Empirical Evaluation of Broader Job Search Requirements for Unemployed Workers*

– *Work in progress* –

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Abstract

This paper estimates the causal effects of imposing broader job search requirements upon unemployed workers. To do so, we exploit exogenous variation in the assignment of the search requirements during caseworker meetings stemming from quasi-random assignment of the unemployed to caseworkers and variation in assignment rates across caseworkers. Our findings show that the caseworker meetings are cost-efficient and accelerate job finding, while workers whose job search is restricted during those meetings by the search requirements stay longer unemployed. We argue that restricting the job search of workers results in sub-optimal search strategies and therefore worse labor market outcomes.

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

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

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1 Introduction

Broader job search receives much attention of both policy makers and researchers as it is seen as a promising policy tool to decrease unemployment. The reason for this is that recent findings show that unemployed workers persistently misjudge their labor market prospects as they keep their expectations too high (Krueger & Mueller, 2016; Mueller et al., 2018). In turn, this suggests that workers anchor their reservation wage on their previous wage or search too narrow, for example for jobs that resemble their previous occupation (Mueller et al., 2018; Belot et al., 2019). Following the same line of thought, there are substantial rigidities for unemployed workers to apply for jobs for which they do not have the large majority of the required task competencies or that are geographically distant (Goos et al., 2019; Manning & Petrongolo, 2017; Marinescu & Rathelot, 2018). With this in mind, workers might find work faster if incentivized to search more broadly. However, ways to implement a low-cost tool that directly increases the scope of search of unemployed workers attracted little attention up until now. Similarly, there is yet little evidence on the labor market effects of broader job search.

This paper investigate the labor market effects of imposing broader job search requirements on unemployed workers. To this end, we use data from a large-scale field experiment conducted by the Dutch unemployment insurance (UI) administration among more than 130,000 workers. In the initial setup of the experiment, the search requirements were imposed upon the treated workers during a caseworker meeting, requiring them to apply for jobs that are below their level of education, that are in different sectors, with lower wages or that have longer commuting times. The challenge of this paper lies in estimating and interpreting the separate effects of the imposed search requirements. The central problem is that – similar to that of several other experiments – the combination of the treatment of interest (in our case the search requirements) and the caseworker meetings were randomized simultaneously, making it problematic to assess the effectiveness of either (Ashenfelter et al., 2005). A simple comparison of the outcomes of the identical treatment and control group would not answer the research question, as there is substantial evidence showing non-negligible effects of caseworker meetings (McVicar, 2008; Van den Berg et al., 2012; Maibom et al., 2017; Schiprowski, 2020). However, despite large compliance to the set-up of the experiment, the search requirements were not imposed in all meetings. Instead, caseworkers selected the workers based on their expected returns to the treatment, making the search requirements endogenous to both observed and unobserved individual characteristics.

To isolate the effects of the broader job search requirements from that of the caseworker meetings in which they were imposed, we use the stringency of caseworkers as an instrument for the search requirements. More precisely, we exploit exogenous variation in the assignment of the search requirements during caseworker meetings stemming from quasi-random assignment of the unemployed to caseworkers and variation in assignment rates among caseworkers.[1] By doing so, we compare the labor market outcomes of homogeneous workers who were or were not subject to the broader job search requirements only based on the random assignment to caseworkers with different assignment rates. This variation in outcomes is unrelated to both observed and unobserved characteristics of the unemployed workers. Since caseworkers could also choose to assign other policy tools to the unemployed, we need extra assumptions regarding the exclusion restriction to ensure causal interpretation of the estimates. In other words, we need to assure that the estimated effects only reflect the effect of the search requirements and not variation in assignment rates of other policy tools correlated to our instrument. Accordingly, we can estimate causal effects of imposing broader job search requirements during caseworker meetings on the labor market outcomes of the workers.

There exists a large body of literature examining the effects of activation programs on unemployment duration and job finding – see Card et al. (2010, 2018) and

[1]Our estimation strategy is similar to that of studies exploiting judge stringency as an instrument, such as Kling (2006) who uses quasi-randomly assigned judges to examine the effects of incarceration length on labor market outcomes after release from prison, Aizer & Doyle Jr (2015) to estimate the effect of juvenile incarceration on high school completion and adult recidivism, Doyle Jr (2007, 2008) to estimate the effects of foster care placement on juvenile and adult crime, teen motherhood and employment, Bhuller et al. (2020) to estimate the effect of incarceration on recidivism and employment, and Maestas et al. (2013) and French & Song (2014) to estimate the effects of disability insurance receipt on employment The only other study to our knowledge that uses caseworker stringency in the setting of the UI scheme is by Arni & Schiprowski (2019), who estimate the returns of required job search efforts.

Kluve (2010). The studies concerned with policies focusing on adapting the scope of the job search of unemployed workers are less common. There are a few recent studies that show that providing information to unemployed workers can alter their search behavior and can be a low-cost policy tool with positive outcomes. Altmann et al. (2018) find increased employment as a result of an information brochure – with among others information about job search strategies and consequences of unemployment – among workers who are at risk of long-term unemployment. Belot et al. (2019) provide workers in an in-lab experiment with additional occupations in which comparable workers have successfully found employment and where skill transferability is high. They find that these workers consider a broader set of vacancies and have more job interviews, this especially holds for workers with an initially narrow search who have been unemployed for several months. Similarly, findings by Skandalis (2018) suggest that information provision could increase geographical mobility, and with that, decrease the mismatch on the labor market. On the other hand, however, there is little known about the effects of enforcing specific job search behavior upon unemployed workers.[2]

By providing such an assessment of broader job search requirements, this paper is the first to investigate the labor market effects of enforcing unemployed workers to adapt their scope of search. Our paper adds to the literature on several points. First, we study the effects of actively imposing search requirements upon workers by a caseworker. This is different from the studies that focus on tighter search requirements, as in these studies the search requirements apply to all treated unemployed workers and are not actively mandated. In our study, the search requirements also apply to all workers, however, on top of that they are actively enforced upon a subsample of the workers during a caseworker meeting and backed up with an increased

[2]Mixed effects stricter search requirements. Johnson & Klepinger (1994) and Klepinger et al. (2002) show using experimental data that the presence of search requirements decreases UI receipt. Similarly, Lammers et al. (2013) find that the introduction of search requirements for older workers significantly increases job finding. Arni & Schiprowski (2019) find that un- and non-employment duration decline if the level of job search requirements increases. Contrary to those findings, Keeley & Robins (1985) show in their seminal paper that search requirements increase the unemployment duration for married men and youth. Similarly, Manning (2009) and Petrongolo (2009) find no evidence of increased search activity as a consequence of tighter search requirements and the introduction of an administrative hurdle. Moreover, they do find lower employment rates, which are driven by individuals who move out of UI without finding work.

threat of (financial) sanctions. In this paper, we estimate the effects of the latter; hence, the effects of actively imposing search requirements upon workers during a caseworker meeting. Second, we isolate the effects of imposing tighter search requirements, while keeping the monitoring and the degree of job search assistance unaltered. More precisely, this implies that we will separate the effects of imposing tighter search requirements from the effects induced by the meeting in which the search requirements are imposed. In comparison, in the vast majority of studies the tightening of search requirements are accompanied by increased monitoring and/or job search assistance and are evaluated jointly. Third, we are interested in the labor market effects of broader job search requirements specifically. These search requirements aim at changing the job search strategy, where previous studies aimed at either increasing the search effort or lowering the reservation wage of the unemployed. The only exception is the study by Belot et al. (2019), but their data does not suit the estimation of effects on labor market outcomes.

Our main research findings can be summarized as follows. We first compare workers with and without a caseworker meeting – both meetings with and without broader job search requirements imposed – and find that caseworker meetings are successful in decreasing the un- and non-employment of workers, making the meetings a cost-efficient policy tool. Contrasting to this, imposing broader job search requirements increases the unemployment duration and delays the job finding of workers substantially. Additionally, they work less hours and have more often a temporary contract two years after the meeting. In other words, the search requirements do not only postpone re-employment, but also result in matches with less attractive jobs. As these workers stay unemployed for a longer period, the additional costs of continued benefit payments due to the search requirements are substantial.

The remainder of the paper is organized as follows. First we describe the UI-system, and the design and implementation of the experiment. Next, we give a description of the data and estimate the effects of the caseworker meetings in section 3. In section 4 we introduce and elaborately assess the variation in caseworker stringency as our instrument to isolate the effects of imposing broader job search requirements from all other effects inherent to the caseworker meetings. Section 5 presents the results of the main analysis before section 6 concludes.

