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

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Abstract

We design and evaluate a communication intervention targeting job seekers searching in occupations with low labor demand. In a randomized experiment with the Dutch Public Employment Service, treated job seekers receive a personalized message informing them about poor employment prospects in their primary search occupation and suitable higher-demand alternatives. After 18 months, employment, hours worked, and labor income increased by 5–6 percent for treated job seekers, and they were more likely to find employment in a different occupation. A multi-occupation job search model confirms minimal SUTVA violations, with positive treatment effects outweighing negative externalities, suggesting the intervention is scalable.

Keywords: Job search, occupational mobility, randomized experiment, information treatment
JEL codes: J62, J64, C93

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1 Introduction

Occupational transitions play a significant role in labor market adjustments to changes in the economy. Technological development, and automation have been associated with profound changes in the demand for certain occupations, and future technological change is predicted to further shift occupational demands.[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 (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. 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). This may be because workers are unfamiliar with alternative occupations, uncertain about how their skills transfer, or reluctant to leave occupations in which they have experience. For these reasons, common policies often involve costly interventions such as retraining or intensive counseling.

In this paper, we design and evaluate an intervention targeting unemployed job seekers searching in occupations with poor labor market prospects. In collaboration with the Dutch Public Employment Service (PES), we designed an intervention that encourages job seekers to consider alternative occupations that are both realistically within reach and offer substantially better labor market prospects. The experiment involved 30,129 job seekers who receive unemployment insurance benefits and are registered to search in one of 21 occupations with poor employment prospects as identified by the Public Employment Service, mainly based on market tightness.

Approximately two thirds of the selected job seekers received a personalized message from the Dutch PES. The message presented a visual comparison between the job seeker's occupation of interest and a set of alternative occupations, highlighting differences in employment prospects, skill requirements, and other occupation characteristics. The recommended occupations were constructed by combining information on observed labor market transitions with information on current labor market conditions. Specifically, we identified occupations that work-

[1]For technological development and automation see, e.g., Autor et al. (2003); Autor (2015); Brynjolfsson et al. (2018); Frey and Osborne (2017). For discussion and prediction of occupational effects of AI, see, e.g., Eloundou et al. (2024); Acemoglu et al. (2022); Agrawal et al. (2019).

ers with similar backgrounds most commonly entered in the past and selected those offering substantially better job-finding prospects at the time of the intervention as recommendations. Almost 80% of the treatment group opened at least one of the messages. A sizeable share also clicked on at least one occupational suggestion for more information.

Our main analysis focuses on job seekers without any paid employment at the time of the intervention who were searching in occupations with structurally poor prospects (around 10,000 job seekers). Because we launched our intervention in the spring of 2021, a number of occupations had temporarily poor prospects due to restricted economic activities resulting from policy measures to deal with the Covid-19 pandemic. In line with our pre-registered analysis plan, we report results for job seekers searching in these occupations separately.

Our main outcomes of interest are employment status, labor earnings, hours worked, and unemployment insurance benefits received (all measured from administrative data). Further administrative data from online search records and caseworkers records allow us to investigate job search activities, and we fielded additional pre- and post-intervention surveys.

Little is known about the motivation and beliefs of unemployed job seekers in low-demand occupations. Our pre-intervention survey reveals that unemployed job seekers in low-demand occupations are on average willing to consider alternative occupations, are confident in their ability to work in an occupation where they have no experience, and believe that their skills are transferable. However, they are generally not aware of how poor the job prospects are in their primary occupation of interest, compared 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 labor market tightness or observable proxies of job market success (e.g., job interviews per application). Close to 80% list none of our recommendation occupations as current targets, even though these have close ties to their main search occupation (as indicated by the frequent transitions) and currently offer better job prospects. These findings point towards fertile grounds for our 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 substantial and long-lasting: employment increased

quickly over the first few months and even after 18 months employment remains elevated by 5.2% relative to the control group. The increase in employment is not associated with lower hours or hourly wages. In fact, we find it translates into higher hours worked and earnings: Monthly hours worked increases by 6.0% and monthly earnings by 6.4% relative to the control group. Reductions in the receipt of unemployment benefits we find are smaller and insignificant. There are two possible reasons for this. First, unemployment benefits are not cut one-for-one with income in the Netherlands, so as to provide incentives to take low-paying or low-hour jobs. Furthermore, recipients are only eligible for a limited period of time. As job seekers lose eligibility over time, this further attenuates potential impacts on benefit receipts. 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 occupation of interest 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 effects naturally raise the question of whether the 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 shows that each additional treated individual benefits the control group by freeing up their primary market. However, as the treated also benefit from this, our estimated treatment impact 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 a little over 1/3 of a job for others, meaning efficiency is at about 2/3 of our empirical estimates. Even in a counterfactual with full roll-out to all individuals searching in low-tightness markets, efficiency remains above 60%. Additional counterfactuals indicate our intervention to be effective even if the share of unemployed in low-demand occupation rises, or if tightness is not quite as low in treated occupations.

Our study connects most closely with the recent literature on automated job recommendation systems. Recent years have seen growing interest in systems that provide tailored guidance and recommendations to job seekers (see, e.g., Altmann et al., 2025; Belot et al., 2019, 2026; Ben Dhia et al., 2022; Bied et al., 2023; Le Barbanchon et al., 2023). The emerging evidence suggests that personalized recommendations can influence search behavior and, in some cases, improve employment outcomes.

The intervention we evaluate does not directly recommend specific jobs and

is not based on past search behavior. It aims at guiding job seekers in defining a search strategy. We focus on job seekers searching in occupations with poor labor market prospects. This group is of particular interest because recommendation tools may be especially valuable for them since they are hard to place in the labor market, but these tools are also particularly difficult to design for this group. Unlike many other job seekers, they may need to fundamentally reconsider the occupations they target, occupations that may differ substantially from the occupation in which they have experience or currently search. Identifying viable alternatives that are both attainable and offer substantially better prospects is far from obvious. This poses a particular challenge for recommendation systems that rely primarily on observed search behavior and are designed to predict what users are most likely to search for or engage with. Such systems may naturally reinforce existing search patterns, whereas workers searching in occupations with weak labor demand may benefit most from considering opportunities that fall outside those patterns. The recommendation strategy we design combines information on observed occupational transitions, derived from the full population of unemployed job seekers, with information on current labor market opportunities. This allows us to identify occupations that are both attainable and offer substantially better job-finding prospects. More generally, the intervention is designed to make unemployed workers aware that promising alternatives may exist outside the set of occupations they search in and to encourage exploration of alternative occupation that are within reach and more promising for finding employment.

The intervention we design can be readily implemented by PES, since it uses existing communication channels and exploits information (occupational transitions and tightness indicators) that PES generally have at their disposal. As such, it provides evidence on a scalable approach to helping workers whose current search strategy is unlikely to be successful. Based on the results of this study, the Dutch PES now sends quarterly emails containing this information to job seekers looking for work in occupations with poor prospects.

Finally, since redirecting search across occupations may affect labor market outcomes for other workers, we complement our analysis with a calibrated search-and-matching model to assess SUTVA violations and general equilibrium effects. This allows us to quantify spillovers and evaluate the implications of scaling the intervention beyond the experimental setting. In doing so, we also contribute to the literature on mismatch unemployment which argues that unemployment can be reduced if tightness is equalized across markets (see, e.g., Şahin et al., 2014; Marinescu and Rathelot, 2018).

The rest of the paper is structured as follows. In Section 2 we further detail the state of the recent literature. In Section 3, we describe the institutional context of our experiment. Section 4 describes the experimental design. We provide descriptive results regarding job search behavior 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 calibrated job search model. Section 8 concludes.

