

DISCUSSION PAPER SERIES

IZA DP No. 14940

**Transparency of the Welfare System and
Labor Market Outcomes of Unemployed
Workers**

Sofie Cairo
Robert Mahlstedt

DECEMBER 2021

DISCUSSION PAPER SERIES

IZA DP No. 14940

Transparency of the Welfare System and Labor Market Outcomes of Unemployed Workers

Sofie Cairo

Copenhagen Business School

Robert Mahlstedt

University of Copenhagen, IZA and DFI

DECEMBER 2021

Any opinions expressed in this paper are those of the author(s) and not those of IZA. Research published in this series may include views on policy, but IZA takes no institutional policy positions. The IZA research network is committed to the IZA Guiding Principles of Research Integrity.

The IZA Institute of Labor Economics is an independent economic research institute that conducts research in labor economics and offers evidence-based policy advice on labor market issues. Supported by the Deutsche Post Foundation, IZA runs the world's largest network of economists, whose research aims to provide answers to the global labor market challenges of our time. Our key objective is to build bridges between academic research, policymakers and society.

IZA Discussion Papers often represent preliminary work and are circulated to encourage discussion. Citation of such a paper should account for its provisional character. A revised version may be available directly from the author.

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Transparency of the Welfare System and Labor Market Outcomes of Unemployed Workers*

We study how the transparency of welfare systems affects labor market outcomes of unemployed workers in a large-scale field experiment. Our low-cost information intervention uses a personalized online tool that informs benefit recipients about their personal risk of a benefit reduction when not complying with a work requirement. We find disparate effects reflecting individuals' job search status. Providing personalized information improves labor market outcomes by mitigating the pressure to accept unstable part-time jobs among active job seekers with a low sanction risk. Inactive persons with a high sanction risk leave welfare and rely on alternative income support more frequently.

JEL Classification: J68, D83, C93

Keywords: unemployment, transfer programs, transparency, sanctions, uncertainty, work requirements, field experiments, information treatment

Corresponding author:

Robert Mahlstedt
Department of Economics
University of Copenhagen
Øster Farimagsgade 5
1353 Copenhagen K
Denmark

E-mail: robert.mahlstedt@econ.ku.dk

* We thank Steffen Altmann, Mette Gørtz, Claus Thustrup Krainer, Steffen Künn, Alexander Sebald and Johannes Wohlfahrt for valuable comments and suggestions. We further thank the participants at the IZA/CREST Conference on Labor Market Policy Evaluation, the Copenhagen Network for Experimental Economics, the Bi-Annual meeting of the Danish Economic Society and seminar participants at the University of Copenhagen for helpful discussions. We are grateful to the Danish Agency for Labor Market and Recruitment (STAR) for data access and their support during the implementation of the field experiment.

1 Introduction

Growing empirical evidence suggests that individuals are often imperfectly informed about their own economic incentives created by tax and transfer policies (see among others Duflo *et al.*, 2006; Chetty and Saez, 2013; Liebman and Luttmer, 2015). This may reflect (i) that it is too costly to acquire the relevant information or (ii) that the underlying incentive schemes are too complex. As a consequence, individuals may make privately suboptimal decisions, which can limit the leverage of a policy and hence the scope for achieving welfare improvements. However, in an increasingly digitalized world, government agencies in many countries are developing online platforms, which allow them to provide personalized information to individual users. This offers unique possibilities to overcome information frictions (Belot *et al.*, 2019) and to increase individuals' understanding of their personal incentive structure.

In our study, we consider the case of social assistance in Denmark where public employment services have to a great extent been transferred to a digital environment during the last decade. In this context, we study how a personalized online information tool that informs unemployed workers in real-time about their individual-specific risk of experiencing a benefit reduction – and the resulting incentives to search for a job – affects their labor market outcomes. Our analysis builds on a large-scale randomized controlled trial that we conducted among the universe of welfare benefit recipients who are required to work a minimum number of hours per year. In case of non-compliance, individuals face the risk of a permanent reduction of their benefit level. The main purpose of such a policy is to increase incentives to search for a job and to return to work, but at the same time it may also improve targeting of benefit payments through a sorting mechanism. If fulfilling the requirement is more costly for inactive persons than for those who are actively searching for a job, the transfer program could become self-targeting by, e.g., encouraging inactive persons to leave the system and rely on different income sources (see e.g. Besley and Coate, 1992; Cuff, 2000; Kreiner and Tranæs, 2005, who theoretically discuss the screening function of work requirements).

Despite these appealing features, the effectiveness of the policy may be limited if unemployed workers have incomplete information about their personal risk of falling short of target. In our sample, we see that the majority of benefit recipients have not even been notified that they are subject to the requirement within the year prior to our intervention.

Moreover, they have no direct access to information about their own situation in relation to the target, such as the number of hours they are missing to avoid a reduction of their benefit level. This lack of transparency allows us to conduct an information experiment investigating the behavioral consequences of relaxing these informational constraints by providing real-time information about the individual’s own situation in relation to the requirement.

The treatment group gains access to a fully-personalized and continuously-updated online information tool that increases the transparency of the welfare system by informing treated individuals of the key features of the policy determining their incentives, namely the number of working hours they have already collected and their specific deadline for compliance with the work requirement. The tool is embedded in the official online platform of the public employment service and it was only made accessible to benefit recipients assigned to the treatment group during the intervention. To inform them about availability of the tool, treated individuals received monthly notifications that they were subject to the requirement including a link to the online tool. Based on comprehensive register data, we evaluate the effectiveness of the treatment against two control groups. The first control group receives almost identical notifications as the treatment group, but has no access to the online tool and therefore receives no personalized information about their risk of falling short of target, i.e. the number of working hours that they lack to fulfill the requirement. This allows us to isolate the partial effect of providing personalized information, which reduces the degree of uncertainty regarding the individual’s personal risk of being sanctioned. The second control group faces a “business-as-usual” environment receiving none of the additional information such that the comparison to the treatment group informs us about the total effect of raising benefit recipients’ awareness of the requirement and of their personal risk of missing the target.

Our intervention targets a disadvantaged group of unemployed workers who rarely find new employment and who are typically not very responsive to traditional policy interventions such as intensified caseworker counseling (see e.g. Rosholm, 2014). Against this backdrop of a very limited labor market attachment of the target population, we find considerable effects of our intervention. While overall treatment take-up is moderate – about 37% of treated individuals opened one of the treatment messages and about 10% directly accessed the online tool through the provided link – and stronger among

benefit recipients who are more attached to the labor market, the intervention reduces the average likelihood of experiencing a benefit cut by 5% relative to the control group receiving none of the additional information.

Moreover, the treatment has substantial effects on labor market outcomes and benefit receipt over a longer time horizon. Over the course of one year, cumulated labor market outcomes of the treatment group are 5-6% higher than those of the control group who only received general notifications. This highlights that informational constraints regarding the personal incentive structure have first-order effects on the behavior of unemployed workers and that providing personalized information increases subsequent working hours and labor earnings. Further analyses show that the treatment response crucially depends on the individual's job search status prior to the intervention.¹ We show that the positive effects on overall employment and earnings are driven by active job seekers and are more pronounced for benefit recipients who receive stronger support from their caseworker. Specifically, the personalized treatment encourages them to focus their employment activities on jobs of higher quality, i.e. more stable full-time employment. This is in line with the notion that providing personalized information affects labor market outcomes either by reducing the perceived risk of being sanctioned or by reducing uncertainty about the risk of falling short of target.

While there is no evidence that the intervention encourages inactive persons to participate in the labor market, we also find important behavioral responses among benefit recipients who do not return to work. Relative to the control group who received none of the additional information, the treatment increased the exit rate from the welfare system into other transfer programs, which are not affected by the requirement, by about 7%. For instance, we observe an increased take-up of disability benefits and more individuals rely on educational benefits when enrolling in post-compulsory education in response to our intervention. We show that predominantly benefit recipients who are likely to be inactive, i.e. who did not collect working hours in the run-up to the intervention, leave the welfare system without returning to work. This suggests that the information treatment triggers a sorting mechanism such that receiving welfare benefits becomes disproportionately unattractive for individuals with a high risk of being sanctioned.

¹We approximate an individual's job search status by an indicator of having collected working hours and registered job applications in the run-up to the intervention.

Our paper adds new insights on the optimal design of social security systems. First of all, we show that information frictions regarding core features of benefit entitlements and personal requirements play an important role for the behavior of benefit recipients. This complements recent evidence indicating that unemployed workers commonly lack information, even about rather basic aspects of the labor market (see e.g. Altmann *et al.*, 2018; Conlon *et al.*, 2018; Belot *et al.*, 2019; Mueller *et al.*, 2021), and that providing information affects individual responses to tax policies (Duflo *et al.*, 2006; Finkelstein, 2009; Chetty and Saez, 2013) and social security systems (Bhargava and Manoli, 2015; Liebman and Luttmer, 2015; Stephan *et al.*, 2016; Fontaine and Kettemann, 2019; Benghalem *et al.*, 2021). While in most of the existing experimental settings information is distributed in a static way (e.g. through e-mails, letters or individual meetings), our study provides an example of how informational constraints can be effectively reduced by relying on dynamic digital tools that can deliver up-to-date personalized information in real-time. While the marginal costs per user are typically low, developing and maintaining a digital infrastructure, which can track individualized outcome data, requires substantial (financial) effort. Against this backdrop, our results indicate that such an investment is to the advantage of tool users such as marginalized job seekers and therefore has the potential to improve overall welfare.

Importantly, personalized information does not only matter for the individuals' immediate response to the policy, in our case the avoidance of a benefit reduction, but also for their labor market integration in the longer run. From a theoretical perspective, the personalized intervention can affect individual behavior either by changing the perceived risk of being sanctioned or by reducing uncertainty about their future benefit level.² Both channels could explain that the treatment mitigates the pressure to accept low quality jobs among active job seekers who face a relatively low objective risk of being sanctioned. However, inactive persons with a high objective risk of being sanctioned leave the welfare system without returning to work when receiving personalized information. The latter suggests that the treatment mainly operates by changing people's perceived risk of being sanctioned rather than by reducing uncertainty.

²Related evidence by Luttmer and Samwick (2018) and Caliendo *et al.* (2019) shows that facing uncertainty about one's future benefit level reduces the utility of risk-averse individuals while receiving benefits. Following this argument, treated individuals who gain access to personalized information and therefore face less uncertainty would have fewer incentives to leave the welfare system.

Moreover, providing personalized information through digital tools does not substitute personal counseling by caseworkers. Rather, our findings show that our intervention is more effective when it is accompanied by intensive caseworker counseling, which suggests that traditional forms of job assistance and digital tools complement each other.

Finally, our results provide direct empirical support for the theoretically appealing idea of using requirements as a screening device that improves the targeting of transfer payments (Nichols *et al.*, 1971; Nichols and Zeckhauser, 1982; Besley and Coate, 1992). Relatedly, Alatas *et al.* (2016) show that adding a small application cost to a transfer program can substantially improve targeting such that those who are not in need of financial support enroll less frequently. We document a similar mechanism even without varying financial incentives by informing benefit recipients about their risk of experiencing a reduction of their benefit level. This triggers sorting of individuals with limited chances of finding employment into other transfer programs that are designed to support unemployed workers with health problems or those in need of further education.

2 Welfare Benefits in Denmark

Before discussing the experimental design, we present the institutional rules that are relevant for our information intervention. Welfare benefits in Denmark provide a safety net for unemployed workers without personal wealth who are not entitled to unemployment insurance benefits. Benefit payments are means-tested and the benefit level depends on the individual's age, the presence of children in the household, and the income of a potential spouse. A single person (older than 30 years) receives 11,554DKK ($\approx 1,680\$, 2020$ -level) per month, while the amount increases to 15,355DKK ($\approx 2,230\$, 2020$ -level) when children live in the household. Benefit recipients, who are younger than 30 years, receive about 65% (without children), respectively 96% (with children), of the baseline level. The benefit level of individuals with a working spouse might be reduced to ensure that the gross household income does not exceed two times the benefit level of the individual recipient.

