

DISCUSSION PAPER SERIES

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ABSTRACT

Do Caseworker Meetings Prevent Unemployment? Evidence from a Field Experiment*

Caseworker meetings have been shown to accelerate exit from unemployment. We explore whether they are also effectual before entering unemployment. In a natural field experiment, we offer caseworker meetings to workers at risk of losing their jobs while they are still employed. We find that the offer induces additional meetings and substantially shifts the first meeting forward but has no effect on entry into unemployment or on labour market outcomes within one year. The intervention does not alter jobseekers' search behaviour, which likely explains its inefficacy.

JEL Classification: J68, J63, J62

Keywords: job search assistance, caseworker meetings, job search, field experiment

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1. Introduction

Job destruction and worker reallocation are important ingredients for economic growth (e.g., [Aghion et al., 2016](#), [Restuccia and Rogerson, 2017](#)). At the same time, job loss and unemployment harm affected workers, as they suffer from lower income (e.g., [Jacobson et al., 1993](#), [Fackler et al., 2021](#)), worse mental health (e.g., [Kuhn et al., 2009](#), [Cygan-Rehm et al., 2017](#)) and higher mortality (e.g., [Sullivan and von Wachter, 2009](#), [Eliason and Storrie, 2009](#)). To support these workers and facilitate the reallocation of resources, governments worldwide provide a variety of active labour market programmes. These programmes typically start some time after workers become unemployed, even though many workers know in advance that they will lose their jobs, for instance, because of notice periods. Actors such as the [OECD \(2018, Chapter 4\)](#) and the [European Commission \(2018\)](#) have pushed for the use of such anticipation periods for early, or ideally preventive, interventions. However, despite the abundant literature on active labour market policies for the unemployed (for an overview, see [Card et al., 2018](#)), we lack credible evidence on the feasibility and efficacy of preventive interventions.

To address this gap, we contribute evidence from a natural field experiment ([Harrison and List, 2004](#)) on preventive caseworker meetings as a specific form of job search assistance. [Schiprowski \(2020\)](#) shows that cancelling one caseworker meeting substantially deteriorates the labour market trajectories of unemployed workers. As the counselling provided in caseworker meetings could benefit persons who are still employed, such meetings are a promising preventive intervention. We implemented such an intervention in Germany, where workers who are at risk of losing their jobs have to register with the employment agency as seeking a new job up to three months before the expected end of their current job. Similar requirements would be feasible in many developed countries as notice periods are typically substantial and often at least as long as in Germany.¹

¹Considering workers with four years of tenure, 26 out of 36 countries included in the overview by [OECD \(2020, Chapter 3\)](#) have notice periods at least as long as that in Germany. For further details, see the OECD Indicators of Employment Protection available at <https://www.oecd.org/employment/emp/oecdindicatorsofemploymentprotection.htm>. The US is the only country without a general requirement of advance notice for these workers, though the Worker Adjustment and Retraining Notification

In our field experiment, jobseekers who registered at least one month before the expected end of their jobs were randomly assigned to two groups.² The “preventive meeting” group was invited to meet their caseworkers as soon as possible, i.e., well before the expected end of their jobs, whereas the “late meeting” group was invited to meet their caseworkers shortly before they actually become unemployed. This experiment uncovers the effect of offering preventive meetings on the number of caseworker meetings and on later employment outcomes, i.e., the intention-to-treat effect of preventive meetings.

We implemented the field experiment for about 10,500 jobseekers who registered from May to December 2018 at four local employment agencies across Germany. At the time, employment agencies regularly invited jobseekers to an initial meeting while they were still employed, but this was not required by law. Jobseekers are not obliged to have such a meeting and not all jobseekers attend initial meetings while they are still employed. Hence, both treatments were widespread before we fielded the experiment. After the experiment was conducted but before the results were available, Germany amended its social code. Since 2022, employment agencies have been required to offer an initial caseworker meeting upon registration as a jobseeker, but the meetings are still not mandatory for jobseekers while they are employed. The current regulation thus resembles the “preventive meeting” treatment.

Offering preventive meetings raised the probability that jobseekers have at least one caseworker meeting by 16 percentage points (baseline: 50%) and increased the number of meetings held within one year of registration by 0.3 meetings (baseline: 1.2 meetings). However, such an offer neither prevented entry into unemployment nor improved jobseekers’ later labour market outcomes. This pattern held true across various sociodemographic groups. Assuming that offering preventive meetings affects labour market outcomes only via actual meetings, our estimates practically rule out that the effects of these additional meetings are as large as those previously documented for meetings of

(WARN) Act mandates advance notice for mass layoffs. Notice periods are generally found to benefit workers; see [Addison and Blackburn \(1994\)](#) for a review of the effects of the WARN Act and [Cederlöf et al. \(2021\)](#) for recent results from Sweden.

²Throughout the paper, we refer to workers who register as seeking a job while still employed as *jobseekers*.

caseworkers with unemployed persons in Switzerland by [Schiprowski \(2020\)](#).

We documented two reasons for the absence of effects. First, 42% of the jobseekers in both groups did not lose their jobs within half a year of their registration (in line with [Stephan, 2016](#)), e.g., because fixed-term contracts were renewed. The additional meetings accrued equally to jobseekers who lost their jobs and those who did not. Not finding an effect for workers who kept their jobs is not surprising, but we also find no effects for those who lost them. Second, offering preventive meetings neither altered this group's search behaviour while they were still employed nor reduced the reservation utility. The treatment did hence not affect the two main determinants of search duration in models of job search.