2 Related Literature

Our paper connects closely with the recent literature on automated job recommendation systems. Recent studies have evaluated systems that provide automated recommendations to job seekers about occupations, firms, vacancies, or offering broader search assistance. With some exceptions (e.g., Ben Dhia et al., 2022), the general finding is that such advice improves labor market outcomes of job seekers but magnitudes differ across the various recommendation methodologies. Belot et al. (2019) first introduced the methodology of recommending occupations based on occupational transitions in a small-scale experiment ($N = 300$) and showed promising impacts on interview invitations. In a second study implementing a similar methodology focusing on 800 long-term unemployed individuals, Belot et al. (2026) found large positive impacts on stable employment for such job seekers. Bächli et al. (2025) introduced skill-based job recommendations in addition to experience-based recommendations in a sample of $1,250$ job seekers, and showed that this can also improve job finding rates, though the group for which the methods are effective differ. Le Barbanchon et al. (2023) provided vacancies suggestions to those searching on the Swedish PES platform ($N = 380,000$) and Behaghel et al. (2024) partnered with the French PES to identify firms that are likely to hire and to recommend these to job seekers in a way that maximizes job creation ($N = 800,000$), taking into account labor market congestion effects. Both found positive albeit small impacts on job finding. Ben Dhia et al. (2022) evaluated a broader assistance program ($N = 212,277$) in France, and found no notable impact.

Altmann et al. (2025) provide online job-search advice to a broad population of roughly $92,000$ unemployment insurance recipients in Denmark. They test two interventions: (i) occupational recommendations and (ii) vacancy information, which displays the number of job openings in a job seeker's primary search occupations. A third group receives both, which looks to be least effective possibly due

to job seekers receiving mixed signals on whether to adjust their search strategy.[2] The study uses clustered randomization and varies treatment intensity across regions to assess spillover effects on non-treated job seekers, a concern that applies more generally to experimental interventions targeting job seekers (Crépon et al., 2013; Gautier et al., 2018). At moderate treatment intensity (60% of job seekers in a region) they find 2-3% higher employment, cumulative hours and cumulative earnings over six months, with persistent effects for the latter two also over twelve months; but they find no effects at high intensity (90% of job seekers in a region). They also show stronger effectiveness for the longer-term unemployed and those in low-tightness occupations.

In contrast to these studies, our study focuses exclusively on job seekers in low-demand occupations and the intervention is designed to help them redefine their job general search strategy (rather than direct them to specific vacancies or firms) with an explicit intended direction (from low-demand to high-demand occupations). It does so via a personalized communication, sent by the PES, that explicitly contrasts job finding prospects in the job seeker's occupation of interest with the prospects in attainable alternatives. The communication includes a visual illustration that is easy to understand and act upon.

Our intervention further relates to a second strand of literature, on "mismatch unemployment". In their seminal paper, Şahin et al. (2014) find a limited role for mismatch across geographical markets, which is confirmed by Marinescu and Rathelot (2018). The role for occupational mismatch is much larger (Şahin et al., 2014). Klaeui (2026) finds that job seekers who are assisted by caseworkers who tend to send job seekers to higher-tightness occupations are indeed more likely to find employment, suggesting such retargeting is effective. Our paper contributes to this literature by evaluating a concrete intervention that aims to shift job search from slack to tighter occupational markets. Our results shows that externalities are small and reinforce the point made in the mismatch literature: more equal market tightness is an aggregate improvement and not just an improvement for the treated.

Finally a recent but growing literature has started to focus on the beliefs of job seekers, such as job finding probabilities (see e.g., Spinnewijn, 2015; Mueller

[2]The first intervention does not provide information about prospects. The second intervention informs indirectly about prospects, via the number of vacancies in the occupation of interest. This information is, however, likely imperfect because the number of vacancies is imperfectly correlated with tightness. It also remains unclear to what extent job seekers interpret the vacancy counts as a signal of market tightness. When job seekers receive both interventions, the different information provided about primary and recommended occupations may lead to mixed signals.

et al., 2021; see Mueller and Spinnewijn, 2023 for an overview), outside options (Jäger et al., 2024), and wages (Caliendo et al., 2024; Cortés et al., 2023). We are not aware of a literature that specifically considers beliefs (as well as motivation) of unemployed individuals in low-demand occupations, and their beliefs across different occupations.

3 The Dutch Institutional Context

At the time of our experiment, unemployment in the Netherlands was relatively low (4.2% in 2021), compared with 5.4% in the US and 6.2% on average across OECD countries.[3] Employees who lose their job are often eligible for unemployment benefits, subject to specific conditions and job-search obligations. Recipients are required to engage in job search, accept suitable work, including temporary work, and report their activities through the PES's online system.

In the Netherlands, it is possible under specific conditions to work in a paid job while receiving unemployment benefits ('partial unemployment'). In 2021, about 232,000 individuals received unemployment benefits in an average month. Roughly 140,000 of them did not hold a job. Among these fully unemployed recipients, previous occupations were unevenly distributed: 22% had worked in administrative occupations, 14% in technical occupations, 12% in commercial occupations, and the remainder in other occupations.[4]

The Dutch PES has two main tasks. First, it administers and pays employee insurance benefits, including unemployment benefits. Eligible individuals who register for benefits receive access to an online 'work folder' in which they provide information on their employment history, personal situation, and resume. As part of this process, job seekers can register up to three 'search occupations', i.e. occupations in which they would like to work. Second, it supports job seekers in finding work, particularly those with a greater distance to the labor market. Although job seekers are assigned a caseworker, the agency increasingly emphasizes online self-service. Two of these online tools are central to our experiment. First, using data on registered job seekers and vacancies, the agency publishes occupation-specific job prospects online.[5] Second, it publishes lists of alternative occupations

[3]Data retrieved at https://data.oecd.org/unemp/unemployment-rate.htm on March 6, 2026.

[4]Number obtained directly from Dutch PES.

[5]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, accessed May 4, 2026.

based on common occupational switches observed in resume data.[6] These data are publicly available but not personalized. As a result, job seekers still need a good sense of which alternatives fit their skills and experience. Our experiment builds on these tools by consolidating the available information, adding new information such as automation risk, and providing it proactively in a personalized email.

4 Experimental Design

4.1 Selection of occupations

As explained in Section 3, unemployed job seekers can enter up to three 'search occupations' on the platform of the PES. The aim of our intervention is to help job seekers searching in occupations with low employment prospects to consider different, more promising occupations.

To select occupations with poor job finding prospects, we use the 'job finding score', a metric used by the Dutch PES that combines vacancies, job seekers in the employment office's database, and outflow rates of unemployment insurance recipients.[7] The score ranges from 2 (very poor prospects) to 10 (excellent prospects) and is updated several times per year for more than 600 narrowly defined occupations (4-digit classification). For the experiment, we selected all occupations with a score of 2, 3 or 4 in spring 2021, yielding 21 occupations (henceforth *selection occupations*). These occupations differ substantially in the nature of the work and required qualifications, ranging from animal caretakers and waiters to taxi drivers, graphic designers, and social workers. The full list is provided in Appendix Table A1, including the original Dutch occupation names.

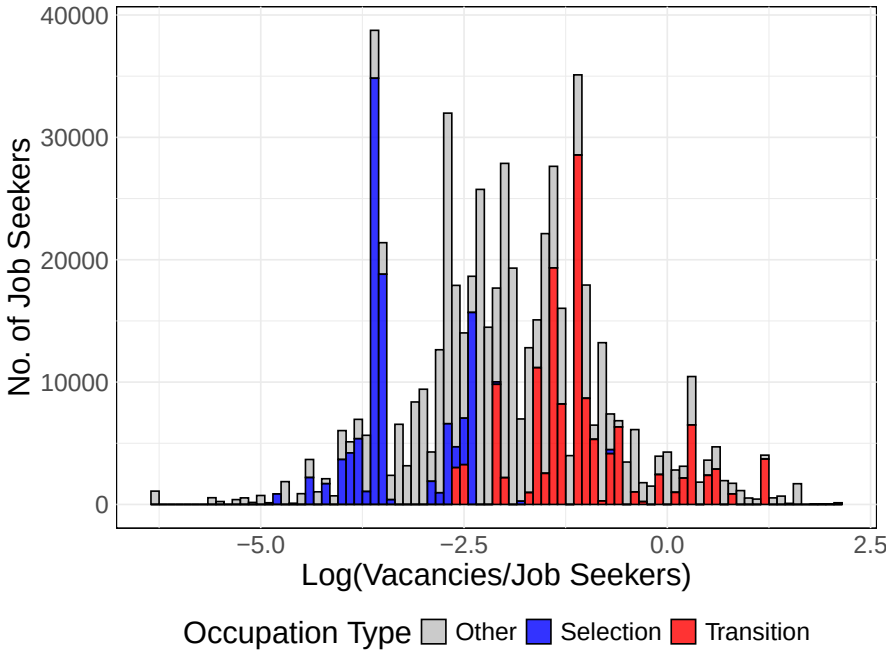
We then select all registered job seekers who indicate on their CV that they are looking for work in one of these 21 occupations. This restricts the sample to job seekers who have completed an online CV, which also ensures a minimum level of digital skills. Because the intervention is delivered by email, this is desirable. Finally, we require that job seekers have at least one month of unemployment insurance eligibility left at the time of sample selection, so that they do not mechanically leave the sample before receiving the first message.

