The Disparate Effects of Information Provision: A Field Experiment on the Work Incentives of Social Welfare

Sofie Cairo* **Robert Mahlstedt†**

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Abstract

Recipients of transfer payments must navigate intricate rules and regulations governing their incentives. This complexity carries the risk of prompting them to make privately suboptimal decisions and may, in turn, reduce overall welfare. Combining data from a large-scale scale field experiment and detailed administrative records, we investigate the labor market effects of informing social welfare recipients about their work incentives resulting from a minimum work requirement. Our intervention employs two treatments that provide (1) personalized and continually updated information on the relevant number of hours individuals have worked in the past and (2) generic information about the general rules governing their incentives. We find that the labor supply effects of these treatments differ significantly depending on individuals' personal situation at the start of the experiment. While the provision of personalized information increases employment among individuals who have not yet worked the required number of hours, generic notifications reduce the labor supply of those who face a low personal risk of financial sanctions due to non-compliance with the requirement in the near future.

Keywords: Social Welfare, Unemployment, Labor Supply, Field Experiments, Sanctions, Work Requirements, Information Interventions

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*Harvard Business School; scairo@hbs.edu

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

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1 Introduction

Understanding the economic incentives created by tax and transfer systems can pose significant challenges for individuals (see, among others, Altmann et al., 2022a; Chetty and Saez, 2013; Duflo et al., 2006; Liebman and Luttmer, 2012) and, thereby, increase their risk of making privately suboptimal decisions. This may in turn limit the effectiveness of public policies in achieving welfare improvements. As a consequence, researchers and policymakers have shown a growing interest in the use of low-cost strategies to address informational constraints and enhance individuals' decision-making. However, identifying effective interventions for correcting misperceptions can be daunting, as individuals' perception of incentives will vary with the nature and availability of information, the context in which it is presented, and in response to their personal situation (see, e.g., Chetty et al., 2009; Finkelstein, 2009; Liebman and Luttmer, 2015).

This paper presents the findings of a large-scale field experiment conducted among disadvantaged workers in Denmark who are dependent on social assistance.[1] As in many countries, the Danish welfare system provides time-unlimited benefit payments and imposes high implicit taxation on earnings. To mitigate the resulting work disincentives, individuals on welfare are required to work a minimum number of hours (i.e., they need to have worked 225 hours over the past 12 months) and face the risk of a financial sanction—a reduction in their monthly payments—if they fail to comply with the work requirement. In this context, we explore workers' labor supply responses to different types of information about the incentives that result from the work requirement. The randomized controlled trial, which we conducted among the universe of social welfare recipients, leverages the fact that public employment services in Denmark have, to a great extent, been transferred to a digital environment. This makes it possible to disseminate customized information to a large group of workers at low marginal costs (Belot et al., 2019; Altmann et al., 2022b; Bied et al., 2023; Le Barbanchon et al., 2023). At the same time, it allows us to exogenously vary the information individuals receive about the rules governing their incentives and their personal situation in relation to these rules.

In the first treatment arm, referred to as the *tool treatment*, benefit recipients gain access to a personalized online tool that offers continually updated personalized information about the key features of the policy that determines their individual work incentives. This includes the number of working hours they have accumulated within the preceding twelve-month period and their personal deadline for compliance with the work requirement. The tool is embedded

[1] Social assistance is designed to provide support to individuals without income who do not qualify for any other forms of social security benefits. This is often the case when their time-limited unemployment insurance (UI) benefits have already expired.

in the official online platform of the public employment service and is exclusively accessible to individuals assigned to the tool treatment during a period of six months following the start of the intervention. Moreover, we inform treated individuals about the availability of the personalized tool by sending them monthly notifications that include general information about the work requirement and a link to the online tool.

In a second treatment arm, referred to as the *message treatment*, benefit recipients receive notification messages that are almost identical to those of the individuals in the tool treatment, but workers assigned to the message treatment do not gain access to the online tool. This implies that they only receive generic information about the existence of the work requirement, the general risk of incurring a financial sanction and the associated rules, without any personalized information about their own situation. Finally, a third group of benefit recipients, assigned to the *baseline treatment*, is subject to a business-as-usual environment, which means they neither receive notification messages nor have access to the online tool.

By combining the data from our experiment with comprehensive administrative records, we study the labor market effects of our intervention over a period of up to one year following the start of the experiment. On average, the overall working hours and labor earnings of individuals assigned to the tool treatment (those who received notification messages and gained access to the online tool) are similar to those observed for the baseline group. However, at the same time, we show that personalized and generic information provoke very different labor supply responses, which exhibit significant heterogeneity depending on the personal situations of the workers in relation to the work requirement.

Among workers who are not in compliance with the requirement at the onset of the intervention, and consequently may perceive a heightened risk of facing sanctions in the upcoming months, the tool treatment increases employment and earnings by approximately 8.0% relative to comparable workers in the baseline group. This positive labor supply effect can be attributed to the availability of the personalized online tool, while the impact of the generic notification messages is relatively small and not statistically significant for the group of non-compliers. Evidently, these workers seem to deduce from the personalized information that their risk of a sanction is higher than they had initially assumed.

Several additional findings indicate that using the online tool enhances individuals' comprehension of their work incentives resulting from the dynamic nature of the requirement, where past hours worked continually expire. For example, the time profile of treatment effects reveals that those with access to the tool gradually adjust their labor supply in response to the additional information they receive over time. Moreover, the tool treatment not only encourages them to work more hours to ensure compliance, it also motivates them to transition from welfare to permanent full-time positions. Lastly, the effects of the tool treatment are less pronounced among individuals who have prior experience with the work requirement, suggesting that they already possess a greater understanding of the rules in the absence of the intervention. These patterns are consistent with the notion that the tool improves individuals' understanding of the benefit rules. At the same time, the observed positive labor supply effects might be reinforced by individuals developing an enhanced perception of being monitored due to the availability of the personalized tool.

In contrast to the effects of the tool treatment, we find that generic notification messages reduce the average levels of employment and earnings by about 5.5% and 5.7%, respectively. The negative labor supply effects are primarily concentrated among individuals who perceive a low personal risk of facing sanctions in the near future. This includes individuals who have previously worked a sufficient number of hours and those who are at the beginning of their benefit spell, and thus are not yet exposed to the risk of being sanctioned. Apparently, for these workers, the treatment messages act as reminders that they are not presently required to work additional hours, leading to a reduction of their labor supply.

Our findings contribute to a growing strand of the literature that suggests that workers face significant information frictions that have first-order effects on their labor market integration. In particular, our study complements recent evidence indicating that workers frequently lack essential information about the job search process (see, e.g., Altmann et al., 2022b; Conlon et al., 2018; Krueger and Mueller, 2016; Mueller et al., 2021), potential job matches (see, e.g., Belot et al., 2019; Jäger et al., 2022), and the social security system (see, e.g., Liebman and Luttmer, 2012). While previous experimental studies have made efforts to alleviate these information constraints, our results underline the key factors that influence the labor market effects of information provision: Specifically, (1) the type of information provided, (2) the method of distribution and (3) individuals' personal situations all play significant roles in determining the labor market effects of information provision.

In various contexts, interventions provide advice that is generic to all workers. For example, several studies have explored the effects of regular letters, emails and brochures providing general information on topics such as retirement decisions (Liebman and Luttmer, 2015), job search strategies (Altmann et al., 2018), training programs (Van den Berg et al., 2023), tax credits (Bhargava and Manoli, 2015) and part-time employment (Benghalem et al., 2023). Our findings reveal that simple notification messages can have adverse employment effects on certain groups of workers. This adds to an expanding body of research documenting that incentives, or in our case providing information about them, can sometimes be counterproductive and trigger unintended behavioral responses (see, e.g., Bowles and Polania-Reyes, 2012; Frey and Jegen, 2001; Gneezy et al., 2011, for overviews). In our specific context, the binary nature of the work requirement may lead to strategic behavior, as individuals strive to adhere strictly to the legally required minimum number of hours. Our findings suggest that this inclination is further reinforced by the generic notifications, possibly because the treatment messages make the threshold of 225 working hours more salient.[2]

While a few existing studies have examined the effects of delivering personalized information, the provision of tailored advice can be very costly, especially when it relies on professional one-to-one counseling. For instance, Bettinger et al. (2012), Chetty and Saez (2013), and Duflo et al. (2006) study the provision of personalized information about tax credits and college aid through individual consultations, whereas Fuentes et al. (2022) explore the effects of a personalized pension simulation program, the availability of which is limited to access within governmental offices. In a study conducted in parallel to ours, Altmann et al. (2022a) utilize a personalized digital tool to inform recipients of time-limited unemployment insurance (UI) benefits in Denmark about their potential benefit duration. Their intervention focuses on job seekers with a stronger attachment to the labor market than the recipients of social assistance in our study and informs them about a distinct set of rules and incentives. Despite these differences, their analysis uncovers heterogeneous labor market effects based on individuals' initial knowledge and beliefs, their personal employment prospects, and the timing of the intervention—factors that also prove to be of primary importance in our setting.

Our study underscores the effectiveness of digital tools in reducing informational constraints among disadvantaged workers by providing personalized and up-to-date information on demand. While the marginal costs per user remain low, the development and maintenance of digital infrastructure that is capable of tracking individual outcome data requires a substantial financial investment. Nonetheless, our findings indicate that such an investment can be beneficial to individual users and has the potential to improve overall welfare. However, the context-specific nature of behavioral responses to incentives emphasizes the importance of understanding which workers truly benefit from the information provided, and which methods of communicating the rules are most effective in achieving these welfare gains.

On a more general level, our paper contributes to the literature on the effects of search requirements and benefit sanctions that are aimed at motivating unemployed workers to increase

[2] A related empirical literature has documented bunching with respect to individuals' labor supply at kink points generated by tax and transfer programs (see, e.g., Saez, 2010; Chetty et al., 2011; Kleven, 2016; Bitler et al., 2021).

their labor market participation. Theoretically, such restrictive policies can lead to welfare improvements compared to a benefit system without monitoring (Boone et al., 2007; Kreiner and Tranæs, 2005; Pavoni and Violante, 2007). Consistent with these ideas, empirical evidence demonstrates that imposing job search requirements (Petrongolo, 2009; Manning, 2009; Lammers et al., 2013; Arni and Schiprowski, 2019) and enforcing benefit sanctions (Abbring et al., 2005; Lalive et al., 2005, 2006; Van den Berg et al., 2004) can stimulate exits from registered unemployment. In this context, our study offers new insights by examining a system with a requirement to work a minimum number of hours. Our information treatment emphasizes the key features of this policy and encourages individuals, who are likely to perceive the threat of being sanctioned, to exit the welfare system. Notably, our intervention not only motivates treated individuals to return to paid employment but also to explore alternative transfer programs, such as disability or educational benefits, which are not subject to the work requirement. This finding aligns with the notion of using work requirements as screening devices to improve the targeting of transfer payments (Besley and Coate, 1992; Nichols et al., 1971; Nichols and Zeckhauser, 1982).

The rest of the paper is structured as follows: The next section provides an overview of the relevant features of the Danish welfare system. Section 3 outlines the design of our experiment, while Section 4 discusses the potential effects of our intervention based on a labor supply framework. Moving on, Section 5 presents the main results of our empirical analysis. Finally, we discuss the implications of our findings in Section 6.

2 The Danish Welfare System

Social welfare provides a safety net for unemployed workers without any personal wealth and who are not entitled to unemployment insurance (UI) benefits. Social assistance benefits are means-tested and individuals can receive them for an unlimited period of time. The level of benefits provided depends on various factors such as age, the presence of children in the household, and the income and employment status of a spouse if there is one. For individuals aged 30 and above without children, the monthly benefits amount to DKK 11,554 ($\approx$ USD 1,680, 2020-level). However, if there are minors living in the household, the benefits increase to DKK 15,355 ($\approx$ USD 2,230, 2020-level). Younger beneficiaries below the age of 30 without children receive approximately 65% of the baseline benefit level, while younger recipients with children receive around 96% of the baseline amount.[3]

[3] In cases where the benefit recipient has a working spouse, the benefit level may be adjusted to ensure that the total gross household income does not exceed two times the benefit level of the individual recipient.

The Danish welfare system offers limited work incentives for those claiming welfare benefits, as benefit payments are reduced by one kroner for every kroner earned, with only a small deduction of DKK 28 (≈ 4 USD) per working hour.[4] Simultaneously, individuals who have received benefit payments for a minimum of 12 months within the past three years must adhere to a work requirement. This entails that individuals only remain eligible for the full-rate of benefits if they have worked at least 225 hours in a non-subsidized job during the last 12 months. To meet the requirement, a benefit recipient can opt to work five hours per week consistently throughout the year or work full-time for approximately seven weeks within the year. The distribution of working hours is left to the individual's discretion, but it is crucial to understand that the criteria must be met at any given point when considering working hours accumulated within the preceding twelve-month period. Any hours worked beyond the 12-month window do not count towards fulfilling the requirement, which means that individuals who meet the criteria in one month might face a reduction in benefits in the following month. Moreover, should individuals re-enter the welfare system after a short period of employment, they remain subject to the same work requirement, and the count of working hours they previously accumulated is retained (along with any additional hours they worked during their employment).