Our study contributes the first evidence on the causal effects of labour market interventions before workers lose their jobs. We are aware of two earlier studies that have examined interventions before the formal start of unemployment. [Winter-Ebmer \(2006\)](#) finds positive effects of a full-time reemployment programme with an average duration of one year for workers affected by structural change in Austria. [Cavaco et al. \(2013\)](#) find positive effects of a six-month programme in France. Both studies have to rely on less clear sources of identification, and although both interventions took place before the formal start of the participants' unemployment, these time-demanding interventions would have been infeasible while jobseekers still worked at their jobs.³

Our study adds to previous studies on the effectiveness of job search assistance and caseworker meetings for unemployed workers. Several experimental studies have documented positive effects of job search assistance for unemployed workers (see, e.g., [Graaversen and Van Ours, 2008](#), [Maibom et al., 2017](#), [Van Landeghem et al., 2017](#), [Michaelides and Mueser, 2020](#), [McConnell et al., 2021](#), [Cheung et al., 2023](#)).⁴ The programmes ex-

³In a separate experiment, [van den Berg et al. \(2022\)](#) find that different design options for preventive meetings have little influence on their effectiveness, but their experiment does not speak to the main effect of offering preventive meetings. Specifically, [van den Berg et al. \(2022\)](#) examine the effects of integration agreements between caseworkers and jobseekers as well as action plans filled out by the jobseekers in advance of the meetings.

⁴Whereas most of these studies have focused on individual-level effects for the treatment group, several papers have shown that the positive effects of job search assistance come at least partly at the expense of the control group ([Crépon et al., 2013](#), [Gautier et al., 2018](#), [Cheung et al., 2023](#)). As we do not find effects at the individual level, we do not consider such spillover effects in our analysis.

amined in these studies typically combine several elements, such as eligibility reviews, group meetings, and individual caseworker meetings. Caseworker meetings are the focus of [Schiprowski \(2020\)](#) who shows that the cancellation of one meeting prolongs unemployment by 12 days, which she argues to be a lower bound for the effect. Thus, in her setting, meetings emerge as an effective intervention, but she cannot disentangle the underlying mechanism. Caseworker meetings and job search assistance more broadly combine supportive services, e.g., providing information on vacancies and job search, with monitoring, e.g., regarding the fulfillment of search requirements; hence, it is unclear which of these ingredients drive the effects ([Rosholm, 2014](#), [Rothstein and Von Wachter, 2017](#)). As monitoring is absent in our setting, the null effect points towards information provision alone not being effective.

This paper proceeds as follows. Section 2 presents the setting and the intervention in more detail. Section 3 explains the data sources and gives descriptive statistics. Section 4 documents the effects of offering preventive meetings on actual meetings and on labour market outcomes. Section 5 explores the reasons for the absence of effects on labour market outcomes. Section 6 concludes the paper.

2. Institutional setting and intervention

German unemployment insurance requires workers to register as jobseekers three months before they expect their jobs to end. If notice periods are shorter, workers must register within three days after learning the end date of their job.⁵ The requirement to register early is enforced, and late registrations are sanctioned with the loss of one week of unemployment benefits.

Workers can register in person, by phone or online. Workers who register in person typically have direct access to a caseworker. Registrations by phone and online are handled by internal call-centres. At the end of the registration process, the call centres

⁵These rules apply for permanent and fixed-term contracts alike. According to German civil law, the notice period for permanent contracts amounts to at least one month after two years of tenure and three months after eight years of tenure. Employers and employees can collectively agree on different notice periods, but individual agreements on shorter notice periods are limited to specific cases.

schedule a meeting for the jobseeker with the caseworker. If scheduling a meeting is not possible at the time of the call, call centres forward this task to the caseworker. At the time of our experiment, the standard process for agencies was to try to hold these meetings as soon as possible, i.e., months or weeks before jobseekers expected their jobs to end. According to anecdotal evidence, jobseekers were regularly reluctant to schedule meetings so early in the job search process, for instance, because they were afraid that taking time off from work would harm their employment prospects. Until they start to receive unemployment benefits, jobseekers can easily cancel any meetings—also on short notice—with the employment agency, creating nonrandom variation in the timing of the initial meeting.

Our field experiment focuses on jobseekers who registered by phone or online at least one month before the expected end of their employment. Focussing on registrations by phone or online ensures that all jobseekers are in contact with the call centres, which can influence the timing of the initial meeting. Requiring at least one month before the expected end of one’s employment ensures sufficient time to schedule a caseworker meeting before one’s unemployment starts and for this meeting to affect one’s labour market outcomes.⁶

The experiment intervened at the point where the call centres scheduled the meeting with the caseworker. Using a randomisation software tool, which prevents rerandomisation, the tool assigned jobseekers to either the “preventive meeting” or the “late meeting” group. For the “preventive meeting” group, the call-centre staff aimed to immediately schedule a meeting using the employment agency’s scheduling software. For the “late meeting” group, the call centre staff communicated that the jobseeker would have a meeting with the caseworker shortly before becoming unemployed but did not schedule

⁶In preparation for the field experiment, we obtained aggregate data on when and how jobseekers register. Regarding when, 44% of jobseekers in 2017 registered at least one month in advance of the expected end of their employment, most of the remaining jobseekers likely informed later. Regarding how, 37% of jobseekers registered either online or via telephone from January to November 2017. Data on the joint distribution of both characteristics are not available. However, a back-of-the-envelope calculation using the available information suggests that one in four to five persons who register as seeking a job fulfills these criteria. During our field period, 24% of the jobseekers who registered with the participating agencies while being employed entered the experiment.

this meeting. The group assignments were forwarded to the caseworkers to enable them to (re)schedule meetings accordingly. For jobseekers who become unemployed, the caseworker meeting also served as in-person registration of their unemployment. Both groups had to do this on their first day of unemployment at the latest.