The work requirement: While there is no limit regarding the duration of welfare benefit receipt, individuals are confronted with a work requirement. This applies to all individuals who have received benefits for at least 12 months within the last three years.

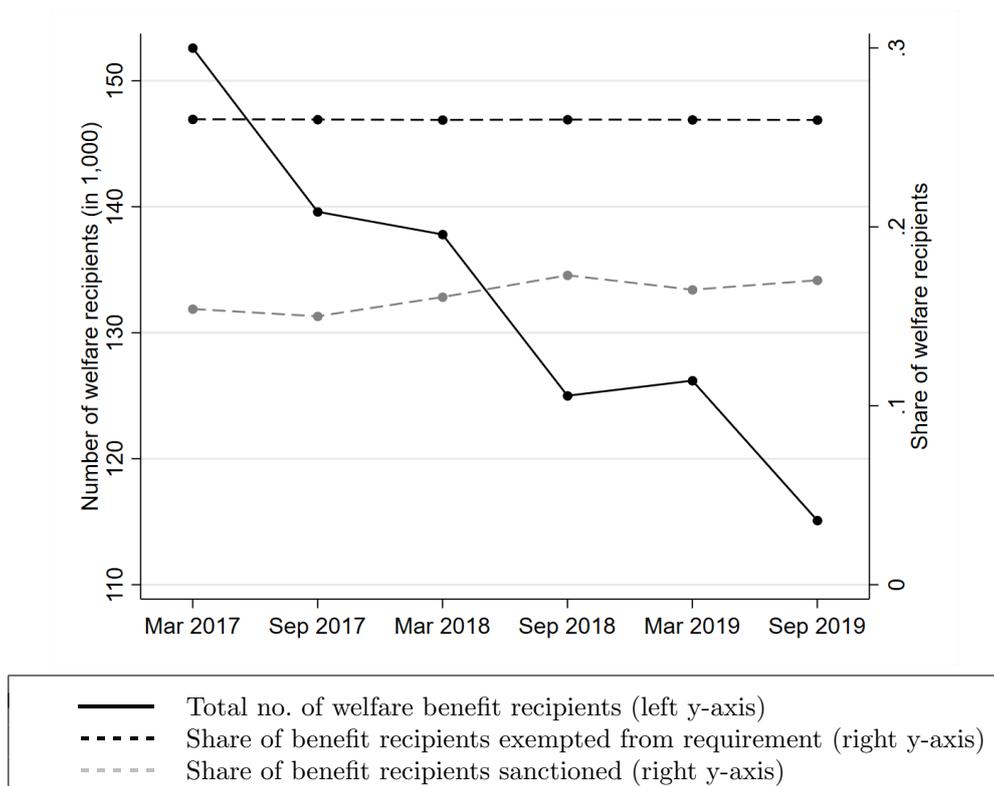
After receiving benefits for one year, individuals are entitled to the full benefit level only if they have worked at least 225 hours within the previous 12 months in regular non-subsidized jobs.³ The requirement is equivalent to five working hours per week or about six weeks of full-time work per year and the exact allocation of working hours is at the individual's discretion. This implies that the benefit period can be interrupted by episodes of employment during which the individual receives no benefit payments. An individual who re-enters the welfare system after a short period of employment is still subject to the requirement and the count of working hours is maintained (plus the newly collected hours).

If a benefit recipient does not comply with the work requirement, the benefit level is reduced by approximately 500 to 1,000DKK per month, though the exact amount depends on the individual's family status. The criteria must be fulfilled at each point in time (considering the preceding 12-month window). This implies that even if the individual fulfills the requirement in a given month, they might be subject to a reduction in the following month. The first benefit reduction can be imposed at the earliest 12 months after the initial registration for welfare benefits, while working hours are counted from the first day of registration onward. After a sanction has been imposed, resulting in a permanently lower monthly benefit level, the count of accumulated working hours is set to zero and the individual has to restart collecting 225 working hours before the sanction is removed. Caseworkers can grant exemptions from the requirement when individuals are deemed incapable of working at least five hours per week, e.g. due to mental or physical constraints.

Figure 1 shows the total number of welfare recipients, as well as the share of exempted and sanctioned over the course of time. While the number of individuals receiving welfare benefits decreases by about one quarter between March 2017 and September 2019, the share of benefit recipients who are exempted from the requirement is remarkably stable at about 26%. Moreover, in a given month, 15-18% of those who are subject to the requirement receive benefits at a reduced level, which highlights that the requirement plays an important role for many welfare recipients and there seems to be substantial scope to improve their personal situation.

³Specifically, the requirement applies to all individuals who received welfare benefits for at least 12 months within the last three years.

Figure 1: Welfare benefit recipients, exemptions and sanctions over time



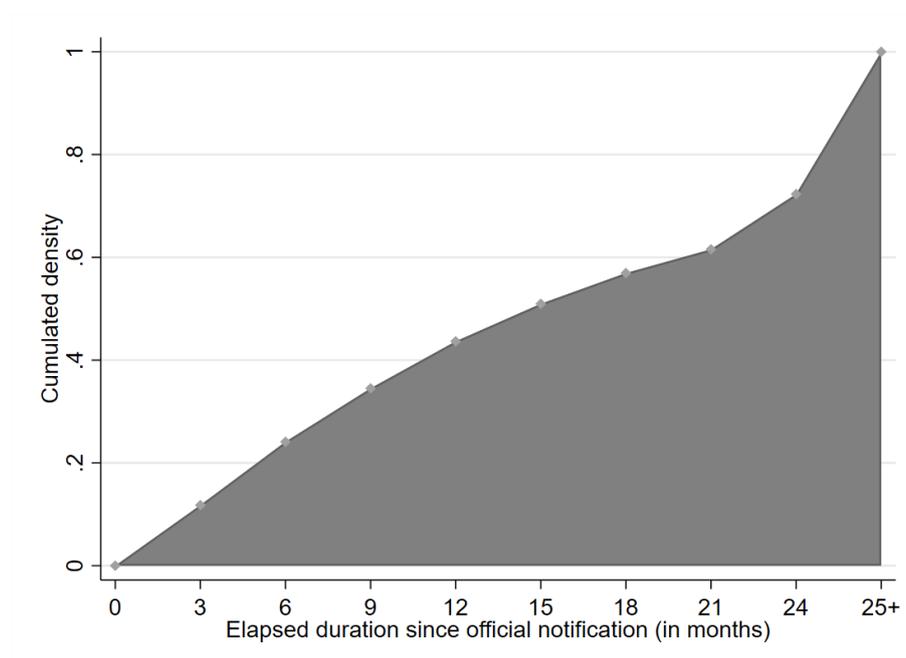
Note: Depicted are the total number of welfare recipients (left y-axis) and the share of individuals being sanctioned and exempted from the requirement (right y-axis) in a given calendar month over the course of time.

It should be noted that similar policies exist in many countries. For instance, the US (see, e.g., Bloom and Michalopoulos, 2001; Moffitt, 2003; Grogger and Karoly, 2009) and Canada (Berg and Gabel, 2015) implemented work requirements in combination with benefit sanctions already during the 1990s, while in many European countries benefit entitlements are directly related to comparable requirements, e.g., to apply for a minimum number of jobs, or to engage in certain work activities (see, e.g., Venn, 2012, for an overview).

Information management: The main purpose of a work requirement is to increase incentives to search for a job and to return to work. However, for such a policy to be effective, benefit recipients (i) should be aware that they are subject to the requirement and (ii) should have an accurate assessment of their personal risk of falling short of target. By default, all benefit recipients receive an official notification from the municipality, which is responsible for their benefit payments, when they have received welfare benefits for about six months and have not worked a sufficient number of hours to fulfill the

requirement. The notification letter informs them about their potential reduction date and the number of working hours they are missing to prevent the sanction from being effectuated. This means that all welfare recipients who stand to be sanctioned if they do not work additional hours have been notified about the risk of a permanent benefit reduction. However, the complexity of the underlying rules, i.e. the fact that working hours continuously forfeit, and differences between municipalities regarding the implementation of the rules imply that there is substantial heterogeneity regarding the duration between the notification and the actual imposition of a sanction.

Figure 2: Timing of official notification about work requirement



Note: Depicted is the cumulated density function of the elapsed duration since the official notification about the work requirement (received by the municipality that is responsible for the benefit payments) for the full sample of welfare benefit recipients who already received the official notification but have not been sanctioned as of August 15, 2018 ($N = 47,294$).

Figure 2 shows the cumulated distribution of the elapsed duration since the last official notification for the stock of welfare recipients who have already been notified (as of August 15, 2018), but have not had their benefit level reduced. It can be seen that the majority of benefit recipients received the notification about the requirement more than 12 months ago. While a benefit recipient who does not collect any working hours should in principle experience a benefit reduction approximately six months after the official notification, different factors could lead to a prolongation of this period. Obviously, by collecting working hours individuals can postpone the reduction date. Alternatively, caseworkers can

grant discretionary extensions if the individual had periods of mental or physical illness. Moreover, it could be that individuals have experienced a reduction of their benefits (after receiving the notification) and the sanction has already been removed. In all these cases, the individual is still subject to the requirement, but the lack of information may imply that they are not aware of it.⁴

Besides the official notification letter, benefit recipients have only limited access to real-time information about their own situation, i.e. the number of working hours they have already collected, and their personal risk of being sanctioned.⁵ Individuals can arrange a meeting with their caseworker, who can then contact a corresponding database to retrieve information about the number of collected working hours. Alternatively, benefit recipients can keep count of their working hours themselves. However, both options require substantial effort and welfare recipients might not be aware of the importance of keeping such a count, respectively of the possibility to contact the corresponding database. At the same time, there is evidence that caseworkers, who are responsible for implementing the rules, provide relatively little assistance that is tailored towards the benefit recipients' personal situation in relation to the work requirement. For instance, according to a caseworker survey that aims to shed light on the counseling situation in relation to the work requirement⁶, 47% of caseworkers claim that they have no possibilities to support individuals who are subject to the requirement. Moreover, 71% report capacity constraints and only 7% say that they receive sufficient IT support in relation to the work requirement. Altogether, this suggests that caseworkers provide little informational support and individuals may commonly lack information about their personal risk of being sanctioned.

Other transfer programs: The presence of a requirement may not only affect job search incentives, but could also encourage individuals, especially those who are detached from the labor market, to leave the welfare system. In this regard, it is important to note

⁴In Section 3.2, we present descriptive statistics for the sample of benefit recipients who had been notified and currently receive benefits at the full rate. While about 11% experienced a reduction of the benefit level during the last 12 months, 35% have been exempted from the requirement and 12% worked in a paid job during this period.

⁵Four weeks before the benefit level is actually reduced the municipality contacts the benefit recipient, which provides the last possibility to report any working hours that have not been registered yet.

⁶The survey was conducted by the Danish Association of Social Workers in November 2016 including the responses of 137 caseworkers who work with welfare benefit recipients. The caseworker survey can be accessed [here](#) (in Danish).

that various other transfer programs exist in Denmark, which may provide alternative income sources for welfare benefit recipients. For instance, Danish citizens are generally eligible for income support, when enrolling in a higher educational program (secondary or tertiary education). This also applies to welfare recipients. Moreover, individuals who are already eligible for early retirement schemes could possibly rely on pension payments⁷, while those with physical or mental constraints can claim sickness or disability benefits. In general, the level of other benefits is the same as the level of welfare benefits, but otherwise the rules are different: the individual no longer faces the work requirement and the benefit sanction only applies to welfare benefits. When considering our sample of welfare recipients (which is described in more detail in Section 3.2), about 22% of all individuals received other forms of income support over the course of one year. This shows that unemployed workers in Denmark regularly switch between different transfer programs.

3 Study Design

To study how personalized information affects the labor market reintegration of unemployed workers, we combine data from a countrywide randomized controlled trial and administrative data from Danish social security records. The experiment focuses on welfare benefit recipients who are subject to the work requirement and lack important information about their own situation as described in Section 2. In what follows, we outline the relevant experimental and empirical details of our setting.

