

Screening, Deterrence, or Both? Work Requirements and the Labor Market Performance of Welfare Recipients

Sofie Cairo* Robert Mahlstedt†

Preliminary Version
This draft: April 23, 2021

Abstract

Providing a guaranteed minimum income to unemployed workers may create disincentives to work. Therefore, benefit payments are often associated with certain requirements while non-compliance leads to punitive sanctioning. In our large-scale field experiment, we investigate how the transparency of a requirement to work a minimum number of hours affects the labor market outcomes of welfare recipients. Our information treatment allows us to disentangle the effects of (i) general information about the existence of the requirement and (ii) personalized information about the individual's own situation. We find that the information treatment reinforces the deterring effect of the requirement and stimulates exits from the welfare system, respectively the acceptance of low-quality jobs. Moreover, we provide evidence that the requirement acts as a screening device making welfare less attractive to individuals not actively searching for employment.

Keywords: Welfare Benefits, Work Requirements, Sanctions, Job Search, Field Experiments

JEL codes: J68, D83, C93

*Copenhagen Business School; sc.si@cbs.dk

†University of Copenhagen, IZA Bonn and DFI; robert.mahlstedt@econ.ku.dk

1 Introduction

Social security systems, such as unemployment and welfare benefits, typically face a trade-off: They aim to guarantee a minimum income, which allows displaced workers to smooth their consumption and to improve subsequent job match quality, without distorting the incentives to search for new employment (see e.g. Hopenhayn and Nicolini, 1997; Acemoglu and Shimer, 2000; Chetty, 2008). This leads many developed countries to combine the provision of a minimum income with certain requirements, e.g. to apply for a minimum number of jobs, to participate in labor market programs or to engage in suitable work activities, while non-compliance leads to sanctioning, e.g. a reduction of the benefit level (see e.g. Venn, 2012, for an overview). Although imposing a requirement may theoretically reduce moral hazard¹, the effectiveness of such a policy clearly depends on how visible and transparent the requirement is to the unemployed worker.

In this paper, we study how the transparency of a requirement to work a minimum number of hours affects the labor market outcomes of Danish welfare recipients. As discussed by Besley and Coate (1992), the presence of such a requirement makes the welfare system less attractive, which should encourage individuals to adjust their behavior in a way that reduces the likelihood of depending on welfare payments. Moreover, the magnitude of this deterring effect is presumably larger for benefit recipients with a low willingness to actively search for employment. The latter implies that the requirement could serve as a screening device by encouraging inactive persons to leave welfare such that the system becomes self-targeting.

In general, individuals in the Danish welfare system have limited access to information about their own situation in relation to the requirement. Against this backdrop, we exogenously increase how visible and transparent the requirement is to the individual welfare recipient in a large-scale field experiment. In a first treatment arm (we refer to this as the partly-transparent treatment), we increase the transparency to some extent by sending out monthly messages reminding welfare recipients about the existence of the requirement and the risk of being sanctioned in case of non-compliance. In a second treat-

¹It has been argued theoretically by Kreiner and Tranæs (2005), Boone *et al.* (2007) or Pavoni and Violante (2007) that imposing a requirement in combination with a sanction regime can lead to welfare improvements relative to a system without any monitoring. These positive effects might be even more pronounced, when job seekers have hyperbolic time preferences as imposing individual requirements may limit harmful procrastination (see DellaVigna and Paserman, 2005; Paserman, 2008; Cockx *et al.*, 2014).

ment arm (the fully-transparent treatment), welfare recipients receive similar reminder messages, but additionally gain access to an online tool that provides personalized, up-to-date information about the number of working hours, which they have already collected, and the specific deadline for rule compliance. By providing full transparency about the individuals' own situation in relation to the requirement, this treatment arguably minimizes the degree of uncertainty regarding their personal risk of being sanctioned. We evaluate the effectiveness of both treatments against a control group receiving no further information.

We find that increasing the transparency of the requirement partly through reminder messages impairs individual labor market outcomes. Over the course of one year, the cumulated working hours and labor earnings of welfare recipients who only receive the messages, but have no access to the online tool, are reduced by 5-6% relative to the control group. Additionally gaining access to the online tool, however, counteracts these adverse effects such that those receiving the fully-transparent treatment have similar working hours and earnings as the control group. A further analysis indicates that the disparate effects of both treatments on cumulated labor market outcomes are provoked by differences regarding the type of jobs accepted by individuals in the two treatment groups. In comparison to the fully-transparent treatment, sending only general reminder messages leads to a lower number of monthly working hours, a shorter job duration and more job changes. This indicates that the greater uncertainty regarding the risk of being sanctioned encourages welfare recipients to accept jobs with a lower match quality.

Moreover, providing full transparency about the requirement stimulates exits from welfare, i.e. it reduces the time period spent receiving benefit payments by about 8% relative to the control group over the course of one year. While this is not a consequence of an increased job finding rate, we show that the treatment increases the usage of other types of public benefits, such as disability benefits, that are not affected by the requirement and encourages individuals to start further education. These effects are in line with the notion that the work requirement serves as a screening device by making welfare less attractive especially to individuals who are not actively searching for employment (see e.g. Besley and Coate, 1992; Cuff, 2000; Kreiner and Tranæs, 2005, for theoretical discussions).² Our results lend support to this idea since individuals who are also eligible

²Although the aim of welfare is to ensure a minimum income for workers suffering from employment constraints, individuals who are without a job because they prefer not to work are in reality often also

for alternative benefit schemes or consider to start further education may also have an increased tendency to not actively search for paid employment.

Finally, a subgroup analysis complements our findings. It reveals that the response to the information treatment is very different depending on the individual's job search activities before the start of the intervention. On the one hand, the partly-transparent treatment encourages active job seekers, who recently registered job applications in the public online registration portal, accept jobs with a lower match quality. On the other hand, welfare recipients who are inactive react to the fully-transparent treatment by leaving the welfare system and switching to other types of income support.

Our findings have important implications for the optimal design of social security systems. First of all, we provide novel evidence that the transparency of requirements plays an important role for the behavioral response of welfare recipients. This suggests that they face substantial information frictions regarding their own situation and 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).

Increasing the transparency of the requirement, however, does not create additional employment among welfare recipients. This suggests that the work requirement is not effective in restoring job search incentives. At the same time reducing the benefit level in case of non-compliance harms the individual's possibility to smooth consumption, which is typically seen as a socially beneficial and desirable feature of welfare systems (see e.g. Chetty, 2008). Nevertheless, by serving as a screening device, the requirement may allow policy-makers to target public policies towards individual needs and to improve the design of the welfare system for those actively searching for employment, while the promotion of further education is an additional favorable side effect.

Our study also contributes new insights to the existing literature evaluating comparable policy tools, such as job search requirements (Petrongolo, 2009; Manning, 2009; Lammers *et al.*, 2013; Arni and Schiprowski, 2019), benefit sanctions (Lalive *et al.*, 2005; Arni *et al.*, 2013) and mandatory participation in labor market programs (Black *et al.*,

eligible for welfare benefits. Chiu and Karni (1998) discuss how private information regarding individuals' preferences for non-working time may create an adverse selection problem in UI, or equivalently in welfare systems. With work requirements in place, the (perceived) utility of welfare benefits of those individuals who are not actively searching for a job may drop disproportionately such that the choice of exiting welfare becomes an optimal strategy (see e.g. Manning, 2009).

2003; Geerdsen, 2006; Graversen and Van Ours, 2008). In line with existing evidence, our information treatment stimulates exits from UI or welfare, while consequences for re-employment are often less clear cut (see e.g. Meyer, 1995). Our setting, however, allows us to contribute new insights along various important dimensions.

First, we focus on a requirement to work a minimum number of hours, which seems more difficult to fulfill than a requirement to participate in a certain pre-defined program or to document search activities.³ Second, the large-scale field experiment allows us to exploit truly exogenous variation with respect to people’s specific knowledge about the work requirement. Hence, our setting overcomes two major caveats of the existing literature. While there is a strand of non-experimental studies estimating the effect of search requirements or sanctions relying on relatively strong identifying assumptions⁴, existing experimental evidence is scarce and originates from settings that do not distinguish between job search advice and the stricter enforcement of benefit rules (see Ashenfelter *et al.*, 2005, for an overview).⁵ Finally, in contrast to the vast majority of papers, we focus on welfare rather than UI benefit recipients (see Van den Berg *et al.*, 2004; Van der Klaauw and Van Ours, 2013, for two exceptions). This is a group of unemployed workers with substantial heterogeneity regarding their attachment to the labor market. Our findings suggest that this heterogeneity explains some of the disparate effects of tighter benefit rules, especially on job seekers’ reemployment prospects, found by previous studies. While active job seekers tend to accept jobs with a lower match quality, individuals with limited labor market attachment seem to leave the system without starting paid employment.

³Welfare recipients subject to the requirement are not eligible for extra job search assistance.

⁴For instance, various studies exploit the so-called timing-of-events approach to identify the effect of benefit sanctions or warnings (see e.g. Abbring *et al.*, 2005; Van den Berg *et al.*, 2004; Lalive *et al.*, 2005; Svarer, 2011), which requires that individuals do not anticipate, when a sanction, respectively a warning is imposed (Abbring and Van den Berg, 2003). Other papers have used inter-temporal (McVicar, 2008; Petrongolo, 2009; Manning, 2009) or caseworker (Arni and Schiprowski, 2019) variation regarding job search requirements to identify the corresponding effects.

⁵Field experiments among unemployed workers featuring elements of both job search assistance and verification of search activities have been conducted in the US (see e.g. Johnson and Klepinger, 1994; Klepinger *et al.*, 2002; Lachowska *et al.*, 2016), the Netherlands (see e.g. Van den Berg and Van der Klaauw, 2006) and Denmark (Graversen and Van Ours, 2008). In a laboratory experiment, Boone *et al.* (2009) show that the threat of getting a sanction has a substantially larger effect on outflow from unemployment than the exposure to a sanction.

2 Work Requirements and their Possible Effects

Before presenting the experimental design, we present the relevant institutional rules to clarify the concept of work requirements and outline how their presence might possibly affect the behavior and subsequent labor market outcomes of unemployed individuals.

2.1 Institutional Setting

Welfare benefits provide a safety net for unemployed workers 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 ($\approx 1,680\text{\$}$, 2020-level) per month, while the amount increases to 15,355DKK ($\approx 2,230\text{\$}$, 2020-level) when children live in the household. Welfare 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 is corrected to ensure that the gross household income does not exceed two times the benefit level of the individual recipient.

There is no limit regarding the duration of welfare benefit receipt, but individuals are confronted with a work requirement. This applies to all individuals who have received welfare benefits for at least 12 months within the last three years. They are entitled to the full benefit level only if they have worked at least 225 hours in a non-subsidized job within the previous 12 months, which is equivalent to about six weeks of full-time work.

If the welfare recipient does not comply with the work requirement, a benefit sanction is imposed that reduces the individual's benefit level by approximately 500 to 1,000DKK per month, while 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 complies with the rule in a given month, they might be subject to a reduction in the following month. The first benefit sanction 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 onwards. 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 hours of employment before the sanction is removed.

Six months before the individual is potentially sanctioned, they receive an official warning from the municipality, which is responsible for the benefit payments (we also refer to this as the “default warning” in the following). All welfare recipients who stand to be sanctioned if they do not work additional hours during the next six months receive such a warning letter. It informs them about their potential reduction date and the number of working hours they are missing to prevent the sanction. Those who are still at risk of being sanctioned receive another warning letter one month before a potential reduction of their benefit payments. Apart from these two official warnings sent out per default, welfare recipients have only limited access to direct information about their own situation, i.e. the number of working hours they have already collected. Individuals can arrange a meeting with their caseworker, who can then contact a corresponding database to receive information about the number of collected working hours. Alternatively, benefit recipients can keep count of the 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.