Caseworkers were instructed to run the initial meetings (and the later process) as they would normally given the timing and the jobseekers' characteristics. As preventive and late meetings both occurred outside of the field experiment, caseworkers should have had sufficient experience to handle both situations. An initial meeting usually takes 30 to 45 minutes and focuses on job counselling. Typical contents are the jobseeker's profile, advice on job search in general and a joint search for suitable vacancies in the employment agency's database. A survey conducted among jobseekers, which we describe below, showed no differences in the characteristics of the first meeting between the two groups (see Table A.1 in the Online Appendix).

In cooperation with the federal employment agency, we implemented the experiment in four (out of 156) local agencies for jobseekers who registered from 22 May to 31 December 2018. The agencies did not receive additional resources to implement the experiment, but allocated resources differently to the "preventive meeting" group and the "late meeting" group in the experiment. The four local agencies were selected to provide a mixture of urban and rural regions in East and West Germany and to yield a representative sample of jobseekers. The four agencies are served by two (out of 46) regional call centres.

To ensure that the intervention was implemented as intended, we held joint and individual meetings with the chief operation officers of the four local agencies and their project coordinators as well as representatives from the call centres and the federal employment agency. We held additional instruction meetings with the managers of the four agencies and the two internal call centres. Finally, we offered support during the whole field period.

3. Data and descriptive statistics

Our analysis combines data from four sources, which can be linked using person identifiers. First, the randomisation tool (EMU) records the group assignment and the date of the randomisation. Throughout the analysis, we use this date as the starting point of the job search process and refer to it as the date of registering as a jobseeker.

Second, we use data on meetings obtained from the federal employment agency’s internal scheduling software, ATV. This data source gives us precise and reliable information on meetings between jobseekers and caseworkers at the daily level. We use these data to examine the effects of offering preventive meetings on actual meetings.

Third, we use the Integrated Employment Biographies (IEB, see [IAB, 2020](#)) described in [Jacobebbinghaus and Seth \(2007\)](#). The data include information on spells of employment and the receipt of unemployment benefits at the daily level, as well as on earnings. This information is used to calculate contributions to and payments from social security and is highly reliable. The IEB also provide information on jobseekers’ sociodemographics (e.g., age, sex, and education). Regarding job characteristics, we observe the sector and the occupation as well as whether jobseekers were hired on a permanent or a fixed-term contract, but we do not observe contract conversions ([BA, 2018](#)). From these data, we derive four measures of jobseekers’ labour market outcomes: an indicator of whether they received unemployment benefits, the number of days they received unemployment benefits, the number of days they were employed, and their labour income. Unemployment and employment refer to the year after registration. Income is cumulated over all employment spells in the year 2019, as it is reported on an annual basis for ongoing jobs.

Our fourth data source is an online survey conducted among jobseekers who entered the field experiment. To survey jobseekers shortly after the expected end of their employment, we sent out invitations to participate in the survey in nine waves.⁷ The

⁷As part of the experiment, call centre agents were able to record the expected end of employment in the randomisation tool, but this information is often missing. In these cases, we defaulted to sending out the invitation about four months after the date of the registration. Because of the large share of missings, we do not use the expected end of employment in our analysis beyond this. The expected end of employment recorded (or not) in the randomisation tool was not used in administrative processes such

questionnaire focused on the contents of the first caseworker meeting and the job search conducted while still employed. We use the survey data to describe the intervention in more detail and to investigate potential mechanisms. We contacted the jobseekers up to three times—first via mail and then up to two times via e-mail. Slightly above 10% of all jobseekers participated in the survey. The survey participants have a higher education level and better labour market outcomes before registering as jobseekers than the average participant in the field experiment, see Table A.2 in the Online Appendix for a comparison.

Table 1: Descriptive statistics

	Jobseekers	Participating regions	Participants in experiment	Treatment status	
				“preventive”	“late”
Age (years)	39.47 (12.52)	40.10 (12.67)	40.29 (12.07)	40.38 (12.17)	40.20 (11.98)
Female	0.45 (0.50)	0.45 (0.50)	0.54 (0.50)	0.55 (0.50)	0.54 (0.50)
Tertiary degree	0.16 (0.37)	0.15 (0.36)	0.22 (0.42)	0.23 (0.42)	0.22 (0.41)
Non-German nationality	0.18 (0.39)	0.13 (0.34)	0.04 (0.21)	0.04 (0.20)	0.05 (0.21)
Days in employment in 52 weeks before	288.84 (113.68)	292.78 (107.93)	320.91 (80.54)	321.66 (79.45)	320.19 (81.58)
Fixed-term contract (initially)	0.44 (0.50)	0.42 (0.49)	0.59 (0.49)	0.59 (0.49)	0.59 (0.49)
Full-time worker	0.70 (0.46)	0.70 (0.46)	0.67 (0.47)	0.67 (0.47)	0.68 (0.47)
Monthly gross wage in Euro (full-time workers)	2,628.86 (1323.23)	2,513.68 (1196.45)	2,870.18 (1273.35)	2,870.56 (1295.81)	2,869.83 (1252.09)
Working for same employer 26 weeks post registration	0.33 (0.47)	0.37 (0.48)	0.47 (0.50)	0.46 (0.50)	0.47 (0.50)
N	1,354,515	43,913	10,555	5,155	5,400

Notes: Column 1 gives the descriptive statistics for all persons who registered as jobseekers in Germany while being employed between 22 May and 31 December 2018. Columns 2 restricts the observations to jobseekers in the participating regions. Column 3 gives the descriptive statistics for the participants in the experiment, and Columns 4 and 5 split the sample by treatment status. Characteristics were measured when persons registered as jobseekers. Standard deviations are given in parentheses. The p -value for any difference by treatment status is 0.701. *Sources:* EMU and IAB (2020).