2 Background of the experiment

2.1 The Dutch UI system

In the Netherlands, UI benefits replace lost labor income of workers that are laid off by their employers and support them in their search for work in the first period of their unemployment. The maximum duration of this period depends on the work history of the worker – each year worked increases the entitlement period with one month, with a maximum duration of 38 months.[3] A rightful claim requires that the worker loses at least five working hours and that the worker has worked at least 26 of the previous 36 weeks. If these requirements are met, benefits equal 75 percent of the previous earnings in the first two months of unemployment and 70 percent thereafter, with a maximum insurable earnings level of approximately €52,000 ($62,000) per year at the time of the experiment.[4]

During their unemployment, UI recipients have the obligation to: (i) inform the UI administration about everything that is relevant for their benefits entitlement – e.g. their income; (ii) make at least one job application each week for a job considered suitable by the UI administration;[5] (iii) accept suitable job offers; and (iv) attend meetings with a caseworker if they are invited.[6] If they fail to meet these

[3] The eligibility criteria changed from January 2016, after which only the first ten years of employment increase the entitlement period with one month per year, thereafter each year increases the period with half a month with a maximum total entitlement of 24 months. The workers that accumulated entitlement to more than 24 months before January 2016 lose one month entitlement per three moths until their entitlement equals 24 months. These changes influence the last nine months of the experiment, however, we consider the impact to be negligible as it has only small effects on workers with relatively long entitlements to start with.

[4] The exchange rate was 1 Euro to 1.19 US Dollar in August 2020.

[5] Whether a job is considered suitable for the worker by the UI administration is based on the workers' educational level, experience and previous wage. Until July 2015, all jobs were considered suitable after 12 consecutive months of unemployment, since then this is the case after 6 months.

[6] At the time of the experiment, UI recipients have three caseworkers meetings in thier first year of unemployment – in the fourth, seventh and tenth month. In these meetings, the caseworker addresses the recent job search and the availability for employment of the workers. The meetings may also be used to discuss future search activities, qualifications of the worker, participation in training or activation programs, and information on specific job openings. See Rosholm (2014) and Van den Berg & Van der Klaauw (2006) for a more elaborate discussion on caseworker meetings

obligations, they risk losing partial or complete eligibility of their benefits.

2.2 The treatment

UI benefit recipients receive an invitation for a caseworker meeting after six consecutive months of unemployment.[7] These meetings will take place in the seventh month of unemployment. In these meetings, the job search activities of the workers are discussed and – if the caseworker finds it necessary – broader job search requirements are imposed upon them. More specifically, workers imposed with the search requirements are obliged to apply for and – if these applications lead to a job offer – accept jobs that are below their educational level, in a different sector, with a lower wage or have a longer commuting time than their previous job. Additionally, workers have to apply for two specific vacancies that are in accordance with the broader job search requirements. The obligations are recorded by the caseworkers, such that they can be evaluated in the subsequent months. This way, workers who did not apply for a broader set of jobs risk financial sanctions.

The impact of imposing broader job search requirements on the workers' labor market outcomes is theoretically ambiguous. Previous studies show that workers might benefit from broadening their job search as they have kept their expectations too high and searched too narrow (Krueger & Mueller, 2016; Belot et al., 2019). At the same time, workers are willing to expand their search beyond their initial scope of search if provided with additional information (Belot et al., 2019; Skandalis, 2018). As a consequence, broader job search requirements could broaden their scope of search and redirect their search to vacancies where they might be more successful in finding a job. On the other hand, workers are constrained by the tighter search requirements, resulting in sub-optimal job search behavior. This especially holds

and the channels through which they affect the search strategy of unemployed workers.

[7] The unemployed workers receive an letter in which they are informed about the fact that their unemployment spell exceeds six months and that they have to start applying for a broader set of jobs (below their educational level, in a different sector, with lower wage or with a longer commuting time). For that reason, they are invited for a meeting in which recent search activities and future search strategies are addressed. The unemployed workers should bring two suitable vacancies, curriculum vitae, their past applications and the reactions of employers on those applications with them. The letter states that attendance is obligatory and that an illegitimate no-show will result in a benefit reduction. The letter further indicates that the travel costs will be reimbursed.

for the unemployed who were more adequate in correctly predicting their labor market prospects. One possible reason for sub-optimal outcomes could be that the enforcement of search requirements along one dimension of the search process – in our case the scope of search – leads to reduced optimal level of search effort or inefficient substitution between search methods (Keeley & Robins, 1985; Van den Berg & Van der Klaauw, 2006). This effect might be amplified by a decrease in search effort as a result of aversion to the search requirements and less experienced autonomy (Koen et al., 2016). In the most extreme case, some unemployed workers might move out of the claimant status all together (Manning, 2009; Petrongolo, 2009). Additionally, workers might have to apply for jobs with lower (perceived) chances as they might not have the required task competences.

2.3 The experiment

To investigate the effectiveness of the broader job search requirements, the UI administration set out a randomized controlled trial among the workers who reached the point of receiving UI benefits for six consecutive months between April 2015 until March 2017. The target population of the experiment were UI recipients who are younger than 50 and entitled to at least ten months of benefits. The randomization of the experiment is based on the social security number, excluding all workers with a specific last digit from the treatment. As a result, 90 percent of the participants are randomly assigned to the treatment group and the remaining 10 percent form the control group.

The unemployed workers in the treatment group are randomly assigned to a caseworker of the UI administration office in their region. Generally, caseworkers try to see as many workers as possible, but cannot meet all the workers assigned to them; therefore, they select a sub-sample based on the workers' profiles. In virtue of the experiment, the caseworkers were informed to treat the workers differently during the meeting after six months of unemployment. Hence, the caseworker meetings in which the broader job search requirements are imposed replace the regular meetings in the seventh month of unemployment. In addition to their normal tasks, the caseworkers discuss a broader set of vacancies with the workers and – if the caseworkers find it necessary – impose the workers with the obligation to search more broadly. These decisions depend largely on the initial search of the workers. Workers with an initially broad search comply with the notion of broader job search and no additional requirements are necessary for them, while initially narrow searching workers have to expand their search and will be imposed with the search requirements. Consequently, the broader job search requirements are imposed upon workers for whom the requirements are restrictive, namely workers with an initially narrow search.

Table 1: Participation in the experiment

	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 requirements	43.1	4.5	0.00
Panel C: Services 37 weeks or 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: All numbers in columns (1) and (2) represent percentages of the baseline sample of all participants in the experiment stratified by their assignment to the treatment or control group. Column (3) presents the p-values of t-tests of different means for the treatment and control group.

Table 1 shows that the unemployed workers in the treatment and control group received virtually the same services from the UI administration until the time of randomization. Roughly 80 percent of the workers met with a caseworker in their first months of unemployment. Next, we see that the compliance to the randomization was high. More than 60 percent of the workers in the experimental group met a caseworker, compared to only 25 percent in the control group. Similarly, we see that the broader job search requirements were often imposed during the caseworker meetings in the treatment group. As a result, the unconditional probability on broader search requirements was substantially higher in the treatment group (43 percent) than in the control group (5 percent). This can be explained by both the higher number of meetings in the treatment group and a higher assignment rate in the treatment group conditional on a meeting. Further, we see that workers in the control group utilized other options of contact with the UI administration more often. Finally, we see that in the period after the experiment that both groups have almost the same probability of a caseworker meeting. The difference between the two groups is significant, but with one percentage point not substantial.

3 Data and the experiment

3.1 Data description

For our analysis we extract information from several datasets from the UI administration that we can link using anonymized identification numbers. The datasets provide us with information on the unemployment spells (start and end date of the unemployment spell, monthly UI benefits, benefit eligibility and re-integration activities), employment contracts (start and end date of the spells, monthly wage, contract hours, type of contract and sector) and personal characteristics such as the date of birth, gender, nationality and educational level.

For our baseline sample we select all workers who comply with the selection criteria mentioned earlier. Hence, we select those who entered UI between October 2014 and September 2016, are entitled to at least ten months of benefits, received benefits for at least six consecutive months and are younger than 50 after the first six months of benefit receipt. In addition, we exclude workers whose participation was not mandatory, e.g. those who participated in an entrepreneurship program or worked in education or for the government.[8] As a result, our baseline sample

[8]Workers can be exempt from the application duties under several circumstances, examples are when they participate in a training program, are allowed to start as an entrepreneur or receive partial benefits to supplement their labor earnings. In these situations they are less often invited for caseworker meetings or enrolled in (other) programs. Workers who worked in education or for

consists of 132,177 observations of distinct individuals. We merge the individual data with information on the unemployment spells and employment contracts for the first 32 months after entering UI. We thus have information of a sufficiently long period to assess the effects up to two years after the caseworker meeting – as the meeting took place in the seventh month of registered unemployment.