Finally, it should be noted that the work requirement applies to welfare recipients deemed capable of working a minimum of five hours per week. Exemptions from the rule are granted to welfare recipients at the discretion of their caseworker, typically to individuals with very limited ability to work due to either mental or physical constraints.

2.2 The Incentive Effects of Work Requirements

To understand how the increased transparency of the requirement as a consequence of our information treatment may affect the individual behavior it is crucial to clarify how the presence of the requirement affects individuals’ incentives. As highlighted by Besley and Coate (1992), two incentive arguments can be made in favor of work requirements in association with welfare programs.

First, they may have a *detering effect* encouraging individuals to adjust their behavior in a way that reduces the likelihood of depending on welfare payments. The presence of a requirement, which leads to a reduction of the benefit payments in case of non-compliance, reduces the (perceived) value of remaining in the welfare system and provides incentives to search more intensively for new employment. Similar effects have been empirically documented for various comparable policies. For instance, tighter job search requirements

(Petrongolo, 2009; Arni and Schiprowski, 2019) and sanction regimes (Abbring *et al.*, 2005; Van den Berg *et al.*, 2004; Lalive *et al.*, 2005; Svarer, 2011) typically increase outflows from registered unemployment.⁶ Moreover, mandatory requirements to participate in certain labor market programs can increase exit rates from unemployment (see e.g. Black *et al.*, 2003; Geerdsen, 2006; Graversen and Van Ours, 2008). The consequences for cumulated earnings and overall employment, however, remain theoretically ambiguous. Stricter benefit rules, respectively the presence of a work requirement should encourage welfare recipients to increase their search effort and lower their reservation wages.⁷ On the one hand, this could increase overall employment and consequently income (see e.g. Michalopoulos *et al.*, 2005), but, on the other hand, it may reduce accepted wages and lower the job match quality (see also Arni *et al.*, 2013; Van den Berg and Vikström, 2014; Nekoei and Weber, 2017, who document similar mechanisms).

It should be noted that the deterring effect of the work requirement, which makes welfare less attractive, may not only affect job search activities, but could also trigger behavioral adjustments in other dimensions such as educational decisions or the accumulation of human capital (see e.g. Mukoyama and Şahin, 2006, for a related theoretical model). Moreover, there could be substantial heterogeneity regarding the way in which individuals react to the presence of a work requirement. This implies that it may not only have a deterring effect, but could serve as a *screening device* which increases the scope of policy makers for targeting governmental policies (see also Besley and Coate, 1995).⁸ This reflects the idea that welfare systems could become self-targeting if the individual's response to the work requirement depends on (unobserved) factors that determine also the eligibility for benefits. For instance, Kreiner and Tranæs (2005) argue that work requirements may pave the way for distinguishing between voluntary and non-voluntary unemployment. While the aim of welfare is to ensure a minimum income for individuals who are unemployed due to employment constraints, those who prefer not to work are

⁶Relatedly, a large literature has shown that a more generous UI benefit system reduces individuals' search effort (Lichter, 2016; Marinescu, 2017) and prolongs the unemployment duration (Katz and Meyer, 1990; Card and Levine, 2000; Lalive *et al.*, 2006; Schmieder *et al.*, 2012). Similar effects have been found by studies analyzing the effect of the potential welfare duration (Fortin *et al.*, 2004) or the level of benefit payments (Lemieux and Milligan, 2008; Bargain and Doorley, 2011) among welfare recipients.

⁷See, 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.

⁸This builds on the seminal ideas of Akerlof (1978) and Nichols and Zeckhauser (1982) who argued that welfare could be improved if individuals who, e.g., have extreme difficulties in becoming employed could be identified and income transfers could be targeted accordingly.

in reality often also eligible for benefit payments. Imposing a work requirement, however, should create a larger disutility for individuals who are voluntarily unemployed due to their higher preferences for non-working time.⁹ If this is the case, the requirement would predominantly stimulate exits from the welfare system among unemployed individuals who are not actively searching for a job.

However, besides preferences for non-working time, potentially unobserved skills that directly affect the individuals' productivity may be important as well. If there is heterogeneity regarding individuals' ability to work a minimum number of hours, the costs of fulfilling the requirement would be higher for unemployed workers who suffer from employment constraints due to their lack of ability (see Moffitt, 2006). In such a situation, the requirement may disproportionately discourage low-skilled individuals who would otherwise actively search for employment. Such a discouragement effect could imply that unemployed workers suffering from employment constraints leave welfare as well.

3 Design of the Experiment

Our randomized field experiment targets welfare recipients in the Danish labor market. These individuals are confronted with a work requirement, while non-compliance is sanctioned with a reduction of their benefit level. The information intervention targets the individuals' awareness of the general rules and information frictions regarding their personal situation in relation to the rules. We combine the experimental data with detailed administrative records to measure the causal effect on labor market outcomes. In what follows, we outline the relevant institutional, experimental and empirical details of our setting.

3.1 Randomized Controlled Trial

Our experiment builds on the fact that individual welfare recipients face a substantial degree of uncertainty regarding their personal risk of receiving a benefit sanction. The information intervention targets a group of welfare recipients who stand to incur a benefit reduction within the next six months. Hence, they have already received their first official

⁹Factors that induce voluntary unemployment are typically the presence of alternative income sources or the engagement in other non-work activities, which should also decrease the marginal utility of work.

default warning, but a sanction has not yet been imposed. They may need to work additional hours in order to avoid a benefit sanction.

To investigate how the provision of information about the general work requirement and their personal situation causally affects the individual's labor market performance, we divide the sample into three treatment arms. First, the control group receives no additional information beyond the official warning letters sent out by the municipality.

Tool treatment. Second, individuals in the *tool treatment* receive up to six (additional) monthly reminders about the requirement that they may still incur a benefit reduction. The messages contain general information about the requirement of working 225 hours per year and provide some examples of how to fulfill the criteria. They emphasize the risk of receiving reduced welfare benefits and the importance of keeping an overview of collected working hours. Hence, the treatment makes the requirement and the threat of benefit sanctions more salient. Moreover, the treatment messages also include a link to an online tool that provides treated individuals with personalized information about their collected working hours. The tool displays the next potential reduction date and graphically illustrates how many hours the individual has already worked within the 12-month window prior to their reduction date (see Figure A.1 in Appendix A.2). Moreover, since only the last 12 months are relevant for the work requirement, the tool also shows the next date (and the corresponding number of hours), when collected working hours will forfeit.¹⁰ Finally, the tool also provides more detailed general information about the 225-hours rule, which are accessible by pressing information buttons on the tool-webpage.

Message treatment. Third, individuals in the *message treatment* also receive up to six (additional) monthly reminders that they may still incur a benefit reduction. Similar to the tool treatment, the messages contain general information about the requirement of working 225 hours per year and provide some examples of how to fulfill the criteria. However, individuals in this third treatment arm do not have access to the online information

¹⁰It should be noted that the online tool does not take into account the dynamic nature of the 225-hours rule. Due to the fact that the work requirement has to be fulfilled at each point in time (when considering the past 12 months), collected hours will continuously forfeit. Therefore, the individual might still be at risk of being sanctioned in the following month even after fulfilling the requirement in a given month, while the tool only displays information related to the next potential reduction date.

tool.¹¹ Hence, the treatment only informs the individual that there is a requirement and a risk of receiving a sanction, while it does not provide personalized information on how many hours the specific welfare recipient has to work in order to avoid the sanction.

Possible Behavioral Responses. The experimental design allows us to investigate various mechanisms. By informing welfare recipients about the presence of the work requirement and the risk of being sanctioned both treatments are expected to trigger the incentive effects described in Section 2. However, the two treatments differ regarding the degree to which they address potential information frictions. The tool treatment allows welfare recipients to follow their personal situation in relation to the requirement fairly accurately (i.e. with respect to the number of collected working hours and the deadline for rule compliance), while the message treatment leaves more uncertainty. The effect that this would have on individual incentives depends on how difficult it is to meet the requirement. If it is relatively easy to fulfill the work requirement, the deterring effect should be lower when welfare recipients have more information about their personal situation and vice versa. Furthermore, the strength of the screening effect depends on the degree of heterogeneity regarding the deterring effect of the requirement.

Besides affecting individual behavior in a similar way as the work requirement itself, both treatments may also reduce the probability of actually being sanctioned for two reasons. On the one hand, individuals who leave the welfare system are no longer subject to the requirement, which obviously reduces their risk of being sanctioned. On the other hand, the treatment also provides additional information allowing individuals to organize their working hours in a way that maximizes their individual utility. It is, however, a priori unclear whether this would increase overall employment as the binary incentive (associated with the 225-hours) might also give rise to gaming effects. Specifically, welfare recipients might want to exert just enough effort to fulfill the work requirement, which could encourage some of them to reduce their working hours, while others with collected hours below the cut-off would try to increase their working hours. Given that the online tool provides more accurate information, one could be concerned that gaming responses are particularly pronounced in the tool treatment.

¹¹During the experimental period, the online tool was only activated for individuals who are assigned to the tool treatment and there was no possibility to access the online tool for individuals assigned to the message and the control groups.

3.2 Procedural Details

All registered welfare recipients who belonged to the target group, which means they had already received their first default warning on August 15, 2018, were randomly assigned to one of the three treatment groups. Individuals in the message and the tool group 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 services (PES) to the individual's inbox at the official web portal of the Danish PES (*jobnet.dk*). The web portal also contained the online tool, activated only for individuals assigned to the tool treatment during the experiment.

3.3 Data and Sample Selection

To investigate the consequences of the intervention for subsequent labor market outcomes, we link the experimental data to comprehensive register data administered by the Danish Employment Agency and 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. We construct various outcome variables that allow us to identify potential effects of the intervention on labor market outcomes and benefit receipt. First, we consider monthly indicators of whether the individual received any welfare benefits, respectively any wage income for the first 12 months following the intervention. Moreover, we consider the cumulated number of working hours and labor earnings, as well as the number of months receiving welfare or other benefits within the first year after the beginning of the intervention. Finally, we exploit additional data collected by the employment agency, including information on imposed sanctions, granted exemptions and registered job applications. The latter are drawn from the official online platform of the PES, which is called *joblog*. Although welfare recipients are not legally required to document their job search activities, they are encouraged to do so as this information is typically used as the basis for individuals' meetings with their caseworkers (see Fluchtmann *et al.*, 2019).

From the full stock of welfare recipients on August 15, 2018, our intervention targets all individuals who have already received their first official default warning. Since this takes place six months before a potential sanction is imposed, the experimental population only

includes individuals who received welfare benefits for at least six months within the last three years. In total, our sample comprises 47,294 welfare recipients who are randomly assigned into the three treatment arms as described in Section 3.1.¹²

3.4 Descriptive Statistics

Table 1 summarizes descriptive statistics regarding socio-demographic and household information, as well as labor market histories separately for the three treatment groups in our estimation sample. 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 “requiring activation” (as opposed to “being ready for employment”), which implies that their caseworkers do not consider them capable of starting full time employment without further support. Moreover, the average monthly gross labor income in the year before the intervention was only about 1,800DKK (equivalent to approx. 235\$), and the average individual has already received welfare benefits for more than two years without interruption. 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% have been exempted from the work requirement at some point during this period.

[INSERT TABLE 1 ABOUT HERE]

To assess treatment take up, we consider individual-level click data. Around 36.6% of all welfare recipients in the message and tool treatment opened at least one of the messages that they received, while there is no difference with respect to the likelihood of reading the message between the two groups ($p = 0.987$). Moreover, 9.6% of the

¹²Overall, the work requirement applies to about 92,500 welfare recipients, while about 25% of all welfare recipients are exempted from the work requirement. Among the remaining 75% recipients about 12% are sanctioned in any given month.

individuals in the tool treatment clicked on the link to the online tool at least once within a year after the intervention. Table 2 shows the relationship between treatment compliance and individual background characteristics. It can be seen that women, native Danes and especially higher educated welfare recipients 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 welfare recipients who experienced a reduction of the benefit level in the past.

