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ABSTRACT

Do Job Seekers Understand the UI Benefit System (And Does It Matter)?*

We study how job seekers' understanding of complex unemployment benefit rules affects their labor market performance. Combining data from a large-scale scale field experiment, detailed administrative records, and a survey of unemployed job seekers, we document three main results. First, job seekers exhibit pronounced knowledge gaps about the prevailing unemployment benefit rules and their personal benefit entitlements. Second, we show that a low-cost information strategy using a personalized online tool increases job seekers' understanding of the rules and their personal benefit situation. Finally, we document heterogeneous labor-market effects of the intervention depending on job seekers' baseline knowledge and beliefs, their personal employment prospects, and the timing of the intervention during the benefit spell.

JEL Classification: J68, J64, D83, C93

Keywords: unemployment benefits, field experiments, information frictions, labor market policy, job search

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1 Introduction

Unemployment insurance systems in modern labor markets are riddled with a multitude of rules and regulations governing job seekers' economic situation and their incentives to search for employment. These rules include detailed regulations specifying individuals' benefit level and the period of benefit payments (Card and Levine, 2000; Schmieder et al., 2012, 2016), job search requirements (Lalive et al., 2005; Arni et al., 2013), and allowances to work in part-time or short-term jobs while receiving public benefits (Caliendo et al., 2016; Benghalem et al., 2022). The rules and regulations serve important objectives in providing financial means to job seekers, while trying to minimize moral hazard problems that commonly arise in social insurance systems (Hopenhayn and Nicolini, 1997; Krueger and Meyer, 2002; Acemoglu and Shimer, 2000; Chetty, 2008). The complexity of the rules and regulations might, however, render it difficult for job seekers to thoroughly understand the prevailing rules and their implications. The challenges in understanding the rules may, in turn, distort individuals' job search behavior, thereby potentially affecting the extent to which the regulations serve their objectives.

In this paper, we study the interplay between job seekers' understanding of complex UI benefit rules, their personal benefit entitlements, and the resulting labor market outcomes. Our analysis proceeds in three steps. First, we document that job seekers exhibit substantial knowledge gaps about their own benefit entitlements and the prevailing rules that govern these entitlements. The analysis in this part of the paper is based on a large-scale online survey conducted among unemployed workers in Denmark. We find that the lack of understanding does not only apply to detailed regulations, which are relevant only for a few job seekers or in relatively rare circumstances. Instead, knowledge gaps are also prevalent for core rules of the UI benefit system. For instance, more than 40% of job seekers hold beliefs about their potential benefit duration (PBD) that deviate from their actual entitlements by four weeks or more, with an average deviation of more than two months.

In a second step, we investigate whether job seekers' understanding of the UI benefit system can be improved with a low-cost information strategy. We present the results of a randomized controlled trial that we conducted among the universe of UI benefit recipients in Denmark (N$\sim$98,000). In the experiment, treated job seekers are encouraged to use an online information tool that provides up-to-date, personalized information on individuals' UI benefit situation and the corresponding rules. The tool is embedded in the official online platform of the Danish public employment service and aims to increase job seekers' understanding of two central elements of the UI benefit system. First, the tool provides job seekers with personalized information about their benefit entitlements, i.e., the remaining period before their benefits will expire. Second, the tool provides information about possibilities to extend the benefit period by working additional hours during one's UI benefit spell, e.g., in part-time or temporary jobs. By linking the data from the RCT to our online survey, we show that promoting the usage of the information tool substantially improves job seekers' understanding of the UI benefit rules and their personal benefit entitlements.

In a final step of our analysis, we study how the enhanced understanding of the prevailing rules and their personal situation affects individuals' labor market outcomes. To this end, we combine the data from our randomized controlled trial with comprehensive administrative data containing information on individuals' employment and earnings up to two years after the beginning of the intervention. On average, the overall working hours and labor earnings of treated individuals are slightly lower than in the control group. While these average effects are relatively small and not statistically significant, we find important heterogeneity regarding the labor market effects of our intervention among different groups of job seekers.

For individuals who have already been unemployed for more than a year and who, consequently, face a relatively high risk of benefit expiration, the treatment reduces overall employment and earnings by about 2.3% within 24 months, relative to the comparable group of long-term unemployed job seekers in the control group. In contrast, we observe positive employment and earnings effects of our intervention among individuals who entered unemployment less than six months prior to the intervention, and who have a low predicted risk of staying unemployed for an extended period. In this group, the treatment increases individuals' total number of working hours and labor market earnings by about 1.6% within the first two years after the beginning of the intervention.

A number of factors are likely to contribute to the heterogeneous labor market effects of our intervention. First, we find that long-term unemployed individuals in our sample tend to be overly optimistic about their remaining benefit period. This optimism is reduced by our intervention. At the same time, the group of long-term unemployed individuals consists of job seekers with poor overall labor prospects (reflecting dynamic selection over the benefit spell), whose benefit expiration date is already relatively close. We find that our intervention, which informs job seekers about benefit extension possibilities, encourages these job seekers to work in "marginal jobs" (i.e., temporary or part-time employment). This reaction is in line with the observation that—in the absence of our intervention—job seekers underestimate the possibilities to extend their UI benefit entitlements through marginal employment. Yet, our data also show that this shift towards marginal employment has adverse effects on overall working hours and labor earnings in the longer run. This finding suggests that, in our setting, lock-in effects are severe relative to stepping-stone aspects of marginal employment. In line with this notion, we also find that the treatment has negative effects on overall financial well-being of long-term unemployed job seekers, as measured by their likelihood of subsequently entering social welfare and their overall disposable income accounting for taxes and transfers.

Conversely, we find that job seekers who only recently entered unemployment are relatively pessimistic about their personal benefit entitlements. Our data illustrates that the intervention leads to a reduction of these overly pessimistic beliefs. Moreover, while this group also acquires better knowledge about benefit extension possibilities, their incentives to immediately act upon this knowledge are limited. Yet, having the option to extend one's UI benefit duration in the future may reduce the perceived short-run pressure to accept a job. Our results indicate that such a mechanism may be underlying the positive treatment effects on labor market outcomes of low-risk job seekers. Specifically, we find long-run positive treatment effects on the propensity to work in jobs with relatively high wages for this group. At the same time, the positive labor market effects are particularly pronounced for low-risk job seekers who tend to be more pessimistic about their own situation in absence of the intervention.

Our paper adds new insights to a growing literature documenting that unemployed workers face substantial information frictions, which impair their transition from unemployment to employment. Existing evidence highlights that job seekers commonly lack information about the general labor market situation and potentially promising matches (see e.g. Conlon et al., 2018; Altmann et al., 2018, 2022; Belot et al., 2019, 2022; Mueller et al., 2021). Our findings complement this evidence by documenting knowledge gaps about core elements of the UI system such as the job seekers' benefit entitlements.[1] Our analysis also shows that online information tools provide a promising low-cost solution to tackle these knowledge gaps.

A key advantage of online tools is the possibility to reduce complexity and increase understanding through personalization and targeting of information about policies and regulations (see also Fuentes et al., 2017; Belot et al., 2019). While our results show that job seekers' understanding of the UI benefit system can have first-order effects on their labor market performance, the exact consequences

[1] A number of related studies analyze interventions addressing limited knowledge in other policy domains such as educational choices (see e.g. Hastings and Weinstein, 2008; Jensen, 2010; Bettinger et al., 2012; Wiswall and Zafar, 2014), cash transfer programs (Alatas et al., 2016), retirement schemes (Liebman and Luttmer, 2015), food stamps (Finkelstein and Notowidigdo, 2019), medical support (Kling et al., 2012) and tax credits (Chetty and Saez, 2013; Bhargava and Manoli, 2015).

crucially depend on the individual's personal situation. Ultimately, an improved understanding can have adverse effects when the underlying incentives promote short-sighted behavior, such as the take-up of marginal employment in an environment where such jobs seem to be detrimental to the longer-run labor market integration of unemployed workers. In this regard, our analysis provides new insight on the effectiveness of promoting marginal employment (see, e.g., Booth et al., 2002; Heinrich et al., 2005) and supports the notion that marginal employment might be associated with lock-in effects (see, e.g., Fremigacci and Terracol, 2013; Kyyrä et al., 2013).

Perhaps most closely related to our paper is a contemporaneous study by Benghalem et al. (2022), who conduct a randomized controlled trial that informs unemployed workers in France about the existence of a part-time UI benefit program. In line with our results for long-term unemployed job seekers, they find that a higher take-up of marginal jobs among treated job seekers is associated with a reduced propensity to exit unemployment, pointing to a lock-in effect of part-time insurance. Complementing our findings (and an exploratory analysis of treatment spillovers in Section 6.4 below), Benghalem et al. (2022) also provide convincing evidence that their intervention induces no spillover effects among unemployed workers, based on a clustered randomized design in the spirit of Crépon et al. (2013). Our additional survey data, in turn, allows us to better understand existing knowledge gaps among job seekers and to shed further light on how closing these gaps matters for different groups of job seekers. Moreover, based on detailed administrative data, we can provide a comprehensive picture of how our intervention affects the nature of the resulting job matches.

On a more general level, our paper also contributes to an extensive empirical literature that investigates the effects of the generosity of UI systems on labor market outcomes. Existing studies document that more generous UI coverage encourages individuals to search less intensively for new jobs (Marinescu, 2017; Lichter and Schiprowski, 2021), increase the time spent in unemployment and non-employment (Katz and Meyer, 1990; Card and Levine, 2000; Lalive et al., 2006; Van Ours and Vodopivec, 2006; Chetty, 2008; Schmieder et al., 2012, 2016), and result in ambiguous effects on the quality of subsequent job matches (Le Barbanchon et al., 2019; Centeno and Novo, 2009; Nekoei and Weber, 2017; Van Ours and Vodopivec, 2008). In this context, we provide new insights from a system with 'flexible generosity', i.e., a setting in which individuals' job search and work effort after unemployment registration endogenously determine their individual benefit entitlements.

Our information treatment, which highlights the key features of such a system, provokes behavioral reactions that are closely linked to the UI system's central trade-off between insurance and job search incentives (Baily, 1978; Chetty, 2008). At the same time, our results suggest that these effects interact with dynamic selection and incentives over the benefit spell in a way that may reinforce existing trends regarding job seekers' reemployment prospects.[2] Among benefit recipients who are closely attached to the labor market, a better understanding of the relatively generous benefit rules and their personal situation seems to increase the (perceived) value of UI insurance such that job seekers can be more selective when searching for employment. However, when the prospects to find a job in the regular labor market are poor and benefit expiration is already in sight, the possibility to prolong the PBD by working additional hours reinforces the incentives to accept any employment, which may come at the cost of a lower match quality and worse long-run labor market outcomes.

The remainder of the paper is organized as follows. Section 2 describes the Danish UI benefit system and documents existing knowledge gaps among unemployed workers. Section 3 presents the design of our randomized controlled trial, while Section 5 presents the effects of our intervention on individuals' understanding of their personal benefit situation and the UI benefit rules. Section 6 analyzes the labor market effects of our intervention and Section 7 concludes.

2 Unemployment Benefit Rules and Knowledge Gaps

2.1 The Danish UI Benefit System

Unemployment insurance in Denmark is organized in a voluntary opt-in system, where unemployed workers are eligible to receive UI benefits for a period of up to two years if they have paid contributions for at least 12 months within the last three years. The level of monthly benefits is fixed at 90% of a worker's prior wage earnings, up to a ceiling of DKK 18,866 per month before taxes (equivalent to approx. €2,500; values for 2019). Around 85% of Danish wage earners participate in the insurance system. Roughly 75% of the actual benefit recipients receive the maximum amount of UI benefits, yielding an effective average replacement rate of approximately 60%.

Besides these basic rules, UI benefit recipients have the opportunity to extend their potential benefit duration (PBD) from two up to three years by taking up employment after the start of their UI benefit spell. Specifically, each hour that a job seeker works after the initial unemployment registration is converted into two extra hours of UI benefits at the expiration of the two-year benchmark benefit period. Through this mechanism, individuals can prolong their PBD by maximally one year, if they work for the equivalent of six months of full-time employment during the two-year

[2] It is typically observed that short-term unemployed individuals find jobs at a faster rate (Shimer, 2008) and earn higher wages (Schmieder et al., 2016) than job seekers who have already been unemployed for an extended period, which can be attributed to dynamic selection (i.e., the most employable workers leave unemployment most rapidly), human capital depreciation, or stigma associated with long-term unemployment (Pavoni, 2009; Kroft et al., 2013).

benchmark benefit period.

Practically, benefit recipients can extend the PBD either by working in temporary jobs or by working part-time while receiving UI benefits. This implies that the total benefit period can consist of multiple unemployment periods, which are interrupted by episodes of employment, or periods in which the job seeker works in part-time jobs in parallel with an ongoing period of benefit receipt. In the following, we refer to "marginal employment" in order to account for temporary as well as part-time jobs.

An individual who re-enters unemployment after a short period of employment is eligible to receive benefits for the remaining two-year period, plus the earned extension. If the hours worked add up to one year of full-time employment, the individual is eligible for a new two-year benefit period. When UI benefits expire, individuals can receive social assistance providing substantially lower means-tested benefits. For instance, social assistance for a single person amounts to maximally DKK 11,423 per month—roughly 60% of the maximum UI benefits (value for 2019).

In addition, the Danish UI system features a (small) benefit sanction for job seekers who do not take up any employment while receiving UI benefits. Specifically, benefit payments lapse for one day—a so called *qualifying day*—every four months, if the benefit recipient has not worked the equivalent of four full-time work weeks (148 hours) within the prior four-months window.

As a result of these rules, the Danish system of UI benefits is highly flexible, but also complex to navigate for job seekers. It is worth noting that comparable regulations to increase the flexibility of unemployment insurance exist in many countries. For instance, various US states allow benefit entitlements to depend on the business cycle (Farber and Valletta, 2015; Kroft and Notowidigdo, 2016), while various European countries, such as Germany (Caliendo et al., 2016), France (Fremigacci and Terracol, 2013; Benghalem et al., 2022), and Finland (Kyyrä, 2010), have rules supporting the take-up of part-time or marginal employment (see also Cahuc, 2018, for an overview).

2.2 Knowledge gaps

To examine job seekers' understanding of the UI benefit rules and their personal benefit entitlements, we conducted an online survey among job seekers receiving UI benefits in March 2018—one week before the beginning of our intervention. A random sample of UI benefit recipients (N = 7,430) was invited for the survey. 1,154 job seekers completed the survey, corresponding to a participation rate of about 16%. Table A.1 in the appendix summarizes sociodemographic characteristics of survey participants. Compared to the average UI benefit recipient, survey respondents tend to be better educated, are more likely to be female and married, less likely to be migrants, and they have been unemployed for a somewhat longer time period.[3]

Further details about the design and implementation of the survey can be found in Appendix A.1. Most importantly, the survey included various questions to pinpoint job seekers' understanding of the UI benefit rules and their implications for the individual job seeker. We focus on two main dimensions. First, we measure job seekers' understanding of their personal UI benefit situation. In particular, we ask respondents to state the date when their own UI benefits would expire in case they do not take up any employment before benefit expiration. We then compare participants' responses to their actual benefit expiration date, which we derive from individual-level administrative data on public transfer payments. This allows us to examine whether job seekers hold accurate beliefs regarding their potential benefit duration, or whether they over- or underestimate their personal benefit entitlements.

Second, we elicit information on job seekers' knowledge of the UI benefit rules. Specifically, respondents answered four questions related to the possibilities to extend the PBD, one question related to the income effect of taking-up a short-term work opportunity, and one question related to the qualifying day described in Section 2.1. An English translation of the different survey items can be found in Appendix A.2. Based on participants' answers to the six knowledge questions, we construct a composite knowledge index, which captures a job seeker's overall understanding of the UI benefit rules. The index measures the share of correct answers on a scale from 0 (none of the six questions answered correctly) to 1 (all six questions answered correctly).

