for the cumulative amount of the UI benefits payments, which increases by, on average, €800. The

Figure 3: Effects of imposing the broader search task - instrumental variable estimates



(a) UI benefits receipt

(b) Employment

(c) Amount of UI benefits

(d) Earnings

Note: Regressions include controls for age, gender, nationality, education, previous wage, sector and working hours, UI benefits eligibility, and local office fixed effects interacted with month fixed effect. $N = 42,605$ for all estimated effects in all panels. Dashed lines display the 95% confidence interval based on standard errors clustered on caseworker level. $t = 0$ is the time of the caseworker meeting.

increased benefits payments cause that imposing the broader search task is costly for the UI administration. The cumulative weeks of employment decreases less than the weeks of UI benefits increase. The broader search task causes that individuals have about 1.6 fewer weeks of employment. After one year the negative effect on earnings has about the same size as the positive effect on UI benefits payments. The negative effect on earnings increases further during the second year (the large standard error causes that the effect is not significant). We should, however, be careful in concluding that individuals financially suffer from receiving the broader search task. When UI benefits end during the second year, individuals may become eligible for welfare benefits and these benefits are not registered in our data.

Table 7: Effects of imposing the broader search task on cumulative outcomes - instrumental variable estimates

Dependent variable:	UI duration	UI benefits	Employment duration	Earnings
	(1)	(2)	(3)	(4)
1 Year after meeting	2.27***	838**	-1.17	-671
	(0.86)	(389)	(0.79)	(515)
Dependent mean	36.96	10,728	27.93	10,958
2 Years after meeting	2.70*	856	-1.65	-1,462
	(1.45)	(562)	(1.55)	(1,117)
Dependent mean	50.92	14,210	63.91	28,126
Number of workers		42,605		

Note: Regressions include controls for age, gender, nationality, education, previous wage, sector and working hours, UI benefits eligibility, and local office fixed effects interacted with month fixed effect. Standard errors in parentheses are robust and clustered at the caseworker level. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Our estimation results show that imposing the broader search task has adverse effects on job finding. This contradicts that the broader search task repairs systematic mistakes in the job search behavior, for example because unemployed workers are too optimistic about their labor market prospects. We use the theoretical predictions from the previous subsection to explain the reduced job finding. The broader search task forces unemployed workers to change their job search behavior and they can no longer search optimally. The negative effect on job finding is in line with the model specification where broader job search is a substitute for narrow job search. That different search channels are close substitutes concurs with Van den Berg & Van der Klaauw (2019). The theoretical model predicts that reservation wages decline and more often jobs will be found using broader job search. The latter is associated with lower wages. We investigate these predictions by considering the effects of imposing the broader search task on the job characteristics.[20]

[20]Ideally, we would also consider job application data. However, job applications in the online account are only observed for less than 40% of the individuals and often bunch at one application each week, which is the mandatory job search requirement. Also data from the online job search platform are incomplete and older applications are overwritten.

Table 8 shows the estimated effects of the jobs that the individuals had one year after the caseworker meeting and two years after the caseworker meeting. Imposing the broader search task does not have a significant effect on the hourly wage, but two years after the meeting it significantly reduces weekly working hours and the likelihood of having a permanent contract. The effects are quite substantial, the reduction in weekly working hours due to the broader search task is more than 6% and the broader search task reduces the probability to have a permanent contract by about 21%. Permanent contracts and more working hours are indicators for better job quality. So, due to the broader search task unemployed workers may have lowered their job requirements. This concurs with the theoretical predictions. We do not find evidence that due to the broader search task unemployed workers are more likely to work in a different sector than before they became unemployed or that the commuting distance to their job is larger.

Table 8: Effect of broader search task on job characteristics - instrumental variable estimates

Dependent variable:	Hourly wage	Weekly hours	Permanent contract	Different sector	Distance > 20km
	(1)	(2)	(3)	(4)	(5)
1 Year after meeting	-0.688	-0.687	0.012	-0.027	-0.021
	(3.963)	(0.732)	(0.023)	(0.022)	(0.036)
Dependent mean	17.45	26.93	0.23	0.80	0.51
Number of workers	27,358				26,088
2 Years after meeting	-0.302	-1.800***	-0.082***	-0.002	0.010
	(0.843)	(0.688)	(0.029)	(0.024)	(0.034)
Dependent mean	17.37	28.74	0.39	0.81	0.49
Number of workers	30,249				29,235

Note: All estimations are conditional on employment. Regressions include controls for age, gender, nationality, education, previous wage, sector and working hours, UI benefits eligibility, and local office fixed effects interacted with month fixed effect. Regressions of distance have fewer observations because the postal code of individuals and jobs are sometimes missing. Standard errors are robust and clustered at the caseworker level. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

The effects of the broader search task may differ between individuals. Table C.1 in appendix C shows the estimated effects for different groups of benefits recipients on the UI benefits duration and the employment duration. The estimated effect on the UI benefits duration is slightly higher for females than for males, for lower and middle educated than for higher educated and for older individuals than for younger individuals. But differences between groups are often not significant.