3.1 Randomized Controlled Trial

To increase the transparency of the benefit recipients' personalized incentive structure, we rely on an online information strategy that relaxes informational constraints of treated individuals with respect to two dimensions. First and foremost, members of the treatment group gain access to an online tool providing them with personalized information about (i) the number of working hours they have already collected, respectively the hours they are missing to fulfill the requirement and (ii) the potential reduction date when their benefit entitlements will be reduced. Importantly, the tool (which is visualized in Figure A.1 in

⁷Individuals are eligible for early retirement five years before the legal retirement age. The latter depends on the individual's birth date and is currently fixed at 67 years for those born after 1955.

Appendix A.2) provides personalized real-time information that is continuously updated when benefit recipients collect additional working hours or when collected hours forfeit. By providing access to this information around the clock, the tool minimizes the effort of keeping count of working hours for benefit recipients. Thereby, it reduces uncertainty about the likelihood of falling short of target and facing the risk of a benefit reduction. The tool is embedded in the official online platform of the Danish public employment service (jobnet.dk) and is only accessible for individuals who are assigned to the treatment group.

Moreover, to inform treated individual about the existence of the tool, they receive up to six (additional) monthly notifications that they are still at risk of incurring a benefit reduction. The messages themselves contain only general information about the requirement of working 225 hours per year and provide some examples of how to fulfill the criteria, but more importantly they also include a link to the personalized online tool. They further emphasize the risk of a benefit cut and the importance of keeping an overview of collected working hours. Given that benefit recipients do not receive regular notifications that they are (still) affected by the requirement in the absence of the intervention, the treatment may raise their awareness that this is the case. As discussed in Section 2, more than 50% of our sample received the official notification more than a year before the start of the intervention (see also Figure 2) possibly without being further notified that they are subject to the requirement.

To disentangle the causal effect of the two treatment elements (the general message and the personalized tool), we compare the outcomes of treated individuals to benefit recipients in two control groups. The first control group receives reminder messages, which are almost identical to the one's of the treatment group, but the messages do not include a link to the online tool. Hence, they do not receive any personalized information regarding the number of working hours they have already collected. This leaves a substantial degree of uncertainty about how many hours the specific individual has to work, respectively about the likelihood of actually facing a reduction of the benefit level. The presence of a control group that also receives reminder messages allows us to isolate the partial effect of providing personalized information through the online tool. Finally, individuals in the second control group face a “business-as-usual” environment and neither receive reminder message nor have access to the online tool. This allows us to identify the total

effect of raising individuals' awareness of the requirement and of providing personalized information about their risk of falling short of target.

3.2 Sample and Procedures

From the full stock of welfare benefit recipients on August 15, 2018, the intervention targets all individuals who are subject to the work requirement as described in Section 2 and have already received their official notification letter. In total, our sample comprises 47,294 individuals who are randomly assigned into the three treatment arms as described in Section 3.1. Individuals received the initial message on August 15, 2018, followed by up to five monthly reminders as long as they were subject to the work requirement. All messages were sent out by the Danish public employment service to the individual's inbox at the official web portal (*jobnet.dk*), which also contains the online tool (only available for individuals in the treatment group).

We link the experimental data to comprehensive register data administered by Statistics Denmark. This provides us with detailed information on socio-demographic background characteristics obtained from population registers, benefit payments (DREAM), as well as income and employment (E-income), including labor market histories of individuals in our sample. Moreover, we exploit additional data collected by the public employment service, including information on imposed sanctions, granted exemptions, collected working hours displayed in the online tool, individual-level click data and registered job applications.

Table 1 summarizes descriptive statistics regarding socio-demographic background and household characteristics, as well as labor market histories separately for the three treatment groups. First, one should note that there is no indication of imperfect randomization since the background characteristics are balanced across treatment groups. When considering basic socio-demographic information, we see that about 48% of the participants are younger than 35 years, while 49% are female and 17% are married. Moreover, a large share of the experimental population seems to have a very limited attachment to the labor market. For instance, only about 21% had any paid employment in the year before the intervention and 71% are categorized as incapable of starting full-time employment without further support by their caseworker. Moreover, the average monthly gross labor income in the year before the intervention was only about 1,800DKK (equivalent to

Table 1: Descriptive statistics by treatment status

	Mean value by treatment status ^(a)			<i>P</i> -values	
	Control I (general notification)	Control II (no general notification)	Treated	Treated - control I	Treated - control II
No. of observations	15,764	15,769	15,761		
Female	0.491	0.495	0.500	0.099	0.418
Married	0.171	0.172	0.176	0.249	0.339
Education					
Less than high school	0.033	0.033	0.034	0.509	0.749
High school	0.589	0.587	0.582	0.207	0.389
Bachelor degree or equivalent	0.266	0.269	0.271	0.259	0.619
Master degree or equivalent	0.089	0.086	0.088	0.666	0.526
Age					
16-25 years	0.220	0.216	0.215	0.380	0.877
26-35 years	0.269	0.272	0.268	0.807	0.356
36-45 years	0.210	0.213	0.219	0.052	0.181
46-55 years	0.193	0.193	0.193	0.971	0.926
56-65 years	0.108	0.105	0.104	0.260	0.739
Migration background					
1 st generation	0.252	0.246	0.250	0.783	0.397
2 nd generation	0.037	0.037	0.036	0.678	0.840
Living in Capital Region	0.324	0.326	0.320	0.546	0.287
Children					
One child	0.154	0.151	0.154	0.870	0.509
Two or more children	0.220	0.228	0.232	0.011	0.378
Household size					
One person	0.299	0.298	0.302	0.524	0.429
Two persons	0.232	0.234	0.229	0.518	0.285
Three persons	0.167	0.169	0.169	0.690	0.932
Four or more persons	0.302	0.298	0.300	0.712	0.792
Not deemed capable of full-time employment	0.712	0.712	0.712	0.991	0.993
Consecutive weeks on welfare	134.1	134.5	135.1	0.519	0.746
Pre-treatment outcomes (in previous year)					
Any paid employment	0.210	0.216	0.215	0.222	0.834
Total working hours	138.2	144.4	144.5	0.135	0.806
Labor earnings in DKK	21,552	22,368	21,912	0.588	0.497
Any benefit reduction	0.111	0.114	0.112	0.910	0.556
Exempted from requirement	0.352	0.344	0.346	0.267	0.797

Note: Percentage share unless indicated otherwise. *P*-values measured based on two-tailed t-tests on equal means.

^(a)Treated individuals receive up to six monthly reminder messages including general information about the work requirement and a link to the online tool providing personal information. Individuals assigned to the first control group (control I) receive general reminder message without having access to the personalized online tool. Individuals assigned to the second control group (control II) receive neither reminder message nor have access to the online tool.

approx. 235\$), and the average individual had already received welfare benefits for more than two years without interruption. This indicates that many individuals in our sample have a very limited labor market attachment. However, it should be noted that, although clearly not the majority, a part of the experimental population showed substantial labor market activity in the past and seems capable of meeting the work requirement. Finally,

about 11% experienced a reduction of the benefit level during the last year, while about 35% were exempted from the work requirement at some point during this period.

To assess treatment take-up, we consider individual-level click data. Around 36.6% of all treated individuals opened at least one of the messages that they received⁸ and 9.6% clicked on the link to the online tool at least once within a year after the intervention. Moreover, Table A.1 in Appendix A.2 shows the relationship between treatment take-up and individual background characteristics. Women, native Danes and higher educated individuals with a stronger labor market attachment are more likely to open the treatment message and to click on the link to the online tool. The same is true for benefit recipients who experienced a reduction of the benefit level in the past.

3.3 Possible Effects of the Information Treatment

The main objective of our intervention is to make the welfare system more transparent by relaxing informational constraints regarding the benefit recipients' personal situation. It is plausible that the increased transparency reduces the likelihood that treated individuals experience a reduction of their benefit level. However, such an effect can be associated with different mechanisms that have disparate effects on the individual labor market outcomes over a longer horizon.

Raising awareness of requirement: First of all, the intervention could affect individuals' behavior independently of the personalized content that is presented in the online tool. For instance, individuals who were not aware of the work requirement before the experiment may realize that they face the general risk of a benefit reduction. Hence, they may respond to the treatment by shifting their search focus towards temporary or part-time jobs, which are often perceived as a means to collect a sufficient number of working hours to fulfill the work requirement. There is some empirical evidence that promoting these non-regular jobs can stimulate the creation of permanent employment, especially for workers with low labor market attachment (see Caliendo *et al.*, 2016; Gerfin *et al.*, 2005). However, non-regular employment can be also associated with a locking-in effect, i.e. individuals spend less time on search for regular full-time jobs meanwhile, which can

⁸It should be noted that there is no difference with respect to the likelihood of reading the message between the treatment group and the control group who receives general reminder messages ($p = 0.987$).

have adverse effects on overall labor market outcomes (see Fremigacci and Terracol, 2013; Kyyrä *et al.*, 2013).

Moreover, benefit recipients who become aware that they are affected by the requirement should perceive the welfare system as generally less attractive. This provides incentives to search more intensively for new employment in order to leave the system and to no longer be subject to the requirement.⁹ While individuals may return to work faster (see e.g. Michalopoulos *et al.*, 2005), it could also encourage them to accept jobs with a lower match quality (see also Arni *et al.*, 2013; Van den Berg and Vikström, 2014; Nekoei and Weber, 2017, who document similar mechanisms). Hence, the expected consequences for overall employment and earnings are not clear cut.¹⁰

Subjectively perceived sanction risk: Gaining access to the online tool allows individuals to follow their own situation more accurately, which could affect labor market outcomes by influencing individuals' subjective beliefs about their personal risk of experiencing a benefit reduction. In this context, the behavioral consequences depend on the benefit recipients' expectations prior to the intervention. On the one hand, individuals who overestimate their personal risk of a benefit reduction, i.e. because they underestimate the number of working hours that they already collected, may experience less pressure to leave welfare when they they have access to personalized information. Such a scenario appears more likely among benefit recipients who have a low objective risk because they typically work many hours. On the other hand, the personalized treatment would have the opposite effect on benefit recipients who underestimate their objective sanction risk in absence of the intervention. This might be more widespread among benefit recipients who do not collect a large number of working hours.

Moreover, providing personalized information may not only change individuals' perception about their personal likelihood of being sanctioned, but it may also reduce uncertainty about their future benefit level. As argued by, e.g., Luttmer and Samwick (2018),

⁹A large empirical literature has documented that tighter job search requirements (Petrangolo, 2009; Arni and Schiprowski, 2019), sanction regimes (Abbring *et al.*, 2005; Van den Berg *et al.*, 2004; Lalive *et al.*, 2005; Svarer, 2011) and mandatory requirements to participate in certain labor market programs (see e.g. Black *et al.*, 2003; Geerdsen, 2006; Graversen and Van Ours, 2008) increase outflows from registered unemployment. Similarly, a less generous UI benefit or welfare system increases individuals' search effort (Lichter, 2016; Marinescu, 2017) and shortens the unemployment duration (Katz and Meyer, 1990; Card and Levine, 2000; Fortin *et al.*, 2004; Lalive *et al.*, 2006; Lemieux and Milligan, 2008; Bargain and Doorley, 2011; Schmieder *et al.*, 2012).

¹⁰See, e.g., Keeley and Robins (1985) for a comprehensive theoretical discussion of the possible effects of work and search requirements in a job search model.

uncertainty about future benefit payments reduces the recipients' opportunities to smooth consumption and therefore also individual utility while receiving benefit payments. This implies that minimizing the degree of uncertainty through personalized information may reduce the pressure to leave the system, e.g., by having job seekers accept low-quality jobs. Again, the consequences for overall employment and earnings are not clear cut. On the one hand, this could lead to lower job finding rates. On the other hand, it may improve the job match quality.