The data from the pre-intervention survey, summarized in Table 1, document pronounced gaps in job seekers' knowledge of the UI benefit system. As shown in Panel A of Table 1, only about 35% of job seekers know their benefit expiration date (to the exact week), whereas 19.2% of job seekers underestimate and 20.5% overestimate their remaining benefit duration by four weeks or more. Notably, job seekers who over- or underestimate their PBD do so by a considerable margin. On average, the absolute difference between the expected and actual expiration date amounts to more than two months (8.95 weeks).

Panel B of Table 1 summarizes responses to the questions related to UI benefit rules. On average, participants answer 52.3% of the knowledge questions correctly, indicating substantial mispercep-

[3]The characteristics that are over-represented among survey respondents tend to be positively associated with individuals' understanding of the UI benefit system. For example, job seekers with higher education, no migration background, and longer unemployment spells tend to have a better understanding of the UI benefit rules (see also Section 5 below). This suggests that knowledge gaps in the overall population of unemployed workers may be even more pronounced than those measured in our survey.

Table 1: Descriptive statistics: understanding of UI benefit system

	Mean	SD
A. Personal benefit entitlements		
Absolute difference between expected and actual PBD in weeks[(a)]	8.953	15.137
Reporting correct PBD (within a week)[(b)]	0.354	0.478
Overestimating PBD[(b)]		
by one to three weeks	0.075	0.263
by four weeks or more	0.205	0.404
Underestimating PBD[(b)]		
by one to three weeks	0.175	0.380
by four weeks or more	0.192	0.394
B. UI Benefit rules		
Knowledge index (share of knowledge questions answered correctly)[(c)]	0.523	0.271
Fraction of correct answer to question:		
(Q1) Existence of extension	0.760	0.427
(Q2) Extension gained	0.331	0.471
(Q3) Required period	0.420	0.494
(Q4) Maximum extension	0.451	0.498
(Q5) Income effect	0.529	0.499
(Q6) Qualifying day	0.484	0.500
No. of observations	1,154	

Note: The table reports descriptive statistics based on the pre-intervention survey. An English translation of the corresponding survey questions can be found in Appendix A.2.
[(a)] Absolute difference between the subjectively expected remaining benefit duration and the actual remaining benefit duration (observed in the administrative records) in weeks.
[(b)] Percentage share of survey respondents who report an expected remaining benefit duration that is (1) within the same week as the actual remaining benefit period, (2) one to three weeks longer/shorter than the actual remaining benefit period, (3) at least four weeks longer/shorter than the actual remaining benefit period.
[(c)] Share of correct answers to the six knowledge questions (Q1)–(Q6).

tions of the prevailing labor market rules among unemployed job seekers. Considering individuals' replies to the different survey items sheds further light on which aspects of the UI benefit rules are more or less well understood. While about 76% of respondents are aware that it is generally possible to extend the PBD (Q1), only 30-40% understand how extensions are calculated (Q2 and Q3), and only 45% of respondents know by how many months the PBD can maximally be extended (Q4). Similarly, the share of individuals who understand the determinants and consequences of the 'qualifying days' (Q6) and the income consequences of accepting a short-term job (Q5) are only 48% and 53%, respectively. When looking into individuals' survey responses in more detail (see Figure B.1 in the appendix), it turns out that the majority of respondents who do not answer the knowledge questions correctly tend to underestimate the overall generosity of the benefit extension rules. For instance, about 62% of respondents underestimate the length of the extension they receive for working two additional weeks, while only 5% overestimate it. Similarly, 42% of respondents underestimate by how much the benefit duration can maximally be extended, whereas only 8% overestimate the maximum benefit extension.

Result 1. *Job seekers exhibit substantial knowledge gaps regarding the UI benefit rules and their personal benefit entitlements. While a roughly equal share of job seekers over- vs. underestimates their own potential benefit duration, a majority of job seekers underestimate the possibilities for extending the UI benefit period.*

3 Randomized Controlled Trial

In our randomized controlled trial, we aim at improving job seekers' understanding of the UI benefit system by encouraging them to make use of an online information tool. The tool is embedded in the official online platform of the Danish public employment service (*jobnet.dk*). It informs job seekers about their personal benefit situation, the corresponding rules concerning benefit entitlements, and the possibilities to prolong the UI benefit period. Specifically, the online information tool comprises information on (i) the consumption of UI benefits and the remaining benefit entitlements, (ii) accumulated working hours that can be used for an extension of the PBD, (iii) the conditions to become eligible for a new two-year benefit period and (iv) the avoidance of qualifying days (see Section 2.1). The different elements of the online information tool are depicted in Appendix A.3.

The information provided in the online tool is customized to each job seeker's personal situation, and continuously updated throughout the benefit period. While the tool is accessible for all registered UI benefit recipients, our experiment aims at encouraging the usage of the tool among treated individuals by drawing their attention to the tool.[4] In particular, individuals assigned to our main treatment (henceforth also denoted as *tool treatment*) received messages containing a short, non-personalized summary of the flexible PBD rules, information about the online tool and its features, and a direct link to access the tool. After a somewhat longer first message, individuals received up to four monthly reminders if they were still registered as benefit recipient. All messages were sent to the job seekers' personal mailbox on the *jobnet.dk* platform. The exact content of the messages can be found in Appendix A.4.

In addition to our main treatment group, we consider two other groups of job seekers. First, individuals assigned to the *control group* faced a "business-as-usual" environment. As noted above, this implies that they had access to the online information tool, but received no messages or reminders encouraging them to use the tool. A third group of job seekers (henceforth denoted as

[4]In absence of our intervention, the usage of the online tool is relatively modest. The public employment service does not systematically advertise the tool as part of its counseling process. On a typical pre-intervention day, the tool generates approximately 3,000 page visits among the roughly 100,000 UI benefit recipients (cp. Figure 2).

message group) received generic messages containing general information about job search and the *jobnet.dk* platform. These messages were sent at the same points in time as those for our main treatment group, but their content was unrelated to the information tool and the UI benefit rules (cp. Appendix A.4). By comparing outcomes in the message treatment to the control group, we can identify potential effects of receiving messages or reminders by the labor market authorities *per se* (i.e., effects that are independent of the specific content of our main treatment).

3.1 Data and procedures

Our intervention was pre-registered at the RCT registry of the American Economic Association (AEARCTR-0002666). The participants in our trial comprise the full stock of UI benefit recipients in Denmark in the beginning of March 2018. For our analysis, we focus on those individuals, who are full-time insured, which yields an estimation sample of 98,641 individuals. Participants were sampled one week before the beginning of the intervention and randomly assigned to three equally sized groups (tool, message, and control), with approximately 33,000 individuals in each. To investigate the causal effects of the intervention on labor market outcomes, we link data from the experiment to comprehensive register data administered by Statistics Denmark. This provides us with detailed and highly accurate information on individuals' labor market outcomes and socio-demographic background characteristics.

In parallel to the randomized controlled trial, we also conducted a post-intervention survey among a subset of participants in the experiment. The survey allows us to assess whether the intervention had the desired effect of enhancing job seekers' understanding of the UI benefit system and their personal benefit entitlements. Towards this end, the survey included the knowledge questions described in Section 2.2. In addition, we included questions on individuals' job search strategies, their overall motivation, and perception of the public employment service. For the survey, which was conducted five weeks after the beginning of the intervention, we invited 22.5% of the overall population from the RCT ($N = 22,352$; three equally-sized subsamples from the different treatment arms). A more detailed summary of the survey design and implementation, and an overview of respondents' sociodemographic characteristics, is provided in Appendix A.1.

Figure 1: Timeline of the study (in weeks)

Pre-intervention survey	Main Message	Reminder 1	Post-intervention Survey	Reminder 2	Reminder 3	Reminder 4
$t = -1$	$t = 0$	$t = 4$	$t = 5$	$t = 8$	$t = 12$	$t = 16$

The timing of our intervention is illustrated in Figure 1. At the beginning of week $t = 0$ (March 05, 2018), the corresponding messages were sent to the tool and message group, respectively. Subsequently, individuals in both groups received up to four treatment-specific reminder messages (in weeks $t = 4, 8, 12$, and 16). Only job seekers who were still registered as unemployed within the four-week period prior to the sending date received the reminder messages. Individuals who exited and re-entered the UI system during the intervention returned to their originally assigned treatment and received the subsequent reminders, if they continued their original two-year benefit period. All treatment messages were sent out by the public employment service to individuals' personal mailbox on the *jobnet.dk* platform. Notably, all UI benefit recipients in Denmark are required to visit the platform at least once a week, ensuring that treatments are administered in a timely and comprehensive manner.

3.2 Sample characteristics and treatment take-up

Table 2 provides an overview of participants' background characteristics, separated by treatment status. The job seekers in our experiment are on average 40 years old, about 52% of participants are female, 34% are married, and 34% have a university degree. The average participant spent about 51 weeks in unemployment during the last five years, had an average gross monthly labor income of roughly DKK 18,000 (approx. €2,450), and worked on average 22 hours per week in the past five years. While we observe only minor differences in background characteristics across treatments, a few of the balancing tests reported in the rightmost column of the table turn out to be statistically significant (e.g., time spent in unemployment in the past year). To address these small differences between treatment arms, we condition on a rich set of covariates in our empirical analysis. Furthermore, it should be noted that that the treatment does not affect survey participation (see Table A.2 in the appendix).

To examine treatment take-up, Figure 2 plots the overall usage of the online information tool based on data from Google Analytics. During our intervention, weekly visits to the tool increase by roughly 50% relative to the pre-intervention period. It can be seen that the increase is concentrated to a few days following the dates of sending out the intervention messages, but the additional page visits do not crowd out usage of the tool in the periods between sending dates. The stark and concentrated spikes in page visits strongly suggest that the increase in usage of the tool is connected to our treatment (rather than additional traffic from non-treated users, who could—in principle—also access the tool; cp. Section 3). As a second, more direct measure of treatment take-up, we also

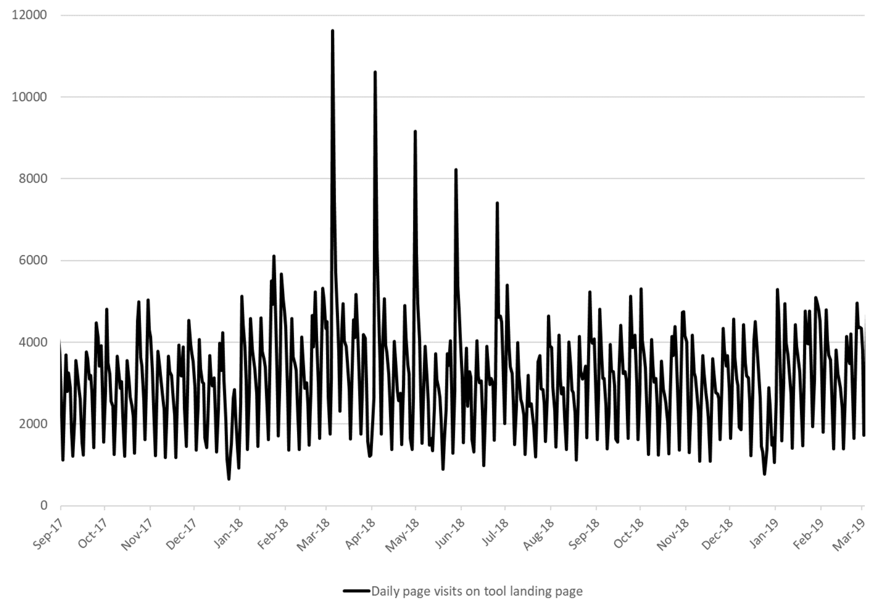
Table 2: Summary statistics and balancing tests

	Treatment			
	Control (C)	Message (M)	Tool (T)	$P-$value
No. of observations	32,905	32,876	32,860	
Educational level				
None (or missing)	0.082	0.082	0.087	0.038
Less than high school	0.176	0.176	0.181	0.264
High school	0.402	0.401	0.397	0.332
Bachelor degree (or equiv.)	0.241	0.241	0.241	0.978
Master degree (or equiv.)	0.098	0.100	0.095	0.094
Male	0.482	0.475	0.481	0.154
Age				
18-25 years	0.117	0.118	0.115	0.428
26-35 years	0.332	0.332	0.331	0.926
36-45 years	0.192	0.191	0.195	0.402
46-55 years	0.195	0.198	0.194	0.366
56-65 years	0.165	0.161	0.165	0.252
Household size				
One person	0.195	0.195	0.193	0.797
Two persons	0.344	0.342	0.345	0.652
Three persons	0.204	0.204	0.200	0.327
Four or more persons	0.258	0.259	0.262	0.492
Married	0.338	0.339	0.345	0.111
Children				
One child	0.164	0.164	0.162	0.650
Two or more children	0.172	0.171	0.173	0.730
Migration status				
1^{st} generation	0.193	0.191	0.198	0.065
2^{nd} generation	0.032	0.034	0.033	0.446
Weeks of UI benefits (current spell)	31.78	32.44	32.24	0.011
Weeks of UI benefits				
in last year	24.07	24.44	24.24	0.013
in last 5 years	50.23	50.80	50.72	0.128
Months employed				
in last year	6.049	5.994	6.032	0.261
in last 5 years	38.288	38.020	38.276	0.065
Average monthly earnings				
in last year	17,868	17,752	17,833	0.718
in last 5 years	18,399	18,281	18,389	0.461
Average weekly working hours				
in last year	19.20	19.13	19.06	0.425
in last 5 years	22.74	22.16	22.24	0.494

Note: Percentage shares unless indicated otherwise. $P-$values are based on F-tests for joint significance of treatment coefficients in separate regressions of each of the characteristics on dummies for the different treatment conditions.

collected individual-level data on whether job seekers in the tool treatment opened their messages and clicked on the link to the online information tool (see Table B.1 in the appendix for a detailed overview). We observe that 85-95% of job seekers assigned to the tool treatment open the main message and subsequent reminders. The figures for the message treatment also lie in this range. For each message sent to job seekers in the tool treatment, about 20% of participants click on the link to the online information tool. When considering all messages together, 45% of individuals in the tool treatment clicked on the link to the tool at least once.

Figure 2: Usage of the online information tool



Note: The figure depicts the number of page visits for the online information tool, based on data from Google Analytics.

4 Theoretical Framework

Before presenting the results of our intervention, we discuss the potential effects of improving job seekers' understanding of the UI benefit system through the lens of a stylized partial-equilibrium job search model with endogenous effort and a finite benefit period (following Mortensen, 1986; Van den Berg, 1990). We adapt the framework to the Danish UI benefit rules by allowing individuals to endogenously extend their benefit period. A more extensive theoretical analysis of part-time unemployment benefits and their consequences for job search and unemployment duration can be found in Benghalem et al. (2022).

4.1 Search model with finite benefit duration and extension possibility

While being unemployed, job seekers receive UI benefit payments b for a certain period T. The latter covers the initial two-year benefit period, denoted T_0, plus any earned extensions of the benefit period. If individuals are unemployed beyond T, they receive social assistance, which we normalize to 0 for expositional simplicity. At each time t of their unemployment spell, job seekers decide on their total level of search effort, s_t, and how they allocate their effort to search for either regular or marginal employment, where $\kappa_t \in [0, 1]$ indicates the share of the total effort devoted to the search for regular jobs. Regular job offers arrive at rate $\lambda(\kappa_t s_t)$ and accepting an offer yields a present value of V. Effort costs $\gamma(s_t)$ depend on the total effort level, with $\gamma'(s_t) > 0$ and $\gamma''(s_t) > 0$.

For illustrative purposes, we make three simplifying assumptions. First, individuals who find a regular job leave unemployment forever, while marginal employment provides a means to extend the potential benefit period beyond T_0. This is motivated by the fact that the benefit extension rules provide explicit incentives to work in temporary or part-time jobs.[5] Second, we assume that the wages in marginal jobs are equivalent to the benefit level b, to abstract from incentives of generating extra income. Third, we also abstract from possible stepping-stone effects and assume that the chances of finding a regular job do not depend on the effort to search for marginal jobs (other than through a reduction of κ_t).