[INSERT TABLE 2 ABOUT HERE]

4 Empirical Analysis

In the following, we present the results of our empirical analysis. Before examining the immediate response of welfare recipients to the main treatment in Section 4.2 and their subsequent labor market outcomes in Section 4.3, we first present our estimation strategy. Afterwards we investigate the relevance of information frictions and further analyze effects on job characteristics, educational activities and take into account different income sources. Finally, we examine heterogeneous treatment effects.

4.1 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 D_i indicates the treatment status (dummy variables for both groups), 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. The model estimates the intention-to-treat effect (ITT), ignoring whether treated individuals actually read the message, respectively clicked on the link to the tool.

Our randomization procedure ensures that the empirical model specified in equation (1) identifies the causal effects of both treatments. Nevertheless, it should be noted that, relative to the control group, the message treatment should only affect the individuals' behavior if they actually open the message. Therefore, we also estimate *treatment effects*

on the treated, i.e. treatment effects for those who actually opened the treatment message, by two-stage least squares (see e.g. Angrist *et al.*, 1996). Specifically, we instrument the dummy indicating whether the individual opened the message with an indicator of the treatment status ($0 \equiv control$ and $1 \equiv message$). Similarly, we estimate the effect on those who clicked on the link to the online tool. Since the only difference between the tool and the message treatment is the link to the online tool, we can use the treatment status ($0 \equiv message$ and $1 \equiv tool$) as an instrument for the individual decision to click on the link. Since only about 37% of the welfare recipients opened the treatment message and about 10% clicked on the link, this allows us to scale the treatment effects by the share of compliers.¹³

4.2 Immediate Treatment Reaction

Before analyzing treatment effects on realized labor market outcomes, we analyze the welfare recipients' immediate response to the information treatment. We focus on three outcome variables: (i) the number of registered job applications within the first four weeks, (ii) an indicator of whether a benefit sanction was imposed and (iii) an indicator of whether an exemption from the work requirement was granted. Since all participants in our experiment have already received their first default warning and face a potential sanction within the upcoming six months, we consider imposed sanctions and granted exemptions within the first six months after the start of the intervention. Table 3 presents ITTs of the tool treatment relative to the control group.

[INSERT TABLE 3 ABOUT HERE]

Considering the number of registered job applications is informative as it provides an indication of whether the information treatment indeed increases the perceived strictness of the welfare system. In standard job search models (Mortensen, 1986), tighter benefit rules, e.g. in form of a work requirement accompanied by benefit sanctions, should encourage unemployed workers to increase their search effort. As shown in Table 3, there is indeed evidence that the main treatment operates as expected. Welfare recipients who receive monthly reminder messages and gain access to the online tool register on average

¹³To separate the additional effect of the online tool compared to the pure message, it is required that individuals who opened the treatment message are comparable in both treatment arms. As shown in Table A.1 in Appendix A.2, there is no indication that this assumption is violated.

8% more job applications than individuals in the control group ($p = 0.055$). It should be noted that the number of job applications registered in the official online portal is only an imperfect proxy for the individual search effort since welfare recipients are not legally required to document their job search activities. Hence, the higher number of registered job applications may not necessarily reflect an increased job search effort, but rather could be an attempt to document search activities in the hope of preventing a sanction. However, even if the latter is true, the result is in line with the idea that the treatment group perceives the welfare system as less generous than the control group and confirms the notion that the information treatment increases the salience of the work requirement and the associated threat of a sanction.

In addition, we find that the tool treatment reduces the likelihood that welfare recipients receive a benefit sanction by about 5% relative to the control group, which is equivalent to a reduction of 0.6 percentage points ($p = 0.098$). One possibility to avoid a sanction is to receive an exemption from the work requirement, which could be granted by the caseworker. To achieve an exemption, the welfare recipient would have to argue that it is unreasonable to fulfill the work requirement due to their personal mental or physical condition. However, there is no empirical evidence that the lower sanction rate is provoked by an increased likelihood of getting an exemption from the work requirement. This gives a first indication that our intervention must either increase employment and/or encourage individuals to leave welfare, since these are the only possibilities to prevent a benefit sanction, if the individual is not exempted from the work requirement.

4.3 Labor Market Outcomes

Next, we investigate the effects of the information treatment on realized labor market outcomes. Specifically, we consider (i) monthly indicators of whether the individual left the welfare system (i.e. at least one month without benefit payments), respectively started paid employment (i.e. at least one month with positive labor earnings) and (ii) cumulated outcomes over the first year after the start of the intervention. The latter comprises the total number of months without any welfare benefit payments, the cumulated working hours from paid employment and the corresponding total earnings. Moreover, we also consider the number of months receiving other public benefits, not subject to the work requirement, as an additional outcome variable. This is important as it allows us to shed

further light on behavioral consequences of our information treatment beyond the take-up of paid employment, which might be particularly relevant for welfare recipients with limited labor market attachment.

4.3.1 Baseline Results

Figure 1 shows ITTs of the main tool treatment on monthly indicator variables.¹⁴ The results reveal a striking difference when comparing exits from welfare and the start of paid employment. While the tool treatment increases the likelihood of leaving the system by almost two percentage points ($p < 0.01$) within the first 12 months after the start of the intervention¹⁵, we find no evidence that this is provoked by welfare recipients taking up a regular job. Any effect on the likelihood to start paid employment is small and statistically insignificant. The results presented in Table 4 further emphasize this pattern. When considering cumulated labor market outcomes, we find that the tool treatment increases the total number of months without receiving welfare benefits by about 8% relative to the control group ($p < 0.001$). This, however, neither translates into a higher number of cumulated working hours nor into higher labor earnings. The treatment effects on both outcome variables are close to zero and statistically insignificant.

[INSERT FIGURE 1 AND TABLE 4 ABOUT HERE]

One explanation for the discrepancy between treatment effects on working hours, respectively earnings on one hand and benefit payments on the other hand might be that treated individuals switch to other types of income support, which are not subject to the sanction regime. For instance, Danish citizens are generally eligible for public support, when enrolling in a higher educational program (secondary or tertiary education). Since this also applies to our sample of welfare recipients, one could expect that treated individuals, for whom the welfare system appears less attractive due to the work requirement, make greater use of this opportunity. Similarly, welfare recipients who are already eligible

¹⁴The corresponding effects of the message treatment are presented in Figure A.2 in Appendix A.2.

¹⁵The results presented in Figure A.3 in Appendix A.2 further show that the tool treatment encourages individuals to leave the welfare system for sustained periods of at least six months, which suggests that the tool treatment promotes permanent (rather than transitory) exits from the welfare system.

for early retirement schemes could rely on pension payments¹⁶, while others may avoid a sanction by claiming sickness or disability benefits.¹⁷

For the sake of brevity, we pool all these different categories of public benefits, which are not affected by the work requirement and the sanction regime, and consider the cumulated number of months receiving other benefits as an additional outcome variable. It can be seen that the tool treatment significantly increases the number of months receiving other benefits by about 8% relative to the control group. The increased take-up of alternative benefit schemes accounts for more than two thirds of the treatment effect on the number of months without welfare benefit payments. This highlights that welfare recipients receiving the tool treatment reduce their risk of being sanctioned by avoiding the work requirement rather than by increasing their job search and work effort. Table A.2 in Appendix A.2 shows separate effects on different benefit categories. It can be seen that the overall effect on the receipt of other benefits is mainly driven by an increased take-up of disability benefits (+10%) and educational support (+12%). We provide further evidence regarding the treatment effects on educational activities in Section 4.5.

It is plausible that this result is a direct consequence of the incentive effects described in Section 2. Since the work requirement reduces the attractiveness of welfare, the information treatment is likely to have a deterring effect, which encourages individuals to rely on other benefits if this is possible. Moreover, welfare recipients who are entitled to other benefits, such as educational support and disability benefits, might be less likely to actively search for employment. Hence, the results also suggest the presence of a screening effect, such that especially individuals with a stronger preference for non-working time leave welfare. In what follows, we further investigate this idea.

4.3.2 The Role of Personalized Information

Besides the pure incentive effects of the work requirement itself as discussed in Section 2, the degree to which individuals are informed about their own situation in relation to the requirement might be important for the individual response to the treatment. So far, we presented the effects of the tool treatment, which contains two elements: (i) monthly

¹⁶Individuals 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.

¹⁷Van den Berg *et al.* (2019) investigate the role of this additional layer of moral hazard in the German unemployment insurance system. They find evidence for strategic sick-reporting in order to avoid benefit sanctions.

messages reminding the individual about the general rules and (ii) access to the online tool providing personalized information about the welfare recipients own situation in relation to the work requirement.

As a crucial feature of our experimental design, we can disentangle the causal effects of these two elements by utilizing the message treatment as described in Section 3.1. Individuals assigned to the message treatment do not gain access to the online tool. Hence, by comparing the message treatment group to the control group, we can identify the causal effect of providing general information. Moreover, the comparison of individuals assigned to the tool and the message treatment allows us to identify the additional effect of having access to personalized information and provides insight into the relevance of information frictions.

Intention-to-treat effects. Table 5 shows three different ITTs. Specification (1) replicates the effect of the main tool treatment already presented in the previous section, while specification (2) shows the effect of the message treatment relative to the control group and specification (3) shows the effect of the tool relative to the message treatment. In general, it can be seen that the message treatment has substantially different effects than the tool treatment. While the pure message has no statistically significant impact on the number of months without welfare payments, it reduces total working hours and labor earnings of welfare recipients by about 5-6% relative to the control group (see specification 2). Moreover, when comparing the tool and the message treatment (see specification 3), we see that additionally having access to the personalized online tool counteracts the negative effect of the pure message, containing only general information about the work requirement, on working hours and earnings. Hence, the zero effect of the main tool treatment on working hours and earnings masks two contrary effects. On the one hand, the generalized information about the work requirement provided through the message (which is also part of the tool treatment) reduces the welfare recipients' subsequent labor market performance. On the other hand, gaining access to personalized information regarding the number of collected working hours seems to have the opposite effect.

[INSERT TABLE 5 ABOUT HERE]

We further test the sensitivity of our results with respect to possible gaming effects, i.e. welfare recipients provide just enough effort to fulfill the requirement. Therefore, we re-

estimate ITTs on cumulated working hours and labor earnings, but set the corresponding outcome variable to zero if the total number of working hours within 12 months falls below various thresholds. Thereby, we disregard working hours and earnings of those who have the strongest incentives to game the system.¹⁸ As shown in Figure A.4 in Appendix A.2, the results are remarkably stable when considering different thresholds, which implies that gaming responses are unlikely to affect the estimated treatment effects.

Finally, it is important to note that the disparate effects of the two treatments are mainly provoked by differences regarding how individuals adjust their labor market activities, approximated by working hours and earnings, while both treatments similarly increase the take-up of other benefits. This suggests that information frictions regarding the individuals' own situation in relation to the requirement have a strong impact on the behavior of welfare recipients who actively search for employment, but are less relevant for behavioral responses related to non-working activities.

Treatment effects on treated. Before further investigating the underlying mechanisms, we first relate the main results to the take-up of the two treatment elements. Although the estimated ITTs identify the causal effect of both treatments, we obviously neglect the fact that only 10% of the individuals receiving the message with the link to the online tool actually click on the link, while about 37% read the message. Hence, we now estimate treatment effects on the treated of reading the message, respectively clicking on the link to the tool, as described in Section 4.1 to assess the effect of actually accessing the provided information.