Table 2: Testing random assignment of workers to treatment and control group

	Explanatory variables		Dependent Variables			
			Treatment group		**Pr(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)
Native	0.954	(0.210)	0.0085**	(0.0041)	0.016**	(0.007)
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						
Wage (1,000€)	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	87.63	(28.36)	0.0000	(0.0001)	-0.001***	(0.000)
Employed at 6 months	0.296	(0.457)	-0.0015	(0.0021)	-0.159***	(0.003)
Sector last job						
Financial	0.23	(0.42)	0.0037	(0.0034)	-0.002	(0.005)
Retail and trade	0.20	(0.40)	-0.0010	(0.0035)	0.020***	(0.006)
Health care	0.19	(0.39)	-0.0010	(0.0036)	-0.006	(0.006)
Temporary employment	0.09	(0.28)	0.0058	(0.0040)	-0.032***	(0.006)
Transport	0.06	(0.23)	-0.0022	(0.0045)	0.009	(0.007)
Other	0.15	(0.36)	0.0034	(0.0036)	-0.017***	(0.006)
F-statistic for joint test			1.40		93.48	
[p-value]			[.138]		[.000]	
Number of workers = 132,177						

Note: Baseline sample of all participants in the experiment. Reported F-statistic refers to a joint test of the null hypothesis for all variables. The omitted category for education is "Low educated" and for last sector is "Industrial". Robust standard errors in parenthesis. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

the government are covered by a different benefit scheme and are therefore not included in our sample.

Table 2 presents the descriptive statistics of our baseline sample, a test for randomization of the workers over the treatment and control group, and a test to check which variables explain the probability of a caseworker meeting in the seventh month of unemployment. The first column shows that the workers in the baseline sample are relatively young with an average age of almost 40 years, as workers older than 50 are excluded from the experiment. Nevertheless, they are on average entitled for one year and 8 months of UI benefits. Around 57 percent is female and the large majority (about 95 percent) is Dutch. If the workers are randomly assigned to the treatment and control group and comparable based on their characteristics, we must be unable to predict the treatment indicator by their background characteristics. Indeed, column (3) of Table 2 shows only one significant coefficient, meaning that the workers in the treatment and control group are virtually the same. The joint hypothesis test also verifies that the unemployed workers are randomly allocated between the two groups (p-value=0.138).

Similarly, column (5) shows which characteristics predict the incidence of a caseworker meeting. Although the coefficients are arguably small, we observe that older workers, females and those without labor income after six months of receiving UI benefits have more often a meeting. The latter difference is most notable and can be explained by the relatively high labor market attachment of workers with some labor income during their unemployment spell and therefore lower expected returns of a meeting for this group. From this test we conclude that the probability of a caseworker meeting is endogenous to the characteristics of the workers, as caseworkers systematically invite a non-random sub-sample of the workers.

3.2 Evaluating the caseworker meetings

In this section we will estimate the causal effects of the caseworker meetings on worker outcomes Y_{it}. Hence, we are interested in the following outcome equation:

$$Y_{i,t} = \alpha + \delta_t M_i + X_i' \theta_t + \varepsilon_{i,t} \tag{1}$$

where α is a constant, M_i is the variable indicating whether unemployed worker i had a caseworker meeting and X_i is a vector of control variables that may influence the outcome variable (all variables displayed in Table 2). Note that we for now include all meetings – both meetings that are succeeded by broader job search requirements and meetings in which this is not the case. We address the selection issue discussed in the previous section – namely, workers being selectively invited for the caseworker meetings – by exploiting the randomized nature of the experiment, which provides us with a natural identification strategy for estimating causal effect of the caseworker meetings. Specifically, we use the exogenous treatment status T_i as an instrument for the incidence of a caseworker meeting. Accordingly, the first-stage regression model can be written as followed:

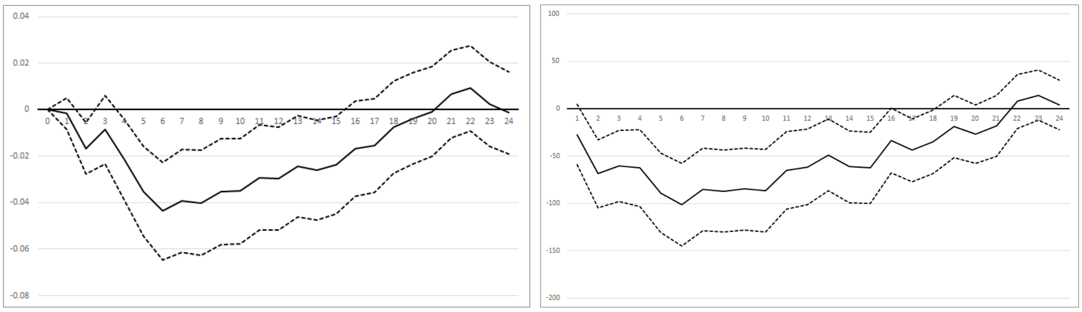
$$M_i = \alpha + \lambda T_i + X_i' \phi + \nu_i \tag{2}$$

As a result, parameter δ_t in equation (1) provides us with the local average treatment effect (LATE) of the caseworker meetings. The results displayed in this section show the effects starting six months after entering UI ($t = 0$), as the experiment does not influence the outcomes in the first six months of unemployment.

Figure 1 shows the LATE coefficients for the effects of caseworker meetings on the unemployment probability and UI benefits. We see that caseworker meetings decreases both unemployment and benefits in the first 6 months, after which the treatment effect slowly disappears. This results in a difference in the unemployment probability between the treatment and control group of at most 4 percentage points. This translates to a reduction of the unemployment duration with roughly 1.4 weeks and of UI benefits with almost €900 ($1,100) in the first year (see Table A.10 in the appendix for aggregate outcomes after one and two years, respectively). In Figure A.6 in the appendix we show using a back-of-the-envelope calculation that the caseworker meetings are cost-efficient after no more than six months. After two years, the retained UI benefits outweigh the costs of the experiment by 1.5 times the amount.

The lower unemployment probability can be largely explained by an increase in job finding of roughly 3 percentage points (see Figure 2). Similar to the effect on unemployment, the effect on employment peaks after roughly half a year before it diminishes. After a full year, the effect seems to be annulled by postponed job find-

Figure 1: Estimated local average treatment effects of a caseworker meeting on their unemployment probability and UI benefits

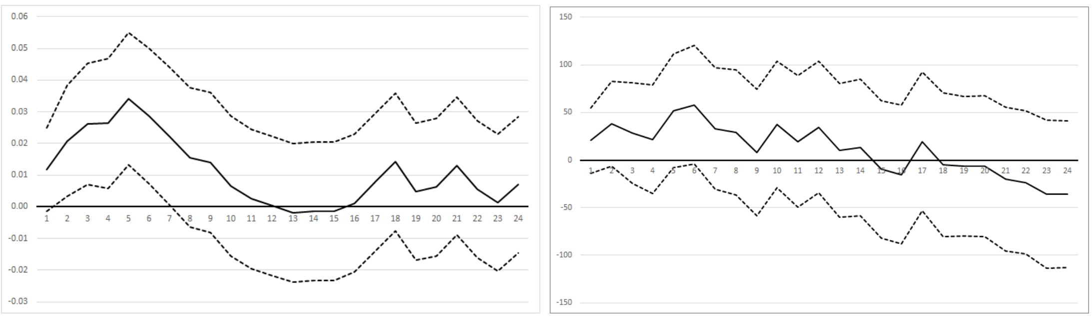


(a) Unemployment probability **(b)** UI benefits

Note: Dotted lines display the 95% confidence interval, based on robust standard errors.

ing of workers without a caseworker meeting and remains virtually zero thereafter. The positive short-term effects of caseworker meetings on un- and employment are consistent with previous findings (Card et al., 2010). Despite the higher job finding rates in the treatment group and on average one week longer employment, the earnings of the workers are not significantly affected by the meeting. It should be pointed out that the total effect on the income of workers seems to be negative, since we did find a negative effect on UI benefits.[9]

Figure 2: Estimated local average treatment effects of a caseworker meeting on the employment probability and earnings



(a) Employment probability **(b)** Earnings

Note: Dotted lines display the 95% confidence interval, based on robust standard errors.

[9]We have no data on other social security benefits nor income from self-employment, as they are not recorded by the UI administration. Therefore, we cannot rule out that there is a zero or positive effect on income.

4 Exploiting the caseworker assignment

4.1 The instrumental variables model

In the context of the experiment, the estimates in the previous section represent not only the effects of the caseworker meetings, but also the effects of the broader job search requirements that were imposed more often upon workers in the treatment group. However, for policy makers it is important to understand the mechanisms behind the results and thus know the separate effects of the search requirements. Hence, we want to isolate the effects of imposing broader job search requirements from the other effects inherent to the caseworker meetings. This captures the idea of comparing the workers with only a caseworker meeting to similar workers who also get the search requirements imposed upon them. This can be captured by the following regression model:

$$Y_{i,t} = \alpha + \beta_t B_{i,0} + X_i' \theta_t + \varepsilon_{i,t} \tag{3}$$

where β_t is the parameter of interest, $B_{i,0}$ indicates whether worker i was imposed broader job search requirements in the caseworker meeting at time zero (normalized to the time of the caseworker meeting), $Y_{i,t}$ is the dependent variable measured at some point t after the meeting and X_i is a vector of control variables that may influence the outcome variable. It is important to stress that in the remainder of the paper we only use the workers in the treatment group who also attended a caseworker meeting. Ideally, you would want to estimate a model that estimates both the effects of the caseworker meetings and of the broader job search requirements simultaneously exploiting the full sample. However, in a situation with two endogenous regressors, you would need the same number of exogenous instruments covering the full sample Angrist & Pischke (2008). Unfortunately, our instrument for the broader job search requirements is only observed for a sub-sample of the workers, namely those who attended a caseworker meeting, as the caseworkers are not identified if no meeting took place. Our baseline sample further restricts the dataset to those workers who attended a meeting with a caseworker who met with at least 50 and at most 400 workers during the experiment period.[10] Finally, we exclude the workers who attended a caseworker meeting in the control group as those may represent exceptional cases. After applying these restrictions, our baseline sample includes 44,086 workers for whom we observe the instrument.