Given the UI benefit rules described in Section 2.1, whether a job seeker is eligible for UI benefits in the next period $t + 1$ is indicated by the function $\mu(t + 1)$:

$$\mu(t+1) = \begin{cases} 1 & \text{if } t < T_0 \\ I(\sum_{i=1}^{t}(1-\kappa_i)s_i > \bar{s}_t) & \text{if } t \geq T_0 \end{cases} \tag{1}$$

During the initial two-year benefit period (until period T_0), benefit payments are guaranteed such that job seekers receive UI benefits independently of their search behavior. After period T_0, benefit eligibility depends on the previously collected working hours and, hence, on the effort exerted searching for marginal jobs. In particular, searching more intensively for marginal jobs, that is, raising $(1 - \kappa_t)s_t$, increases the likelihood of avoiding benefit expiration. In this context, $\bar{s_t}$ denotes the minimum effort level invested up until period t that ensures benefit eligibility in the subsequent period $t + 1$.

If job seekers can freely choose the number of hours they would like to work in marginal jobs and have perfect knowledge about the UI benefit system, there is no uncertainty about benefit eligibility in future periods. However, in reality, this might not be the case for two reasons. First, individuals may have imperfect knowledge about the UI benefit rules and their remaining entitlements. Second, there is uncertainty as to how effort translates into working hours because job seekers need to search for jobs in a competitive job market. Therefore, individuals hold a subjective belief about their

[5] Note that, in principle, all working hours, independently of the type of employment, can be used for benefit extensions. However, once individuals collected working hours equivalent to 12 months of full-time employment, they earned "fresh" entitlements that allow them to receive UI benefits for a new two-year period. This implies that especially working hours collected in marginal jobs can effectively be used for extensions of the original benefit period.

future eligibility for UI benefits, which is denoted by $\widehat{\mu}$. This belief could be affected by individuals' knowledge gaps regarding (1) the initial benefit expiration date T_0 and (2) the minimum effort level $\bar{s}_t$ that is required to extend the benefit period.

For a given discount rate β, the (subjective) value of being unemployed at time t, $U(t)$, is given by:

$$U(t) = \max_{s_t, \kappa_t} b - \gamma(s_t) + \beta \left\{ \lambda(\kappa_t s_t)V + \widehat{\mu}(1 - \lambda(\kappa_t s_t))U(t+1) \right\} \tag{2}$$

The value of unemployment consists of (i) utility during unemployment (utility of benefits b minus search costs γ), (ii) the expected income when a regular job is found at rate $\lambda(\kappa_t s_t)$, and (iii) the perceived likelihood of still receiving UI benefits in the future when no regular job is found, $\widehat{\mu}$. In each period, the optimal search strategy can be described by the effort level $s^*(t)$ and the allocation of search effort devoted to regular vs. marginal employment, $\kappa^*(t)$. The optimal search strategy thus trades off effort costs and marginal returns to effort in both dimensions, i.e., the search for regular and marginal employment.

4.2 Potential effects of the information intervention

The information intervention aims at improving job seekers' understanding in two dimensions—their personal entitlements and the precise rules underlying PBD extensions. Both of these dimensions affect a job seeker's beliefs about future benefit eligibility $\widehat{\mu}$. A change in $\widehat{\mu}$, in turn, may trigger a number of different behavioral responses depending on an individual's personal situation.

Learning about personal entitlements: Informing job seekers about their personal benefit entitlements affects their belief about the remaining period until their benefits expire, $T_0 - t$, and therefore how severe they perceive the risk of benefit expiration. The consequences for their search behavior depend on whether they under- or over-estimate their remaining benefit duration in absence of treatment. Job seekers who were too optimistic about their own situation should increase their overall search effort $s^*(t)$ in each period when they learn about their true entitlements. This is because the risk of benefit expiration is more severe than they thought, while we would expect to find opposite effects for job seekers who were too pessimistic initially.

Moreover, as discussed in more detail by Nekoei and Weber (2017), job seekers may not only decide on their search effort, but also about the type of jobs they target characterized by their present value, V. The fact that benefits expire after a certain period should encourage job seekers to become less selective over the unemployment spell, i.e. accepting jobs of lower quality. This implies that those who become more pessimistic (optimistic) about their remaining benefit period due to our intervention should also become less (more) selective regarding their job choice.[6]

Understanding of benefit extensions: Improving job seekers' knowledge of the benefit rules allows them to update their beliefs about the minimum effort level $\bar{s}_t$ that is required to receive UI benefits beyond the initial expiration date, T_0. Given that individuals in our sample tend to systematically underestimate the possibilities to extend the UI benefit period initially (see Section 2.2), we expect this to provoke two types of behavioral responses.

First, the intervention should increase the *perceived returns to search for marginal jobs* such that job seekers choose lower values of κ. This effect is stronger for those with a high personal risk of exhausting UI benefits because individuals who exit unemployment faster are less likely to make use of the extensions they gained. This implies that especially job seekers who are already unemployed for an extended period of time and are therefore close to benefit expiration (i.e. $T_0 - t$ is small) should respond to the intervention. For the same reason, individuals who anticipate that they have low prospects of finding a regular job also have greater incentives to work in marginal jobs even when they are still at the beginning of their benefit spell. The resulting labor market effects can be twofold. On the one hand, a stronger focus on marginal employment could create a lock-in effect that might be detrimental to the job seeker's labor market integration (see Böheim and Weber, 2011; Fremigacci and Terracol, 2013; Kyyrä et al., 2013, for empirical examples). On the other hand, marginal employment can also provide a stepping stone towards a permanent full-time job.[7]

Second, the forward-looking nature of the optimization problem implies that even individuals who do not search for marginal jobs at all could change their behavior in response to a better understanding of the benefit rules. This is because the expected value of remaining unemployed $U(t+1)$ increases when job seekers learn about the possibility of gaining an extension in the future. Such an *option value effect* should allow job seekers to be more selective and may encourage them to search less intensively for new employment. The effect is particularly strong when there is more

[6] In general, learning about the true benefit duration should provoke similar behavioral consequences as actual changes of the PBD would. Various studies have shown that a longer benefit duration encourages job seekers to reduce their search effort (Marinescu, 2017; Lichter and Schiprowski, 2021), increases the unemployment duration (Katz and Meyer, 1990; Card and Levine, 2000; Lalive et al., 2006; Van Ours and Vodopivec, 2006; Chetty, 2008; Schmieder et al., 2012, 2016) and, in some cases, improves subsequent job matches (Centeno and Novo, 2006; Nekoei and Weber, 2017).

[7] In particular, marginal jobs can act as an effort signal, provide a valuable network, or reduce the employers' uncertainty about the quality of a candidate (see, e.g., Booth et al., 2002; Freier and Steiner, 2007; Fremigacci and Terracol, 2013; Kyyrä et al., 2013; Caliendo et al., 2016).

time before benefits expire ($T_0 - t$ is large).

Taken together, the theoretical discussion suggests that our empirical analysis should account for various dimensions of heterogeneity. For individuals at the beginning of the benefit spell, we expect the option value effect to be particularly pronounced. Those who are at-risk of staying unemployed for an extended period at the same time face strong incentives to work in marginal jobs. While the latter is also true for job seekers who are already unemployed for an extended period, the option value effect is less important for them because there is little time left until benefit expiration. Finally, we expect differential effects for those who are baseline optimistic and pessimistic about their own entitlements. Our discussion of empirical results in the next sections will focus on the impact of our intervention on job seekers' understanding of the UI benefit system and labor market outcomes, accounting for these dimensions of heterogeneity.

5 Does the Intervention Increase Job Seekers' Knowledge?

In a first step of our empirical analysis, we examine whether the intervention improves job seekers' understanding of the UI benefit rules and their personal benefit entitlements. Using data from the post-intervention survey, we estimate models of the following form:

$$Y_i = \beta_0 + \beta_1 D_i + \beta_2 (D_i \times EBD_i) + \beta_3 X_i + \varepsilon_i. \tag{3}$$

As our two main outcome variables, Y_i, we consider (i) the accuracy of job seekers' beliefs regarding their own benefit entitlements and (ii) their score on the knowledge index described in Section 2.2. D_i indicates the treatment status (dummy variables for the tool and message group, respectively). X_i is a vector of pre-intervention control variables, including socio-demographic characteristics and labor market histories, as presented in Table 2, plus dummies for the job seeker's place of residence (98 municipalities) and her unemployment-fund membership (24 different funds). Since the information tool might affect different subgroups of job seekers differentially, we also consider specifications that allow for group-specific treatment effects. In particular, we interact the treatment indicator with an indicator of the job seeker's elapsed benefit duration at the beginning of our intervention, EBD_i. We distinguish between three subgroups: (i) short-term benefit recipients with an elapsed benefit duration of less than six months (54% of individuals in our sample), (ii) medium-term benefit recipients who received UI benefits for six to twelve months before the intervention started (24%), and (iii) long-term benefit recipients with an elapsed benefit duration of more than

12 months (22%).[8]

Throughout our analysis, we focus on intention-to-treat estimates (ITTs), ignoring whether treated individuals actually opened the treatment messages or clicked on the link to the information tool to avoid selection bias. Note that deriving local average treatment effects is not straightforward in our setup, as exposure to the treatment may already commence when individuals open the treatment message, and not only when they use the online information tool.

Panel A of Table 3 documents the impact of the intervention on job seekers' understanding of their personal benefit entitlements. We find that the (absolute) difference between job seekers' subjectively expected and actual benefit expiration date is about 1.7 weeks smaller for treated individuals than for the control group ($p = 0.047$; see Column 1 of Table 3). With a baseline knowledge gap of 8.8 weeks among untreated job seekers (see bottom part of Table 3), the inaccuracy regarding personal benefit entitlements is thus reduced by about 20%. The effect is especially pronounced for long-term benefit recipients and individuals who are still at the beginning of the unemployment spell (see Column 2 of Table 3), whereas the effect for job seekers with an intermediate elapsed benefit duration is rather small and statistically insignificant.

Interestingly, the reduced inaccuracy among short- and long-term unemployed job seekers has very different origins. It turns out that—in absence of our intervention—job seekers at the beginning of the unemployment spell are more likely to underestimate their potential benefit duration, whereas long-term unemployed individuals tend to overestimate their remaining benefit entitlements. This can be seen in Panel A of Figure B.2 in the appendix, which displays group-specific distributions of the knowledge gap for benefit entitlements, based on data from our pre-intervention survey. In Columns (3)–(6) of Table 3, we therefore estimate treatment effects on the accuracy of job seekers' beliefs, distinguishing between individuals who overestimate vs. underestimate their potential benefit duration.[9] In line with the baseline differences in (mis)perceptions, we find that the tool treatment predominantly reduces the degree to which short-term unemployed job seekers underestimate their benefit entitlements (Column 6), whereas it decreases excessive optimism about the remaining benefit entitlements among long-term unemployed job seekers (Column 4).

Panel B of Table 3 reports treatment differences in job seekers' knowledge of the UI benefit

[8] The six-month threshold is oriented towards the definition of long-term unemployment used by the Danish public employment service to determine job seekers' eligibility for various ALMP programs, such as training and wage subsidies. The one-year threshold corresponds to the more common international definition of long-term unemployment (e.g., OECD, 2019).

[9] In particular, we consider the absolute difference between the expected and actual PBD in weeks (as in Columns 1 and 2), but set the corresponding outcome variable to zero if the individual does not overestimate the PBD (see Columns 3 and 4), and if the individual does not underestimate the PBD (Columns 5 and 6), respectively.

Table 3: Treatment effects on knowledge about UI benefit rules and personal benefit entitlements

	A. Personal benefit entitlements						B. UI benefit rules	
	Difference between expected and actual PBD[(a)]						Knowledge index	
	Absolute difference (in weeks)		Overestimation of PBD (in weeks)		Underestimation of PBD (in weeks)		Share of correct answers (0=low; 1=high)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Tool treatment	-1.67** (0.84)		-0.90 (0.74)		-0.77 (0.51)		0.050*** (0.012)	
Tool × UI benefit duration ≤26 weeks		-2.30* (1.22)		-0.94 (1.08)		-1.36* (0.74)		0.046*** (0.018)
Tool × UI benefit duration 27-52 weeks		0.36 (1.53)		0.69 (1.35)		-0.33 (0.93)		0.055** (0.024)
Tool × UI benefit duration >52 weeks		-2.89* (1.70)		-2.75* (1.50)		-0.13 (1.03)		0.052** (0.026)
Message treatment	-0.14 (0.82)		0.52 (0.72)		-0.65 (0.50)		0.025** (0.012)	
Message × UI benefit duration ≤26 weeks		-2.00 (1.25)		-1.11 (1.10)		-0.89 (0.76)		0.034* (0.018)
Message × UI benefit duration 27-52 weeks		0.17 (1.48)		1.92 (1.30)		-1.75* (0.90)		0.023 (0.023)
Message × UI benefit duration >52 weeks		2.41 (1.58)		1.33 (1.39)		1.08 (0.96)		0.011 (0.025)
No. of observations	2,000	2,000	2,000	2,000	2,000	2,000	2,805	2,805
Mean values control group								
full sample	8.78		5.43		3.36		0.505	
UI benefit duration ≤26 weeks		9.64		5.43		4.21		0.457
UI benefit duration 27-52 weeks		7.25		3.39		3.85		0.522
UI benefit duration >52 weeks		9.02		7.96		1.06		0.591
P-values tool v. message								
full sample	0.068		0.055		0.816		0.045	
UI benefit duration ≤26 weeks		0.809		0.881		0.539		0.518
UI benefit duration 27-52 weeks		0.901		0.360		0.126		0.189
UI benefit duration >52 weeks		0.001		0.004		0.223		0.098

Note: The table reports treatment differences in knowledge (intention-to-treat effects) among participants in the main survey. Standard errors are reported in parentheses. ***/**/* indicates statistical significance at the 1%/5%/10%-level, respectively. In all models, we control for socio-demographic characteristics, labor market histories, place of residence (98 municipalities) and membership of unemployment funds (24 in total).

[(a)]Refers to the difference between the subjectively expected remaining benefit duration elicited through the online survey and the actual remaining benefit duration observed in the administrative records in weeks. In columns (3) and (4), we consider the absolute difference. In column (5)-(8), we decompose the absolute difference into instances that over-, respectively underestimate their remaining benefit duration. Therefore, we set the corresponding outcome variable to zero if the individual does not overestimate (Column (5) and (6)), respectively if the individual does not underestimate (Column (7) and (8) the PBD).

rules. We find that the share of correct answers to the knowledge questions is significantly higher for job seekers in the tool treatment than for their counterparts in the control group. As shown in Column (7), the improvement on the knowledge index amounts to five percentage points ($p < 0.001$), corresponding to an increase of 10% compared to the baseline knowledge level measured for individuals in the control group. When separately considering the subgroups of short-, medium-, and long-term unemployed job seekers, we see that the (absolute) improvement in knowledge scores is rather similar across subgroups. The treatment differences in the knowledge index range from 4.6 to 5.5 percentage points (cp. Column (8) of Table 3). As long-term unemployed job seekers tend to have a better understanding of the rules than job seekers at the beginning of the unemployment spell (see bottom part of Table 3), this implies somewhat larger relative effect sizes for short-term unemployed individuals.

In the middle part of Table 3, we examine knowledge differences between individuals in the message treatment and the control group. We observe no systematic and statistically significant effect of the message treatment on job seekers' understanding of their personal benefit entitlements (Columns 1–6 of Table 3). For the knowledge index depicted in Columns (7)–(8), we observe a modest positive effect of the message treatment relative to the control group ($p = 0.037$). The subgroup analysis reveals that this effect is most pronounced for individuals who have been unemployed for a relatively short period of time. Although the message treatment does not contain any information about the UI benefit rules, the treatment might encourage job seekers to further explore the *jobnet.dk* platform.[10] One could speculate that doing so is particularly beneficial for job seekers who have not yet become accustomed with the online portal and the UI benefit system. When comparing knowledge differences between individuals in the tool treatment and the message treatment, we find significant positive effects of the tool treatment both on job seekers' knowledge of the UI benefit rules and the understanding of their own benefit entitlements ($p = 0.045$ and $p = 0.068$ respectively; see post-estimation test at the bottom of Table 3).