Sorting mechanism: The intervention may not only affect job search and employment, but could also trigger behavioral responses even when benefit recipients do not return to paid employment. Increasing the perceived risk of a benefit reduction may encourage unemployed workers to leave the welfare system and to rely on alternative income sources.¹¹ Since the Danish social security system comprises various other transfer programs that aim to support unemployed workers with health problems or a lack of education (see Section 2) and do not involve a work requirement, we expect an increased take-up of other types of income support.

The magnitude of such an effect should be stronger for individuals who find it very difficult or costly to work a sufficient number of hours. For instance, Kreiner and Tranæs (2005) argue that the presence of a work requirement makes the welfare system disproportionately unattractive for individuals who are voluntarily unemployed due to their higher preferences for non-working time. Informing benefit recipients that they are subject to the requirement may have a similar effect by stimulating exits from the welfare system to non-work activities predominantly among unemployed individuals who are not actively searching for a job and typically collect zero or only very few working hours. This means that our information intervention could trigger a sorting mechanism such that inactive persons leave welfare, e.g. to enter other transfer programs, and the system becomes self-targeting towards unemployed workers actually searching for employment (see also Akerlof, 1978; Nichols and Zeckhauser, 1982; Besley and Coate, 1992, 1995; Alatas *et al.*, 2016, who discuss similar arguments).

¹¹On a more general level, Card *et al.* (2007) document that many unemployed workers de-register after their benefits expire without returning to work.

Caseworker counseling: Finally, we are interested in how our intervention interacts with the intensity of caseworkers’ counseling activities. This is interesting because caseworkers play a crucial role in supporting job seekers during the search process (Behncke *et al.*, 2010; Schiprowski, 2020) and there are regional differences in the extent to which local employment services support benefit recipients who are at risk of experiencing a benefit sanction.¹² There are two possible channels through which more intensified counseling activities may interact with our treatment. On the one hand, it could be that individuals who receive stronger support by their caseworker are better informed about their personal situation in absence of the treatment. Therefore, providing additional information through the online tool should have a larger effect on individuals who receive little support by their caseworker. This, however, builds on the notion that caseworkers inform benefit recipients about their personal situation in relation to the requirement in a similar way as the online tool, which appears questionable given the complexity of the underlying rules and the caseworkers’ difficulties to tailor their counseling activities towards the benefit recipients’ personal situation in relation to the work requirement. On the other hand, there could exist complementarities such that the information treatment is more effective when accompanied by more intensive counseling because caseworkers might be able to refer job seekers, who receive personalized information, to suitable vacancies.¹³

3.4 Estimation Strategy

We identify the causal effects of our intervention by estimating the following empirical model:

$$Y_i = \beta_0 + \beta_1 D_i + \beta_2 X_i + \varepsilon_i, \quad (1)$$

where X_i is a vector of pre-intervention control variables, i.e. socio-demographic characteristics and labor market histories, as presented in Table 1 and dummies for place of residence (98 municipalities), and Y_i denotes the different outcome variables of interest. Finally, β_1 identifies the parameter of interest, the intention-to-treat effect (ITT),

¹²For instance, the caseworker survey, discussed in Section 2, suggests that about 22% of caseworkers are advised to provide more intensified counseling, 20% have the instruction to directly refer benefit recipients to small work opportunities, while 34% provide no additional support.

¹³It has been shown that employment agencies in many countries use vacancy referrals as an important tool to facilitate unemployed workers (see e.g. Fougère *et al.*, 2009; Engström *et al.*, 2012; Bollens and Cockx, 2017; van den Berg *et al.*, 2019).

while D_i indicates the individual’s treatment status. Specifically, we estimate two separate models comparing the treatment group to each of the two control groups (with and without general notifications). This allows us to identify (i) the *partial effect* of providing personalized information when both groups have been informed about the general risk of a benefit reduction and (ii) the *total effect* of providing personalized and general information compared to the non-transparent “business-as-usual” environment. Considering treatment effects in comparison to the two different control groups is crucial as it allows us to separate the effect of providing personalized information from the effect of increasing individuals’ general awareness of the requirement.

Moreover, we estimate *treatment effects on the treated* by two-stage least squares (see e.g. Angrist *et al.*, 1996) to account for the fact that 37% of treated individuals opened the treatment message, while only about 10% clicked on the link directing them to the online tool. This allows us to identify the effect of (i) reading the general reminder messages and (ii) utilizing the personalized online tool. We use the assigned treatment status as an instrumental variable. First, we estimate the effect of reading the general message by instrumenting a dummy indicating whether the individual opened the message with an indicator of being assigned to control group I who received general reminders without having access to the tool ($0 \equiv$ control II and $1 \equiv$ control I). Second, we estimate the additional effect of using the personalized online tool using the treatment status ($0 \equiv$ control I and $1 \equiv$ treated) as an instrument for the individual decision to click on the link.

4 Results

In the following, we present the results of our empirical analysis in several steps. First, we test whether our intervention reduces the risk of actually experiencing a reduction of the benefit level. Second, we consider employment and earnings to investigate whether the (perceived) incentives to search for and accept a job have been altered and whether this actually matters for realized labor market outcomes. Third, we consider behavioral responses among individuals who do not return to work. Finally, we show heterogeneous treatment effects with respect to (i) individuals’ job search status prior to the intervention and (ii) regional differences in the intensity of caseworker counseling.

4.1 Reduction of Benefit Level

First of all, we expect that relaxing informational constraints reduces the risk of being sanctioned among benefit recipients. Therefore, we consider an indicator for whether an individual’s benefits were reduced within the first six months after the start of the intervention. All participants in our experiment had already received the official notification and potential benefit sanctions are typically imposed about six months after the official notification if benefit recipients do not collect a sufficient number of working hours or receive an exemption from the requirement.

Table 2: Intention-to-treat effects on reduction of benefit level

	(1) Partial effect of personalized information		(2) Total effect of personalized and general information	
	Treated - control I		Treated - control II	
	Intention- to-treat effect	Mean value control I	Intention- to-treat effect	Mean value control II
Dependent variable (within six months after start of intervention):				
Any benefit reduction imposed	0.000 [0.936]	0.109	-0.006 [0.098]	0.115
Any exemption from requirement granted	0.003 [0.461]	0.116	0.000 [0.975]	0.119
No. of observation	31,525		31,530	
Control variables	Yes		Yes	

Note: Depicted are intention-to-treat effect of being assigned to the treatment group in comparison to each of the two control groups (with and without receiving a general notification). P -values are shown in square brackets. Control variables are depicted in Table 1.

(1) Partial effect: compares the treatment group who receives general notifications message and has access to the online tool to control group I who only receives general notifications.

(2) Total effect: compares the treatment group who receives general notifications and has access to the online tool to control group II who receives none of the additional information.

While the partial effect of providing personalized information is close to zero and statistically insignificant (see specification 1 in Table 2), we find that having access to the online tool in combination with the general notifications reduces the likelihood of experiencing a benefit cut by about 0.6 percentage points (see specification 2 in Table 2) relative to the control group who receives none of the additional information. This is equivalent to a reduction of the sanction rate by about 5% relative to the mean value of the control group. Although this effect is imprecisely estimated and only marginally significant ($p = 0.098$), it indicates that relaxing informational constraints in relation to the work requirement has an impact on the behavior of benefit recipients. While

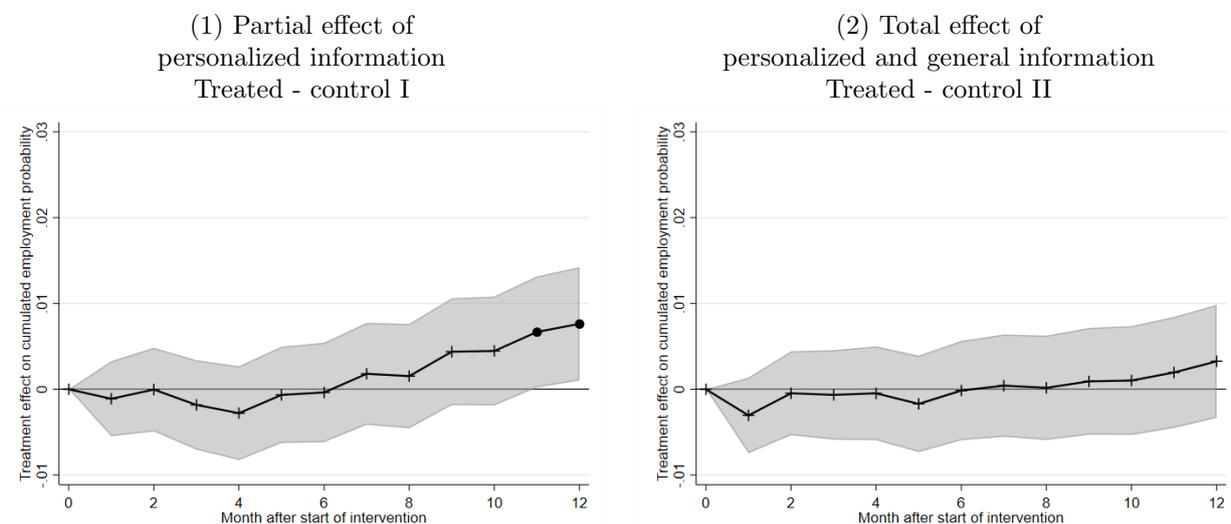
personalized information is no more effective than general notifications, they may operate through different mechanisms (see our discussion in Section 3.3). Therefore, it is crucial to consider a broader set of outcome variables to assess the behavioral responses of benefit recipients.

Finally, it should be noted that we find no evidence that the lower sanction rate is provoked by a higher likelihood of being exempted from the requirement.¹⁴

4.2 Employment and Earnings

Next, we present treatment effects on outcome variables related to paid employment, which informs us to what extent relaxing information constraints may affect the perceived job search incentives and whether this matters for the benefit recipients' labor market reintegration. We present ITTs on two types of outcome variables. First, cumulated job finding rates (as presented Figure 3) inform us about the treatment effects on the extensive margin of employment. Second, total working hours and labor earnings accumulated over the first 12 months after the start of the intervention (see Table 3) account for the intensive margin.

Figure 3: Intention-to-treat effects on cumulated job finding rates over time



Note: Depicted are intention-to-treat effect of being assigned to the treatment group in comparison to each of the two control groups (with and without receiving the general message) on cumulated job finding rates up until month t including 90% confidence intervals.

Mean value control I after 12 months: 0.236

Mean value control II after 12 months: 0.244

¹⁴Caseworkers can grant exemptions if benefit recipients claim that it is unreasonable to fulfill the work requirement due to their personal mental or physical condition.

We find that providing personalized information by giving individuals access to the online tool substantially increases job finding rates (+3%, $p = 0.057$), as well as the total number of working hours (+7%; $p = 0.007$) and labor earnings (+6%; $p = 0.019$) in comparison to individuals assigned to the first control group who only received general notifications (see specification 1). This is a key result as it highlights that providing accurate information about the sanction risk and the individual's personal incentives has first-order effects on their labor market performance. This implies that digital tools, as a means for transmitting personalized information in real-time, can be to the advantage of unemployed individuals who use such tools.

Interestingly, we find stronger treatment effects when taking into account the intensive margin with respect to earnings and working hours compared to the pure extensive margin effects. This is in line with the idea that the personalized information treatment mainly reduces the pressure to accept low-quality jobs by reducing either the perceived risk of being sanctioned or uncertainty. To test this idea more explicitly, we also take into account the nature of the resulting job matches by differentiating between full-time and part-time employment, i.e. between jobs with an average weekly number of working hours above and below 90% of the full-time equivalent (corresponding to 37 hours per week). Our results show that almost 80% of the additional working hours and earnings are generated in full-time jobs, while there is no evidence that having access to the online tool promotes part-time employment.