As demonstrated in column (5) of Table 3, the broader job search requirements are endogenous to (observed) characteristics, even if we control for fully interacted office and month fixed effects. Workers that are e.g. older, female, higher educated or have no labor income have a larger probability of having broader job search requirements imposed upon them. Hence, the OLS estimator of β_t could suffer from selection bias. We address this problem by exploiting random assignment of unemployed workers to caseworkers at the UI administration office level and systematic variation in likeliness to impose the requirements (or: stringency) between caseworkers. In other words, workers face different probabilities of having the search requirements imposed upon them depending on the caseworker they are assigned to.[11] This method is inspired by studies exploiting judge stringency as an instrument (e.g. Kling, 2006; Maestas et al., 2013; Aizer & Doyle Jr, 2015; Bhuller et al., 2020).

If this variation in the caseworker stringency is exogenous it will allow for causal inference of imposing broader job search requirements. In that case, parameter β_t of Equation 3 can be estimated using instrumental variables (IV) regression where the first stage is defined as:

$$B_{i,0} = \alpha + \gamma Z_{j(i)} + X_i'\delta + \nu_{i,0} \tag{4}$$

where $Z_{j(i)}$ measures the stringency of caseworker j to whom worker i is assigned and X_i includes office-month indicators. Under the assumptions of instrument exogeneity and instrument relevance, β_t in Equation 2 can be interpreted as the causal effect of imposing broader job search requirements to workers on the margin of having the search requirements imposed upon them.

[10] When we assess caseworker stringency as our instrument for broader job search requirements we will also perform a number of specification checks to assess the robustness of the first-stage results to this restriction on the number of meetings per caseworker.

[11] The sample include 461 caseworkers, meaning that each of the 36 UI administration offices has on average 13 caseworkers over the experimental period.

Table 3: Testing for random assignment of unemployed workers to caseworkers

	Explanatory variables		Dependent variables			
			Caseworker stringency		**Pr(Broader search)**	
	Mean	Standard Deviation	Coefficient Estimate	Standard Error	Coefficient Estimate	Standard Error
	(1)	(2)	(3)	(4)	(5)	(6)
Demographics						
Age	39.47	(6.33)	0.0001	(0.0002)	0.002***	(0.001)
Female	0.591	(0.492)	0.0020	(0.0016)	0.054***	(0.005)
Native	0.959	(0.197)	-0.0002	(0.0030)	0.041***	(0.012)
Middle educated	0.520	(0.500)	0.0022	(0.0015)	0.045***	(0.006)
High educated	0.313	(0.464)	0.0039*	(0.0021)	0.066***	(0.007)
Previous employment and benefit eligibility						
Wage (1,000€)	2,335	(1,094)	-0.0006	(0.0007)	0.002	(0.003)
Hours per week	31.28	(9.10)	0.0000	(0.0001)	0.000	(0.000)
Maximum entitlement	87.80	(28.04)	0.0000	(0.0000)	0.001***	(0.000)
Employed at 6 months	0.238	(0.426)	-0.0025	(0.0016)	-0.157***	(0.006)
Sector last job						
Financial	0.24	(0.43)	0.0029	(0.0027)	0.014	(0.009)
Retail and trade	0.20	(0.40)	0.0019	(0.0027)	0.027***	(0.009)
Health care	0.20	(0.40)	0.0026	(0.0027)	-0.005	(0.010)
Temporary employment	0.08	(0.27)	0.0064**	(0.0030)	-0.037***	(0.011)
Transport	0.06	(0.23)	0.0003	(0.0034)	-0.021*	(0.012)
Other	0.14	(0.35)	0.0035	(0.0026)	-0.003	(0.009)
F-statistic for joint test			1.42		88.46	
[p-value]			[.129]		[.000]	
	Number of workers = 44,086			Number of caseworkers = 461		

Note: Baseline sample of worker with a caseworker meeting between their 24th and 36th week of benefit receipt. All estimations control for office x month FEs. Reported F-statistic refers to a joint test of the null hypothesis for all variables listed in the table. The omitted category for education is "Low educated" and for sector of last job it is "Industrial". Standard errors are clustered at the caseworker level. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

For our preferred specification, we define stringency $Z_{j(i)}$ as the leave-out mean assignment rate of a caseworker, meaning that the average assignment rate is calculated over all other meetings by a caseworker than that with worker i. Excluding worker i is necessary to prevent biasing of the instrument due to the inclusion of the worker's own assignment decision. Assuming conditional random assignment and conditional on the characteristics of the worker, $Z_{j(i)}$ is only correlated with the worker's assignment decision if there are systematic differences in assignment rates between caseworkers. As mentioned before, workers are randomly assigned within the available pool of caseworkers at the office level after entering UI and there are large differences in implementation of the experiment and local labor market conditions both between offices, as well as within offices over time. Therefore, it is crucial to condition on fully interacted office-month fixed effects in the empirical analysis. This way, we assure that $Z_{j(i)}$ reflects only the variation in assignment rates between caseworkers and not differences in implementation and local labor market conditions.

Before we proceed to the results, we will first discuss caseworker stringency as our instrument and the assumptions under which we identify the LATE of imposing broader job search requirements on the workers who are susceptive to having these requirements imposed upon them. In the next section we will thus discuss: (i) conditional independence of the instrument and the exclusion restriction, (ii) instrument relevance, and (iii) the monotonicity assumption. The last assumption is mainly important when estimating heterogeneous treatment effects. Our assessment of the instrument is inspired by Bhuller et al. (2020), who test whether judge stringency is an appropriate instrument for incarceration.

4.2 Assessment of caseworker stringency as instrument

Instrument validity – As argued previously, the workers are randomly allocated between the available caseworkers at the office level the moment they are unemployed for six consecutive months. If this assignment is truly random, we must be unable to predict the caseworker stringency using the characteristics of the workers. Column (3) of Table 3 provides support for this hypothesis. We use the same background characteristics as we used to check the randomization of the experiment and find no relation between them and the caseworker stringency, conditional on office and month fixed effects. The p-value of the joint test equals .129 and only the coefficients of *high educated* and the *temporary employment sector* are significant at the 5 or 10 percent level – finding two (marginally) significant coefficients could easily be chance when testing for this many variables. These results are consistent with conditional random assignment after correcting for the assignment process, hence we claim that the (conditional) independence assumption is satisfied.

Conditional random assignment is sufficient for causal interpretation of the first-stage estimates. However, for causality in the second stage, it is also required that the caseworker stringency only affects the workers' outcomes indirectly by increasing the probability of getting the broader job search requirements imposed upon them. One possible threat to the exclusion restriction is that our instrument is correlated with other policy choices made by the caseworker.[12] For example, caseworker that impose the search requirements more often might also choose to assign other policy tools more regularly. In similar vein, caseworkers who are stricter when imposing the search requirements might also be more strict concerning sanctions or more reluctant to exempt workers from their search requirements.

Although the exclusion restriction is not directly testable, we do investigate whether there is a relation between the broader job search requirements and other policy tools assigned during the first six months after the meeting. By doing so, we follow Arni & Schiprowski (2019) who test whether caseworker stringency is an appropriate instrument for individual search requirements. We differentiate between participation in workshops, whether sanctions are imposed and whether search exemptions are allowed. From Table 4 we conclude that – perhaps somewhat surprising – all three policy tools are positively related to the broader search requirements, even when we control for office and month fixed effects. This is, however, only problematic for our identification approach if these relations exist through the caseworker. As a logical next step we check if there is a relation between our instrument – caseworker stringency – and the other policy tools. Columns (5) and (6) of the table confirm that there is no significant relation between them. In other words, workers imposed with broader job search requirements are also more often participating in workshops, imposed with sanctions and allowed an exemption from their search based on their own characteristics and not as result of more stringent caseworkers

[12] Another possible threat to the exclusion restriction is that our instrument is correlated with the quality of the caseworker. We therefore test whether our instrument correlates with the un-/employment of the workers who entered UI in the six months before the experiment or the workers that were excluded from the treatment based on their age (those older than 50). These reduced-form estimates are all insignificant, however we have to point out that the standard errors are quite large due to small samples.

being more strict on other dimensions as well.