Table 1 describes the participants in the experiments in comparison to other jobseekers. Columns 1 and 2 describe all newly registered, employed jobseekers in Germany and in the regions that participated in the experiment. Although the share of non-Germans is somewhat smaller and the wage is somewhat lower in participating regions, jobseekers in

as decisions on sanctions.

the participating region appear to be representative of jobseekers in Germany. Column 3 describes the participants in the experiment. The jobseekers in the experiment have more favourable characteristics than the average jobseeker. Specifically, the participants are more likely to have a tertiary degree, were employed more steadily before registering as jobseekers and those in full-time employment earned higher wages. Almost 60% of the jobseekers in the experiment were previously hired on a fixed-term basis. Although their jobs are thus less secure than that of an average worker, fixed term contracts also imply that their contracts might be extended or converted into permanent ones. This is reflected in the higher share of jobseekers still working for the same employer 26 weeks after registration.⁸ We cannot disentangle to what extent the differences between participants and other jobseekers are driven by the requirement to register early versus the requirement to register online or by phone.

The last two columns report descriptive statistics by treatment status. The two groups do not differ in observable characteristics, including the share of those working for the same employer 26 weeks after registration.

4. Effects of offering preventive meetings

4.1. Effects on timing and number of meetings

We start by examining whether offering preventive meetings to jobseekers while they are still employed affects actual meetings. Figure 1, Panel A shows the share of jobseekers who have had at least one meeting over time by treatment status. Within 60 days after registering as a jobseeker, about 50% of the jobseekers in the “preventive meeting” group have at least one meeting, in contrast to 22% of those in the “late meeting” group. The difference between the groups decreases over time. From 150 days after the registration onwards, the difference stabilises at about 15 percentage points. In both groups, a substantial share of the jobseekers had no meeting over the course of one year.

⁸Appendix Figure A.1 shows that the hazard rate from the current job is elevated for about half a year after registration and remains constant afterwards. We therefore differentiate between jobseekers who are employed with the same employers 26 weeks after registration and those who are not.

As meetings are mandatory only for the unemployed, this likely reflects that not all the jobseekers lost their jobs.

To further illustrate the shift in the initial meeting’s timing relative to job loss, we examine the timing of the first meeting for those who exited their jobs within 26 weeks after registration and have at least one meeting in the year after registration. Jobseekers who were offered preventive meetings had their first meeting on average 31.2 days before losing their jobs, in contrast to only 1.6 days before losing their job for those offered a late meeting.

To shed light on later meetings, Panel B of Figure 1 shows the average cumulated number of meetings for both groups. As shown in Panel A, persons in the “preventive meeting” group have initially more meetings. From the fourth month onwards, the number of meetings evolves in parallel for both groups, i.e., persons in the “late meeting” group do not catch up in terms of meetings. This pattern shows that the “preventive meeting” treatment not only shifted the timing of the first meeting, but also increased the overall number of meetings for the “preventive meeting” group.

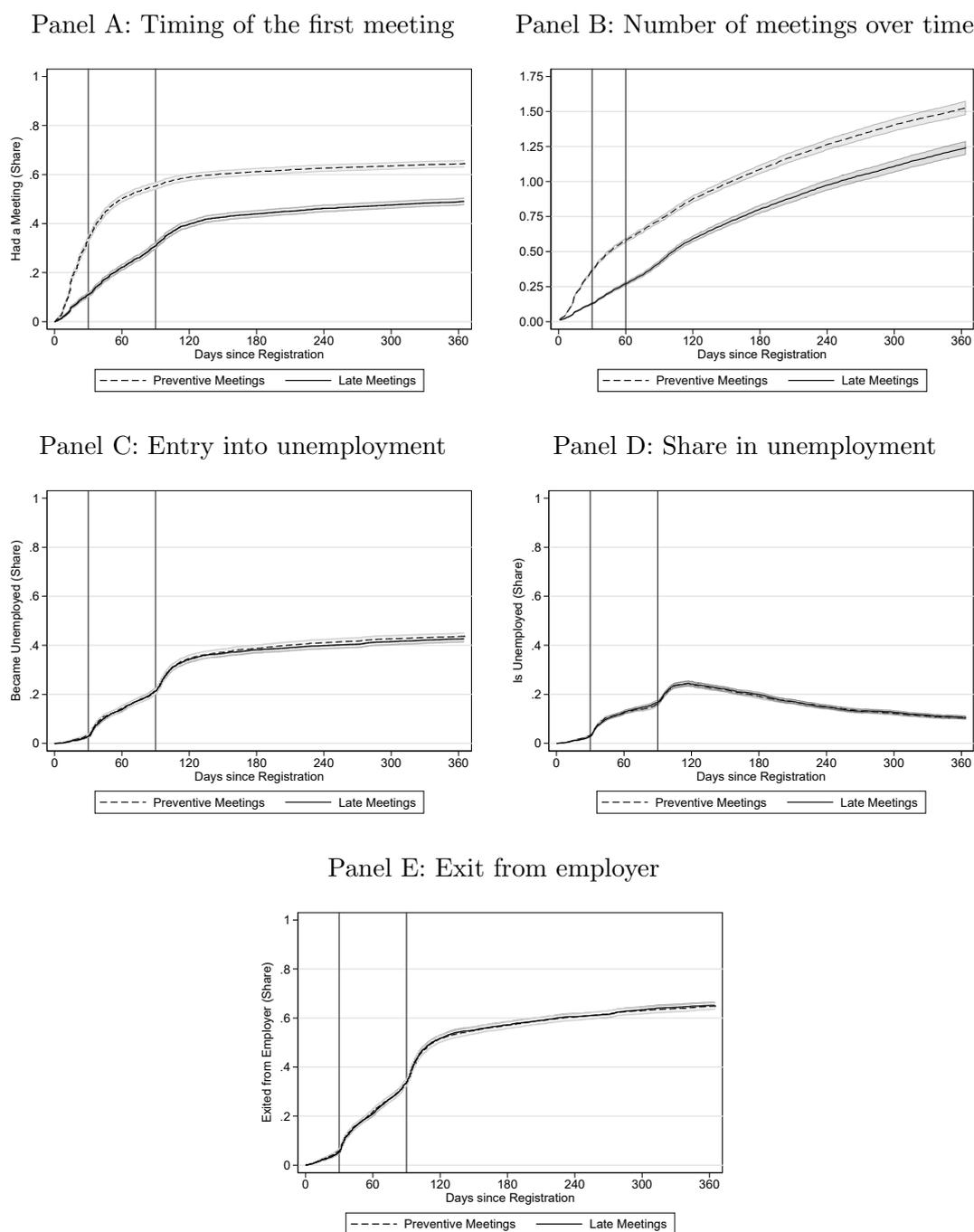
The first two columns of Table 2 report the regression estimates for the effects on the probability of having at least one meeting and on the number of meetings held within one year. Offering preventive meetings raises the probability of having at least one meeting by 16 percentage points and increases the number of meetings by 0.3.⁹ To summarise the effects on meetings, offering preventive meetings substantially shortens the time lag until the first meeting, increases the share of jobseekers having at least one meeting, and raises the total number of meetings.