In the estimations above we used the leave-out mean to compute stringency of the caseworker. The robustness of the estimated effects of the broader search task is assessed by applying the sample-split approach and the reverse-sampling approach. The estimates in Table B.4 in appendix B and Table C.2 in appendix C show that both alternative approaches give very similar estimated effects of the broader search task.[21] In subsection 4.2 we stated that the exclusion restriction requires that caseworker stringency does not affect outcomes other than via the broader search task. We earlier showed that other possible support offered by the caseworker is orthogonal to caseworker stringency. We now explore this further by including caseworker policy use as additional regressors to the empirical model. Table B.6 in appendix B shows that all estimated effects are very robust against including these additional regressors. These results do not provide any indication that the exclusion restriction may be violated.

Our empirical findings do not concur with Belot et al. (2019), who stress that unemployed workers may benefit from broader job search. An important difference is that in their experiment unemployed workers can decide themselves whether they want to apply on the broader vacancies which are randomly provided. In our setting the broader search is a formal policy and unemployed workers are obliged to comply. The mandatory nature of our policy may also explain why our results differ from Altmann et al. (2018) and Skandalis (2019), who consider information provision on alternative job search strategies. In our setting, also unemployed workers without biased beliefs on their labor market prospects have to change their search behavior to comply to the broader search task. The theoretical predictions of Belot et al. (2019) and Moscarini (2001) suggest that broadening the search behavior is only beneficial for more disadvantaged workers (e.g. non-specialized and long-term unemployed

[21] Table B.5 in appendix B shows the robustness of our results to the period effects. If we include quarters instead of month fixed effects, the results remain unaffected.

workers).

5.3 Marginal treatment effects

The rate at which caseworkers impose the broader search task differs substantially between caseworkers. The caseworker stringency states which share of a random subsample of unemployed workers would receive the broader search task from the caseworker. If monotonicity holds, then unemployed workers could be ranked by their propensity to receive the broader search task. Unemployed workers with a high propensity would receive a broader search task from all caseworkers, while unemployed workers with a low propensity would only receive a broader search task from the most strict caseworkers. In that case, our empirical analysis provides a mixture of marginal treatment effects (Carneiro et al., 2010; Heckman & Vytlacil, 2001). Below we provide an analysis of the marginal treatment effects (MTEs) to study if treatment effects differ between unemployed workers who are very likely and very unlikely to receive a broader search task.

We define z as the inverse propensity that an unemployed workers receives the broader search task. The variable z is uniformly distributed within the sample of unemployed workers. When an unemployed worker is assigned to a caseworker with stringency Z, a broader search task is imposed if $z < Z$. We use a polynomial $\delta_0 + \delta_1 z + \delta_2 z^2$ for the treatment effect for an unemployed worker with characteristic z. The key problem is that z is unobserved. However, we observed the caseworker stringency $Z_{j(i)c}$ faced by unemployed worker i in local office c. If this caseworker receives a broader search task, then z is uniformly drawn from 0 to $Z_{j(i)c}$, so the expected treatment effect δ_{ic} equals

$$\delta_{ic} = \frac{1}{Z_{j(i)c}} \int_0^{Z_{j(i)c}} \delta_0 + \delta_1 z + \delta_2 z^2 dz = \delta_0 + \delta_1 \frac{Z_{j(i)c}}{2} + \delta_2 \frac{Z_{j(i)c}^2}{3}$$

Therefore, we extend the second-stage regression to

$$Y_{ic} = \alpha_c + \delta_0 B_{ic} + \delta_1 \frac{Z_{j(i)c} B_{ic}}{2} + \delta_2 \frac{Z_{j(i)c}^2 B_{ic}}{3} + X_i' \beta + \varepsilon_{ic} \tag{5}$$

Figure 4 shows the estimated MTEs for the cumulative labor market outcomes one year after the caseworker meeting.[22] The effects of the broader search task are most adverse for individuals with a low value for z. Recall that a low level of z implies that the broader search task will be imposed by all caseworkers. The adverse effects of the broader search task are smallest for unemployed workers with z is about 0.65. But labor market outcomes of these individuals still do not improve after receiving the broader search task. The conclusion is thus that no unemployed worker benefits from receiving a broader search task, but the targeting of caseworkers makes the average effect worse. However, only for cumulative earnings the marginal treatment effects are significantly different from a homogeneous treatment effect (see the coefficient estimates in Table C.3 in appendix C).

A possible explanation is that caseworkers target the broader search task towards unemployed workers who devote the least job search effort toward broader search. The reason why these individuals mainly search narrowly is that this yields the highest returns for them. These are what Moscarini (2001) refers to as the specialized workers. If they substitute broader job search for narrow job search, their labor market outcomes become substantially worse.