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

[6] https://www.werk.nl/arbeidsmarktinformatie/kansen-arbeidsmarkt/overstapberoepen-werk-vinden-in-ander-beroep, accessed May 4, 2026.

[7] The occupational classification used is called 'BRC+', which is a slight extension of the Dutch 'BRC' classification, which itself is a modified version of the ISCO classification. Its modifications are meant to better reflect the Dutch labor market.

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



Note: Job seekers can register up to three occupations as their main search occupations. As a result, some job seekers are included in multiple occupations in this figure.

tions), we combine two criteria: historical occupational switches and current labor market prospects. Using resume information from the PES, we identify the occupations to which similar job seekers most often switch. We interpret these observed transitions as evidence that such moves are feasible. To ensure favorable current prospects, we restrict attention to occupations with a job finding score of at least 6. Depending on the primary occupation of interest, we selected 7 to 9 alternatives. This procedure ensures that suggested occupations are both plausible transitions and offer substantially better job finding prospects. Among occupations with sufficiently good prospects, we generally selected those with the highest number of historical transitions and presented them in order of observed transition frequency, while allowing limited adjustments based on the expertise of the PES.[8]

To put these differences in perspective, Figure 1 shows the distribution of market tightness, defined as the logarithm of vacancies divided by the number of job seekers, for selection, transition, and other occupations.[9] The figure confirms the expected pattern: selection occupations have much lower vacancy-to-job-seeker

[8]In particular, the PES advised combining some occupations that were deemed very similar and omitting others considered less suitable transition options.

[9]These statistics are based on 'Open Match Data' published on April 13, 2021 by the Dutch PES.

ratios than transition occupations. The average ratio is approximately 0.04 for selection occupations, compared with more than 0.5 for transition occupations.[10] Appendix Tables A2 and A3 further compare job seekers in selection occupations with those who are looking for or have experience in transition and other occupations. They are broadly similar in terms of education, experience, and search radius.

4.2 Interventions

Our intervention provides information through a visualization designed together with the communication department of the Dutch PES and sent to job seekers by email.[11] In the introductory text, we emphasize two points: first, that the occupation in which job seekers are currently searching has poor labor market prospects because there are few vacancies relative to the number of job seekers; second, that given their skills and experience, there are alternative occupations for which they likely qualify and that offer much better prospects. The message is therefore meant both to create urgency about reconsidering the current search strategy and to reassure job seekers that feasible alternatives exist.

Figure 2 shows an example of the visualization. We first list the job seeker's primary occupation of interest and display its job finding score using a colored bar (1). We then show a set of matched alternative occupations, ordered largely by historical transition frequency and, to a lesser extent, by job prospects. For each alternative occupation, we display the job finding score in the same way (2), together with two associated skills (3), to make clear that existing skills and experience can also be valuable elsewhere. For occupations with low automation risk, we additionally mention this in the message (4).[12] Finally, we provide a link to more information on the occupation, including job descriptions, required certifications, and related job titles (5).

Our approach differs from prediction-based recommendation systems such as

[10] One may worry that the suggested occupations offer lower wages or worse conditions in terms of working hours or contract stability. Constructing reliable occupation-level indicators is difficult because the administrative data do not contain occupations, while the Dutch Labor Force Survey only reports broader occupational codes that combine several occupations from our set. Rather than relying on such imperfect measures, we assess this concern directly through our treatment effects by showing that earnings per hour are very similar in the treatment and control groups.

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

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

Figure 2: Example of information message visualization



Le Barbanchon et al. (2023) and Behaghel et al. (2024). Rather than relying on clicks or predicted matches based primarily on occupational proximity, we combine observed successful transitions with occupation-level labor market tightness. This allows us to recommend attainable occupations with substantially better prospects, including transitions across broad occupational classes that proximity-based methods may overlook.[13] An additional advantage is that our intervention aims to shift job search strategies rather than recommend specific vacancies or firms, and is transparent to job seekers.

The experiment also included a second treatment arm that added a link to a motivational video showing job seekers who had recently made a career transition. Because only a small fraction clicked on the link, we merge both treatment arms in the analysis. Details on this motivation intervention are provided in Appendix C.

[13] In the model we develop in Section 7, we find similar transition penalties to Behaghel et al. (2024) despite this larger average distance.

4.3 Randomization, data collection and timeline

We selected the sample on March 15, 2021 from the stock of job seekers registered as primarily searching in one of the 21 selection occupations. The final sample included 30,129 individuals who remained at least partially unemployed until the first message on April 12. These individuals constitute our full experimental sample.[14]

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 of the sample, balanced across treatment groups, was selected to receive the pre- and post-intervention surveys, with responses incentivized through donations to charity. After sample selection, we administered the pre-intervention survey, sent the intervention messages, and then administered the post-intervention survey. Subsequently, we sent outflow surveys to those who appeared to have found jobs, based on a month-to-month earnings increase. Finally, we obtained administrative data on employment, earnings, hours worked, and unemployment insurance benefits for the full sample over 2020–2023. Table 1 gives the precise timeline and sample sizes.

The first intervention message was sent on April 12, 2021 and contained the information visualization for both treatment groups, as well as the video link for the motivation group. Section 6.1 reports engagement with these messages: 64% opened the first message, but few clicked on the video link. We therefore sent an additional video-only message to the motivation group on May 10, 2021. A final reminder was sent on May 28, 2021, containing the information visualization and adjusted using click statistics from the first message. In total, 79% opened at least one message.

The pre- and post-surveys included questions on job search behavior, beliefs about labor market prospects and wages, and willingness to explore occupations other than the primary occupation. Further details are provided in Section 5, where we report descriptive statistics. Because the administrative data do not contain occupations, we administered outflow surveys to collect information on the occupations people moved into. Every two to three months, we selected job seekers with a labor income increase of at least €300 in the preceding months. We also added individuals who exited unemployment insurance with registered

[14] We did not conduct power analyses, as the maximum sample size was fixed ex ante and the marginal costs of sending an additional email were negligible. We also do not report post-hoc power calculations, following Gelman (2019).

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	597	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.

indication 'employed'. The survey asked about the new job, including starting date, occupation, and how tasks compared with those in the pre-unemployment job. Because the survey was intended only for those who had found work, the invitation and the opening survey question both explicitly asked respondents to confirm that they had indeed found a job.

4.4 Sample restrictions

For the main analyses, we impose two additional sample restrictions on top of those discussed in Section 4.1. First, we focus on occupations with structurally poor prospects rather than occupations whose poor prospects were primarily driven by Covid. Labor market conditions fluctuated sharply around the start of our experiment, as shown by the unemployment and vacancy series in Figure B1 in the Appendix. Although all selection occupations had poor prospects in early 2021, they differed substantially in their longer-run trends, in part because some were much more affected by social distancing measures than others. We therefore identify as 'Covid occupations' those occupations whose job finding score fell by at least two points at the onset of the pandemic (February 2020) and recovered by at

least two points by September 2021. Of the 21 selection occupations, 7 meet this definition and 14 do not. Table A4 lists all occupations and their classification, while Figure B2 shows the evolution of job finding scores for both groups. As expected, the Covid occupations recover strongly in late 2021, whereas the non-Covid occupations display persistently weak prospects.

That said, the effects for job seekers in Covid occupations are also of interest. We pre-registered the Covid/non-Covid split as a heterogeneity analysis before it became clear that their labor market prospects would diverge. At the time of the intervention, there was substantial policy interest in mobility out of Covid-affected occupations, so one might have expected sizable treatment effects. At the same time, effects could be smaller for two reasons: job seekers may have expected demand in their original occupations to recover and therefore ignored the advice, or the intervention may have induced switching without increasing eventual job finding much because poor prospects in these occupations were temporary.