In cases where benefit recipients fail to comply with the work requirement, the welfare administration imposes financial sanctions, resulting in a reduction of monthly benefit payments of approximately DKK 500 to 1,000 (≈ 70-140 USD). The exact amount of the sanction is determined by objective criteria accounting for age and family status of the individual. This benefit reduction can only be imposed if the individual has received benefit payments for at least 12 months within the last three years. Once a sanction has been applied, benefits remain at the lower monthly level and individuals can only regain benefits at the full rate after fulfilling the work requirement of at least 225 hours. The benefit rules clearly outline the conditions for imposing a sanction, but caseworkers have the authority to grant temporary exemptions from the work requirement in cases where they determine that the individual is unable to work at least five hours per week due to mental or physical constraints.[5] Furthermore, individuals are also exempted from the work requirement during periods of illness or parental leave, which can postpone the imposition of a sanction.

It is worth noting that similar policies are in place in numerous countries. For example, the US (see, e.g., Bloom and Michalopoulos, 2001; Grogger and Karoly, 2009; Moffitt, 2003) and

[4]As a result, a benefit recipient who works part-time at the minimum hourly wage rate of around DKK 130 (≈ 19 USD) faces an implicit marginal tax rate of approximately 78%, which rises further with higher hourly wages.

[5]Between March 2017 and September 2019, approximately 26% of benefit recipients were exempted from the requirement in any given month, while 15-18% of those subject to the requirement received benefits at a reduced level during that period.

Canada (Berg and Gabel, 2015) introduced work requirements combined with benefit sanctions as early as the 1990s. Additionally, in several European countries, benefit entitlements are directly linked to comparable requirements, such as the obligation to apply for a minimum number of jobs (see, e.g., Arni and Schiprowski, 2019; Petrongolo, 2009; Manning, 2009), or to engage in specific work activities (see also Venn, 2012, for an overview).

3 Study Design

To study how the provision of information about work incentives affects the labor market reintegration of social welfare recipients, we combine data from a countrywide randomized controlled trial and administrative data from the Danish social security records. The experiment commenced in August 2018 and focused on benefit recipients who were subject to the work requirement during that period. The central components of our information intervention include a personalized online tool and generic notification messages, which are elaborated upon in the subsequent sections.

3.1 Information provision at the status quo

Before outlining the experimental design, we briefly discuss the availability of information in the absence of our intervention. By default, all benefit recipients receive an official notification from the welfare administration, when they have received benefits for about six months. The notification letter provides recipients with basic information about the rules associated with the work requirement and their potential reduction date, that is, the date when they will incur a financial sanction if they do not meet the requirement. This implies that all welfare recipients who are at risk of being sanctioned if they fail to work a sufficient number of hours have already received a notification about the possibility of a permanent benefit reduction. However, despite this official notification, the continual expiration of working hours might make it challenging for individuals to fully comprehend their personal incentives resulting from the work requirement. Moreover, benefit recipients may face limitations in accessing real-time information regarding their personal situation in relation to the work requirement. While individuals can manually track their working hours over the past year or request their caseworker to access an administrative database for the relevant information, both options demand considerable effort. Simultaneously, individuals may not be fully aware of the significance of monitoring their working hours nor of the possibility to contact their caseworker for this purpose.

Lastly, there is suggestive evidence indicating that caseworkers who conduct regular meetings with benefit recipients encounter challenges in providing tailored information concerning the work requirement based on individual circumstances. For instance, a survey conducted in November 2016 found that 47% of the 137 surveyed caseworkers reported having no means of adequately supporting individuals subject to the requirement. Moreover, 71% of them cited capacity constraints, and only 7% stated that they received sufficient IT support related to the work requirement (see Danish Association of Social Workers, 2017). In this context, it is important to note that caseworkers are responsible for assisting job seekers in their job search, while financial sanctions are imposed by the welfare administration, which lacks direct personal contact with individual benefit recipients. This situation can contribute to substantial uncertainty among individuals about the potential risk of facing a reduction in their benefit level due to non-compliance with the work requirement.

3.2 Randomized controlled trial

In the randomized field experiment, we exogenously varied the information provided to workers by dividing the universe of welfare benefit recipients into three equally sized groups. These groups differed in terms of their access to information about the work requirement.

Tool treatment: The first group of individuals, referred to as the *tool group*, was granted access to an online tool that offered personalized information regarding their situation in relation to the work requirement. This online tool, depicted in Figure A.1 in Appendix A.1, is integrated into the official online platform of the Danish public employment service *jobnet.dk* and was exclusively accessible to individuals assigned to the tool group during the initial six months of the experiment. The tool provides customized details about the number of hours individuals have worked within the last 12 months, the remaining hours they need to accumulate, and the potential date when their benefit payments might be reduced. As a result, individuals assigned to the tool treatment had access to real-time feedback, which was continually updated whenever they worked additional hours or when previously accumulated hours expired.

Moreover, to make treated individuals aware of the existence of the tool, they received up to six monthly notification messages. These messages did not contain personalized information. Rather, they informed individuals about the existence of the work requirement, the total number of working hours required for compliance, and emphasized the potential risk of facing a benefit reduction. In addition, the notifications also included specific examples of how individuals could meet the work requirement, serving as practical references for individuals to compare with their own work activities.[6]

[6] Specifically, the messages emphasized that the requirement of 225 working hours over a year is equivalent to the following options: (i) working 5 hours a week for 52 weeks, (ii) working 10 hours a week for 23 weeks, (iii) working 20 hours a week for 12 weeks or (iv) working 37 hours a week for 7 weeks.

Message treatment: The second group of individuals, referred to as the *message group*, received notifications that were nearly identical to those received by the tool group. However, unlike the tool group, the message group did not have access to the online tool, and their notifications did not include the corresponding link. Consequently, individuals in the message group were informed about the general rules governing their incentives but did not receive any personalized information about their specific situation in relation to the work requirement. By comparing the outcomes of individuals in the tool and message groups, we can effectively isolate the impact of providing access to personalized information through the online tool.

Baseline treatment: Finally, a third group of individuals, referred to as the *baseline group*, were not contacted at all. They experienced business-as-usual and did not receive the general notifications, nor were they granted access to the online tool. The presence of this baseline control group enables us to assess the effects of providing personalized and generic information in comparison to the status quo.

3.3 Procedures, data, and descriptive statistics

All welfare benefit recipients who were subject to the work requirement as of August 15, 2018, were randomly assigned to one of the three treatment arms (as described in Section 3.2) by the Danish public employment service. The randomization process was stratified based on two groups of benefit recipients who differed in their caseworkers' assessment of their capability to commence full-time employment. Both groups are subject to the work requirement, but they differ in terms of their overall labor market prospects and the support they receive from their respective caseworkers. This stratification ensures a balanced distribution of treatment among the distinct caseworker-assessed groups.

On the same day, August 15, 2018, the online tool was activated for individuals assigned to the tool treatment, and the initial treatment messages were sent to both the tool and message groups. Subsequently, up to five monthly reminders were delivered to these groups as long as they remained subject to the work requirement. All messages were sent by the public employment service to the individual's inbox at the official online portal (*jobnet.dk*), where the online tool was also embedded. After six months, the online tool was activated for all social welfare recipients subject to the work requirement. As a result, individuals in the message and baseline groups could potentially access the online tool beyond the initial six-month period, which could lead to the mitigation of treatment differences over time. To examine this possibility, we present treatment differences over various time horizons. In this context, it is important to note that only individuals assigned to the tool group were explicitly encouraged to utilize the online tool through the treatment messages.

We have linked the experimental data to the comprehensive register data administered by Statistics Denmark. By doing so, we gain access to detailed information on the socio-demographic background characteristics, benefit payments, income, and employment status of the individuals in our sample.

Table 1: Summary statistics and balancing tests

	Mean value by treatment status			
	Tool group	Message group	Baseline group	Balancing stat. p-value
No. of observations	15,761	15,764	15,769	
Female	0.500	0.491	0.495	0.256
Married	0.176	0.171	0.172	0.466
Education				
Less than high school	0.034	0.033	0.033	0.804
High school	0.582	0.589	0.587	0.436
Bachelor's degree or equivalent	0.271	0.266	0.269	0.528
Master's degree or equivalent	0.088	0.089	0.086	0.564
Age in years	37.67	37.68	37.67	0.999
Migration background				
1st generation	0.036	0.037	0.037	0.917
2nd generation	0.250	0.252	0.246	0.505
Living in Capital Region	0.320	0.324	0.326	0.566
Children				
One child	0.154	0.154	0.151	0.789
Two children	0.112	0.105	0.109	0.117
Three or more children	0.115	0.118	0.120	0.392
Not deemed capable of full-time employment	0.712	0.712	0.712	1.000
Elapsed benefit duration in weeks	135.1	134.1	134.5	0.813
Pre-intervention outcomes (in previous year)				
Any paid employment	0.215	0.210	0.216	0.305
Total working hours	144.5	138.2	144.4	0.277
Labor earnings, DKK	21,912	21,552	22,368	0.478
Any benefit reduction	0.112	0.111	0.114	0.752
Exempted from requirement	0.346	0.352	0.344	0.347

Note: The table reports summary statistics of background characteristics among individuals assigned to the three treatment groups. Percentage shares are reported unless otherwise indicated. $P-$values are based on F-tests for joint significance of treatment indicators.

Table 1 presents a summary of the characteristics of the experimental population, grouped according to the three treatment arms. On average, individuals in our sample are 38 years old, with approximately 50% of participants being female. About 17% of the individuals are married, and 36% hold a university degree. Moreover, a majority of social welfare recipients belong to a disadvantaged group of workers who are disconnected from the labor market. For instance, around 71% of the experimental population, as assessed by caseworkers, are considered incapable of starting full-time employment without further support. Additionally, only 21% had any paid employment in the year before the intervention.[7]

Furthermore, our analysis shows no evidence of imperfect randomization, as the background characteristics of individuals are well-balanced across the treatment groups. We further investigate the extent to which individuals' background characteristics collectively predict their treatment status, as shown in Table A.1. The results reveal that individual characteristics have minimal explanatory power (as indicated by the p-values at the bottom of Table A.1). This reinforces our confidence in the assumption that we can effectively identify causal treatment effects by comparing the outcomes of individuals assigned to the three treatment groups.

4 Theoretical Considerations

Before we present the results of our experiment, we sketch the work incentives of welfare recipients in a stylized labor supply framework and discuss the potential effects of providing individuals with generic and personalized information through the lens of this framework.

4.1 Work incentives of social welfare

We consider workers who decide how many hours, ℓ_t, they would like to work in a given month, t. They care about consumption, c_t, and leisure time, ℓ_t, such that their period utility is given by $v(c_t) - \ell_t$, where the function v is increasing and concave in c_t. We ignore any search-theoretic, or related issues, and assume that individuals can freely choose how many hours they would like to work at an exogenous wage rate, w. Social welfare ensures a minimum income, b, and, for expositional simplicity, we assume that benefits are reduced by one kroner for each kroner earned. At the same time, benefit payments are reduced by a fixed amount, p, if individuals have worked less than the required minimum number of hours, $\bar{\ell}$, over the preceding twelve-month period. Hence, the consumption level can be described as follows:

$$c_t = \max[w\ell_t, b - \mu(t)p] \tag{1}$$

depending either on the work income generated in period t, $w\ell_t$, or the benefit payments, where $\mu(t)$ indicates whether a financial sanction is imposed in period t. For a given discount rate, δ, the expected present value of income is given by:

$$U(t) = \max_{\ell_t} \left\{ v(c_t) - \ell_t + \delta U(t+1) \right\}. \tag{2}$$

[7] For further context, Table A.2 in the Appendix offers summary statistics comparing the experimental population (i.e., the group of social welfare recipients) to unemployed workers who receive unemployment insurance (UI) benefits. Despite the relatively high level of education in our experimental population, their levels of employment and earnings remained considerably lower over the previous years compared to the group of UI benefit recipients.

The individuals' optimal labor supply decision trades-off their forgone leisure time against the returns to work, which depend on the wage that they can earn, w, and on how their current labor supply affects their prospects of avoiding a financial sanction.