4.2. Effects on labour market outcomes

Panel C of Figure 1 visualises the first results on labour market outcomes. It shows the share of jobseekers who entered unemployment over time by treatment status. Within one year of registration, about 43% of jobseekers became unemployed. This share evolves identically in both groups, indicating that the intervention does not prevent entry into

⁹Appendix Table A.3 additionally shows all the results for the regressions reported in Table 2 when omitting the control variables. As one would expect, the results are virtually identical.

Figure 1: Timing of meetings, entry into unemployment and exit from one's employer



Notes: Panel A shows the share of jobseekers who had a meeting over time by group assignment. Panel B shows the average number of meetings that jobseekers had over time by group assignment. Panel C shows the share of jobseekers who became unemployed over time, and Panel D the share in unemployment over time. Unemployment is defined as receiving unemployment benefits. Panel E shows the share of jobseekers who exited from their employer over time. Vertical lines at 30 and 90 days. Shaded areas give 95% confidence intervals. *Sources:* ATV, EMU, and IAB (2020).

Table 2: Effect of offering preventive meetings

	Meetings		Labour market outcomes			Exit from employer
	any	count	Days in unemployment	Days in employment	Labour income (in 2019)	
Panel A: All jobseekers (N=10,330)						
Treatment: preventive	0.153 (0.010)	0.285 (0.036)	-0.373 (1.634)	0.329 (2.131)	-0.390 (0.308)	-0.004 (0.010)
Mean	0.495	1.241	52.826	281.234	23.511	0.580
Panel B: Jobseekers who exited their jobs (N=5,971)						
Treatment: preventive	0.113 (0.011)	0.304 (0.054)	-0.239 (2.430)	1.022 (2.959)	-0.444 (0.428)	
Mean	0.692	1.885	85.579	231.046	19.967	
Panel C: Jobseekers who did not exit their jobs (N=4,359)						
Treatment: preventive	0.212 (0.014)	0.273 (0.027)	0.158 (0.887)	-1.704 (1.368)	-0.392 (0.391)	
Mean	0.223	0.354	7.666	350.437	28.398	

Notes: Meetings held until one year after registering as a jobseeker. Exit from one’s employer within 26 weeks of registration. Days of unemployment and employment cumulated over the 52 weeks after one’s registration, total labour income subject to social security in 1,000 Euro referring to the 2019 calendar year. Unemployment is defined as receiving unemployment benefits, employment is defined as the person being employed subject to social security (excluding marginal employment). Control variables are quadratics in age and wage as well as dummies for sex, occupation, fixed-term contract, education, foreign nationality, being employed in the public sector, and being employed in the service sector (all measured at randomisation). Results without control variables are available in Appendix Table A.3. The number of observations is lower than in Table 1 due to missing information for control variables. Robust standard errors are given in parentheses. Mean is the average of the dependent variable in the “late meeting” group. *Sources:* ATV, EMU and IAB (2020).

unemployment. Panel D gives the share of jobseekers un unemployment for both groups over time again yielding no differences between the two groups.

The regression results in the third to fifth columns of Table 2 underpin that offering preventive meetings does not affect labour market outcomes. Specifically, the point estimates for the effects on jobseekers’ durations in unemployment and employment during the first year after their registration are all close to zero, and the differences are small and likely reflect random group differences. We reach the same conclusions regarding labour income as a measure of job quality.

As specific subgroups may benefit from the intervention despite the zero average effect, Figure 2 depicts the results by sex, age, and other socioeconomic characteristics. For

Table 3: Effect of offering preventive meetings on job search while still employed

