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IZA DP No. 17905

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ABSTRACT

Advising Job Seekers in Occupations with Poor Prospects: A Field Experiment*

We study the impact of online information provision to unemployed job seekers who are looking for work in occupations in slack markets, i.e. with only few vacancies per job seeker. Job seekers received suggestions about suitable alternative occupations, and how the prospects of these alternatives compare to their current occupation of interest. Some additionally received a link to a motivational video. We evaluate the interventions using a randomized field experiment covering all eligible job seekers registered to search in the target occupations. The vast majority of treated job seekers open the message revealing the alternative suggestions. The motivational video is rarely watched. Effects on unemployed job seekers in structurally poor labor markets are large: their employment, hours of work and labor income all improve by 5% to 6% after 18 months. Additional survey evidence shows that treated job seekers find employment in more diverse occupations.

JEL Classification: J62, J64, C93

Keywords: job search, occupational mobility, randomized experiment, information treatment

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1 Introduction

Occupational transitions play a significant role in labor market adjustments to changes in the economy. The Covid-19 pandemic, technological development, and automation have been associated with profound changes in the demand for certain occupations.[1] Adjusting to such a changing environment means that workers need to transit from occupations with low labor demand relative to supply into occupations with better prospects. If this does not happen, the "mismatch" induced by unequal tightness (vacancies per job seeker) can be a source of elevated unemployment, and the benefits of equalizing tightness across occupations far exceed the benefits of equalizing tightness across geography (Şahin et al., 2014; Marinescu and Rathelot, 2018).

A major challenge is that workers may not be well informed about occupations they could or should consider. Moreover, even if well informed, there may be psychological hurdles to consider an occupational change. The lack of familiarity and uncertainty about the fit of one's skills with the skills needed in other occupations may constitute significant hurdles to occupational transitions. Evidence indeed suggests that job seekers tend to narrowly focus on occupations in which they have experience (Belot et al., 2019; Faberman and Kudlyak, 2019), and this can be problematic when these occupations are in low demand.

In this paper, we evaluate a low-cost and carefully designed digital intervention aimed at unemployed job seekers who are primarily looking for work in occupations that are in low demand, i.e., that have low tightness. The intervention aims at addressing informational deficits about job prospects in search occupations and alternative occupations. We designed and conducted the intervention in collaboration with the Dutch Public Employment Service (UWV). The experiment involved 30,129 job seekers who recently registered as (fully or partially) unemployed in one of 21 occupations with poor employment prospects as measured by a job finding score created by the Public Employment Service mainly based on tightness.

In our jointly designed intervention, the Public Employment Service informed 20,125 of these job seekers about the poor prospect in their primary occupation of interest and suggested alternative occupations with better prospect that are particularly well-suited to their skills background. Each suggested occupation is displayed alongside information about job prospects according to the 'job finding score' (i.e., vacancies to job seekers ratio), the skills required to do well in the

[1] For the Covid-Pandemic, see, e.g., del Rio-Chanona et al. (2020); Forsythe et al. (2020). For technological development and automation see, e.g., Autor et al. (2003); Autor (2015); Brynjolfsson et al. (2018); Frey and Osborne (2017).

occupation, and a link to a webpage with more detailed information about the occupation. To ensure that the occupational suggestions we make are realistic switches for the targeted job seekers, our suggestions are based on the most common occupational transitions observed from millions of resumes from former job seekers that we cross-checked to be representative for occupation mobility of the Dutch labor force. From these common and attainable transitions, we include those that currently offer sufficiently good job finding prospects based on the job finding score, and that thus have favorable market tightness. The experiment also includes an additional motivational component for a third of the participants. These participants received a link to a short video where people who recently made a successful transition from one occupation to another shared their positive experience.

Almost 80% of the treatment group opened the informational message. A sizeable share also clicked on at least one occupational suggestion for more information. The motivational video, however, was rarely watched, so this treatment group is effectively only exposed to the information part of the treatment.

Our main analysis focuses on the sample of job seekers without any paid employment at the time of the intervention who were searching in an occupation that had structurally poor prospects (around 10,000 job seekers). Because of the timing of the intervention (spring of 2021), a number of occupations had 'temporarily' poor prospects due to restricted economic activities from the Covid-19 pandemic, and we report results for job seekers searching in these occupations separately. The remaining group (job seekers who were partially (un)employed at the time of the intervention) is also interesting to study, but one may expect the intervention to affect them differently.[2]

Our key outcomes of interest are the job finding probability, labor earnings, hours worked, and benefits received (all measured from administrative data). Further administrative data from online search records and caseworkers records allow us to investigate job search activities. In addition, we assess how the intervention impacts job search activities and labor market beliefs, using survey data collected before and after the interventions.

The survey data at baseline shows that job seekers are, on average, willing to consider alternative occupations and confident that they will be able to do well in alternative occupations. However, job seekers are generally not aware of how poor the job prospects are in their primary occupation of interest, compared

[2] We report results for job seekers in temporarily poor occupations and for partially employed job seekers in section 6.2.4.

to suitable alternatives. While most job seekers do consider one (or a couple of) alternative occupations, their assessment of the job finding chances in these alternatives is hardly correlated with true job finding prospects. These findings point towards fertile grounds for our information intervention. Note that we do not force unemployed job seekers to broaden their job search: suitable switches with better prospects are only suggested to them. As shown in van der Klaauw and Vethaak (2022), forcing a broader job search, especially without adequate guidance, could backfire and reduce job finding rates.

The effects of the information we provide to unemployed job seekers in structurally poor labor markets are large: after 18 months employment increased by 5.2%, monthly hours worked by 6.0% and monthly earnings by 6.4% relative to the control group. The effects on employment are large and not associated with lower hours or lower wages: so employment effects carry over to hours worked and earnings. In the Netherlands, unemployment benefits are not cut one-for-one with income in order to provide incentives to take low-paying or low-hour jobs, and the reductions in the receipt of unemployment benefits we find are therefore smaller and insignificant. In line with the nature of our information treatment, we find in our survey that individuals in the treatment group who found a job report a significantly lower probability to work in their primary search occupation relative to those in the control group, and instead report a larger share of employment in the recommended occupations as well as in other occupations.

These large results beg the question to which extent treatment negatively affects other job seekers. To assess this, we calibrate a search and matching model to the data from our intervention and to publicly available data on individuals who search in other occupations. The calibration reveals that each additional treated individual benefits the control group and individuals already in the treatment group by freeing up their tight primary market. The difference between treatment and control is hardly affected by spillovers. Negative effects on other individuals who search primarily in more promising markets imply that each additional job created within our sample displaces 1/3 of a job for others. Because of the strong imbalance in tightness, efficiency remains at roughly two third of our empirical estimates even in a counterfactual with full roll-out to all individuals searching in low-tightness markets.

Our study contributes to a recent and growing literature on the effectiveness of targeted and tailored recommendations on job search behavior and labor market outcomes (Belot et al., 2019, 2022; Altmann et al., 2022; Le Barbanchon et al., 2023; Ben Dhia et al., 2022; **?**), which we discuss in more detail in Section 2.

The current study is of a much larger scale than Belot et al. (2019) and Belot et al. (2022), it focuses on job seekers searching in occupations with poor prospects (in contrast to Altmann et al. (2022)), and evaluates the effects on finding employment, including the type of occupation. The intervention we propose here is not embedded in a job search platform or an ‘automated recommendation system’. It therefore decouples the informational aspect, which can be used broadly in searching for jobs, from the saliency effect associated with specific vacancies receiving a more prominent position when searching for jobs. Here, the idea is to induce a change in the general ‘job search strategy’. Contrary to Le Barbanchon et al. (2023) our occupational suggestions do not rely on Machine Learning technology using search behavior as input, but are based on observed recent successful occupational transitions. As such, our algorithm is more suited to encourage the exploration of jobs that may not come to mind, in contrast to the above-mentioned machine learning algorithms (Li et al., 2020). This encouragement to explore beyond job seekers’ own search patterns is particularly relevant to those individuals most in need of alternative suggestions as they are better off moving away from the structural low demand in their occupation.

The design of the intervention, both in terms of individualizing publicly available information and in terms of the carefully designed message provide relevant and immediately applicable policy insights. As it is easy to implement, this intervention highlights a promising avenue to help job seekers in slack labor markets, a core task of Public Employment Services.

The rest of the paper is structured as follows. In Section 2 we review the recent literature. In 3, we describe the institutional context of our experiment. Section 4 describes the experimental design. We provide descriptive results regarding job search behavior of our sample (based on a pre-intervention survey) in Section 5. In Section 6, we present our empirical evaluation of the impact of the intervention using both administrative and survey data. Section 7 assesses externalities between job seekers through a calibration. Section 8 concludes.

2 Related Literature

Our study contributes to a body of work targeting information frictions in the labor market. Belot et al. (2019) design and test a recommendation system aimed at broadening the set of occupations considered. The experiment was conducted on a small sample (300) of UK job seekers and included tailored occupational recommendations in an online job search platform. The authors observed job seekers’ search behavior over the course of 3 months, and they found that personalized suggestions of alternative occupations affect job search and increase the chances of getting an interview. Most closely related to our study, Belot et al. (2022), Bächli et al. (2025) and Altmann et al. (2022) evaluate recommendation systems aimed at broadening job search and suggesting alternative occupations on larger samples.

Belot et al. (2022) focus on a sample of long-term job seekers. The recommendations are also embedded in an online job search platform, and they find a large and positive impact on labor market outcomes (probability of finding a stable job). The effects they report, based on a sample of 800, are relatively large and possibly driven by the specific focus on long-term unemployed.

Altmann et al. (2022) test a similar intervention in Denmark, although the suggestions are not directly linked to job ads, but displayed in the job seeker portal at the employment agency. They also evaluate another intervention providing information about the number of available vacancies in occupations that fit the job seeker's personal job search profile. The information provided is not one-off but is instead visible every time job seekers log into the portal. The study also implements a clustered randomization, varying the intensity of treatment across regions, to evaluate possible negative spillover effects on non-treated job seekers. Such spillovers are a concern that applies to experimental interventions targeting job seekers (Crépon et al., 2013; Gautier et al., 2018). Altmann et al. (2022) find significant and large effects of around 4-4.5% for working hours and earnings in the year after the intervention if treatment intensity is low. The effects are, however, canceled out in regions where the fraction of treated is higher. Since all treatment arms are varied in equal proportion, the design does not identify externalities separately. Interestingly, there are no apparent negative spillover effects on non-treated job seekers. It appears that the job seekers in the treated group crowded each other out in following the same new suggestions, but reduced the congestion in the occupations considered by the non-treated. Importantly, these interventions are not targeting occupations based on market tightness. In contrast, a distinguishing and novel feature of our intervention is that it reallocates workers from slack markets to occupations in much tighter markets. In such setting, congestion effects are likely to be smaller, as highlighted in Section 7.

Bächli et al. (2025) propose and evaluate new approaches to recommendations of occupations. One approach is to recommend occupations based on past experience, the other is to recommend occupations based on the skills needed in that occupation. They build a job search platform and measure job seekers' skills that correspond to non-cognitive skills identified in the O*NET descriptors. In a sample of 1,250 participants, they find that both recommendation tools tend to improve job-finding rates, although the effects are only significant for some subgroups of job seekers, specifically those who transit out of occupations that were a poor match to their skills.

Our study also relates to recent work by Le Barbanchon et al. (2023) who embed an AI-based recommendation system, and Behaghel et al. (2022) who encourage job seekers to use a system that predicts which firms are likely to recruit in the near future. These studies use prediction algorithms to recommend suitable opportunities. Le Barbanchon et al. (2023) implement a two-sided experiment on the job search platform of the Swedish Public Employment Service, where half of the vacancies and job seekers are treated. Some labor markets are left out as supercontrols. They find positive but small effects on the probability of employment (0.6%) among treated individuals after six months, and small and insignificant general equilibrium effects. The intervention designed by Behaghel et al. (2022) intends, like in our study, to move individuals from slack to tight labor markets. They find small positive effects and suggest that the lack of larger effects may be due to the fact that recommendations were not integrated in a job search platform. While our recommendations are not integrated in a platform either, another important difference between our approach and the AI-based approaches is that our algorithm aims at encouraging the exploration of jobs that do not come to mind. In contrast, AI-based approaches usually use past search behavior as an input, which limits exploration of very different alternatives. Our approach also uses information from successful *transitions* rather than data on search behavior. It is possible to alter AI-based algorithms to force more exploration, as shown in Li et al. (2020), but it remains challenging to identify *feasible* transitions without information on actual matches. Bied et al. (2023) argue that recommendation algorithms should be based on an objective that is close to that of job seekers, while avoiding the replication of their potentially biased behavior. They propose a hybrid algorithm that aims at maximizing the expected utility of a match (equal to the job's utility multiplied by the application's success probability), leveraging on information on job seekers' and vacancies' characteristics and predicted hiring probabilities derived from actual observed matches.

Lastly, Ben Dhia et al. (2022) evaluate an intervention aimed at providing assistance and advice using online tools. Job seekers receive online personalized advice on sectors and geographic locations, step-by-step planning assistance, regular reminders and encouragement messages, and general tips, such as how to behave during a job interview. While the advice touches on a large range of aspects of job search, effects in their setting are very small or absent.

Our intervention further relates to the literature on "mismatch unemployment" that argues that unemployment could be lower if tightness (vacancies per job seeker) would be equalized across markets. In their seminal paper, Şahin et al. (2014) find a limited role for this across geographical markets, but a much larger role across occupations. Our intervention intends to exactly encourage occupational shift. Marinescu and Rathelot (2018) do find quantitatively smaller effects after taking into account that workers search beyond their "primary" occupation. This also happens in our setting. In fact, even our control group obtains most of their jobs outside their primary search occupation, and we account for this in our calibration. Even though our intervention focuses on the most mismatched occupations and a relatively small country (the Netherlands), it still involves tens of thousands of job seekers. For this slice of the market it improves employment by more than 5% simply through low-cost information about feasible alternatives with much higher demand. Our calibration shows that externalities are small for the reason highlighted in the mismatch literature: more equal market tightness is an aggregate improvement and not just an improvement for the treated.

To summarize, our main contribution to literature lies in (1) our focus on job seekers searching in particularly challenging markets, (2) our design of a novel intervention providing viable *search strategies* based on recent successful transitions and tightness indicators, (3) which we evaluate in a large-scale randomized experiment. We provide the information in a way that is designed to be intuitive and easy to understand, and that is part of the existing way of working at the Public Employment Service. While embedding such information in a job search platform may further reduce frictions, an information message is very easy to implement in most employment service's infrastructures and adjust to reflect relevant changes in the labor market. It also requires minimal digital competency from the target population.

3 The Dutch Institutional Context

The Dutch Public Employment Service's core responsibility is the administration and payment of employee insurances, including unemployment benefits. In the Netherlands, individuals can apply for unemployment benefits when they lose their job if they meet all of the following criteria: they are insured for unemployment (which is generally included in regular employment contracts), their hours of work are decreased by more than five hours per week, they are available to start a different job immediately, they have worked at least 26 out of the last 36 weeks, and their transition to unemployment was not their own fault. When eligible, the unemployed can register with the Dutch Public Employment Service to receive unemployment benefits.[3] Upon registration, unemployed individuals get access to an online 'work folder' in which they are asked to share relevant information and documents (such as information about their previous job, personal situation, and their resume). As part of this process, unemployed job seekers can register up to three 'search occupations', i.e., occupations that the individual would like to find employment in.

All this information is used by the employment office to carry out another one of their core tasks: assisting job seekers in finding employment, particularly those with a large distance to the labor market. To this end, the employment office provides a number of services. While job seekers do get assigned a caseworker, the employment office also states that they "are calling on Dutch citizens to assume their own responsibility and on their self-reliance; the services we provide will increasingly be based on online self-service" (Uitvoeringsinstituut Werknemersverzekeringen , UWV). An important part of these 'online services' is the employment office's provision of two types of labor market information that we use in our experiment. Using data on the number of registered job seekers with a certain 'search occupation', as well as the number of available vacancies, the Public Employment Service assesses occupation-specific job prospects that they publish online.[4] The Public Employment Service also publishes a list of alternative occupations based on common occupational switches observed in resume data.[5]. While these data are available to job seekers, the website is not personalized. That means job seekers would have to have a good sense of their suitability to alternative occupations to act on that information. In our experiment, we (i) consolidate the available – and add new (e.g., on automation risk) – labor market information about occupations, and (ii) proactively provide this information in a personalized manner through email.

[3]Note that it is possible to work in a paid job while receiving unemployment benefits under certain conditions, e.g., partial unemployment

[4]Via a website with information about which occupations are most in demand and for which there is less work: https://www.werk.nl/arbeidsmarktinformatie/kansen-arbeidsmarkt

[5]https://www.werk.nl/arbeidsmarktinformatie/kansen-arbeidsmarkt/overstapberoepen-werk-vinden-in-ander-beroep

4 Experimental Design

4.1 Selection of occupations

As explained in Section 3, unemployed job seekers enter up to three 'search occupations' on the platform of the Public Employment Service. The aim of our experiment is to help unemployed job seekers who search in occupations with low employment prospects to consider different, more promising, occupations. Job seekers who search in these occupations are most likely to benefit from information on alternative occupations with better prospects.

To select occupations with poor job finding prospects, we use the 'job finding score'. The job finding score is a metric used by the employment office based on the ratio of vacancies to job seekers in the employment office's database and outflow rates of unemployment insurance recipients that is updated multiple times per year. These scores are computed for over 600 narrowly defined occupations (5-digit classification).[6] The score runs from 2 (very poor job prospects) to 10 (excellent job prospects). For the experiment, we selected all individuals interested in occupations with a score of 2, 3 or 4 in the spring of 2021, leading to 21 occupations (henceforth *selection occupations*). These 21 occupations exhibit a substantial variety in terms of their field (e.g., animal caretakers, waiters/bartenders, taxi drivers, graphic designer). While most of these occupations require a low or intermediate vocational education level, some (e.g., social workers) have higher requirements. The complete list can be found in Table 2 (including their relative share within the sample). Appendix Table A1 provides the original occupation names in Dutch.