4.2 Potential effects of the information intervention

While our intervention does not alter the actual monetary incentives to work, it may influence individuals' perceptions of benefit rules and their personal circumstances. To gain insights into the resulting labor supply effects, it is useful to consider the perceived likelihood that a benefit sanction is imposed in period $t+1$:

$$\widehat{\mu}(t+1) = Pr\left(\ell_t + \varsigma < \bar{\ell} - \ell_0\right), \tag{3}$$

which depends on the current labor supply, ℓ_t, but also on individuals' subjective beliefs about the overall number of working hours that is required to avoid a sanction, $\bar{\ell}$, and the number of hours they have worked in the past, ℓ_0. Moreover, ς denotes a random variable capturing any uncertainty about these aspects or about whether a sanction will actually be enforced by the authorities in case of non-compliance with the requirement. The tool and message treatments may trigger behavioral responses among individuals who are imperfectly informed about these aspects by changing their subjective risk of incurring a financial sanction. At the same time, we expect systematic heterogeneities among different groups of welfare benefit recipients (see also Bitler et al., 2006; Kline and Tartari, 2016). In the following, we outline the various mechanisms, which are also summarized in Table 2, along with an overview of the proxies used in our empirical analysis to assess the significance of the corresponding mechanisms.

Perception of past labor supply: To begin with, Equation (3) highlights that the work incentives for a given period t are contingent upon the number of working hours individuals have accumulated at the onset of that period, ℓ_0. The online tool, in turn, offers accurate information about this specific aspect. Consequently, individuals who gain access to the online tool may adjust their subjective beliefs regarding the number of additional hours needed to meet the target or whether they are already in compliance with the requirement (i.e., the right-hand side of Equation (3)). As a result, benefit recipients who learn from our intervention that their past working hours fall short of the target may increase their labor supply. Conversely, we expect opposite effects among those who infer from the online tool that they are already complying with the requirement.

While this is evident for the online tool, one may also expect heterogeneous responses to the generic notifications. Specifically, the treatment messages provide individuals with information

Table 2: Predicted labor supply effects of information treatment

Mechanism	Labor supply effect		Feature	Empirical proxy
(1) Perception of past labor supply	+	On non-compliers who worked less than 225 hours at onset	Message or tool	Heterogeneity by working hours accumulated at onset of intervention
	−	On compliers who worked more than 225 hours at onset		
(2) Existence and schedule of benefit sanctions	+	On short-term benefit recipients unaware of requirement	Message	Heterogeneity by elapsed benefit duration at onset of intervention
	−	On short-term benefit recipients learning about sanction schedule		
(3) Continuous sanction threat and monitoring	+	On individuals unaware of dynamic requirement	Tool	Exits from welfare system Time profile of treatment effects
	+	On individuals unaware of monitoring		Heterogeneity by prior exposure to benefit sanction or exemption

Note: The table presents forecasts of the labor supply effects of our intervention, triggered by different mechanisms (first column), and affecting distinct groups of welfare benefit recipients (second column). In the third column, we identify the specific feature of the intervention (e.g., the notification message or the online tool) that is anticipated to trigger each mechanism. The fourth column provides an overview of the proxies used in our empirical analysis to evaluate the significance of each corresponding mechanism.

about the exact number of overall working hours required, $\bar{\ell}$, and emphasize that working five hours per week throughout the year or working for about seven weeks in a full-time job will suffice to avoid a benefit reduction. By comparing these examples with their own work activities, individuals may reassess their personal risk of facing a benefit sanction and the number of additional hours they would need to work to meet the target. Against this backdrop, our empirical analysis accounts for heterogeneous treatment effects concerning individuals' accumulated working hours at the onset of the intervention. We anticipate that individuals who have worked more (less) hours than the stipulated requirement in the past may decrease (increase) their labor supply.

Existence and schedule of benefit sanctions: Apart from influencing individuals' understanding of their personal situation, the notification messages may also enhance their basic knowledge of the rules associated with the requirement. While some benefit recipients might be unaware of the work requirement altogether, others may lack knowledge about the specific schedule of benefit sanctions. It is crucial to note that financial sanctions can only be imposed after individuals have received benefit payments for at least 12 months within the last three years. Hence, individuals at the beginning of their benefit spell are not yet exposed to the risk of sanctions in the upcoming months. Simultaneously, the administration informs individuals about the rules related to the work requirement only after they have been receiving welfare benefits for approximately six months. Given this context, we anticipate that the message treatment, containing similar information to the official notifications, will primarily impact short-term benefit recipients within the first six months of their welfare benefit spell. However, the direction of their labor supply response depends on which aspect of the benefit rules they misjudge at the onset of the intervention. Individuals who were previously unaware of the requirement should increase their labor supply. Conversely, we expect the opposite effect on short-term benefit recipients who learn from our intervention that they cannot face sanctions during the first year of their benefit spell.

Continuous sanction threat and monitoring: Lastly, individuals may gradually improve their comprehension of their work incentives by utilizing the online tool. On the one hand, they may understand the dynamic nature of the requirement, which means that compliance is contingent on the total labor supply over the preceding twelve-month period and that past hours worked expire continually. This aspect implies relatively strong incentives to work, even when the risk of being sanctioned in period $t+1$ is low, because any additional hour worked in the current period reduces the risk of facing a sanction throughout the following twelve-month period. Access to the continually updated and personalized online tool could prompt individuals to recognize this dynamic aspect. Consequently, those assigned to the tool treatment may perceive greater incentives to work compared to their non-treated counterparts, who may only consider the short-run consequences of their labor supply decisions.[8] On the other hand, benefit recipients who have access to the tool may become aware that their work incentives are closely monitored by the administration. In such cases, treated individuals may deduce that non-compliance with the requirement will be promptly detected, and the likelihood of sanctions being enforced is higher than initially assumed (i.e., an increased value of ς in Equation (3)).

Both mechanisms may not only encourage treated individuals to increase their labor supply, but also to leave the welfare system and, for example, seek permanent full-time employment. Moreover, individuals who use the tool may gradually adapt their labor supply, as it might require a certain period for them to develop a more profound understanding of the welfare system. Finally, it appears reasonable to assume that these effects could be less pronounced for individuals who have previously encountered the work requirement, as they may already comprehend these factors in the absence of the intervention. Therefore, in our empirical analysis, we identify workers who have previously been exposed to the work requirement, either through

[8]This can be seen from the first-order condition of Equation 2. A forward-looking agent who recognizes the dynamic nature of the requirement accounts for the fact that $\ell_0(t) = \ell_{t-1} + ... + \ell_{t-11}$. Conversely, a myopic agent who only thinks one period ahead would consider the hours she worked in the past, ℓ_0, as being exogenously given when deciding about her work effort in period t.

(1) experiencing a reduction in benefits due to non-compliance or (2) being officially exempted from the requirement.

5 Empirical Analysis

In this section, we summarize the main results of our experiment. To document the impact of personalized and generic information on labor market outcomes, we estimate regressions of the following form:

$$Y_i = \beta_0 + \mu M_i + \theta T_i + X_i \beta_1 + \varepsilon_i, \tag{4}$$

where M_i and T_i indicate whether individuals were assigned to the message and tool treatments, respectively, and X_i represents a vector of pre-intervention control variables (i.e., socio-demographic characteristics, labor market histories and fixed effects for individuals' municipality).[9] The coefficients μ (message treatment) and θ (tool treatment) identify treatment effects relative to the baseline group. As the main outcome variable of interest, Y_i, we consider individuals' working hours and labor earnings accumulated over twelve months after the start of the experiment. The choice of these outcomes is motivated by the benefit rules requiring individuals to have worked at least 225 hours over the course of one year.

In addition to the average effects of the tool and message treatments in the overall sample, we also consider group-specific treatment effects, as the intervention is expected to provoke disparate responses among different subgroups of social welfare recipients. Specifically, individuals may react differently depending on their personal situation in relation to the work requirement. To account for this, we first distinguish between (1) workers who are not in compliance with the requirement at the onset of the intervention, that is, they have accumulated less than 225 working hours within the preceding twelve-month period (henceforth denoted as *non-compliers*) and (2) workers who have worked more than the required number of hours within this period (henceforth denoted as *compliers*). It is important to note that this distinction refers to compliance with the work requirement at the start of the experiment and, consequently, the two groups should differ regarding their short-run sanction risk when being on welfare benefits. At the same time, even individuals who comply with the requirement at this point in time are at risk of incurring a sanction in the longer run, because hours accumulated in the past cease to count after 12 months.

Throughout the analysis, we focus on intention-to-treat effects (ITTs) and ignore whether the treated individuals have actually opened the treatment messages or clicked on the link to the

[9] Additionally, Table A.5 in the Appendix reports the results from specifications without control variables showing that our main set of results remains very similar when dropping all covariates.

information tool. To gain a sense of the first-stage effects of our intervention on exposure to the additional information, it is instructive to consider individual-level click data. This shows that around 36.6% of all the treated individuals opened at least one of the messages they received, while 9.6% clicked on the link to the online tool provided in the treatment messages at least once within a year after the intervention. However, deriving local average treatment effects is not straightforward in our setup, as exposure to the tool treatment may already commence when individuals open the treatment message, and they can access the tool directly through the online portal without clicking on the link provided in the messages. Furthermore, Table A.3 in the Appendix demonstrates that individuals who responded to our intervention by reading the messages or accessing the online tool tend to have higher levels of education and were more closely attached to the labor market in the past compared to the average recipient of social welfare benefits.[10]

5.1 Does the intervention alter workers' labor market outcomes?

Table 3 summarizes the effects of our intervention on overall working hours and earnings accumulated within one year after the start of the experiment. We observe that the workers' responses to our intervention differ significantly based on their compliance with the work requirement at the onset of the intervention. Moreover, we find disparate effects of the tool and message treatments on individuals' labor market integration.

Effects of tool treatment: We start by discussing the differences between the tool group, who received notification messages and gained access to the online tool, and the baseline group, who received none of the additional information. As depicted in specifications (1) and (4), the tool treatment does not exhibit a significant effect on the labor market outcomes of the average recipient of social welfare benefits in our sample. The point estimates for the effects on working hours and labor earnings accumulated over one year are small and statistically insignificant in the overall sample. However, upon considering outcomes separately for subgroups that differ in terms of the number of hours accumulated in the past and their compliance with the requirement, we find that the seemingly small effects in the overall sample conceal significant and pronounced heterogeneous responses.

Among the group of non-compliers, who have worked less than 225 hours in the past, individuals assigned to tool treatment group work, on average, 7.0 hours more ($p = 0.038$; column 2) and earn about DKK1,059 more ($p = 0.046$; column 5) than those in the baseline group. These

[10]However, it can be seen in Table A.2 in the Appendix that treated individuals who opened the messages or clicked on the link to the online tool had notably lower levels of employment and earnings in the past compared to the group of UI benefit recipients, who are more closely attached to the labor market.

Table 3: Effects of information treatments on labor market outcomes

Dependent variable	**Total working hours** (within twelve months)			
	Overall sample	Non-Compliers$^{(a)}$	Compliers$^{(a)}$	Difference
	(1)	(2)	(3)	(3) – (2)
Treatment status (ref. baseline group)				
Tool treatment	1.56	7.02	-24.66	-31.69
	[0.673]	[0.038]	[0.092]	[0.002]
Message treatment	-8.31	-4.23	-32.29	-28.06
	[0.026]	[0.210]	[0.028]	[0.005]
No. of observations	47,294	39,478	7,816	
P-value (tool = message)	0.008	0.001	0.605	0.719
Mean dep. variable	150.11	87.24	467.62	
Dependent variable	**Total labor earnings** (DKK, within twelve months)			
	Overall sample	Non-compliers$^{(a)}$	Compliers$^{(a)}$	Difference
	(4)	(5)	(6)	(6) – (5)
Treatment status (ref. baseline group)				
Tool treatment	53	1,059	-4,917	-5,976
	[0.929]	[0.046]	[0.043]	[0.000]
Message treatment	-1,319	-646	-5,099	-4,453
	[0.028]	[0.224]	[0.037]	[0.006]
No. of observations	47,294	39,478	7,816	
P-value (tool = message)	0.022	0.001	0.941	0.348
Mean dep. variable	23,100	13,114	73,537	

Note: The table reports treatment differences in working hours and labor earnings accumulated over the course of 12 months after the start of the experiment among participants in the randomized controlled trial. Depicted are the effects of the tool and message treatments relative to the baseline group. $P-$values are shown in square brackets. In all specifications, we control for covariates as depicted in Table 1. Specifications (1) and (4) present average treatment differences in the overall sample.