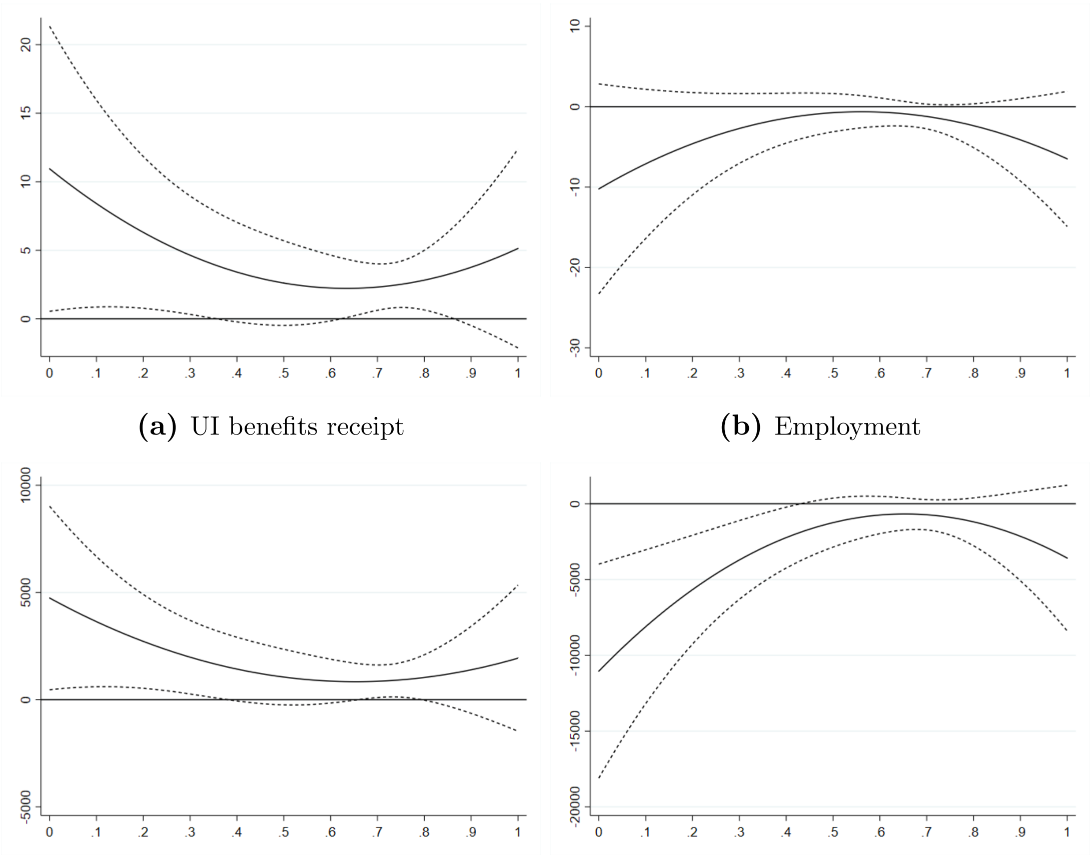
5.4 Decomposing the effects of caseworker meetings

The broader search program has positive effects on labor market outcomes. The program consists of a caseworker meeting and possibly a broader search task. Imposing the broader search task has negative effects on labor market outcomes. The program effect is estimated for the compliers to the experiment, while the effect for the broader search task is estimated for unemployed workers in the treatment group who attended a caseworker meeting. The latter includes both compliers and always takers, which complicates a direct comparison of effects. Below, we decompose the program effects in an effect of the caseworker meeting and an effect of the broader search task.

To compare the estimated effects of participating in the broader search program

[22]The same figures with the estimated MTEs for the cumulative outcomes after two years are in Figure C.1 in appendix C. These results yield similar conclusions.

Figure 4: Marginal treatment effects of imposing the broader search task on cumulative outcomes after one year



(a) UI benefits receipt

(b) Employment

(c) Amount of UI benefits

(d) Earnings

Note: The horizontal axis displays the unobserved resistance to the broader search task. Regressions include controls for age, gender, nationality, education, previous wage, sector and working hours, UI benefits eligibility, and local office fixed effects interacted with month fixed effect. Dashed lines display the 95% confidence interval based on standard errors clustered on caseworker level.

to the estimated effect of imposing the broader search task, it is necessary to make the populations comparable. Let ϕ_C and ϕ_A be the sizes of populations of respectively the compliers and always takers in the randomized experiment. We define Y^{11} as the potential outcome after attending a caseworker meeting in which a broader search task has been imposed and Y^{10} as the potential outcome after attending a caseworker meeting in which the caseworker meeting has not been imposed. Following Imbens & Rubin (1997), we write

$$\mathrm{E}[Y^{11} - Y^{10}|C] = \frac{1}{\phi_C}\left((\phi_C + \phi_A)\mathrm{E}[Y^{11} - Y^{10}|C \vee A] - \phi_A \mathrm{E}[Y^{11} - Y^{10}|A]\right)$$

The population shares ϕ_C and ϕ_A are observed in the randomized experiment and equal 0.366 and 0.259, respectively. The treatment effect $\mathrm{E}[Y^{11} - Y^{10}|C \vee A]$ is estimated using the sample of program participants in the treatment group, which coincides with the results in subsection 5.2. We can estimate $\mathrm{E}[Y^{11} - Y^{10}|A]$ using the program participants in the control group.[23] The estimates on cumulative outcomes for the subsequent estimates for $\mathrm{E}[Y^{11} - Y^{10}|C]$ and $\mathrm{E}[Y^{11} - Y^{10}|A]$ are shown in panels A and B of Table D.1 in appendix D. The effects of the broader search task are more adverse for the always takers than for the compliers to the randomized experiment.

We define Y^1 as the potential outcome of participating in the broader search program and Y^0 as the potential outcome of not participating. The evaluation of the randomized experiment provides an average treatment effect for the compliers to the random assignment, so $\mathrm{E}[Y^1 - Y^0|C]$. The potential treated outcome can be decomposed in a potential outcome Y^{11} with the broader search task and a potential outcome Y^{10} without the broader search task. For ease of simplicity we assume that the effects of the broader search task do not vary by the propensity to receive the task. Accordingly, we can write $Y^1 = pY^{11} + (1 - p)Y^{10}$ where p is the propensity to be assigned the broader search task. Recall that the marginal treatment effects did not provide strong evidence that treatment effects vary with the propensity to receive the task.[24] Then the effect of only the caseworker meeting without the broader search task becomes

$$\mathrm{E}[Y^{10} - Y^0|C] = \mathrm{E}[Y^1 - Y^0|C] - p\mathrm{E}[Y^{11} - Y^{10}|C]$$

The probability p to be assigned to the broader search task should apply to the

[23] The caseworker stringency is estimated using the treatment group in the randomized experiment. Next, we pool the data on the unemployed workers with a caseworker meeting in the treatment and control group to get a precise estimate for the treatment effects for the always takers in the control group. This is necessary because there are too few unemployed working with a caseworker meeting in the control group to estimate all local office and time fixed effects. Pooling the treatment and control group allows us to estimate the average treatment effect for the compliers directly.