While part-time jobs are often perceived as a means to collect a sufficient number of working hours to fulfill the requirement¹⁵, the provision of personalized information encourages benefit recipients to focus their job search activities on regular full-time employment. This suggests that more accurate information about the personal risk of being sanctioned reduces the perceived pressure to search for and accept small work opportunities. Given that working in these non-regular jobs is often associated with a locking-in effect, the stronger focus on regular employment can explain how the personalized intervention improves the benefit recipients' labor market performance.

Second, when comparing the treatment group to the second control group who received no general notifications (see specification 2), we find no significant differences in labor

¹⁵It can be also seen in Table 3 that, in general, the majority of working hours and earnings is generated in part-time jobs, which highlights the importance of non-regular employment among the target population.

Table 3: Intention-to-treat effects on employment and earnings

	(1) Partial effect of personalized information		(2) Total effect of personalized and general information	
	Treated - control I		Treated - control II	
	Intention- to-treat effect	Mean value control I	Intention- to-treat effect	Mean value control II
Dependent variable (within 12 months after start of intervention):				
Total no. of working hours	10.00 [0.007]	142.59	1.94 [0.609]	153.07
in part-time job ^(a)	2.04 [0.367]	85.77	-0.81 [0.725]	90.22
in full-time job ^(a)	7.97 [0.003]	56.82	2.75 [0.317]	62.85
Total labor earnings in DKK	1,392 [0.018]	21,978	104 [0.864]	23,676
in part-time job ^(a)	367 [0.402]	13,524	-94 [0.801]	14,198
in full-time job ^(a)	1,085 [0.009]	8,454	199 [0.644]	9,477
No. of observation	31,525		31,530	
Control variables	Yes		Yes	

Note: Depicted are intention-to-treat effect of being assigned to the treatment group in comparison to each of the two control groups (with and without receiving the general message). *P*-values are shown in square brackets. Control variables are depicted in Table 1.

(1) Partial effect: compares the treatment group who receives general reminder message and has access to the online tool to control group I who only receives general reminder messages.

(2) Total effect: compares the treatment group who receives general reminder message and has access to the online tool to control group II who receives none of the additional information.

^(a) Full-time/part-time refers to jobs with a weekly number of working hours above (or equal to)/below 90% of the full-time equivalent (corresponding to 37 hours per week).

market outcomes. This zero effect, however, masks two contrary effects. On the one hand, gaining access to personalized information regarding the number of collected working hours improves benefit recipients' subsequent labor market performance. On the other hand, non-personalized notifications have the opposite effect compared to the opaque environment where benefit recipients may not even be aware that they are subject to the requirement. It appears plausible that the latter is the consequence of raising benefit recipients' awareness of the requirement. However, without having access to the online tool they face considerable uncertainty about their personal risk of experiencing a benefit reduction. In such a situation, only receiving general notifications increases the pressure to accept non-regular jobs because they may overestimate the risk of a sanction. This apparently reduces overall employment due to locking-in effects.

A potential cause of concern arises from the binary nature of the work requirement. Given that the online tool provides accurate information about the number of collected working hours, providing personalized information may reinforce potential bunching effects such that benefit recipients reduce their work effort when realizing that they are above target.¹⁶ We test the sensitivity of our results by re-estimating ITTs on working hours and labor earnings, but set the corresponding outcome variable to zero if the total number of working hours within 12 months is below a certain threshold. Thereby, we disregard working hours and earnings of those who are relatively close to the cutoff and are most likely to be affected by the binary nature of the incentive scheme.¹⁷ As shown in Figure A.2, the results are remarkably stable when considering different thresholds. Altogether, we find no evidence that bunching behavior around the threshold explains our results.

The disparate effects of general and personalized information: The differential treatment effects on labor market outcomes in comparison to the two control groups suggest that the two treatment elements – receiving general notifications and gaining access to the personalized online tool – provoke disparate behavioral responses. At the same time, we observe substantial differences with respect to treatment take-up, which may affect their relative assessment. While about 37% of treated individuals read the message that provides general information about the work requirement, only 10% actually click on the link directing them to the personalized online tool. Therefore, we also estimate treatment effects on the treated of reading the message, respectively of clicking on the link to the tool, which allows us to quantify treatment effects on the group of compliers, who actually utilize the information provided by our intervention.

As shown in specification (1) of Table 4, only reading the message that contains general information about the requirement and risk of being sanctioned significantly reduces working hours and earnings by about 15% relative to the control group who receives none of the additional information ($p < 0.05$). Hence, raising benefit recipients' awareness of the requirement without providing accurate information about their personal risk of

¹⁶Figure A.3 in Appendix A.2 shows the distribution of the total working hours within 12 months after the intervention. It can be seen that there is a slightly higher density close to the cutoff for treated individuals who have access to the online tool compared the control group who only receives reminder messages.

¹⁷We choose the values of the first, second and third quartile of the corresponding distribution of the cumulated working hours and the requirement of 225 hours as the margins of interest.

Table 4: Treatment effects on treated on employment and earnings

	Mean value control group II (no general notification)	Treatment effects on treated	
		Opening general notification (1)	Accessing personalized online tool (2)
First-stage: treatment take-up		0.366 [0.000]	0.096 [0.000]
Dependent variable (within 12 months after start of intervention):			
Total no. of working hours	153.1	-22.55 [0.025]	104.75 [0.007]
Total labor earnings in DKK	23,676	-3,626 [0.026]	14,577 [0.018]
F-statistic for weak identification		10,338.66	1,765.44
No. of observations		31,533	31,525
Control variables		Yes	Yes

Note: Depicted are treatment effects on treated of (i) opening the message (relative to the second control group who received none of the additional information) and (ii) accessing the online tool through the link in the message (relative to the first control group who only received general reminder messages). P -values are shown in square brackets. Control variables are depicted in Table 1.

being sanctioned impairs their labor market performance. This suggests that they may feel more pressure to accept low-quality jobs as the threat of a sanction becomes more salient.

However, the negative effects are relatively moderate compared to the positive effects of accessing the personalized online tool, which are 4-5 times larger (see specification 2). This indicates that providing personalized information and reducing uncertainty is even more beneficial for the labor market performance than indicated by the ITTs when considering individuals who actually respond to the intervention in the designated way. For those who comply with the treatment and access the tool, relaxing information constraints regarding their personal risk of falling short of target leads to substantially improved labor market outcomes even in comparison to the opaque environment where benefit recipients may not be aware of the requirement.

Potential challenges may arise from the low responsiveness of welfare benefit recipients. Although we sent out up to six monthly reminders, only about 10% of the corresponding treatment group accessed the online tool through the provided link. It is important to consider this information for two reasons. First, it seems crucial to search for ways of communication that reach a larger share of the target population. Second, one could expect that individuals who actually react to the information treatment and utilize the

online tool are those who benefit the most from the information presented in the tool. Hence, one may expect that the estimated treatment effects on the treated represent an upper bound for the average treatment effect, while the lower bound is given by the corresponding ITTs.

4.3 Sorting Effects

As discussed in Section 3.3, our intervention may also trigger behavioral responses among benefit recipients who do not return to work. Table 5 shows ITTs on different outcome variables that are related to these possible sorting effects. Interestingly, we find a striking discrepancy between treatment effects on outflows from the welfare system and inflows into paid employment. For instance, when considering the total effect of the personalized tool and the general notification, we find that – despite its zero effect on job finding rates – the treatment increases the likelihood of leaving the welfare system by about 5% relative to the control group receiving none of the additional information ($p < 0.001$).

Table 5: Sorting effects

	(1) Partial effect of personalized information		(2) Total effect of personalized and general information	
	Treated - control I		Treated - control II	
	Intention- to-treat effect	Mean value control I	Intention- to-treat effect	Mean value control II
Dependent variable (within 12 months after start of intervention):				
Starting paid employment	0.008 [0.057]	0.236	0.003 [0.416]	0.244
Leaving welfare system	0.011 [0.028]	0.340	0.018 [0.000]	0.334
Entering other transfer program	0.009 [0.059]	0.218	0.014 [0.003]	0.213
Starting post-compulsory education	0.003 [0.231]	0.054	0.007 [0.006]	0.050
No. of observation	31,525		31,530	
Control variables	Yes		Yes	

Note: Depicted are intention-to-treat effect of being assigned to the treatment group in comparison to each of the two control groups (with and without receiving the general message). P -values are shown in square brackets. Dependent variables refer to indicators whether the corresponding event takes place within 12 months after the start of the intervention. Control variables are depicted in Table 1.

(1) Partial effect: compares the treatment group who receives general reminder message and has access to the online tool to control group I who only receives general reminder messages.

(2) Total effect: compares the treatment group who receives general reminder message and has access to the online tool to control group II who receives none of the additional information.

An explanation for this discrepancy could be that treated individuals switch to other transfer programs, which are not subject to the requirement and the sanction regime. To test whether treated individuals indeed avoid the work requirement by claiming benefits that are not affected by the work requirement, we consider an indicator of whether the individual receives any other benefits (such as educational support, sickness or disability benefits) as an additional outcome variable. The total effect of the personalized tool and the general message is such that it increases the likelihood of receiving other benefits by about 6.5% relative to the control group ($p = 0.003$), while about 60% of the effect can be attributed to the personalized information (see specification 1).¹⁸ Hence, increasing the transparency of the welfare system and informing benefit recipients about the risk of being sanctioned stimulates exits from the welfare system also among individuals not returning to work and triggers sorting into other transfer programs.

Finally, we also consider an indicator of whether the individual enters post-compulsory education after the start of the intervention as an additional outcome variable. This is motivated by the fact that receiving income support related to further education turns out to be an important factor when analyzing sorting effects. We find that the treatment indeed increases enrollment rates by about 14% ($p = 0.006$) relative to the control group receiving none of the additional information (see specification 2). This implies that the intervention not only encourages benefit recipients to sign up for the designated financial support program, but also to start post-compulsory education, which possibly promotes the accumulation of human capital.

An interesting remark can be made when comparing the effects of personalized and general information. While we find opposite effects of the two treatment elements on employment and earnings (see Section 4.2), personalized and general information turn out to affect the behavior of individuals who do not start paid employment in a similar way. Therefore, the total effect of having access to the personalized online tool and receiving general notification exceeds the partial effect of providing personalized information.

This pattern is in line with the idea that the intervention operates by affecting individuals' perceived risk of experiencing a benefit sanction. Receiving general information about the existence of the work requirement and the possible threat of an income re-

¹⁸We also estimate separate effects on different benefit categories. It turns out that the overall treatment effect on the receipt of other benefits is mainly driven by an increased take-up of disability benefits (+10%) and educational support (+12%). Results are available upon request.

duction should increase the perceived sanction risk and therefore the pressure to leave welfare independently of the individual's personal situation. Conversely, the effect of the personalized treatment should depend on the benefit recipient's objective risk of being sanctioned. When the objective risk is high, personalized information is likely to further increase the perceived risk of a benefit reduction, while the opposite is true for individuals who actually face a low objective sanction risk.

4.4 Heterogeneous Treatment Effects

In a last step of our analysis, we examine heterogeneous treatment effects with respect to the (i) benefit recipients' labor market attachment and (ii) regional differences regarding the intensity of caseworker counseling.

Job search status: First, we test more explicitly to what extent the behavioral response to the information depends on the benefit recipient's personal situation. Those who are closely attached to the labor market and collect many working hours even in the absence of the intervention have a relatively low personal risk of falling short of target. In that case, providing personalized information reduces the pressure to accept non-regular jobs. Conversely, benefit recipients with limited labor market attachment face a high objective sanction risk, which becomes more salient when having access to the online tool. It appears plausible that they may only avoid a reduction of their benefit level by leaving welfare benefits and relying on other sources of income without returning to work.

Therefore, we approximate the individual's job search status prior to the intervention by taking into account the number of collected working hours at the onset of the intervention and the number of registered job applications in the online portal of the public employment service within the last four weeks.¹⁹ Specifically, we assume that a positive number of collected working hours or at least one registered job application identifies active job seekers. This allows us to investigate whether the intervention leads active job seekers to change their search behavior or whether it encourages inactive persons to start searching for a job.