As a second test, we check whether the experience of the caseworkers affect the outcomes. We test this by including several measures of caseworker experience to our IV model. This test – which we will discuss in more detail in section 5 – further supports the exclusion restriction, as the additional controls do not change our findings.[13] Taken together, we find no reason to assume that the exclusion restriction might be violated.

Table 4: Relation of other policy tools to the broader search requirements and caseworker stringency

	Explanatory variables		Dependent variables			
	Mean	S.d.	Pr(Broader search)		Caseworker stringency	
	(1)	(2)	(3)	(4)	(5)	(6)
Participation workshop	0.034	(0.181)	0.088***	0.119***	-0.007	0.004
			(0.014)	(0.012)	(0.007)	(0.005)
Sanction	0.075	(0.263)	0.057***	0.065***	0.000	0.001
			(0.008)	(0.008)	(0.003)	(0.002)
Search exemption	0.188	(0.391)	0.084***	0.077***	0.003	0.001
			(0.006)	(0.005)	(0.003)	(0.001)
Office x month FEs			—	✓	—	✓
	Number of workers = 44,086		Number of caseworkers = 461			

Note: The standard errors in parenthesis in columns (3-6) are clustered at the caseworker level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

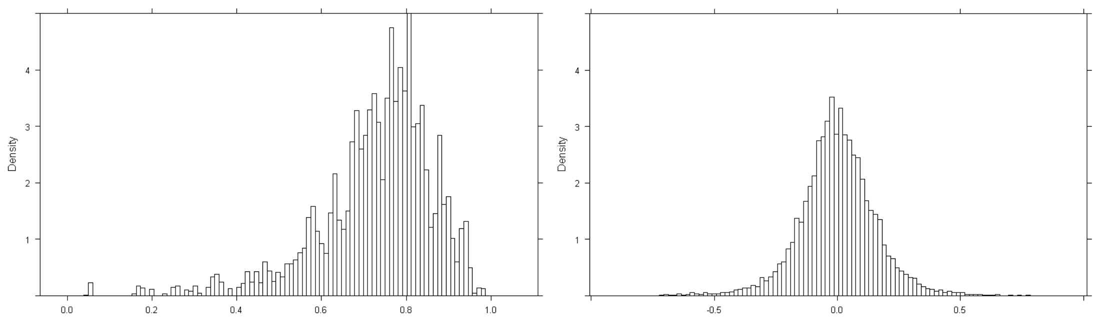
Monotonicity – Instrument independence and exogeneity are sufficient for causal inference of the IV estimates if the effects are constant across workers. However, since we are also interested in heterogeneous effects we must also validate the monotonicity assumption to be able to interpret these effects as causal as well. Monotonicity implies that workers who have the broader job search requirements assigned to them by a lenient caseworker would also have had that by a stricter caseworker, and vice versa for those who did not have the search requirements assigned to them.

[13] Additionally, we checked whether the predetermined measures of caseworker experience are correlated with our instrument. This test showed no significant relation between them.

De Chaisemartin (2017) debates that this condition is often implausible, for example in studies where randomly assigned judges are used as an instrument. The reason for this is that judge A can be more stringent towards defendants with specific characteristics than judge B, even if judge A is on average less strict. Although we are using a similar approach, we are less concerned about a violation of the monotonicity condition in our application. Reason for this is that we are under the presumption that most of the variation in average assignment rates can be explained by differences in how the instructions are communicated to and interpreted by the caseworkers and their beliefs in the effectiveness of assigning broader search requirements in general, rather than its effectiveness for certain sub-samples. Nevertheless, we need to be cautious when interpreting the results of the heterogeneity analysis.

Unfortunately, whether workers who have broader job search requirements assigned to them by a lenient caseworker would also have had that by a stricter caseworker and vice versa is not formally testable. However, another implication of the monotonicity assumption is that caseworkers who are strict on one type of unemployed workers, e.g. females, should also be strict on any other type, e.g. males. This implication is testable as we can check whether the first-stage coefficients of different subgroups are all non-negative ($\gamma_i > 0 \ \forall i$). We do this by constructing the instrument on the baseline sample of workers, but estimating the first stage on the different sub-samples. The first-stage results for the different sub-samples and whether the monotonicity assumption holds will be discussed in the next part, after examining the first stage for the full sample.

Figure 3: Caseworker stringency (left panel) and demeaned by UI administration office and month (right panel)



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

Instrument relevance – Figure 3 displays the distribution of the caseworker specific stringency, both unconditional and conditional on office-month interactions. Overall, there is quite some variation in the unconditional caseworker stringency (left panel). For instance, a caseworker at the 10th percentile assigns in 54 percent of the meetings the broader job search requirements to the worker, while that is 88 percent for a caseworker at the 90th percentile. When we condition on office-month interactions, there is still more than enough variation in the caseworker stringency – the standard deviation of the conditional instrument is 0.152 (weighted by the number of meetings per caseworker). The remaining variation depicts that some caseworkers are stricter than others, resulting in higher assignment rates conditional on worker characteristics.

Table 5 presents the average assignment rates and the first-stage estimates using the leave-out mean instrument of the full sample and the different sub-samples, respectively. The first-stage estimate for the full sample is highly significant and shows that the caseworker stringency has large predictive power ($F = 1,582$).[14] The coefficient of 0.8 suggests that being assigned to a caseworker with a 10 percentage points higher overall stringency increases the probability of getting the broader job search requirements assigned with 8 percentage points on average. Table A.11 in the appendix shows that this first-stage result does not change substantially if we include or exclude caseworkers with few or many meetings, or when including additional control variables. In Table A.12 we show the robustness of the results to an alternative approach to measure the caseworker stringency, namely the split-sample approach. All first-stage coefficients when using the split-sample approach are roughly 10 percent smaller compared to those using the leave-out mean approach. This is consistent with more noise and a bias towards zero due to smaller sample sizes. These results are consistent with random assignment of workers to caseworkers within UI administration offices.

Next, we check whether there are differences in average assignment rates and the influence of the caseworker stringency on the assignment probabilities between

[14] In the case of one endogenous regressor and one instrument the incremental F-statistic matches the squared t-statistic.

Table 5: First-stage results by demographics

	N	Dependent Mean	coefficient	s.e.	t-value	Relative Likelihood†
	(1)	(2)	(3)	(4)	(5)	(6)
Full sample						
Full sample	44,086	0.713	0.812	(0.020)	40	1.000
Gender						
Female	26.069	0.736	0.807	(0.027)	29	0.994
Male	18.095	0.680	0.807	(0.027)	30	0.994
Nationality						
Native	42,294	0.716	0.810	(0.021)	38	0.998
Non-native	1,870	0.636	0.931	(0.122)	8	1.147
Educational level						
Low educated	7,358	0.655	0.845	(0.047)	18	1.041
Middle educated	22,991	0.710	0.805	(0.028)	29	0.992
High Educated	13,815	0.748	0.803	(0.037)	22	0.989
Age						
Younger than 40	21,038	0.692	0.831	(0.029)	29	1.024
Older than 40	23,126	0.732	0.800	(0.027)	29	0.986
Employment status						
Not employed at 6 months	33,681	0.751	0.797	(0.024)	33	0.982
Employed at 6 months	10,483	0.590	0.826	(0.043)	19	1.018

Note: All estimations control for office x month FEs. Standard errors are clustered at the caseworker level. †The relative likelihood is the ratio of the sample-specific first-stage coefficient to the coefficient of the full sample.

sub-samples. Column (2) shows that e.g. female, native, higher educated and older workers, and those without some labor income during their unemployment get more often broader job search requirements imposed upon them during the caseworker meeting. That the average assignment rate is increasing in the education level of workers is in line with the idea that workers who can potentially expand their search most also face higher assignment rates. However, to check for which sub-samples the caseworker stringency is most relevant, we compare the sub-sample specific coefficient to the coefficient of the full sample (following Abadie, 2003). This ratio provides the relative likelihood that workers with certain characteristic are assigned broader search requirements if they were assigned to a more stringent caseworker. The likelihoods in column (6) show that the first-stage coefficient is larger for non-natives, lower educated and younger workers. In other words, the assignment decision of the workers with these characteristics is most likely to change if the worker is assigned to a more stringent caseworker. However, the relative likelihoods of all sub-samples are close to 1, meaning that stricter caseworkers are in general more strict to all workers. That the first-stage coefficients are very comparable across the sub-samples also means that they are all non-negative and significant for all sub-samples.[15] Hence, we find no evidence that the monotonicity assumption as discussed in the previous section does not hold.