Result 2. *The intervention improves job seekers' knowledge of UI benefit rules and their personal benefit entitlements.*

(i) Individuals in the tool treatment have more accurate expectations regarding their own benefit entitlements. The overall higher accuracy results from a reduction of overly pessimistic beliefs among short-term unemployed individuals and a reduction of overly optimistic beliefs among long-term unemployed job seekers.

[10] The message received by individuals in this treatment arm started with the sentence "Use *jobnet.dk* regularly to know your possibilities and make the most out of them", cf. Appendix A.4.

(ii) Individuals in the tool treatment answer a significantly higher share of questions about the prevailing UI benefit rules correctly.

6 Does the Intervention Alter Job Seekers' Labor Market Outcomes?

In a next step, we analyze whether the knowledge increase in response to our intervention is associated with treatment effects on realized labor market outcomes. As our main outcome variables, we consider individuals' working hours and labor earnings, cumulated over time horizons of one year and two years after the beginning of the intervention. Note that, while our analysis in the previous section relied on the combination of our RCT with survey data on job seekers' knowledge, the following analysis is based on the linkage between the experiment and administrative data from Statistics Denmark. This does not only provide us with highly accurate data on individuals' labor market outcomes, but it also allows us to examine labor market effects for the full stock of UI benefit recipients.

Columns (1)–(4) of Table 4 present the treatment effects on cumulated working hours and earnings in the first year after the beginning of the intervention. As shown in specifications (1) and (3), the tool treatment has no significant effect on the average UI benefit recipient in our sample. While point estimates are negative for both outcomes, the effects are rather small and not statistically significant. When considering outcomes separately for the subgroups with shorter and longer elapsed UI benefit durations, we observe that treatment effects are substantially more pronounced and also statistically significant for long-term benefit recipients. Over the course of one year after the beginning of the intervention, long-term benefit recipients in the tool treatment work, on average, about 22 hours less ($p = 0.016$) and earn about DKK 3,300 less ($p = 0.069$) than those in the control group. These numbers correspond to a relative decrease of employment and earnings of 3.3% and 2.7%, respectively, when comparing treatment coefficients to the baseline employment and earnings levels for the corresponding job seekers in the control group. In contrast, for short- and medium-term benefit recipients, we observe no significant differences in cumulated labor market outcomes between treated and untreated individuals.

Columns (5)–(8) of Table 4 depict differences in labor-market outcomes over a two-year time horizon. The overall pattern of results is very similar to the shorter one-year observation period. In particular, we still observe no significant treatment differences in the overall sample, but sizable and statistically significant effects for long-term benefit recipients. Comparing the point estimates on

Table 4: Treatment effects on cumulated labor market outcomes

Dependent variable	Cumulated outcomes within 12 months				Cumulated outcomes within 24 months			
	Working hours		Labor earnings in DKK		Working hours		Labor earnings in DKK	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Tool treatment	-5.43 (4.08)		-948 (804)		-3.63 (8.57)		-566 (1,703)	
Tool × UI benefit duration ≤26 weeks		1.41 (5.99)		283 (1,265)		12.4 (12.18)		2,534 (2,575)
Tool × UI benefit duration 27-52 weeks		-4.00 (9.51)		-1,297 (1,776)		-6.09 (15.64)		-1,365 (2,761)
Tool × UI benefit duration >52 weeks		-22.26** (9.25)		-3,302* (1,813)		-37.20** (18.67)		-6,628* (3,629)
Message treatment	-1.76 (4.86)		-420 (968)		-0.47 (9.43)		-321 (1,972)	
Message × UI benefit duration ≤26 weeks		-0.97 (7.42)		-445 (1,586)		1.32 (13.40)		-552 (2,969)
Message × UI benefit duration 27-52 weeks		1.98 (8.09)		48 (1,566)		2.65 (16.09)		583 (3,173)
Message × UI benefit duration >52 weeks		-7.33 (9.49)		-779 (1,718)		-5.11 (19.94)		-540 (3,730)
No. of observations	98,641	98,641	98,641	98,641	98,641	98,641	98,641	98,641
Mean values control group								
full sample	796		149,076		1,856		351,720	
UI benefit duration ≤26 weeks		879		169,268		2,015		393,166
UI benefit duration 27-52 weeks		710		130,366		1,710		317,784
UI benefit duration >52 weeks		681		117,991		1,608		283,272
P-values tool v. message								
full sample	0.454		0.585		0.680		0.880	
UI benefit duration ≤26 weeks		0.738		0.617		0.342		0.231
UI benefit duration 27-52 weeks		0.588		0.481		0.652		0.558
UI benefit duration >52 weeks		0.100		0.116		0.110		0.081

Note: The table reports treatment differences in labor market outcomes (intention-to-treat effects) among participants in the randomized controlled trial. Standard errors are reported in parentheses. ***/**/* indicates statistical significance at the 1%/5%/10%-level, respectively. In all models, we control for socio-demographic characteristics, labor market histories, place of residence (98 municipalities) and membership of unemployment funds (24 in total).

cumulated labor market outcomes within 12 and 24 months indicates that the negative effect of the tool treatment is not substantially alleviated over time. Overall working hours and earnings decrease by about 2.3% within 24 months ($p = 0.046$ and $p = 0.068$, respectively), relative to comparable long-term unemployed job seekers in the control group.

Message treatment: Before turning to the analysis of potential mechanisms behind the observed effects in the tool treatment, it should be noted that the message treatment has no significant effects on employment and earnings among any subgroup (see estimations in the middle part of Table 4).

This indicates that the increase in understanding of the benefit rules in this treatment does not translate into differences in labor market outcomes. Given that the knowledge effects of the message treatment are rather small and do not come along with a better understanding of the job seekers' personal entitlements, this finding is perhaps not too surprising. As we find no indications that the message treatment affects overall labor market outcomes or the nature of subsequent employment relationships, our discussion in what follows will focus on the effects of the tool treatment.

Result 3. *We observe no significant treatment effects on employment and earnings in the full sample of UI benefit recipients. For individuals with an elapsed UI benefit duration of more than 52 weeks, the tool treatments causes a reduction of working hours and labor earnings in the two years after the beginning of the intervention.*

6.1 Dynamic selection and the risk of long-term unemployment

While it appears plausible that a better understanding of the UI benefit system triggers differential behavioral reactions among individuals at different points of the benefit spell, this heterogeneity can have different origins. First, as documented in Table 3, the intervention has differential effects on job seekers' understanding of their personal benefit entitlements. While the treatment primarily reduces overly optimistic expectations among long-term unemployed individuals, it tends to make short-term unemployed job seekers less pessimistic about their remaining benefit entitlements. Second, as noted in Section 4, different groups of job seekers may exhibit different reactions to information about the UI benefit rules. In particular, individuals with a high risk of benefit expiration (e.g., long-term unemployed individuals who are already close to the expiration date) may react more strongly to information about possibilities to extend one's PBD. Finally, and relatedly, specific types of job seekers (e.g., individuals with low overall job finding prospects) may be particularly represented in the group of long-term unemployed job seekers, as a result of dynamic selection.

To shed further light on the role of dynamic selection and the drivers behind the observed differences in treatment effects for short- and long-term unemployed individuals, we take a closer look at the group of short-term benefit recipients, who have been unemployed for less than 26 weeks at the start of the intervention. This group includes, both, job seekers with good overall employment prospects and others with a higher risk of remaining unemployed for an extended period of time. To better understand how differences in job seekers' overall labor market prospects—and corresponding differences in the risk of benefit expiration—contribute to the heterogeneous treatment effects documented above, we divide the group of short-term benefit recipients based on their personal (predicted) risk of long-term unemployment.

To obtain a proxy for the risk of staying unemployed for an extended period, we first estimate job seekers' probability of becoming long-term unemployed based on an out-of-sample prediction. For our main specification, we draw an additional sample of entries into unemployment in the year 2017 from the administrative records and estimate the determinants of an individual's likelihood of staying on unemployment benefits for more than one year. Specifically, we estimate a LASSO-logit model and account for regional characteristics, education, socio-demographic information and labor market histories. In a second step, we use the estimated coefficients to predict the risk of long-term unemployment (LTU) for short-term unemployed individuals in the experimental sample. Based on the predicted probabilities, we divide the sample of short-term benefit recipients into individuals with a low (below median) and high (above median) LTU risk.[11]

Table 5: Treatment effects by risk of long-term unemployment$^{(a)}$

	Sample: UI benefit duration $\leq$ 26 weeks	
	Total working hours within 24 months	Total labor earnings (in DKK) within 24 months
	(1)	(2)
Effect of tool treatment by risk of long-term unemployment (LTU)$^{(a)}$		
Low LTU risk	35.07*	7,441*
	(18.36)	(3,904)
High LTU risk	-9.56	-1,582
	(17.45)	(3,710)
P-value: low v. high LTU risk	0.078	0.094
No. of observations	53,383	53,383
Mean values control group		
Low LTU risk	2,195	452,033
High LTU risk	1,856	340,659

Note: The table reports the effects of the tool treatment on labor market outcomes (intention-to-treat effects) among participants in the randomized controlled trial with an elapsed benefit duration of 26 weeks or less. Standard errors are reported in parentheses. ***/**/* indicates statistical significance at the 1%/5%/10%-level, respectively. In all models, we control for socio-demographic characteristics, labor market histories, place of residence (98 municipalities) and membership of unemployment funds (24 in total). Additionally, all specifications account for the effect of the message treatment (coefficients are not shown).
$^{(a)}$The risk of long-term unemployment (LTU) is estimated based on a sample of entries into unemployment in 2017 using a LASSO logit approach. A robustness check relying on entries in 2016 to predict the risk of long-term unemployment is presented in Table B.3 in Appendix B.3.

Table 5 presents estimated treatment effects on working hours and labor earnings over a two-year time horizon, separately for low- and high-risk job seekers. The analysis reveals two important

[11]We test the sensitivity of our results with respect to the exact specification used for the prediction model. Table B.2 presents the estimated coefficients for different prediction models. Moreover, Table B.3 shows estimates paralleling those in Table 5, but based on a prediction model that relies on a sample of entries into unemployment in 2016 rather than 2017.

results. First, we find that the tool treatment improves the labor market outcomes of job seekers with a low LTU risk. Over a period of 24 months, working hours ($p = 0.056$) and labor earnings ($p = 0.057$) increase by about 1.6%, relative to the mean of low-risk individuals in the control group. This positive treatment effect on the group of benefit recipients with good overall labor market prospects stands in stark contrast to the negative treatment effects for long-term unemployed job seekers documented in Table 4. Second, the results from Table 5 also indicate that low- and high-risk types react differently to the treatment. Treatment effects for short-term unemployed job seekers with a high LTU risk are qualitatively similar to those observed for long-term unemployed individuals (cp. Table 4). The negative effects on labor market outcomes of high-risk individuals are, however, substantially smaller and not statistically significant. At the same time, treatment effects on working hours and earnings differ systematically from those observed for low-risk individuals ($p = 0.078$ and $p = 0.094$, respectively).

When comparing the estimated treatment effects in Table 5 to those in Table 4, it is worth noting that treatment take-up differs between job seekers who have been unemployed for shorter vs. longer periods of time. While a similar fraction of job seekers opens the main treatment message (94.7% for long-term unemployed job seekers, and 92.4% vs. 91.2% for short-term unemployed job seekers with a high vs. low LTU risk, respectively), there are pronounced differences in the fraction of job seekers who access the online information tool. 57.1% of those who already received benefits for more than 52 weeks click on the link to information tool. This compares to only 34.4% of short-term benefit recipients with a low LTU risk, and 38.8% of short-term benefit recipients with a high LTU risk, respectively.

Result 4. *We observe differences in treatment effects between short-term unemployed job seekers with a high vs. low predicted risk of staying unemployed for an extended period of time.*

(i) For short-term unemployed individuals with a low LTU risk, the treatment leads to an increase in working hours and earnings in the two years after the beginning of the intervention.

(ii) For short-term unemployed individuals with a high LTU risk, the treatment has no statistically significant effect on working hours and earnings.

The treatment effects for high- and low-risk job seekers indicate that differences in job seekers' personal characteristics and overall employment prospects contribute to the heterogeneous labor market effects of our intervention.[12] To further explore how the differential treatment effects on

[12] At the same time, we find no indication for differential *knowledge effects* of the intervention between the low- and high-risk group (see Appendix Table B.4) or for differential effects of the intervention on job seekers' overall motivation and their perceptions of being pressured or monitored by the labor-market authorities (Appendix Table B.5). This is

overall working hours and earnings come about, we next take a closer look at differences in job search patterns and the characteristics of the resulting matches. We always present separate results for three groups: short-term benefit recipients with a low risk and a high risk of long-term unemployment (henceforth denoted as *low LTU risk* and *high LTU risk*, respectively), and actual long-term unemployed job seekers who have already received UI benefits for more than 52 weeks (henceforth denoted as *already LTU*).

6.2 Job characteristics

In a first step, we investigate the nature of the resulting job matches. Specifically, we analyze treatment effects on hours worked in different "types" of jobs, accounting for two dimensions of job characteristics. First, we differentiate between regular and marginal employment, which is motivated by the fact that the benefit extension rules provide additional returns to work in temporary or part-time employment relationships (as literally every hour worked can be used for a benefit extension). For our analysis, we define marginal jobs as those with weekly working hours below 25% of the full-time equivalent (of 37 working hours per week) to capture small work opportunities that are typically perceived as a means for gaining a benefit extension. As a second job characteristic, we consider different wage levels to proxy for the quality of employment. In particular, we present treatment effects for employment in the bottom and the top tercile of the wage distribution for individuals in our sample (calculated within a given calendar month).

The findings, presented in Table 6, indicate that the three groups of interest respond differently to our intervention with regard to the types of jobs they accept. For treated individuals with a low LTU risk, we observe an increase in regular employment (see Panel A.1) and employment in high-wage jobs (see Panel A.2), relative to the corresponding group of job seekers in the control group. This indicates that low-risk job seekers might become more selective regarding their job choices. In line with this idea, we also find that the tool treatment significantly reduces the geographical search radius of short-term benefit recipients with a low LTU risk (see Table B.6 in the appendix).[13] While we observe no systematic treatment differences in job characteristics of short-term unemployed job seekers with a high LTU risk (see Panels B.1 and B.2 of Table 6), the intervention does have an impact on job seekers who are already long-term unemployed. For workers who have already received

consistent with the notion that an enhanced understanding of the UI benefit system (e.g., regarding the possibilities to extend the PBD) may trigger differential reactions for job seekers with a higher or lower risk of exhausting their UI benefits.

[13] This analysis relies on job applications registered through the *jobnet.dk* platform within 12 months after the start of the intervention (see also Fluchtmann et al., 2022). An overview of treatment effects on other job search measures elicited through registered applications and our online survey can be found in Appendix Table B.6.