$^{(a)}$Specifications (2), (3), (5) and (6) present separate treatment effects on (i) individuals who do not comply with the requirement at the onset of the intervention (i.e. they have worked less than 225 hours within the preceding twelve-month period) and (ii) individuals who do comply with the requirement at the onset of the intervention (they have worked 225 or more hours within the preceding twelve-month period).

figures correspond to relative employment and earnings increases of about 8.0% when comparing the treatment coefficients to the average levels of employment and earnings among the corresponding workers in the baseline group. Conversely, we find negative employment and earnings effects among individuals who are in compliance with the requirement at the onset of the intervention. For this group of workers, overall working hours decrease by about 5.2% ($p = 0.092$; column 3) and labor earnings decrease by approximately 6.7% ($p = 0.043$; column 6) relative to the baseline group. Moreover, the treatment effects on both outcomes differ systematically from the positive effects observed among the group of non-compliers ($p = 0.002$ and $p < 0.001$, respectively).

Effects of message treatment: In contrast to the tool treatment, we observe that the message treatment has a negative and statistically significant impact on the labor market outcomes of the average individual in our overall sample. Over the course of one year after the beginning of the intervention, individuals assigned to the message group work, on average, about 8.3 hours less ($p = 0.026$) and earn about DKK1,319 less ($p = 0.028$) than those in the baseline group. These estimates correspond to relative decreases of employment and earnings of about 5.5% and 5.7%, respectively. Moreover, the working hours and earnings of the message group in the overall sample lie significantly below the corresponding outcomes of individuals assigned to the tool group ($p = 0.008$ and $p = 0.022$, respectively; see post-estimation test in Table 3).

At the same time, we observe substantial heterogeneity regarding the effects of the message treatment as well. Specifically, the negative employment effects are primarily concentrated among a relatively small group of compliers, that is, workers who have worked more than the required number of hours in the past. The message treatment reduces their overall working hours ($p = 0.028$; column 3) and earnings ($p = 0.037$; column 6) by about 6.9% relative to the average of comparable individuals in the baseline group. In contrast, the effects of the message treatment on the larger group of non-compliers are small and statistically insignificant at conventional levels (see columns 2 and 5).

It is important to highlight that we observe similar negative employment and earnings effects of both the message and tool treatments among workers who are in compliance with the requirement at the start of the experiment. This suggests that the adverse labor market effects of the intervention are primarily driven by the generic notification messages rather than the personalized online tool. Both treatment arms received comparable messages, with the sole difference being that the tool group also received a link to the online tool. Thus, it appears that the negative impact on labor market outcomes is mainly influenced by the generic notifications. Conversely, the positive employment and earnings effects observed among the non-compliers appear to be attributable to the availability of the personalized online tool.

Time profiles: Besides the outcomes accumulated over the one-year period, Figure A.2 in the Appendix displays the time profiles of treatment effects on monthly outcomes over 24 months after the start of the intervention. Panel A.1 of Figure A.2 illustrates that the tool treatment has positive impacts on both employment and earnings of non-compliers that increase during the initial year after the start of intervention. This observation aligns with the notion that individuals who use the online tool gradually improve their understanding of the benefit rules—perhaps recognizing the dynamic aspect of the work requirement—and adjust their labor supply accordingly. However, during the second year, the positive effect of the tool treatment diminishes.[11] Conversely, Panel B.2 of Figure A.2 unveils that the negative labor supply effects of the message treatment on the group of benefit recipients who were complying with the requirement at the start of the intervention become evident immediately and persist over time.

5.2 Job characteristics

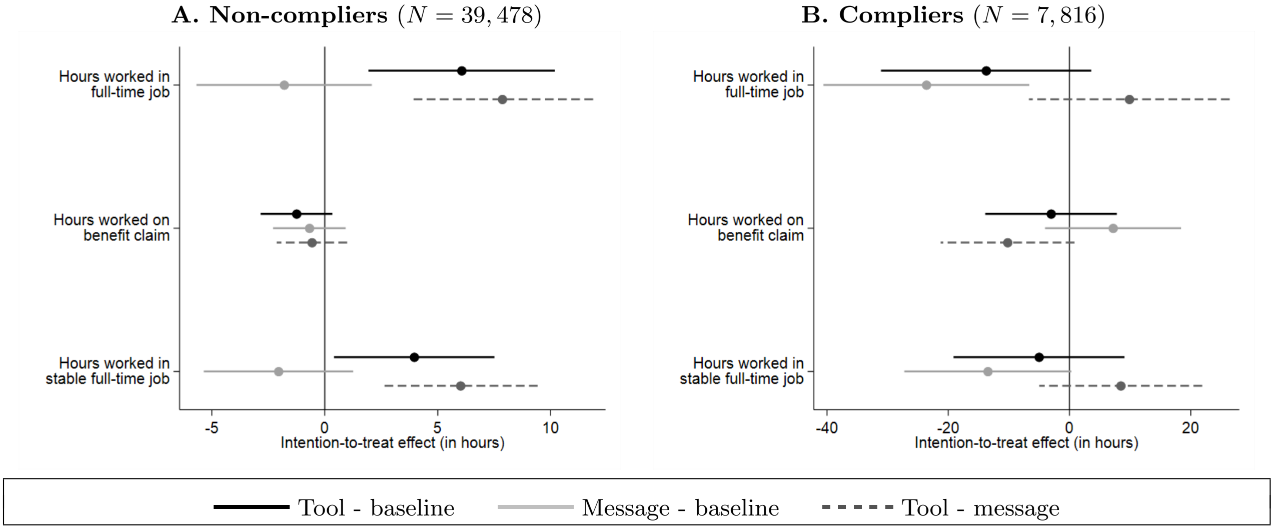
To further explore the origins of the differential treatment effects on overall working hours and earnings, we now examine the nature of resulting job matches more closely. The benefit rules imply that individuals can fulfill the requirement by working only a few hours per week (e.g., accumulating five working hours per week throughout the year would suffice). However, at the same time, the requirement also creates a constant sanction risk as previously accumulated hours continually expire. As a result, individuals who understand this aspect may perceive stronger incentives to leave welfare and secure a permanent full-time job. With this in mind, we proceed to estimate treatment effects on working hours accumulated in different "types" of jobs. Specifically, we distinguish between hours worked in full-time employment, which is defined as working at least 37 hours per week for the entire month, and part-time work opportunities that benefit recipients take up while still claiming welfare benefits.

As illustrated in Figure 1, the overall employment effects of the intervention (cf. Table 3) are closely associated with the uptake of full-time employment (top Panel in Figure 1). For instance, among individuals who had previously worked fewer hours than required, we observe that those assigned to the tool treatment work approximately 6.1 hours more (+15.6%; $p = 0.016$) in full-time jobs over the span of one year compared to similar workers in the baseline group. On the other hand, the message treatment reduces full-time employment by 23.6 hours (-13.3%; $p = 0.022$) over the course of one year among the group of compliers. Notably, we do not find any statistically significant evidence that our intervention affects the uptake of part-time employment while still claiming welfare benefits (middle Panel in Figure 1).

This pattern is intriguing, considering that less than two months of full-time employment would suffice to comply with the requirement and eliminate the risk of a benefit sanction for the following ten-month period. Keeping this in mind, we also explore the treatment effects on hours worked in stable jobs (bottom Panel in Figure 1), which refers to full-time employment relationships that last for at least six months with the same employer. Remarkably, it appears that, for those who are not yet in compliance, more than half of the overall difference in working hours between the tool and baseline groups can be attributed to full-time employment relationships that last for at least six months (+4.0 hours; $p = 0.067$). This suggests that the

[11] This reduction seems plausible, given that the online tool was made available to all benefit recipients after the first six months. At the same time, it is worth noting that the work requirement was temporarily suspended due to COVID-related restrictions in spring 2020, specifically from month 21 after the start of the intervention.

Figure 1: Effects of information treatment on job characteristics



Note: The figure depicts treatment differences in working hours accumulated in different types of jobs over the course of 12 months after the start of the experiment. Depicted are pairwise comparisons of three treatment groups (tool, message and baseline) including 90% confidence intervals. We present separate treatment effects on (A) individuals who do not comply with the requirement at the onset of the intervention (i.e. they have worked less than 225 hours within the preceding twelve-month period) and (B) individuals who do comply with the requirement at the onset of the intervention (they have worked 225 or more hours within the preceding twelve-month period). In all specifications, we control for covariates as depicted in Table 1. The dependent variables are depicted on the y-axis:
Full-time employment: Hours worked in jobs with more than 37 hours per week for the entire month.
Working on benefit claim: Hours worked in part-time jobs while receiving welfare benefits in the same month.
Stable full-time employment: Hours worked in full-time jobs that last for at least six months (i.e. working with the same employer).

personalized information provided in the online tool encourages individuals to seek jobs that enable them to transition out of the welfare system altogether, as opposed to merely working the minimum required hours to comply with the requirement while still receiving benefits.

5.3 Individuals' prior experience with the welfare system

Thus far, we have examined heterogeneity concerning individuals' past labor supply, revealing it to be a significant factor influencing treatment responses. However, we also acknowledge that workers may respond differently based on their prior knowledge of the benefit rules and their overall perception of the welfare system. While we lack a direct measure of individuals' baseline knowledge or prior beliefs, we explore heterogeneity concerning two dimensions of their previous experience with the welfare system. These aspects are likely to shape workers' perceptions of the rules that govern their incentives as outlined in Section 4.

Elapsed benefit duration: To begin with, we investigate heterogeneous effects concerning the elapsed duration of benefit receipt at the onset of the intervention, which is particularly intriguing for two reasons. First, financial sanctions can only be imposed after individuals have received benefit payments for at least 12 months. Second, the welfare administration typically informs individuals about the work requirement after they have been receiving benefit payments for approximately six months. Given this context, we estimate separate treatment effects for two groups: (1) short-term benefit recipients with an elapsed benefit duration of up to 26 weeks, and (2) long-term benefit recipients who have been receiving welfare benefits for more than six months.

As shown in Panel A of Table 4, the adverse labor supply effects of the generic notifications primarily stem from the group of short-term benefit recipients who have entered the welfare system within the last six months. This pattern is consistent with the idea that workers who have only recently entered the welfare system are more likely to exhibit knowledge gaps about the general rules, making it more likely that the treatment messages convey new information to them.[12] Moreover, our findings indicate that the negative employment effects are not limited to those workers who are already in compliance with the requirement at the start of the experiment. Instead, the message treatment also leads to a reduction in working hours (-11.9%; $p = 0.035$) and earnings (-12.5%; $p = 0.033$) among those short-term benefit recipients who have worked less than 225 hours in the last 12 months (see columns 1 and 3 Panel A.1). Altogether, this pattern aligns with the notion that the message treatment reduces the perceived pressure to work among short-term benefit recipients because they may infer from the notification messages that they cannot face sanctions during the first year of their benefit spell.

Prior exposure to benefit rules: Moreover, we expect that individuals who have personally experienced the work requirement will possess a more comprehensive understanding of the welfare system. Consequently, we aim to identify workers who have previously encountered the work requirement, either through (1) facing a benefit reduction due to non-compliance or (2) being granted an official exemption from the requirement.[13] In both cases, individuals might be aware of the dynamic nature of the requirement or the fact that their labor supply is being monitored because caseworkers may explicitly outline these aspects when imposing sanctions or granting exemptions, respectively.

Based on this categorization, Panel B of Table 4 illustrates differential treatment effects for individuals who have prior experience with the requirement compared to those who have none. The findings indicate that the tool treatment yields more favorable employment and earnings effects for workers who have had no direct exposure to the work requirement in the past. For instance, among the tool treated non-compliers who have not previously been affected by the

[12] In accordance with this notion, a related study by Altmann et al. (2022a) reveals that Danish UI benefit recipients tend to exhibit an improved understanding of the benefit rules the longer they have been unemployed. This could be attributed to their perception of the rules becoming more relevant or the result of spending more time within the system, enabling them to gather more knowledge over time.

[13] This refers to the period from October 2016, when the work requirement was initially implemented, up until the commencement of our intervention in August 2018.