Second, we restrict the main analysis sample to unemployed individuals who were not working at the start of the intervention. The main reason is a striking pre-intervention difference in employment rates between treatment and control, despite balance on demographic characteristics, which suggests that randomization itself was successful. Figure B7 in the Appendix documents this difference. To avoid bias from this pre-treatment imbalance, we restrict the sample to individuals with zero hours worked in March 2021, just before the intervention. Within this sample, all characteristics are balanced between treatment and control groups. This group is also particularly relevant from a policy perspective, as it is the most detached from the labor market, and it aligns closely with our stated focus on "unemployed job seekers" in the pre-analysis plan. This restriction is similar to that used in Crépon et al. (2013).[15] Unless otherwise noted, all figures and tables from Section 5 onward are based on the 'Non-covid, 0-hours worked prior to the experiment' sample, which we refer to as the 'main sample'.

4.5 Hypotheses

The aim of the intervention is to make job seekers aware of the poor prospects in the main occupation they are currently searching in, and to provide suitable alternatives with better prospects that may encourage them to redirect search toward those occupations. If job seekers are imperfectly informed about the prospects in

[15] They "focus on results for those who did not claim to be employed at baseline" (Crépon et al., 2013, p.550).

their own occupation relative to alternatives, as we document in Section 5.2, this information makes the current occupation less attractive relative to other options.

In models of multi-occupation job search, such as Belot et al. (2019), unemployed individuals allocate search effort across occupations to maximize the discounted value of future income, including unemployment benefits. Negative information about matching prospects in the main occupation reduces search effort there, while positive information about particular alternative occupations increases search effort toward those occupations.[16] In such models, more accurate information improves choices and, under standard assumptions, should raise job finding and employment, increase hours worked and cumulative earnings, and reduce unemployment benefit receipt. We therefore study these outcomes in the administrative data, and use the exit survey to examine occupational shifts away from the primary search occupation and toward recommended occupations.

These benchmark predictions need not hold, however, once the model is generalized. If total search effort is endogenous, negative information about the main occupation may reduce overall search rather than simply reallocate it. If the recommended occupations offer higher wages but are difficult to enter for switchers, employment may fail to rise even when search shifts toward them. Likewise, higher employment need not translate into substantially lower benefit receipt if benefits expire before re-employment or are only partially reduced upon returning to work. Finally, if job seekers overreact or update in a non-Bayesian way, they may shift search toward occupations in which they have little realistic chance of success.

To assess these possibilities empirically, we compare outcomes in the treated and control groups. We then use the observed changes in search behavior to parameterize a quantitative model of labor market spillovers, allowing us to study externalities beyond the experimental sample.

5 Descriptive results

Before turning to the impact analysis in Section 6, we present descriptive statistics for our sample and document a range of findings on job search behavior and beliefs.

Table 2 reports summary statistics for our sample and shows how these change

[16] In Belot et al. (2019) search costs are increasing and strictly convex in total effort, so a reduction in search in one occupation lowers marginal search costs and expands search in others. They also cover the limiting case of fixed total search effort. We additionally note that their framework can be interpreted as one in which beliefs concern job-finding chances per unit of search effort; introducing matching frictions and uncertainty about tightness maps naturally into such beliefs.

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%	48%	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.

when we impose the sample restrictions discussed in Section 4.4. The first column shows that our initial sample of 30,129 unemployment insurance recipients skews female and has an average age of 47. Average unemployment duration exceeds 7 months, suggesting that many job seekers face persistent difficulties in leaving unemployment, although most are still entitled to substantial benefits: 51 weeks on average. More than half have attained a two- to four-year vocational degree (medium education), while the remainder is roughly equally split between those without a post-secondary degree[17] and those with a higher education degree. Particularly relevant for the construction of the main analysis sample are the shares still working at the time of sample selection and searching in 'Covid occupations'. The table shows that about two-thirds did not work at all, while about half searched in a Covid occupation.

The second column shows how restricting the sample to those without work in March 2021 affects the sample composition. There are very few changes.[18]

The third column adds the 'non-Covid' restriction. By construction, this changes the occupational composition (see Table A4 in the appendix). Because receptionists, a female-dominated occupation, are excluded, the share of men rises. This sample is also more highly educated than the full sample, with about 31% holding a higher education degree. Naturally, the opposite holds for the 'Covid'

[17]This includes individuals who graduated from a 1-year vocational education program.

[18]Table A4 in the appendix compares the distribution of selection occupations, showing that taxi drivers account for the only major change when restricting to those without employment in March 2021.

sample (Column 4).

5.1 How do job seekers search?

For the subsample that completed the pre-intervention survey and meets the sample selection criteria ($N = 1{,}040$), we observe a rich set of beliefs and search activities. Although respondents are not fully representative, Appendix Table A7 shows that they are fairly similar to the rest of the sample on observable characteristics. We therefore use these survey data to describe how broadly job seekers search before turning to whether they correctly perceive differences in job prospects.

Survey respondents indicate their primary search occupation, typically the selection occupation, as well as any alternative occupations they are considering.[19] Figure B3 in the Appendix shows the number of occupations they report searching in. About 25% search in only one occupation, nearly 50% in two or three, and around 25% in more than three. Appendix Table A9 further shows that most respondents (i) spend at least some time exploring alternative occupations, (ii) are fairly willing to consider new occupations, (iii) are reasonably confident in their ability to work in occupations in which they have no experience, and (iv) believe that their skills are transferable. More than half expect to widen their occupational search if they are still unemployed two months later.

For the primary and first alternative search occupation, we collect measures of job search activity and elicit beliefs about search returns (Table 3). For most respondents, the primary search occupation is the selection occupation and therefore has a low average job finding score (3.00, Row 1). The first alternative offers somewhat better prospects, with an average score of 4.29. In the two weeks preceding the survey, respondents submitted on average 3.2 applications in the primary occupation and 2.5 in the first alternative (Row 2). The corresponding numbers of interviews are 0.44 and 0.36. Interviews per application are slightly higher in the alternative occupation (Row 4), consistent with its higher job finding score. Except for the raw number of interviews, these differences are statistically significant at at least the 10% level. Taken together, these patterns suggest that many job seekers already consider alternatives, but devote less search effort to them than to their primary occupation. At the same time, these alternatives still tend to have fairly weak prospects: transition occupations in our intervention were required to have a score of at least 6. Consistent with this, Figure B4 in the Appendix

[19] 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.

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. Main sample (zero employment in March-2021, non-Covid occupations)."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.

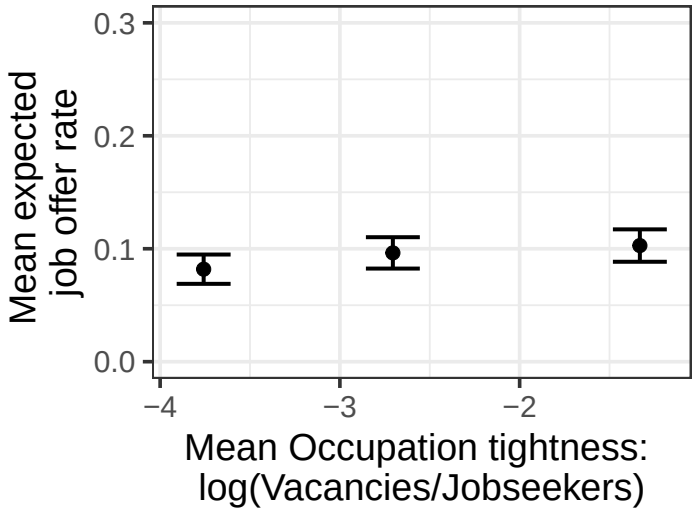
shows that 78% were not yet searching in any of the suggested occupations before receiving the information.

5.2 How well are job seekers informed about job prospects?

We elicit a range of beliefs about job search returns and labor market prospects. Our main question is whether expectations about prospects across occupations align with actual prospects, and whether these expectations help explain search behavior. Respondents first report how many applications they believe are needed, on average, to obtain one acceptable job offer in their primary occupation and first alternative. Inverting this gives the expected job offer rate per application, which is low on average (0.09, Row 5 in Table 3) and strikingly similar across the two occupations.