We have access to all registered job seekers' records in the Netherlands and select all who have indicated on their CV that they are looking for a job in one of the 21 occupations with a very low job finding score. This implies that we also restrict our sample to job seekers who have completed their online CV, which automatically ensures a minimum level of computer skills. Given that we send our labor market information by email, this was desirable as we exclude those who may be less likely to read emails. Finally, we impose the restriction that, at the time of sample selection, job seekers should have at least one month of unemployment insurance benefits eligibility left, to ensure they would not automatically exit the sample before receiving the first information message.

To determine suitable alternative occupations (henceforth *transition occupa-*

[6]The occupational classification used is called 'BRC+' which resembles the ISCO classification, but more detailed and slightly modified to better reflect the Dutch labor market.

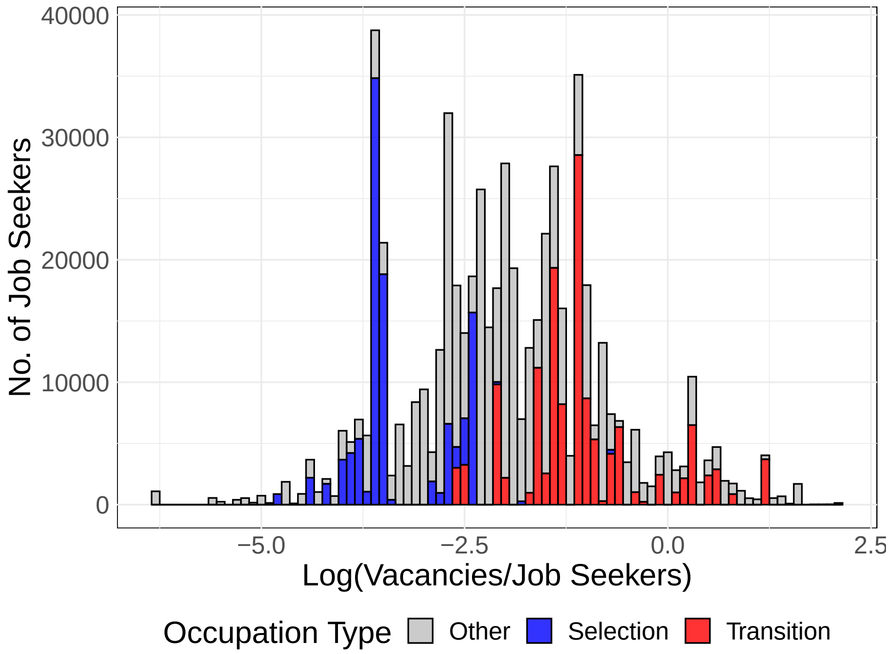
tions) these job seekers can transition to, we consider two metrics: historical switches, and current labor market prospects. Data on historical occupational switches comes from resume data that the Public Employment Service collects for all registered job seekers. This allows us to identify the occupations that other job seekers with skills, experience and educational backgrounds similar to the job seekers in our data most often switch to. We are agnostic about how these transitions have occurred, but the fact that they do occur suggests it is easy enough to move from one occupation to the other. A concern with this approach is that the occupational transitions we observe from past stocks of unemployed might not be representative of occupational mobility in the full population. While our primary focus is on unemployed job seekers, using panel data from the Dutch Labor Force Survey, we do find that occupational mobility in the Dutch population is similar to that from the data of the Public Employment Service. In our list of suitable alternatives, we include only occupations with favorable current labor market prospects, as indicated by a job finding score of at least 6. Depending on a job seeker's preferred occupation, we selected 7 to 9 alternative occupations. The combination of these two criteria ensures that we send job seekers a list of occupations that (i) they are likely to be (or can easily become) qualified for and (ii) have good job finding prospects. While we generally selected occupations with the highest number of historical switches among those with sufficiently good job opportunities — presenting them in order of observed switch frequency — we allowed some leeway for the expertise of the Public Employment Service.

To put the differences in labor market prospects in perspective, Figure 1 shows the distribution of market tightness (the logarithm of vacancies divided by the number of job seekers) for selection, transition and other occupations.[7] The figure confirms the expected pattern: selection occupations have much lower vacancy to job seeker ratios than transition occupations. The average ratio for selection occupations is approximately 0.04, whereas it is above 0.5 for transition occupations. Data from the Dutch Labour Force Survey further shows that the occupations we suggest offer better wages, more often full-time hours and are less often jobs with temporary contracts compared to the selection occupations.

One final concern is that job seekers looking for work in our selection occupations differ from those looking for work in transition occupations in ways that

[7] These statistics are based on 'Open Match Data' published on April 13, 2021 by the Dutch Public Employment Service. These data contain information on the number of job seekers who have indicated to search for work (or have experience) in a certain occupation and the number of vacancies in these occupations. An important caveat is that job seekers may register in multiple occupations. As the data is available at the occupation level, we can not observe this.

Figure 1: Labour market tightness of selection, transition and other occupations



make them less attractive on the labor market. Tables A2 and A3 in the Appendix shows how job seekers looking for work or with experience in, respectively, our selection occupations differ from those in the transition occupations we selected, and all other occupations. There are a number of striking patterns. Educational attainment does not materially differ between job seekers in the different categories of occupations. We consider this to be encouraging, as it suggests that lack of educational attainment is not a barrier to transitions. With regards to geographical mobility, job seekers in our selection occupations have an equally sized search radius compared to those in transition occupations. We consider this another positive signal about the quality of our transition occupations, as it indicates that job seekers will not be forced to compete with others who are willing to consider a much larger set of jobs. In contrast, job seekers in other occupations are willing to search for work in geographically more distant places. Job seekers in our selection occupations further are younger on average, and less likely to be male than in transition and other occupations. Table A3 shows that there is no difference in experience between the selection occupations and transition occupations, despite the age difference; another positive signal for the feasibility of these transitions. In short, while job seekers looking for work in our selection occupations differ from those looking for work in transition occupations, they are equally educated, experienced, and willing to commute.

4.2 Interventions

Our intervention aims to ensure job seekers (i) are aware of the poor labor market prospects in their occupation of interest and (ii) learn about suitable alternative occupations. We present the information through a visualization that we designed together with the communication department of the Dutch Public Employment Service and was sent to job seekers by email.[8] In the message's introductory text, we stress a number of key points. First, we provide information about market tightness in the main occupation of interest. Specifically, we inform job seekers that the occupation in which they are currently looking for work has few vacancies available, but that many unemployed people are looking for work in that occupation. This implies poor prospects of finding employment. Second, we mention that with their skills and experience, there are alternative occupations they would qualify for (or could relatively easily qualify for) that provide much better job prospects. In this way, we try to convince job seekers of the urgency of considering alternatives, as well as reassure them that their skills and experience will fit in the new occupation.

Figure 2 shows an example of the visualization we use. We first list job seekers' primary occupation of interest, together with a bar of which the length and color (green, yellow or red) represent the likelihood of finding a job (1). Then, we show each of the alternative occupations that we matched to the job seeker's primary occupation of interest. The order in which we show these alternative occupations is largely based on the number of historical transitions we observed and, to a lesser degree, on the job finder prospects associated with the alternative occupation. For each of the alternative occupations, we show the job finding score in the same way as for their occupation of interest (2). We also include two main skills associated with the occupation (3). While the use of historical switches between occupations ensures that all presented suggestions are relevant, individuals may have idiosyncratic skills that fit well with one occupation in particular. We want to ensure that job seekers realize that their existing skills and experience can be valuable in another occupation. Many of the occupations with poor prospects we select are at risk of being automated. The set of alternative occupations we propose to them have better short-term job prospects. However, the longer-term prospects of these occupations vary. As job seekers may want to avoid occupations with poor long term prospects due to automation risks, we include this information in the treatment as well. If an occupation is at low risk of automation (50^{th} percentile of

[8]The design was selected based on a pilot study where job seekers indicated their preferences over multiple versions of the visualization.

Figure 2: Example of information message visualization

automation risk or lower), we mention this to the job seeker (4).[9] Lastly, there is a link for more information about the occupation (extended description, required certifications, various job titles, etc.) (5).

The experiment included another treatment arm, adding to the informational message a link to a motivational video showcasing job seekers who recently made a career transition. In cooperation with a professional video maker, we compiled their stories into a motivational compilation video that addresses the main challenges, costs and benefits of occupational transitions. Since a small fraction of people clicked on the link, we merged both treatment arms for the analysis. Details on the motivation intervention are provided in Appendix C.

4.3 Randomization, data collection and timeline

We selected the sample on 15 March 2021, which was composed of the stock of job seekers that were registered to search primarily for one of the 21 selection occupa-

[9]The automation risk is measured with the indicator proposed by Nedelkoska and Quintini (2018)

Table 1: Timeline experimental set-up and sample sizes

Date	Event	Treatment 1 (Information) N = 10,075	Treatment 2 (Info + video) N = 10,050	Control N = 10,004	Total N = 30,129
March 23, 2021	Pre-survey sent	3308	3310	3292	9910
	Respondents	899	959	931	2789
April 12, 2021	First message	*Information*	*Info + video*	*No message*	
		10,075	10,050	10,004	30,129
May 10, 2021	Second message		*Only video*		
			9022		
May 28, 2021	Third message	*Information*	*Information*	*No message*	
		8388	8450	8399	25,237
June 7, 2021	Post-survey	2766	2781	2752	8299
	Respondents	400	457	421	1278
June 24, 2021	Outflow survey 1	1833	1813	1799	5445
	Respondents	579	550	588	1735
Sept 9, 2021	Outflow survey 2	1427	1402	1411	4240
	Respondents	473	491	439	1403
Dec 1, 2021	Outflow survey 3	1057	1037	1004	3098
	Respondents	377	353	327	1057
April 5, 2022	Outflow survey 4	402	412	443	1257
	Respondents	106	107	136	349
August 30, 2022	Outflow survey 5	402	411	389	1202
	Respondents	130	118	104	352
Jan-01-2020 until Dec-31-2023: Admin data: employment, benefits		10,068	10,041	9,992	30,101

Minor sample selection steps were applied prior to each intervention message: only those who (1) did not yet exit unemployment insurance, (2) had valid email addresses and (3) did not change their 'unemployment-indication' were included. Prior to the post-survey an additional subset was removed that either denied the consent statement in the pre-survey or that clicked the 'unsubscribe' button in the pre-survey. Each survey was followed by an email reminder after one week.

tions. We ended up with 30,129 individuals who remained (partially) unemployed until the first message (April 12). These individuals constitute our experimental sample. Job seekers were randomly assigned to three equally sized groups: (1) the information group, (2) the information + motivation group and (3) the control group. Randomization was stratified by gender, unemployment duration and selection occupation. A random third was selected to receive pre- and post-intervention surveys (equally-sized across treatment groups).[10] After selecting the baseline sample, we administered the pre-intervention survey followed by the intervention messages and the post-intervention survey. Subsequently, we sent out 'outflow surveys' to those who found jobs. Finally, we obtain access to administrative data on employment and unemployment insurance (UI) benefits for the entire sample for the time period 2020 - 2023. Table 1 provides a precise timeline with corresponding sample sizes.

The pre- and post-survey contained questions about job search behavior (primary search occupations, alternative search occupation, applications and interviews), questions about beliefs (job findings prospects in the primary and alterna-

[10]Response was incentivized through donations to charity.

tive occupations, beliefs about wages) and various questions regarding willingness to explore and search for occupations other than the primary occupation. Further details can be found in Section 5 where we present descriptive statistics.

We sent the first intervention message on April 12, 2021. It contained the information visualization for both treatment groups and the additional video link for the motivational treatment group. In Section 6.1 we provide statistics on the engagement with the email. We find that 64% opened the message, but few clicked on the link to the video. As a result we sent an extra message with only the video link to the corresponding treatment group on May 10, 2021. Finally, a general reminder was sent on May 28, 2021, containing the information visualization, modified based on clicking statistics from the first message.

The administrative data that we use contains employment spells, including earnings and hours, as well as benefits receipts. However, it does not contain information about the occupation. To collect information on the occupations the unemployed exit to, we therefore administered outflow surveys. Every two to three months, we selected all job seekers in our sample for whom we observed in the administrative data a labor income increase of more than €300 in the preceding months.[11] Such a substantial increase in earnings should reflect a new job. Since many job seekers obtain part-time and temporary jobs during their unemployment spell, they may not have left the unemployment insurance system yet and therefore this is a preferred selection criteria. In addition, we also added everyone who left the unemployment insurance system with registered indication 'employed' to the outflow-survey sample. The outflow survey contains a number of questions about the new job (starting date, occupation, and a comparison of tasks relative to the pre-unemployment job). It is important to note that these outflow surveys are intended only for those who found a job. For that reason, we specify in the invitation that the survey is only relevant if individuals indeed found a job. Once individuals open the survey, they are asked once again if they indeed found a job and only then do they continue on to the survey.

4.4 Sample restrictions

For the main analyses in our paper, we impose two more sample restrictions on top of those discussed in Section 4.1. First, we restrict our main analyses to occupations with structurally poor prospects, rather than those whose poor prospects

[11]For example, for the first outflow survey (in June 2021) we selected recipients for whom monthly earnings in April and/or May were at least €300 higher than their highest monthly earnings in February and March.

can primarily be attributed to Covid. Due to the Covid pandemic, the state of the labor market fluctuated substantially around the start of our experiment, as illustrated by the fluctuations in unemployment and vacancy rates depicted in Figure B1 in the Appendix. Until early 2020, unemployment was low and stable, while it increased to 5.5% in the summer of 2020 and steadily decreased from there on. Vacancies mirror this trend. Despite our selection occupations sharing low prospects in early 2021, they differ substantially in the longer-run trends. Most importantly, there was large variation in the degree to which occupations were affected by the various social distancing measures that were imposed to minimize the number of Covid cases. We can, in fact, identify a subset of our selection occupations that offered poor prospects primarily because of the Covid measures, but offered substantially better prospects prior to the Covid pandemic *and* after many restrictions were lifted over the summer of 2021. We classify all selection occupations as 'Covid-occupations' if the job finding score decreased with at least two points at the onset of the Covid pandemic (measured February 2020) *and* increased at least two points in the summer of 2021 (measured September 2021). Within our 21 selected occupations, there are 7 'Covid occupations' and 14 'Non-covid occupations'. In Figure B2 in the Appendix, we show how the job finding score evolves for the two groups. As expected, the Covid occupations (right panel) offer decent prospects before the pandemic and almost fully recover in late 2021. For the non-Covid occupations (left panel) this is not the case, and job prospects have been structurally poor during the past years.

That said, the impact of our treatment on those looking for work in Covid occupations is also interesting. In fact, we preregistered the split between Covid and non-Covid occupations as a heterogeneity analysis without knowing that their labor market prospects would diverge. At the time of our intervention, there was much policy interest in labor market mobility for occupations affected by Covid, and one might therefore have expected that the impacts of our treatment would be large. However, there are two reasons for why one might expect smaller treatment impacts for those looking for work in Covid occupations. First, many job seekers may have anticipated that the Covid restrictions were temporary and these individuals may therefore have been less willing to consider switching occupations. Second, even if the intervention does encourage occupational transitions, the improvement in labor market opportunities following these transitions is smaller because the low demand in those occupations was not structural.

Second, we restrict the main analysis sample to unemployed who did not work at all before the start of our experiment. The main reason for doing this is that we observe a striking difference in pre-intervention hours worked, despite the control and treatment group being balanced on demographics (indicating the randomization was successful). Figure B6 in the Appendix shows this difference. This difference in hours worked between control and treatment group prior to the intervention may create a bias in our treatment effect estimates. We address this by restricting our sample to those individuals who do not work in March 2021 (just before our intervention starts). Within this sample all characteristics are balanced between control and treatment groups. This group in particular can be expected to benefit most from the information about potential career switches since they do not work at all. This restriction is similar to that used in Crépon et al. (2013).[12] Unless otherwise mentioned, the Figures and Tables from Section 5 onwards are based on the 'Non-covid, 0-hours worked prior to the experiment' sample.

4.5 Hypotheses

The aim of the intervention is to make job seekers aware of suitable alternatives to the occupations they are currently looking for work in, and motivate them to look for work in these occupations. If effective, the likely impact on job finding is not straightforward, however. In the short term, the expected effect on the likelihood of finding a job is ambiguous. On the one hand, when individuals start looking for work in more promising occupations, they will likely have more vacancies to apply to, with fewer competing job seekers per vacancy. On the other hand, despite the relevancy of the suggested alternatives, job seekers will likely have less experience in these new occupations, decreasing their comparative advantage. Moreover, they might need some time to adjust their search efforts.

Once individuals have had time to adjust, a successful intervention would likely lead to treated job seekers ending up in different occupations. Since the alternative occupations offer better job opportunities, one would expect that these job seekers will be more often employed and remain with the same employer for longer. While the differences in the demand for and supply of labor between these occupations may lead to higher wages in the alternative occupations, it is important to note that we do not take this into account in the intervention. We therefore make no predictions on changes in earnings conditional on having a job. Regardless, total earnings are likely to be different between the control and treatment groups, because of different employment rates.

[12] They "focus on results for those who did not claim to be employed at baseline" (p.550).

Table 2: Descriptive statistics (administrative data): comparison of samples

	Overall sample	0 hours worked in Mar-2021		
		Overall	Non-Covid	Covid
Demographics:				
Male	25%	25%	28%	22%
Age	47 (13)	47 (13)	48 (12)	47 (13)
Unemployment duration (wks.)	32 (28)	30 (26)	32 (27)	28 (24)
Remaining benefits (wks.)	51 (30)	52 (30)	52 (29)	53 (31)
Lower education	22%	22%	17%	29%
Medium education	56%	55%	52%	58%
Higher education	22%	23%	31%	14%
Experiment:				
Zero hours March-2021	68%	100%	100%	100%
Covid selection occ.	49%	52%	0%	100%
Treatment	67%	67%	66%	67%
Pre-survey completed	9.3%	10.0%	9.7%	10%
Observations	30,129	20,632	10,738	9,894

Remaining benefits and unemployment duration are measured in March 2021.

5 Descriptive results

Before turning to the analysis of the impact of the interventions in Section 6, we first provide descriptive statistics for our sample and document a range of descriptive findings regarding job search behavior and beliefs in our data.