Table 4: Heterogeneous effects by experience with the welfare system

Dependent variable	**Total working hours** (within twelve months)			**Total labor earnings** (DKK, within twelve months)		
	Short-term benefit recipients[a]	Long-term benefit recipients[a]	Difference	Short-term benefit recipients[a]	Long-term benefit recipients[a]	Difference
	(1)	(2)	(2) – (1)	(3)	(4)	(4) – (3)
A.1 Non-compliers (at onset of intervention)						
Treatment status (ref. baseline group)						
Tool treatment	2.55	8.03	5.48	106	1,255	1,149
	[0.822]	[0.020]	[0.571]	[0.952]	[0.022]	[0.450]
Message treatment	-24.05	-1.41	22.64	-3,750	-173	3,577
	[0.035]	[0.683]	[0.019]	[0.033]	[0.752]	[0.019]
No. of observations	5,698	33,780	39,478	5,698	33,780	39,478
P-value (tool = message)	0.020	0.006	0.076	0.028	0.009	0.111
Mean dep. variable	143.21	77.78		21,235	11,744	
A.2 Compliers (at onset of intervention)						
Treatment status (ref. baseline group)						
Tool treatment	-34.77	-10.90	23.87	-5,200	-3,723	1,477
	[0.132]	[0.556]	[0.417]	[0.188]	[0.208]	[0.762]
Message treatment	-38.82	-27.39	11.42	-6,235	-4,041	2,195
	[0.093]	[0.140]	[0.698]	[0.114]	[0.172]	[0.653]
No. of observations	3,734	4,082	7,816	3,734	4,082	7,816
P-value (tool = message)	0.862	0.373	0.673	0.795	0.914	0.883
Mean dep. variable	551.12	319.25		88,654	59,709	
Dependent variable	**Total working hours** (within twelve months)			**Total labor earnings** (DKK, within twelve months)		
	Not exposed to rule before[b]	Exposed to rule before[b]	Difference	Not exposed to rule before[b]	Exposed to rule before[b]	Difference
	(5)	(6)	(6) – (5)	(7)	(8)	(8) – (7)
B.1 Non-compliers (at onset of intervention)						
Treatment status (ref. baseline group)						
Tool treatment	19.01	4.19	-14.82	3,267	584	-2,684
	[0.059]	[0.181]	[0.083]	[0.065]	[0.244]	[0.046]
Message treatment	-3.93	-4.11	-0.18	-88	-698	-610
	[0.733]	[0.190]	[0.983]	[0.960]	[0.164]	[0.649]
No. of observations	7,639	31,839	39,478	7,639	31,839	39,478
P-value (tool = message)	0.047	0.008	0.086	0.058	0.010	0.123
Mean dep. variable	202.14	59.68		29,960	9,072	
B.2 Compliers (at onset of intervention)						
Treatment status (ref. baseline group)						
Tool treatment	-13.07	-35.06	-21.99	-3,133	-6,476	-3,343
	[0.572]	[0.062]	[0.457]	[0.418]	[0.037]	[0.496]
Message treatment	-30.70	-35.33	-4.63	-4,904	-5,337	-433
	[0.180]	[0.063]	[0.875]	[0.201]	[0.088]	[0.930]
No. of observations	3,700	4,116	7,816	3,700	4,116	7,816
P-value (tool = message)	0.448	0.989	0.560	0.649	0.716	0.556
Mean dep. variable	531.93	409.81		84,409	63,765	

Note: The table reports treatment differences in working hours and labor earnings accumulated over the course of 12 months after the start of the experiment for different subgroups of participants in the randomized controlled trial. Depicted are the effects of the tool and message treatments relative to the baseline group. $P-$values are shown in square brackets. In all specifications, we control for covariates as depicted in Table 1.

[a] Panel A presents separate treatment effects for groups who differ with respect to the elapsed benefit duration: short-term benefit recipients have an elapsed benefit duration of less than 26 weeks at the start of the intervention, while long-term benefit recipients have an elapsed benefit duration of 26 weeks or more.

[b] Panel B presents separate treatment effects for groups who differ regarding their prior exposure the benefit rules: individuals who were not exposed to the rules before were neither sanctioned due to non-compliance nor exempted from the requirement, while those who were exposed to the rule were either sanctioned due to non-compliance or exempted from the requirement. Among those who were exposed to the rule before, 7% were sanctioned due to non-compliance (and not exempted), 79% were exempted from the requirement (and not sanctioned) and 14% were sanctioned and exempted in the past.

requirement (see column 1 in Panel B.1), we find significant increases in their working hours (+9.4%; $p = 0.059$) and earnings (+10.9%; $p = 0.065$). In contrast, the corresponding effects on the larger group of non-complying workers who have already experienced the requirement (see column 2) are small and statistically insignificant. This pattern aligns with the idea that the positive labor supply effects of the tool treatment can be attributed to individuals who achieve an improved understanding of the welfare system by utilizing the online tool.

5.4 Additional results

In our main analysis, we concentrated on evaluating the impact of our intervention on individuals' overall working hours and labor earnings. The results revealed disparate effects of the tool and message treatments, contingent on workers' personal situations at the onset of the experiment, including labor supply and knowledge of rules based on experience and exposure. Before discussing the implications of our findings, we provide empirical evidence on four additional aspects that are crucial for interpreting our results.

Benefit payments and sanctions: First, the information treatments may trigger behavioral responses among individuals who do not return to paid employment. In Denmark, several alternative transfer programs exist that can provide alternative income sources. Notably, workers facing educational barriers can access income support through student benefits when enrolling in higher educational programs (secondary or tertiary education). Similarly, individuals with physical or mental constraints may be eligible for sickness or disability benefits. One common feature of these alternative income support programs is that beneficiaries are neither subject to work requirements nor at risk of related financial sanctions.

Against this backdrop, columns (1)–(3) of Table 5 present treatment effects on additional outcomes related to the receipt of welfare and other benefits, as well as the imposition of sanctions due to non-compliance with the work requirement. Among those who have worked less than the required number of hours in the past, the tool and the message treatments both lead to a significant increase in the take-up of other transfers by approximately 7.2% ($p = 0.003$) and 5.0% ($p = 0.044$), respectively (see column 1). Additionally, we observe that the higher likelihood of individuals entering alternative transfer schemes is associated with a decrease in their welfare benefit payments (see column 2) and a reduction in the likelihood of experiencing a financial sanction due to non-compliance with the work requirement (see column 3).[14] It is intuitive

[14]Our dataset includes information on benefit sanctions imposed due to non-compliance with the work requirement for the initial six months after the start of the intervention (i.e., the period when the online tool was only activated for the tool group). To facilitate a meaningful comparison of the effects on the likelihood of receiving a sanction with other outcome measures, we present treatment effects on labor market outcomes and benefit payments over this six-month period in Table A.4 in the Appendix. The overall pattern of results suggests that

Table 5: Effects of information treatments on benefit payments, margins of employment and annual income measures

Dependent variable	**Benefit payments and sanctions**[a]			**Hours worked within 12 months**			**Annual income in 2019**	
	Take-up of other transfers (%-points)	Welfare benefits received (in DKK)	Sanction imposed (%-points)	Any hours (%-points)	1,000+ hours (%-points)	225–249 hours (%-points)	Work income (DKK)	Disposable income (DKK)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Non-compliers (at onset of intervention)								
Treatment status (ref. baseline group)								
Tool treatment	1.45	-2,536	-0.69	0.96	0.42	-0.06	1,558	665
	[0.003]	[0.000]	[0.051]	[0.037]	[0.054]	[0.465]	[0.021]	[0.075]
Message treatment	0.99	-1,256	-0.63	0.10	-0.21	-0.03	-774	-249
	[0.044]	[0.026]	[0.076]	[0.810]	[0.315]	[0.750]	[0.172]	[0.548]
No. of observations	39,478	39,478	39,478	39,478	39,478	39,478	39,478	39,478
P-value (tool = message)	0.343	0.023	0.857	0.034	0.000	0.706	0.002	0.012
Mean dep. variable	19.90	126,739	20.18	17.47	2.98	0.54	17,353	120,864
B. Compliers (at onset of intervention)								
Treatment status (ref. baseline group)								
Tool treatment	1.34	1,914	0.24	-0.78	-2.10	0.53	-4,182	277
	[0.230]	[0.214]	[0.819]	[0.615]	[0.085]	[0.263]	[0.113]	[0.788]
Message treatment	-0.55	1,322	-0.42	-1.45	-1.94	0.71	-4,126	-503
	[0.624]	[0.391]	[0.686]	[0.336]	[0.082]	[0.030]	[0.141]	[0.674]
No. of observations	7,816	7,816	7,816	7,816	7,816	7,816	7,816	7,816
P-value (tool = message)	0.093	0.703	0.529	0.587	0.861	0.724	0.979	0.471
Mean dep. variable	21.29	95,010	18.00	70.44	19.66	1.63	84,519	138,417

Note: The table reports treatment differences in additional outcomes separated for individuals who have worked less (Panel A) and more (Panel B) than 225 hours within the twelve-month period preceding the intervention. Depicted are the effects of the tool and message treatments relative to the baseline group. $P-$values are shown in square brackets. In all specifications, we control for covariates as depicted in Table 1.

[a] The take-up of other benefits (column 1) and the amount of welfare benefit payments received (column 2) are measured over a period of 12 months after the start of the intervention. Imposed benefit sanctions due to non-compliance with the work requirement (column 3) are only observed for a period of six months following the start of the experiment.

that our intervention encourages individuals to leave the welfare system and seek enrollment in other transfer schemes, which are not subject to the work requirement. Specifically, eligibility for disability or educational benefits—the two most relevant alternative transfer schemes—is usually contingent on health issues or educational barriers, factors which in and of themselves may pose challenges for the labor market reintegration of individuals, including for working enough hours to comply with the requirement. While sanctions might not be enforced in many cases due to temporary exemptions from the requirement, our intervention brings attention to the potential risk of an income reduction. Consequently, unemployed workers may perceive it to be worthwhile to claim other benefits, even if doing so involves additional costs, such as providing proof of eligibility.[15]

Margins of employment and strategic behavior: Second, the binary nature of the requirement may incentivize workers to strictly adhere to the legal minimum requirement of hours, without exceeding them, that is, to allocate their working hours strategically over time to minimize their sanction risk.[16] To explore whether our intervention reinforces this sort of strategic behavior, we analyze treatment effects on different employment margins, specifically examining whether individuals (1) worked any hours, (2) worked more than 1,000 hours, or (3) worked between 225 and 249 hours over the course of one year. As displayed in columns (4)–(6) of Table 5, both the tool and message treatments exhibit significant effects on the likelihood of working more than 1,000 hours within a year, which is more than four times the required number of hours. At the same time, we find that the message treatment significantly increases the likelihood of working just enough hours to comply with the requirement (i.e., accumulating between 225 and 249 hours within a year) among those who are compliant at the start of the experiment, by approximately 0.71 percentage points ($p = 0.030$). This suggests that the adverse employment effects observed among this group of workers may, in part, be a result of strategic behavior.

Implications for financial well-being: Third, it is possible that the earnings effects of our intervention are (partially) offset by tax and transfer payments. To gain a more comprehensive

the reduced sanction rates among treated non-compliers are primarily driven by their increased uptake of other transfer programs.

[15] To obtain educational benefits, individuals must enroll in an educational program and provide documentation of ongoing study activity. On the other hand, for sickness or disability benefits, individuals would need to present a medical certificate as part of the application process.

[16] Figure A.3 in the Appendix shows the distribution of working hours accumulated within 12 months after the start of the experiment. Evidently, there is a noticeable spike in the distribution just above the cutoff of 225 hours. Specifically, in the overall sample, the proportion of individuals who worked between 225 and 249 hours is approximately 20% larger than the lower percentages in the two adjacent bins (i.e., the proportion of individuals who worked between 200 and 224 hours, and between 250 and 274 hours). Additionally, the spike appears most pronounced among individuals assigned to the message group (refer to Panel B).

understanding of individuals' overall financial well-being, we account for two additional income measures derived from the annual tax records for the calendar year 2019: (1) individuals' work income from paid employment and self-employment, and (2) their overall disposable income net of taxes and transfers. The findings, presented in columns (6) and (7) of Table 5, indeed suggest that the earnings effects are somewhat mitigated by the tax and transfer system. For example, the positive effect of the tool treatment on the disposable income of non-compliers is approximately 57% smaller than the corresponding increase in their work income. However, despite this mitigating effect of the tax and transfer system, the tool treatment still has a positive and marginally significant impact on the disposable income (+0.6%; $p = 0.075$) of workers who are not yet in compliance at the onset of the intervention.

Treatment spillovers: Finally, it is worth considering that our intervention may not only have had direct effects on the treated individuals but also indirect effects on others with whom they interact (see, e.g., Crépon et al., 2013; Gautier et al., 2018; Altmann et al., 2022b). These indirect effects could manifest in various ways, such as information spillovers if treated individuals inform their (untreated) peers about their newly acquired knowledge or displacement effects among job seekers competing for the same vacancies. To investigate the relevance of spillover effects, we exploit the natural variation in the share of treated individuals across different clusters of benefit recipients who are more likely to interact with each other. Our analysis, presented in more detail in Appendix A.2, indicates little evidence of systematic positive or negative spillovers. While our analysis cannot completely rule out all possible forms of treatment spillovers, it seems unlikely that they would have a substantial net effect on our results.

6 Discussion

Recipients of transfer payments frequently encounter complex rules and regulations that govern their incentives. Although these rules serve essential purposes, such as minimizing moral hazard problems and enhancing targeting efficiency (see, e.g., Kleven and Kopczuk, 2011), it remains a crucial concern for policymakers to ensure that individuals perceive their incentives accurately, enabling them to make optimal decisions.