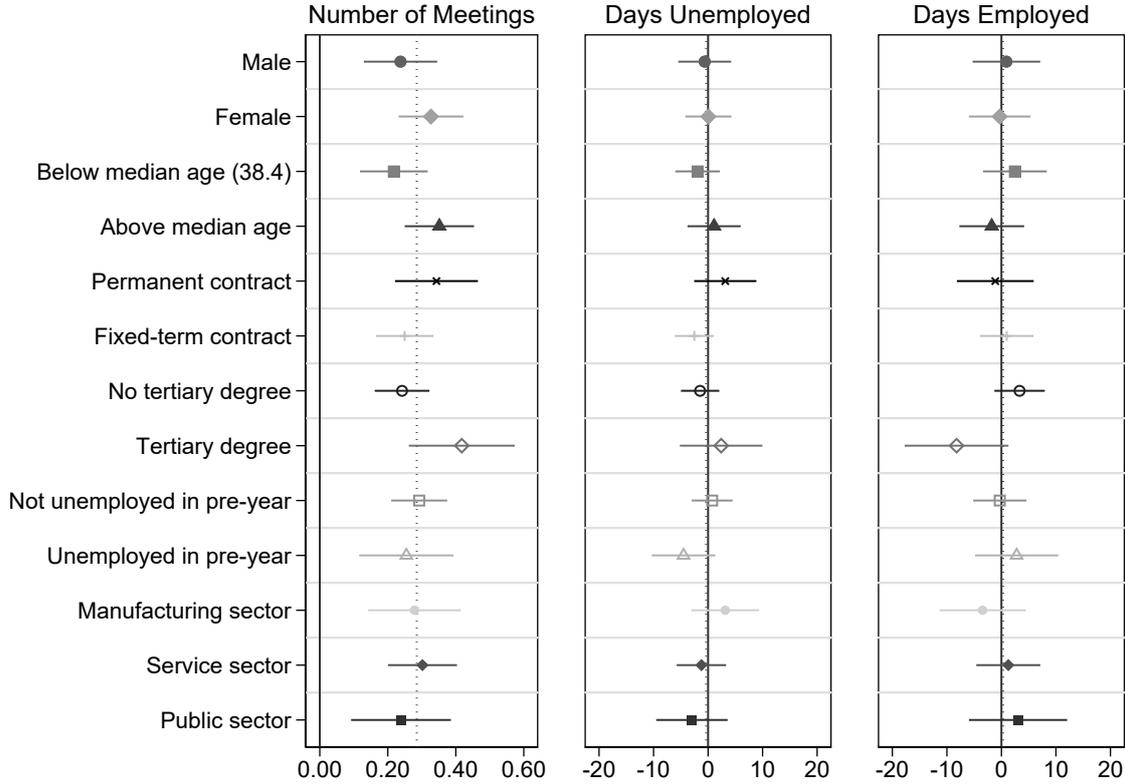
	(1)	(2)	(3)
	Assigned meeting		
	preventive	late	<i>p</i> -value
Panel A: Searching for a job			
Actively searched for a new job	0.78	0.82	0.28
Received vacancy referral from agency	0.50	0.47	0.45
Number of vacancy referrals from agency	1.91	2.14	0.51
N	316	315	
Panel B: Intensity of job search (conditional on searching)			
Number of applications	8.83	9.14	0.65
Hours of search (per week)	7.31	6.96	0.64
N	255	268	
Panel C: Willingness to make concessions for new job			
Fixed-term contract	0.47	0.53	0.16
Longer commute than previously	0.50	0.49	0.90
Less flexible working time	0.34	0.36	0.69
Lower wage than previously	0.35	0.37	0.63
Longer hours than previously	0.33	0.28	0.16
Work on weekends	0.20	0.17	0.26
Moving	0.19	0.16	0.34
Work in shifts	0.13	0.12	0.60
Not willing to make any of these	0.20	0.15	0.09
N	319	314	

Notes: Only jobseekers who exited from their employers within 26 weeks of registering as a jobseeker. All items refer to job search while still employed and finding a new job while still employed. The third column gives the *p*-values from regressing each search variable separately on a treatment indicator using robust standard errors. *Sources:* EMU, [IAB \(2020\)](#), and survey data.

all subgroups, offering preventive meetings raises the number of meetings substantially showing that compliers with the offered preventive meeting stem from all subgroups. Offering preventive meetings does however not affect labour market outcomes for any of the subgroups.

Our results suggest that caseworkers should meet with unemployed workers rather than implement our intervention because our estimates practically rule out effects of the size of those found for unemployed workers in [Schiprowski \(2020\)](#). She finds that the cancellation of one caseworker meeting prolongs unemployment by twelve days and argues

Figure 2: Effect heterogeneities



Notes: The figure gives treatment effects of offering preventive meetings (and 95% confidence intervals) from separate regressions as described in Table 2 for each subgroup. Vertical dotted lines depict the average treatment effects from these tables. *Sources:* ATV, EMU, and IAB (2020).

that this likely underestimates the effects of such meetings as half of the cancelled meetings are replaced by meetings with other caseworkers. Assuming that offering preventive meetings only affects labour market outcomes via actual meetings, we would expect a reduced form effect of four days for 0.3 additional meetings if the additional meetings reduced unemployment on average by twelve days. An effect of this size is highly unlikely given our point estimates and their precision.

5. Reasons for the absence of effects on labour market outcomes

Understanding the reasons for the absence of effects is crucial for designing future preventive interventions. Any preventive intervention faces two challenges. First, it has to reach workers with a high risk of losing their jobs; second, it has to work for those who

actually lose their jobs. We will explore both aspects, starting with the targeting of the intervention.

As the caseworker meetings focus on job-search assistance for the next job, such meetings are unlikely to affect whether workers lose their current job. Figure 1, Panel D presents Kaplan-Meier graphs for exiting one's current job by treatment status. In line with our presumption, we see no difference by treatment status. Therefore, we use exiting from one's employment to examine the intervention's targeting. About 58% of the jobseekers left their employer within 26 weeks after registration.¹⁰ The regression results in the final column of Table 2 confirm that the treatment had no effect on the likelihood of exiting one's employer.

We first examine whether those workers who lost their jobs actually had the additional meetings induced by the treatment. Ex-post selecting these jobseekers is akin to analysing the effects of an ideally targeted interventions, where caseworkers have perfect foresight about which jobseekers will lose their jobs and meet only with these. Such a targeting appears unrealistic, unless one restricts preventive meetings to very specific groups and forgoes preventive caseworker meetings with many jobseekers who will become unemployed. The results for this group are still informative about the effects of preventive caseworker meetings with substantially improved targeting.

Panel B of Table 2 presents the effects of offering preventive meetings for jobseekers who exited their employer and Panel C those for jobseekers who remained in their job.¹¹ The effect of offering preventive meetings on the number of meetings is practically identical in both groups, implying that about 40% of the meetings accrued to jobseekers who remained with their employer. Although this leaves a substantial scope with which to improve the targeting, more than half of the induced meetings accrued to jobseekers

¹⁰As workers can be recalled by their previous employers, the share of jobseekers who exited their employer is slightly higher than the share of jobseekers not working for the same employer reported in Table 1. We focus on workers who exited because they could have benefitted from having a caseworker meeting at some point.