[INSERT TABLE 6 ABOUT HERE]

The results are summarized in Table 6. First, Panel A shows the first stage estimates. Again, we can see that treatment compliance is substantially higher for the message than for the tool treatment, which is not surprising given that accessing the online tool requires more effort than simply opening the treatment message. Nevertheless, for both

¹⁸Specifically, we choose the values of the first, second and third quartile of the corresponding distribution of collected working hours and the requirement of 225 hours as the margins of interest. If gaming effects play an important role, one would expect that the effect of the tool treatment (relative to the message treatment) is sensitive with respect to the choice of threshold. For instance, when only considering working hours of individuals who worked more than 775 hours within a year (which is more than three times the requirement) it seems very unlikely that gaming incentives influence the estimated effects.

treatments the first stage seems sufficiently strong to avoid any problems due to weak instruments (Staiger and Stock, 1997). Moreover, Panel B shows the treatment effects on the treated. The results are qualitatively similar to the ITTs presented in Table 4, which is reassuring as it confirms that the observed changes in labor market outcomes are driven by individuals, who actually read the message, respectively utilized the online tool. Moreover, the estimates reveal an interesting pattern when considering the overall number of working hours and labor earnings. The positive effect of receiving personalized information through the tool treatment is 4-5 times larger than the negative effect of the pure warning through the message treatment.

This further highlights the importance of the informational content transmitted through the information treatment. On the one hand, welfare recipients who only read the message and receive general information about the work requirement show an impaired labor market performance, in terms of cumulated working hours and labor earnings, relative to the control group. It should be noted that we find no evidence of any significant effects of the message treatment on the extensive margin, i.e. when considering the individual employment probability (see Figure A.2). Together, this suggests that the message treatment encourages individuals to accept jobs with a lower match quality. On the other hand, individuals gaining access to the online tool and actually utilizing the personalized information experience substantially improved labor market outcomes in comparison to the message group, but also to the control group. In summary, the findings indicate that solely emphasizing the threatening aspects of the welfare system (through the message treatment) may have a deterring effect that impairs individual labor market performance, while there is a large potential to improve labor market outcomes by relaxing information frictions in relation to the work requirement.

Finally, it should be noted that potential challenges may arise from the low responsiveness of welfare 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 take this information into consideration for two reasons. First, to increase the overall effectiveness of the online tool it seems obvious 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 ITTs presented in Table 4.

4.4 Job Characteristics

The differential effects of the message and the tool treatment raise the question how welfare recipients use the personalized information that is provided through the online tool. Therefore, we now consider the characteristics of the first paid job individuals accept after the start of the intervention. Specifically, we estimate ITTs on the job duration, as well as on the average monthly earnings and working hours associated with the first job. Moreover, we consider the total number of employers within the first 12 months after the start of the intervention as another indicator of job stability. It should be noted that the analysis is somewhat descriptive as it does not take into account that only a selection of individuals accept a job at all. However, as we shall see, data patterns provide suggestive evidence into what are the most likely causal pathways. It should be noted that there are no statistically significant treatment effects on the likelihood of accepting any employment, which mitigates potential concerns regarding the selectivity of the considered sample.

[INSERT TABLE 7 ABOUT HERE]

The corresponding estimation results are presented in Table 7 and reveal an interesting pattern. When comparing the tool and the message treatment, it turns out that each group tends to accept different types of jobs. In particular, welfare recipients who gained access to personalized information accept jobs with a higher number of monthly working hours (+4.5%), respectively earnings (+4.1%) and tend to change employers less often (-5.5%) than those who only received the general message (with $p < 0.05$ for all three outcome variables). Moreover, in comparison to the control group, we see that the job duration among those who received the pure message is significantly reduced by about 3% ($p = 0.045$).

The overall pattern is in line with the idea that by emphasizing the work requirement and the threat of being sanctioned, the message treatment increases the perceived strictness of the welfare system. Existing evidence indicates that tighter requirements

and sanction regimes encourage job seekers to accept lower quality jobs (Arni *et al.*, 2013; Van den Berg and Vikström, 2014; Arni and Schiprowski, 2019). In our case, the increased willingness to accept a lower match quality translates into reduced cumulated labor market outcomes since no additional employment is created. However, providing personalized information regarding the welfare recipient’s own situation in relation to the work requirement can counteract these adverse effects with respect to the job match quality. Hence, the greater uncertainty associated with the message treatment (relative to the tool treatment) seems to reinforce the deterring effect of the work requirement for individuals who are actively searching for paid employment.

4.5 Educational Activities

Besides the effects on work-related activities, we have seen that the information treatment promotes the usage of other public benefits, especially educational support. This could reflect two possible behavioral responses. On the one hand, the treatment may cause individuals to enroll in an educational program and therefore promote the accumulation of human capital. Alternatively, it might be the case that some welfare recipients are enrolled in an educational program independently of our intervention, but the treatment encourages them to apply for the designated financial support program.¹⁹ To disentangle these two possible effects, we consider an indicator of whether the individual starts a higher educational program after the start of the intervention as an additional outcome variable.

[INSERT TABLE 8 ABOUT HERE]

As shown in Table 8, the tool treatment indeed promotes the enrollment in higher education. Relative to the control group, individuals who have access to the online tool are 14% more likely to start further education after the start of the intervention ($p = 0.006$). Moreover, we also show that the increase in take-up of educational support is driven by those who enroll in an educational program after the start of the intervention, while there is no effect on those who do not. This indicates that the presence of the work requirement may have favorable side effects by promoting the accumulation of human capital. This

¹⁹The level of educational support is equivalent (for individuals below age 30) or slightly below (for individuals of age 30 or above) the level of welfare benefit payments, which may imply that welfare recipients who enroll in an educational program only have little incentives to apply for the designated financial support program, but rather continue receiving welfare benefits.

might be particularly beneficial given the limited overall labor market attachment of welfare recipients.

4.6 Alternative Income Sources

Our main analysis focuses on income generated through paid employment. This obviously neglects other economic activities such as self-employment²⁰ and does not necessarily allow us to draw conclusions regarding the overall welfare effects. Therefore, we consider an alternative data source that provides information about different income types, including paid employment, self-employed work, capital gains and public benefits, on an annual basis. This allows us to provide a more holistic assessment regarding the effects of our intervention.

[INSERT FIGURE 2 ABOUT HERE]

Figure 2 shows ITTs for different types of income: (i) wage income from paid employment, which is comparable to the baseline results presented in Section 4.3, (ii) all work-related earnings also including income from self-employment, (iii) the total personal gross income and (iv) the disposable personal income (net of taxes). We show separate effects for the calendar years 2018 and 2019, while it should be noted that the impact on income in 2018 is limited since the intervention only started in August 2018. While the overall pattern confirms our previous findings, the estimates provide two further insights. First, when comparing Panel A and B, it becomes visible that self-employment does not seem to play an important role among our sample of welfare recipients. Second, relative to the message treatment, the tool treatment also has a positive effect on the total and disposable personal income. This is an important finding as it emphasizes that information frictions about the individuals' own situation in relation to the work requirement reduce their consumption possibilities. This suggests that information frictions may reinforce the adverse effects of a work requirement and the associated sanction regime on welfare recipients' ability to smooth their consumption, which in turn could increase the pressure to accept jobs with a lower match quality in the future.

²⁰Income earned from self-employed work is translated into working hours using a specific translation rule and therefore also contributes to the fulfillment of the work requirement.

4.7 Heterogeneous Treatment Effects

Finally, we examine heterogeneous treatment effects with respect to different background characteristics. Specifically, we focus on the welfare recipients' (registered) job search activities before the start of the intervention, as well as age and family status. The results are presented in Figure 3. To simplify the presentation of the results, we present ITTs on our four main outcome variables, which are standardized to have a mean of zero and a standard deviation of one.

[INSERT FIGURE 3 ABOUT HERE]

First, we investigate heterogeneous effects based on the individuals' previous job search activities to provide further insights regarding the screening effect of the work requirement. Although welfare recipients are not required to register their job search activities, it appears highly likely that those who do so are actively searching for a job. Hence, we divide the estimation sample by whether the individual registered any job application within the last four weeks before the intervention. This allows us to explicitly document any disparate effects of the work requirement on welfare recipients who do, respectively do not actively search for employment. The results presented in Panel A of Figure 3 reveal an interesting pattern. On the one hand, the positive and significant effect of the tool treatment on the number of months without welfare benefits, respectively receiving other benefits is driven by individuals who did not register any application before the intervention. This is in line with the idea that mainly individuals with higher preferences for non-working time decide to leave the welfare system when receiving additional information about the work requirement. On the other hand, the negative effect of the message treatment on the individuals' subsequent working hours and earnings stems solely from welfare recipients who did actively register at least one job application in the pre-intervention period. This is reassuring for the idea that registered applications are to some extent informative regarding the actual job search behavior of the unemployed. Only individuals who actively search for a job are assumed to adjust their behavior by accepting jobs with a lower match quality.

Altogether, the findings indicate that the presence of a work requirement can have disparate effects on unemployed workers as a reflection of their willingness to apply for jobs. This highlights the role of work requirements as a screening device as outlined

in Section 2. For welfare recipients who are not actively searching for employment the additional information about the work requirement most likely creates a larger disutility than for those who searched for a job even before the intervention. Hence, welfare becomes less attractive and the treatment therefore encourages them to leave the system and rely on other types of income support.²¹

There are several reasons to assume that age is an important factor in determining the individual's response to the treatment. First, as mentioned in Section 2.1, the benefit rules differ for welfare recipients below and above age 30. The overall benefit level, but also the size of the benefit sanction is lower for welfare recipients, who are below age 30. Second, our previous results have shown that the (tool) treatment stimulates exits from welfare, while promoting inter alia educational activities, which are obviously widespread among younger individuals. Therefore, we now estimate separate treatment effects for welfare recipients above and below age 30. The results are shown in Panel B of Figure 3. When considering only individuals who are older than 30 years, the overall pattern for both treatments looks very similar to the baseline results presented in Table 5. This is reassuring as it shows that our main results are not driven by the particularities of welfare recipients below age 30.

Finally, the presence of a partner affects the individual's incentives to react to the information treatment. One reason is that having a partner with working income reduces the individual's dependence on welfare benefits, e.g., they can rely on a partner's income for consumption smoothing. Moreover, the overall amount of welfare benefits for individuals with a partner is limited such that the gross income of the couple does not exceed an amount equal to twice the regular benefit level of a single person. Both reasons would imply that the incentives to react to the information treatment are lower for welfare recipients who have a partner. However, the size of the benefit sanction also depends on the family status. In most cases, the sanction will be larger as the presence of a partner

²¹Since registered job applications may not cover all job search activities, we also conduct a subgroup analysis based on the official classification of the PES, which distinguishes between welfare recipients who are deemed capable of full-time employment and those who are not, as an alternative proxy for active job search. As shown in Figure A.5 in Appendix A.2, the overall pattern looks similar. This mitigates concerns that a possible discouragement effect of low-skilled individuals (who do not register their job applications) explains the results.

generally implies that the benefit level of a welfare recipient who does not fulfill the work requirement is reduced by 100%.²²

As shown in Panel C of Figure 3, the tool treatment has similar effects for individuals with and without a partner, but the effect of the message treatment seems to depend on family status. In particular, we only find a negative effect of the message treatment for individuals with a partner. This pattern could be explained by the fact that the potential benefit sanction is presumably larger for individuals with a partner. Hence, they have stronger incentives to work some hours in order to avoid the upcoming benefit sanction. However, since they can also rely on the partner's income to smooth their consumption they might have a higher willingness to accept part-time or temporary jobs with a relatively low wage, which could explain the weaker cumulated labor market outcomes.

5 Conclusion

Work requirements in combination with benefit sanctions are one of the key tools to mitigate the distortionary effects of welfare and social security systems. Our paper provides novel experimental evidence that provides a better understanding of the causal effect of such a policy regime on the behavior of unemployed workers and their subsequent labor market outcomes. Based on a large-scale randomized controlled trial conducted among Danish welfare recipients, we make several contributions.