Table 6: Treatment effects on working hours by job characteristics

	A.1 Low LTU risk[a]		B.1 High LTU risk[b]		C.1 Already LTU[c]	
	Working hours in 24 months by type of job[d]		Working hours in 24 months by type of job[d]		Working hours in 24 months by type of job[d]	
1) Type of employment	Regular employment	Marginal employment	Regular employment	Marginal employment	Regular employment	Marginal employment
	(1)	(2)	(3)	(4)	(5)	(6)
Effect of tool treatment	36.60^{**}	0.11	-10.26	-1.05	-38.32^{**}	3.20^{**}
	(17.51)	(1.03)	(17.10)	(1.18)	(18.59)	(1.43)
Mean value control group	2,043.7	42.6	1,735.7	41.3	1,498.5	43.0
No . of observations	25,274	25,274	28,109	28,109	21,938	21,938
	A.2 Low LTU risk[a]		**B.2 High LTU risk**[b]		**C.2 Already LTU**[c]	
	Working hours in 24 months by wage level[e]		Working hours in 24 months by wage level[e]		Working hours in 24 months by wage level[e]	
2) Wage level	High-wage employment	Low-wage employment	High-wage employment	Low-wage employment	High-wage employment	Low-wage employment
	(7)	(8)	(9)	(10)	(11)	(12)
Effect of tool treatment	23.25^{*}	1.68	-9.61	-10.81	-3.08	-29.67^{**}
	(13.33)	(12.47)	(10.25)	(12.37)	(10.19)	(11.47)
Mean value control group	870.2	519.4	515.1	605.8	349.6	704.6
No . of observations	25,274	25,274	28,109	28,109	21,938	21,938

Note: The table reports treatment differences in the number of hours worked in different types of jobs within 24 months after the start of the experiment for different subgroups of participants in the randomized controlled trial. Standard errors are reported in parentheses. ***/**/* indicates statistical significance at the 1%/5%/10%-level, respectively. In all models, we control for socio-demographic characteristics, labor market histories, place of residence (98 municipalities) and membership of unemployment funds (24 in total). Additionally, all specifications account for the effect of the message treatment (coefficients are not shown).
[a] Benefit duration of 26 weeks or less at start of intervention with low predicted LTU risk.
[b] Benefit duration of 26 weeks or less at start of intervention with high predicted LTU risk.
[c] Benefit duration of more than 52 weeks at start of intervention.
[d] Non-regular (regular) employment refers to jobs with working hours below (above) 25% of the full-time equivalent of 37 hours per week.
[e] High-/low-wage employment refers to jobs with hourly wages in the top/bottom tercile of the wage distribution (among individuals in our sample in a given calendar month).

UI benefits for more than a year, the tool treatment significantly increases marginal employment (see Panel C.1 of Table 6), which seems to come at the cost of a reduction of working hours in regular jobs (see Panel C.1) with relatively low wages (see Panel C.2).

Result 5. *We observe heterogeneous treatment effects on the nature of the resulting job matches.*

(i) For long-term unemployed job seekers, the treatment decreases regular employment while increasing employment in marginal jobs.

(ii) For short-term unemployed individuals with a low LTU risk, the intervention leads to an increase in regular employment relationships with relatively high wages.

(iii) For short-term unemployed individuals with a high LTU risk, we observe no systematic differences in job characteristics relative to the control group.

Overall, the observed pattern is in line with the notion that an improved understanding of the UI benefit rules and their personal benefit entitlements allows job seekers with good overall labor market prospects—who have a low risk that their UI benefits will expire—to be more selective when applying to vacancies or accepting job offers. This could be due to the fact that having the option of extending the benefit period by working some hours in the future reduces the pressure to accept a job with a low match quality. As discussed in Section 4, this option-value effect should be stronger, the lower is a job seeker's baseline risk of exhausting her benefits, and the more time is left until the actual benefit expiration date. This, in turn, could explain why the positive treatment effects are concentrated among job seekers with good overall employment prospects who receive the information early during their benefit spell. Moreover, a similar mechanism could be also triggered (or reinforced) by the reduction of pessimism about the remaining benefit duration among treated short-term benefit recipients, as documented in Table 3.

For long-term benefit recipients, in contrast, the treatment tends to reduce overly *optimistic* beliefs about their remaining benefit entitlements (Table 3). At the same time, this group is already closer to benefit expiration and tends to have relatively poorer employment prospects in the regular labor market, diminishing the option-value effect of being able to acquire PBD extensions in the future. In response to our treatment, this groups exhibits an increased focus on marginal employment. It seems plausible that this is an attempt to prolong their benefit eligibility by working additional hours while being on benefit claim. Yet, this shift towards marginal jobs seems to have adverse effects on the overall working hours and labor earnings in the longer run of the group of long-term benefit recipients, as documented in Table 4.

For short-term benefit recipients with a high risk of staying unemployed for an extended period, both effects might play a role. On the one hand, they also have incentives to shift their focus to marginal employment—similar to those who are already long-term unemployed—if they anticipate that they have an increased risk of reaching benefit expiration. On the other hand, the pressure to act immediately might be lower for this group, since their actual benefit expiration date is still far away (i.e., the option-value effect is stronger than for the already long-term unemployed workers). On net, the treatment seems to have only minor effects on search patterns, job characteristics, and overall labor market outcomes of the short-term unemployed individuals with a high LTU risk.

6.3 Job seekers' baseline knowledge and occupational background

Given that our intervention improves job seekers' understanding of the UI benefit system, one would expect that job seekers' response to the treatment depends on their prior knowledge of the UI benefit rules, and their prior beliefs about their personal benefit entitlements. To examine whether this is the case, we make use of information on job seekers' baseline knowledge, as measured in our online survey. The survey, however, was administered only to a relatively small fraction of the job seeker population observed in the experiment. To get a better understanding of the subgroups of job seekers with better or worse baseline knowledge—and their responses to the intervention—we therefore predict job seekers' baseline knowledge for the full experimental sample, based on the direct measures of knowledge in the survey subsample. Specifically, we make use of all survey respondents who have not (yet) been exposed to the intervention when answering the survey, i.e., we rely on all responses from the pre-intervention survey, plus the post-intervention survey responses from individuals assigned to the control group.

Similar to the prediction model for long-term unemployment risk in Section 6.1, we estimate LASSO models that account for regional characteristics, education, socio-demographic information and labor market histories and use the estimated coefficients to predict the individual baseline knowledge with respect to two dimension: the job seekers' understanding of the UI benefit rules, and their expected personal benefit entitlements. We then consider median splits for job seekers' predicted baseline knowledge of their personal benefit entitlements and the UI benefit rules, respectively. Table B.8 in the appendix summarizes the results of the prediction models. Table 7 shows the effects of the tool treatment for benefit recipients with a low (Panel A), respectively high (Panel B) LTU risk and those who are already long-term unemployed (Panel C) on total working hours and labor earnings within 24 months.

Baseline knowledge about personal entitlements: First, we consider individuals' prior (predicted) beliefs about their remaining benefit duration. The results suggest that optimism and pes-

Table 7: Treatment effects on labor market outcomes by baseline characteristics

	A. Low LTU risk[a]		B. High LTU risk[b]		C. Already LTU[c]	
	Cumulated outcomes within 24 months		Cumulated outcomes within 24 months		Cumulated outcomes within 24 months	
	Working hours	Labor earnings	Working hours	Labor earnings	Working hours	Labor earnings
	(1)	(2)	(3)	(4)	(5)	(6)
1) Effect of tool treatment by baseline knowledge about personal entitlements[d]						
Pessimistic about PBD	43.8*	13,824**	-19.1	-2,743	27.1	6,746
	(26.1)	(6,081)	(21.8)	(4,255)	(38.9)	(7,296)
Optimistic about PBD	26.5	941	14.6	1,081	-52.3**	-9,678**
	(23.7)	(5,520)	(31.3)	(6,089)	(22.9)	(4,293)
P -value: pessimistic v. optimistic	0.623	0.117	0.377	0.607	0.079	0.052
No. of observations	25,274	25,274	28,109	28,109	21,938	21,938
2) Effect of tool treatment by baseline knowledge about benefit rules[d]						
Low knowledge	53.6**	9,430*	-10.6	-2,162	-64.3	-7,113
	(22.7)	(5,296)	(20.5)	(3,989)	(48.5)	(9,088)
High knowledge	6.1	2,867	-0.2	619	-25.4	-5,110
	(27.6)	(6,424)	(36.9)	(7,190)	(21.6)	(4,053)
P -value: low v. high knowledge	0.184	0.430	0.805	0.735	0.463	0.841
No. of observations	25,274	25,274	28,109	28,109	21,938	21,938
3) Effect of tool treatment by occupational background[e]						
Infrequent use of marginal jobs	32.4*	6,834	1.0	2,238	-67.2***	-11,074**
	(19.6)	(4,567)	(25.0)	(4,878)	(24.5)	(4,597)
Frequent use of marginal jobs	42.4	6,625	-18.1	-5,481	33.3	4,935
	(39.4)	(9,161)	(25.6)	(4,992)	(33.3)	(6,231)
P -value: infrequent v. frequent use	0.820	0.984	0.595	0.269	0.015	0.039
No. of observations	25,274	25,274	28,109	28,109	21,938	21,938

Note: The table reports treatment differences in labor market outcomes (intention-to-treat effects) for different subgroups of participants in the randomized controlled trial. Standard errors are reported in parentheses. ***/**/* indicates statistical significance at the 1%/5%/10%-level, respectively. All models account for the effect of the message treatment (coefficients are not shown).

[a]Benefit duration of 26 weeks or less at start of intervention with low predicted LTU risk.

[b]Benefit duration of 26 weeks or less at start of intervention with high predicted LTU risk.

[c]Benefit duration of more than 52 weeks at start of intervention.

[d]Baseline knowledge and PBD optimism/pessimism are predicted based on the control group using a linear LASSO model. For each subgroup analysis, the sample is dived at the (overall) sample median for (predicted) baseline knowledge and PBD optimism/pessimism, respectively.

[e]Individuals are classified based on their occupational background proxied by their membership in different unemployment funds (24 total). Infrequent/frequent use of non-regular jobs refers to unemployment funds (24 in total) with a share of non-regular jobs below/above the median in the period from 2014-2016.

simism about one's personal entitlements matter for job seekers' reactions to the treatment. Among short-term benefit recipients with a low LTU risk (see Panel A of Table 7), the positive treatment effects on working hours and labor earnings, which we observed in Table 5, tend to be concentrated among job seekers who are (predicted to be) baseline pessimistic about their remaining benefit duration in absence of the treatment. Notably, the heterogeneity in treatment effects seems more pronounced when considering earnings compared to working hours. This pattern is in line with the notion that short-term unemployed job seekers with a low LTU risk become more selective regarding their job choice, when they learn that their actual benefit expiration date is further away than what they thought. Conversely, we find that the negative employment effects for long-term unemployed job seekers (see Table 4) are primarily driven by job seekers, who—in the absence of the intervention—tend to be overly optimistic about their remaining benefit duration. This suggests that especially benefit recipients, who learn that their own UI benefit situation is less comfortable than they thought, are encouraged to search for marginal employment, with adverse effects on their overall labor market performance.[14] For short-term benefit recipients with a high LTU risk, we find little systematic differences in treatment effects between baseline optimistic and pessimistic job seekers, paralleling the overall less pronounced treatment effects in this group.

Baseline knowledge about benefit extension rules: Second, we divide the sample based on individuals' predicted knowledge of the UI benefit rules in absence of the intervention, as reflected in the knowledge index described in Section 2.2. Panel A of Table 7 suggests that the baseline knowledge gaps regarding the UI benefit rules seem particularly important for the treatment response of job seekers with a low risk of becoming long-term unemployed. Specifically, the positive treatment effects on working hours and labor earnings in this group are substantially larger among individuals who are predicted to have little knowledge of the benefit rules. This is intuitive since (i) knowledge gaps tend to be most pronounced for short-term unemployed job seekers (see Table 3) and (ii) job seekers who lack knowledge of the UI benefit rules predominantly underestimate the possibilities for UI benefit extensions (see Section 2.2). It appears that the positive labor market effects tend to be driven by individuals who learn about the possibility of extending their benefit period, in line with the option-value effect discussed in Section 4. For the remaining groups of job seekers depicted in Panel B and C of Table 7, the differences in treatment effects between the subgroups with a high vs. low predicted baseline knowledge of the UI benefit rules are less clear-cut: while

[14] In line with this notion, we also find a stronger treatment effect on the likelihood to take up marginal employment among benefit recipients who are predicted to be optimistic about their PBD.

the point estimates tend to be larger for the subgroups with a low predicted knowledge of rules, the differences between the high- and low-knowledge groups are less pronounced and the effects are rather imprecisely estimated.

Occupational background: Finally, we examine whether treatment effects differ for individuals with a high vs. low baseline probability of working in marginal jobs. To do so, we rely on job seekers' membership in different unemployment funds, which are related to their occupational and educational background. As shown in Panel C of Table 7, the negative labor market effects for long-term unemployed job seekers are concentrated among worker, who exhibit a low likelihood to work in marginal jobs in the absence of the intervention. This indicates that the adverse employment effects of the treatment predominantly emerge for worker from occupations in which marginal employment is rather uncommon. This could potentially point to stronger lock-in or scarring effects of marginal work in some occupations, or conversely, stronger stepping-stone effects in occupations where marginal employment is more common.

Result 6. *We observe heterogeneous treatment effects depending on job seekers' (predicted) baseline knowledge and their occupational background:*

(i) The positive employment and earnings effects for job seekers with a low LTU risk are concentrated among workers who are (1) predicted to be pessimistic about their benefit entitlements and (2) have low knowledge of the UI benefit rules.

(ii) The negative employment and earnings effects for long-term unemployed job seekers are predominantly driven by workers who are (1) predicted to be overly optimistic in the absence of treatment and (2) by workers from occupations in which marginal employment is less common.

6.4 Discussion

The results from our main analysis suggest that job seekers with good labor-market prospects benefit from an improved understanding of the UI benefit system. At the same time, our findings show negative effects of our intervention on the labor market outcomes of long-term unemployed individuals. In what follows, we discuss two additional aspects that are important for the interpretation of our results.

The role of treatment spillovers: First, one may expect our intervention to have, not only, a direct effect on treated job seekers, but also an indirect effect on the behavior or labor market outcomes of non-treated job seekers. Such treatment spillovers (see, e.g., Crépon et al. 2013, Ferracci et al. 2014, Gautier et al. 2018, Benghalem et al. 2022, Altmann et al. 2022) could arise for various reasons. For instance, treated individuals might inform their untreated peers about their newly acquired knowledge of the UI benefit system. Besides such information spillovers, the intervention could provoke displacement effects among untreated job seekers, as a result of crowding out between treated and untreated individuals who compete for the same vacancies. While our experiment is not designed to precisely pin down the existence and nature of treatment spillovers in our setting, we can shed some light on the relevance of spillover effects, by exploiting natural variation in the share of treated individuals across different local labor markets. Our analysis—presented in more detail in Appendix B.5—yields little evidence for systematic positive or negative spillovers. While our analysis does, ultimately, not allow us to rule out all possible forms of treatment spillovers (e.g., the simultaneous presence of displacement effects and positive informational spillovers, which tend to offset each other), it appears unlikely that treatment spillovers have a large net effect on our results. This is consistent with recent evidence by Benghalem et al. (2022), who find no evidence for treatment spillovers from an information intervention similar to ours, based on a clustered randomized design.

Implications for individuals' financial well-being: Second, our main analysis focused on individuals' labor market outcomes in terms of employment and labor earnings after the intervention. However, it is conceivable that, for instance, the negative earnings effects for long-term unemployed individuals documented in Table 4 are partially or fully offset by the progressive tax system or higher benefit payments. To get a broader perspective on overall financial well-being, we consider three additional outcomes: First, we analyze treatment effects in the likelihood of receiving social assistance. As social assistance can only be claimed by unemployed individuals who are no longer eligible for UI benefits, this is a direct measure of job seekers' likelihood of UI benefit expiration (accounting for potential benefit extensions). Second, we examine effects on the sum of job seekers' cumulated labor earnings and UI benefit payments. As our data contains detailed information on benefit payments only for the first 12 months after the beginning of the intervention, we focus our analysis of these two outcomes on a one-year observation window. Finally, we consider treatment differences in overall disposable income accounting for taxes and transfers in the calendar years 2018 and 2019.

Detailed estimates for treatment differences in the three additional outcomes are presented in Table B.7 in the appendix. Altogether, the results support the notion that an improved understanding of the UI benefit system can have a negative impact on the financial well-being of long-term unemployed individuals. For instance, 12 months after the beginning of the intervention, treated individuals from this group have a one percentage point higher likelihood of receiving social assistance—implying a 20% higher risk of entering the social assistance relative to the corresponding individuals in the control group ($p < 0.001$). This finding suggests that treated long-term unemployed job seekers exhibit an increased likelihood of UI benefit exhaustion. At the same time, we find no evidence that our intervention has positive effects on the overall income (accounting for benefit payments) of those who are already unemployed for an extended period. Rather, shifting their focus towards marginal jobs seems to be detrimental to their labor market integration. This suggests that, in our setting, lock-in effects are severe relative to the stepping-stone aspect of marginal employment (see also Kyyrä et al. 2013, who document similar lock-in effects in the Danish labor market, and Benghalem et al. 2022, who find lock-in effects associated with a French part-time UI benefit program). For short-term unemployed job seekers (especially those with a low LTU risk), we observe some compression of the estimated treatment effects when accounting for benefit payments and taxes (cp. Panel A and B of Table B.7). The one-year observation window is, however, too short to examine whether the countervailing effects fully offset the positive earnings effects observed in Table 5 in the longer run.