Table 2 shows the characteristics of our sample, and how these change when we impose the sample restrictions mentioned in Section 4.4. The first column shows that our initial sample of 30,129 job seekers receiving unemployment insurance skews female and is 47 years old on average. Average unemployment duration is over 7 months, indicating that many seem to be struggling to exit unemployment, though most job seekers are still entitled to substantial benefits: 51 weeks on average. More than half of the job seekers have attained a two- to four-year vocational degree (medium education), with the rest being equally divided between those without a post-secondary education degree[13] (low education) and those who obtained a higher education degree (higher education). Perhaps most interesting is the share of job seekers that was still actively working at the time of selection into the experiment (March 2021) and the share looking for work in 'Covid occupations', as these are what determine inclusion in the main analysis sample. The Table shows that about two-thirds of our sample did not work at all. About half of selected job seekers is looking for work in a Covid occupation.

The second column of Table 2 shows how restricting the sample to those who

[13]This includes individuals who graduated from a 1-year vocational education program, as these are not sufficient to obtain a 'starting qualification'.

do not have any job in March-2021 affects the sample characteristics. There are very few changes.[14] The third column adds the 'Non-Covid' restriction. This, by construction, affects the occupational composition (see Table A4 in the appendix). As receptionists, a female-dominated occupation, is now excluded, the share of men increases. Moreover, this sample is clearly higher educated than the overall sample, with about 31% having a higher education degree. Naturally, the opposite holds for the 'Covid' sample (Column 4).

5.1 How do job seekers search?

For the subsample that completed the pre-intervention survey that meets the sample selection criteria (N = 1,040) we obtain a rich set of responses regarding job search beliefs and activities. While those invited to the survey were randomly selected, those who responded may not be. Comfortingly, in Table A7 in the Appendix we compare the survey respondents to the rest of the sample and conclude they are fairly similar. There are no significant differences in gender composition or unemployment duration. The only differences are in remaining benefit rights (higher for respondents), age (respondents are older) and there is a slight difference in the distribution across selection occupations. Based on observable characteristics, we conclude that we can interpret the survey responses as fairly representative of the full experimental population.

Survey respondents first indicate what their primary search occupation is (typically the selection occupation) and which alternative occupations they consider. In Figure B3 in the Appendix we show how many occupations respondents list as their search occupations (their primary occupation, as well as alternatives). Almost 25% searches for work in only one occupation, while 40% searches in two or three occupations. Around 35% searches in more than three occupations. In Appendix Table A9, we show that most respondents (i) spend at least some hours per week exploring alternative occupations, (ii) are fairly willing to consider new occupations, (iii) have quite some confidence in their ability to work in an occupation in which they have no experience, and (iv) believe that their skills are transferable. Over 50% of respondents expects to widen their search in terms of occupations if they are still unemployed in two months.

For the primary and first alternative search occupation, we collect various measures of job search activities and elicit beliefs about the returns to job search (see

[14] Table A4 in the appendix contains a similar sample comparison for the distribution across selection occupation, showing that the share of taxi drivers is the only major change when selecting those with no-employment in March 2021.

Table 3: Comparison primary and alternative occupation (survey data)

	Primary occupation	Alternative occupation	P-value
Job search activities:			
Job finding score	3.00 (0.55)	4.29 (1.55)	<0.001
Applications sent (past 2 weeks)	3.2 (7.0)	2.5 (4.9)	0.009
Job interviews (past 2 weeks)	0.44 (1.55)	0.36 (1.00)	0.21
Interviews per application	0.15 (0.54)	0.21 (0.50)	0.078
Expectations:			
Expected job offer rate	0.09 (0.10)	0.10 (0.11)	0.091
Expected wage (in euro)	2,937 (893)	2,929 (951)	0.86
Reservation wage (in euro)	2,823 (863)	2,801 (873)	0.61
Job stability	0.67 (0.31)	0.70 (0.28)	0.013
Exp. appl. if equal job offer rate	3.92 (6.81)	4.33 (8.60)	0.31
Exp. appl. if equal wage	4.10 (6.93)	4.46 (9.13)	0.41
Exp. job offer rate in 2 months	0.08 (0.10)	0.08 (0.10)	0.60
Observations	1,040	1,040	

Standard deviations in brackets. Only Non-covid occupations and individuals that worked zero hours in March-2021."Primary occupation" is the occupation that the respondent searches primarily in. "Alternative occupation" is the occupation that the respondent considers the most important alternative occupation of search. The number of observations varies slightly across variables due to item non-response. "Job stability" is defined as the expected probability of being able to keep a new job for at least two years. "Exp. appl. if equal job offer rate" is the expected number of applications per week in case the job offer rate would be equal in the primary and alternative occupation. "Exp. appl. if equal wage" is the expected number of applications per week in case the job offer rate *and* the expected wage would be equal in the primary and alternative occupation. "Exp. job offer rate in 2 months" is the expected job offer rate in case the respondent is still unemployed in two months time.

Table 3). As the primary search occupation is for most individuals the selection occupation, it has a low job finding score (3.00, Row 1).[15] The first alternative occupation that they search in offers better prospects with an average job finding score of 4.29. In the previous two weeks the average number of applications for jobs in the primary occupation is 3.2, while it is 2.5 for the first alternative occupation (Row 2). The resulting number of job interviews follows a similar pattern: 0.44 for the primary occupation and 0.36 for the first alternative. The number of interviews per application is slightly higher for the alternative occupation (Row 4), which is consistent with the higher job finding score. Except for the raw number of interviews, all of these differences are statistically significant at at least the 10% level.

5.2 How well are job seekers informed about job prospects?

We elicit a range of beliefs about the returns to job search activities and labor market prospects. The key question of interest is whether expectations regarding

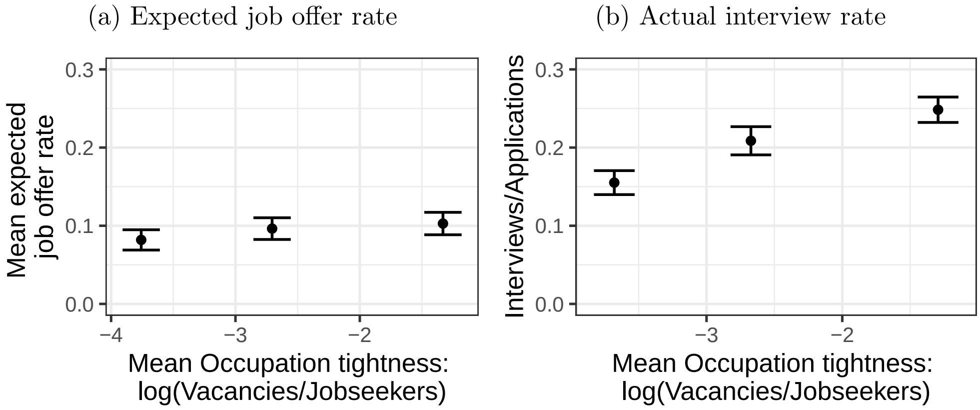
[15]Participants can indicate at the start of the pre- and post-surveys that the selection occupation is not their primary occupation of search and provide a different primary occupation.

job prospects in various occupations align with actual prospects. In addition, we explore whether these expectations drive job search activities. First, respondents indicate their belief about the number of applications it requires on average to obtain one acceptable job offer, both for their primary occupation and their first alternative. By inverting this number we obtain the expected job offer rate (per application), which is fairly small on average (0.09, Row 5 in Table 3) and strikingly similar between the primary and alternative occupation. The job finding score differs substantially but is low for both: 3.00 and 4.29 for the primary and alternative occupation, respectively. For context, we only included occupations as transition occupations if their score was at least 6. In Figure B4 in the Appendix, we indeed confirm that the vast majority (78%) searches in none of the suggested occupations before receiving the information. A small group was already searching in one of our suggested occupations (18%) and a negligible share was already searching for more than one suggested occupation.

To provide further insights on how well job seekers are informed about job prospects, we link their beliefs to the actual job finding prospects. We exploit variation across individuals in their selection of alternative occupations. Specifically, we examine the relation between occupation's log-tightness (in three equally sized bins) and expected job offer rate in Figure 3. In Panel (a) we find that this relation is very flat: regardless of the true tightness, the expected job offer rate of an application is always close to 0.1. It seems that job seekers do not select their alternative search occupations on the basis of better job prospects: most job seekers select alternatives with only marginally better job prospects, while, surprisingly, even those who select high-prospect alternatives do not seem to be aware that these have better job finding chances.

These conclusions hinge on the occupational tightness indeed being a good measure of the likely job finding rate for our selected set of job seekers. Conditional on their background, the prospects in these alternatives might not actually be so favorable. In Panel (b) of Figure 3, we investigate whether the better job prospects translate into better returns to job search based on the reported number of applications and interviews. Occupations with a more favorable tightness indeed show a higher interview-per-application rate. Job seekers do not seem to learn much about the difference in these prospects either. Table A10 shows how beliefs change over time for the control group. While job seekers become slightly more optimistic about the probability of finding a job in two months, we find no evidence that they (differentially) update their beliefs about the prospects of the primary and alternative occupation.

Figure 3: Occupational job finding prospects for job seekers' first alternative occupation. Only Non-covid occupations and individuals that worked zero hours in March-2021. Observations grouped into three equally sized bins.



Returning to Table 3, we find that job seekers also have extremely similar wage expectations for the primary and alternative occupations and also hold similar reservation wages. Expectations about job stability (the probability of keeping a new job for at least two years), are slightly more optimistic for the alternative occupation with a small but significant difference. Rows 9 and 10 show how many applications job seekers think they would send *per week* to their primary and alternative occupations if the job offer rate and the wage, respectively, are equalized. Interestingly, this number is much higher than what they do in reality and closes the gap between the primary and alternative occupation. Finally, the last row shows that job seekers expect to update their expectations about job offer rates, but only slightly. If they are still unemployed in two months time, they expect the job offer rate to be 0.08 for the primary occupation (compared to 0.09 now) and 0.08 for the alternative occupation (compared to 0.10 now).

Summing up, we draw the following two key conclusions regarding job search strategies of the job seekers in our sample.

1. While most job seekers indicate that they are willing to search in alternative occupations, and confident about doing so, the majority searches only in 1 to 3 occupations with mediocre prospects.

2. Job seekers do not appear to be informed about the difference in job finding prospects between their primary search occupation and potential alternatives.

These two findings are encouraging for the potential of our information interventions, which bring to job seekers' attention the stark differences in job prospects between their primary occupation of interest and a set of suitable alternatives.

6 Empirical analysis

6.1 Take-up: opening message and clicking statistics

Job seekers in the treatment groups received their first message with occupational information on April 12, 2021 (see Section 4.3). We first compare the suggested occupations to the occupations in which job seekers report they search, to assess to what degree we provide 'new' information. Then we present statistics on engagement: whether they opened the email and clicked on the links. These statistics provide strong indication of 'treatment take-up'.

A total of 19,960 job seekers received the first message (both treatment groups). From these, 12,804 opened the email (64%). Each occupation is clickable for more information about the occupation (e.g., description, tasks, skills, related occupations, educational level). The share of recipients that clicks on each occupation provides a measure of how interesting each occupation is to job seekers. In total, we observe 4,975 clicks on occupations. These are not evenly distributed across the total of 165 presented suggestions (21 selection occupations with each between 7 and 9 occupational suggestions). Appendix Table A11 shows that job seekers are more likely to click on occupations that have (i) many historical transitions, (ii) a higher job finding score, and (iii) show the low automation risk indicator. Also, the occupation ranked first receives more than double the clicks of those ranked lower.

We sent a reminder email with a similar visualization on May 28th. In coordination with the communication experts from the Public Employment Service we decided to change the content slightly. Using the regression model from Column (3) of Table A11 we generated predicted interest, controlling for the rank in the first message. Thus, we predict interest based on the job finding probability, the automation risk indicator and the number of occupational transitions. Using these predictions we created a new ordering which was implemented in the second message. In addition only the new top-5 suggestions were included to make the message slightly shorter. The message was sent out to 16,838 individuals, of which 11,475 opened it (68.1%). Of those who opened it, 2,442 clicked on a link (21.3%). Over both emails, 15,867 individuals opened at least one (78.8%), of which 4,874 clicked on at least one link (30.7%).

The motivational treatment group received a version of the first message that contained an extra paragraph with a link to the motivational video. In contrast to our occupational suggestion links, very few people (0.5%) clicked on the video link. A likely explanation might be that the video was only provided *after* the information visualization, and many readers may not have reached this part of the message. Of course, it might also be that job seekers are simply not interested in the video. We sent an additional message to this treatment group that *only* provided the video link (not the occupation information). This message led to a slightly higher click rate (7.5%), but still the overall share of the motivational treatment group that has seen the video remains low. Given the low 'take-up' of the video, our analysis in the next sections combine the two treatment groups and only measures the effect of the informational content that both groups received.

6.2 Experimental analysis

Given the randomized assignment, the empirical strategy is straightforward and we can simply compare outcomes across the treatment and the control group.[16] Following our pre-analysis plan, we first consider the primary outcomes, which are employment (earnings, hours and occupation) and benefit receipt.

6.2.1 Treatment effects on employment, hours worked, labor earnings and benefits receipt

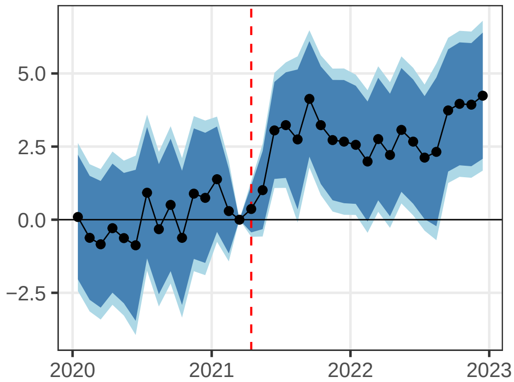
We have monthly data on hours worked, benefits receipt, and total labor earnings for our entire sample through the administrative records provided by the Public Employment Service. Using the data on hours worked and earnings, we further construct an indicator for whether an individual is employed or not.[17] We define employment status in two ways: (i) whether the individual had positive earnings in a month and (ii) whether the individual earned at least €300 in the month. We consider hours worked as the most comprehensive measure of employment as it aggregates across potentially multiple part-time and temporary jobs. Finally, we examine total labor earnings as the most complete measure that takes the wage level into account. For benefit dependence, we use the amount of benefits received.

[16]Balancing checks are reported in Appendix. We obtain near-perfect balance on the stratification variables (gender, unemployment duration in three bins and selection occupation), but the samples are also balanced on all other characteristics (see Appendix Table A5 for our main analysis sample and Appendix Table A6 for the entire sample). In Table A8 in the Appendix, we show that the samples are also balanced in terms of responses to the pre-intervention survey.

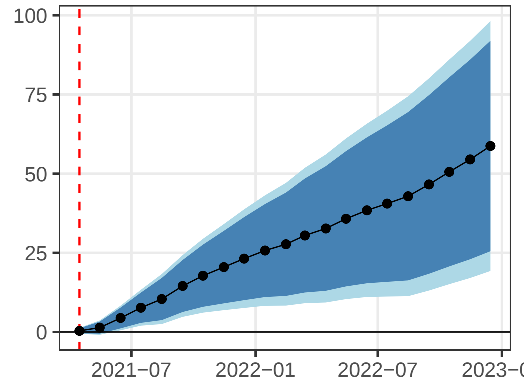
[17]Many job seekers find temporary and part-time jobs while continuously receiving (fluctuating) unemployment insurance benefits. Therefore it is typically impossible to define outflow from unemployment insurance benefits at one point in time.

Figure 4: Treatment impact on hours worked and benefit receipt

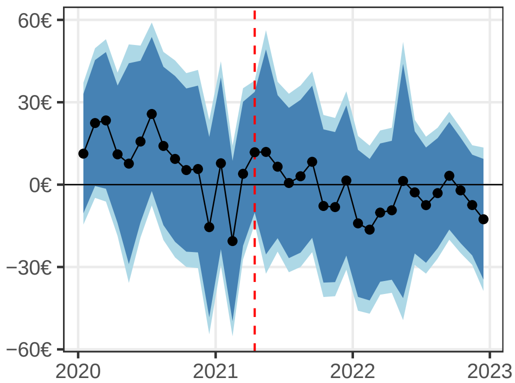
(a) Hours worked



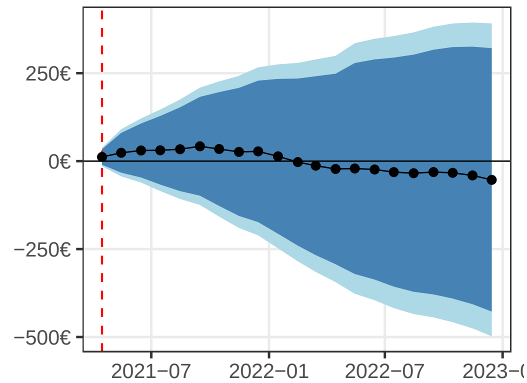
(b) Hours worked (cumulative difference)



(c) UI benefit receipt



(d) UI benefit receipt (cumulative effects)



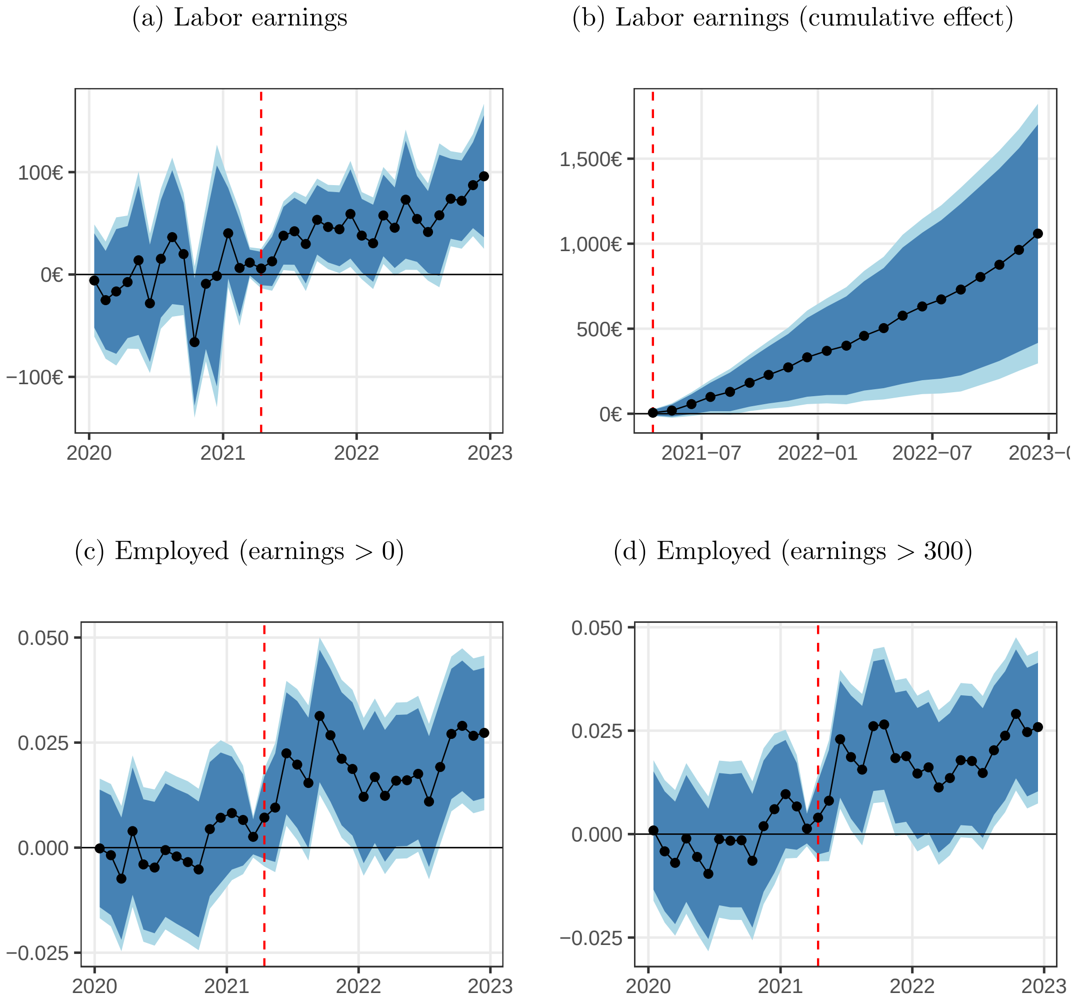
Note: Sample contains Non-covid occupations and individuals that worked zero hours in March-2021. The dashed red line indicates the first intervention message.