[24] Allowing the effects of the broader search task to depend on the propensity to receive the task, would require to estimate the distribution of the propensity to receive the task among the compliers from the distributions in the control and treatment group and to also compare marginal treatment effects of both treatment groups.

compliers in the experiment. So this means that $p = \frac{1}{\phi_C}((\phi_C + \phi_A)\bar{Z}_1 - \phi_A \bar{Z}_0)$, where $\bar{Z}_1$ and $\bar{Z}_0$ are the rates of applying the broader search task in the treatment and control group of the randomized experiment. The rates of applying the broader search task $\bar{Z}_1$ and $\bar{Z}_0$ follow from Table 1 and equal 0.691 en 0.174, respectively, which implies $p = 1$.[25]

Table 9: Effects of the program, the broader search task and the caseworker meeting

Dependent variable:	Weeks of collecting UI	UI Benefits	Weeks of employ-ment	Earnings
	(1)	(2)	(3)	(4)
1 Year after meeting				
Program: $E[Y^1 - Y^0 \mid C]$	-1.41	-879	0.90	379
Broader search task: $E[Y^{11} - Y^{10} \mid C]$	1.77	622	-0.57	-444
Caseworker meeting: $E[Y^{10} - Y^0 \mid C]$	-3.18	-1,501	1.48	823
2 Years after meeting				
Program: $E[Y^1 - Y^0 \mid C]$	-1.84	-1,202	1.15	265
Broader search task: $E[Y^{11} - Y^{10} \mid C]$	1.67	351	0.61	-413
Caseworker meeting: $E[Y^{10} - Y^0 \mid C]$	-3.51	-1,553	0.54	678

Note: The standard errors of the program effects and the broader search task effects for the compliers are in Table 7 and Table D.1 in appendix D, respectively.

The results of the decomposition are shown in Table 9. The program and the broader search task have opposite effects. Therefore, the caseworker meeting is very effective. The caseworker meeting shortens the period of collecting UI with more than three weeks, which reduces UI benefits payments with about €1,500. These effects are in agreement with (e.g. Card et al., 2010; Schiprowski, 2020; Maibom et al., 2017), who also find strong effects of caseworker meetings. The effect on weeks of employment and earnings is less strong. So workers do not fully compensate the reduced UI benefits with additional earnings.

[25] Filling in all fractions actually gives $p = 1.06$.

6 Conclusion

The paper evaluates a broader search program for unemployed workers in the Netherlands. Results from a field experiment show that participation in the program stimulates the exit from unemployment. However, the reduced benefits payments are not fully compensated with additional earnings from work. The broader search program starts with a caseworker meeting, which takes place at the moment that unemployed workers should expand their job search to jobs that not necessarily match their skills and requirements. If during the meeting the caseworker assesses the past job search as too narrow, the caseworker can give the unemployed worker a mandatory task to apply for a broader set of vacancies. We exploit that the rate of giving this task differs between caseworkers and that unemployed workers are randomly assigned to a caseworker. Our estimation results show that the broader search task has a negative effect on labor market outcomes. Our decomposition analysis, therefore, shows a positive effect of the caseworker meeting, which is in agreement with e.g. Card et al. (2010), Schiprowski (2020) and Maibom et al. (2017).

The adverse effects of the broader search task seem in contradiction with Altmann et al. (2018) and Belot et al. (2019), who show positive effects of encouraging broader search. While our study considers recipients of unemployment insurance benefits, Belot et al. (2019) focus on more disadvantaged unemployed workers. Moscarini (2001) argues that broader search is more beneficial for workers with weak comparable advantages, such as the more disadvantaged workers studied by Belot et al. (2019). Furthermore, Altmann et al. (2018); Belot et al. (2019) consider an encouragement to search more broadly, while our broader search task is mandatory. An encouragement may mainly affect unemployed workers with biased belief about the returns to job search, while a mandatory program also affects unemployed who already optimize their search effort. Our job search model shows adverse effects of mandatory broader search for specialized workers who benefit more from narrow search than broader search. Since caseworkers direct broader search tasks to unemployed workers who are mainly searching narrowly, specialized workers are most likely to receive the broader search task. Our marginal treatment effect estimates suggest that the adverse effects of the broader search task are highest for unemployed workers that are most likely to receive the task.

Our findings show that evaluations from an encouragement or information treatment are not easily translated in a (low-cost) active labor market program. Active labor market programs are often more mandatory, which implies that the treated population is larger than the compliers to an encouragement or information treatment. In particular, caseworkers will target the program to a different group than the respondents to, for example, a brochure on the advantages of broader search as is studied by Altmann et al. (2018). To some extent we could interpret the caseworker meetings without the mandatory task as more similar to the information treatments. If no task is given to the unemployed worker, the meeting still focuses on broader search but is advisory and informative. The results from our decomposition show strong positive effects of the caseworker meeting without task that are in line with encouragement and information treatments on broader search.