4.3 Identification of the compliers

It is of substantial policy interest to know the proportion and characteristics of the workers for whom the assignment of the broader job search requirements depends on the caseworker they are randomly assigned to – hence, the compliers. It is the same group of workers for which the average causal effect is identified (Imbens & Angrist, 1994). To identify the proportion of compliers we will follow the back-of-the-envelope calculation proposed by Maestas et al. (2013). The proportion of compliers can be derived by multiplying the first-stage coefficient with the range of assignment rates of the caseworkers. As displayed in Figure 3, the assignment rates cover virtually the complete interval: from 0.06 to 0.98. Accordingly, 74 ($= 0.81 * 0.92$) percent of the workers can be considered compliers. Given an average assignment rate of 71 percent (see Table 5), this means that 53 percent are imposed broader job search requirements only based on being assigned to a strict caseworker, while 21 percent are not imposed with the search requirements as they are assigned to a lenient caseworker. Although 74 percent can be considered compliers, it has to be stressed that this does not mean that all of them would have a different outcome if assigned

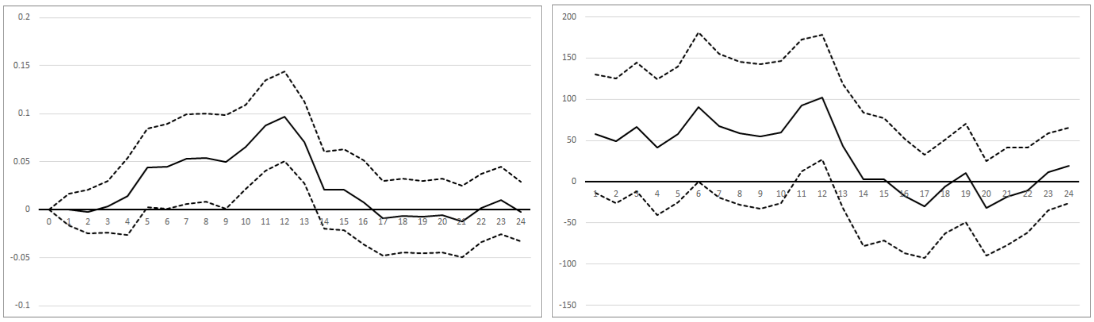
[15] In Table A.13 in the appendix we also show the first-stage estimates using the reverse-sample instrument which excludes own-type individuals, e.g. the instrument for women is constructed based on men only. Again, the first-stage coefficients are all positive and significantly different from zero. That the coefficients for *native* and *unemployed* workers are considerably smaller than the estimates using the leave-out mean instrument in Table 5 can be explained by the limited sample size of the sub-samples on which the reverse-sample instrument is constructed.

to different caseworkers. For example, if all workers were assigned to a caseworker with the average stringency of their UI administration office, only a little over 6 percent would have a new outcome.[16]

To discover the characteristics of the compliers, we can again use the first-stage coefficients of the sub-samples stratified by background characteristics as reported in Table 5.[17] Specifically, sub-samples of workers who are more likely to be compliers have higher first-stage coefficients. As discussed before, all relative likelihoods are close to 1, indicating that all different sub-samples are almost proportionally represented among the compliers. The group with a marginally higher relative likelihood is that of the non-native workers; however, this group is limited in size and thus accounts still only for a small fraction of the compliers.

5 Effects of broader job search requirements

Figure 4: Estimated effects of assigning broader search requirements to unemployed workers on their unemployment probability and benefits



(a) Unemployment probability **(b)** UI benefits

Note: Dotted lines display the 95% confidence interval, based on standard errors clustered at the caseworker level.

5.1 Effects on UI benefit receipt

Using the sample of workers who attended a caseworker meeting in the treatment group, Figure 4 shows the effects of broader job search requirements on the unem-

[16]This is derived by multiplying the fraction of compliers by the mean absolute deviation of $Z_{j(i)}$ (conditional on fully interacted office and month fixed effects) which is 0.086.

[17]With this approach to identify the compliers we follow Abadie (2003).

ployment probability and UI benefits in the two years succeeding the caseworker meeting. From panel (a) we observe that workers imposed with the search requirements stay unemployed for a longer period, with a maximum difference of 10 percentage points one year after the meeting.[18] After this point, the effect promptly disappears and remains virtually zero thereafter. This rapid decline of the effect 12 months after the meeting can only partly be explained by higher job finding rates among the workers with the search requirements imposed upon them. Both the size and the swift nature of the decline, as well as the timing suggest that another factor plays a role in it. It is plausible that the convergence in unemployment rates of workers with and without the search requirements imposed is induced by an increased outflow from UI among both groups of workers as they have exhausted their benefits. The workers in our sample are on average entitled to 18 months of benefits. Hence the timing of the decline coincides with the exhaustion of benefits 12 months after the meeting (roughly 18 months after the start of the unemployment spell). Altogether, the broader job search requirements increase the unemployment duration on average with 2,5 weeks (see Table 6 for the aggregate outcomes). On the contrary, it is somewhat reassuring that the search requirements have not led to large flows out of claimant status of workers without a job, as observed in the case of the UK with the introduction of much larger hurdles (Manning, 2009; Petrongolo, 2009).

Panel (b) repeats the same exercise, but this time for UI benefits. We see that the benefit receipt is increased during the first 12 months. As workers who are imposed with the search requirements are depending on UI benefits for a longer period, it is not surprisingly that they also receive more benefits over time. In total, the effects add up to an increase of UI payments of €800 ($1,000) per person 12 months after the meeting and remains constant thereafter.

[18] To put the size of the effect in perspective, recall that the (negative) effect that we find for caseworker meetings is at most 4 percentage points.

Table 6: The effect of broader search requirements on time in un-/employment and cumulative benefits/earnings

Dependent variable:	Unemployment duration	Benefits	Employment duration	Earnings
	(1)	(2)	(3)	(4)
1 Year after meeting	2.20***	800**	-1.32*	-724
	(0.85)	(381)	(0.78)	(503)
Dependent mean	36.97	10,719	27.93	10,952
2 Years after meeting	2.56*	776	-1.70	-1,622
	(1.43)	(554)	(1.52)	(1,090)
Dependent mean	50.93	14,198	63.87	28,093
Number of workers	44,086			

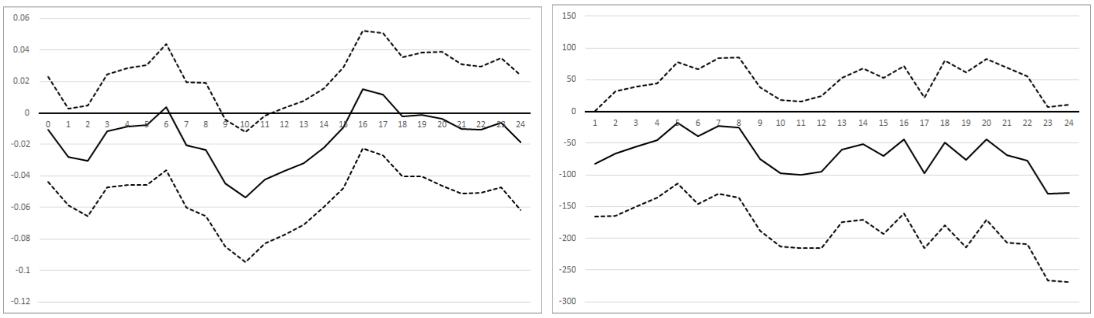
Note: The un-/employment duration is measured in weeks. All estimations control for office x month FEs and control variables. Standard errors in parenthesis are clustered at the caseworker level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

5.2 Effects on employment and job characteristics

With large effects on the unemployment duration and UI benefits, we now turn to the effects on employment and earnings. Figure 5 shows the resulting graphs for these outcomes. In line with expectations, the general patterns found for the unemployment probability and benefits are mirrored in these graphs. In panel (a) we see that workers who are imposed with broader job search requirements are less likely to work than comparable workers with a meeting during the first year, implying that the search requirements delay re-employment. This negative effect is again quite sizable with a maximum difference between workers with and without imposed broader search requirements of almost 6 percentage points. In total, the search requirements decrease the average employment duration with roughly 1,5 weeks – see column (3) of Table 6.

Naturally, the averse effects on employment in the first year following the caseworker meeting are also reflected in the earnings of workers, adding up to roughly €750 ($900). Interestingly, the effect on earnings does not shrink in the second year, while the effect on employment is virtually zero at the time. This results in a cumulative decline in earnings of already €1,600 ($1,900) after two years. Despite

Figure 5: Estimated effects of assigning broader search criteria to unemployed workers on their employment probability and earnings



(a) Employment probability **(b)** Earnings

Note: Dotted lines display the 95% confidence interval, based on standard errors clustered at the caseworker level.

the fact that the decline in earnings is quite sizable, the aggregate outcomes are not significant at the conventional levels. As the increase in UI benefits equals roughly €800, about half of the decline in earnings is compensated by the increase in benefits. However, this should be interpreted as a lower bound as the effect on earnings remains negative up till at least two years after the meeting.