¹¹We do not interpret differences in the effects on the probability of having at least one meeting separately for these two groups, because workers who lose their jobs are more likely to become unemployed, making meetings mandatory. This institutional feature mechanically creates a difference between the two groups in the treatment effect on the probability of having at least one meeting.

who actually lost their jobs, showing that the intervention reached workers who were at a high risk of becoming unemployed.

The remaining columns in the two panels show the effects of offering preventive meetings on labour market outcomes for both groups. Even when focusing on jobseekers who lost their jobs, there is no discernible effect of offering preventive meetings on the durations in either unemployment or employment. The point estimates are close to zero and still estimated precisely enough to render substantial effects on the duration of unemployment unlikely. The p -value for the null hypothesis of a reduction by at least four days is 0.085.¹² As the results suggest that offering preventive meetings did not have an effect even when targeted to jobseekers who became unemployed, we next explored the search behaviour of this group.

Table 3 shows the results from our online survey shedding light on jobseekers' search activities and reservation utility. Restricting the sample to jobseekers who lost their jobs leaves us with about 600 observations. Jobseekers in the "preventive meeting" group were slightly more likely to report that they received a vacancy referral while still employed, but they did not engage in job searching more often nor more intensely (Panels A and B). According to Panel C, jobseekers in the "preventive meeting" group are also not more willing to make concessions in various dimensions regarding a new job offer. If anything, the share of jobseekers who is not willing to make any concessions suggests an effect in the opposite in the opposite direction, though the differences in all of these items are jointly not statistically significant ($p = .29$). Despite the limited sample size, we can conclude from these statistics that offering preventive meetings did not induce additional search effort or reduce reservation utility while jobseekers were still employed.

6. Conclusions

We document that offering preventive caseworker meetings to all jobseekers is resource-demanding, but ineffectual. Although this approach considerably increases the number of

¹²For the subgroup of workers who lost their jobs, we still have substantial statistical power. An ex-post power analysis yields a statistical power of 0.50 when testing for an effect of this size at the 5%-level and of 0.64 when testing at the 10%-level.

meetings, and thus caseworkers' workload, it does not improve jobseekers' labour market outcomes. We find two likely reasons for the absence of effects; first, a substantial share of the additional meetings accrues to jobseekers who do not lose their jobs and for whom we would not expect any treatment effect. Second, offering preventive meetings also fails to work for those jobseekers who lose their jobs, most likely because it does not increase their search effort or reduce their reservation utility, which are the two main determinants of search duration in job search models. Although our results show that the additional meetings induced by the agencies' invitation do not improve labour market outcomes, the results do not speak to the efficacy of preventive meetings for jobseekers who actively request preventive meetings.

The results for all registered jobseekers and for those who lose their jobs provide insights into the effects of preventive caseworker meetings from two distinct perspectives. First, the overall effect sheds light on allocating caseworker resources to preventive meetings, given the current registration requirements and targeting of the meetings. Given this, caseworker meetings with the unemployed emerge as a more efficient use of public resources. Second, the effects for jobseekers who lose their jobs offer insights into the effectiveness of precisely targeted interventions. While there is potential for enhancing targeting accuracy, leveraging advanced techniques such as machine learning—similar to [Ernst et al. \(2024\)](#) and [van den Berg et al. \(2023\)](#)—our findings suggest that even with significantly refined targeting, allocating caseworker meetings to the unemployed likely remains a more efficient use of public resources compared to preventive measures.

Notwithstanding our results show scope for preventive interventions as offering preventive meetings increases the number of meetings even though jobseekers are not legally required to attend such meetings. Despite the sobering results, the field experiment thus demonstrates that such interventions to prevent unemployment might be possible. To improve jobseekers' labour market outcomes, preventive interventions arguably have to increase their search effort or effectiveness. As jobseekers who are still employed have limited time available, improving search effectiveness seems more promising. This will presumably require more specific interventions. For instance, providing resources for job

searching online similar to [Briscese et al. \(2022\)](#) might be helpful for these jobseekers. Interventions will likely also have to be more intense, although participation has to remain feasible while still employed.

Why do caseworker meetings work for unemployed workers, whereas additional meetings for employed jobseekers do not work in our setting? As previously explained, caseworker meetings for the unemployed not only provide counselling services but also serve as a monitoring device. Which of these two aspects matters more (or their combination) is not yet well understood (see [Rosholm, 2014](#)). As caseworker meetings in our setting do not include monitoring activities, the absence of an effect might indicate the importance of monitoring. It might increase the search effort—particularly if search requirements can be enforced—but not necessarily the search effectiveness. An alternative explanation that also calls for further investigations is that the effect of caseworker meetings changes over time, for instance, because jobseekers are initially overconfident and only later make full use of the provided resources.

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Online Appendix

Table A.1: Descriptive statistics on initial meetings

	(1) all	(2) preventive meetings	(3) late meetings
Panel A: Meetings and timing of survey			
Timing of survey: days since registration	118.12	118.14	118.11
Had meeting (admin data)	0.65	0.75	0.55
Had meeting (survey data)	0.59	0.66	0.53
Treatment: preventive	0.49		
N	967	477	490
Panel B: Information on meetings (conditional on having one)			
Timing of survey: days since meeting (admin data)	75.19	84.46	62.39
duration (minutes)	36.89	36.75	37.08
CV, strengths and weaknesses	0.79	0.78	0.82
Job search methods	0.54	0.54	0.55
Used the agency's search platform	0.44	0.43	0.45
Measures to improve skills	0.41	0.42	0.40
Acceptable working conditions	0.40	0.41	0.38
Options for financial support	0.21	0.20	0.22
N	514	298	216

Notes: Had a meeting as of survey date. The p -values for differences in duration and contents of meeting are 0.92 and 0.79. Based on survey data.