First, we find conclusive evidence that increasing welfare recipients' awareness of the requirement and the risk of receiving a benefit sanction has a deterring effect, which stimulates exits from the welfare system and encourages active job seekers to accept jobs with a lower match quality. Since the latter is not accompanied with the creation of additional employment, our results suggest that imposing a work requirement misses the main objective of restoring search incentives. Rather, it may harm the individual's possibility to smooth consumption, which is typically seen as a socially beneficial feature of welfare systems to correct failures in the credit and insurance market (see e.g. Chetty, 2008).

²²There is one exception when both partners receive welfare benefits and do not fulfill the work requirement. In this situation, only the benefit level of the partner who first misses the target is reduced by the full amount, while the other partner does not face a reduction of the benefit level. In such a situation, the couple can only increase their income if both partners comply with the work requirement.

Second, we explicitly show that the work requirement has disparate effects depending on the welfare recipients' job search activities, which highlights that it can serve as a screening device encouraging individuals who are not actively searching for employment to leave the welfare system. This may allow policy-makers to improve targeting of governmental policies and therefore potentially increase overall welfare, while the promotion of educational activities and the accumulation of human capital is an additional favorable side effect. However, one should take into account the possibility that the presence of a work requirement may discourage low-skilled workers who have difficulties to work the minimum number of hours.

Moreover, we explicitly investigate the role of information frictions regarding the individual's own situation with respect to the work requirement and the potential sanction. General information emphasizing the presence of the requirement and the risk of receiving a benefit sanction encourage individuals to accept jobs with a lower match quality. While the presence of such a deterring effect is in line with existing (non-experimental) findings for comparable policy instruments, such as job search requirements, we explicitly show that the provision of personalized information can counteract these adverse effects. This highlights that is important how the presence of a requirement and a possible sanction is communicated and that information frictions regarding the individual's own situation may play an important role. By relaxing these frictions, policy-makers may increase the effectiveness of welfare and social security systems.

Finally, by considering a group of unemployed workers with substantial heterogeneity regarding their attachment to the labor market, we shed further light on the disparate effects of work requirements and sanctions found by earlier studies. While active job seeker tend to accept jobs with a lower match quality, individuals with limited labor market attachment seem to leave welfare without starting paid employment. This indicates that the effectiveness a policy involving personal requirements crucially depends on the composition of the target population.

References

- ABBRING, J. H. AND G. J. VAN DEN BERG (2003): “The nonparametric identification of treatment effects in duration models,” *Econometrica*, 71, 1491–1517.
- 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.
- ACEMOGLU, D. AND R. SHIMER (2000): “Productivity gains from unemployment insurance,” *European Economic Review*, 44, 1195–1224.
- 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.
- 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.
- ASHENFELTER, O., D. ASHMORE, AND O. DESCHÊNES (2005): “Do unemployment insurance recipients actively seek work? Evidence from randomized trials in four US States,” *Journal of Econometrics*, 125, 53–75.
- 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.
- 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.
- 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.
- 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.
- BOONE, J., P. FREDRIKSSON, B. HOLMLUND, AND J. C. VAN OURS (2007): “Optimal unemployment insurance with monitoring and sanctions,” *The Economic Journal*, 117, 399–421.
- BOONE, J., A. SADRIEH, AND J. C. VAN OURS (2009): “Experiments on unemployment benefit sanctions and job search behavior,” *European Economic Review*, 53, 937–951.
- 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.

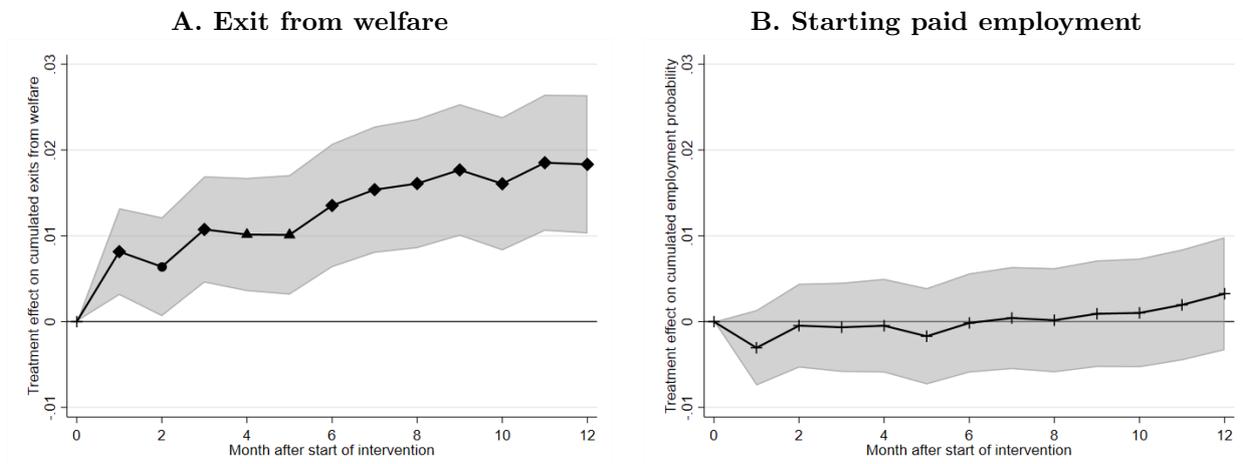
- CHETTY, R. (2008): “Moral hazard versus liquidity and optimal unemployment insurance,” *Journal of Political Economy*, 116, 173–234.
- CHIU, W. H. AND E. KARNI (1998): “Endogenous adverse selection and unemployment insurance,” *Journal of Political Economy*, 106, 806–827.
- COCKX, B., C. GHIRELLI, AND B. VAN DER LINDEN (2014): “Is it socially efficient to impose job search requirements on unemployed benefit claimants with hyperbolic preferences?” *Journal of Public Economics*, 113, 80–95.
- 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.
- DELLAVIGNA, S. AND M. D. PASERMAN (2005): “Job search and impatience,” *Journal of Labor Economics*, 23, 527–588.
- FLUCHTMANN, J., A. M. GLENNY, N. A. HARMON, AND J. MAIBOM (2019): “The dynamics of job search in unemployment,” *mimeo*.
- 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.
- GEERDSEN, 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.
- 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.
- HOPENHAYN, H. A. AND J. P. NICOLINI (1997): “Optimal unemployment insurance,” *Journal of Political Economy*, 105, 412–438.
- JOHNSON, T. R. AND D. H. KLEPINGER (1994): “Experimental evidence on unemployment insurance work-search policies,” *Journal of Human Resources*, 695–717.
- 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.
- KLEPINGER, D. H., T. R. JOHNSON, AND J. M. JOESCH (2002): “Effects of unemployment insurance work-search requirements: The Maryland experiment,” *ILR Review*, 56, 3–22.
- KREINER, C. T. AND T. TRANÆS (2005): “Optimal workfare with voluntary and involuntary unemployment,” *Scandinavian Journal of Economics*, 107, 459–474.
- LACHOWSKA, M., M. MERAL, AND S. A. WOODBURY (2016): “Effects of the unemployment insurance work test on long-term employment outcomes,” *Labour Economics*, 41, 246–265.
- 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.
- LAMMERS, M., H. BLOEMEN, AND S. HOCHGUERTEL (2013): “Job search requirements for older unemployed: Transitions to employment, early retirement and disability benefits,” *European Economic Review*, 58, 31–57.
- 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.
- MANNING, A. (2009): “You can’t always get what you want: The impact of the UK Jobseeker’s Allowance,” *Labour Economics*, 16, 239–250.
- MARINESCU, I. (2017): “The general equilibrium impacts of unemployment insurance: Evidence from a large online job board,” *Journal of Public Economics*, 150, 14–29.
- MCVICAR, D. (2008): “Job search monitoring intensity, unemployment exit and job entry: Quasi-experimental evidence from the UK,” *Labour economics*, 15, 1451–1468.
- MEYER, B. D. (1995): “Lessons from the US unemployment insurance experiments,” *Journal of Economic Literature*, 33, 91–131.
- 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. (2006): “Welfare work requirements with paternalistic government preferences,” *The Economic Journal*, 116, F441–F458.
- MORTENSEN, D. T. (1986): “Job search and labor market analysis,” *Handbook of Labor Economics*, 2, 849–919.
- 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.
- MUKOYAMA, T. AND A. ŞAHİN (2006): “Specialization and human capital in search equilibrium,” *Journal of the European Economic Association*, 4, 503–512.
- 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.
- PASERMAN, M. D. (2008): “Job search and hyperbolic discounting: Structural estimation and policy evaluation,” *The Economic Journal*, 118, 1418–1452.
- PAVONI, N. AND G. L. VIOLANTE (2007): “Optimal welfare-to-work programs,” *Review of Economic Studies*, 74, 283–318.
- PETRONGOLO, B. (2009): “The long-term effects of job search requirements: Evidence from the UK JSA reform,” *Journal of Public Economics*, 93, 1234–1253.
- 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.

- STAIGER, D. AND J. H. STOCK (1997): “Instrumental variables regression with weak instruments,” *Econometrica*, 557–586.
- 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. AND B. VAN DER KLAUW (2006): “Counseling and monitoring of unemployed workers: Theory and evidence from a controlled social experiment,” *International Economic Review*, 47, 895–936.
- 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.
- VAN DER KLAUW, B. AND J. C. VAN OURS (2013): “Carrot and stick: How re-employment bonuses and benefit sanctions affect exit rates from welfare,” *Journal of Applied Econometrics*, 28, 275–296.
- VENN, D. (2012): “Eligibility Criteria for Unemployment Benefits: Quantitative Indicators for OECD and EU Countries,” *OECD Social, Employment, and Migration Working Papers*.