In this paper, we combined data from a large-scale field experiment that we conducted among social welfare recipients with detailed administrative records to examine the labor market effects of two information treatments. Both treatments revolve around a requirement to work a minimum number of hours, but they differ in terms of the information provided (general vs. personal) and the method of information dissemination (static vs. on-demand). Specifically, one group of benefit recipients received generic messages, informing them about the general rules governing their incentives. In contrast, the other group was granted access to an online tool, providing them with continually updated real-time information about their individual situation in relation to the work requirement. It is one of the central results of our study that these two methods of informing unemployed workers about their work incentives had profoundly divergent effects on the labor market outcomes of treated individuals.

The tool treatment, which provides access to the personalized online tool along with information about the general rules, encourages workers who have not yet worked the required number of hours to increase their labor supply. This group of workers may deduce from the personalized information that their risk of facing a sanction is higher than they had initially assumed. Multiple results in our study indicate that utilizing the online tool enhances individuals' understanding of the welfare system, particularly the dynamic aspects of their work incentives. For instance, the time profile of treatment effects indicates that individuals who use the tool gradually adjust their labor supply in response to the additional information they receive over time. Moreover, the tool treatment not only motivates them to work enough hours to meet the requirement but it also prompts them to leave the welfare system. In particular, they take on permanent full-time positions and enter other transfer programs that are not subject to the work requirement, perhaps because they recognize that the risk of a sanction is always present when being on social welfare benefits. Finally, the effects of the tool treatment are less pronounced among individuals who are already expected to exhibit a greater understanding of these aspects in the absence of our intervention. These findings consistently support the notion that the positive labor supply effects resulting from personalized information can be attributed to an improved understanding of the benefit rules. At the same time, the positive labor supply effects resulting from the online tool could be amplified by individuals developing an enhanced perception of being monitored.

To isolate the empirical relevance of these mechanisms, it would be ideal to gather supplementary survey evidence focused on benefit recipients' subjective beliefs, their knowledge of the benefit rules, and their perceptions of the welfare system. In this context, it is worth mentioning the study by Altmann et al. (2022a) that was conducted in parallel to ours. They employed a personalized digital tool to inform recipients of unemployment insurance benefits in Denmark about their potential benefit duration. Although there are differences regarding the study population and the rules governing individuals' incentives when compared to our setting, the personalized online tools used in both studies share certain similarities (such as providing information about an individual's past hours worked). Complementing our findings, Altmann et al. (2022a) present direct survey evidence suggesting that individuals who are encouraged to use the online tool exhibit an increased understanding of the associated benefit rules, while their perception of being monitored by the administration remains unaffected.

In contrast to personalized information, the message treatment, which consists of generic notification messages, reduces benefit recipients' overall levels of employment and earnings. These adverse labor supply effects are concentrated among workers (1) who are already in compliance with the requirement at the start of the experiment and (2) who have entered the welfare system within the last six months. Apparently, the treatment messages remind these workers that they are not presently required to work additional hours to comply with the work requirement, leading to a reduction in their labor supply.

The finding that simple notification messages about general work incentives have adverse labor supply effects for certain groups of benefit recipients is worrying for researchers and policymakers who regularly rely on similar approaches to reduce information constraints. Our results provide a cautionary tale that policy interventions providing basic information can backfire, for instance, when there is a risk that the underlying incentives encourage individuals to act strategically. At the same time, our findings suggest that digital tools that allow policymakers to disseminate tailored information towards different groups of workers at low marginal costs can effectively reduce information frictions and help to improve individual decision-making. While this indicates that public investments in such digital infrastructure have the potential to improve overall welfare, some words of caution are warranted.

First, the context-specific labor supply responses imply that it is important to understand which groups of workers actually benefit from the information provided and which ways of communicating are most effective in achieving potential welfare gains. Second, we found that the intervention encouraged some benefit recipients to leave the welfare system and to rely on other transfer schemes designed to support individuals with health problems or a lack of education. While such sorting effects could improve the targeting of benefit payments and may allow policymakers to tailor government policies more efficiently (see, e.g., Besley and Coate, 1992), individuals who leave social welfare could face an increased risk of poverty—a concern that might be more severe in countries with less comprehensive transfer programs than Denmark. Finally, we provide suggestive evidence that treatment spillovers are unlikely to have had a large net effect on our results. However, a full roll-out and the simultaneous adoption of different information technologies could induce further equilibrium effects, and as such, offer an interesting avenue for future research.

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A Online Appendix

A.1 Further Details on Study Design

Treatment message to message group (without link to online tool)

How to avoid loosing your welfare benefits

The 225-hour rule means that you risk having your allowance reduced or completely losing it. This happens if you have not worked 225 hours within the last year. The rule comes into effect after you have received social welfare for a total of one year within a period of three years.

If you want to avoid losing or reducing your allowance, you should keep track of how many hours you need to work to accumulate 225 hours.

Check your hours regularly, so that you can plan how many hours you need to work per week to reach at least 225 hours. With just a few hours of work per week, you can reach the target and avoid having your allowance reduced.

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 count for a whole year. Therefore, it is still worth accumulating hours after you have worked 225 hours.

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

Treatment message to tool group (with link to online tool)

How to avoid loosing your welfare benefits

The 225-hour rule means that you risk having your allowance reduced or completely losing it. This happens if you have not worked 225 hours within the last year. The rule comes into effect after you have received social welfare for a total of one year within a period of three years.

If you want to avoid losing or reducing your allowance, you should keep track of how many hours you need to work to accumulate 225 hours. You can use a new tool on jobnet.dk that helps you keep track of your working hours. '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 'counter of hours' now. [LINK]

Check your hours regularly, so that you can plan how many hours you need to work per week to reach at least 225 hours. With just a few hours of work per week, you can reach the target and avoid having your allowance reduced.

<u>225 hours are equivalent to:</u>

- 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

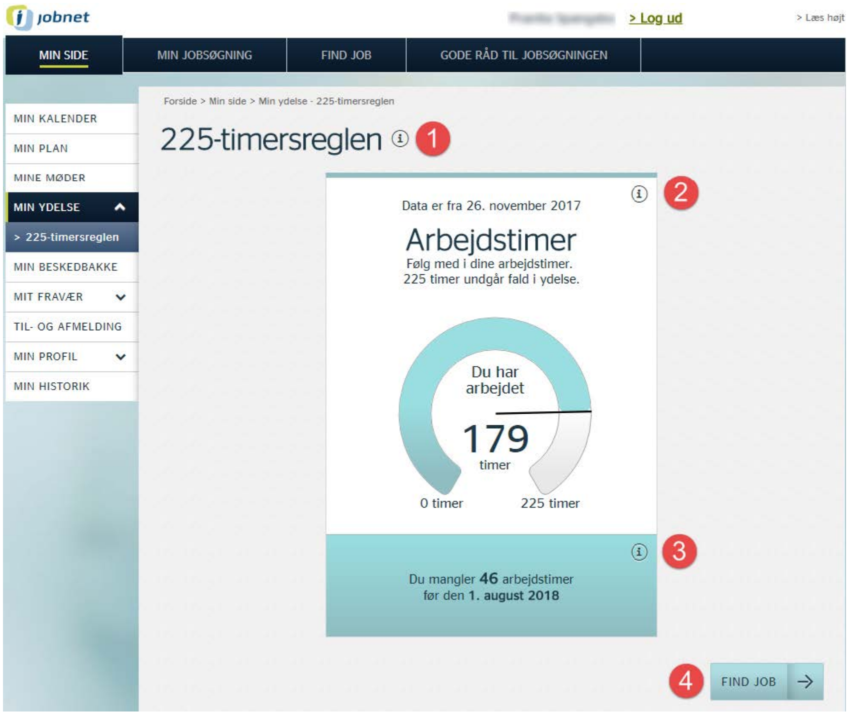
All the hours you work today count for a whole year. Therefore, it is still worth accumulating hours after you have worked 225 hours.

When you log on to jobnet.dk to check your job adds, it is easy for you to keep an eye on 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 loosing money.

Illustration of online tool

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 accumulated 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.

A.2 Analysis of Treatment Spillovers

The large-scale nature of our experiment could raise concerns that there are not only direct effects on treated individuals, but also spillovers on other, untreated individuals. For instance, there could be information spillovers such that treated benefit recipients inform their untreated peers about their newly acquired knowledge (Duflo and Saez, 2003). Moreover, spillovers could occur if treated and untreated individuals compete for the same vacancies (Crépon et al., 2013; Ferracci et al., 2014; Gautier et al., 2018).

While our experimental design does not explicitly account for the analysis of spillover effects, e.g., through a clustered randomization procedure with varying treatment intensity across different regions (see, e.g., Crépon et al. 2013, Altmann et al. 2022b), our randomization procedure gives rise to natural exogenous variation in the share of treated individuals in subgroups of the experimental population, who are likely to interact with each other. Specifically, to examine the relevance of spillovers in our setting, we calculate local treatment intensities as the share of individuals being assigned (1) to the message treatment or (2) to the tool treatment within clusters of the experimental population. These clusters take into account individuals' place of residence (98 municipalities), their last occupation before becoming unemployed (171 occupations) and their age (three cohorts). Assuming that individuals within a cluster are, on average, more likely to interact with each other than individuals from different clusters (either by informing each other or by competing for similar vacancies), we can use variation in the treatment intensity to shed light on treatment spillovers. As shown in Figure A.4, we observe substantial variation with respect to treatment intensities across the different clusters. Moreover, it is shown in Table A.6 that individual characteristics have very little predictive power regarding the local treatment intensity. This suggests there are no systematic differences across clusters with different treatment intensities.

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

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

where D_i refers to the treatment status of individual i (dummies for being assigned to either the message or the tool treatment) and TI_j, refers to the local treatment intensities within cluster j (i.e. the share of individuals assigned to the message groups, respectively the tool group at the region-occupation-age level). Moreover, X_i is a vector of individual-level control variables. Equation (A.1) allows us to estimate different parameters of interest. First, δ identifies the direct effect of the message and tool treatments relative to the baseline group in the absence of spillovers. Second, γ shows possible spillovers on individuals who are not assigned to the corresponding treatment. For instance, a negative coefficient on the local message (tool) intensity would imply that a larger share of treated individuals has a negative impact on the labor market outcomes of non-treated individuals in the baseline and tool (message) group. Finally, the interaction effects of the actual treatment assignment and the local treatment intensity TI_j, given by θ, inform us about differential spillovers between treated and non-treated individuals. This means that the overall spillover effect on the message and tool groups is given by $(\gamma + \theta)$. We employ two-way clustered standard errors at the level of municipalities and previous occupations.

Table A.7 shows the results for cumulated working hours and earnings over 24 months for two different specifications. First of all, we consider the continuous treatment intensity as depicted in Figure A.4 (see Specification 1). Alternatively, in Specification 2, we consider indicator variables accounting for low-intensity (high-intensity) clusters with a local treatment intensity below 25% (above 50%). Overall, we find little evidence for systematic positive or negative treatment spillovers. For instance, the estimates from Specification 1 indicate that higher treatment intensities are positively related to working hours and earnings among those assigned to the message treatment (though the effect is rather imprecisely estimated). While our analysis does, ultimately, not allow us to rule out all possible forms of treatment spillovers (e.g., interaction effects between the two information treatments), it appears unlikely that treatment spillovers have a large net effect on the results presented in Section 5.

A.3 Additional Tables and Figures

Table A.1: Determinants of treatment assignment

	Dependent variable: treatment indicator					
	A. Message (≡ 1) - baseline (≡ 0)		B. Tool (≡ 1) - message (≡ 0)		C. Tool (≡ 1) - baseline (≡ 0)	
	Coef.	P-value	Coef.	P-value	Coef.	P-value
Female	-0.004	[0.472]	0.006	[0.278]	0.002	[0.709]
Married	-0.005	[0.576]	0.008	[0.396]	0.003	[0.769]
Education (ref.: less than high school)						
High school	0.016	[0.216]	-0.016	[0.233]	0.000	[0.991]
Bachelor's degree or equivalent	0.013	[0.337]	-0.007	[0.601]	0.005	[0.704]
Master's degree of equivalent	0.024	[0.128]	-0.015	[0.339]	0.009	[0.580]
Age (ref. 16-25 years)						
Age 26-35 years	-0.005	[0.576]	-0.000	[0.977]	-0.005	[0.552]
Age 36-45 years	-0.004	[0.707]	0.009	[0.356]	0.005	[0.580]
Age 46-55 years	-0.002	[0.813]	0.002	[0.871]	-0.001	[0.954]
Age 56-65 years	0.003	[0.777]	-0.006	[0.621]	-0.003	[0.823]
Migration background						
1st generation	0.016	[0.054]	-0.013	[0.102]	0.002	[0.799]
2nd generation	0.008	[0.581]	-0.009	[0.566]	-0.000	[0.996]
Living in capital region	-0.004	[0.475]	-0.003	[0.676]	-0.007	[0.254]
Children (ref.: no children)						
One child	0.001	[0.869]	0.004	[0.633]	0.006	[0.510]
Two or more children	-0.013	[0.208]	0.019	[0.054]	0.007	[0.509]
Three or more children	-0.011	[0.260]	0.015	[0.127]	0.004	[0.721]
Requires activation	-0.003	[0.774]	-0.006	[0.524]	-0.009	[0.337]
Elapsed benefit duration: up to 26 weeks	-0.001	[0.879]	0.000	[0.984]	-0.001	[0.894]
Pre-intervention outcome (in previous year)						
Any paid employment	0.036	[0.473]	0.034	[0.483]	0.069	[0.158]
Labor earnings (log)	-0.020	[0.112]	-0.003	[0.771]	-0.023	[0.063]
Weekly working hours (log)	0.020	[0.129]	0.001	[0.930]	0.020	[0.114]
Any benefit reduction	-0.002	[0.852]	-0.001	[0.891]	-0.002	[0.803]
Exempted from requirement	-0.002	[0.794]	0.015	[0.109]	0.013	[0.174]
Constant	0.496	[0.000]	0.498	[0.000]	0.494	[0.000]
No. of observations	31,533		31,525		31,530	
P-value joint significance	0.843		0.503		0.961	
Mean value dependent variable	0.500		0.500		0.500	
R^2	0.001		0.001		0.000	

Note: The table reports coefficients of an OLS regression of treatment indicators on background characteristics. $P-$values are depicted in square brackets.