Table A.2: Descriptive statistics on survey participation

	(1)	(2)
	all	survey participants
Age (years)	40.29	42.74
Female	0.54	0.56
Tertiary degree	0.22	0.47
Non-German nationality	0.04	0.05
Days in employment in 52 weeks before	320.91	323.04
Days in unemployment in 52 weeks before	16.21	13.28
Monthly wage in Euro	2,520.68	3,132.03
Fixed-term contract (initially)	0.59	0.52
Treatment: preventive	0.49	0.50
N	10,555	1,088

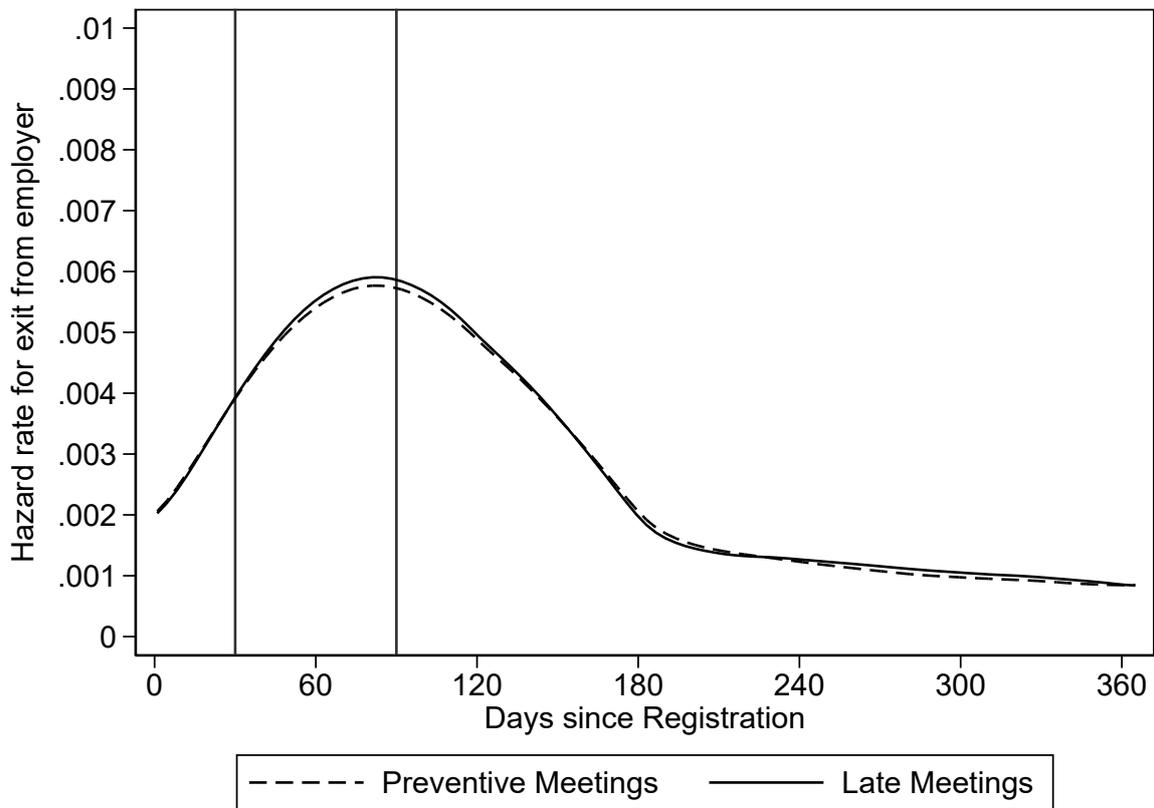
Notes: Means of socioeconomic characteristics by participation in survey. Based on survey data, EMU, and [IAB \(2020\)](#).

Table A.3: The effect of offering preventive meetings - without control variables

	Meetings		Labour market outcomes			Exit from
	any	count	Days in		Labour income	employer
			unemployment	employment	(in 2019)	
Panel A: All jobseekers (N=10,330)						
Treatment: preventive	0.153	0.285	-0.373	0.329	-0.390	-0.004
	(0.010)	(0.036)	(1.634)	(2.131)	(0.308)	(0.010)
Mean	0.495	1.241	52.826	281.234	23.511	0.580
Panel B: Jobseekers who exited their jobs (N=5,971)						
Treatment: preventive	0.113	0.304	-0.239	1.022	-0.444	
	(0.011)	(0.054)	(2.430)	(2.959)	(0.428)	
Mean	0.692	1.885	85.579	231.046	19.967	
Panel C: Jobseekers who did not exit their jobs (N=4,359)						
Treatment: preventive	0.212	0.273	0.158	-1.704	-0.392	
	(0.014)	(0.027)	(0.887)	(1.368)	(0.391)	
Mean	0.223	0.354	7.666	350.437	28.398	

Notes: Meetings held until one year after registering as a jobseeker. Exit from one's employer within 26 weeks of registration. Days of unemployment and employment cumulated over the 52 weeks after one's registration, total labour income subject to social security in 1,000 Euro referring to the 2019 calendar year. Unemployment is defined as receiving unemployment benefits, employment is defined as the person being employed subject to social security (excluding marginal employment). Robust standard errors are given in parentheses. Mean is the average of the dependent variable in the "late meeting" group. *Sources:* ATV, EMU and [IAB \(2020\)](#).

Figure A.1: Hazard rate for job exit



Notes: The figure shows the hazard rate for jobseekers' exit from their employer over time by treatment assignment. Vertical lines at 30 and 90 days. *Sources:* EMU, ATV, and IAB (2020).