To estimate the treatment impact at various points in time, we regress the outcome measure in month t on a month fixed effect (γ_t), demographic (time-invariant) controls (X_i) with time varying coefficients, and a treatment group dummy (T_i) with time varying coefficient:

$$Y_{it} = \gamma_t + X_i\beta_t + \lambda_t T_i + \varepsilon_{it} \tag{1}$$

Standard errors are clustered at the individual level and the months t run from January 2020 (14 months prior to the treatment) until December 2022 (20 months after treatment). The pre-treatment months are included as an additional check of adequate randomization between treatment and control groups. Covariates X_i

Figure 5: Treatment impact on earnings and employment



Note: Sample contains Non-covid occupations and individuals that worked zero hours in March-2021. The dashed red line indicates the first intervention message.

are included to increase precision, although point estimates are hardly affected when they are excluded.

Figure 4 shows estimates of the treatment effects (λ_t) on hours worked and benefits receipt, including 90 and 95% confidence intervals, for our baseline sample (job seekers in Non-covid occupations who did not work in March 2021). In Panel (a) we see that prior to the treatment there are no significant differences in monthly hours worked, with, by construction, a zero difference in March-2021. After the intervention, the treatment group always worked around 2-4 hours per month more than the control group. This difference is statistically significant in most months. Given the consistently positive coefficients, we consider the cumulative number of hours worked (starting from the treatment in April 2021) in Panel (b). We find indeed a monotone increasing difference between the control and treatment group reaching approximately 55 additional hours worked by the end of 2022. The difference is, again, statistically significant at the 5% level. We conclude that there is clear evidence that our treatment increased hours worked.

Benefit dependence is assessed in Panel (c), where the outcome is monthly UI benefit receipt (in €). We find no indication of a treatment impact, with the post-treatment coefficients close to zero and never statistically significant. The lack of results also translates into cumulative UI benefit receipt as shown in Panel (d). There are several explanations for why the increase in hours worked is not reflected in reduced benefit receipt. First, the hours increase may simply be too small in magnitude to induce a reduction in benefits. Second, a substantial part of the increased hours of work may have occurred for individuals who, at that time, exhausted their UI benefits. The latter is particularly relevant, since we show in Section 6.2.3 that the treatment impacts are larger among long-term unemployed who are likely to exhaust their benefits during our observation window.

In Figure 5 we consider monthly labor earnings. In Panel (a) we find no significant differences prior to the intervention, while the post-treatment coefficients are all positive, with most months showing statistical significance at the 5% level. In Panel (b) we find that the treatment impact on cumulative earnings grows over time, again significant at the 5% level. Toward the end of 2022, the treatment impact reaches a magnitude of around €1100. In Panel (c) we consider employment, measured by an indicator for positive labour earnings. Again we find no significant differences in any pre-treatment period, and a positive difference in all post-treatment periods (significant at the 5% level for most months). Employment is about 2 - 2.5 percentage points higher in the treatment group. These findings are corroborated by Panel (d) where we use a higher threshold for earnings to capture 'substantial' labour earnings (exceeding €300 per month). Here we also find a significant increase in employment for those that received the information messages.

The impact on the binary employment indicators appears of similar magnitude as the impact on hours and earnings, suggesting that (most of) the positive effects are extensive margin responses. Indeed, if we estimate our model for hours worked conditional on positive hours, or on earnings conditional on positive earnings, we find zero impact (see Appendix figures B5). While this supports the interpretation that effects work mainly on the extensive margin, it also suggests that the *additional* jobs found due to the the intervention are not of worse quality: they

Table 4: Outflow survey

	Control	Treatment	P-value
Invited to survey	40.4%	42.0%	0.10
Opened survey	13.2%	14.8%	0.019
Responded job found	11.7%	13.1%	0.030
Observations	3,611	7,127	

Sample contains Non-covid occupations and individuals that worked zero hours in March-2021. Figures are aggregates across all outflow survey waves (see Table 1 for details).

are associated with a similar number of working hours and similar earnings.

To provide a sense of the magnitudes, we can compare these estimates against the control-group means measured 18 months after the intervention. For monthly hours worked the control mean is 66.1 hours, such that the 4.0 additional hours (Figure 4, Panel (a)) constitute a 6.0% increase. For labor earnings the control mean is €1118 such that the approximate €72 increase (Figure 5, Panel (a)) constitutes a 6.4% increase. For employment the control mean is 55.9% such that the approximate increase of 2.9%-point (Figure 5, Panel (c)) constitutes a 5.2% increase. These effect sizes appear of similar magnitude and we consider them sizable given the relatively light-touch nature of the intervention and the (likely) partial take-up of the informational content.

6.2.2 Type of work found

Our intervention was intended to stimulate mobility towards alternative occupations. Because administrative data does not capture the occupations of jobs found, we analyze our outflow survey. As described in more detail in Section 4.3, the outflow survey was sent at three-month intervals to all experiment participants for whom administrative records reported a substantial increase in monthly earnings over the preceding months (i.e., €300). Such an increase in earnings is a strong proxy of job finding.[18] As a result, the survey provides occupational information for individuals who (i) for the first time post-treatment experienced a substantial labor earnings increase and (ii) confirmed in the survey that they started a new job and completed the survey questions.

Table 4 provides a summary of the response to the survey. Of the 10,738 individuals in our main sample, 4,452 (41%) received an invitation to fill out the outflow survey. The first row shows that the share of treated individuals who

[18]Practical challenges in terms of data access made it impossible to use actual data on job finding on a rolling basis for selecting survey recipients.

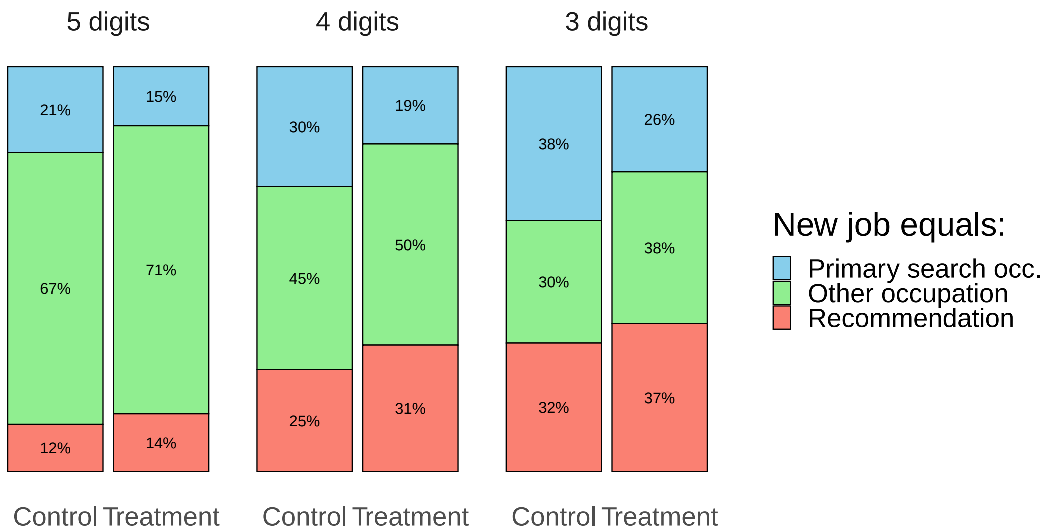
received an invitation is slightly higher (42.0%) than that of the control group (40.4%). This is consistent with our results based on administrative data from the previous section, although the difference here is not statistically significant. The share opening the survey is significantly higher in the treatment group. This is noteworthy, as the invitation explicitly mentioned that the survey was intended for people who had found a job. In the last row we show that 11.7% of the control group individuals confirmed in the survey that they started a new job, while this is larger for the treatment group (13.1%). Another indication that our treatment indeed increased employment. While good news for our treatment, this difference may create selection bias in the responses. As we will show below, our qualitative results hold even when accounting for selection bias.

Figure 6 provides information on the types of jobs found. The figure shows whether the occupation in which a job was found was the same as their primary search occupation, equal to one of our recommended alternatives, or an entirely different occupation. We show these results at three levels of granularity: 5-digit (the most detailed level and the level at which selection took place), 4-digit, and 3-digit.[19]

At the 5-digit level, we find that 21% of control group individuals find a job in their primary search occupation. In the treatment group, this number is only 15%; a difference of 6 percentage points (p-value for equal shares = 0.02). At this fine-granular level, we are likely to also capture switches to occupations that are similar (i.e., the same at the 4-digit level). Occupational changes at a higher level of aggregation are likely to reflect more substantial changes of occupations. At the 4-digit and 3-digit level, we find that difference between treatment an control groups increases to 11 and 12 percentage points (both strongly statistically significant). At the 4-digit level, the treated group is 6 percentage points more likely to find a job in a recommended occupation, though this difference is only weakly significant (p-value = 0.06). Also at the 3-digit level this difference persists, although not statistically significant (p-value = 0.16). Note that classification errors would lead us to underestimate the shares in the figure for primary occupations (in blue) and recommendations (in red). Since the classification was performed blindly with respect to the treatment status, there is, however, no reason to believe that this affects the difference between treatment and control group.

[19] The survey asked for a free text job title, which were blindly coded into a 5-digit occupational code. It is possible that a 4-digit or 3-digit occupation contains both the 5-digit primary occupation and a 5-digit recommended occupation. In these cases, we consider whether for that individual, the new job was in the primary occupation or a recommendation at the more granular (more digits) level and use that to classify the job at the less granular (fewer digits) level.

Figure 6: Comparison of occupations of new jobs with the primary search occupations and the set of recommendations (at different occupational digit levels)



Note: Sample contains Non-covid occupations and individuals that worked zero hours in March-2021. 'Primary search occ' reports the share of individuals who found work in the same occupation as their initial 'primary occupation of search' (registered at time of registration for UI benefits). 'Recommendation' reports the share of individuals who found work in one of the occupations recommended to them, *excluding* the primary search occupation (especially at higher occupational coding levels these two often overlap). The p-values for equal shares of Primary search occupations are 0.02 (5 digit), 0.001 (4 digit) and 0.0005 (3 digit). The p-values for equal shares of Recommendation Occupations are 0.28 (5 digit), 0.06 (4 digit) and 0.16 (3 digit).

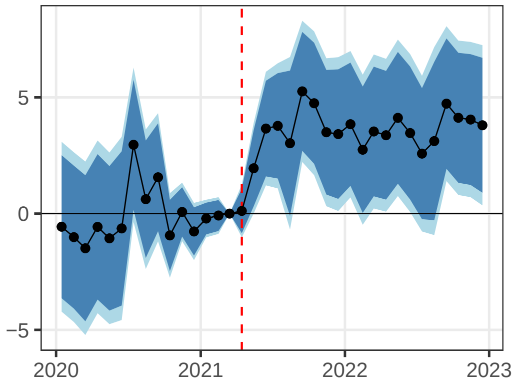
As shown in Table 4, the response rate to the outflow survey was slightly higher in the treatment group (13.1/42.0 = 31.2%) than in the control group (11.7/40.4 = 29.0%). However, it is straightforward to show that potential selectivity in the response rate cannot explain the occupational differences.[20]

In summary, we find evidence that employment (hours worked and earnings) increased in the treatment group, while UI benefit receipt did not change. In addition, new jobs were found in more diverse occupations in the treatment group.

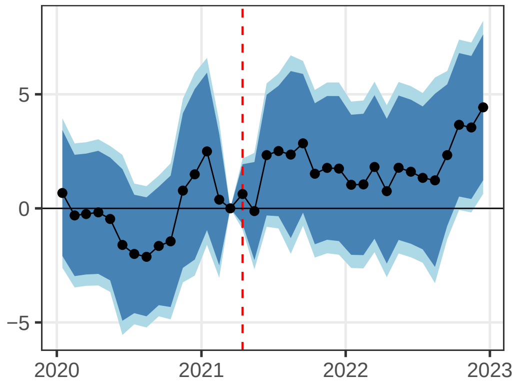
[20] If the response rate in the treatment group had been equal to that of the control group, the number of respondents would have been reduced by 67. In the extreme case, all of these additional respondents had reported an occupation different from their primary search occupation. Under that assumption, the true statistic for the share of new jobs equal to the primary search occupation in the treatment group (blue area in Figure 6, 5-digit panel) would have been 15.7% instead of the observed value of 14.6% (note that the figure reports 15% due to rounding). Indeed, even in this extreme scenario the number is substantially smaller than in the control group (21%).

Figure 7: Heterogeneous treatment impact by unemployment duration

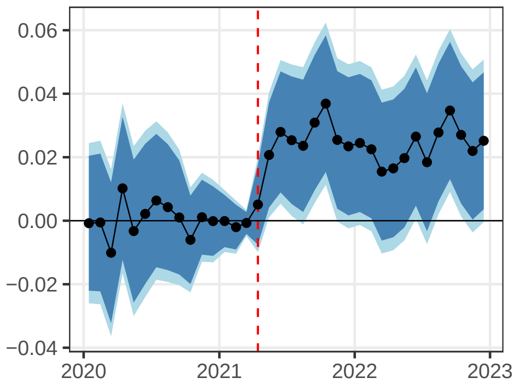
(a) Hours worked: long duration



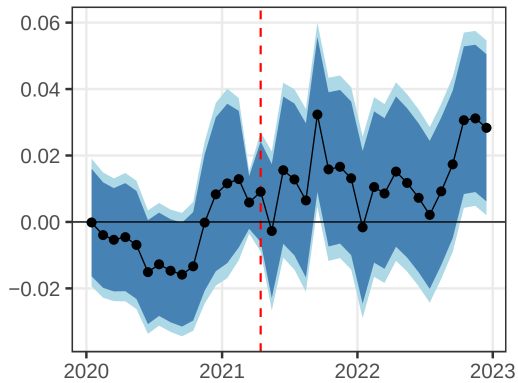
(b) Hours worked: short duration



(c) Employed (earnings > 0): long duration



(d) Employed (earnings > 0): short duration



Note: Individuals in Non-covid occupations without employment in March-2021. Long unemployment duration is defined as longer or equal than 24 weeks at the time of sample selection. Short unemployment duration means shorter than 24 weeks. The dashed red line indicates the first intervention message.

6.2.3 Heterogeneity by unemployment duration

Our large sample size allows us to investigate which groups of individuals were particularly receptive for the intervention. We pre-registered two dimensions of heterogeneity to avoid data mining. These dimensions are based on previous studies that found stronger impacts among (1) long-term unemployed and (2) job seekers who search ‘narrowly’ (Belot et al., 2019, 2022). Unfortunately, our search breadth measures are not sufficiently detailed to classify job seekers, and we focus solely on unemployment duration. We split the sample at the median UI duration in our sample (measured at the time of sample selection), which is

24 weeks. Results are presented in Figure 7, where we focus on hours worked and employment for brevity. We find clear heterogeneity. The impact on hours worked is immediate, large, and statistically significant for long-term unemployed in Panel (a). For the short-term unemployed, the immediate impact is also positive but much smaller and statistically insignificant, although it ticks up at the end of the observational period in Panel (b). For employment (measured as positive labor earnings) we find a similar difference: large and significant impacts for long-term unemployed (Panel c) and positive but initially small and insignificant estimates for the short-term unemployed (Panel d).

These compelling results raise the question whether it is the longer unemployment duration per se that drives the responsiveness to information about alternative occupations, or whether dynamic selection alters the composition of the sample relative to short-term unemployed. In Table A12 in the Appendix we compare the characteristics of the two groups. We find that long-term unemployed are significantly older and slightly lower educated, two differences that one might have associated with lower willingness to explore a career switch. Other differences exist, but are fairly minor (e.g., gender and occupational shares). While only suggestive, these results support the idea that it is indeed the extended duration that causes treatment impacts to be larger, most likely because it boosts the willingness and incentives to explore alternative occupations. Indeed, if we compare responses to the pre-intervention survey among short- and long-term unemployed, we find that beliefs about job findings are significantly more pessimistic among the long-term unemployed (see Table A13 in the Appendix).