To assess how well informed job seekers are, we relate these beliefs to actual job finding prospects. Using variation in the alternative occupations respondents consider, Figure 3 plots expected job offer rates against occupational log-tightness, grouped into three equally sized bins. The Figure shows a very flat relationship: regardless of true tightness, the expected job offer rate remains close to 0.1. This

Figure 3: Expected job offer rate for job seekers' first alternative occupation



Note: Figure shows the expected job offer rate by the occupational tightness of their alternative occupation into three equally sized bins.

suggests that job seekers do not select alternative occupations on the basis of better prospects. Most choose alternatives with only slightly better tightness, and even those who consider relatively high-tightness alternatives do not appear to recognize that these offer better chances of finding work.[20] Job seekers seem not to learn much about these differences over time either. Table A10 shows that, while the control group becomes slightly more optimistic about finding a job within two months, there is no evidence that they differentially update beliefs about the prospects of their primary and alternative occupations.

Returning to Table 3, job seekers also report very similar wage expectations and reservation wages across the two occupations. Expectations about job stability, measured as the probability of keeping a new job for at least two years, are slightly more optimistic for the alternative occupation. Rows 9 and 10 show how many applications job seekers think they would send *per week* to the primary and alternative occupations if job offer rates and wages, respectively, were equalized. These hypothetical application numbers are substantially above actual search intensity and largely eliminate the gap between the two occupations. Finally, the last row shows that job seekers expect only limited updating: if still unemployed two months later, they expect the job offer rate to fall only slightly, from 0.09 to 0.08 for the primary occupation and from 0.10 to 0.08 for the alternative occupa-

[20] As we show in Section 6.2, job seekers do tend to struggle to find work in their low-tightness selection occupation despite relatively high search intensity, indicating that tightness is likely a good predictor for job offer rates.

tion.

Taken together, the descriptive evidence suggests that the main barrier is not a complete reluctance to consider other occupations. Many job seekers report being willing to broaden their search and do list alternatives, but the occupations they consider tend to offer only modestly better prospects and their beliefs are weakly aligned with actual labor market tightness.

6 Results

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). In total, 19,960 job seekers received this first message.[21] Of these, 12,804 opened the email (64%). Each suggested occupation contained a clickable link with more information, such as tasks, skills, related occupations, and educational requirements. Across the first message, we observe 4,975 clicks on occupations. These clicks are far from evenly distributed across the 165 suggested occupations shown in the experiment. Appendix Table A11 shows that job seekers are more likely to click on occupations with more historical transitions, higher job finding scores, and a low automation risk indicator. Ranking also matters strongly: the occupation shown first receives more than twice as many clicks as lower-ranked suggestions.

We sent a reminder email with a similar visualization on May 28, 2021. In consultation with communication experts from the PES, we shortened the message to five suggestions and reordered occupations based on predicted click interest from the first message, using job finding prospects, automation risk, and historical transitions as predictors. This second message was sent to 16,838 individuals, of whom 11,475 opened it (68.1%). Among those who opened it, 2,442 clicked on at least one link (21.3%). Across both messages, 15,867 individuals opened at least one email (78.8%), and 4,874 clicked on at least one occupation link (30.7%).

The motivational treatment group received the same first message, with an additional paragraph linking to a motivational video. Very few recipients clicked on that link (0.5%). An additional video-only message increased the click rate to 7.5%, but overall take-up of the video remained low. We therefore combine the

[21] Due to data limitations, we cannot apply the same sample restrictions (zero employment in March 2021, non-Covid occupations) to the email opening statistics, and therefore report these statistics for the larger sample.

two treatment groups in the analysis that follows and focus on the effects of the informational content they both received.

6.2 Treatment effects

Given randomized assignment, our empirical strategy is straightforward: we compare outcomes between the treatment and control groups.[22] Following our pre-analysis plan, we first consider the primary outcomes: unemployment insurance benefit receipt, employment, hours worked, labor earnings, and whether individuals find work outside their initially preferred occupation.

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

We observe monthly hours worked, benefit receipt, and labor earnings for the full sample in administrative records. Using hours and earnings, we also construct employment indicators.[23] We define employment in two ways: having positive earnings in a month, and having monthly earnings of at least €300. We view hours worked as the broadest measure of labor market attachment as it aggregates across potentially multiple part-time and temporary jobs. We examine labor earnings as the most complete measure that takes the wage level into account, and benefit receipts as our measure of benefit dependence.

To estimate treatment effects over time, we regress outcome Y_{it} in month t on month fixed effects (γ_t), demographic controls (X_i) with time-varying coefficients, and a treatment indicator (T_i) with time-varying coefficient:

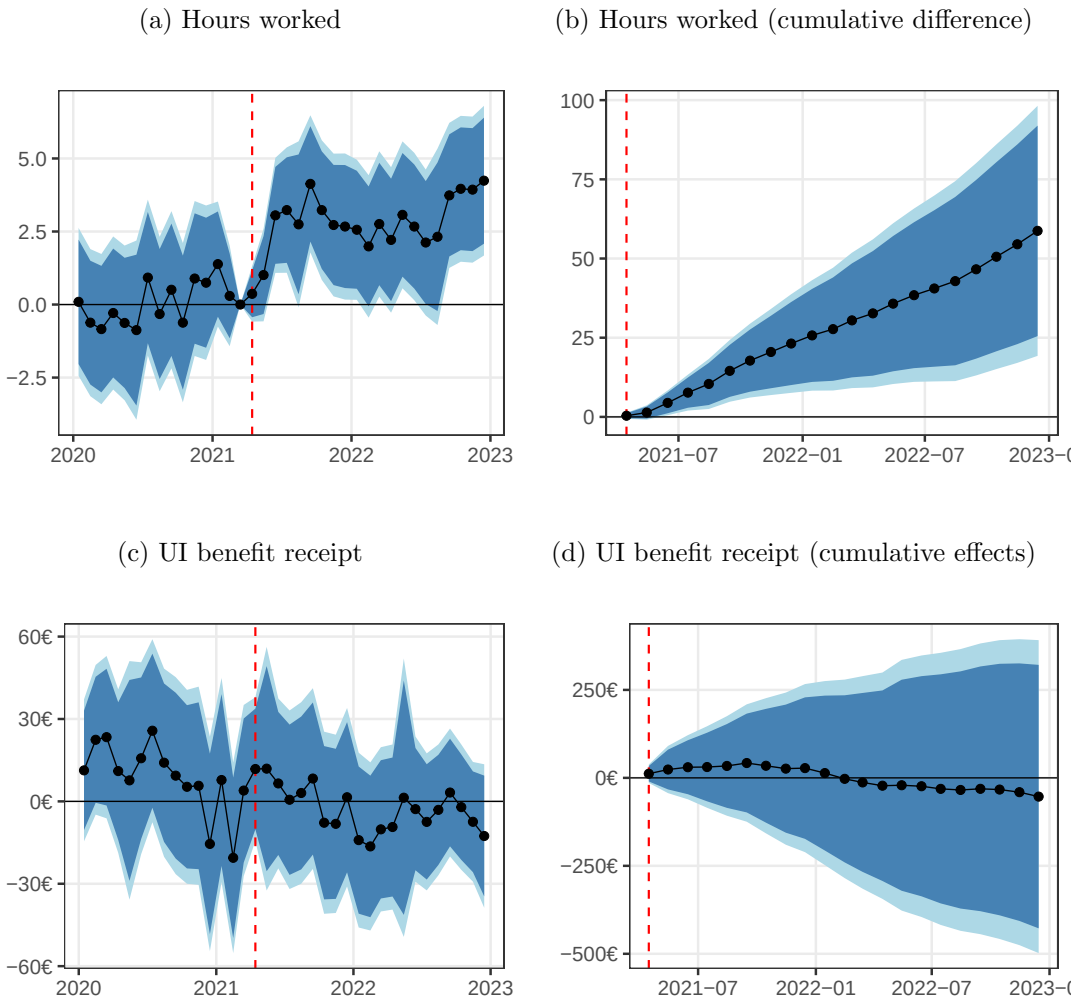
$$Y_{it} = \gamma_t + X_i\beta_t + \lambda_t T_i + \varepsilon_{it}. \tag{1}$$

Standard errors are clustered at the individual level. The months t run from January 2020, 14 months before treatment, through December 2022, 20 months after treatment. Pre-treatment months provide an additional check of randomization, and including covariates mainly improves precision.