7 Conclusion

In this paper, we studied the interplay between complex UI benefit rules, job seekers' understanding of these rules, and the resulting labor market outcomes. We rely on data from a randomized controlled trial with the universe of Danish UI benefit recipients, a large-scale online survey, and detailed administrative data on individuals' labor market outcomes. This allows us to demonstrate that job seekers exhibit substantial knowledge gaps regarding important aspects of the UI benefit system, and that the provision of personalized information through an online information tool substantially increases job seekers' understanding of the prevailing rules and their personal entitlements.

We found substantial heterogeneity with respect to the labor market effects of our intervention, depending on the job seekers' personal benefit situation and their estimated baseline knowledge of the UI benefit system. Among job seekers who have already been unemployed for more than one year, our intervention reduces optimism about their remaining benefit entitlements and increases individuals' understanding of existing possibilities to extend UI benefits. Learning about the option of earning additional entitlements while being on benefit claim encourages this group of job seekers to shift their work activities towards marginal employment—in line with the notion that these job seekers exhibit a relatively high risk of benefit expiration and relatively poor prospects of finding regular full-time jobs. Our results, however, also show that the stronger focus on marginal jobs does not generate additional employment for this group. Rather, lock-in effects seem to reduce their overall labor market performance in the longer run compared to the control group.

Conversely, job seekers with particularly good labor market prospects—i.e., individuals with low LTU risk who are treated early during their benefit spell—learn that the UI benefit system and their personal benefit entitlements tend to be more generous than they thought prior to the intervention. In line with the notion that the possibilities for UI benefit extensions entail a positive option value, these job seekers seem to become more selective regarding the jobs they target, with positive effects on their overall working hours and earnings.

The fact that higher knowledge translates into differential labor market effects for different "types" of job seekers highlights that policies, which aim to relax information constraints in tax and transfer systems, should take into account the built-in rules and incentives that they inform about. Digital tools seem to reduce information frictions effectively and have the potential to improve the welfare of some job seekers. At the same time, however, others may experience adverse effects when the underlying incentives promote short-sighted behavior, such as working in a marginal job in an environment where lock-in effects seem to be strong. One could speculate that an improved targeting of information policies towards the job seeker's individual needs may help to avoid such adverse effects. For instance, the usage of digital tools could be complemented by caseworker counseling to improve efficiency.

For evaluating the overall usefulness of UI benefit systems with part-time insurance or flexible extensions, it should also be noted that our intervention was conducted in a period of low unemployment. Similar policies might be more effective in times of economic downturns, when there is a higher need for and potentially higher benefits from non-standard employment.

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Appendix

A Details on Study Design

A.1 Implementation of the online survey

The online survey was implemented in two waves, involving a 30% subsample of our overall study population. A first survey wave was administered in the week immediately before the experiment started (week $t = -1$ in Figure 1). 7.5% of the total study population were invited to this pre-intervention survey (n=7,430; three equally-sized subsamples from each of the treatment arms). The main, post-intervention survey was administered in $t = 5$, roughly one month after treated individuals received the main treatment message. For the post-intervention survey, we invited 22.5% of the overall population from the RCT (n=22,352; equally-sized subsamples from the different treatments). Roughly 20% of the experimental population had already left unemployment at the time of the post-intervention survey. The invitation to the survey was independent of a participant's employment status at $t = 5$, but some survey questions, e.g., concerning individuals' personal benefit entitlements, were only included for respondents who were still unemployed by the time of the survey.

The online survey serves two purposes. First, it allows us to measure job seekers' knowledge about the UI benefit system and their personal benefit entitlements. Second, we also elicited additional information regarding potential mechanisms through which the intervention might affect the labor market outcomes of treated individuals. Specifically, the survey included measures of individuals' job search strategies, their overall motivation, and their subjective perceptions of being monitored and pressured by the public employment service. Treatment effects on these additional outcome variables are documented in Appendix Tables B.6 and B.5, respectively.

Individuals are incentivized to fill in the survey as they may participate in a lottery for 200 shopping vouchers of DKK 500 (approx. €65) each. Survey participants are invited by the public employment service on behalf of the University of Copenhagen, using their private e-mail addresses.[15] Using a different communication channel and a different sender for the online survey and the information treatment reduces the risk that respondents connect the online survey to the treatment messages.

The overall response rate in the online survey is about 14%, with a slightly lower value in the post-intervention survey (12.5%) than in the pre-intervention survey (15.5%). The difference primarily

[15] Only participants who agreed to be contacted by the public employment service via e-mail are invited to the survey. This applies for about 50% of the overall population.

reflects a lower likelihood to respond among individuals who have already left unemployment.

Table A.1 compares sociodemographic characteristics of participants in the pre- and post-intervention survey and the overall study population. Compared to the average UI benefit recipient, survey participants tend to be somewhat older and better educated, they are more likely to be female and married, less likely to be migrants, and they have been unemployed for a somewhat longer time period. While this indicates that survey respondents are not necessarily representative of the full experimental population, it is important to note that the treatment does neither affect the likelihood of being already employed by the time of the post-intervention survey, nor the likelihood of completing the survey (see Table A.2). Moreover, there are only minor differences regarding the composition of survey respondents across treatments arms (see Table A.3). Altogether, this suggests that the survey data are suitable to identify the causal effects of the intervention on job seekers' knowledge.

A.2 Knowledge questions

(Q1) *Existence of extension:* Suppose you will work for two full weeks while being on unemployment benefits. How will this affect your situation at the end of the two-year unemployment benefit period? Can you use the two weeks to extend your benefit period?

- Yes
- No

(Q2) *Extension gained:* For how long can you extend your unemployment benefit period if you have been working for two weeks? *Please indicate the number of weeks.*

- Length of extension in weeks:

(Q3) *Required period:* The unemployment benefit period is two years with the possibility of an extension. How many hours do you have to work to extend the benefit period by 12 weeks? *(This could be by working in a small job during the benefit period.)*

- 111 hours (3 weeks)
- 222 hours (6 weeks)
- 444 hours (12 weeks)
- 666 hours (18 weeks)
- 888 hours (24 weeks)

(Q4) *Maximum extension:* In general, by how much can the two-year unemployment benefit period be extended by working while you receive unemployment benefits?

- 481 hours (3 months)
- 962 hours (6 months)

- 1443 hours (9 months)
- 1924 hours (12 months)
- 2405 hours (15 months)
- 2886 hours (18 months)

(Q5) *Income effect:* Suppose that you have an offer of working for one week (equivalent to 37 hours). The salary before tax is 5.500 kr and you receive unemployment benefits for the rest of the month. How will it affect your total monthly income (working salary and unemployment benefits) in comparison to a month where you receive unemployment benefits only, if you accept the job?

- My income decreases
- My income is the same
- My income increases

(Q6) *Benefit sanction:* Suppose you have received unemployment benefits for a period of 4 months and you are not working during the period, how will it affect your unemployment benefit in the fourth month compared to the first 3 months of the period? My benefits in the 4th month are:

- Lower
- Unchanged
- Higher

(Q7) *Expected benefit duration:* When will your unemployment benefits expire? Enter the date your unemployment benefit period ends if you include current extensions. Assume that you do not take any further work.

- Day
- Month
- Year

Table A.1: Comparison of full sample and survey respondents

	Full sample	Respondents pre-intervention survey	Respondents post-intervention survey
No. of observations	98,641	1,154	2,805
Educational level			
None (or missing)	0.083	0.026	0.030
Less than high school	0.178	0.107	0.099
High school	0.400	0.395	0.353
Bachelor degree (or equiv.)	0.241	0.308	0.338
Master degree (or equiv.)	0.098	0.163	0.180
Male	0.479	0.432	0.435
Age			
18-25 years	0.116	0.049	0.040
26-35 years	0.332	0.232	0.230
36-45 years	0.193	0.159	0.175
46-55 years	0.196	0.261	0.272
56-65 years	0.163	0.299	0.282
Household size			
One person	0.194	0.221	0.229
Two persons	0.344	0.399	0.398
Three persons	0.203	0.189	0.185
Four or more persons	0.260	0.192	0.188
Married	0.341	0.406	0.411
Children			
One child	0.164	0.144	0.146
Two or more children	0.172	0.130	0.132
Migration status			
1^{st} generation	0.194	0.088	0.099
2^{nd} generation	0.033	0.011	0.010
Weeks of UI benefits (current spell)	32.15	36.63	34.57
Weeks of UI benefits			
in last year	24.25	27.10	26.47
in last 5 years	50.58	54.75	54.81
Months employed			
in last year	6.025	5.255	5.688
in last 5 years	38.194	38.95	39.49
Average monthly earnings			
in last year	17,818	21,299	22,445
in last 5 years	18,356	23,775	23,413
Average weekly working hours			
in last year	19.13	18.94	20.24
in last 5 years	22.24	24.12	24.46

Note: Depicted are summary statistics for the full experimental population and the samples of survey respondents who completed the pre-intervention survey (middle column) and post-intervention survey (rightmost column), respectively. Percentage shares unless indicated otherwise.

Table A.2: Treatment differences in participation in post-intervention survey

Dependent variable	Invited to survey (1)	Survey for non-UI-recipients$^{(a)}$ (2)	Completed survey (3)
Tool treatment	0.000	0.000	-0.009
	(0.005)	(0.007)	(0.006)
Message treatment	-0.000	-0.004	-0.009
	(0.005)	(0.005)	(0.008)
No. of observations	98,641	22,327	22,327
Mean value control group	0.226	0.264	0.133
P-value tool v. message	0.893	0.620	0.971

Note: The table reports treatment differences regarding the participation in the post-intervention survey (intention-to-treat effects) among participants in the randomized controlled trial. Standard errors are reported in parentheses. ***/**/* indicates statistical significance at the 1%/5%/10%-level, respectively.

$^{(a)}$Refers to an indicator that the individual was invited to an adjusted version of the online survey, which excludes questions that explicitly address UI benefit recipients. All individuals who left unemployment by the time of the main survey in week t_5 were invited to the adjusted survey.

Table A.3: Summary statistics for participants in main survey

	Treatment status			
	Control (C)	Message (M)	Tool (T)	$P-$value
No. of observations	986	918	901	
Educational level				
None (or missing)	0.026	0.029	0.034	0.598
Less than high school	0.093	0.105	0.101	0.698
High school	0.345	0.365	0.349	0.628
Bachelor degree (or equiv.)	0.355	0.329	0.330	0.394
Master degree (or equiv.)	0.181	0.172	0.186	0.724
Male	0.438	0.408	0.457	0.106
Age				
18-25 years	0.040	0.037	0.044	0.726
26-35 years	0.234	0.227	0.230	0.923
36-45 years	0.174	0.174	0.176	0.991
46-55 years	0.259	0.281	0.279	0.476
56-65 years	0.293	0.281	0.271	0.560
Household size				
One person	0.220	0.220	0.246	0.311
Two persons	0.398	0.401	0.396	0.978
Three persons	0.205	0.178	0.171	0.137
Four or more persons	0.177	0.202	0.186	0.406
Married	0.421	0.412	0.401	0.672
Children				
One child	0.166	0.141	0.130	0.074
Two or more children	0.123	0.142	0.132	0.477
Migration status				
1^{st} generation	0.095	0.100	0.102	0.877
2^{nd} generation	0.013	0.011	0.007	0.318
Weeks of UI benefits (current spell)	33.227	36.960	33.615	0.008
Weeks of UI benefits				
in last year	26.12	27.91	25.37	0.002
in last 5 years	53.28	56.35	54.92	0.223
Months employed				
in last year	5.948	5.386	5.710	0.016
in last 5 years	40.072	39.210	39.133	0.378
Average monthly earnings				
in last year	23,016	21,285	23,003	0.195
in last 5 years	23,969	22,752	23,478	0.263
Average weekly working hours				
in last year	20.68	19.57	20.50	0.199
in last 5 years	24.64	24.31	24.46	0.810

Note: Percentage shares unless indicated otherwise. $P-$values are based on F-tests for joint significance of treatment coefficients in separate regressions of each of the characteristics on dummies for the different treatment conditions.

A.3 The online information tool

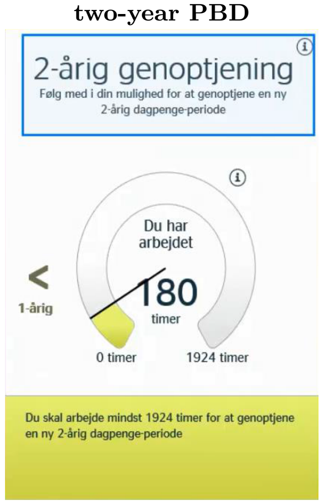
Figure A.1 shows the different elements of the online tool that provides personalized information about job seekers' UI benefit situation and the prevailing UI benefit rules. Panel (a) displays the possible extension of the PBD based on the job seeker's accumulated working hours. Panel (b) shows the consumption of benefit hours within the current benefit period and informs the job seeker about her current benefit expiration date. Panel (c) shows the working hours saved for gaining a new 2-year benefit period. Panel (d) shows how many working hours have been saved within the current 4-months window to reach the goal of 148 hours and avoid a benefit sanction.

Figure A.1: The Online Information Tool

(a) Extension of PBD

(b) Usage of UI benefits

(c) Renewing two-year PBD



(d) Avoidance of benefit sanction

A.4 Text of treatment messages

Main message to treatment group:

Dear X,

Your unemployment benefits will expire at some point, but did you know that you can influence the duration of your unemployment benefit period yourself? Every hour you work translates into up to two extra hours of unemployment benefits, which you can use to extend your unemployment benefit period. At the same time, every hour you work helps you avoid a qualification day, at which you receive no unemployment benefits.

A new tool on *jobnet.dk* makes it easy for you to keep an eye on your accumulated working hours and get an overview of the most relevant benefit rules. The dynamic and personalized tool is called "Dagpengetæller" ["benefit meter"]. It is continuously updated with your unemployment benefit hours and your working hours; and you can calculate how extra working hours will affect your unemployment benefit period.

Your benefit meter gives you an overview of:

1. The hours you have worked

2. Your consumption of unemployment benefits and your remaining benefit hours

3. Rules that are important for you. Check the information boxes by clicking on the "i"-button

Learn more about your unemployment benefits now. [LINK]

Use your benefit meter regularly to know your possibilities and make the most out of them. You may, for instance, check your benefit meter when you log on to jobnet.dk to check your suggested job ads or register your job applications.

Did you know that there are about 20,000 vacancies available at jobnet.dk right now? There are more possibilities than you may think.

Good luck with your job search.

Reminder message to treatment group:

Dear X,

Your unemployment benefits will expire at some point in time, but did you know that you can influence the duration of your unemployment benefit period yourself?

A new tool on *jobnet.dk* makes it easy for you to keep an eye on your accumulated working hours and get an overview of the most relevant benefit rules.

Learn more about your unemployment benefits now. [LINK]

Use your benefit meter regularly to know your possibilities and make the most out of them.

Did you know that there are about 20,000 vacancies available at jobnet.dk right now? There are more possibilities than you may think.

Good luck with your job search.

Message to message group:

Dear X,

Use *jobnet.dk* regularly to know your possibilities and make the most out of them.

Did you know that there are about 20,000 vacancies available at jobnet.dk right now? There are more possibilities than you may think.

Good luck with your job search.

B Additional Tables and Figures

B.1 Additional summary statistics

In the following, we provide additional summary statistics about treatment take-up (see Table B.1) and the distribution of job seekers' knowledge about the UI benefit system in absence of our intervention, focusing on knowledge of the possibilities to extend UI benefits (Figure B.1) and individuals' knowledge of their remaining benefit entitlements (Figure B.2).