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A Back-of-the-envelope costs-benefits analysis of the broader search program

For the UI administration the average costs of the broader search program are €169 per invited unemployed worker.[26] To compare these costs to the benefits for the UI administration, we consider the reduction in cumulative UI benefits payments for each invited unemployment workers. Therefore, we consider the intention-to-treat effect, which is estimated using the following regression equation

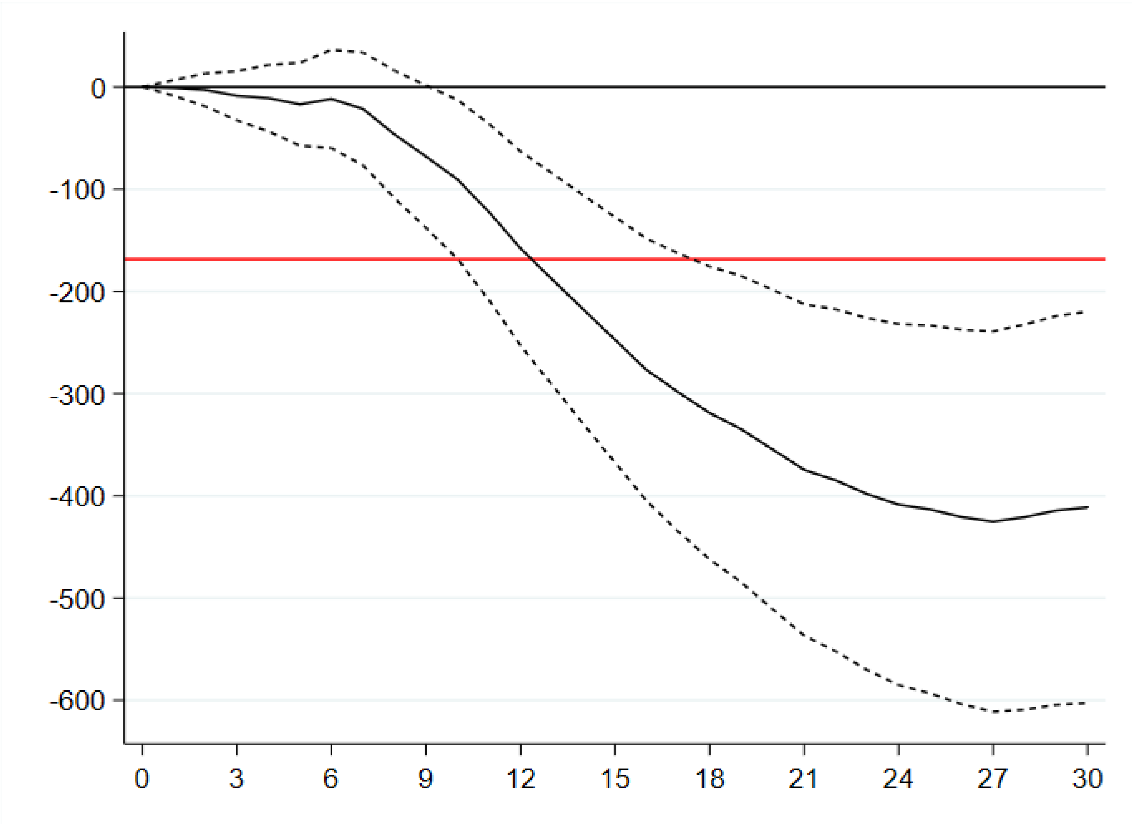
$$Y_i = \alpha + \delta T_i + X_i' \beta + \varepsilon_i$$

Since this specification regresses the outcome on the random assignment, it can be estimated using OLS. Further, we assume an annual discount rate of 5 percent.

The results are summarized in the Figure A.1. The break-even moment is after 13 months of collecting UI benefits (roughly 6 months after the meeting with the caseworker). In the long run the program is cost effective for the UI administration since the reduction in UI benefits equals about €400 per invited worker.

[26] The average costs per treated worker is calculated by dividing the total lump-sum costs of the experiment (€20 million) by the number of workers in the treatment group ($N = 118,697$).

Figure A.1: Intention to treat effects of the broader search program on cumulative UI benefits - OLS estimates



Note: The black line displays the estimated intention to treat effects. The estimated effects are based on regressions controlling for age, gender, nationality, education, previous wage, sector, working hours and UI benefits eligibility. Dotted lines display the 95% confidence interval, based on robust standard errors. The red line displays the average costs per invited worker. $t = 0$ is the start of collecting UI, the broader search program starts with a caseworker meeting in the seventh month of UI.