[22] Balancing checks are reported in the Appendix. We obtain near-perfect balance on the stratification variables (gender, unemployment duration in three bins, and selection occupation), and the samples are also balanced on other characteristics (see Appendix Table A5 for the main analysis sample and Appendix Table A6 for the full sample). Appendix Table A8 shows that survey responses are also broadly balanced.

[23] Many job seekers find temporary and part-time jobs while continuing to receive fluctuating unemployment insurance benefits, so outflow from benefits cannot be cleanly defined at a point in time.

Figure 4: Treatment impact on hours worked and benefit receipt

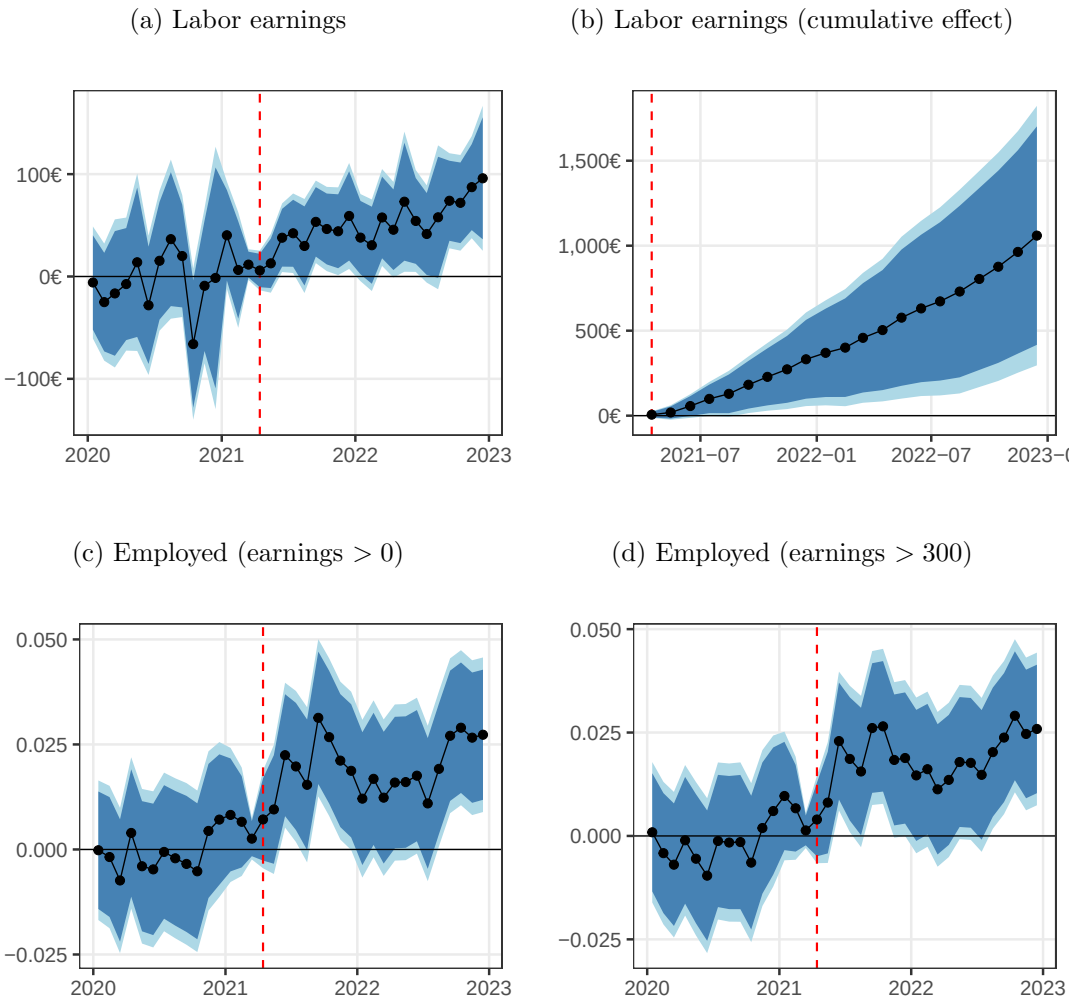


Note: Main sample (zero employment in March-2021, non-Covid occupations). The dashed red line indicates the first intervention message.

Figure 4 reports treatment effects on hours worked and benefit receipt for the main sample. Prior to treatment, there are no meaningful differences in hours worked, with, by construction, a zero difference in March 2021. From about two months after the intervention onward, treated individuals work roughly 2 to 4 hours more per month than controls, with significance in most months. The cumulative effect rises steadily and reaches about 55 additional hours by the end of 2022 (panel b). We therefore conclude that the intervention increased labor supply.

Panel (c) shows no comparable effect on monthly unemployment insurance benefit receipt: post-treatment coefficients are close to zero and never statistically

Figure 5: Treatment impact on earnings and employment



Note: Main sample (zero employment in March-2021, non-Covid occupations). The dashed red line indicates the first intervention message.

significant, and cumulative benefit receipt follows the same pattern. This likely reflects two features of the benefit system. First, benefits are not reduced one-for-one with hours or earnings.[24] Second, over time many individuals exhaust eligibility and mechanically receive zero benefits, limiting the scope for treatment effects. Appendix Figure B5 shows substantial dispersion in remaining benefit duration. The latter is particularly relevant because Section 6.2.3 shows larger treatment effects among longer-duration unemployed, many of whom exhaust benefits during our observation window.

Figure 5 turns to earnings and employment. Monthly labor earnings show no

[24]Only when income exceeds 87.5% of the average monthly unemployment benefit for 2 months in a row will the unemployment benefit stop.

pre-treatment differences and become consistently positive after the intervention, with significance in most post-treatment months. The cumulative earnings effect also grows steadily, reaching about €1,100 by the end of 2022. Panels (c) and (d) show similar patterns for both employment measures: there are no pre-treatment differences, while post-treatment employment is consistently higher in the treatment group by roughly 2 to 3 percentage points. This holds both for any positive earnings and for the stricter threshold of earnings above €300.

The employment effects are of similar magnitude to the effects on hours and earnings, suggesting that the gains arise primarily on the extensive margin. Consistent with this, treatment has essentially no effect on hours conditional on working or on earnings conditional on working (Appendix Figure B6).

To gauge magnitudes, consider the control-group means 18 months after the intervention. Monthly hours worked are 66.1 in the control group, so an increase of 4.0 hours corresponds to 6.0%. Monthly labor earnings average €1,118, so an increase of roughly €72 corresponds to 6.4%. Employment based on positive earnings is 55.9% in the control group, so a 2.9-percentage-point increase corresponds to 5.2%. These effect sizes are similar across outcomes and sizable given the light-touch nature of the intervention and the likely partial take-up of its content. The fact that earnings rise at least as much as hours also suggests that the intervention did not shift job seekers into worse-paying jobs.

6.2.2 Treatment effects on occupational change

Our administrative data show that the intervention improved labor market outcomes, but they do not reveal the occupations in which job seekers found work. We therefore turn to the outflow survey to examine whether the treatment changed the occupation entered upon re-employment. The survey was sent to participants who, based on administrative records, experienced a substantial post-treatment increase in earnings (at least €300) and who subsequently confirmed in the survey that they had started a new job.[25]

Table 4 summarizes outflow-survey take-up. Of the 10,738 individuals in our main sample, 41% received an invitation. Treated individuals were somewhat more likely to be invited and, conditional on invitation, somewhat more likely to open the survey and report having started a new job. This pattern is consistent with the administrative evidence above, but it also means that occupational outcomes are observed for a slightly different selected sample in the treatment and

[25]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.

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	

Main sample (zero employment in March-2021, non-Covid occupations). Figures are aggregates across all outflow survey waves (see Table 1 for details).

control groups. The difference in response rates is modest, however: 31.2% in the treatment group (13.1/42.0) versus 29.0% in the control group (11.7/40.4), and too small to explain the qualitative occupational patterns we document below.[26]

Figure 6 shows whether the occupation in which a job was found matches the primary search occupation, one of the recommended alternatives, or an entirely different occupation. We present these results at the 4-digit, 3-digit, and 2-digit levels.[27,28]

At the 4-digit level, 21% of control-group job finders enter their primary search occupation, compared with 15% in the treatment group, a difference of 6 percentage points ($p = 0.02$). Because 4-digit occupations still treat closely related jobs as distinct, we also consider broader definitions of occupational mobility. At the 3-digit and 2-digit levels, the treatment-control difference rises to 11 and 12 percentage points, respectively, and remains strongly statistically significant. Appendix Table A12 reports the full set of statistics and tests.