Table A.2: Comparison of social welfare and UI benefit recipients

		Experimental population		
	Sample of UI benefit recipients (1)	Full sample of social welfare recipients (2)	Opened treatment messages (3)	Clicked on link to online tool (4)
No. of observations	98,641	47,294	11,539	1,507
Female	0.521	0.495	0.508	0.507
Married	0.341	0.173	0.185	0.176
Educational level				
High school	0.400	0.586	0.474	0.422
University degree	0.339	0.357	0.485	0.537
Age				
16-25 years	0.116	0.217	0.103	0.064
26-35 years	0.332	0.270	0.278	0.238
36-45 years	0.193	0.214	0.263	0.255
46-55 years	0.196	0.193	0.231	0.272
56-65 years	0.163	0.106	0.125	0.171
Children				
One child	0.164	0.153	0.167	0.169
Two or more children	0.172	0.226	0.230	0.192
Descendant	0.033	0.037	0.026	0.017
Immigrant	0.194	0.249	0.245	0.236
Living in Capital Region	0.333	0.323	0.318	0.318
Any paid employment				
in last year	0.790	0.214	0.341	0.498
in last five years	0.968	0.500	0.656	0.768
Total working hours				
in last year	994	142	246	370
in last five years	5,776	1,018	1,577	1,928
Total Labor earnings (DKK)				
in last year	213,811	21,944	38,955	58,150
in last five years	1,101,374	154,389	249,528	306,632

Note: Depicted are summary statistics for the group of individuals receiving unemployment insurance (UI) benefits in 2018 (column 1), the full experimental population of social welfare recipients in 2018 (column 2), as well as treated individuals who red the treatment messages (column 3), respectively who clicked on the link to the online tool (column 4). Percentage shares unless indicated otherwise.

Table A.3: Determinants of treatment take-up

	Dependent variable			
	A. Opening treatment message		B. Accessing online tool	
	Coef.	P-value	Coef.	P-value
Female	0.020	[0.005]	0.008	[0.085]
Married	-0.023	[0.044]	-0.018	[0.013]
Education (ref.: less than high school)				
High school	0.025	[0.121]	0.002	[0.853]
Bachelor's degree or equivalent	0.117	[0.000]	0.023	[0.027]
Master's degree of equivalent	0.191	[0.000]	0.062	[0.000]
Age (ref. 16-25 years)				
Age 26-35 years	0.211	[0.000]	0.063	[0.000]
Age 36-45 years	0.301	[0.000]	0.099	[0.000]
Age 46-55 years	0.292	[0.000]	0.119	[0.000]
Age 56-65 years	0.286	[0.000]	0.138	[0.000]
Migration background				
1st generation	-0.104	[0.000]	-0.035	[0.000]
2nd generation	-0.073	[0.000]	-0.029	[0.016]
Living in capital region	-0.0145	[0.052]	-0.0029	[0.547]
Children (ref.: no children)				
One child	-0.010	[0.324]	-0.008	[0.223]
Two or more children	-0.009	[0.472]	-0.024	[0.002]
Three or more children	-0.062	[0.000]	-0.032	[0.000]
Requires activation	-0.335	[0.000]	-0.128	[0.000]
Elapsed benefit duration: up to 26 weeks	-0.045	[0.000]	-0.011	[0.072]
Pre-intervention outcome (in previous year)				
Any paid employment	0.072	[0.000]	0.085	[0.000]
Labor earnings (DKK10,000)	0.006	[0.001]	0.002	[0.051]
Total weekly working hours ($\times$1,000)	-0.008	[0.781]	-0.007	[0.720]
Any benefit reduction	0.095	[0.000]	0.034	[0.000]
Exempted from requirement	0.002	[0.795]	-0.012	[0.018]
Constant	0.343	[0.000]	0.096	[0.000]
No. of observations	15,761		15,761	
Mean value dependent variable	0.366		0.096	
R^2 (adj.)	0.210		0.112	

Note: The table reports coefficients of an OLS regression of (1) an indicator for opening the general notification messages (see Panel A) and (2) an indicator for accessing the online tool through the link in the notification message (Panel B) on background characteristics among individuals assigned to the tool group. $P-$values are depicted in square brackets.

Table A.4: Effects of information treatments over six-month horizon

	Outcomes measured within six months after start of experiment				
Dependent variable	Total working hours	Total labor earnings (DKK)	Take-up of other transfers (%-points)	Welfare benefits received (DKK)	Sanction imposed (%-points)
	(1)	(2)	(3)	(4)	(5)
A. Non-compliers (at onset of intervention)					
Treatment status (ref. baseline group)					
Tool treatment	2.21	337	1.11	-1,127	-0.69
	[0.157]	[0.166]	[0.005]	[0.000]	[0.051]
Message treatment	-0.99	-183	0.82	-633	-0.63
	[0.525]	[0.451]	[0.041]	[0.026]	[0.076]
No. of observations	39,478	39,478	39,478	39,478	39,478
P-value (tool = message)	0.040	0.032	0.457	0.084	0.857
Mean dep. variable	31.15	4,680	11.74	70,587	20.18
B. Compliers (at onset of intervention)					
Treatment status (ref. baseline group)					
Tool treatment	-12.89	-2,413	0.96	1,324	0.24
	[0.084]	[0.050]	[0.317]	[0.113]	[0.819]
Message treatment	-19.04	-2,937	-0.22	917	-0.42
	[0.011]	[0.017]	[0.818]	[0.274]	[0.686]
No. of observations	7,816	7,816	7,816	7,816	7,816
P-value (tool = message)	0.413	0.673	0.221	0.629	0.529
Mean dep. variable	220.30	34,548	14.06	54,901	18.00

Note: The table reports treatment differences in outcomes measured within six months after the start of the intervention. Depicted are the effects of the tool and message treatments relative to the baseline group. $P-$values are shown in square brackets. In all specifications, we control for covariates as depicted in Table 1. We present separate treatment effects on (i) individuals who do not comply with the requirement at the onset of the intervention (i.e. they have worked less than 225 hours within the preceding twelve-month period) and (ii) individuals who do comply with the requirement at the onset of the intervention (they have worked 225 or more hours within the preceding twelve-month period).

Table A.5: Robustness check: specifications without control variables

Dependent variable	**Total working hours** (within twelve months)			
	Overall sample	Non-Compliers$^{(a)}$	Compliers$^{(a)}$	Difference
	(1)	(2)	(3)	(3) – (2)
Treatment status (ref. baseline group)				
Tool treatment	1.59	6.84	-18.44	-25.28
	[0.704]	[0.053]	[0.221]	[0.015]
Message treatment	-10.48	-4.36	-30.44	-26.08
	[0.012]	[0.216]	[0.044]	[0.012]
No. of observations	47,294	39,478	7,816	
P-value (tool = message)	0.004	0.001	0.430	0.939
Mean dep. variable	150.11	87.24	467.62	
Dependent variable	**Total labor earnings** (in DKK within twelve months)			
	Overall sample	Non-compliers$^{(a)}$	Compliers$^{(a)}$	Difference
	(4)	(5)	(6)	(6) – (5)
Treatment status (ref. baseline group)				
Tool treatment	-30.14	1,009	-4,243	-5,252
	[0.964]	[0.069]	[0.091]	[0.002]
Message treatment	-1,698	-669	-5,141	-4,472
	[0.012]	[0.227]	[0.041]	[0.008]
No. of observations	47,294	39,478	7,816	47,294
P-value (tool = message)	0.013	0.002	0.723	0.643
Mean dep. variable	23,100	13,114	73,537	

Note: The table reports unconditional treatment differences (without control variables) in working hours and labor earnings accumulated over the course of 12 months after the start of the experiment among participants in the randomized controlled trial. Depicted are the effects of the tool and message treatments relative to the baseline group. $P-$values are shown in square brackets. Specifications (1) and (4) present average treatment differences in the overall sample.

$^{(a)}$Specifications (2), (3), (5) and (6) present separate treatment effects on (i) individuals who do not comply with the requirement at the onset of the intervention (i.e. they have worked less than 225 hours within the preceding twelve-month period) and (ii) individuals who do comply with the requirement at the onset of the intervention (they have worked 225 or more hours within the preceding twelve-month period).

Table A.6: Predictability of local treatment intensity

	Dependent variable: local treatment intensity (in %-points)			
	(1) Message treatment		(2) Tool treatment	
	Coef.	$P-$value	Coef.	$P-$value
Female	-0.018	[0.950]	0.198	[0.482]
Married	-0.404	[0.163]	0.152	[0.610]
Education (ref.: less than high school)				
High school	0.156	[0.631]	-0.141	[0.696]
Bachelor's degree or equivalent	0.172	[0.647]	0.059	[0.883]
Master's degree of equivalent	-0.103	[0.846]	-0.374	[0.410]
Age (ref. 16-25 years)				
26-35 years	-0.451	[0.387]	0.208	[0.712]
36-45 years	-0.716	[0.202]	0.791	[0.155]
46-55 years	-0.077	[0.910]	0.070	[0.904]
56-65 years	0.119	[0.874]	-0.001	[0.999]
Migration background				
1st generation	0.312	[0.251]	-0.028	[0.926]
2nd generation	-0.456	[0.432]	0.001	[0.999]
Children (ref.: no children)				
One child	0.289	[0.259]	-0.639	[0.040]
Two children	-0.462	[0.224]	0.022	[0.953]
Three or more children	-0.146	[0.630]	-0.261	[0.446]
Not deemed capable of full-time employment	-0.185	[0.578]	0.288	[0.439]
Elapsed benefit duration: 26 weeks or less	-0.390	[0.173]	0.344	[0.197]
Living in capital region	0.046	[0.911]	-0.478	[0.285]
Pre-intervention outcome (in previous year)				
Any paid employment	-0.477	[0.243]	-0.238	[0.561]
Total weekly working hours	0.508	[0.661]	1.476	[0.211]
Labor earnings in 10,000DKK	-0.038	[0.599]	-0.072	[0.332]
Experienced benefit reduction	-0.263	[0.320]	0.302	[0.270]
Exempted from work requirement	-0.140	[0.655]	-0.096	[0.763]
Constant	33.97	[0.000]	33.10	[0.000]
No. of observations	47,243		47,243	
R^2	0.001		0.001	
$P-$ value joint significance	0.487		0.348	

Note: OLS estimation. The dependent variable refers to the local treatment intensity, which is given by share of individuals assigned to the message treatment (specification 1) or the tool treatment (specification 2) across combinations of 98 municipalities, 171 previous occupations (3-digit DISCO level) and three age cohorts. $P-$values based on clustered standard errors at the municipality level are shown in square brackets.