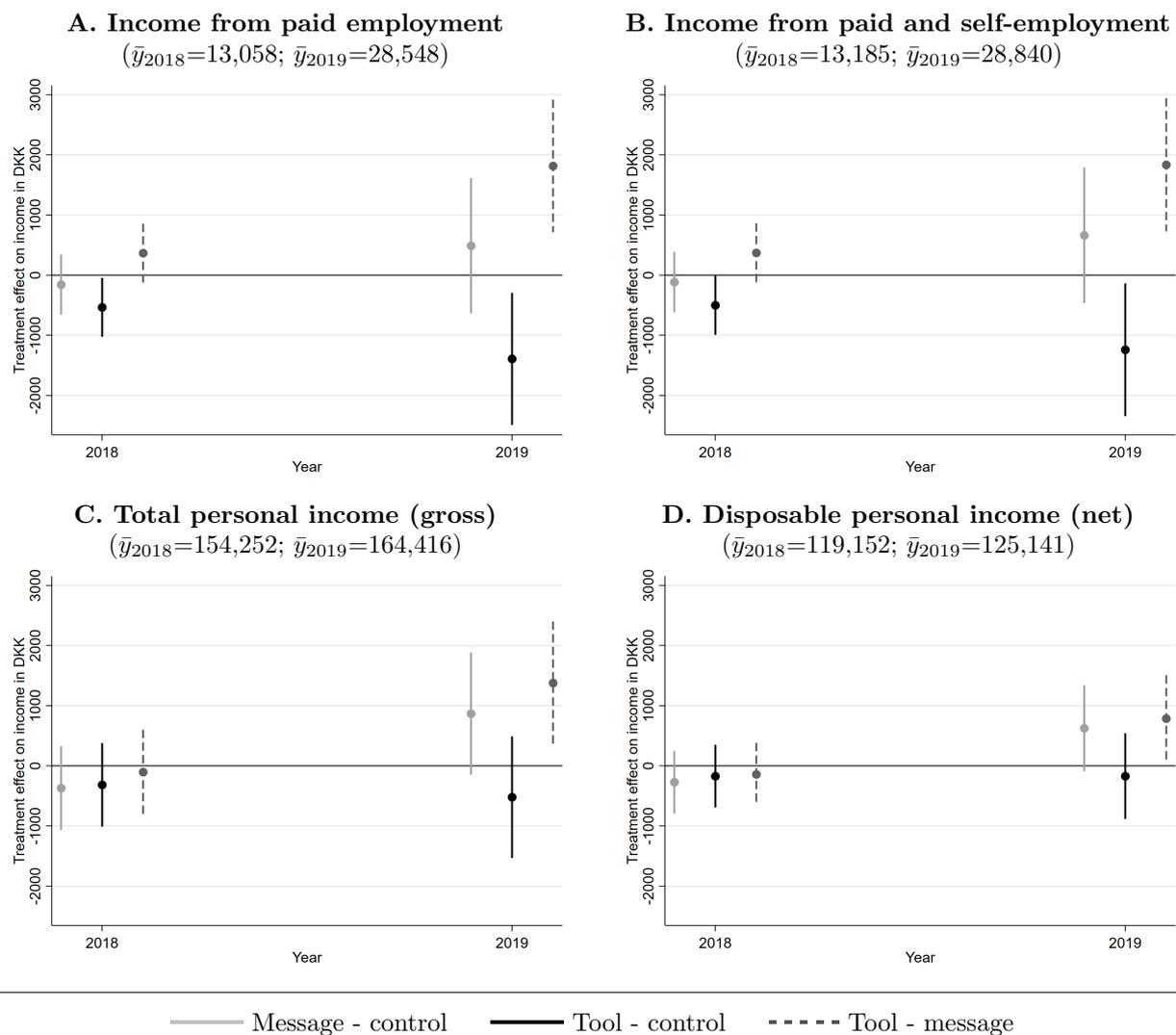
Figures and Tables

Figure 1: Effect of tool treatment on labor market status over time



Note: Depicted are intention-to-treat effects of the tool treatment (relative to the control group) on monthly indicator variables for (a) exiting the welfare system (i.e. at least one month without welfare benefit payments) and (b) starting paid employment (i.e. at least one month with positive labor earnings) up until month t including 90% confidence intervals. The corresponding effects of the message treatment are presented in Figure A.2 in Appendix A.2.

Figure 2: ITTs of tool and message treatment on different types of yearly income



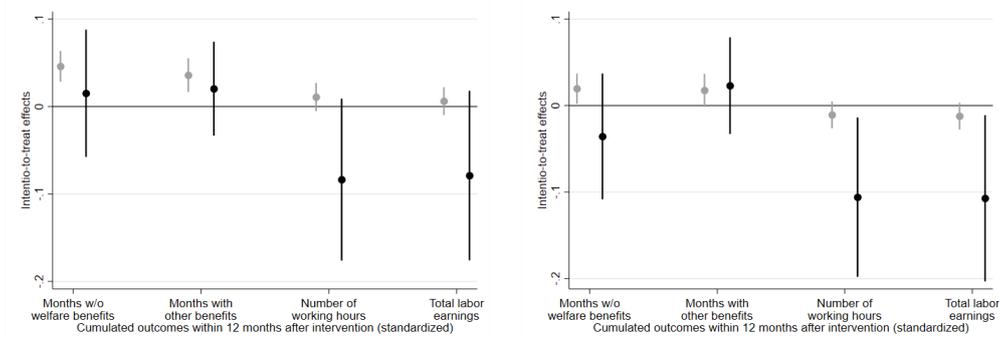
Note: Depicted are intention-to-treat effects including 90% confidence intervals. The dependent variable refers to the individual total income in a given calendar year for different types of income.

Figure 3: Subgroup analysis

A. Subgroup analysis by registered job applications

A.1 Tool - control

A.2 Message - control

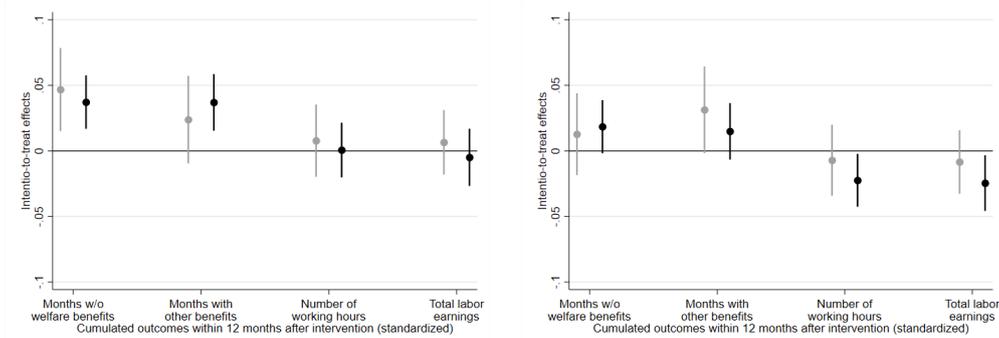


Any registered job applications in last four weeks: — No — Yes

B. Subgroup analysis by age

B.1 Tool - control

B.2 Message - control

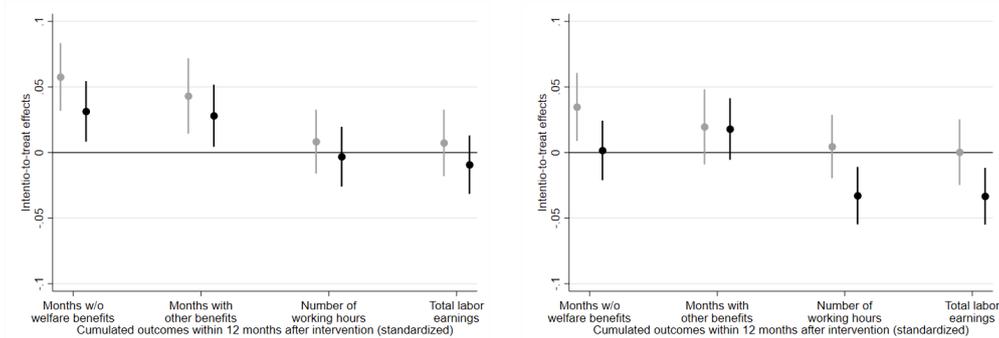


Age in years: — <30 years — ≥ 30 years

C. Subgroup analysis by family status

C.1 Tool - control

C.2 Message - control



Partner living in same household: — No — Yes

Note: Depicted are intention-to-treat effects of the tool and message treatment (relative to the control group) on cumulated outcomes within the first 12 months after the intervention for different subgroups including 90% confidence intervals. Outcome variables are standardized to have a mean of zero and a standard deviation of one.

Table 1: Descriptive statistics by treatment status

	Treatment status				
	Control (C)	Message (M)		Tool (T)	
	Mean	Mean	<i>P</i> -value	Mean	<i>P</i> -value
No. of observations	15,769	15,764		15,761	
<i>Socio-demographic characteristics</i>					
Female	0.495	0.491	0.401	0.500	0.418
Married	0.172	0.171	0.844	0.176	0.339
Education					
Elementary education (or none)	0.033	0.033	0.733	0.034	0.749
Lower secondary education	0.587	0.589	0.689	0.582	0.389
Upper secondary education	0.269	0.266	0.527	0.271	0.619
Tertiary education	0.086	0.089	0.287	0.088	0.526
Age					
16-25 years	0.216	0.220	0.469	0.215	0.877
26-35 years	0.272	0.269	0.497	0.268	0.356
36-45 years	0.213	0.210	0.544	0.219	0.181
46-55 years	0.193	0.193	0.955	0.193	0.926
56-65 years	0.105	0.108	0.427	0.104	0.739
65 years+	0.001	0.001	0.532	0.001	0.684
Ethnicity					
Danish	0.717	0.711	0.244	0.713	0.467
Descendant	0.037	0.037	0.830	0.036	0.840
Immigrant	0.246	0.252	0.261	0.250	0.397
Living in Capital Region	0.326	0.324	0.644	0.320	0.287
<i>Household information</i>					
Children					
No children	0.621	0.627	0.284	0.614	0.211
One child	0.151	0.154	0.619	0.154	0.509
Two children	0.109	0.105	0.199	0.112	0.441
Three or more children	0.118	0.115	0.349	0.120	0.690
Household size					
One person	0.298	0.299	0.878	0.302	0.429
Two persons	0.234	0.232	0.672	0.229	0.285
Three persons	0.169	0.167	0.628	0.169	0.932
Four persons	0.119	0.126	0.062	0.121	0.678
Five or more persons	0.179	0.175	0.399	0.179	0.970
<i>Labor market histories</i>					
Type of welfare benefits					
Regular welfare benefits	0.629	0.629	0.989	0.629	0.972
Integration allowance	0.068	0.067	0.970	0.067	0.938
Education allowance	0.304	0.304	0.995	0.304	0.996
Requires activation	0.712	0.712	0.985	0.712	0.993
Subsequent weeks on welfare	134.5	134.1	0.750	135.1	0.746
Any paid employment in year $t - x$					
$t - 1$	0.216	0.210	0.153	0.215	0.834
$t - 2$	0.249	0.249	0.904	0.251	0.594
$t - 3$	0.247	0.245	0.659	0.248	0.896
Average weekly working hours in year $t - x$					
$t - 1$	2.44	2.37	0.234	2.48	0.478
$t - 2$	3.55	3.55	0.700	3.59	0.671
$t - 3$	3.85	3.81	0.593	3.89	0.735
Average monthly labor earnings in DKK in year $t - x$					
$t - 1$	1,864	1,796	0.231	1,826	0.497
$t - 2$	2,504	2,480	0.731	2,482	0.746
$t - 3$	2,586	2,567	0.793	2,581	0.938
Average no. of employers in year $t - x$					
$t - 1$	0.273	0.268	0.453	0.272	0.858
$t - 2$	0.331	0.329	0.791	0.336	0.382
$t - 3$	0.324	0.324	0.981	0.329	0.411
Experienced benefit reduction within previous year	0.114	0.111	0.482	0.112	0.556
Exempted from requirement within previous year	0.344	0.352	0.171	0.346	0.797

Note: Depicted are descriptive statistics separated for individuals in the three treatment arms measured at the start of the intervention. Percentage share unless indicated otherwise. *P*-values measured based on two-tailed t-tests on equal means.