Treatment take-up

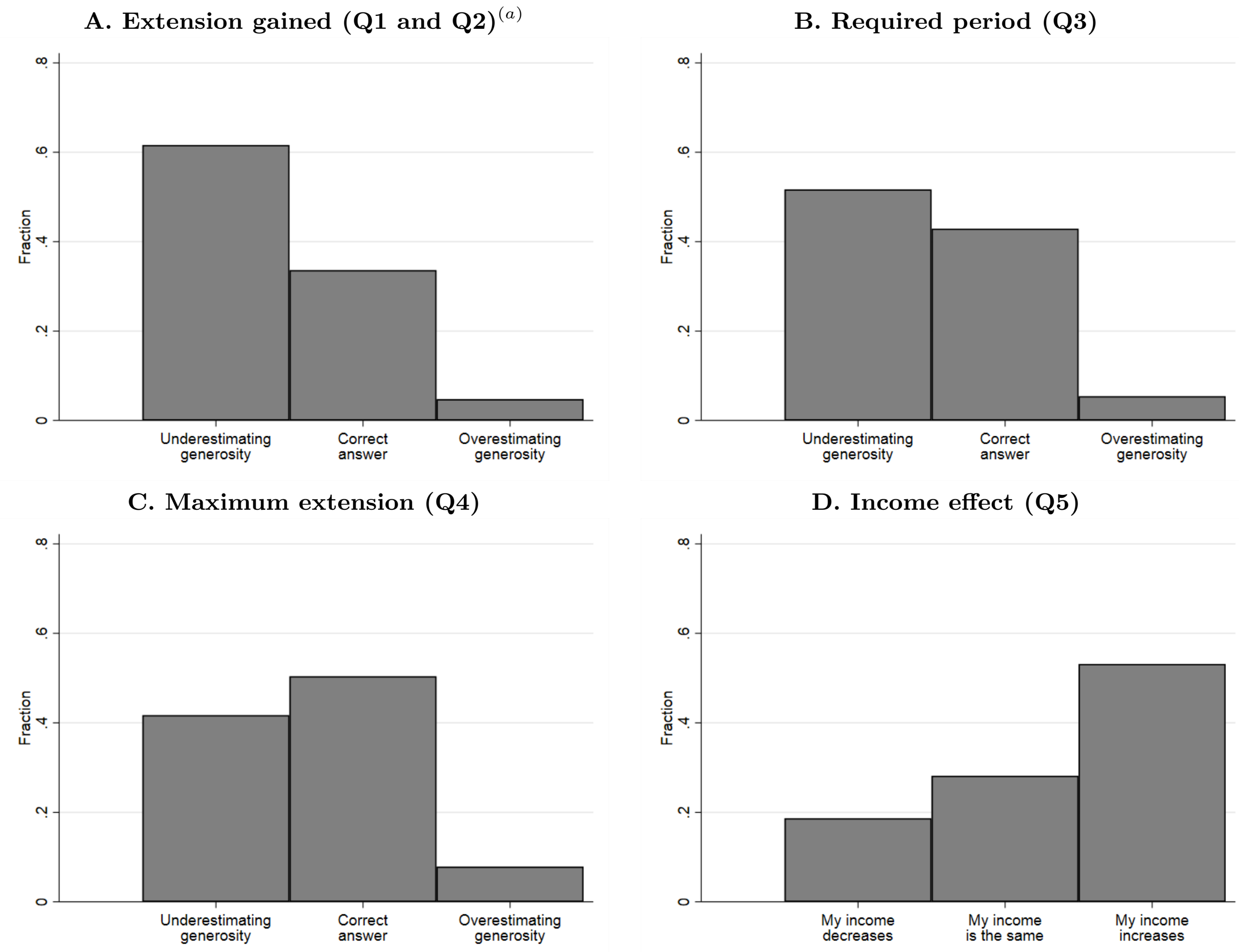
Table B.1: Treatment take-up: clicking behavior by treatment status

	Main message	Reminder 1	Reminder 2	Reminder 3	Reminder 4
Date sent	March 05, 2018	April 03, 2018	April 30, 2018	May 28, 2018	June 25, 2018
Tool treatment					
Messages sent$^{(a)}$	32,857	30,460	26,905	22,839	19,968
Messages opened$^{(b)}$					
total	30,717	26,806	22,904	19,366	16,777
share of sent	0.935	0.880	0.851	0.848	0.840
Click on link$^{(c)}$					
total	6,539	6,311	4,747	3,949	3,711
share of sent	0.199	0.207	0.176	0.173	0.186
share of opened	0.213	0.235	0.207	0.204	0.221
Message treatment					
Messages sent$^{(a)}$	32,874	30,552	26,927	22,801	19,941
Messages opened$^{(b)}$					
total	30,946	27,420	23,761	20,082	17,663
share of sent	0.941	0.897	0.879	0.881	0.886

Note: The table depicts summary statistics on the take-up of the intervention, separately for the tool and message treatment.
$^{(a)}$Refers to the total number of individuals receiving the corresponding message to their inbox on *jobnet.dk*. Reminders are only sent to individuals who have been registered as UI benefit recipients within the last four weeks before the date of the reminder.
$^{(b)}$Refers to all individuals opening the corresponding message.
$^{(c)}$Refers to all individuals clicking on the link to the online information tool.

Distribution of knowledge about the UI benefit system

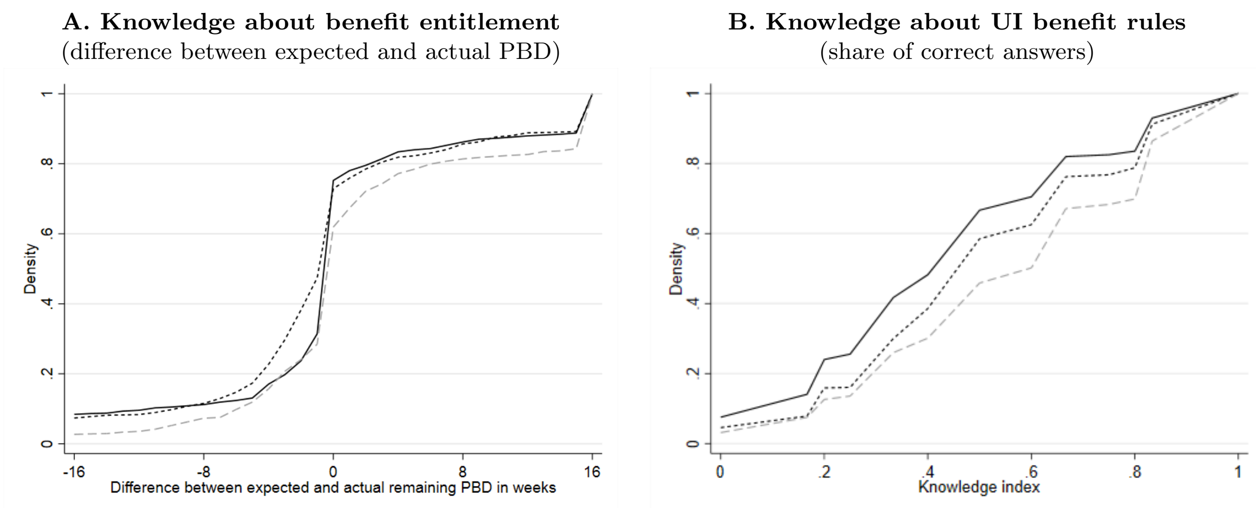
Figure B.1: Distribution of survey answers



The figure shows the distribution of answers to the survey questions regarding individuals' knowledge about the UI benefit rules. If there are more than three response options (Panel A - C), incorrect answers are classified based on whether the respondent perceives the rules as less or more generous than they actually are.

[a] Depicted is the distribution of a variable that takes into account the respondents answers to question Q1 (i.e. the possibility of gaining a benefit extension) and Q2 (i.e. the length of the possible extension).

Figure B.2: Distribution of knowledge by elapsed benefit duration



P−value[a] (Panel A)	
short v. medium	< 0.001
short v. long	< 0.001
medium v. long	< 0.001

P−value[a] (Panel B)	
short v. medium	< 0.001
short v. long	< 0.001
medium v. long	< 0.001

Elapsed benefit duration: ≤26 weeks (short) 27-52 weeks (medium) >52 weeks (long)

The figure shows the distributions of knowledge about individuals remaining benefit duration (Panel A) and the UI benefit rules (Panel B) among participants in the pre-intervention survey separated by their elapsed benefit duration at the time of the survey. P-values are based on two-sample Kolmogorov-Smirnov tests of the equality of distributions.

B.2 Prediction LTU risk

Table B.2: Predicting the risk of long-term unemployment

Dependent variable:	**Realized unemployment duration > 52 weeks**			
	A. Logit model Sample 2017		B. LASSO-Logit model Sample 2017	B. LASSO-Logit model Sample 2016
	Coef.	SE	Coef.	Coef.
Educational level (ref. none)				
Less than high school	-0.099*	(0.055)	-0.084	-0.0956
High school	-0.149***	(0.053)	-0.133	-0.206
BA degree (or equiv.)	0.058	(0.052)	0.074	0.0696
MA degree (or equiv.)	-0.018	(0.057)		
Male	-0.076***	(0.019)	-0.078	
Age (ref. 18-25 years)				
26 - 35 years	0.117***	(0.032)	0.093	0.0330
36 - 45 years	0.222***	(0.038)	0.195	0.299
46 - 55 years	0.333***	(0.039)	0.302	0.445
56 - 65 years	0.595***	(0.041)	0.559	0.789
Household size (ref. one person)				
Two persons	-0.129***	(0.0253)	-0.109	-0.124
Three persons	-0.307***	(0.056)	-0.226	-0.274
Four or more persons	-0.452***	(0.091)	-0.310	-0.137
Married	0.064***	(0.024)	0.049	
Children (ref. none)				
One child	0.168***	(0.046)	0.109	0.0241
Two or more children	0.292***	(0.083)	0.166	
Migration status				
1^{st} generation	0.528***	(0.024)	0.528	0.541
2^{nd} generation	0.326***	(0.045)	0.319	0.385
Average monthly earnings (in 10,000DKK)				
in last year	0.033*	(0.017)	0.016	0.074
in last 5 years	-0.118***	(0.021)	-0.094	-0.160
Average weekly working hours (×10)				
in last year	-0.044***	(0.015)	-0.029	-0.022
in last 5 years	0.018	(0.019)	0.012	
Weeks of UI benefits				
in last year	-0.013***	(0.001)	-0.013	-0.037
in last 5 years	-0.005***	(0.001)	-0.005	-0.007
Municipality FE	Yes		Yes	Yes
Unemployment fund FE	Yes		Yes	Yes
Pseudo-R^2	0.061			
No. of observations	69,230		69,230	42,251
Mean value dependent variable	0.300		0.300	0.227

Note: The table reports coefficients of a logit (Panel A) and a LASSO-logit (Panel B) model predicting the risk of long-term unemployment for a sample of entries into unemployment in 2017 (specification 1 and 2), respectively 2016 (specification 3). The dependent variable is a dummy indicating a realized unemployment duration of 52 weeks or more. Standard errors are reported in parentheses. ***/**/* indicates statistical significance at the 1%/5%/10%-level, respectively.

B.3 Additional estimates of treatment effects

Table B.3: Treatment effects by risk of long-term unemployment based on alternative prediction model[(a)]

	Sample: UI benefit duration $\leq$ 26 weeks	
	Total working hours within 24 months	Total labor earnings (in DKK) within 24 months
	(1)	(2)
Effect of tool treatment by risk og long-term unemployment (LTU)		
Low risk of LTU	45.94**	9,726**
	(22.52)	(4,790)
High risk of LTU	-4.22	-543
	(15.29)	(3,252)
P-value: low v. high risk of LTU	0.065	0.076
No. of observations	53,383	53,383
Mean value control group		
Low risk of LTU	1,851	363,185
High risk of LTU	1,861	340,621

Note: The table reports the effects of the tool treatment on labor market outcomes (intention-to-treat effects) among participants in the randomized controlled trial. Standard errors are reported in parentheses. ***/**/* indicates statistical significance at the 1%/5%/10%-level, respectively. All specifications account for the effect of the message treatment (coefficients are not shown).
[(a)]The risk of long-term unemployment (LTU) is estimated based on a sample of entries into unemployment in 2016 (rather than 2017) using a LASSO logit approach.

Table B.4: Treatment effects on knowledge by risk of long-term unemployment

	A. UI benefit rules	**B. Personal benefit entitlements**		
	Knowledge index	Difference between exp. and actual PBD$^{(b)}$		
	Share of correct answers (0=low; 1=high)	Absolute difference (in weeks)	Overestimation of PBD (in weeks)	Underestimation of PBD (in weeks)
	(1)	(2)	(3)	(4)
Effect of tool treatment by risk of long-term unemployment (LTU)$^{(b)}$				
Low LTU risk	0.045**	-1.64	-1.14	-0.50
	(0.021)	(1.61)	(1.30)	(1.13)
High LTU risk	0.048**	-2.58*	-1.28	-1.31
	(0.021)	(1.48)	(1.20)	(1.04)
P-value: low v. high LTU risk	0.929	0.666	0.938	0.601
No. of observations	1,955	1,430	1,430	1,430
Mean values control group				
Low LTU risk	0.519	9.141	5.564	3.577
High LTU risk	0.486	9.234	5.625	3.609

Note: The table reports treatment differences in knowledge (intention-to-treat effects) among participants in the main survey. Standard errors are reported in parentheses. ***/**/* indicates statistical significance at the 1%/5%/10%-level, respectively. In all models, we control for socio-demographic characteristics, labor market histories, place of residence (98 municipalities) and membership of unemployment funds (24 in total).

$^{(a)}$Refers to the difference between the subjectively expected remaining benefit duration elicited through the online survey and the actual remaining benefit duration observed in the administrative records in weeks. In columns (3) and (4), we consider the absolute difference. In column (5)-(8), we decompose the absolute difference into instances that over-, respectively underestimate their remaining benefit duration. Therefore, we set the corresponding outcome variable to zero if the individual does not overestimate (Column (5) and (6)), respectively if the individual does not underestimate (Column (7) and (8) the PBD).

$^{(b)}$The risk of long-term unemployment (LTU) is estimated based on a sample of entries into unemployment in 2017 using a LASSO logit approach. A robustness check relying on entries in 2016 to predict the risk of long-term unemployment is presented in Table B.3 in Appendix B.3.

Table B.5: Treatment effects on perceived pressure and general motivation

	A. Low LTU risk[a]			B. High LTU risk[b]			C. Already LTU[c]		
	A.1 Perceived pressure and monitoring			**B.1Perceived pressure and monitoring**			**C.1 Perceived pressure and monitoring**		
	Feels pressure to search for job	Feels pressure to accept job	Feels monitored by authorities	Feels pressure to search for job	Feels pressure to accept job	Feels monitored by authorities	Feels pressure to search for job	Feels pressure to accept job	Feels monitored by authorities
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Effect of tool treatment	0.188	0.130	0.127	0.304	0.400	0.195	0.232	0.375	-0.304
	(0.224)	(0.254)	(0.230)	(0.215)	(0.249)	(0.222)	(0.225)	(0.275)	(0.242)
Mean control group	7.971	6.288	7.609	7.634	6.145	7.192	7.862	6.248	7.463
No . of observations	944	944	944	903	903	903	935	935	935
	A.2 General motivation			**B.2 General motivation**			**C.2 General motivation**		
	Positive affect (PANAS)	Negative affect (PANAS)		Positive affect (PANAS)	Negative affect (PANAS)		Positive affect (PANAS)	Negative affect (PANAS)	
	(10)	(11)		(12)	(13)		(14)	(15)	
Effect of tool treatment	-0.0186	0.0468		-0.0543	0.0978		0.00787	0.110	
	(0.061)	(0.070)		(0.059)	(0.069)		(0.065)	(0.082)	
Mean control group	3.091	2.392		3.038	2.477		2.960	2.718	
No. of observations	944	944		903	903		935	935	

Note: The table reports treatment differences in search outcomes (intention-to-treat effects) based on the online survey conducted about five weeks after the start of the intervention for different subgroups of participants in the randomized controlled trial. Standard errors are reported in parentheses. ***/**/* indicates statistical significance at the 1%/5%/10%-level, respectively. All models account for the effect of the message treatment (coefficients are not shown).