B Robustness of the effects of the broader search task

Table B.1: First-stage estimates using different sample selections on caseworkers and different controls

	Worker per caseworker				
Sample selection	40-400	50-400†	60-400	50-300	50-500
	(1)	(2)	(3)	(4)	(5)
Panel A. No controls					
Caseworker stringency	0.820***	0.834***	0.842***	0.836***	0.832***
	(0.018)	(0.020)	(0.023)	(0.020)	(0.020)
F-stat. (Instrument)	2,104	1,782	1,395	1,804	1,754
Panel B. Add demographic controls					
Caseworker stringency	0.820***	0.830***	0.836***	0.832***	0.827***
	(0.018)	(0.020)	(0.023)	(0.020)	(0.020)
F-stat. (Instrument)	2,082	1,763	1,377	1,784	1,735
Panel C. Add labor market history controls					
Caseworker stringency	0.820***	0.826***	0.834***	0.828***	0.824***
	(0.018)	(0.020)	(0.022)	(0.019)	(0.020)
F-stat. (Instrument)	2,076	1,778	1,388	1,811	1,739

Note: †The baseline analysis uses caseworkers meeting 50-400 benefits recipients. All regressions include local office fixed effects interacted with month fixed effect. The demographic controls are age, gender, nationality and education. The labor market history controls are previous wage, sector, working hours and UI benefits eligibility. Standard errors are robust and clustered at the caseworker level. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

Table B.2: First-stage estimates using different sample selections on caseworkers and different controls - split-sample approach

Sample selection	40-400	50-400	60-400	50-300	50-500
	(1)	(2)	(3)	(4)	(5)
Panel A. No controls					
Caseworker stringency	0.718***	0.745***	0.774***	0.751***	0.743***
	(0.033)	(0.038)	(0.043)	(0.038)	(0.038)
F-stat. (Instrument)	468	386	328	393	392
Panel B. Add demographic controls					
Caseworker stringency	0.715***	0.742***	0.768***	0.748***	0.739***
	(0.033)	(0.038)	(0.043)	(0.038)	(0.038)
F-stat. (Instrument)	463	383	323	390	388
Panel C. Add labor market history controls					
Caseworker stringency	0.709***	0.733***	0.762***	0.740***	0.731***
	(0.034)	(0.039)	(0.044)	(0.038)	(0.038)
F-stat. (Instrument)	444	363	299	373	367

Note: All regressions include local office fixed effects interacted with month fixed effect. The demographic controls are age, gender, nationality and education. The labor market history controls are previous wage, sector, working hours and UI benefits eligibility. Standard errors are robust and clustered at the caseworker level. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

Table B.3: First-stage estimates for different groups of benefits recipients - reverse-sample approach

	Coefficient	S.e.	F-stat	N	Dependent Mean
	(1)	(2)	(3)	(4)	(5)
Gender					
Female	0.874***	(0.086)	103	11,491	0.769
Male	0.750***	(0.051)	220	12,282	0.720
Nationality					
Native	–			476	0.742
Non-native	0.985***	(0.127)	61	1,608	0.669
Educational level					
Low educated	0.876***	(0.061)	209	6,167	0.682
Middle educated	0.777***	(0.077)	102	11,643	0.749
High Educated	0.771***	(0.057)	182	11,194	0.768
Age					
Younger than 40	0.732***	(0.066)	124	12,046	0.730
Older than 40	0.757***	(0.075)	103	12,454	0.771
Employment status					
Not employed at 6 months	0.343***	(0.076)	20	10,480	0.778
Employed at 6 months	0.836***	(0.068)	153	7,977	0.619

Note: Regressions include with the exception of the control for the relevant group controls for age, gender, nationality, education, previous wage, sector and working hours, UI benefits eligibility, and local office fixed effects interacted with month fixed effect. Standard errors are robust and clustered at the caseworker level. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

Table B.4: Effects of imposing the broader search task on cumulative outcomes - split-sample approach

Dependent variable:	UI duration	UI benefits	Employment duration	Earnings
	(1)	(2)	(3)	(4)
1 Year after meeting	3.11***	879*	-1.62	-621
	(1.20)	(534)	(1.30)	(720)
Dependent mean	36.89	10,710	27.94	11,007
2 Years after meeting	3.63*	846	-2.46	-1,312
	(2.12)	(787)	(2.48)	(1,608)
Dependent mean	50.81	14,210	63.87	28,173
Number of workers		21,342		

Note: Instrumental variable regressions include controls for age, gender, nationality, education, previous wage, sector and working hours, UI benefits eligibility, and local office fixed effects interacted with month fixed effect. Standard errors in parentheses are robust and clustered at the caseworker level. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

Table B.5: Effects of imposing the broader search task on cumulative outcomes – instrumental variable estimates with quarter fixed effects

Dependent variable:	UI duration	UI benefits	Employment duration	Earnings
	(1)	(2)	(3)	(4)
1 Year after meeting	2.21**	905**	-1.25	-713
	(0.88)	(391)	(0.79)	(517)
Dependent mean	36.96	10,728	27.93	10,958
2 Years after meeting	2.57*	940*	-1.76	-1,519
	(1.47)	(560)	(1.56)	(1,120)
Dependent mean	50.92	14,210	63.91	28,126
Number of workers		42,605		

Note: Regressions include controls for age, gender, nationality, education, previous wage, sector and working hours, UI benefits eligibility, and local office fixed effects interacted with quarter fixed effect. Standard errors in parentheses are robust and clustered at the caseworker level. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

Table B.6: Effects of imposing the broader search task on cumulative outcomes – instrumental variable estimates with additional controls for caseworker policy choices

Dependent variable:	UI duration	UI benefits	Employment duration	Earnings
	(1)	(2)	(3)	(4)
1 Year after meeting	2.27***	849**	-1.22	-676
	(0.88)	(393)	(0.79)	(518)
Dependent mean	36.96	10,728	27.93	10,958
2 Years after meeting	2.66*	876	-1.75	-1,487
	(1.48)	(573)	(1.56)	(1,127)
Dependent mean	50.92	14,210	63.91	28,126
Number of workers		42,605		