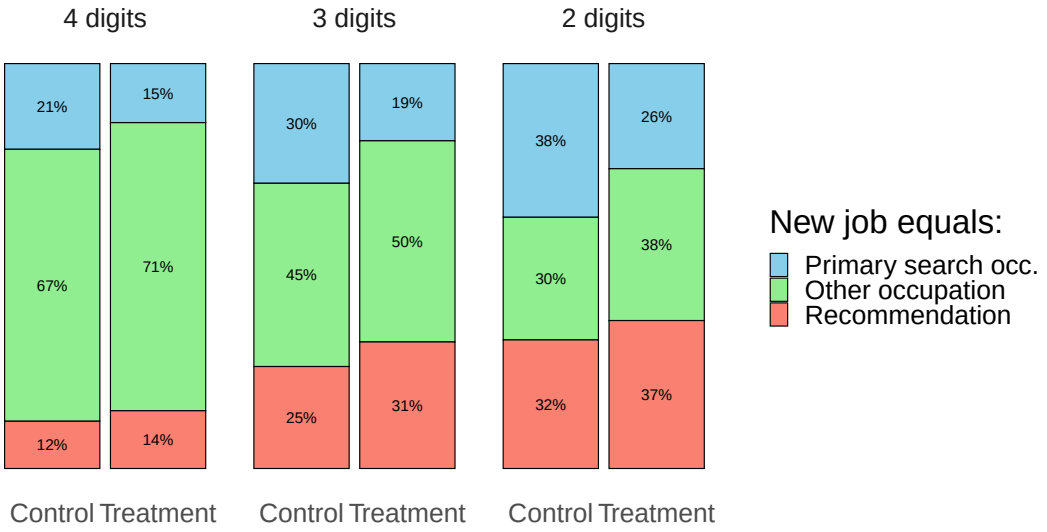
Where do treated job seekers go instead? Across aggregation levels, they are more likely to find work both in recommended occupations and in other occupations. At the 4-digit level, the increase in recommended occupations is small and insignificant (2 percentage points). At the 3-digit level, it rises to 6 percentage points and is marginally significant ($p = 0.06$); at the 2-digit level, the estimate

[26]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, suppose all of these additional respondents had reported an occupation different from their primary search occupation. Then the treatment-group share finding work in the primary search occupation (blue area in Figure 6, 4-digit panel) would be 15.7% rather than the observed 14.6% (reported as 15% in the figure due to rounding), still well below the 21% observed in the control group.

[27]The 4-digit codes refer to the "BRC+" classification level. While it technically has 5 digits, it is a Dutch modification of the ISCO 4-digit classification, and we therefore refer to it as a 4-digit code. It contains 700 distinct occupations.

[28]The survey asked for a free-text job title, which was blindly coded into a 4-digit occupational code. At coarser levels, the same broader occupation can contain both the 4-digit primary occupation and a 4-digit recommended occupation. In those cases, classification at the broader level follows the more granular classification for that individual.

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



Note: Main sample (zero employment in March-2021, non-Covid occupations). '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 (4 digit), 0.001 (3 digit) and 0.0005 (2 digit). The p-values for equal shares of Recommendation Occupations are 0.28 (4 digit), 0.06 (3 digit) and 0.16 (2 digit).

remains positive but is no longer statistically significant ($p = 0.16$).[29]

The framework in Section 4.5 suggests two mechanisms. Negative information about the primary occupation may redirect search more broadly toward alternatives (a push effect), while positive information about recommended occupations may disproportionately shift search toward those specific occupations (a pull effect). If the pull effect dominates, the share of recommended occupations should rise more, in relative terms, than the share of other occupations. At the 4-digit and 3-digit levels, the point estimates move in that direction: for example, at the 3-digit level the share of recommended occupations rises by 24%, compared with 10% for other occupations, although the difference is not statistically significant.[30] We do not see the same pattern at the 2-digit level.

Overall, the evidence is consistent with an important push component, though

[29]The increase in "other occupations" is statistically significant only at the 2-digit level.

[30]A formal test based on the log odds ratio does not reject equality of the relative changes.

not exclusively so. This interpretation should be made with some caution, because in the framework of Belot et al. (2019) workers learn separately about each occupation, whereas in practice positive information about recommended occupations may also signal that other alternatives are worth exploring. Our results therefore do not support providing negative information in isolation; what appears useful is negative information paired with clear evidence that better alternatives exist.

In summary, the evidence shows that the intervention not only improved labor market outcomes but did so in part by shifting job seekers toward different occupations.

6.2.3 Heterogeneity by unemployment duration and breadth of search

Following our pre-analysis plan, we examine heterogeneity along two dimensions for which prior work has found stronger impacts: unemployment duration and search breadth (Belot et al., 2019, 2026).

We split the sample at the median elapsed unemployment insurance duration at sample selection, which is 24 weeks. Figure 7 reports treatment effects on hours worked and employment. The results show clear heterogeneity. For long-term unemployed, effects on hours worked are immediate, sizable, and statistically significant (Panel a). For short-term unemployed, effects are also positive but much smaller and initially insignificant, though they increase toward the end of the observation period (Panel b). The same pattern appears for employment: treatment effects are large and significant for the long-term unemployed (Panel c), but smaller and initially insignificant for the short-term unemployed (Panel d).

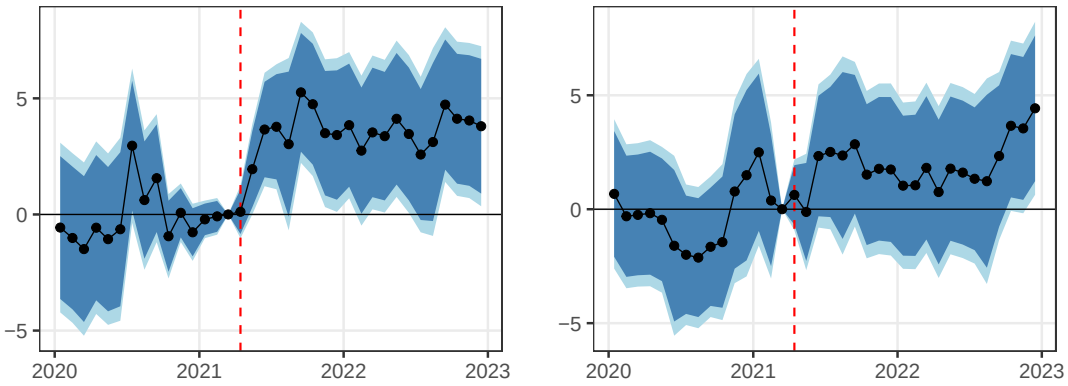
These results raise the question whether responsiveness reflects longer unemployment duration itself, or compositional differences between long- and short-term unemployed. Appendix Table A13 shows that the long-term unemployed are older and slightly less educated, characteristics one might have associated with less willingness to switch occupations. Other differences are modest. While only suggestive, this pattern supports the interpretation that longer unemployment duration itself increases responsiveness, likely because it strengthens both the willingness and the incentives to explore alternatives. Consistent with this, Table A14 shows that long-term unemployed are significantly more pessimistic about job finding prospects.