6.2.4 Treatment effects on job seekers in 'Covid' occupations and with paid employment at the time of intervention

While we consider the findings reported in Section 6.2.1 to be our main results, it is worthwhile to investigate how the treatment affected job seekers looking for work in the 'Covid occupations' (i.e., occupations that quickly recovered after the Covid-19 pandemic) and registered job seekers with a paid job at the time of the intervention. Table 5 shows the treatment effect on a range of outcomes measured 18 months after the intervention. For comparison, the first row shows the impact of the treatment on our main sample, reproducing the positive and significant impact on hours worked and earnings. The second row considers individuals in Covid-occupations, where we find smaller and insignificant effects. The point estimates for the cumulative impacts on hours and earnings are about half of those in Non-

Table 5: Treatment effects on other samples: Covid occupations and job seekers with partial employment

	Dependent variable:						
	Monthly hours worked	Monthly labour earnings	Monthly UI benefits received	Labour earnings above 0	Cumulative hours	Cumulative earnings	Cumulative UI benefits
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Zero employment in March-2021, non-covid occupations (baseline sample)							
Treatment	3.96***	71.97***	−2.10	0.03***	50.56***	876.21**	−33.22
	(1.27)	(24.02)	(11.59)	(0.01)	(18.17)	(347.86)	(216.11)
Observations	10,738	10,738	10,738	10,738	10,738	10,738	10,738
Covid-occupations (non-employed in March-2021)							
Treatment	1.00	14.26	0.25	−0.003	24.59	349.98	−28.66
	(1.29)	(20.39)	(8.48)	(0.01)	(18.83)	(283.08)	(166.76)
Observations	9,894	9,894	9,894	9,894	9,894	9,894	9,894
Positive employment in March-2021, non-covid occupations							
Treatment	−0.24	9.24	11.21	−0.003	−0.14	−221.34	353.64
	(1.63)	(32.67)	(11.67)	(0.01)	(24.48)	(514.42)	(220.74)
Observations	4,563	4,563	4,563	4,563	4,563	4,563	4,563

Each cell contains a treatment effect estimate from a regression that controls for demographics. All outcomes are measured 18 months after the intervention (October 2022). Robust standard errors in parentheses.

covid occupations. The lack of impact for this group can be explained in one of two ways. First, job seekers in these occupations may have benefited less from our advice, as their occupations recovered in subsequent months. That is, the impact of heeding our advice is smaller. A second explanation is that individuals in these groups were less likely to take our advice, as they saw demand in their occupations return to their pre-Covid levels. Figure B7 in the Appendix shows that the latter explanation is more likely. While Figure 6 shows that treated individuals in Non-covid occupations find jobs in different occupations than those in the control group, Figure B7 shows that this is not the case for the Covid sample. Note that despite Covid occupations reverting to their baseline levels, job prospects in the suggested occupations were still significantly better, even after Covid restrictions were lifted in the Netherlands. Individuals therefore may still have benefited from switching occupations.

The last row of results shows the impact for those who already held a paid job while receiving unemployment insurance at the time of our intervention. Note that this group exhibited a pre-treatment difference, so results should be interpreted with care. We find no effects and estimates that are comparatively much closer to zero. We conclude that job seekers who were already employed were much less likely to integrate the advice into their job search.

6.2.5 Secondary outcomes: effects on beliefs and job search

The positive impacts on employment and the corresponding increase in occupational mobility suggest that job seekers have adopted our advice in their job search strategies. As secondary outcomes of interest we discuss treatment impacts on job search and labor market beliefs. Our measures stem from a number of different sources which we discuss one by one.

Job search activities registered at the Public Employment Service First, we examine job search activities that are registered through the official Public Employment Service platform. Job seekers face a requirement to complete at least four activities every four weeks.[21] We observe all activities registered between March 2021 and April 2022. The advantage of this measure is that it encompasses search through all possible channels (different online job boards, direct applications, applications through social networks). In addition, the search requirement (with potential sanctions in case of failure to meet the minimum) improves the reporting rate. The weakness is, however, that job seekers may face limited incentives to report anything beyond their required number of activities. In addition, the data contain only the activity and date, without any information about the job that was considered. We re-estimate equation 1, using the number of activities registered per month as the outcome.[22] We present the estimates in Panel (a) of Figure B8 in the Appendix. We find some indication that the number of applications increased after our intervention: the difference between control and treatment group is close to zero before the first message was sent, and turns positive (and statistically significant in most months until early 2022) after our intervention. The increase is about 0.1 applications, relative to a control mean of 1.8 in March 2021; a 5.5% increase, similar to our main outcomes of interest.

Online job search Our second source of information about job search are online search activities on the Public Employment Service job search platform.[23] We observe all job search activities (vacancies viewed, saved and applied to) for individuals in our sample that use the platform while being logged in, between

[21] These activities can consist of job applications or other search activities such as network meetings and open inquiries with employers. The activities are registered online by the job seekers or by the case worker if a meeting took place.

[22] One complication is dynamic selection: once job seekers find a job, they stop registering job search activities. To deal with this, we remove individuals from the sample once they work at least 40 hours within one month.

[23] This platform is one of the largest in the Netherlands and is generally recommended to UI beneficiaries for their job search.

March-June 2021. The advantage of these data is that they contain the occupation title of the vacancies. The disadvantage is that it is not mandatory for job seekers to use the platform, and as a result only a small (and selective) share does. In an attempt to obtain insights from the data, we first restrict our analysis to all individuals who used the platform at least once before our intervention. Second, we only consider vacancy views, as observation numbers for saved and applied vacancies are very low. Again we re-estimate equation 1, using the number of viewed vacancies per month as the outcome. The results in Panel (b) of Figure B8 in the Appendix show that there is no indication of a treatment effect on the number of vacancies viewed, but the estimates are imprecise.

Survey responses about job search and beliefs Through the pre- and post-intervention surveys we collected information about job search and labor market beliefs. To control for baseline differences, we estimate a difference-in-differences model.[24] Importantly, this implies that the sample is limited to the small subset of participants who completed both of these surveys. Using the survey data, we consider measurements of job search activities in Table A14 in the Appendix (time spent on searching, applications, interviews, type of occupations included in the search set). We find that the treatment effect is never statistically significantly different from zero. Thus we cannot reject that the treatment has no observable impact on job search activities as measured along these six dimensions. In Table A15 in the Appendix we consider measurements of labor market beliefs (expectations about job offer probabilities, job stability and job finding). We do not find significant treatment impacts here either.

There are a number of possible explanations for the lack of impact that we find on search behavior and beliefs despite clear evidence of changes in job finding. First, sample size becomes fairly small at this stage, with only around 200-800 observations for some outcomes (implying 100-400 individuals per treatment/-control). Starting from an experimental sample of 10,738, this limits statistical precision. Indeed, wide confidence intervals cannot reject substantial positive (or negative) impacts. Second, the small sample size also hints at the possibility of selective response: while those invited to answer the survey were randomly drawn, those who completed both the pre- and post-survey are certainly not rep-

[24]The baseline specification is

$$Y_{it} = \beta_0 + \beta_1 T_i + \beta_2 P_{it} + \beta_3 P_{it} T_i + \varepsilon_{it}, \tag{2}$$

with T_i a treatment indicator and P_i a time period indicator (equal to 1 for the post-intervention period, and 0 otherwise).

resentative of the full sample. Third, search activities and beliefs may be difficult concepts to measure in a survey, resulting in measurement error (in both the pre- and post-survey) and attenuation bias in our estimates.

We conclude that there is some evidence that the intervention has boosted job search, while data limitations prevent a more detailed investigation of how job search was exactly affected.

7 Search Externalities and Violations of SUTVA

In our experiment, we only observe participants whose primary job search activity is in occupations with low vacancy-to-job seeker ratios. Moreover, when we report the difference in outcomes between the treatment and control group, we implicitly interpret the results as if the treatment has no effect on the control group.[25] However, we know from standard job search theory that changes in one individual's job search behavior affects others. That is, when we treat an additional participant, it might affect job prospects for the control group, other treated individuals, and job seekers outside of our subject pool. To address this, we take the observed change in behavior of the treatment group relative to the control group as given and use a calibrated job search model to assess the degree of SUTVA violations and the efficiency of the intervention.[26] We limit the discussion here to the main features of the model and calibration, with finer details relegated to Appendix D.

Theoretical Framework There are different occupations $G \in \mathbb{G}_F$ and different types of job seekers $g \in \mathbb{G}_W$. Job seekers in g have search intensity s_{gG} in occupation G. The number of meetings in "market" G in time period t, is determined according to a constant returns matching function $M(V_{Gt}, U_{Gt})$ that depends on current vacancies in the occupation ($V_{G,t}$) and the search-effort-weighted number of unemployed ($U_{Gt} = \sum_g s_{gG} u_{g,t}$). Given market tightness $\theta_{Gt} = V_{Gt}/U_{Gt}$, $m(\theta_{Gt}) = M(\theta_{Gt}, 1)$ denotes the meeting probability per unit of search effort and $q(\theta_{Gt}) = M(\theta_{Gt}, 1)\theta_{Gt}$ the meeting probability for a given vacancy.

Meetings turn into matches with probability A_G, which constitutes the market-specific matching efficiency. Searches outside of an individual's primary occupation

[25]This assumption is often referred to as the Stable Unit Treatment Value Assumption (SUTVA) in causal inference.

[26]Kircher (2022) proposes such a procedure to assess likely externalities, but his proposal is purely theoretical and does not attempt to calibrate the size of the externalities. Altmann et al. (2022) outline a similar model to analyze externalities, but again they do not quantify such a model.

additionally fail with probability $(1-P)$, capturing the lower qualifications for such jobs. So, conditional on meeting, there is a probability B_{gG} that a meeting turns into a match, where $B_{gG} = PA_G$ represents occupational change.[27]

For given levels of vacancies ($V_{G,t}$), unemployment (u_{gt}) and employment (e_{gGt}) by group and occupation, employment and vacancies evolve according to

$$e_{gG,t+1} = B_{gG}s_{gG}m(\theta_{G,t})\,u_{g,t} + (1-\delta)e_{gG,t}. \quad (3)$$

$$V_{G,t+1} = V_{G,t}\left[1 - C_{G,t}q(\theta_{G,t})\right][1-\delta] + \mu_G, . \quad (4)$$

Employed workers remain employed at rate $(1-\delta)$ and unemployed workers find jobs at rate $B_{gG}s_{gG}m(\theta_{Gt})$. Vacancies next period constitute new vacancies μ_G and those existing vacancies that do not get matched (first square bracket) and do not get dissolved (second square bracket).[28] Finally, unemployment equals the labor force of each group ($\bar{e}_g$) minus those in employment: $u_{g,t+1} = \bar{e}_g - \sum_G e_{gG,t+1} + \triangle_{gt}$. The term $\triangle_{gt}$ is zero except at the time T of treatment when $\triangle_{g=0,T} = -\triangle_{g=1,T} > 0$ captures the conversion of g=1 (untreated) individuals into g=0 (treated) individuals, which changes their subsequent job search.

Calibration Overview (details in Appendix D) We calibrate the economy as if SUTVA holds, and then use our simulation to detect violations. We use a constant returns to scale Cobb-Douglas matching function with a two-third elasticity with respect to unemployment.[29] We use three occupations: $G = 1$ for selection occupations, $G = 2$ for recommended occupations, and $G = 3$ for "other" occupations. In the initial steady state, $\mathbf{g = 1}$ *represents job seekers with primary occupation in* $G = 1$, and $\mathbf{g = 2}$ *"other" job seekers with primary occupation not in* $G = 1$. Search intensity equals the number of occupations a job seeker

[27]If ρ_{gG} is the fraction of group g individuals with primary search occupation in G, then for search effort $s_{gG} = \rho_{gG}$ there is no occupation switch penalty, so $B_{gG} = A_{gG}$. If they search more than that, they no longer search for their primary occupation, and each additional unit of search intensity incurs the penalty. See the Appendix D for details.

[28]Even if a vacancy meets a worker, the chance $C_{G,t} := \sum_g (B_{gG}s_{gG}u_{g,t}) / \sum_g (s_{gG}u_{g,t})$ that this leads to a match is less than one. We model vacancies as long-lived and entry as mechanic to avoid trivial block-recursivity arising from free entry of vacancies, which (absent a meeting penalty) would trivially ensure SUTVA. See more discussion in Appendix D.

[29]Petrongolo and Pissarides (2001) survey the literature and state "when the dependent variable is the total outflow from unemployment, the estimated elasticity on unemployment is about 0.7" (p.2). Table 1 in Broersma and Van Ours (1999) reviews a number of existing studies, and those based on unemployment to employment flows all have elasticities between 0.6 and 0.8. Those from the Netherlands all have 0.7. Studies that look at all hires, or at flows from either non-employment to employment or employment to employment, find other and often much smaller elasticities. Here we are focused on transitions from unemployment to employment and therefore choose two-thirds, but explore robustness below.

lists. Public records then directly provide market tightnesses ($\theta_1 = 3.6\%, \theta_2 = 52.1\%, \theta_3 = 27.5\%$). These are an order of magnitude more dispersed than the matching efficiencies ($A_1 = 2.8\%, A_2 = 6.0\%, A_3 = 6.5\%$) calibrated to the average job finding for our control group and for "others", and the shares of control group jobs per occupation. The occupation switch penalty $1 - P$ is calibrated to 37%.

Keeping tightness fixed, similar targets for the *treatment group (*$\mathbf{g} = \mathbf{0}$*)* determine its search intensities: compared to the control group ($g = 1$) these are 40% higher in $G = 2$, 19% higher in $G = 3$, and 19% lower in $G = 1$.[30] Average search-weighted tightness is 22% higher in treatment relative to control group.

Findings The second row in Table 6 simulates our intervention where 66% of the unemployed in group $g = 1$ in a single period get treated (and therefore converted into group $g = 0$). The treated change their search and therefore the market tightness. We report outcomes relative to a simulation where we keep market tightness fixed at pre-treatment levels so that SUTVA holds by assumption. Employment after 18 months improves by roughly 0.2pp both for treatment and control group individuals, as they benefit from improved tightness in the selection occupations $G = 1$, but this magnitude of SUTVA violations is minimal relative to employment of roughly 50%. The control group benefits slightly more, so the difference between them is understated by 0.06pp relative to the scenario where SUTVA holds. This magnitude is again minimal relative to the roughly 2.5pp difference between these groups. "Other" individuals in group $g = 2$ get negatively affected, albeit to a much lesser degree as jobs for them are more plentiful. The group of "other" individuals is larger, though, and their losses dominate. Relative to the additional jobs that the treatment creates after 18 months if tightness were fixed, the actual increase in jobs is 67%.

Rows 1 and 3 in Table 6 show the situation where 1% or 99% of a cohort of $g = 1$ individual's get treated. Changes in market tightness start to affect individuals more when treatment is rolled out more broadly, which affects also the difference between treatment and control, but the effect sizes stay minor relative to the calibration target of 2.5% employment difference between treatment and control. Efficiency is hardly affected by broader roll-out. This is the case even if we treat not just one cohort, but start treating all newly unemployed individuals in group g=1 over the 18 months of the study, as reported in the last row.[31]

[30] Backed out search intensities are $s_{01} = 0.97, s = 01 = 0.35, s_{03} = 0.83$. In sum this is 93% of the search observed for the control group: $s_{01} = 1.3, s = 01 = 0.27, s_{03} = 0.74$.

[31] In the last row of Table 6, columns 4-6 report effects on the initial cohort that is treated.

Table 6: Treatment effects in calibrated economy relative to a simulation where market tightness is held fixed (where SUTVA holds).

% of group $g = 1$ treated	Once or repeated	Change in employment after 18 months relative to fixed θ_G (in pp)			Change in difference treatment vs control (in pp)	Efficiency
		control	treatment	other		
1%	once	0.003	0.002	-0.000	-0.001	68%
66%	once	0.22	0.16	-0.03	-0.06	67%
99%	once	0.33	0.24	-0.04	-0.09	66%
99%	repeated	0.44	0.32	-0.05	-0.12	64%

All values in columns 3-6 are percentage points (pp). For comparison: employment after 18 months exceeds 50pp in all groups; the difference between treatment and control is calibrated to 2.5pp with fixed tightness.

The efficiency gains from redirecting search effort from tight to slack markets remains large even under full treatment. Group $g = 1$ only constitutes 7% of all unemployed, so treating all does not congest the whole market because the intervention is targeted to those searching in the worst conditions, i.e., to those who primarily search in occupations with the poorest prospects.

We also consider counterfactuals where a larger number of individuals get treated. If the pre-treatment size of group g=1 covered half of all unemployment (and vacancies are adjusted so that tightness θ_G remain unchanged, all else remains equal) efficiency of our (66% once) treatment would be 10pp higher: the positive externalities for job seekers in groups g=1 and g=0 now get more weight. If instead we also treat two-thirds of group g=2 and changed their search effort by the same percentages efficiency falls by 11pp, but still remains high at 56%.[32] This differs from Altmann et al. (2022) who observe a large reduction in efficiency when they vary treatment intensity. The major difference between our intervention and theirs, however, is that our intervention is directly aimed at shifting search effort out of extremely low-tightness towards high-tightness occupations. Theirs is not. They measure an improvement in tightness by 8% in their treatment, much less than our 22% increase mentioned above which is due to selecting individuals who search predominantly in low-tightness occupations (G=1) and recommending high-tightness occupations (G=3).

Robustness The review in Petrongolo and Pissarides (2001) explicitly highlights estimates of around 0.7 for the elasticity of matching functions aimed to

[32] This treatment means that some g=2 individuals are converted to a group $g = 3$ with $s_{3G}/s_{2G} = s_0G/s_1G$. This change in search effort shifts intensity mostly from G=2 to G=3 (and not from G=1 to G=3 as for group g=1). There is not much further change in efficiency when everyone gets repeatedly treated.

capture unemployment to employment flows.[33] Estimates that also incorporate other flows are wider, and for this broader environment they state that the "plausible range for the empirical elasticity on unemployment is 0.5 to 0.7" (p.2). We explore robustness to our specification of two-thirds by recalibrating the economy with levels of 0.5, 0.6 and 0.7. Results are very similar to our two-third benchmark, except that efficiency varies between 44%, 58% and 70% (for treatment once of 66% of group g=1). Like in the benchmark, full roll-out depresses efficiency by roughly 3pp. Overall, efficiency always remain strongly positive, and full roll-out remains justified. While we might have expected positive efficiency from sending individuals from low-tightness to high-tightness markets, we note that this is by no means obvious: an even higher penalty from occupational switching would have negated this. In the data we do see a lot of control group individuals finding jobs in occupations $G = 2$ and $G = 3$ (see Figure 6), which limits the possible penalty and leads to the positive findings.[34]

8 Conclusion

For some occupations there are few vacancies and many job seekers while for others there are many vacancies and few job seekers. This has been discussed as sources of heightened unemployment (e.g., Şahin et al., 2014). Whether and how one could balance job search more evenly across occupations has received less attention. So what does it take to stimulate job seekers to broaden their horizon and look for jobs in occupations that are in higher demand? To answer this question we provide unemployed job seekers in occupations with poor labor market prospects with personalized information about a manageable number of suitable alternative occupations that offer better prospects.