Table A.7: Treatment effects and spillovers on labor market outcomes

	Working hours within 12 months		Labor earnings (in DKK) within 12 months	
	(1)	(2)	(3)	(4)
Specification 1:				
Message treatment	-17.39		-1,469	
	[0.213]		[0.503]	
Tool treatment	-0.12		358	
	[0.990]		[0.801]	
Local treatment intensity message (cont.)[(a)]	-4.50		804	
	[0.851]		[0.822]	
Local treatment intensity tool (cont.)[(a)]	8.61		1,837	
	[0.503]		[0.371]	
Message treatment × intensity message	23.30		391	
	[0.460]		[0.937]	
Tool treatment × intensity tool	2.37		-8,901	
	[0.911]		[0.759]	
Specification 2:				
Message treatment		-3.63		-490
		[0.444]		[0.509]
Tool treatment		3.48		349
		[0.443]		[0.576]
Local treatment intensity message (cat.)[(b)]				
low intensity (below 0.25)		7.54		933
		[0.329]		[0.418]
high intensity (above 0.5)		5.61		1,251
		[0.282]		[0.131]
Local treatment intensity tool (cat.)[(b)]				
low intensity (below 0.25)		0.71		-328
		[0.900]		[0.679]
high intensity (above 0.5)		7.29		1,067
		[0.193]		[0.264]
Message treatment				
× low intensity (below 0.25)		-24.66		-3,785
		[0.112]		[0.102]
× high intensity (above 0.5)		-5.95		-1,388
		[0.452]		[0.264]
Tool treatment				
× low intensity (below 0.25)		-16.28		-2,559
		[0.288]		[0.291]
× high intensity (above 0.5)		-4.28		-660
		[0.602]		[0.552]
No. of observations	47,243	47,243	47,243	47,243
Mean value dep. variable	148.6	148.6	22,477	22,477

Note: The table reports treatment differences and spillover effects on labor market outcomes for different subgroups of participants in the randomized controlled trial. Local treatment intensity refers to the share of treated job seekers (tool or message treatment) across combinations of 98 municipalities, 171 previous occupations (3-digit DISCO level) and three age cohorts. $P-$values based on two-way clustering at the level of municipalities and previous occupations are reported in square brackets.

[(a)] The local treatment intensity refers to the share of individuals assigned to the message treatment, respectively the tool treatment within a cluster. It is measured on a continuous scale from 0 to 1. The reported coefficients refer to changes in outcome variables when the share of individuals in the changes from zero to 100%.

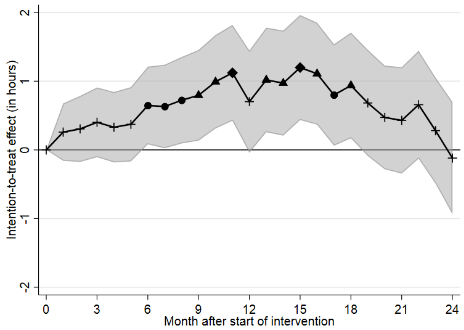
[(b)] The local treatment intensity refers to the share of individuals assigned to the message treatment, respectively the tool treatment within a cluster. Low/high intensity refer to indicator variables identifying clusters with local treatment intensities below 25%/above 50%.

Figure A.2: Time profile of treatment effects

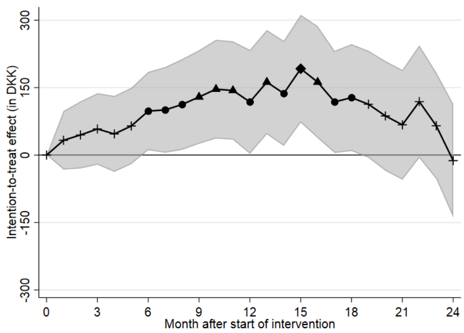
A. Non-compliers (at onset of intervention) ($N = 39,478$)

A.1 Tool group−baseline group

Effect on working hours

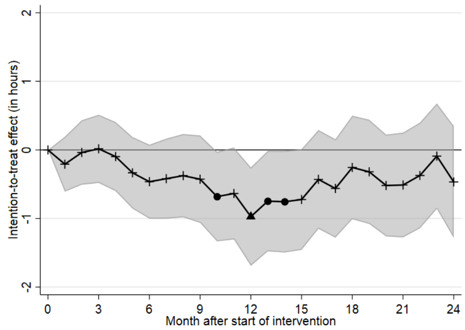


Effect on labor earnings, DKK

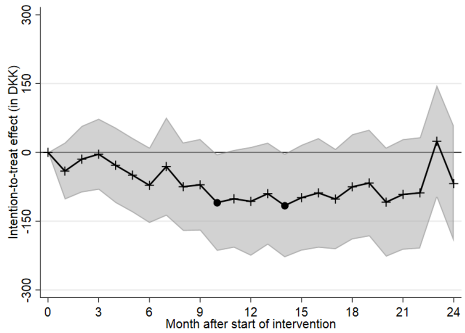


A.2 Message group−baseline group

Effect on working hours



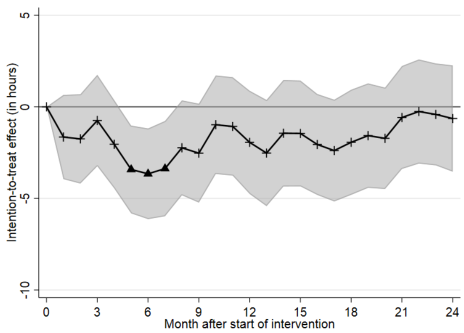
Effect on labor earnings, DKK



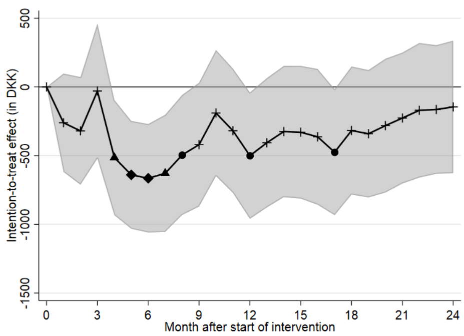
B. Compliers (at onset of intervention) ($N = 7,816$)

B.1 Tool group−baseline group

Effect on working hours

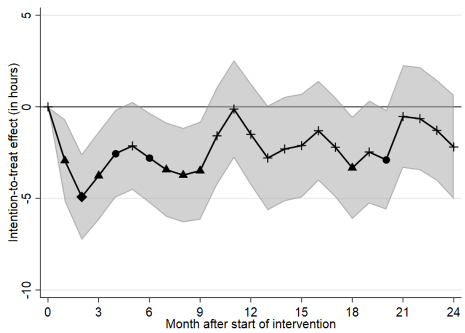


Effect on labor earnings, DKK

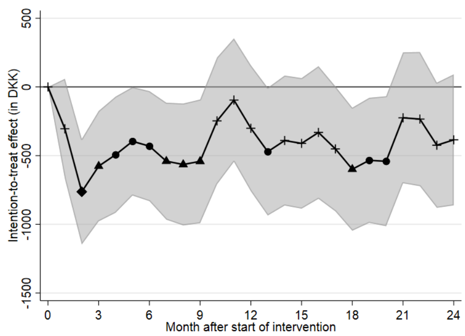


B.2 Message group−baseline group

Effect on working hours

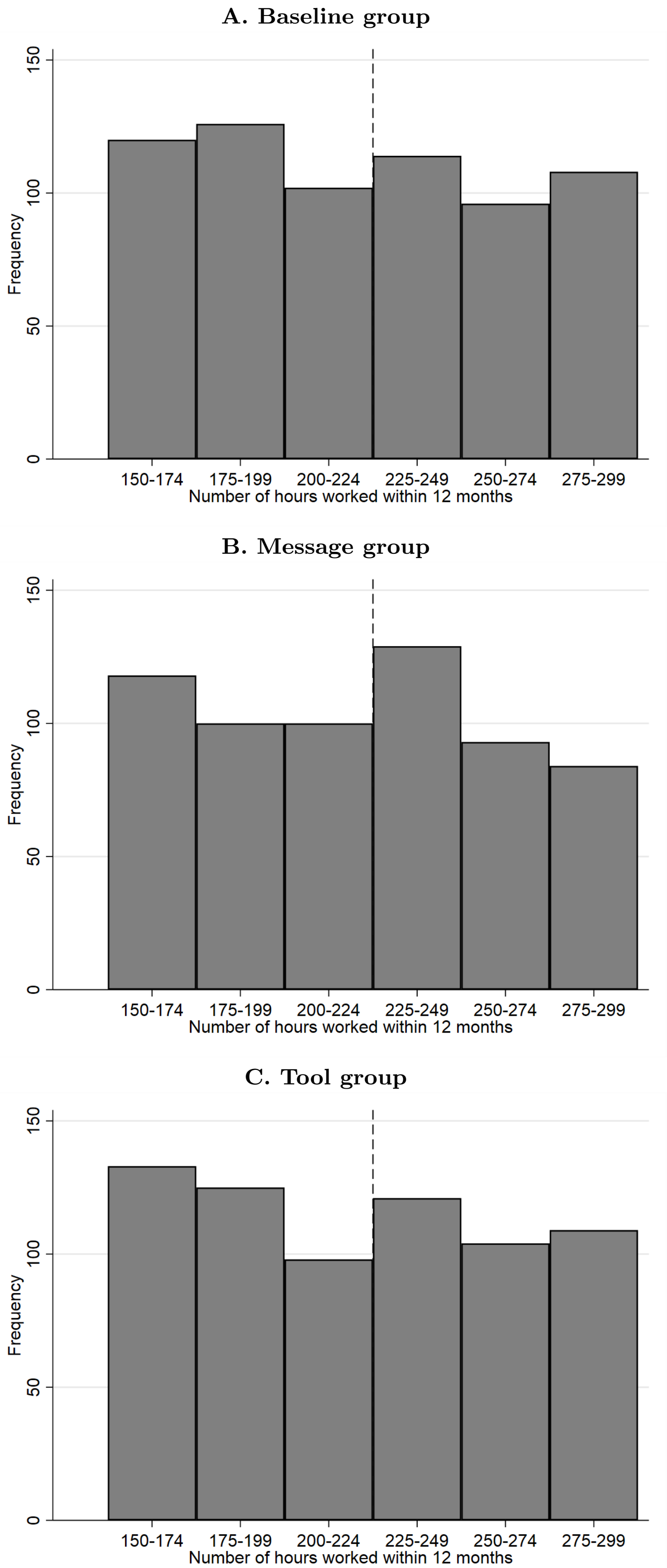


Effect on labor earnings, DKK



Note: The figure presents treatment effects on monthly working hours and labor earnings including 90% confidence intervals for the first 24 months after the start of the intervention. Depicted are the effects of the tool and message treatments relative to the baseline group. In all specifications, we control for covariates as depicted in Table 1. We present separate treatment effects on (A) individuals who do not comply with the requirement at the onset of the intervention (i.e. they have worked less than 225 hours within the preceding twelve-month period) and (B) individuals who do comply with the requirement at the onset of the intervention (they have worked 225 or more hours within the preceding twelve-month period). ●/▲/◆ indicate statistical significance at the 10%/5%/1%-level.

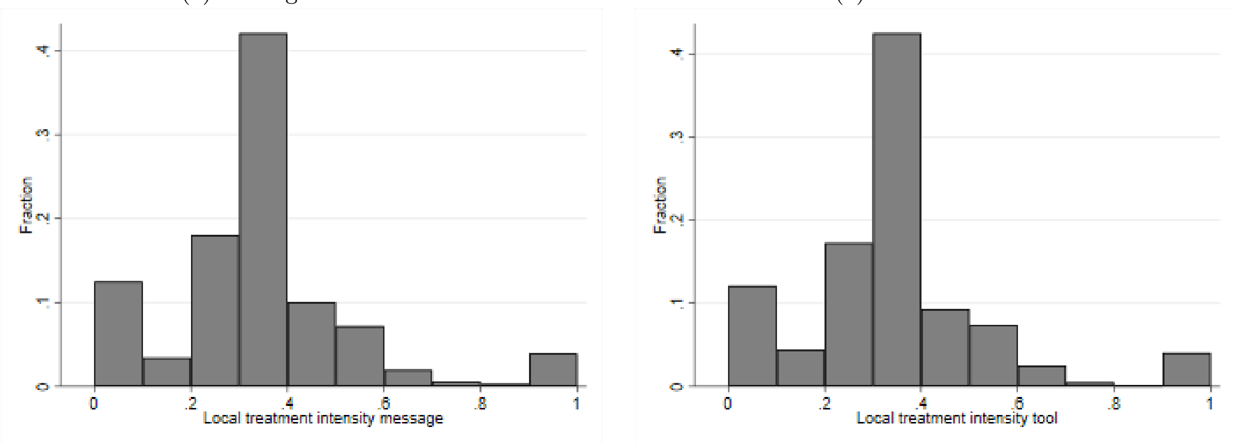
Figure A.3: Distribution of working hours around the cut-off of the work requirement



Note: Depicted is the distribution of working hours accumulated within 12 months after the start of the intervention separated by treatment status among individuals who worked between 150 and 299 hours within that time period ($N = 1,980$).

Figure A.4: Distribution of local treatment intensity

(1) Message treatment (2) Tool treatment



Note: Depicted is the distribution of the local treatment intensity, which is given by share of individuals assigned to the (1) message treatment or (2) tool treatment across combinations of 98 municipalities, 171 previous occupations (3-digit DISCO level) and three age cohorts.