Table 2: Determinants of treatment take-up

	Dependent variable			
	Opened treatment message ^(a)		Clicked on link to online tool ^(b)	
	Coef.	<i>P</i> -value	Coef.	<i>P</i> -value
Female	0.0138	[0.006]	0.0080	[0.088]
Partner living in same household	0.0474	[0.000]	0.0045	[0.606]
Education (ref. elementary or none)				
Lower secondary education	0.0482	[0.000]	0.0050	[0.626]
Upper secondary education	0.1139	[0.000]	0.0173	[0.100]
Tertiary education	0.1905	[0.000]	0.0561	[0.000]
Age (ref. 16-24 years)				
26-35 years	0.0881	[0.000]	0.0226	[0.004]
36-45 years	0.0954	[0.000]	0.0297	[0.004]
46-55 years	0.0856	[0.000]	0.0478	[0.000]
56-65 years	0.0750	[0.000]	0.0643	[0.000]
65+ years	0.0716	[0.443]	0.0842	[0.319]
Type of welfare benefits (ref. regular)				
Integration allowance	-0.1682	[0.000]	-0.0863	[0.000]
Education allowance	-0.2465	[0.000]	-0.0807	[0.000]
Requires activation	-0.3569	[0.000]	-0.1362	[0.000]
Consecutive weeks on welfare: 26 or less	-0.0311	[0.000]	-0.0133	[0.027]
Children (ref. no children)				
One child	0.0051	[0.559]	-0.0080	[0.327]
Two children	0.0102	[0.371]	-0.0302	[0.005]
Three or more children	-0.0105	[0.457]	-0.0324	[0.014]
Household size (ref. one person)				
Two persons	-0.0321	[0.003]	-0.0075	[0.447]
Three persons	-0.0429	[0.001]	-0.0111	[0.360]
Four persons	-0.0400	[0.011]	0.0019	[0.899]
Five or more persons	-0.0651	[0.000]	-0.0019	[0.905]
Ethnicity (ref. Danish)				
Descendant	-0.0564	[0.000]	-0.0258	[0.032]
Immigrant	-0.0905	[0.000]	-0.0285	[0.000]
Living in capital region	-0.0085	[0.104]	-0.0059	[0.226]
Labor market outcomes in year $t - 1$				
Any paid employment	0.0499	[0.000]	0.0293	[0.022]
Average monthly labor earnings in 1,000DKK	0.0028	[0.026]	0.0019	[0.150]
Average weekly working hours ($\times 10$)	0.0002	[0.982]	-0.0021	[0.826]
No. of employers	0.0283	[0.001]	0.0418	[0.000]
Experienced benefit reduction in previous year	0.0856	[0.000]	0.0301	[0.000]
Exempted from work requirement in previous year	0.0084	[0.114]	-0.0101	[0.040]
Constant	0.5559	[0.000]	0.1795	[0.000]
No. of observations	31,525		15,761	
Mean value dependent variable	0.3660		0.0956	
R^2 (adj.)	0.2347		0.1205	

Note: OLS estimation. The dependent variables refer to indicators whether the individual opened the treatment message, respectively clicked on the link to the online tool. Depicted are separate estimates for individuals in message, respectively tool treatment group. *P*-values in square brackets.

^(a) Observed for individuals in message and tool treatment.

^(b) Only observed for individuals in tool treatment.

Table 3: Immediate reaction to tool treatment

	Mean value control group	ITT of tool treatment	Relative effect size in %
Dependent variable:			
No. of registered job applications ^(a)	0.5364	0.0445 [0.055]	+8.30
Benefit sanction imposed ^(b)	0.1149	-0.0057 [0.098]	-4.96
Exemption from work requirement ^(b)	0.1188	0.0001 [0.975]	+0.08
No. of observations		31,530	
Control variables		Yes	

Note: Depicted are intention-to-treat-effects of the tool treatment (relative to the control group). *P*-values are shown in square brackets. Control variables are depicted in Table 1.

^(a)Refers to the number of job applications registered in the online portal of the Danish employment agency (*joblog*) within the first four weeks after the start of the intervention.

^(b)Refers to indicator variables whether a sanction was imposed (an exemption was granted) within six months after the start of the intervention.

Table 4: ITTs of tool treatment on cumulated labor market outcomes

	Mean value control group	ITT of tool treatment	Relative effect size in %
Dependent variable:			
No. of months w/o welfare benefits	2.031	0.159 [0.000]	+7.83
No. of working hours	153.1	1.936 [0.610]	+1.26
Labor earnings in DKK	23,676	104 [0.864]	+0.44
No. of months receiving other benefits ^(a)	1.350	0.110 [0.002]	+8.18
No. of observations		31,530	
Control variables		Yes	

Note: Depicted are intention-to-treat-effects of the tool treatment (relative to the control group) on labor market outcomes cumulated over the first 12 months after the start of the intervention. *P*-values are shown in square brackets. Control variables are depicted in Table 1.

^(a)Includes unemployment insurance (UI) benefits, support for educational enrollment, pension payments related to early retirement, parental and sick leave and other types of public benefit payments.

Table 5: The role of personalized information: comparing effects of tool and message treatment

	Mean value control group	Intention-to-treat effects		
		Tool - control (1)	Message - control (2)	Tool - message (3)
Dependent variable:				
No. of months w/o welfare benefits	2.031	0.159 [0.000]	0.056 [0.142]	0.098 [0.012]
No. of working hours	153.1	1.936 [0.610]	-8.258 [0.025]	10.004 [0.007]
Labor earnings in DKK	23,676	104 [0.864]	-1,328 [0.026]	1,392 [0.019]
No. of months receiving other benefits ^(a)	1.350	0.110 [0.002]	0.059 [0.093]	0.051 [0.157]
No. of observation		31,530	31,533	31,525
Control variables		Yes	Yes	Yes

Note: Depicted are intention-to-treat-effects of the tool and message treatment on labor market outcomes cumulated over the first 12 months after the start of the intervention. *P*-values are shown in square brackets. Control variables are depicted in Table 1.

^(a)Includes unemployment insurance (UI) benefits, support for educational enrollment, pension payments related to early retirement, parental and sick leave and other types of public benefit payments.

Table 6: Treatment effects on treated for message and tool treatment

	Mean value control group	Message - control	Tool - message
<i>A. First stage estimation: treatment take-up</i>			
Dependent variable:			
Opened treatment message		0.366 [0.000]	
Clicked on link to online tool			0.096 [0.000]
F-statistic for weak identification		10,338.66	1,765.44
<i>B. Second stage estimation: treatment effects on treated</i>			
Dependent variable:			
No. of months w/o welfare benefits	2.031	0.154 [0.141]	1.028 [0.012]
No. of working hours	153.1	-22.55 [0.025]	104.75 [0.007]
Labor earnings in DKK	23,676	-3,626 [0.026]	14,577 [0.018]
No. of months receiving other benefits ^(a)	1.350	0.162 [0.093]	0.535 [0.157]
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 control group) and (ii) clicking on the link to the online tool (relative to the pure message) on labor market outcomes cumulated over the first 12 months after the start of the intervention. *P*-values are shown in square brackets. Control variables are depicted in Table 1.

^(a)Includes unemployment insurance (UI) benefits, support for educational enrollment, pension payments related to early retirement, disability benefits, parental and sick leave and other types of public benefit payments.

Table 7: Effect of tool and message treatment on job characteristics

	Mean value control group	Intention-to-treat effects		
		Tool - control	Message - control	Tool - message
Dependent variable:				
Duration of first job in months	6.198	-0.082 [0.399]	-0.196 [0.045]	0.128 [0.187]
Avg. monthly earnings in first job in DKK	10,500	300 [0.103]	-127 [0.549]	430 [0.044]
Avg. monthly working hours in first job	68.7	2.018 [0.059]	-0.924 [0.385]	3.051 [0.004]
No. of employers within 12 months	2.431	-0.083 [0.218]	0.060 [0.385]	-0.138 [0.038]
No. of observations		8,366	8,214	8,264
Control variable		Yes	Yes	Yes

Note: Depicted are intention-to-treat-effects of the tool and message treatment for those who started paid employment within the first 12 months after the intervention. *P*-values are shown in square brackets. Control variables are depicted in Table 1.

Table 8: Effect of tool and message treatment on educational activities

	Mean value control group	Intention-to-treat effects		
		Tool - control	Message - control	Tool - message
Dependent variable:				
Starting any further education	0.050	0.007 [0.006]	0.004 [0.135]	0.003 [0.231]
No. of months receiving educational support when starting further education	0.207	0.049 [0.001]	0.028 [0.065]	0.020 [0.208]
when not starting further education	0.328	0.015 [0.418]	0.032 [0.092]	-0.016 [0.408]
No. of observations		31,530	31,533	31,525
Control variable		Yes	Yes	Yes

Note: Depicted are intention-to-treat-effects of the tool and message treatment on the likelihood to start any further education in the post treatment period (until September 2019) and the number of months receiving educational support only (i) when starting further a education in the post-treatment period and (ii) when not starting further a education in the post-treatment period. *P*-values are shown in square brackets. Control variables are depicted in Table 1.

A Appendix (for Online Publication)

The supplementary appendix provides additional information to the reviewer and is not intended to be published but will be made available online.

Section A.1 presents an English translation of the treatment message sent to individuals assigned to A) the message treatment and B) the tool treatment.

Section A.2 shows additional Figures and Tables:

- Figure A.1 shows the online tool as it is displayed to welfare recipients who are assigned to the tool treatment.
- Figure A.2 shows ITTs of the message treatment on monthly indicator variables A) whether the individual exits welfare or B) starts paid employment for the first 12 months after the start of the intervention.
- Figure A.3 shows ITTs on three indicator variables reflecting the distribution of consecutive months without benefit payments. The first variable takes the value one if the individual does not receive welfare benefits for one month, but does in the month after. The other two outcome variables indicate longer interruptions of the welfare spell of at least six, respectively ten consecutive months. It can be seen that the tool treatment significantly increases the probability of leaving welfare for at least six and ten consecutive months, while there is no effect on short-term interruptions of the welfare spell.
- Figure A.4 shows ITTs on cumulated working hours and earnings when setting the corresponding outcome variable to zero if the total number of working hours within 12 months falls below various thresholds (first, second and third quartile of the corresponding distribution of collected working hours and the requirement of 225 hours). This allows us to test the relevance of gaming response since the incentives to game the system, i.e. just work enough hours to fulfill the requirement, are particularly pronounced when the total number of working hours within 12 months is close to the requirement. Since the estimates are not affected by the choice of the threshold it appears unlikely that gaming responses drive our results.

- Figure A.5 shows the results of a subgroup analysis based on the official classification of the PES, which distinguishes between welfare recipients who are deemed capable of full-time employment and those who are not. We consider this as an alternative proxy for active job search, which confirms the results presented in Panel A of Figure 3.
- Table A.1 compares individual characteristics of welfare recipients who opened the treatment message across the treatment arms (i.e. those assigned to the tool treatment and the message treatment).
- Table A.2 shows ITTs of the tool treatment on the number of months receiving different types of public benefits.

A.1 Text of Treatment Messages

A) Message treatment:

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.

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 losing money.

B) Tool treatment:

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

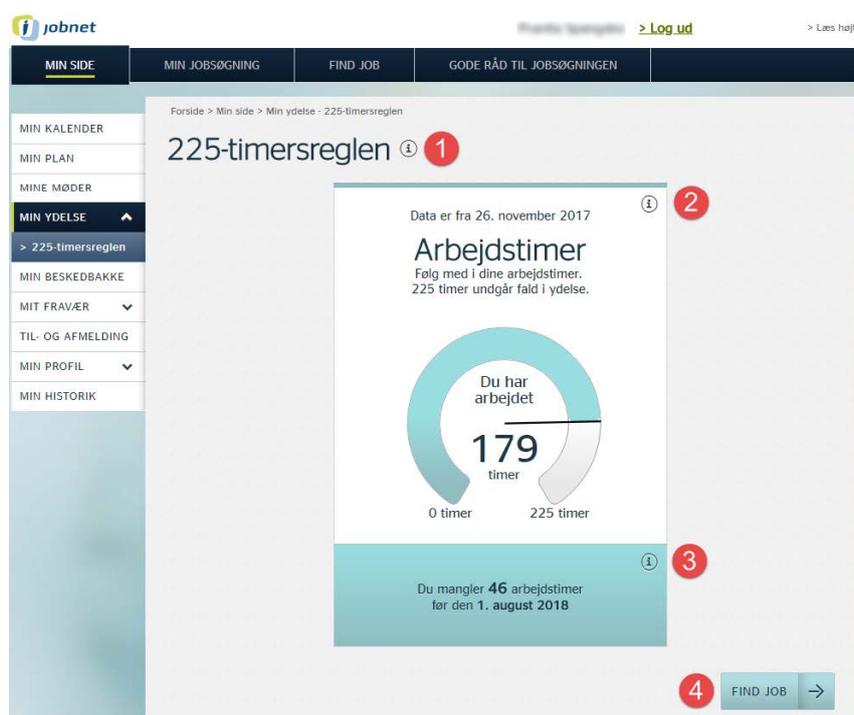
All of the hours you work today will keep counting for the full next year. Therefore it still pays off to gather working hours after you have worked for 225 hours.