A first key insight is that job seekers in occupations with low vacancy-to-job

[33]See Footnote 29 for justification of a value of 0.7 for the matching function elasticity.

[34]If penalty $(1 - P)$ is close to zero, we could only observe our control group finding many jobs in $G \in \{2, 3\}$ if search efficiency A_G there was extremely high. But then "other" individuals who have no penalties should have a much higher job finding probability than we observe. Similarly, we explored a zero penalty when treatment or control switch into occupation $G = 2$ because we designed these recommendations based on particularly easy switching: in the recalibrated economy this turns out to have only minimal effect on the magnitude of SUTVA violations and efficiency, because the probability of occupational mobility per unit of search intensity is an observed calibration target and disciplines our exercise tightly. (For this, we set $B_{01} = B_{11} = A_1$ and $B_2 2 = A_2$, but retain penalties for all other occupational switches such as $B_{02} = B_{12} = PA_3$). After recalibrating our main treatment (once 66% treatment), efficiency remains at 66% and changes in employment after 18 month relative to fixed tightness are 0.24pp (control), 0.17 (treatment) and -0.03 (other). Without recalibrating efficiency changes by more than 10pp, but the calibration targets discipline this.

seeker ratios are not averse to considering alternatives. On the contrary, they are open to exploring other occupations and confident in their ability to secure employment in these occupations. However, they appear unaware of why such a shift might be necessary or beneficial. This is likely caused by their limited knowledge of how unfavorable the prospects are in their primary search occupation. Moreover, they are hardly aware of the (somewhat) better prospects in the alternatives they do consider and they do not consider suitable alternatives with truly much better prospects.

Our intervention provides the necessary information to improve their knowledge about the labor market and has a positive effect on employment outcomes (employment, hours worked, and earnings). The intervention delivered digestible and personalized information about the poor prospects in the occupations job seekers were currently targeting, as well as the good prospects in concrete and attainable alternative occupations. It was presented in a user-friendly format with practical guidance on how to transition into these alternatives. We show that treated job seekers more often find employment in the occupations we suggest to them, as well as other occupations outside of their initial occupation of primary interest. This evidence suggests that the information that we shared has landed, and that unemployed job seekers in occupations with poor prospects indeed broaden their search for a more successful outcome.

Our findings are based on a sample of fully unemployed job seekers in 'Non-covid occupations' (i.e., occupations with a structural low demand for labor). For job seekers in 'Covid occupations' (i.e., occupations for which the demand was low at the time of our experiment due to Covid-related restrictions but rebounded shortly after), and those partially unemployed, we find smaller and insignificant effects. This is likely due to the fact that the information we designed is less relevant to these groups. Finally, our calibration of a job search model shows that the estimated effect size remains large even after accounting for spillover effects and that a full-scale roll-out is warranted.

A key advantage of our intervention from a policy perspective is that it is ver easy to implement. In many countries information on recent trajectories of job seekers is available, which can be used to generate suitable occupational suggestions and our results show that providing this information directly to job seekers is sufficient. While integrating it into a job search platform may further reduce frictions, it is not required. In short, the intervention is a promising tool for Public Employment Services in helping job seekers in slack labor markets.

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Appendix (for online publication)

Appendix A: Additional Tables

Table A1: Selection occupations with low job prospects

Occupation	Occupation (Dutch name)
Activity counsellor	Activiteitenbegeleider
Animal caretaker	Dierenverzorger
Archivist	Archiefmedewerker
Bartender/waiter	Medewerker bediening/bar
Canteen/Buffet employee	Medewerker bedrijfsrestaurant of buffet
Event/conference organizer	Organisator van conferenties en/of evenementen
Graphic designer	Grafisch vormgever
Hairdresser	Kapper
Hotel receptionist	Hotelreceptionist
Janitor/Concierge	Conciërge/huismeester
Office support staff	Ondersteunend medewerker op een kantoor/secretariaat
Primary school teaching assistant	Onderwijsassistent basisonderwijs
Printer	Drukkerijmedewerker
Producer (television/film)	Productieleider/producent
Receptionist	Receptionist/telefonist
Shop attendant household/leisure goods	Verkoopmedewerker huishoudelijke en vrijetijdsartikelen
Social worker	Sociaal werker
Steward/stewardess	Steward/stewardess
Taxi driver	Taxi- of particulier chauffeur
Travel agent	Reisadviseur/reisbureaumedewerker
Video and sound technician	Beeld- en geluidtechnicus

Table A2: Sample comparison for occupation of interest

	Dependent variable:				
	Age	Medium Education	Higher Education	Search Radius	Share Male
	(1)	(2)	(3)	(4)	(5)
Other	2.453***	−0.037*	0.049	8.551***	0.205***
	(0.532)	(0.020)	(0.030)	(1.753)	(0.029)
Transition	2.999***	0.020	−0.046	0.216	0.200***
	(0.634)	(0.023)	(0.036)	(2.088)	(0.035)
Constant	44.716***	0.335***	0.376***	36.245***	0.416***
	(0.513)	(0.019)	(0.029)	(1.691)	(0.028)
Observations	1,882	1,882	1,882	1,882	1,882

*p<0.1; **p<0.05; ***p<0.01

Table A3: Sample comparison for experience occupation

	Dependent variable:					
	Age	Medium Education	Higher Education	Search Radius	Share Male	Years of Experience
	(1)	(2)	(3)	(4)	(5)	(6)
Other	1.925***	−0.025	0.023	7.320***	0.188***	0.408*
	(0.433)	(0.018)	(0.028)	(1.349)	(0.026)	(0.214)
Transition	1.755***	0.032	−0.069**	0.571	0.167***	0.157
	(0.515)	(0.021)	(0.033)	(1.605)	(0.031)	(0.255)
Constant	46.537***	0.339***	0.414***	37.376***	0.437***	5.075***
	(0.419)	(0.017)	(0.027)	(1.306)	(0.025)	(0.208)
Observations	2,413	2,413	2,413	2,413	2,413	2,413

*p<0.1; **p<0.05; ***p<0.01

Table A4: Distribution of selection occupation within various samples

	Overall sample	0 hours worked in Mar-2021		
		Overall	Non-Covid	Covid
Activity counsellor	3.5%	3.3%	6.3%	0%
Animal caretaker	1.4%	1.4%	2.8%	0%
Archivist	1.0%	1.2%	2.3%	0%
Bartender/waiter	16%	16%	0%	34%
Canteen/Buffet employee	6.6%	6.5%	0%	14%
Event/conference organizer	2.5%	2.6%	5.0%	0%
Graphic designer	2.7%	3.0%	5.8%	0%
Hairdresser	1.9%	1.9%	0%	3.9%
Hotel receptionist	1.7%	1.6%	0%	3.4%
Janitor/Concierge	2.9%	2.9%	5.6%	0%
Office support staff	21%	22%	42%	0%
Primary school teaching assistant	2.2%	2.1%	4.1%	0%
Printer	1.1%	1.2%	2.3%	0%
Producer (television/film)	0.9%	1.0%	1.9%	0%
Receptionist	17%	17%	0%	35%
Shop attendant household/leisure	1.4%	1.3%	2.4%	0%
Social worker	8.0%	8.3%	16%	0%
Steward/stewardess	1.0%	1.0%	1.9%	0%
Taxi driver	4.6%	3.1%	0%	6.5%
Travel agent	1.6%	1.8%	0%	3.7%
Video and sound technician	1.0%	1.1%	2.1%	0%
Observations	30,129	20,632	10,738	9,894

P-values refer to tests for equality of the control and treatment columns.

Table A5: Balance table main analysis sample (zero hours worked in March 2021 and Non-covid occupations only): administrative records

	Overall	Control	Treatment	P-value
Demographics:				
Male	28%	28%	28%	0.66
Age	48 (12)	48 (12)	48 (12)	0.81
Unemployment duration (wks.)	32 (27)	32 (27)	32 (28)	0.98
Remaining benefits (wks.)	52 (29)	52 (29)	52 (29)	0.78
Lower education	17%	18%	16%	0.050
Medium education	52%	51%	53%	0.24
Higher education	31%	31%	31%	0.74
Experiment:				
Pre-survey completed	9.7%	9.7%	9.7%	0.88
Selection occupation:				
Activity counsellor	6.3%	6.5%	6.2%	0.56
Animal caretaker	2.8%	2.8%	2.8%	0.89
Archivist	2.3%	2.2%	2.3%	0.81
Event/conference organizer	5.0%	5.3%	4.9%	0.34
Graphic designer	5.8%	6.0%	5.7%	0.50
Janitor/Concierge	5.6%	5.7%	5.6%	0.85
Office support staff	42%	41%	42%	0.82
Primary school teaching assistant	4.1%	3.8%	4.2%	0.39
Printer	2.3%	2.4%	2.3%	0.94
Producer (television/film)	1.9%	1.8%	1.9%	0.69
Shop attendant household/leisure	2.4%	2.5%	2.4%	0.84
Social worker	16%	16%	16%	0.98
Steward/stewardess	1.9%	1.6%	2.1%	0.089
Video and sound technician	2.1%	2.0%	2.1%	0.85
Observations	10,738	3,611	7,127	

P-values refer to tests for equality of the control and treatment columns. Remaining benefits and unemployment duration are measured in March 2021.

Table A6: Balance table overall sample: administrative records

	Overall	Control	Treatment	P-value
Demographics:				
Male	25%	25%	25%	0.99
Age	47 (13)	47 (13)	47 (13)	0.47
Unemployment duration (wks.)	32 (28)	32 (28)	32 (28)	0.68
Remaining benefits (wks.)	51 (30)	51 (30)	51 (30)	0.81
Lower education	22%	22%	21%	0.17
Medium education	56%	55%	56%	0.13
Higher education	22%	23%	22%	0.66
Experiment:				
Zero hours March-2021	68%	69%	68%	0.25
Covid selection occ.	49%	49%	49%	0.77
Selection occupation:				
Pre-survey completed	9.3%	9.3%	9.2%	0.83
Activity counsellor	3.5%	3.4%	3.5%	0.77
Animal caretaker	1.4%	1.4%	1.4%	0.96
Archivist	1.0%	1.1%	1.0%	0.71
Bartender/waiter	16%	16%	16%	0.96
Canteen/Buffet employee	6.6%	6.6%	6.6%	>0.99
Event/conference organizer	2.5%	2.5%	2.4%	0.88
Graphic designer	2.7%	2.8%	2.7%	0.72
Hairdresser	1.9%	1.8%	2.0%	0.35
Hotel receptionist	1.7%	1.7%	1.7%	>0.99
Janitor/Concierge	2.9%	2.8%	2.9%	0.74
Office support staff	21%	21%	21%	0.89
Primary school teaching assistant	2.2%	2.2%	2.3%	0.55
Printer	1.1%	1.1%	1.1%	0.96
Producer (television/film)	0.9%	0.9%	0.9%	0.86
Receptionist	17%	17%	17%	0.69
Shop attendant household/leisure	1.4%	1.4%	1.4%	0.85
Social worker	8.0%	8.1%	8.0%	0.97
Steward/stewardess	1.0%	1.0%	1.0%	0.84
Taxi driver	4.6%	4.7%	4.5%	0.45
Travel agent	1.6%	1.5%	1.6%	0.64
Video and sound technician	1.0%	1.0%	0.9%	0.80
Observations	30,129	10,004	20,125	

P-values refer to tests for equality of the control and treatment columns. Remaining benefits and unemployment duration are measured in March 2021.

Table A7: Comparison of composition of survey-respondents

	Overall	Survey completed	Survey not completed	P-value
Demographics:				
Male	28%	29%	27%	0.44
Age	48 (12)	53 (11)	46 (12)	<0.001
Unemployment duration (wks.)	33 (28)	32 (29)	33 (28)	0.55
Remaining benefits (wks.)	51 (29)	62 (29)	47 (29)	<0.001
Lower education	17%	18%	16%	0.13
Medium education	52%	53%	52%	0.88
Higher education	31%	29%	32%	0.15
Experiment:				
Treatment	67%	66%	67%	0.67
Selection occupation:				
Activity counsellor	6.6%	7.2%	6.3%	0.34
Animal caretaker	2.7%	1.9%	3.0%	0.046
Archivist	2.3%	1.7%	2.5%	0.12
Event/conference organizer	4.8%	4.0%	5.2%	0.13
Graphic designer	5.8%	5.6%	5.9%	0.70
Janitor/Concierge	5.5%	7.5%	4.6%	0.002
Office support staff	42%	46%	41%	0.003
Primary school teaching assistant	4.0%	3.1%	4.3%	0.065
Printer	2.4%	2.2%	2.4%	0.67
Producer (television/film)	1.8%	1.6%	1.9%	0.62
Shop attendant household/leisure	2.4%	2.6%	2.3%	0.59
Social worker	16%	14%	17%	0.087
Steward/stewardess	1.7%	1.1%	2.0%	0.034
Video and sound technician	1.9%	1.0%	2.3%	0.002
Observations	3,493	1,040	2,453	

This table is based on the individuals that received the survey invitations and fall within our baseline sample (Non-covid occupations and individuals that worked zero hours in March-2021). P-values refer to tests for equality of the survey and non-survey columns.

Table A8: Balance table: survey responses (prior to intervention)

	Overall	Control	Treatment	P-value
Job finding score sel. occ.	3.00 (0.55)	3.03 (0.59)	2.99 (0.53)	0.22
Job finding score alternative occ.	4.29 (1.55)	4.27 (1.59)	4.30 (1.53)	0.71
Time exploring alternatives	6.0 (6.1)	6.0 (6.2)	6.0 (6.1)	0.96
Willingness work in new occ.	3.39 (0.88)	3.38 (0.87)	3.39 (0.88)	0.82
Beliefs:				
My skills are transferable	3.82 (0.82)	3.84 (0.83)	3.81 (0.82)	0.58
Prob. job in 2 months	0.36 (0.28)	0.36 (0.30)	0.37 (0.27)	0.89
Appl. needed (primary)	48 (58)	55 (63)	44 (55)	0.028
Appl. needed (alt.)	45 (57)	52 (64)	41 (53)	0.065
Wage (expectations):				
Salary previous job	3,016 (1,187)	3,003 (1,220)	3,022 (1,170)	0.81
Hours previous job	30 (8)	30 (8)	30 (8)	>0.99
Expected wage (main occ.)	2,937 (893)	2,936 (880)	2,938 (900)	0.98
Reservation wage (main occ.)	2,823 (863)	2,808 (863)	2,830 (864)	0.70
Expected wage (alt. occ.)	2,929 (951)	2,912 (921)	2,938 (966)	0.71
Reservation wage (alt. occ.)	2,801 (873)	2,751 (848)	2,827 (885)	0.25
Applications/interviews				
Applications (main occ.)	3.2 (7.0)	3.5 (8.4)	3.1 (6.2)	0.48
Job interviews (main occ.)	0.44 (1.55)	0.37 (1.12)	0.47 (1.73)	0.29
Applications (alt. occ.)	2.45 (4.91)	2.86 (6.52)	2.24 (3.79)	0.17
Job interviews (alt. occ.)	0.36 (1.00)	0.33 (1.00)	0.37 (1.00)	0.63
Applications (other occ.)	2.4 (4.9)	2.4 (5.4)	2.4 (4.7)	0.93
Job interviews (other occ.)	0.36 (1.18)	0.36 (1.24)	0.36 (1.15)	0.97
Observations	1,040	352	688	

Only Non-covid occupations and individuals that worked zero hours in March-2021. P-values refer to tests for equality of the control and treatment columns.

Table A9: Survey responses about broader job search

Statistic	N	Mean	St. Dev.	Min	Max
Search occupations suggested	1,030	0.23	0.50	0	3
Weekly hours exploring alternatives	1,040	6.02	6.10	0.50	20.00
Willingness to consider other occupations (1-5)	1,040	3.39	0.88	1	5
Confidence in working without experience (1-5)	1,040	3.77	0.82	1	5
Believes that skills are transferable (1-5)	1,040	3.82	0.82	1	5
Probability to expand search in two months	1,040	0.54	0.28	0.00	1.00

Sample contains Non-covid occupations and individuals that worked zero hours in March-2021. 'Search occupations suggested' measures the number of occupations that a job seeker searches in that coincide with one of the suggested occupations in our treatment messages. The variables with a 1-5 scale were answered using Likert scales (1 = Strongly disagree, ..., 5 = Strongly agree).

Table A10: How do beliefs change over time? A comparison of of survey responses (control group only).

	Pre	Post	P-value
Prob. job in 2 months	0.27 (0.25)	0.36 (0.32)	0.025
Appl. needed (primary)	69 (66)	56 (67)	0.24
Appl. needed (alt.)	60 (66)	49 (61)	0.33
Reservation wage (main occ.)	2,798 (862)	2,774 (811)	0.82
Reservation wage (alt. occ.)	2,715 (843)	2,815 (999)	0.46
Appl. needed (primary) in 2 months	64 (62)		
Appl. needed (alt.) in 2 months	61 (63)		
Observations	136	136	

Sample contains Non-covid occupations and individuals that worked zero hours in March-2021. 'Pre' refers to the pre-experiment survey and 'Post' refers to the post-experiment survey. Individuals are only included if they responded to both surveys. Standard deviations in parentheses.