¹⁹It should be noted that welfare benefit recipients are not legally required to document their job search activities, which makes it difficult to draw conclusions about the individual's job search effort based on registered job applications. Nevertheless, it appears plausible that individuals who register job applications are actively searching for a job.

Panel A of Table 6 shows the estimation results. While about 75% of benefit recipients are inactive according to our definition providing accurate personalized information mainly promotes the labor market integration of the small group of active job seekers (see Panel A.1). We find that the treatment effects on employment and earnings are 4-5 times larger among active job seekers compared to inactive persons and treatment effects among the two groups are significantly different from each other. Conversely, the treatment stimulates sorting into other transfer programs only among benefit recipients who are categorized as inactive.

Finally, it should be noted that there is little heterogeneity when considering the total effect of having access to the online tool and receiving general notifications (see Panel A.2). This is not surprising since in particular the provision of personalized information should affect individuals with a high and low objective risk of experiencing a benefit reduction differently.

Regional counseling intensity: Next, we investigate the interaction between our intervention and caseworkers' counseling activities. Therefore, we exploit the fact that there are regional differences in how frequently benefit recipients meet their caseworker. In particular, we estimate separate effects for individuals living in municipalities with a counseling intensity – defined as the average number of caseworker meetings within a municipality in the last six months before the start of the intervention – above and below the median.²⁰

The results presented in Panel B of Figure 6 show that our information treatment has a larger positive effect on labor market outcomes in regions where individuals receive more support from their caseworker. The partial effect of personalized information in regions with a high counseling intensity is twice as large compared to regions with a low intensity (see Panel B.1). Moreover, the difference is even more pronounced when considering the total effect of personalized and general information compared to the control group who faces the “business-as-usual” environment (see Panel B.2). We find a positive and significant effect on total working hours in regions with a high counseling intensity

²⁰One could be concerned that the number of caseworker meetings in a given municipality reflects the composition of benefit recipients. In Table A.2 in Appendix A.2, we re-estimate the heterogeneous effects considering only the residual variation of the meeting intensity after conditioning on observable characteristics. We obtain a very similar pattern when dividing the sample based on this alternative measure, which is less likely to be affected by the composition of benefit recipients.

Table 6: Heterogeneous treatment effects

(1) Partial effect of personalized information: treated - control I						
A.1 Job search status			B.1 Local counseling intensity			
Inactive (1)	Active (2)	Difference (2) - (1)	Low (3)	High (4)	Difference (4) - (3)	
Dependent variable (within 12 months after start of intervention):						
Starting paid employment	0.004 [0.320]	0.022 [0.036]	0.018 [0.054]	0.009 [0.133]	0.006 [0.265]	-0.002 [0.754]
Total no. of working hours	5.93 [0.081]	22.85 [0.031]	16.92 [0.046]	6.60 [0.134]	13.35 [0.013]	6.75 [0.314]
Total labor earnings in DKK	708 [0.183]	3,502 [0.044]	2,794 [0.040]	971 [0.220]	1,812 [0.030]	840 [0.453]
Entering other transfer program	0.011 [0.042]	0.001 [0.907]	-0.010 [0.356]	0.018 [0.007]	-0.001 [0.885]	-0.019 [0.037]
No. of observations	23,640	7,915		15,840	15,654	
Control variables	Yes	Yes		Yes	Yes	
(2) Total effect of personalized and general information: treated - control II						
A.2 Job search status			B.2 Local counseling intensity			
Inactive (1)	Active (2)	Difference (2) - (1)	Low (3)	High (4)	Difference (4) - (3)	
Dependent variable (within 12 months after start of intervention):						
Starting paid employment	0.007 [0.090]	-0.005 [0.632]	-0.012 [0.200]	0.003 [0.625]	0.003 [0.627]	-0.001 [0.934]
Total no. of working hours	4.42 [0.202]	-4.34 [0.690]	-8.76 [0.313]	-7.20 [0.245]	10.61 [0.036]	17.81 [0.025]
Total labor earnings in DKK	569 [0.292]	-1,215 [0.498]	-1,784 [0.201]	-993 [0.283]	1,161 [0.117]	2,154 [0.067]
Entering other transfer program	0.012 [0.021]	0.016 [0.079]	0.003 [0.751]	0.018 [0.004]	0.009 [0.118]	-0.009 [0.287]
No. of observations	23,539	7,991		15,778	15,718	
Control variables	Yes	Yes		Yes	Yes	

Note: Depicted are intention-to-treat effects of being assigned to the treatment group in comparison to each of the two control groups (with and without receiving the general message). P -values are shown in square brackets. Control variables are depicted in Table 1.

Panel A shows separate effects for inactive (column 1) and active job seekers (column 2). Active job seekers have collected a positive number of working hours at the onset of the intervention or registered job applications in the online registration portal of the public employment service within the last four weeks before the intervention.

Panel B shows separate effects for benefit recipients living in municipalities with a low (column 1) and high (column 2) meeting capacity. Low/high meeting capacity refers to the average number of meetings within a municipality in the last six months before the intervention below/above the sample median.

- (1) Partial effect: compares the treatment group (general reminder message and access to the online tool) to control group I (general reminder message only).
(2) Total effect: compares the treatment group (general reminder message and access to the online tool) to control group II (none of the additional information).

($p = 0.036$), which is also significantly different from the negative effect in regions with a low counseling intensity ($p = 0.025$). These findings suggest that relaxing informational constraints about the benefit recipients' personal situation and the existence of the work requirement in particular improves the individual labor market performance when it is accompanied with personal caseworker counseling. Interestingly, the opposite is true for the effect on sorting into other transfer programs, which is driven by benefit recipients with limited support by their caseworker. This is not surprising given that counseling activities typically aim to promote reemployment. At the same time, inactive persons who interact less with their caseworker might be more likely to underestimate their sanction risk and therefore the intervention has a larger effect on sorting into other transfer programs in regions with a low counseling intensity.

Summary: Altogether, our findings suggest that relaxing personal informational constraints can have disparate effects on unemployed workers as a reflection of (i) how likely (or how costly) it is to fulfill the work requirement and (ii) the intensity at which they are supported by their caseworker. Among active job seekers, accurate information about their personal risk of falling short of target mitigates the pressure to accept low quality jobs. This effect could reflect that the personalized treatment reduces (i) the perceived risk of being sanctioned or (ii) uncertainty about the future benefit level. Moreover, it is reinforced when job seekers receive more intensive support from caseworkers.

For benefit recipients who are inactive and have a high personal risk of experiencing a benefit cut, the provision of personalized information about the requirement makes it disproportionately unattractive to further receive welfare benefits. It appears unlikely that such a sorting effect is the consequence of reduced uncertainty, while the stronger effect on individuals with little caseworker support is in line with the notion that the treatment makes the threat of a benefit sanction more severe. While this reflects the traditional notion of the screening effect highlighted by, e.g., Besley and Coate (1992), the online tool could also lead to the perception of being monitored, which might create greater discomfort among inactive persons who are unlikely to fulfill the requirement.

5 Conclusion

In this paper, we studied how the transparency of the welfare system affects the labor market outcomes of unemployed workers. The results of a large-scale randomized field experiment show that the provision of personalized information about the individual risk of experiencing a reduction of benefit payments has first-order effects on the labor market integration of benefit recipients who face a requirement to work a minimum number of hours. Relaxing personal information frictions appears to improve the decision-making of disadvantaged unemployed workers. Among benefit recipients who are actively searching for a job, our personalized intervention mitigates the pressure to accept non-regular jobs and promotes more stable full-time employment, which improves overall employment and earnings.

This is a key result as it shows that informational constraints regarding the personal incentive structure can have adverse effects on the welfare of unemployed workers. Digital tools, which offer novel possibilities to provide personalized information in real-time at low marginal costs, can effectively reduce informational constraints. Hence, investments into a digital infrastructure can be to the advantage of unemployed individuals who use these tools and therefore have the potential to improve overall welfare, especially when digital solutions are accompanied by personal caseworker counseling. Given that complex incentive schemes are widespread and often serve important objectives, e.g., by minimizing moral hazard problems and improving targeting efficiency (see e.g. Kleven and Kopczuk, 2011), our findings provide important implications beyond the specific context.

Moreover, we also find that our intervention encourages inactive persons to leave the welfare system and to enter other transfer programs that are designed to support unemployed workers with health problems or a lack of education. The disparate effects on inactive and actively-searching individuals provide empirical support for the theoretically appealing idea of using requirements as a screening device such that unemployed individuals with limited chances to find employment switch into other transfer programs that are designed to support unemployed workers who face difficulties to find a job due to health problems or a lack of education. We show that such a sorting mechanism can be triggered by simply informing benefit recipients about their risk of experiencing a reduction of their benefit level. Self-sorting could improve targeting of benefit payments and may allow policy-makers to tailor governmental policies more efficiently. In our setting,

the availability of alternative sources of income support mitigates potential adverse effects of the sorting mechanism and the promotion of further education resulting in the accumulation of human capital turned out to be a favorable side effect. However, one could be worried that inactive persons who leave the welfare system face an increased risk of poverty in systems with less comprehensive transfer programs.

References

- ABBRING, J. H., G. J. VAN DEN BERG, AND J. C. VAN OURS (2005): “The effect of unemployment insurance sanctions on the transition rate from unemployment to employment,” *The Economic Journal*, 115, 602–630.
- AKERLOF, G. A. (1978): “The economics of “tagging” as applied to the optimal income tax, welfare programs, and manpower planning,” *American Economic Review*, 68, 8–19.
- ALATAS, V., R. PURNAMASARI, M. WAI-POI, A. BANERJEE, B. A. OLKEN, AND R. HANNA (2016): “Self-targeting: Evidence from a field experiment in Indonesia,” *Journal of Political Economy*, 124, 371–427.
- ALTMANN, S., A. FALK, S. JÄGER, AND F. ZIMMERMANN (2018): “Learning about job search: A field experiment with job seekers in Germany,” *Journal of Public Economics*, 164, 33–49.
- ANGRIST, J. D., G. W. IMBENS, AND D. B. RUBIN (1996): “Identification of causal effects using instrumental variables,” *Journal of the American Statistical Association*, 91, 444–455.
- ARNI, P., R. LALIVE, AND J. C. VAN OURS (2013): “How effective are unemployment benefit sanctions? Looking beyond unemployment exit,” *Journal of Applied Econometrics*, 28, 1153–1178.
- ARNI, P. AND A. SCHIPROWSKI (2019): “Job search requirements, effort provision and labor market outcomes,” *Journal of Public Economics*, 169, 65–88.
- BARGAIN, O. AND K. DOORLEY (2011): “Caught in the trap? Welfare’s disincentive and the labor supply of single men,” *Journal of Public Economics*, 95, 1096–1110.
- BEHNCKE, S., M. FRÖLICH, AND M. LECHNER (2010): “Unemployed and their case-workers: should they be friends or foes?” *Journal of the Royal Statistical Society: Series A (Statistics in Society)*, 173, 67–92.
- BELOT, M., P. KIRCHER, AND P. MULLER (2019): “Providing advice to jobseekers at low cost: An experimental study on online advice,” *Review of Economic Studies*, 86, 1411–1447.
- BENGHALEM, H., P. CAHUC, AND P. VILLEDIEU (2021): “The Lock-in Effects of Part-Time Unemployment Benefits,” IZA Discussion Papers No. 14189.
- BERG, N. AND T. GABEL (2015): “Did Canadian welfare reform work? The effects of new reform strategies on Social Assistance participation,” *Canadian Journal of Economics/Revue canadienne d’économique*, 48, 494–528.
- BESLEY, T. AND S. COATE (1992): “Workfare versus welfare: Incentive arguments for work requirements in poverty-alleviation programs,” *American Economic Review*, 82, 249–261.
- (1995): “The design of income maintenance programmes,” *Review of Economic Studies*, 62, 187–221.
- BHARGAVA, S. AND D. MANOLI (2015): “Psychological frictions and the incomplete take-up of social benefits: Evidence from an IRS field experiment,” *American Economic Review*, 105, 3489–3529.
- BLACK, D., J. SMITH, M. BERGER, AND B. NOEL (2003): “Is the threat of reemployment services more effective than the services themselves? Evidence from random assignment in the UI system,” *American Economic Review*, 94, 1313–1327.