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.

A.2 Additional Figures and Tables

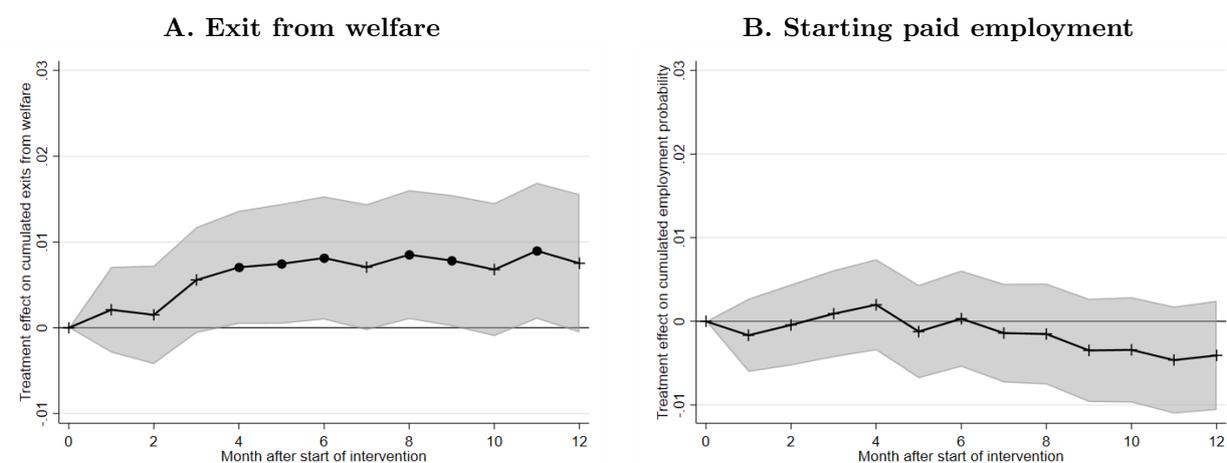
Figure A.1: The online tool



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.

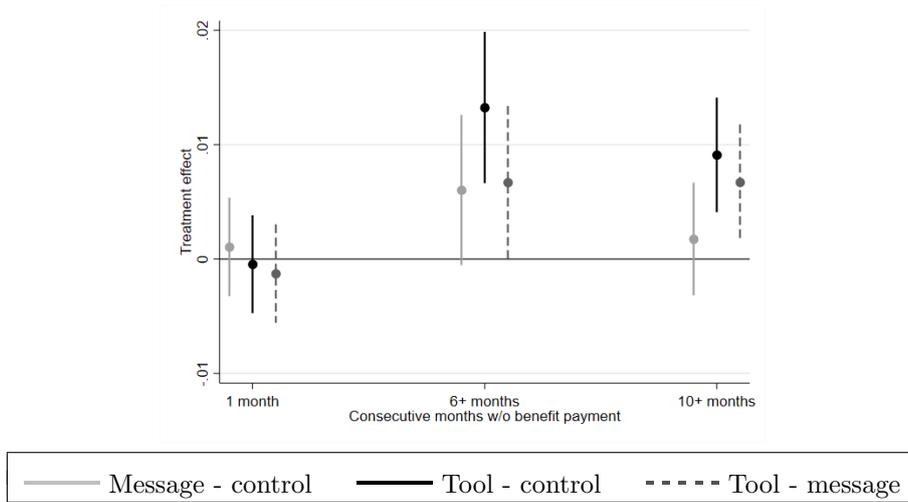
- (1) provides general information about work requirement.
- (2) explains number of collected working hours as of today.
- (3) informs about potential reduction date and the number of hours that is missing to comply with the work requirement.
- (4) link to online job search platform.

Figure A.2: Effect of message treatment on labor market status over time



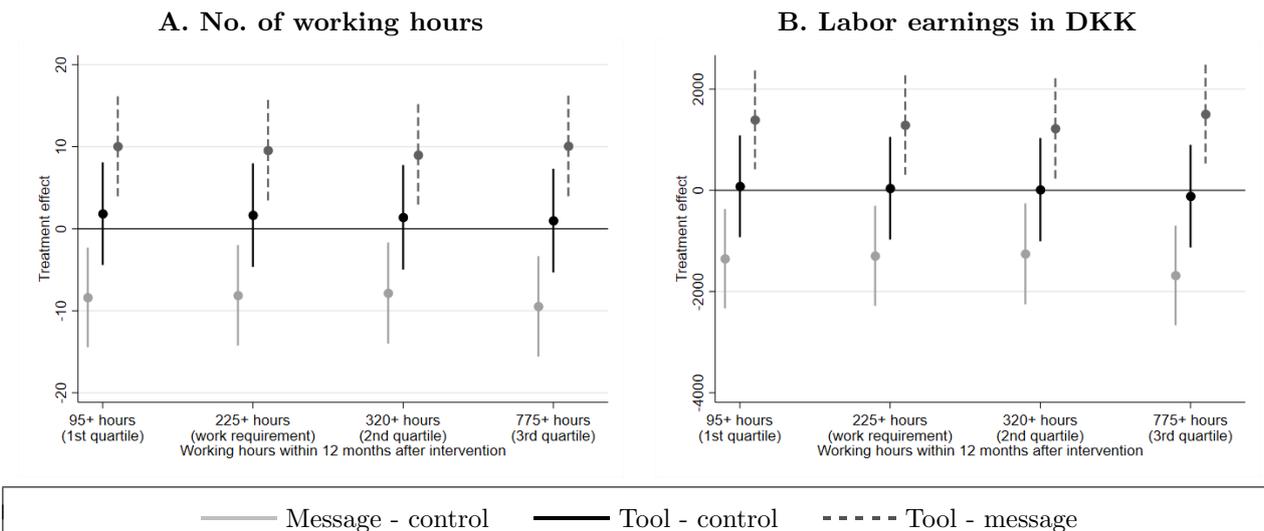
Note: Depicted are intention-to-treat effects of the message treatment (relative to the control group) on monthly indicator variables for (a) exiting the welfare system (i.e. at least one month without welfare benefit payments) and (b) starting paid employment (i.e. at least one month with positive labor earnings) up until month t including 90% confidence intervals.

Figure A.3: Durability of exits from welfare



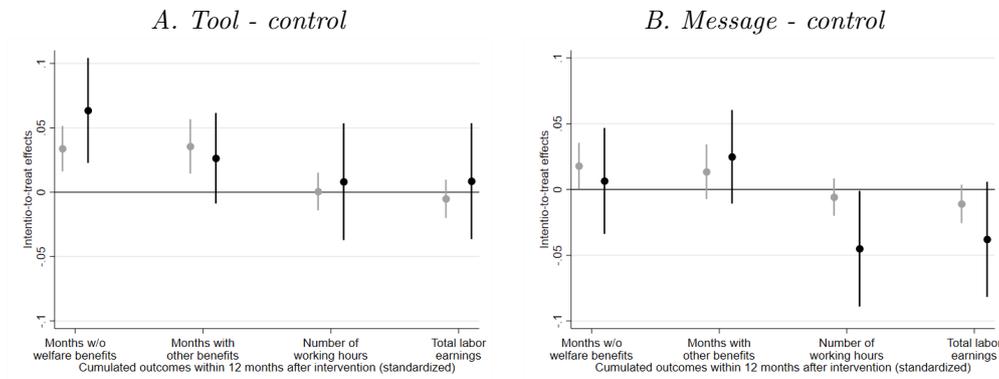
Note: Depicted are intention-to-treat effects including 90% confidence intervals. The dependent variable refers to indicators relating to the number of consecutive months without welfare payments.

Figure A.4: ITTs on labor market outcomes for different margins



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.5: Subgroup analysis by work capacity^(a)



Deemed capable of working full-time employment: — No — Yes

Note: Depicted are intention-to-treat effects of the tool and message treatment (relative to the control group) on cumulated outcomes within the first 12 months after the intervention for different subgroups including 90% confidence intervals. Outcome variables are standardized to have a mean of zero and a standard deviation of one.

^(a) Individuals are categorized based on the official classification of the PES. Those who are not capable of working full-time employment are assumed to require activation in form of active labor market policies.

Table A.1: Descriptive statistics for individuals who opened treatment message

	Treatment status		<i>P</i> -value
	Message	Tool	
No. of observations	5,760	5,779	
<i>Socio-demographic characteristics</i>			
Female	0.501	0.514	0.161
Married	0.189	0.182	0.297
Age			
16-25 years	0.102	0.104	0.667
26-35 years	0.280	0.275	0.543
36-45 years	0.261	0.265	0.612
46-55 years	0.231	0.231	0.923
56-65 years	0.126	0.125	0.902
65 years+	0.000	0.000	.
Ethnicity			
Danish	0.720	0.738	0.032
Descendant	0.027	0.024	0.426
Immigrant	0.253	0.238	0.055
Living in Capital Region	0.325	0.311	0.119
<i>Household information</i>			
Children			
No children	0.602	0.605	0.668
One child	0.168	0.166	0.780
Two children	0.124	0.126	0.722
Three or more children	0.107	0.103	0.467
Household size			
One person	0.303	0.305	0.789
Two persons	0.240	0.242	0.803
Three persons	0.169	0.176	0.328
Four persons	0.138	0.128	0.109
Five or more persons	0.150	0.149	0.879
<i>Labor market histories</i>			
Type of welfare benefits			
Regular welfare benefits	0.791	0.793	0.802
Integration allowance	0.061	0.054	0.100
Education allowance	0.148	0.153	0.433
Requires activation	0.523	0.528	0.628
Subsequent weeks on welfare	99.72	102.2	0.234
Any paid employment in year $t - x$			
$t - 1$	0.341	0.340	0.899
$t - 2$	0.374	0.370	0.685
$t - 3$	0.341	0.341	0.946
Average weekly working hours in year $t - x$			
$t - 1$	4.62	4.86	0.129
$t - 2$	6.21	6.09	0.550
$t - 3$	5.87	5.78	0.648
Average monthly labor earnings in DKK in year $t - x$			
$t - 1$	3,186	3,306	0.306
$t - 2$	4,197	4,068	0.361
$t - 3$	4,064	3,996	0.648
Average no. of employers in year $t - x$			
$t - 1$	0.476	0.454	0.117
$t - 2$	0.523	0.527	0.793
$t - 3$	0.459	0.465	0.662

Note: Depicted are descriptive statistics for individuals who opened the treatment message separated for the message and the tool treatment. Percentage share unless indicated otherwise. *P*-values measured based on two-tailed t-tests on equal means.

Table A.2: ITTs of tool treatment on cumulated labor market outcomes

	Mean value control group	ITT of tool treatment	Relative effect size in %
Dependent variable: no. of months receiving other public benefit payments			
Any benefits excl. welfare	1.350	0.110 [0.002]	+8.18
Unemployment insurance benefits	0.221	-0.004 [0.820]	-1.8
Educational support	0.539	0.065 [0.005]	+12.1
Early retirement	0.151	-0.009 [0.523]	-6.0
Disability benefits	0.361	0.037 [0.070]	+10.2
Sickness benefits	0.174	-0.006 [0.653]	-3.4
Parental leave	0.127	-0.006 [0.639]	-4.7
Other benefits ^(a)	0.524	-0.007 [0.784]	-1.3
No. of observations		31,530	
Control variables		Yes	

Note: Depicted are intention-to-treat-effects of the tool treatment (relative to the control group) on the number of months receiving other types of public benefits within the first 12 months after the start of the intervention. Benefit categories are not mutually exclusive, i.e. in a given month the individual can receive different types of benefits. Control variables are depicted in Table 1.

^(a)Other benefits are typically paid in relation to ALMP programs, e.g. subsidized employment or training participation.