The lower earnings in the second year after the meeting indicates that not only the job finding of workers is delayed, but also that the search requirements had long-lasting effects on the intensive margin or wages of the workers. As a logical next step, we investigate the effect of the search requirements on the observed job characteristics. Specifically, we consider the workers' earnings, hourly wages, weekly hours worked, contract types and the degree to which they switch between sectors. Table 7 shows that workers indeed find different jobs when imposed with the broader job search requirements, suggesting that the search requirements affected their search strategy. What stands out is that the lower earnings are fully confined to a decline in the intensive margin. The workers that have to search more broadly work roughly two hours less per week two years after the meeting, which translates to a decline of almost 10 percent. This effect resembles the percentage decrease in earnings. Similarly, the probability of a permanent contract is 20 percent (5.7 percentage points) lower among the workers who are imposed with the search requirements. Lastly, as these workers are also encouraged to switch between sectors, we also verify whether that happens more frequently among them. Column (5) shows that the opposite is actually true, the probability of a switch between sectors is lower among these workers. However, this decline is largely explained by the lower employment rates among these workers.

Table 7: The effect of broader search requirements on job characteristics

Dependent variable:	Earnings	Hourly wage	Hours worked	Permanent contract	Different sector
	(1)	(2)	(3)	(4)	(5)
1 Year after meeting	-95	-0.212	-1.574**	0.007	-0.052**
	(61)	(2.668)	(0.780)	(0.017)	(0.020)
Dependent mean	1,251	11.18	17.28	0.15	0.52
2 Years after meeting	-129*	0.023	-1.754**	-0.055***	-0.019
	(71)	(0.766)	(0.758)	(0.021)	(0.025)
Dependent mean	1,536	12.34	20.38	0.27	0.58
Number of workers			44,086		

Note: All estimations control for office x month FEs and control variables. Standard errors are clustered at the caseworker level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

5.3 Heterogeneous effects

There are several reasons to assume heterogeneous treatment effects of imposing broader job search requirements. For instance, higher educated workers could increase their scope of search more than their counterparts as they are expected to start out with higher reservation wages. As a final step, we therefore re-estimate the effect of imposing broader job search requirements on employment for samples stratified by gender, educational level (low, middle and high educated) and age groups (younger than 40 vs. 40-49). The estimated effects of these groups are all shown in Table A.14 in the appendix – both one and two years after the caseworker meeting.

We find much heterogeneity across the stratified groups. So do we find that males respond more extremely to the search requirements and are the driving factor behind the lower employment rates among workers with the search requirements imposed.[19] Males who are imposed with the search requirements are 8 percentage points less likely to work one year after the meeting than other males and this difference remains the same for at least another year.

Next, we find that the effects of the broader job search requirements are non-monotonous in the educational level of workers. The largest (negative) effects are among workers with a middle educational level, while there are no significantly negative effects for both lower and higher educated. On the contrary, we find positive – but insignificant – estimates for higher educated workers. With that, higher educated workers seem to be the only group for which the search requirements might increase re-employment. More favorable effects among higher educated workers is in line with expectations as higher educated workers could lower their reservation wages to a larger extend.

Finally, we stratify our sample by the age of the unemployed workers. Here we find that the negative effects of the search requirements are larger for younger workers two years after the meeting. At that time, younger workers are 6 percentage points less like to work than workers of the same age if imposed with the search requirements. This difference in treatment effects between younger and older workers cannot be explained by differences in average employment rates as those are identical two years after the meeting.

5.4 Decomposing the effects of caseworker meetings

$$T = a(pY^*_{11} + (1-p)Y^*_{10}) + (1-a)Y^*_0 \tag{5}$$

$$C = b(qY^*_{11} + (1-q)Y^*_{10}) + (1-b)Y^*_0 \tag{6}$$

$$\beta_t = Y^*_{11} - Y^*_{10} \tag{7}$$

$$T = ap\beta_t + aY^*_{10} + (1-a)Y^*_0 \tag{8}$$

[19] Card et al. (2018) show that active labor market programs have on average larger (positive) impacts for females than for males.

$$C = bq\beta_t + bY^*_{10} + (1-b)Y^*_0 \tag{9}$$

$$\delta_t = \frac{T-C}{a-b} = \frac{ap-bq}{a-b}\beta_t + Y^*_{10} - Y^*_0 \tag{10}$$

$$Y^*_{10} - Y^*_0 = \delta_t - \frac{ap-bq}{a-b}\beta_t \tag{11}$$

Table 8

Dependent variable:	Unemployment duration	Benefits	Employment duration	Earnings
	(1)	(2)	(3)	(4)
1 Year after meeting				
Combined effect	-1.41	-879	0.9	379
Effect search requirement	2.2	800	-1.32	-724
Effect caseworker meeting	-3.74	-1,725	2.30	1,145
2 Years after meeting				
Combined effect	-1.84	-1202	1.15	265
Effect search requirement	2.56	776	-1.7	-1622
Effect caseworker meeting	-4.55	-2,023	2.95	1,980

Note: The un-/employment duration is measured in weeks.

5.5 Robustness checks

As discussed in section 4, caseworker characteristics or other policy choices made by the caseworkers could potentially result in a violation of the exclusion restriction and bias our results. Although we did find that the other policy choices made by the caseworker are unrelated to our instrument, we provide here some additional evidence in support of the exclusion restriction. In Table 9 we show the sensitivity of our main results to the inclusion of different measures of caseworker experience and the inclusion of the leave-out means of the three most important other policy tools used by the caseworkers.

For convenience, we included the baseline results in the first row of the table. In row 2, we include two predetermined measures of caseworker experience to our model, namely the number of meetings and the time that the caseworker was active in the two years preceding the experiment. The inclusion of these variables do not change our main findings. In row 3, we additionally include the leave-out means of the workshops, sanctions and search exemptions. Again, the results remain virtually unaffected. The fact that the results with additional control variables are almost identical to the baseline results support the idea that the caseworkers do not affect the job search of workers imposed with broader job search requirements differently in any other way than by the assignment of these search requirements.

Table 9: Sensitivity analysis for the influence of caseworker experience and other policy tools

Dependent variable:	Unemployment duration	Benefits	Employment duration	Earnings
	(1)	(2)	(3)	(4)
Baseline	2.20***	800**	-1.32*	-724
	(0.85)	(381)	(0.78)	(503)
Add caseworker experience	2.14**	774**	-1.31*	-776
	(0.85)	(381)	(0.79)	(505)
Add other policy tools	2.13**	778**	-1.36*	-773
	(0.86)	(386)	(0.79)	(507)
Dependent mean	36.97	10,719	27.93	10,952
Number of workers		44,086		

Note: All outcome variables are measured 1 year after the caseworker meeting. The un-/employment duration is measured in weeks. Caseworker experience is proxied by the activity of caseworkers in the two pre-experiment years (April 2013 until March 2015). We use the number of persons met (divided by 100) (mean=1.6) and the number of years which the caseworker was active at the UI administration during the pre-sample proxied by the time difference between the first and last observed meeting (mean=1.0). Other policy controls include leave-out means of the policy tools: participation in workshops, sanctions and search exemptions. All estimations control for office x month FEs and control variables. Standard errors are clustered at the caseworker level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

6 Conclusion

In this paper we exploit a large-scale randomized controlled trial conducted among more than 130,000 unemployed workers in the Netherlands. After six consecutive months of unemployment, workers in the treatment group got broader job search requirements imposed during a caseworker meeting, which were backed up with (financial) sanctions in the case of non-compliance. The same rules applied to workers in the control group, however, they were not actively enforced in a caseworker meeting. Treated workers were expected to search for jobs that are below their educational level, that are in a different sector, with a lower wage or that have a longer commuting time. Despite large compliance to the experiment, the broader job search requirements were not assigned in all meetings, but instead imposed upon a non-random sample of the unemployed workers. As a result, there were meetings in which the search requirements were imposed and meetings in which this was not the case.

To isolate the causal effects of the broader job search requirements, we exploit exogenous variation in the assignment of the search requirements during caseworker meetings stemming from quasi-random assignment of the unemployed to caseworkers and exogenous variation in assignment rates across caseworkers. Accordingly, we use the leave-out mean caseworker stringency as an instrument for the broader job search requirements. Following this approach we find that, contrary to the caseworker meetings on themselves, imposing broader job search requirements increases the unemployment duration and delays the job finding of the workers. Additionally, workers with the requirements imposed upon them work less hours and have more often a temporary contract. As a result, they have almost €1,600 lower earnings and roughly €800 lower income than their counterparts two years after the meeting. This loss of income is expected to continue to grow after the first two years due to long-lasting effects on earnings.

The averse effects of imposing broader job search requirements clearly dominate the beneficial effects. From this we conclude that the search requirements work as constraints on the job search of unemployed workers, as they have to adapt their search strategy to avoid financial sanctions. This results in sub-optimal search strategies and worse labor market outcomes, compared to a situation in which the workers experience more freedom to search for the jobs they prefer most and of which they predict their chances on success to be high. This effect might be strengthened by a lower conversion rate of job interviews into employment when a broader set of jobs is considered, for example when workers do not have the required task competences.[20] It is also not unthinkable that workers are demotivated due to the stricter requirements and a loss of experienced autonomy (Koen et al., 2016).