Note: Regressions include controls for age, gender, nationality, education, previous wage, sector and working hours, UI benefits eligibility, and local office fixed effects interacted with month fixed effect. The additional controls for other policy tools used by caseworkers are participation in workshops, sanctions and search exemptions, which are computed using leave-out means. Standard errors in parentheses are robust and clustered at the caseworker level. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

C Heterogeneous effects of the broader search task

Table C.1: Effects of imposing the broader search task on cumulative outcomes after one year for different demographic groups - instrumental variable estimates

Dependent variable:	UI duration	Employment duration
	(1)	(2)
A. GENDER:		
1. Female		
Estimate	2.69***	-1.09
(s.e.)	(0.91)	(1.22)
Dependent mean	37.62	28.23
Number of workers	25,207	25,207
2. Male		
Estimate	1.73	-1.11
(s.e.)	(1.16)	(1.52)
Dependent mean	36.00	27.49
Number of workers	17,398	17,398
B. EDUCATIONAL LEVEL:		
1. Low educated		
Estimate	2.40	-0.62
(s.e.)	(1.72)	(2.26)
Dependent mean	36.90	27.51
Number of workers	7,063	7,063
2. Middle educated		
Estimate	3.04***	-3.13**
(s.e.)	(1.00)	(1.32)
Dependent mean	36.97	28.97
Number of workers	22,156	22,156
3. High Educated		
Estimate	1.16	1.75
(s.e.)	(1.33)	(1.77)
Dependent mean	36.97	26.42
Number of workers	13,386	13,386
C. AGE:		
1. Younger than 40		
Estimate	1.55*	-1.87
(s.e.)	(0.91)	(1.31)
Dependent mean	32.67	29.70
Number of workers	20,323	20,323
2. Older than 40		
Estimate	2.35**	-0.16
(s.e.)	(1.07)	(1.37)
Dependent mean	40.87	26.31
Number of workers	22,282	22,282

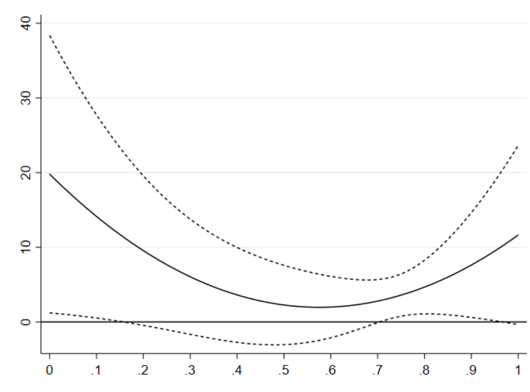
Note: Regressions include controls for age, gender, nationality, education, previous wage, sector and working hours, UI benefits eligibility, and local office fixed effects interacted with month fixed effect. Standard errors in parentheses are robust and clustered at the caseworker level. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

Table C.2: Effects of imposing the broader search task on cumulative outcomes after one year for different demographic groups - reverse-sample instrumental variable estimates

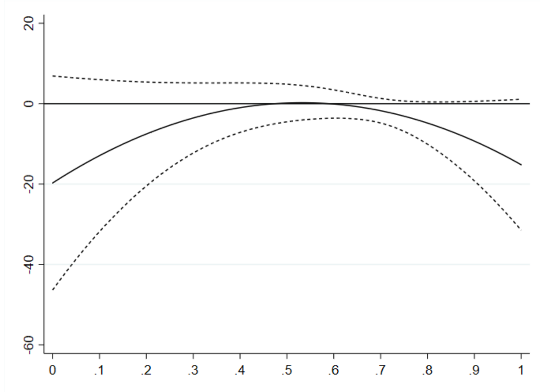
Dependent variable:	UI duration	Employment duration
	(1)	(2)
A. GENDER:		
1. Female		
Estimate	4.49***	-2.14
(s.e.)	(1.52)	(1.70)
Dependent mean	37.37	27.38
Number of workers	11,678	11,678
2. Male		
Estimate	-0.30	-0.01
(s.e.)	(1.46)	(1.70)
Dependent mean	36.17	27.10
Number of workers	12,607	12,607
B. EDUCATIONAL LEVEL:		
1. Low educated		
Estimate	2.24*	1.80
(s.e.)	(1.90)	(3.29)
Dependent mean	36.96	27.40
Number of workers	6,374	6,374
2. Middle educated		
Estimate	2.40*	0.88
(s.e.)	(1.44)	(2.26)
Dependent mean	36.91	28.47
Number of workers	11,875	11,875
3. High Educated		
Estimate	0.98	-0.70
(s.e.)	(1.54)	(1.84)
Dependent mean	36.79	26.16
Number of workers	11,441	11,441
C. AGE:		
1. Younger than 40		
Estimate	1.17	0.41
(s.e.)	(1.85)	(2.63)
Dependent mean	32.51	29.25
Number of workers	12,283	12,283
2. Older than 40		
Estimate	3.61**	-3.21
(s.e.)	(1.68)	(2.23)
Dependent mean	41.00	25.40
Number of workers	12,706	12,706

Note: Regressions include controls for age, gender, nationality, education, previous wage, sector and working hours, UI benefits eligibility, and local office fixed effects interacted with month fixed effect. Standard errors in parentheses are robust and clustered at the caseworker level. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