Table A11: Clicking of occupations

	Dependent variable:		
	Percentage of recipients that clicks		
	(1)	(2)	(3)
Rank 2	-0.06^{***}	-0.05^{***}	-0.05^{***}
	(0.01)	(0.01)	(0.01)
Rank 3	-0.06^{***}	-0.04^{***}	-0.05^{***}
	(0.01)	(0.01)	(0.01)
Rank 4	-0.07^{***}	-0.05^{***}	-0.05^{***}
	(0.01)	(0.01)	(0.01)
Rank 5	-0.07^{***}	-0.06^{***}	-0.06^{***}
	(0.01)	(0.01)	(0.01)
Rank 6	-0.06^{***}	-0.05^{***}	-0.05^{***}
	(0.01)	(0.01)	(0.01)
Rank 7	-0.07^{***}	-0.05^{***}	-0.05^{***}
	(0.01)	(0.01)	(0.01)
Rank 8	-0.05^{***}	-0.04^{***}	-0.04^{***}
	(0.01)	(0.01)	(0.01)
Jobfinding prob. (tightness)			0.01^{***}
			(0.003)
Low Automation-risk			0.03^{***}
			(0.01)
Relative nr of transitions		0.09^{***}	0.09^{***}
		(0.02)	(0.02)
Constant	0.10^{***}	0.08^{***}	0.01
	(0.01)	(0.01)	(0.02)
Observations	165	165	165
R^2	0.27	0.33	0.45

Note: This table displays OLS regression estimates at the recommendation-email level. The omitted baseline category is rank 1. *p<0.1; **p<0.05; ***p<0.01

Table A12: Comparison of characteristics short and long UI duration samples: administrative records

	UI duration < 24 wks.	UI duration >= 24 wks.	P-value
Demographics:			
Male	27%	29%	0.062
Age	44 (13)	51 (10)	<0.001
Unemployment duration (wks.)	11 (6)	52 (25)	<0.001
Remaining benefits (wks.)	62 (32)	43 (23)	<0.001
Lower education	16%	18%	0.004
Medium education	54%	51%	0.011
Higher education	31%	31%	0.66
Experiment:			
Treatment	67%	66%	0.59
Pre-survey completed	9.7%	9.6%	0.85
Selection occupation:			
Activity counsellor	6.3%	6.4%	0.72
Animal caretaker	3.5%	2.1%	<0.001
Archivist	1.8%	2.6%	0.005
Event/conference organizer	5.7%	4.4%	0.001
Graphic designer	6.8%	5.0%	<0.001
Janitor/Concierge	5.1%	6.1%	0.029
Office support staff	39%	43%	<0.001
Primary school teaching assistant	4.2%	4.0%	0.63
Printer	2.2%	2.5%	0.29
Producer (television/film)	2.4%	1.4%	<0.001
Shop attendant household/leisure	2.8%	2.1%	0.021
Social worker	15%	17%	0.016
Steward/stewardess	2.7%	1.2%	<0.001
Video and sound technician	2.1%	2.0%	0.95
Observations	5,100	5,635	

Sample contains Non-covid occupations and individuals that worked zero hours in March-2021. P-values refer to tests for equality of the control and treatment columns. Remaining benefits and unemployment duration are measured in March 2021.

Table A13: Comparison of characteristics short and long UI duration samples: survey responses

	UI duration < 24 wks.	UI duration >= 24 wks.	P-value
Job finding score sel. occ.	2.97 (0.57)	3.03 (0.53)	0.083
Job finding score alternative occ.	4.30 (1.57)	4.28 (1.54)	0.83
Time exploring alternatives	6.2 (6.2)	5.8 (6.0)	0.25
Willingness work in new occ.	3.44 (0.85)	3.35 (0.90)	0.10
Beliefs:			
My skills are transferable	3.87 (0.82)	3.77 (0.82)	0.056
Prob. job in 2 months	0.42 (0.28)	0.31 (0.27)	<0.001
Appl. needed (primary)	37 (49)	58 (64)	<0.001
Appl. needed (alt.)	35 (49)	55 (63)	<0.001
Wage (expectations):			
Salary previous job	3,015 (1,125)	3,016 (1,242)	>0.99
Hours previous job	30 (8)	29 (8)	0.043
Expected wage (main occ.)	2,944 (849)	2,931 (933)	0.81
Reservation wage (main occ.)	2,818 (825)	2,827 (898)	0.87
Expected wage (alt. occ.)	2,960 (933)	2,899 (967)	0.37
Reservation wage (alt. occ.)	2,831 (873)	2,772 (873)	0.35
Applications/interviews			
Applications (main occ.)	3.2 (6.8)	3.2 (7.2)	0.95
Job interviews (main occ.)	0.45 (1.62)	0.42 (1.47)	0.76
Applications (alt. occ.)	2.27 (4.58)	2.64 (5.21)	0.31
Job interviews (alt. occ.)	0.33 (0.89)	0.39 (1.10)	0.40
Applications (other occ.)	2.3 (5.1)	2.5 (4.8)	0.60
Job interviews (other occ.)	0.42 (1.18)	0.31 (1.18)	0.24
Observations	497	543	

Sample contains Non-covid occupations and individuals that worked zero hours in March-2021. P-values refer to tests for equality of the control and treatment columns. Remaining benefits and unemployment duration are measured in March 2021.

Table A14: Difference-in-differences analysis survey outcomes: Job search activities

	Dependent variable:					
	Time exploring	Applications	Interviews	Number of search occupations	Mean jobfinding score	Suggestions used in search set
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.006	0.382	0.582	−0.004	−0.029	0.048
	(0.595)	(1.672)	(0.438)	(0.147)	(0.110)	(0.052)
Post	−0.956	1.024	0.067	−0.257	0.207*	0.053
	(0.682)	(1.927)	(0.510)	(0.168)	(0.126)	(0.060)
Treatment*Post	−0.354	−1.783	−0.088	−0.073	−0.164	−0.058
	(0.841)	(2.364)	(0.619)	(0.208)	(0.156)	(0.074)
Constant	5.801***	6.738***	0.533	2.662***	3.806***	0.183***
	(0.482)	(1.362)	(0.361)	(0.119)	(0.089)	(0.042)
Mean Dep. Var.	5.21	6.91	0.93	2.51	3.84	0.22
Observations	792	250	280	792	746	746

*p<0.1; **p<0.05; ***p<0.01. Individuals in Non-covid occupations without employment in March-2021. The dependent variables are: weekly time spent on exploring alternative occupations (Column 1), total number of weekly applications (Column 2), total number of weekly interviews (Column 3), number of occupations included in the search, (Column 4), the mean job finding score of the set of search occupations (Column 5) and the number of suggestions from the message that are included in the set of search occupations (Column 6).

Table A15: Difference-in-differences analysis survey outcomes: labor market beliefs

	Dependent variable:				
	Job offer rate per application primary	Job offer rate per application alternative	Expected stability primary	Expected stability alternative	Job finding probability
	(1)	(2)	(3)	(4)	(5)
Treatment	−0.008	0.030	−0.012	0.040	0.068*
	(0.059)	(0.089)	(0.034)	(0.039)	(0.038)
Post	0.122*	0.085	−0.010	−0.004	0.092**
	(0.070)	(0.108)	(0.039)	(0.045)	(0.043)
Treatment*Post	−0.048	−0.086	0.003	0.020	−0.017
	(0.084)	(0.126)	(0.048)	(0.055)	(0.053)
Constant	0.101**	0.162**	0.657***	0.660***	0.258***
	(0.050)	(0.076)	(0.027)	(0.032)	(0.031)
Mean Dep. Var.	0.14	0.2	0.64	0.69	0.34
Observations	356	212	792	498	570

*p<0.1; **p<0.05; ***p<0.01. Individuals in non-Covid occupations without employment in March-2021. The dependent variables are: weekly time spent on exploring alternative occupations (Column 1), total number of weekly applications (Column 2), total number of weekly interviews (Column 3), the mean job finding score of the set of search occupations (Column 4) and the number of suggestions from the message that are included in the set of search occupations (Column 5).

Appendix B: Additional Figures

Figure B1: Unemployment and vacancies in the Netherlands

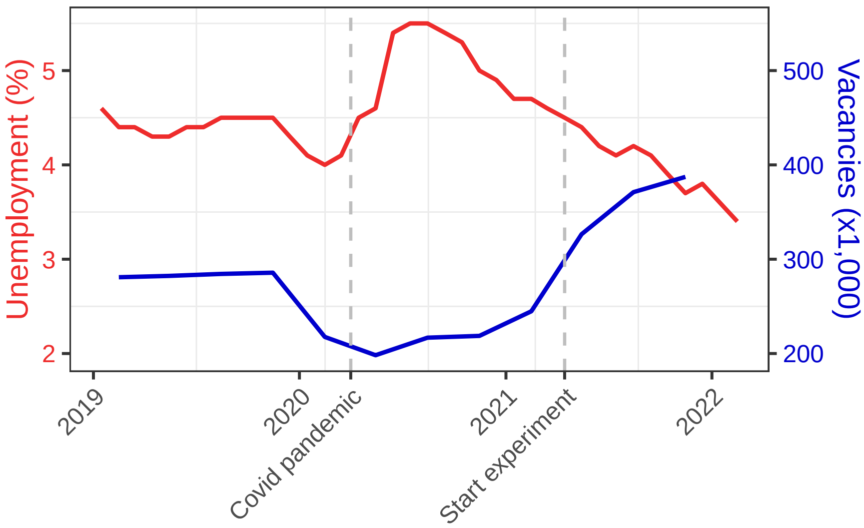


Figure B2: Job prospects of Covid and Non-covid occupations

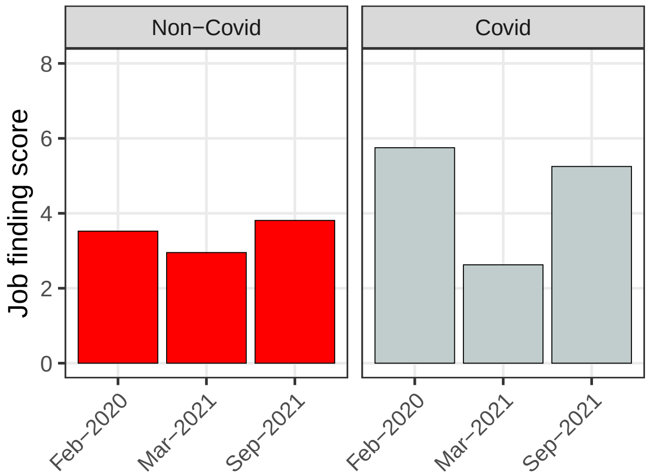


Figure B3: Number of search occupations

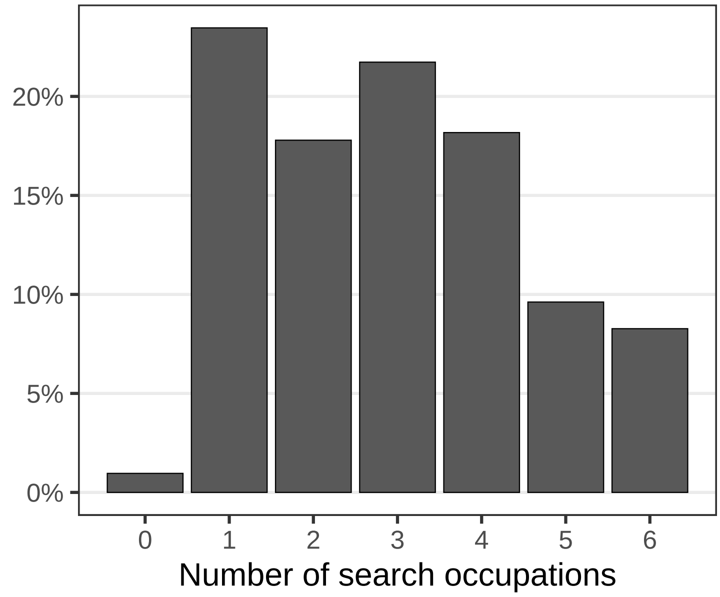


Figure B4: The number of suggested search occupations (form our intervention) that were already part of job seekers' initial set of search occupations

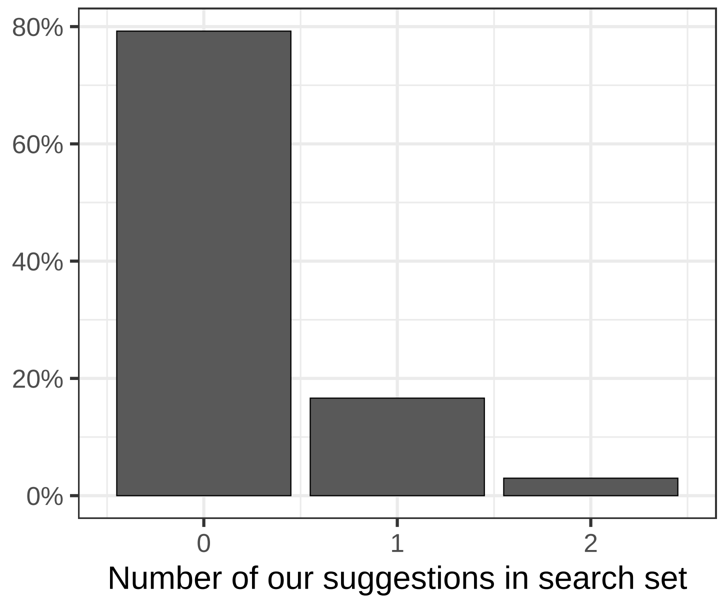
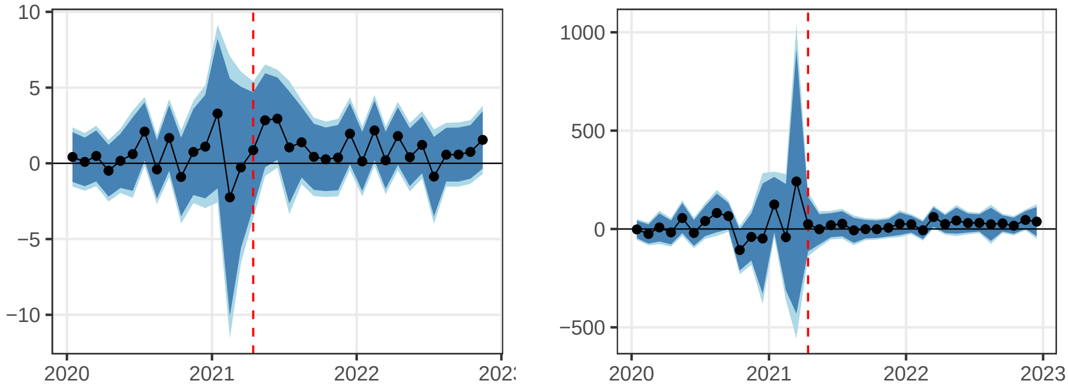


Figure B5: Intensive margin effects: treatment impacts conditional on employment

(a) Hours worked, conditional

(b) Labor earnings, conditional

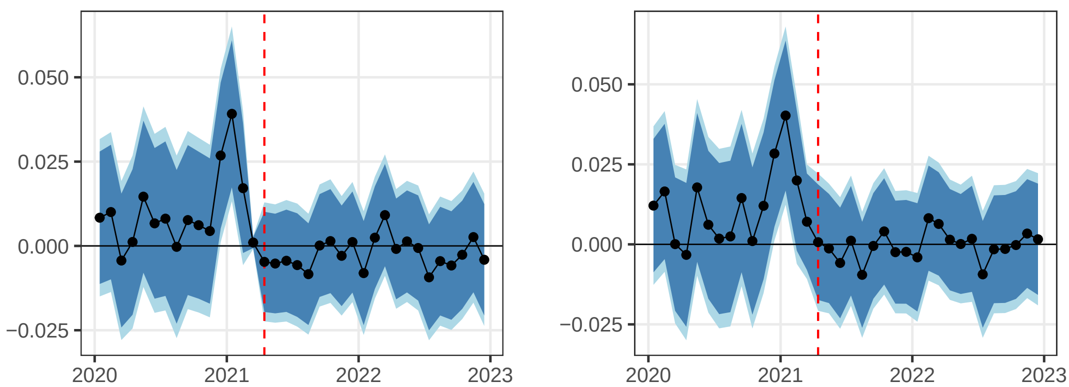


Note: The left panel considers only observations where the individual has positive working hours. The right panel considers only observations where the individual has positive labor earnings. Individuals in Non-covid occupations without employment in March-2021. The dashed red line indicates the first intervention message. The large confidence intervals closely before the intervention are due to our sample selection which includes only individuals without employment at that time.

Figure B6: Employment effects for job seekers working positive hours before treatment

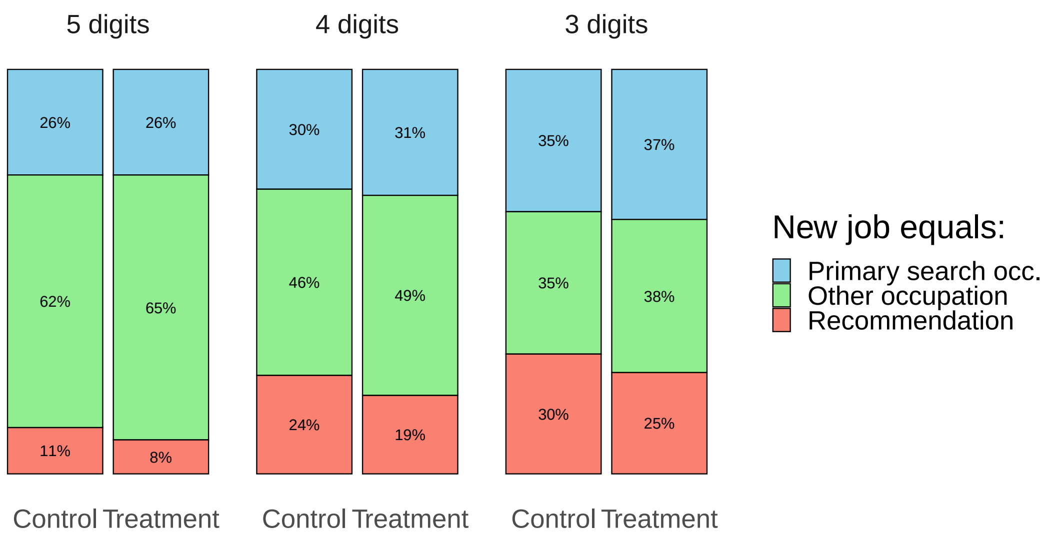
(a) Employment (hours > 0)

(b) Employment (hours > 300)



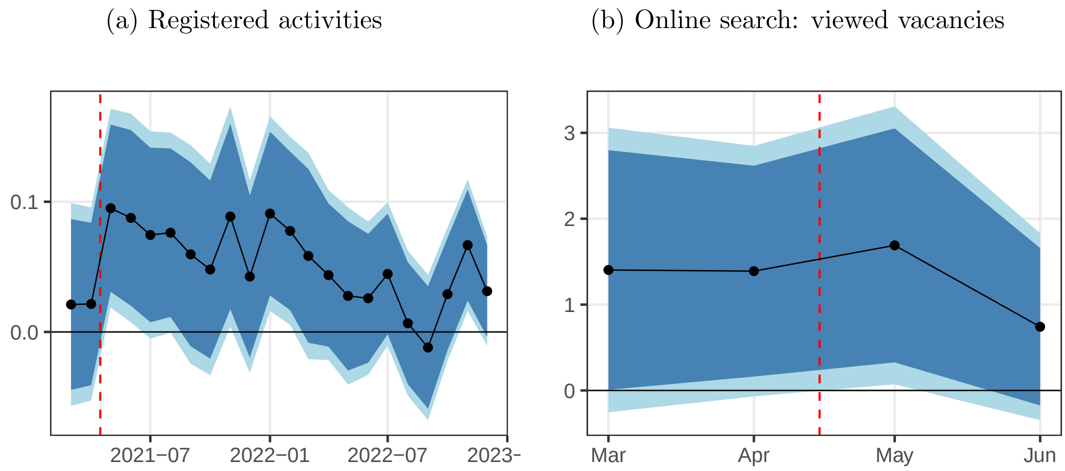
Note: Individuals in Non-covid occupations with employment in March 2021. The dashed red line indicates the first intervention message.