Our findings advocate against imposing strict search requirements upon unemployed workers concerning their scope of search to stimulate employment, contrary to the results of a simple comparison between the treatment and control group might first suggest. Our findings show that for the majority of the workers the potentially positive effects of informing unemployed workers about the benefits of a broader search strategy are more than out-weighted by the negative effects of constraining their job search. Therefore, a more considerate approach focused on information provision or nudging seems more promising and can provided at a low cost, as shown by for example Altmann et al. (2018) and Belot et al. (2019).

[20] Belot et al. (2019) do not find significant effects on employment, even though they find a significant increase in the number of job interviews. They argue that this can be explained by the lack of power or by lower conversion rates of job interviews into employment when a broader set of jobs is considered.

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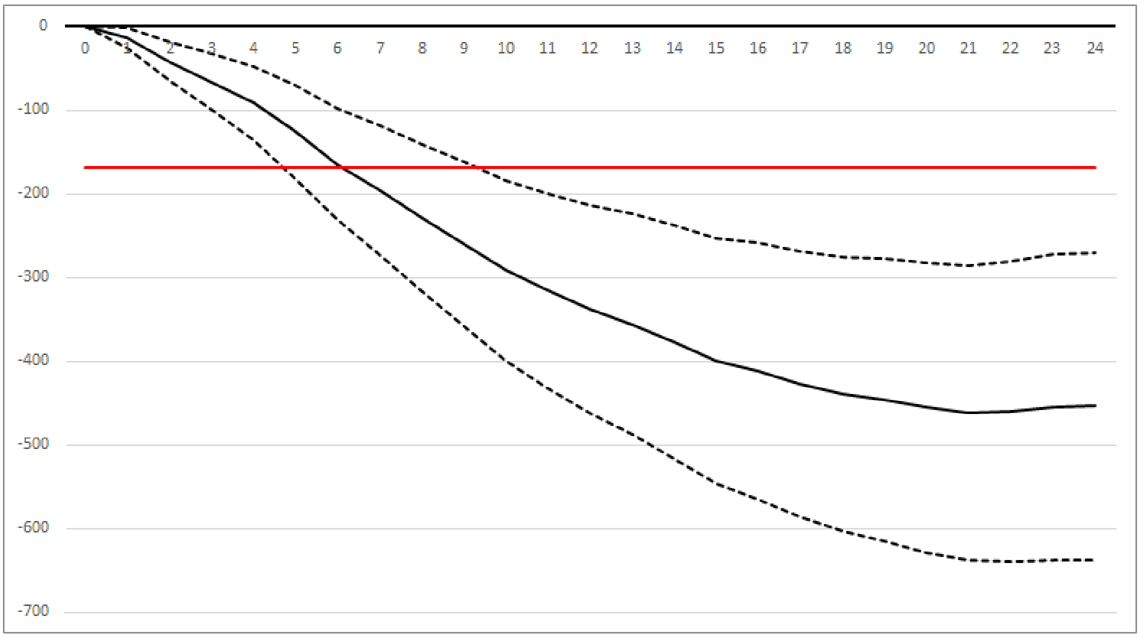
A Appendix

Table A.10: The effect of caseworker meetings on time in un-/employment and cumulative benefits/earnings

Dependent variable:	Unemployment duration	Benefits	Employment duration	Earnings
	(1)	(2)	(3)	(4)
1 Year after meeting	-1.41***	-879***	0.90**	379
	(0.34)	(174)	(0.43)	(291)
Dependent mean	36.76	10,357	28.24	11,635
2 Years after meeting	-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: The un-/employment duration is measured in weeks. All estimations include control variables. Robust standard errors in parenthesis. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Figure A.6: Estimated intention to treat effects of a caseworker meeting on cumulative UI benefits



Note: The intention to treat (ITT) effects on monthly UI benefits are estimated for the full experiment sample using the reduced form Equation 2. We assume a conservative discount rate δ of 5 percent. Accordingly, we estimate: $Y_t = \sum_{t=1}^{t=T}(1+\delta)^{-t/12}ITT_t$. This estimate is an underestimation of the true ITT effect, as we observe non-compliance in the control group. Dotted lines display the 95% confidence interval, based on robust standard errors. The red line displays the average costs per treated worker, calculated by dividing the total lump-sum costs of the experiment (20 million euros) by the number of workers in the treatment group ($N = 118,697$).

Table A.11: Leave-out mean first-stage results by different selection criteria

Sample selection	40-400	50-400†	60-400	50-300	50-500
	(1)	(2)	(3)	(4)	(5)
Dependent variable:			**Pr(Broader search)**		
Panel A. UI administration office X month fixed effects					
Caseworker stringency	0.794***	0.812***	0.817***	0.812***	0.810***
	(0.019)	(0.020)	(0.023)	(0.020)	(0.020)
F-stat. (Instrument)	1815	1582	1235	1590	1576
Panel B. Add demographic controls					
Caseworker stringency	0.789***	0.807***	0.811***	0.807***	0.805***
	(0.019)	(0.020)	(0.023)	(0.020)	(0.020)
F-stat. (Instrument)	1793	1562	1217	1570	1556
Panel C. Add employment history and benefit eligibility controls					
Caseworker stringency	0.786***	0.803***	0.0.809***	0.804***	0.802***
	(0.019)	(0.020)	(0.023)	(0.020)	(0.020)
F-stat. (Instrument)	1776	1562	1213	1579	1548

Note: Standard errors in the parenthesis are clustered at the caseworker level. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$ †The main analysis utilizes caseworkers with 50-400 meetings with workers from the experiment population in the experimental period.

Table A.12: Split-sample first-stage results by different selection criteria

Sample selection	40-400	50-400	60-400	50-300	50-500
	(1)	(2)	(3)	(4)	(5)
Dependent variable:			**Pr(Broader search)**		
Panel A. UI administration office X month fixed effects					
Caseworker stringency	0.723***	0.720***	0.742***	0.720***	0.721***
	(0.032)	(0.036)	(0.041)	(0.036)	(0.036)
F-stat. (Instrument)	525	400	322	396	411
Panel B. Add demographic controls					
Caseworker stringency	0.719***	0.718***	0.739***	0.718***	0.719***
	(0.032)	(0.036)	(0.041)	(0.036)	(0.036)
F-stat. (Instrument)	519	397	319	394	408
Panel C. Add employment history and benefit eligibility controls					
Caseworker stringency	0.715***	0.714***	0.0.734***	0.715***	0.715***
	(0.031)	(0.035)	(0.040)	(0.035)	(0.035)
F-stat. (Instrument)	538	415	335	414	426

Note: The sample is randomly split in two based on the anonymized identification numbers. The first half is used to determine the instrument which is used in the estimation using the second half. Standard errors in the parenthesis are clustered at the caseworker level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.13: Reverse-sample first-stage results by demographics

	N	Dependent Mean	coefficient	s.e.	t-value
	(1)	(2)	(3)	(4)	(5)
Gender					
Female	26.069	0.736	0.676	(0.039)	18
Male	18.095	0.680	0.805	(0.030)	27
Nationality					
Native	42,294	0.716	0.149	(0.023)	7
Non-native	1,870	0.636	0.919	(0.123)	7
Educational level					
Low educated	7,358	0.655	0.858	(0.053)	16
Middle educated	22,991	0.710	0.697	(0.038)	18
High Educated	13,815	0.748	0.758	(0.042)	18
Age					
Younger than 40	21,038	0.692	0.752	(0.039)	19
Older than 40	23,126	0.732	0.693	(0.035)	20
Employment status					
Not employed at 6 months	33,681	0.751	0.482	(0.036)	14
Employed at 6 months	10,483	0.590	0.818	(0.053)	15

Note: The reverse-sample instrument is constructed based on the out-of-sample individuals. All estimations control for office x month FEs. Standard errors in parenthesis are clustered at the caseworker level.

Table A.14: LATE effects of imposing broader search requirements on employment probability stratified by demographics

Dependent variable:	Employed after 1 year	Employed after 2 years
	(1)	(2)
Full sample	-0.038	-0.019
	(0.024)	(0.023)
Dependent mean	0.65	0.71
Female	-0.018	0.013
	(0.031)	(0.030)
Dependent mean	0.65	0.72
Male	-0.081**	-0.084**
	(0.039)	(0.038)
Dependent mean	0.65	0.71
Low educated	-0.006	-0.034
	(0.058)	(0.057)
Dependent mean	0.62	0.69
Middle educated	-0.098***	-0.058*
	(0.033)	(0.032)
Dependent mean	0.67	0.73
High Educated	0.030	0.066
	(0.044)	(0.043)
Dependent mean	0.63	0.70
Younger than 40	-0.033	-0.057*
	(0.033)	(0.033)
Dependent mean	0.69	0.72
Older than 40	-0.036	0.020
	(0.035)	(0.033)
Dependent mean	0.61	0.71

Note: All estimations control for office x month FEs and control variables. Standard errors in the parenthesis are clustered at the caseworker level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$