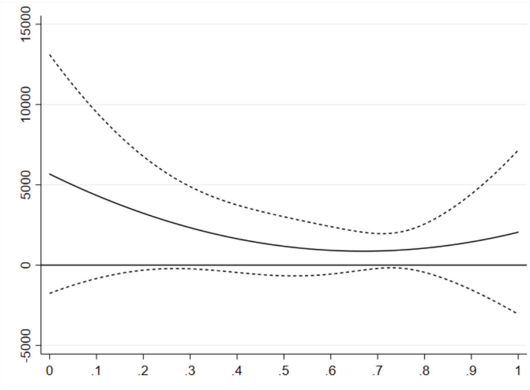
Figure C.1: Marginal treatment effects of imposing the broader search task on cumulative outcomes after 2 years



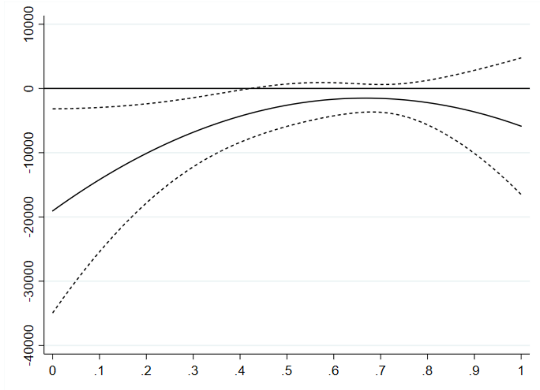
(a) UI benefits receipt



(b) Employment



(c) Amount of UI benefits



(d) Earnings

Note: Regressions include controls for age, gender, nationality, education, previous wage, sector and working hours, UI benefits eligibility, and local office fixed effects interacted with month fixed effect. Dashed lines display the 95% confidence interval based on standard errors clustered on caseworker level.

Table C.3: Marginal treatment effects coefficients of imposing the broader search task on cumulative outcomes - instrumental variable estimates

Dependent variable:	UI duration	UI benefits	Employment duration	Earnings
	(1)	(2)	(3)	(4)
Panel A: Linear specification				
1 Year after meeting				
δ_0	4.45	2,006	-1.11	-3,774*
	(3.69)	(1,583)	(3.75)	(2,208)
δ_1	-3.09	-1,661	-0.08	4,418
	(4.85)	(2,154)	(5.53)	(3,146)
2 Years after meeting				
δ_0	3.69	2,453	1.37	-7.253
	(6.43)	(2,297)	(7.39)	(4,601)
δ_1	-1.41	-2,274	-4.30	8,245
	(8.50)	(3,167)	(10.82)	(6,681)
Panel B: Quadratic specification				
1 Year after meeting				
δ_0	10.95**	4,742**	-10.23	-11,042***
	(5.31)	(2,184)	(6.66)	(3,603)
δ_1	-27.53	-11,943	34.19	31,741**
	(18.84)	(8,255)	(22.82)	(12,403)
δ_2	21.72	9,136	-30.45	-24,283**
	(17.69)	(7,961)	(20.40)	(11,343)
P-value joint test	0.26	0.19	0.32	0.01
2 Years after meeting				
δ_0	19.80**	5,673	-19.73	-19,064**
	(9.48)	(3,792)	(13.58)	(8,110)
δ_1	-61.95*	-14,370	75.04	52,647*
	(32.56)	(14,109)	(46.17)	(28,516)
δ_2	53.80*	10,747	-70.51*	-39,461
	(29.96)	(13,034)	(40.77)	(25,873)
P-value joint test	0.16	0.43	0.22	0.09
Number of workers		42,605		

Note: All outcome variables are measured 1 year after the caseworker meeting. Regressions include controls for age, gender, nationality, education, previous wage, sector and working hours, UI benefits eligibility, and local office fixed effects interacted with month fixed effect. Standard errors in parentheses are robust and clustered at the caseworker level. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

D Decomposition of the effects for the broader search program

Table D.1: Effects of imposing the broader search task on cumulative outcomes for compliers and always takers - instrumental variable estimates

Dependent variable:	UI duration	UI benefits	Employment duration	Earnings
	(1)	(2)	(3)	(4)
Panel A: $\mathbf{E}[\boldsymbol{Y}^{\mathbf{11}} - \boldsymbol{Y}^{\mathbf{10}}\vert\boldsymbol{C}]$				
1 Year after meeting	1.77*	622	-0.57	-444
	(0.91)	(417)	(0.94)	(580)
Dependent mean	36.96	10,728	27.67	10,958
2 Years after meeting	1.67	351	0.61	-413
	(1.54)	(605)	(1.80)	(1,278)
Dependent mean	50.92	14,210	62.61	28,126
Number of workers		42,605		
Panel B: $\mathbf{E}[\boldsymbol{Y}^{\mathbf{11}} - \boldsymbol{Y}^{\mathbf{10}}\vert\boldsymbol{A}]$				
1 Year after meeting	2.83***	1,059**	-2.39**	-1,150*
	(0.99)	(460)	(1.07)	(662)
Dependent mean	37.40	10,500	27.67	10,688
2 Years after meeting	3.85**	1,375*	-4.92**	-3,348**
	(1.72)	(702)	(2.10)	(1,450)
Dependent mean	51.47	13,972	62.61	27,032
Number of workers		1,477		

Note: Pooled regressions include controls for age, gender, nationality, education, previous wage, sector and working hours, UI benefits eligibility, and local office fixed effects interacted with month fixed effect. Standard errors in parentheses are robust and clustered at the caseworker level. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$