- BLOOM, D. AND C. MICHALOPOULOS (2001): “How welfare and work policies affect employment and income: A synthesis of research.” .
- BOLLENS, J. AND B. COCKX (2017): “Effectiveness of a job vacancy referral scheme,” *IZA Journal of Labor Policy*, 6, 15.
- CALIENDO, F. N., A. GORRY, AND S. SLAVOV (2019): “The cost of uncertainty about the timing of social security reform,” *European Economic Review*, 118, 101–125.
- CALIENDO, M., S. KÜNN, AND A. UHLENDORFF (2016): “Earnings exemptions for unemployed workers: The relationship between marginal employment, unemployment duration and job quality,” *Labour Economics*, 42, 177–193.
- CARD, D., R. CHETTY, AND A. WEBER (2007): “The spike at benefit exhaustion: Leaving the unemployment system or starting a new job?” *American Economic Review*, 97, 113–118.
- CARD, D. AND P. B. LEVINE (2000): “Extended benefits and the duration of UI spells: evidence from the New Jersey extended benefit program,” *Journal of Public Economics*, 78, 107–138.
- CHETTY, R. AND E. SAEZ (2013): “Teaching the tax code: Earnings responses to an experiment with EITC recipients,” *American Economic Journal: Applied Economics*, 5, 1–31.
- CONLON, J. J., L. PILOSSOPH, M. WISWALL, AND B. ZAFAR (2018): “Labor market search with imperfect information and learning,” Nber working paper 24988.
- CUFF, K. (2000): “Optimality of workfare with heterogeneous preferences,” *Canadian Journal of Economics/Revue canadienne d’économique*, 33, 149–174.
- DUFLO, E., W. GALE, J. LIEBMAN, P. ORSZAG, AND E. SAEZ (2006): “Saving incentives for low-and middle-income families: Evidence from a field experiment with H&R Block,” *The Quarterly Journal of Economics*, 121, 1311–1346.
- ENGSTRÖM, P., P. HESSELIUS, AND B. HOLMLUND (2012): “Vacancy referrals, job search, and the duration of unemployment: a randomized experiment,” *Labour*, 26, 419–435.
- FINKELSTEIN, A. (2009): “E-ztax: Tax salience and tax rates,” *The Quarterly Journal of Economics*, 124, 969–1010.
- FONTAINE, F. AND A. KETTEMANN (2019): “Quasi-experimental Evidence on Take-up and the Value of Unemployment Insurance,” Manuscript, paris school of economics.
- FORTIN, B., G. LACROIX, AND S. DROLET (2004): “Welfare benefits and the duration of welfare spells: evidence from a natural experiment in Canada,” *Journal of Public Economics*, 88, 1495–1520.
- FOUGÈRE, D., J. PRADEL, AND M. ROGER (2009): “Does the public employment service affect search effort and outcomes?” *European Economic Review*, 53, 846–869.
- FREMIGACCI, F. AND A. TERRACOL (2013): “Subsidized temporary jobs: lock-in and stepping stone effects,” *Applied economics*, 45, 4719–4732.
- GEERDSSEN, L. (2006): “Is there a threat effect of labour market programmes? A study of ALMP in the Danish UI system,” *The Economic Journal*, 116, 738–750.
- GERFIN, M., M. LECHNER, AND H. STEIGER (2005): “Does subsidised temporary employment get the unemployed back to work? Aneconometric analysis of two different schemes,” *Labour Economics*, 12, 807–835.

- GRAVERSEN, B. AND J. VAN OURS (2008): “How to help unemployed find jobs quickly: Experimental evidence from a mandatory activation program,” *Journal of Public Economics*, 92, 2020–2035.
- GROGGER, J. AND L. A. KAROLY (2009): *Welfare reform*, Harvard University Press.
- KATZ, L. F. AND B. D. MEYER (1990): “The impact of the potential duration of unemployment benefits on the duration of unemployment,” *Journal of Public Economics*, 41, 45–72.
- KEELEY, M. C. AND P. K. ROBINS (1985): “Government programs, job search requirements, and the duration of unemployment,” *Journal of Labor Economics*, 3, 337–362.
- KLEVEN, H. J. AND W. KOPCZUK (2011): “Transfer program complexity and the take-up of social benefits,” *American Economic Journal: Economic Policy*, 3, 54–90.
- KREINER, C. T. AND T. TRANÆS (2005): “Optimal workfare with voluntary and involuntary unemployment,” *Scandinavian Journal of Economics*, 107, 459–474.
- KYYRÄ, T., P. PARROTTA, AND M. ROSHOLM (2013): “The effect of receiving supplementary UI benefits on unemployment duration,” *Labour Economics*, 21, 122–133.
- LALIVE, R., J. VAN OURS, AND J. ZWEIMÜLLER (2006): “How changes in financial incentives affect the duration of unemployment,” *Review of Economic Studies*, 73, 1009–1038.
- LALIVE, R., J. C. VAN OURS, AND J. ZWEIMÜLLER (2005): “The effect of benefit sanctions on the duration of unemployment,” *Journal of the European Economic Association*, 3, 1386–1417.
- LEMIEUX, T. AND K. MILLIGAN (2008): “Incentive effects of social assistance: A regression discontinuity approach,” *Journal of Econometrics*, 142, 807–828.
- LICHTER, A. (2016): “Benefit duration and Job search effort: Evidence from a Natural Experiment,” IZA Discussion Paper No. 10264.
- LIEBMAN, J. B. AND E. F. LUTTMER (2015): “Would people behave differently if they better understood social security? Evidence from a field experiment,” *American Economic Journal: Economic Policy*, 7, 275–99.
- LUTTMER, E. F. AND A. A. SAMWICK (2018): “The welfare cost of perceived policy uncertainty: evidence from social security,” *American Economic Review*, 108, 275–307.
- MARINESCU, I. (2017): “The general equilibrium impacts of unemployment insurance: Evidence from a large online job board,” *Journal of Public Economics*, 150, 14–29.
- MICHALOPOULOS, C., P. K. ROBINS, AND D. CARD (2005): “When financial work incentives pay for themselves: evidence from a randomized social experiment for welfare recipients,” *Journal of Public Economics*, 89, 5–29.
- MOFFITT, R. A. (2003): “The negative income tax and the evolution of US welfare policy,” *Journal of economic perspectives*, 17, 119–140.
- MUELLER, A. I., J. SPINNEWIJN, AND G. TOPA (2021): “Job seekers’ perceptions and employment prospects: Heterogeneity, duration dependence, and bias,” *American Economic Review*, 111, 324–63.
- NEKOEI, A. AND A. WEBER (2017): “Does extending unemployment benefits improve job quality?” *American Economic Review*, 107, 527–61.

- NICHOLS, A. L. AND R. J. ZECKHAUSER (1982): “Targeting transfers through restrictions on recipients,” *American Economic Review*, 72, 372–377.
- NICHOLS, D., E. SMOLENSKY, AND T. N. TIDEMAN (1971): “Discrimination by waiting time in merit goods,” *The American Economic Review*, 61, 312–323.
- PETRONGOLO, B. (2009): “The long-term effects of job search requirements: Evidence from the UK JSA reform,” *Journal of Public Economics*, 93, 1234–1253.
- ROSHOLM, M. (2014): “Do case workers help the unemployed?” *IZA World of Labor*.
- SCHIPROWSKI, A. (2020): “The role of caseworkers in unemployment insurance: Evidence from unplanned absences,” *Journal of Labor Economics*, 38, 1189–1225.
- SCHMIEDER, J. F., T. VON WACHTER, AND S. BENDER (2012): “The effects of extended unemployment insurance over the business cycle: Evidence from regression discontinuity estimates over 20 years,” *Quarterly Journal of Economics*, 127, 701–752.
- STEPHAN, G., G. VAN DEN BERG, AND P. HOMRIGHAUSEN (2016): “Randomizing information on a targeted wage support program for older workers: A field experiment,”
- SVARER, M. (2011): “The effect of sanctions on exit from unemployment: Evidence from Denmark,” *Economica*, 78, 751–778.
- VAN DEN BERG, G. J., B. HOFMANN, AND A. UHLENDORFF (2019): “Evaluating Vacancy Referrals and the Roles of Sanctions and Sickness Absence,” *The Economic Journal*, 129, 3292–3322.
- VAN DEN BERG, G. J., B. VAN DER KLAUW, AND J. C. VAN OURS (2004): “Punitive sanctions and the transition rate from welfare to work,” *Journal of Labor Economics*, 22, 211–241.
- VAN DEN BERG, G. J. AND J. VIKSTRÖM (2014): “Monitoring job offer decisions, punishments, exit to work, and job quality,” *Scandinavian Journal of Economics*, 116, 284–334.
- VENN, D. (2012): “Eligibility Criteria for Unemployment Benefits: Quantitative Indicators for OECD and EU Countries,” *OECD Social, Employment, and Migration Working Papers*.

A Appendix

A.1 Text of Treatment Messages

A) Message to treatment group (with link to online tool):

How to avoid losing your welfare benefits

Due to the 225-hours rule, you risk to incur a benefit reduction or to lose your benefits altogether. This will happen, if you have not worked at least 225 hours within the previous year. The rule applies, when you have received benefits for a total of 12 months within the last 3 years.

If you want to avoid losing or incurring a reduction of your benefits, you need to pay attention to how many hours you are missing to gather a total of 225 hours. A new tool on jobnet.dk can help you keep track of your working hours. The tool is called 'counter of hours' and is personal. The tool is regularly updated with your working hours.

Your 'counter of hours' gives you an overview of:

1. Hours you have worked that will be included in the count of 225 hours
2. Hours you are missing to reach 225 hours
3. Your deadline for gathering 225 hours

Check your 'hours counter' now. [LINK]

Check your working hours regularly, so you can plan how many hours to work a week in order to reach a minimum of 225 hours. Just a few hours of work per week can help you reach 225 hours and avoid a reduction of your benefits.

225 hours are equivalent to:

- 5 hours a week for 52 weeks
- 10 hours a week for 23 weeks
- 20 hours a week for 12 weeks
- 37 hours a week for 7 weeks

All of the hours you work today will keep counting for the full next year. Therefore it still pays off to gather working hours after you have worked for 225 hours.

When you log on to jobnet.dk to check your job adds, it is easy to keep track of your 'counter of hours'. You can find it on jobnet.dk under the menu item MY BENEFITS on the left side of the screen. Press the menu item '225-hours rule'.

There are currently 20.000 job adds posted on jobnet.dk. Start in good time to collect working hours so you do not risk losing money.

B) Message to control group I (without link to online tool)

How to avoid loosing your welfare benefits

Due to the 225-hours rule, you risk to incur a benefit reduction or to loose your benefits altogether. This will happen, if you have not worked at least 225 hours within the previous year. The rule applies, when you have received benefits for a total of 12 months within the last 3 years.

If you want to avoid loosing or incurring a reduction of your benefits, you need to pay attention to how many hours you are missing to gather a total of 225 hours.

Check your working hours regularly so you can plan how many hours to work a week in order to reach a minimum of 225 hours. Just a few hours of work per week can help you reach 225 hours and avoid a reduction of your benefits.