[a] Benefit duration of 26 weeks or less at start of intervention with low predicted LTU risk.

[b] Benefit duration of 26 weeks or less at start of intervention with high predicted LTU risk.

[c] Benefit duration of more than 52 weeks at start of intervention.

Table B.6: Treatment effects on job search behavior

	A. Low LTU risk[a]			B. High LTU risk[b]			C. Already LTU[c]		
	A.1 Registered applications			**B.1 Registered applications**			**C.1 Registered applications**		
	# registered applications per month	Average search radius in km		# registered applications per month	Average search radius in km		# registered applications per month	Average search radius in km	
	(1)	(2)		(3)	(4)		(5)	(6)	
Effect of tool treatment	0.288	-0.739**		0.167	-0.333		-0.088	-0.016	
	(0.393)	(0.297)		(0.427)	(0.356)		(0.375)	(0.359)	
Mean control group	14.35	26.87		21.43	29.04		20.06	28.23	
No . of observations	25,274	25,274		28,109	28,109		21,938	21,938	
	A.2 Applications reported in survey			**B.2 Applications reported in survey**			**C.2 Applications reported in survey**		
	# total applications	# applications part-time	# applications temporary	# total applications	# applications part-time	# applications temporary	# total applications	# applications part-time	# applications temporary
	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Effect of tool treatment	-0.033	-0.299	-0.054	1.938	2.855	0.418*	1.572	0.867	1.029
	(0.507)	(0.239)	(0.173)	(1.626)	(2.110)	(0.224)	(1.269)	(0.631)	(0.636)
Mean control group	10.01	1.294	1.184	10.72	1.56	1.64	11.91	1.77	1.96
No. of observations	1,132	1,132	1,132	1,105	1,105	1,105	1,165	1,165	1,165

Note: The table reports treatment differences in search outcomes (intention-to-treat effects) based on the online survey conducted about five weeks after the start of the intervention for different subgroups of participants in the randomized controlled trial. Standard errors are reported in parentheses. ***/**/* indicates statistical significance at the 1%/5%/10%-level, respectively. All models account for the effect of the message treatment (coefficients are not shown).

[a] Benefit duration of 26 weeks or less at start of intervention with low predicted LTU risk.

[b] Benefit duration of 26 weeks or less at start of intervention with high predicted LTU risk.

[c] Benefit duration of more than 52 weeks at start of intervention.

Table B.7: Treatment effects on benefit payments and disposable income

	A. Low LTU risk		
	Receiving social assistance after 12 months$^{(a)}$	UI benefits + labor earnings within 12 months$^{(b)}$	Disposable income 2018/2019$^{(c)}$
	(1)	(2)	(3)
Effect of tool treatment	-0.001	1,529	1,451
	(0.001)	(1,924)	(1,759)
No. of observations	25,274	25,274	25,274
Mean value control group	0.003	210,485	458,897
	B. High LTU risk		
	Receiving social assistance after 12 months$^{(a)}$	UI benefits + labor earnings within 12 months$^{(b)}$	Disposable income 2018/2019$^{(c)}$
	(1)	(2)	(3)
Effect of tool treatment	-0.001	-795	-877
	(0.001)	(1,600)	(1,628)
No. of observations	28,109	28,109	28,109
Mean value control group	0.003	148,135	378,772
	C. Already LTU		
	Receiving social assistance after 12 months$^{(a)}$	UI benefits + labor earnings within 12 months$^{(b)}$	Disposable income 2018/2019$^{(c)}$
	(4)	(5)	(6)
Effect of tool treatment	0.010***	-3,082*	-1,412
	(0.004)	(1,722)	(1,956)
No. of observations	21,938	21,938	21,938
Mean value control group	0.046	124,101	360,074

Note: The table reports treatment differences in outcomes variables related to benefit payments and total income (intention-to-treat effects) among participants in the randomized controlled trial. Standard errors are reported in parentheses. ***/**/* indicates statistical significance at the 1%/5%/10%-level, respectively. All models account for the effect of the message treatment (coefficients are not shown).

$^{(a)}$Refers to a dummy variable indicating whether the individual receives social assistance (the second-tier unemployment assistance system that provides means-tested benefits) 12 months after the start of the intervention.

$^{(b)}$Refers to the sum of UI benefit payments and labor earnings cumulated within 12 months after the start of the intervention.

$^{(c)}$Refers to the individuals' total disposable income (accounting for taxes and transfer payments) in the calendar years 2018 and 2019.

B.4 Prediction baseline knowledge and beliefs

Table B.8: Predicting baseline knowledge and beliefs based on online survey

Dependent variable:	Difference between expected and actual PBD (in weeks)$^{(a)}$	Knowledge index (0=low; 1=high)
	(1)	(2)
Educational level (ref. none)		
Less than high school	0.0459	-0.0242
High school		
BA degree (or equiv.)		
MA degree (or equiv.)		0.0087
Male		0.0030
Age (ref. 18-25 years)		
26 - 35 years		
36 - 45 years		0.0136
46 - 55 years		0.0189
56 - 65 years		0.0322
Household size (ref. one person)		
Two persons	-0.3892	
Three persons	0.1181	-0.0133
Four or more persons		-0.0064
Married		0.0130
Children (ref. none)		
One child	0.0315	-0.0147
Two or more children	-0.5041	-0.0003
Migration status		
1^{st} generation	-1.560	-0.0322
2^{nd} generation		-0.0990
No. of registered job applications in last 4 weeks	-0.0566	0.0002
Average monthly earnings (in 10,000DKK)		
in last year		0.0018
in last 5 years		0.0006
Average weekly working hours (×10)		
in last year		
in last 5 years	0.0001	
Weeks of UI benefits		
in last year	-0.1311	0.0003
in last 5 years	0.0142	0.0004
Elapsed UI benefit duration in weeks	0.1010	0.0004
No. of observations	1,846	2,140
R^2	0.083	0.093
Municipality FE	Yes	Yes
UI fund FE	Yes	Yes
Mean value dependent variable	2.272	0.617

Note: The table reports coefficients of a linear LASSO model predicting knowledge about (i) personal benefit entitlements (difference between the subjectively expected and actual remaining benefit duration) and (ii) UI benefit rules (perceived returns to work while being on UI claim) based on observations from the pre-survey and the control group of the main survey.
$^{(a)}$The dependent variable refers to the difference between the expected and actual benefit expiration date measured in weeks (i.e. the variable takes positive/negative values for optimistic/pessimistic individuals).

B.5 The role of treatment spillovers

The large-scale nature of our experiment potentially raises concerns about the presence of spillovers from treated individuals on other, untreated job seekers. For instance, there could be information spillovers such that treated individuals inform their untreated peers about their newly acquired knowledge of the UI benefit system (Duflo and Saez, 2003). Spillovers could also arise as a result from labor-market competition between treated and untreated job seekers (Crépon et al., 2013; Gautier et al., 2018) and there could be crowding out among job seekers applying for the same vacancies (Ferracci et al., 2014), e.g., marginal jobs. While our experimental design does not explicitly account for the analysis of spillover effects, e.g., through a clustered randomization procedure with varying treatment intensity across different regions (see, e.g., Crépon et al. 2013, Altmann et al. 2022), our randomization procedure gives rise to natural exogenous variation in the share of treated individuals in subgroups of job seekers, who are likely to interact with each other. Specifically, to examine the relevance of treatment spillovers in our setting, we calculate the share of individuals being assigned to the tool treatment within clusters of job seekers, taking into account their place of residence (98 municipalities), their last occupation before becoming unemployed (173 occupations), and their age (five cohorts given by 10-year age bins). Assuming that individuals within a cluster are, on average, more likely to interact with each other than individuals from different clusters (either by informing each other or by competing for similar vacancies), we can use variation in this measure of local treatment intensity to shed light on treatment spillovers. As shown in Figure B.3, we observe substantial variation with respect to treatment intensities across the different clusters. Moreover, Table B.9 shows that individual characteristics have very little predictive power for our measure of local treatment intensity, suggesting there are no systematic differences across clusters with different treatment intensities.

To empirically identify treatment spillovers, we estimate regression models of the following form (similar to Crépon et al., 2013):

$$Y_{ij} = \delta D_i + \gamma TI_j + \theta(D_i \times TI_j) + \eta X_i + \zeta_{ij} \tag{B.1}$$

where TI_j, refers to the local treatment intensity within cluster j (at the region-occupation-age level) and D_i is a dummy variable indicating whether individual i is assigned to the tool treatment. Equation (B.1) allows us to estimate different parameters of interest. First, δ identifies the direct treatment effect in the absence of spillovers. Second, γ show possible spillovers on individuals who are assigned to the control group (or the message treatment). For instance, a negative coefficient would imply that a larger share of treated individuals has a negative impact on the labor market outcomes of non-treated job seekers. Finally, the interaction effects of the actual treatment assignment D_i and the local treatment intensity TI_j, given by θ, inform us about differential spillovers between treated and non-treated individuals. This means that the overall spillover effect on the treatment group is given by $(\gamma + \theta)$. We employ two-way clustered standard errors at the level of municipalities and previous occupations. Table B.10 shows the results for cumulated working hours and earnings over 24 months for two different specifications. First of all, we consider the continuous treatment intensity as depicted in Figure B.3 (see Specification 1). Alternatively, we also consider indicator variables accounting for the top and bottom quintile of the distribution of local treatment intensities (see Specification 2). Overall, we find little evidence for systematic positive or negative treatment spillovers. For instance, the estimates from Specification 2 suggest that higher treatment intensities have a non-linear effect on untreated job seekers, with working hours and earnings in both the top and bottom quintile of the treatment-intensity distribution being both somewhat higher than for intermediate treatment intensities (though both effects are rather imprecisely estimated). While our analysis does, ultimately, not allow us to rule out all possible forms of treatment spillovers (e.g., the simultaneous presence of displacement effects and positive informational spillovers, which tend to cancel each other out), it appears unlikely that treatment spillovers have a large net effect on the results presented in Section 6.

Table B.9: Predictability of local treatment intensity

Dependent variable	**Local treatment intensity**	
	Coef.	SE
Educational level (ref. none or missing)		
Less than high school	-0.0023	(0.0037)
High school	-0.0032	(0.0033)
Bachelor degree (or equiv.)	-0.0027	(0.0032)
Master degree (or equiv.)	-0.0049	(0.0034)
Male	-0.0012	(0.0020)
Age (ref.18-25 years)		
26-35 years	0.0054	(0.0053)
36-45 years	0.0102*	(0.0060)
46-55 years	0.0059	(0.0047)
56-65 years	0.0056	(0.0047)
Household size (ref. one person)		
Two persons	0.0002	(0.0023)
Three persons	-0.0026	(0.0027)
Four or more persons	-0.0001	(0.0032)
Married	-0.0009	(0.0018)
Children (ref. none)		
One child	-0.0007	(0.0024)
Two or more children	-0.0023	(0.0036)
Migration status (ref. Danish)		
1st generation	0.0035	(0.0025)
2nd generation	-0.0012	(0.0045)
Actual nr of joblog at pre-survey	0.0001	(0.0001)
Average monthly earnings in 10,000DKK		
in last year	0.0004	(0.0006)
in last 5 years	0.0001	(0.0003)
Average weekly working hours (×100)		
in last year	-0.0013	(0.0009)
in last 5 years	-0.0001	(0.0003)
Weeks of UI benefits		
in last year	-0.0001	(0.0001)
in last 5 years	0.0000	(0.0000)
Weeks of UI benefit receipt (ref. 26 weeks or less)		
27-52 weeks	0.0037*	(0.0021)
more than 52 weeks	0.0013	(0.0026)
P -value joint sig. UI fund FE		0.162
No. of observations	98,641	
Adjusted R^2	0.0003	

Note: OLS estimation. Two-way clustered standard errors at the municipality-occupational level are shown in parenthesis. ***/**/* indicates statistical significance at the 1%/5%/10%-level, respectively.

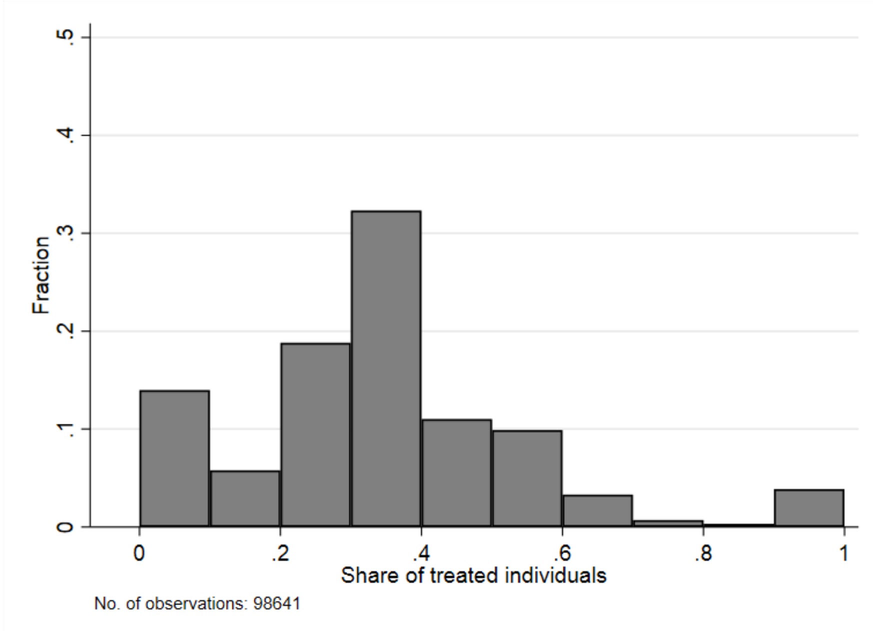
Table B.10: Treatment Effects and spillovers on labor market outcomes

	Cumulated outcomes within 24 months			
	Working hours		Labor earnings in DKK	
	(1)	(2)	(3)	(4)
Specification 1				
Tool treatment	-30.92		-6,583	
	(42.12)		(8,392)	
Local treatment intensity (cont.)	-16.22		-4,635	
	(57.84)		(11,910)	
Tool × local treatment intensity	71.84		16,676	
	(111.7)		(22,126)	
Specification 2[(a)]				
Tool treatment		-11.31		-2,113
		(8.32)		(1,883)
Local treatment intensity (cat.)				
Bottom quintile		18.31		3,011
		(29.32)		(5,376)
Top quintile		42.15		6,383
		(29.51)		(5,442)
Tool treatment				
× bottom quintile		22.42		3,069
		(35.33)		(6,072)
× top quintile		8.95		3,171
		(19.12)		(4,466)
P -value joint significance				
Local treatment intensity (cat.)		0.281		0.476
Tool × treatment intensity (cat.)		0.703		0.649
No. of observations	98,641	98,641	98,641	98,641
Mean value outcome	1,852	1,852	350,582	350,582

Note: The table reports treatment differences and spillover effects on labor market outcomes for different subgroups of participants in the randomized controlled trial. Local treatment intensity refers to the share of treated job seekers (tool treatment) across combinations of 98 municipalities and 173 previous occupations (3-digit DISCO level) and five age cohorts (10-year age bins). Two-way clustered standard errors at the level of municipalities and previous occupations are reported in parentheses. ***/**/* indicates statistical significance at the 1%/5%/10%-level, respectively. All models account for the effect of the message treatment (coefficients are not shown).

[(a)] Top/bottom quintile refer to dummy variables indicating local treatment intensities in the top/bottom quintile of the distribution. The bottom (top) quintile includes all clusters with treatment intensities up to 0.2 (above 0.45).

Figure B.3: Distribution of local treatment intensities



Note: Depicted is the distribution of the local treatment intensity referring to the share of treated job seekers (tool treatment) across combinations of 98 municipalities and 173 previous occupations (3-digit DISCO level) and five age cohorts (10-year age bins).