Figure B7: Comparison of occupations of new jobs with the primary search occupations and the set of recommendations (at different occupational digit levels): only individuals from Covid occupations.



Note: 'Primary search occ' report the share of individuals who found work in the same occupation as their initial 'primary occupation of search' (registered at time of registration for UI benefits). 'Recommendation' reports the share of individuals who found work in one of the occupations recommended to them, *excluding* the primary search occupation (especially at higher occupational coding levels these two often overlap).

Figure B8: Treatment impact on monthly job search: registered activities and online viewed vacancies



Note: Individuals in Non-covid occupations without employment in March 2021. The dashed red line indicates the first intervention message.

Appendix C: Motivational Intervention

The experiment included a second treatment arm, where a link to a motivational video was included. The intervention targeted psychological barriers to consider an occupational transition. A professional short film video was assembled, with former job seekers sharing their personal transition success stories. The aim of this video was to provide job seekers with more relatable stories about motivational challenges associated with occupational transitions and how to overcome them. While job seekers might find our alternative occupational suggestions interesting, they may still wonder if they would really be able to make the switch. Listening to the personal stories of others who have experienced such occupational transitions may be a source of motivation, as evidenced by the role models literature discussed in the introduction. We recruited role models through a newspaper column. In this column, we explained that a lot of people find occupational transitions to be difficult and perhaps even scary, and that individuals considering such a transition may benefit from learning about the experience of others. We asked individuals to submit a short, personal video. We selected nine recordings and asked a professional video maker to compile these clips into a 5-minute video. The video covers three main topics. First, the individuals introduce themselves and describe the transition they made (occupation they had before and new occupation). Second, they talk about how they experienced the transition. Third, they provide general advice and encouragement.

Appendix D: Additional Calibration Details

Datasets, Targets, and Calibration Approach

Datasets: We use several sources released by the Dutch Employment Agency, as well as data from our experiment, to discipline our calibration in Section 7. We use (i) 'Open Match Data' published by the Dutch Unemployment office, and select as date April 13, 2021 which is right before the start of our experiment (Dutch Employment Office, 2021). This provides per occupation the stock of vacancies and the number of unemployment insurance recipients that list this occupation as one of their search occupations (they can list up to three occupations). We also use (ii) a separate dataset by the Dutch Unemployment Agency (Dutch Employment Office, 2024b) listing the stock of job seekers covered by unemployment insurance by their primary search occupation. We select April 2021 for our data extraction.

In contrast to the first dataset, every job seeker is only listed once here, allowing a correct count of job seekers by their registered search activity. We use (iii) a publication on the traditional conversion from leaving unemployment insurance to actual employment (Dutch Employment Office, 2024a), which is 2/3. We use (iv) data from our own experiment, which provides targets for untreated and treated under the assumption that spillovers between groups are negligible. We will then simulate the economy with a non-trivial fraction of treated individuals to gauge how far away from this assumption the economy actually is. Finally, we rely on (iv) data from Statistics Netherlands regarding the size of the overall labor force in April 2021 (Statistics Netherlands, 2021).

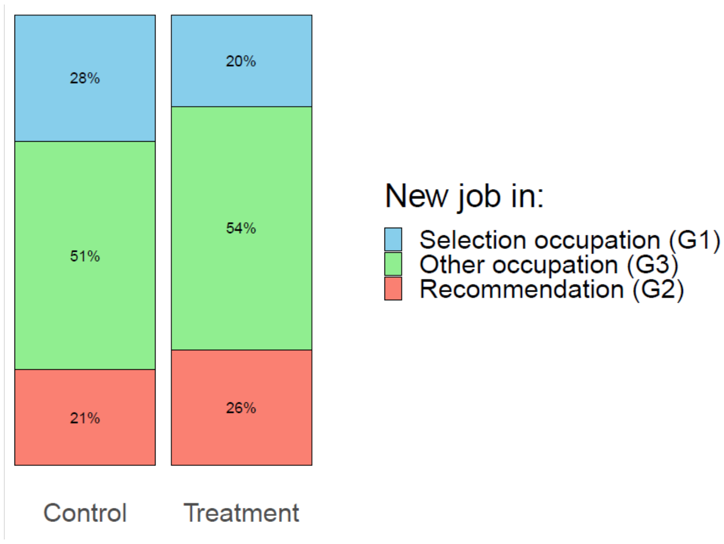
Pre-intervention calibration targets: Our matching function is $M(U,V) = AU^{\alpha}V^{1-\alpha}$ with $\alpha = 2/3$ as our benchmark. We only identify $A_G A$ where A_G is the occupation-specific matching efficiency, so we normalize $A = 1$. We obtain vacancy data V_G from source (i) by occupation group, see first three rows in Table A16. We consider each occupational listing by job seekers as a unit of search intensity. So the declared interests in a particular vacancy group in source (i) identifies search-weighted-unemployment U_G per occupation. See rows 4-6 in A16. Together, these immediately give pre-intervention market tightnesses $\theta_G = v_G/U_G$: $\theta_1 = 3.6\%, \theta_2 = 52.1\%, \theta_3 = 27.5\%$.

We set the number of unemployed individuals in group $g = 1$ equal to the number of individuals in our experiment in non-Covid occupations (row 7). The number of all "other" individuals in group $g = 2$ is set equal to the number of all other job seekers in source (ii) minus u_1 (row 8).

We use the average job finding rate in the control group of our experiment as the job finding rate F_1 of group $g = 1$: We construct it by taking the percentage increase in employment from one month to the next in the control group, averaged across the 18 months of the experiment. See row 9. For the job finding F_2 of "other" workers, we consider all workers with prime occupations in 4-digit-occupations that do not contain a non-Covid selection occupation, use their outflow from unemployment insurance from source (ii) and adjust by the average conversion to employment of 2/3 as in source (iii). See row 10. We use source (ii) to identify the fraction ρ_{2G} of "other" workers who have their primary occupation in occupational group G. See rows 11 and 12.

For the control group, the exit survey in (iv) identifies the fraction H_G of jobs found per occupational group G by job seekers in group $g = 1$. This is similar to Figure 6 in Section 6.2.2, but there we used only the selection and recommendation occupations for the particular job seeker in question, while here

Figure B9: Comparison of occupations of new jobs in the set of all non-Covid selection occupations (G=1), all non-Covid recommendation occupations (G=2) and "other" occupations (G=3) at 5-digit level: only non-Covid individuals.



Note: Only individuals with primary search occupation pre-treatment in a non-Covid occupation. 'Selection occupation (G1)' reports the share of individuals who found work in one of the non-Covid selection occupations, 'Recommendation (G2)' reports the share of individuals who found work in any occupation that was recommended to someone in a non-Covid occupation.

we group all selection and all recommendation occupations together to make the approach more consistent with our overall calibration strategy. Rows 13-15 provide the numbers, and Figure B9 illustrates it.

The pre-intervention survey in (iv) provides a measure of search intensity for group $g = 1$ across occupational groups G: we asked about occupations of interest, and count up to three entries to make this consistent with the search intensity as measured in (i). Rows 16-18 display the average number of times that occupations in group G are mentioned per survey participant. Row 19 provides a measure for the total labor force.

Pre-intervention calibration strategy: A time period is a month. Search intensity for jobs in G by "other" individuals $g = 2$ equals total search intensity minus the total search by group $g = 1$ averaged across individuals in $g = 2$: $s_{2G} = (U_G - s_{1G}u_1)/u_2$.

We parametrize $B_{gG} = PA_G$ for $(g, G) \in \{(1, 2), (1, 3), (2, 1)\}$, so the penalty $P < 1$ applies when there is no primary occupation search effort. We specify $B_{11} = \left(\frac{s_{11}-1}{s_{11}}P + \frac{1}{s_{11}}\right) A_1$, so that search effort beyond the primary search occupation is an occupation switch and gets penalty P. Similarly, for group $g = 2$ we specify $B_{2G} = \left(\frac{s_{2G}-\rho_{2G}}{s_{2G}}P + \rho_{2G}\right) A_G$, so that again every unit of search intensity beyond the fraction that is dedicated to primary search occupations gets penalized as an occupational switch.

We calibrate parameters A_G and P to jointly match the job finding probability of control group individuals per occupational group and the job finding probability of the "other" individuals, so we satisfy

$$\begin{aligned} B_{1G}s_{1G}m(\theta_G) &= H_G F_1 \text{ for } G \in \{1,2,3\}, \text{ and} \\ \sum\nolimits_G B_{2G}s_{2G}m(\theta_G) &= F_2. \end{aligned}$$

This yields $A_1 = 2.8\%, A_2 = 6.0\%, A_3 = 6.5\%, P = 63\%$. Up to this step no steady-state assumptions were used, and in principle all variables could be indexed by time $t =$ April 2021. In order to get a sense of some of the more macro variables that are in the background, such as the total labor force by occupational group or the separation rate, we calculate values for a steady state. In steady state the number of employed people who lose their job equals the number of people who find jobs, so that $\sum_g(\bar{e}_g - u_g)\delta = \sum_g u_g F_g$ or equivalently $\delta = (\sum_g u_g F_g)/(E - \sum_g u_g) \approx 0.2\%$. We can then identify μ_G via the steady-state version of (4) as $\mu_G = v_G - v_{G,t}\left[1 - C_G q\left(\theta_{G,t}\right)\right](1-\delta)$, where we need to use the θ_i that we already identified earlier and $C_{G,t} := \sum_g \left(B_{gG}s_{gG}u_g\right)/\sum_g \left(s_{gG}u_g\right)$. This yields $\mu_1 \approx 404, \mu_2 \approx 5807, \mu_3 \approx 12612$. Finally, to identify $\bar{e}_g$ equation (3) can be rewritten as $e_{gG} = B_{gG}s_{gG}m\left(\theta_G\right)u_g/\delta$, where all values on the right side are known, so that $\bar{e}_g = \sum_G e_{gG} + u_g$ and therefore $\bar{e}_1 \approx 317062, \bar{e}_2 \approx 9258938$.

Intervention calibration strategy: To calibrate the search behavior of the treated, we assume that the share that is treated is sufficiently small that it has no effects on market tightnesses θ_G that we computed for the pre-intervention. Given these tightnesses, we compute the search intensities s_{0G} of the treatment group such that we match the fraction that find a job in each occupational category, and that the employment rate of a control group individuals is roughly 2.5pp higher than that of a treatment group individual after 1 1/2 years. (see Table A17).

Brief discussion of calibration and modeling assumptions

We use each listed occupation of interest of a job seeker as one unit of his/her search intensity. This ties our hands regarding market tightnesses: It generates tightnesses in the same way that the Dutch Unemployment Agency measures them and publicly releases them. Their internal analysis has validated this as one of the main predictors of job finding for applicants, and therefore integrated it as a main part of their "job finding score" per occupation.

We modeled that occupation switchers have a lower job conversion rate $P < 1$. We could have modeled this as lower search intensity (i.e., each primary occupation has search intensity equal to 1, each non-primary occupation has search intensity P). Assuming that it reduces the search intensity instead of the conversion rate of meeting to matches would assume less externalities: With such an assumption, someone who searches in a non-primary occupation has lower arrival rate (similar to a lower conversion rate), but has also fewer externalities in the matching function because s/he does not exert as much job search that would negatively affect others. Our assumption is intentionally more conservative: someone who searches in a non-primary occupation exerts full search effort with associated externalities for others in the matching function, but nevertheless converts offers at a lower rate.

We model the occupational switching penalty even in the primary search markets: if search intensity s_{11} exceeds 1, then individuals search not only for their primary occupation in occupational group G=1. The switching penalty then applies, because even if this occupation is primary for someone else it is not primary for this job seeker. This also allowed us to discipline the "other" group (g=2), who get a penalty in $G \in \{2, 3\}$ if they search more than their share of primary occupations in that occupational group. It is a parsimonious way to penalize occupational switches. Our robustness check where we reduce to zero the penalty for treatment and control when switching to occupation $G = 2$ (or to other occupations in $G = 1$ beyond their primary occupation) shows remarkably little effect on SUTVA violations and efficiency after the economy is re-calibrated. The reason is that the probability of transition per search intensity is tied down by the data (rows 13-18 in Table A16, rows i) and ii) in Table 5). This gives us some confidence that it is the data targets more than the fine details of the penalties that discipline the exercise.

We model vacancies as long-lived: Once created, they remain unless they are filled or destroyed at the job destruction rate. This is intended to allow for larger negative spillovers: If treated individuals take additional jobs from non-treated individuals, these are not immediately re-posted but disappear for a while. Entry is also set to be exogenous to avoid trivially assuming SUTVA as in models of fully directed search with free entry. There, the market tightness adjusts to keep the free entry condition satisfied.[35]

[35]If productivity is the same for all individuals, and if the occupational switching penalty is modeled as lower search effort (see previous paragraph) then one can show that market tight-

nesses do not change when individuals change their search effort. This is known in the search literature as block-recursivity. The additional disadvantage here is that for free entry of firms one would have to assume something about the productivity of different job seekers in different occupations, for which we have little data to discipline it.

Table A16: Calibration of the Pre-Intervention Economy

Row	Target	Description and Source
1	$V_1 = 2165$	Vacancies, Group 1, source (i).
2	$V_2 = 68396$	Vacancies, Group 2, source (i).
3	$V_3 = 108265$	Vacancies, Group 3, source (i).
4	$\sum_g s_{g1} u_g = U_1 = 60181$	Search-weighted unemployment in $G = 1$: Number of times occupation $G = 1$ is listed as occupation of interest, source (i).
5	$\sum_g s_{g2} u_g = U_2 = 131369$	Search-weighted unemployment in $G = 2$: Number of times occupation $G = 2$ is listed as occupation of interest, source (i).
6	$\sum_g s_{g3} u_g = U_3 = 394162$	Search-weighted unemployment in $G = 3$: Number of times occupation $G = 3$ is listed as occupation of interest, source (i).
7	$u_1 = 15317$	Job seekers in group $g = 1$: Experimental subjects, non-Covid occupations, source (iv).
8	$u_2 = 213014$	Job seekers in group $g = 2$: All job seekers in source (ii), minus u_1.
9	$\sum_G B_{1G} s_{1G} m(\theta_G) = F_1 := 3.9\%$	Job finding rate for group $g = 1$, source (iv).
10	$\sum_G B_{2G} s_{2G} m(\theta_G) = F_2 := 8.4\%$	Job finding rate for group $g = 2$, source (ii) and (iii).
11	$\rho_{22} = 45\%$	Fraction of job seekers in group $g = 2$ with primary search occupation in $G = 2$: All job seekers in source (ii).
12	$\rho_{23} = 55\%$	Fraction of job seekers in group $g = 2$ with primary search occupation in $G = 3$: All job seekers in source (ii).
13	$\frac{B_{11} s_{11} m(\theta_1)}{\sum_G B_{1G} s_{1G} m(\theta_G)} = H_1 := 28\%$	% of jobs found in $G = 1$ for $g = 1$, source (iv), Figure B9, Left Panel.
14	$\frac{B_{12} s_{12} m(\theta_2)}{\sum_G B_{1G} s_{1G} m(\theta_G)} = H_2 := 21\%$	% of jobs found in $G = 2$ for $g = 1$, source (iv), Figure B9, Left Panel.
15	$\frac{B_{13} s_{13} m(\theta_3)}{\sum_G B_{1G} s_{1G} m(\theta_G)} = H_3 := 51\%$	% of jobs found in $G = 3$ for $g = 1$, source (iv), Figure B9, Left Panel.
16	$s_{11} = 1.3$	Search effort group of $g = 1$ in occupation $G = 1$: Pre-intervention survey, source (iv).
17	$s_{12} = 0.27$	Search effort group of $g = 1$ in occupation $G = 2$: Pre-intervention survey, source (iv).
18	$s_{13} = 0.74$	Search effort group of $g = 1$ in occupation $G = 3$: Pre-intervention survey, source (iv).
19	$\sum_g \bar{e}_g = E := 9576000$	Total labor force, source (v).

Table A17: Calibration of Intervention

Row	**Target**	**Source**
i)	$\frac{B_{02}s_{02}m(\theta_2)}{\sum B_{0G}s_{0G}m(\theta_G)} = \tilde{H}_2 = 26\%$	Source (iv), Figure B9, Right Panel.
ii)	$\frac{B_{0G}s_{03}m(\theta_3)}{\sum B_{0G}s_{0G}m(\theta_G)} = \tilde{H}_3 := 54\%$	Source (iv), Figure B9, Right Panel.
iii)	2.5pp higher employment of initially unemployed from g=0 (treatment) than from g=1 (control) after 18 months	Source (iv), Figure 5 panel (d).