225 hours are equivalent to:

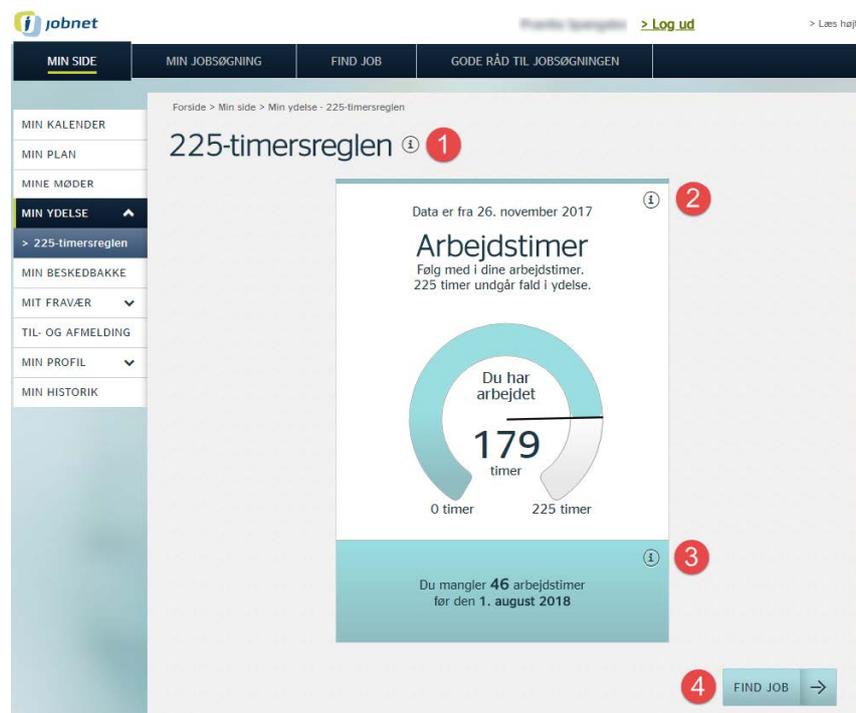
- 5 hours a week for 52 weeks
- 10 hours a week for 23 weeks
- 20 hours a week for 12 weeks
- 37 hours a week for 7 weeks

All the hours, you work today, will keep counting for the full next year. Therefore it still pays off to gather working hours after you have worked for 225 hours.

There are currently 20.000 job adds posted on jobnet.dk. Start in good time to collect working hours so you do not risk loosing money.

A.2 Additional Figures and Tables

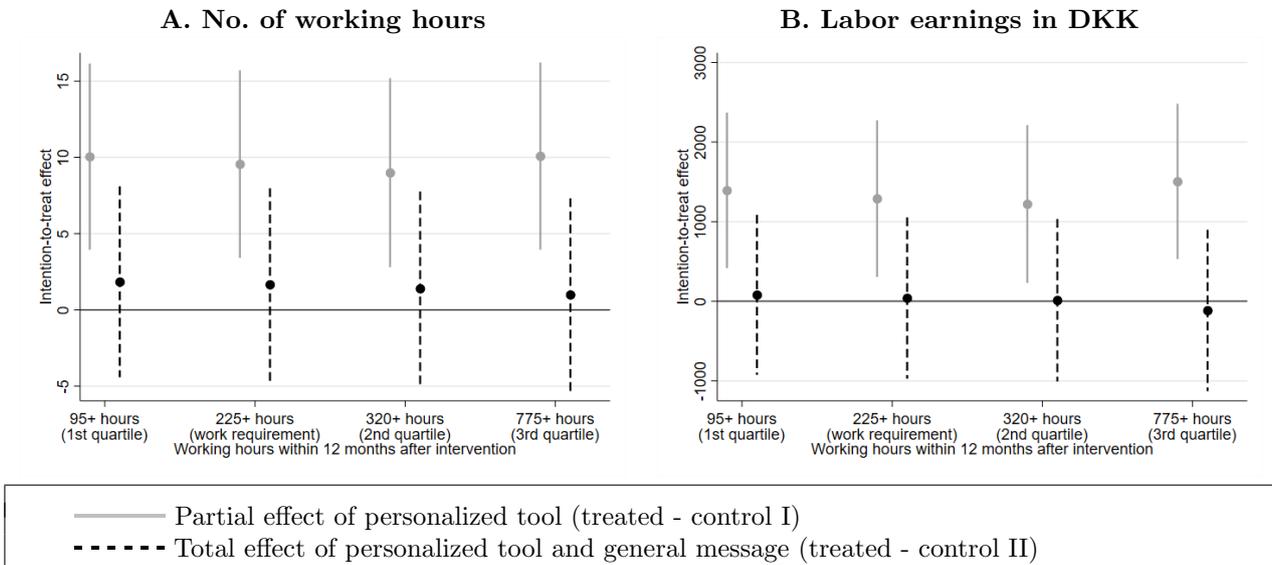
Figure A.1: The online tool



Note: Depicted is the online tool that provides personalized information about the welfare recipients own situation related to the requirement of working 225 hours within 12 months.

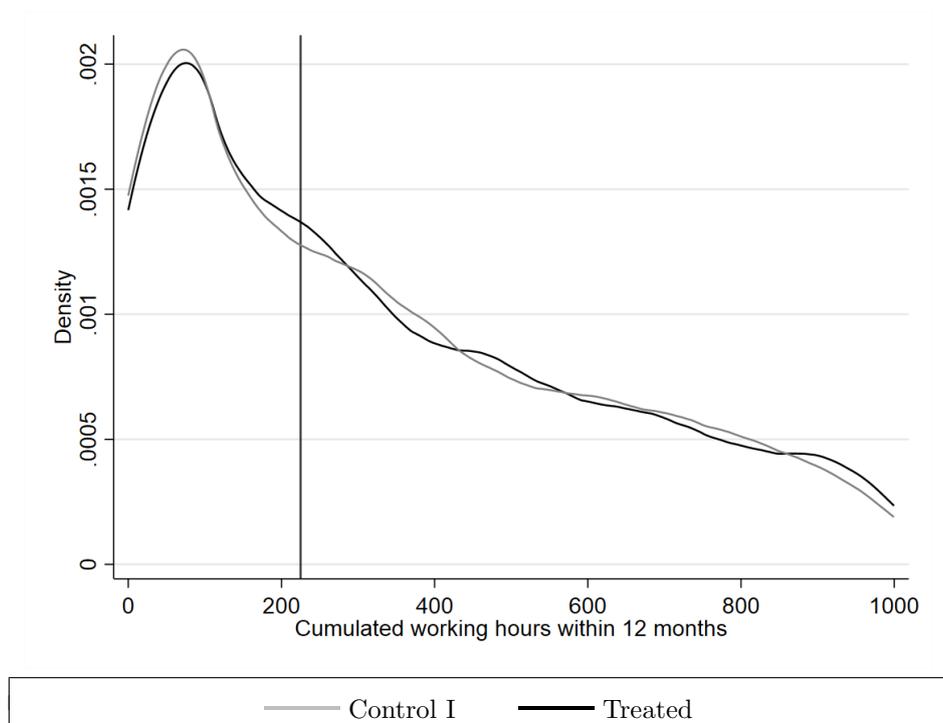
- (1) provides general information about work requirement.
- (2) explains number of collected working hours as of today.
- (3) informs about potential reduction date and the number of hours that is missing to comply with the work requirement.
- (4) link to online job search platform.

Figure A.2: Intention-to-treat effects for different margins of yearly working hours



Note: Depicted are intention-to-treat effects including 90% confidence intervals. The dependent variable relates to the total number of working hours, respectively labor earnings within 12 months after the intervention. Both outcome variables are set to zero if the individual worked less hours within 12 months than the corresponding threshold indicated on the x-axis.

Figure A.3: Distribution of working hours within 12 months by treatment status



Note: Depicted is the kernel density (epanechnikov kernel) of the total number of working hours within 12 months after the start of the intervention for the treatment group and control group I (receiving reminder messages, but having no access to the online tool). The vertical line indicates the work requirement of 225 hours per year.

Table A.1: Determinants of treatment take-up

	Dependent variable			
	A. Opening treatment message		B. Accessing online tool	
	Coef.	<i>P</i> -value	Coef.	<i>P</i> -value
Female	0.021	[0.004]	0.009	[0.052]
Married	0.031	[0.020]	-0.003	[0.768]
Education (ref.: less than high school)				
High school	0.030	[0.058]	0.004	[0.684]
Bachelor degree or equivalent	0.124	[0.000]	0.026	[0.014]
Master degree of equivalent	0.198	[0.000]	0.064	[0.000]
Age (ref. 16-25 years)				
26-35 years	0.212	[0.000]	0.063	[0.000]
36-45 years	0.301	[0.000]	0.098	[0.000]
46-55 years	0.292	[0.000]	0.118	[0.000]
56-65 years	0.286	[0.000]	0.136	[0.000]
Migration background				
1 st generation	-0.120	[0.000]	-0.042	[0.000]
2 nd generation	-0.074	[0.000]	-0.031	[0.011]
Living in capital region	-0.012	[0.099]	-0.002	[0.626]
Children (ref.: no children)				
One child	-0.009	[0.490]	-0.010	[0.225]
Two or more children	-0.026	[0.090]	-0.036	[0.000]
Household size (ref.: one person)				
Two persons	-0.010	[0.520]	-0.001	[0.882]
Three persons	-0.014	[0.455]	-0.003	[0.802]
Four or more persons	-0.025	[0.252]	0.006	[0.645]
Requires activation	-0.333	[0.000]	-0.128	[0.000]
Consecutive weeks on welfare: 26 or less	-0.047	[0.000]	-0.012	[0.053]
Pre-intervention outcome (in previous year)				
Any paid employment	0.073	[0.000]	0.085	[0.000]
Total weekly working hours ($\times 1,000$)	-0.008	[0.770]	-0.007	[0.693]
Labor earnings in 10,000DKK	0.006	[0.001]	0.002	[0.045]
Any benefit reduction	0.092	[0.000]	0.033	[0.000]
Exempted from requirement	0.002	[0.790]	-0.012	[0.017]
Constant	0.327	[0.000]	0.096	[0.000]
No. of observations	15,761		15,761	
Mean value dependent variable	0.366		0.096	
R^2 (adj.)	0.209		0.111	

Note: OLS estimation. The sample includes individuals assigned to the treatment group. The dependent variables refer to indicators whether the individual opened the treatment message (Panel A), respectively clicked on the link to the online tool (Panel B). *P*-values in square brackets.

Table A.2: Heterogeneous effects by residual variation of meeting capacity in municipality

	(1) Partial effect of personalized information: Treated - control I		
	Meeting capacity in municipality		
	Low (1)	High (2)	Difference (2) - (1)
Dependent variable (within 12 months after start of intervention):			
Starting paid employment	0.008 [0.144]	0.007 [0.232]	-0.001 [0.862]
Total no. of working hours	6.07 [0.146]	13.72 [0.010]	7.65 [0.239]
Total labor earnings in DKK	984 [0.193]	1,767 [0.031]	784 [0.464]
Entering other transfer program	0.019 [0.006]	-0.002 [0.772]	-0.020 [0.023]
No. of observations	15,752	15,742	
Control variables	Yes	Yes	
	(2) Total effect of personalized and general information Treated - control II		
	Meeting capacity in municipality		
	Low (1)	High (2)	Difference (2) - (1)
Dependent variable (within 12 months after start of intervention):			
Starting paid employment	0.001 [0.846]	0.005 [0.376]	0.003 [0.688]
Total no. of working hours	-7.38 [0.237]	10.41 [0.032]	17.79 [0.024]
Total labor earnings in DKK	-955 [0.308]	1,069 [0.133]	2,024 [0.085]
Entering other transfer program	0.019 [0.002]	0.008 [0.163]	-0.011 [0.196]
No. of observations	15,666	15,830	
Control variables	Yes	Yes	

Note: Depicted are intention-to-treat effect of being assigned to the treatment group in comparison to each of the two control groups (with and without receiving the general message) separated for benefit recipients living in municipalities with a low (column 1) and high (column 2) meeting capacity. Meeting capacity refers to the residual variation of the average number of meetings within a municipality in the last six months before the intervention conditional on observable characteristics. Low/high meeting capacity refers to the residual variation below/above the sample median. Control variables are depicted in Table 1.

(1) Partial effect: compares the treatment group who receives general reminder message and has access to the online tool to control group I who only receives general reminder messages.

(2) Total effect: compares the treatment group who receives general reminder message and has access to the online tool to control group II who receives none of the additional information.