We next consider heterogeneity by initial breadth of search, which may proxy for openness to alternative occupations. Because our online search data have limited coverage (see Section 6.2.5), we cannot classify job seekers directly using

Figure 7: Heterogeneous treatment impact by unemployment duration

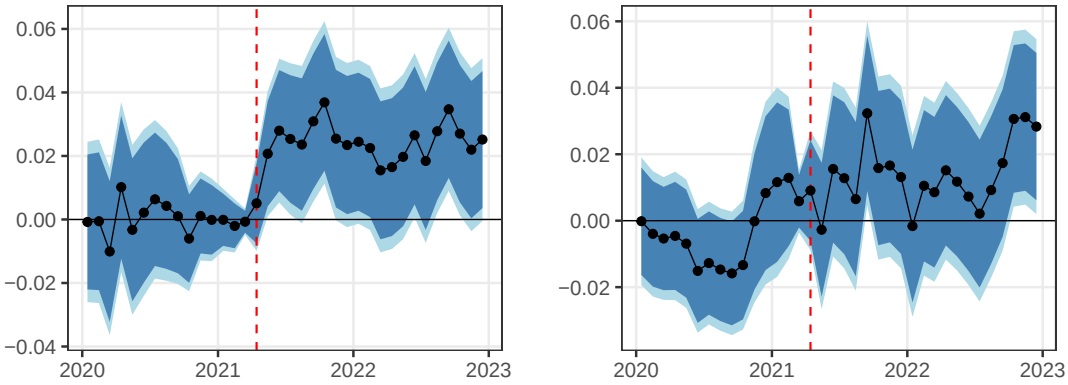
(a) Hours worked: long duration

(b) Hours worked: short duration



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

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



Note: Main sample (zero employment in March-2021, non-Covid occupations). 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.

observed search activity. Instead, we use the breadth of occupations listed in job seekers' online CVs.[31] Table A15 in the Appendix reports treatment effects 18 months after the intervention. The positive impacts are clearly concentrated among individuals who were already searching more broadly before treatment: for them, employment, hours worked, and labor earnings all increase significantly. For narrow searchers, estimates are smaller and mostly insignificant. This suggests that job seekers benefit more from the message when they are already somewhat

[31] Using the set of at most three occupations in the CV, we compute for each pair the number of overlapping digits in the occupational codes. The average across pairs provides an individual measure of breadth, following Belot et al. (2019). The sample is then split at the median.

open to alternative occupations. While this differs from Belot et al. (2019), the measures are not directly comparable: here, broad search means listing two or three occupations in the CV, whereas in their setting it refers to actively searching across a wide range of vacancies.

As pre-registered, we also examine heterogeneity by the survey measure of willingness to explore alternative occupations (Table A15 in the Appendix). Although the smaller survey sample limits precision, the pattern is similar: estimates are positive and large only for individuals who express such willingness, and negative for those who do not. Taken together, these findings suggest that an open orientation toward alternative occupations is an important determinant of whether job seekers benefit from labor market information.

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

We finally consider two groups outside our main analysis sample: job seekers searching in 'Covid occupations' and job seekers who still had paid employment at the time of the intervention. Table 5 reports treatment effects for these groups alongside the main-sample estimates as a benchmark.

For individuals in Covid occupations, estimated effects are smaller and statistically insignificant, with point estimates for cumulative hours and earnings that are roughly half as large as in the main sample. Two explanations are plausible. First, these job seekers may have been less willing to act on the advice because they expected demand in their original occupations to recover once Covid restrictions were lifted. Second, even if they did act on the advice and switched occupations, such switching may have yielded little additional benefit because labor demand in their original occupations recovered soon after. Appendix Figure B8 points more strongly to the first explanation: unlike in the main sample, treated individuals in Covid occupations do not appear more likely to end up in different occupations than those in the control group.

For job seekers who were already employed while receiving unemployment insurance, estimates are close to zero and should be interpreted with caution because this group exhibits pre-treatment differences in labor market outcomes. This suggests that registered job seekers who were already working were much less likely to incorporate the advice into their search behavior.

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)	(23.87)	(11.65)	(0.01)	(18.11)	(343.02)	(216.79)
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)	(20.42)	(8.62)	(0.01)	(18.81)	(282.31)	(166.61)
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
	(1.66)	(32.65)	(11.48)	(0.01)	(24.55)	(516.33)	(218.76)
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.

6.2.5 Secondary outcomes: effects on beliefs and job search

The positive effects on employment and occupational mobility suggest that job seekers adjusted their search behavior. To investigate this more directly, we examine secondary outcomes on job search and labor market beliefs using three complementary data sources.

Job search activities registered at the PES We first examine job search activities registered through the PES platform. Job seekers are required to record at least four activities every four weeks, although these activities are not limited to job applications and may also include orientation activities.[32] We observe these registrations between March 2021 and April 2022. This measure has broad coverage and relatively good reporting, but it contains no information about the occupations targeted, and reporting incentives beyond the required minimum may be weak. We therefore re-estimate equation 1 using the number of registered activities per month as the outcome.[33] Panel (a) of Figure B9 in the Appendix suggests a modest increase in registered search activity after the intervention:

[32]Specifically, alongside job applications, these activities can consist of network meetings and open inquiries with employers.

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

the treatment-control difference is near zero before the first message and positive afterwards, reaching about 0.1 activities relative to a control mean of 2.1 in March 2021.

Online job search Our second source is online search activity on the PES vacancy platform.[34] These data identify the occupations of viewed, saved, and applied vacancies, but coverage is limited, and likely selective, because platform use is not mandatory. We therefore restrict attention to individuals who used the platform at least once before the intervention and, because other activities are too sparse, focus on vacancy views between March and July 2021. Re-estimating equation 1 with monthly vacancy views as the outcome yields no clear treatment effects: Panels (b)–(d) of Figure B9 show no meaningful post-treatment changes in vacancy views overall or by occupation type.

Survey responses about job search and beliefs Finally, we use the pre- and post-intervention surveys to study self-reported job search and labor market beliefs. To account for baseline differences, we estimate a difference-in-differences model, which restricts the analysis to the subset of respondents who completed both surveys.[35] Table A16 in the Appendix reports treatment effects on time spent searching, applications, interviews, and the composition of the search set; none is statistically significant. Table A17 reports corresponding results for labor market beliefs, including expected job offer probabilities, job stability, and job finding, and again we find no significant treatment effects.

There are several possible explanations for the absence of detectable effects in these secondary outcomes despite clear effects on job finding. First, sample sizes are small, especially for the survey-based outcomes, limiting statistical precision. Second, survey response is selective, even though survey invitations were randomly assigned. Third, both search activities and beliefs are likely measured with error, which attenuates estimated effects. We therefore interpret the secondary-outcome evidence as suggestive rather than conclusive: there is some indication that the intervention increased search activity, but the available data do not allow a precise

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

[35] 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 in the post-intervention period.

account of how beliefs and search behavior changed.

7 Search Externalities and Violations of SUTVA

As the treatment group begins to search in new occupations, competition decreases in their original occupations but increases in the newly targeted ones. Such shifts in competition could alter overall labor market efficiency, directly affecting the control group. To be able to speak to the efficiency of our intervention and assess the degree of stable unit treatment value assumption (SUTVA) violations, we develop and calibrate a job search model in the Diamond-Mortensen-Pissarides tradition.[36] This section contains an intuitive discussion of this model, with technical details relegated to the Appendix. At its core, the model lets unemployed job seekers allocate search effort across three occupation groups that differ in labor market tightness. The treatment reallocates some search effort away from low-tightness occupations toward higher-tightness ones, which changes congestion in each market and thereby generates spillovers.

7.1 Model setup and calibration

The model considers three *occupation groups* indexed by G: selection occupations ($G = 1$), recommendation occupations ($G = 2$), and other occupations ($G = 3$). We follow the literature and assume a Cobb-Douglas meeting function, setting the elasticity to $\frac{2}{3}$.[37] Meeting probabilities depend on occupation-specific tightness, defined as $\theta_G = \frac{V_G}{U_G}$, where V_G is the number of vacancies in occupation group G and U_G is search-weighted unemployment in that group, i.e., the total amount of search directed toward occupations in G.

We group the unemployed into two sets indexed by $g \in \{E, O\}$, where $g = E$ denotes the job seekers eligible for our experiment and $g = O$ all other job seekers. Both groups can direct search effort to each occupation group G, with

[36] Kircher (2022) develops a theoretical approach for assessing potential externalities, though without calibrating their magnitude. Altmann et al. (2025) present a similar model, likewise without attempting to quantify the effects. Belot et al. (2019) provide a theoretical model of belief updating that guided our hypothesis about changes in search behavior in Section 4.5, but provide no quantification. In this section we take the changes in search behavior as given, and model the additional externalities that this generates.

[37] Petrongolo and Pissarides (2001, p. 393) report that elasticity estimates of total outflows from unemployment average around 0.7. Similarly, Broersma and Van Ours (1999) review studies of unemployment-to-employment flows, most of which fall between 0.6 and 0.8, with Dutch studies around 0.7. Studies that consider all hires or employment-to-employment transitions, typically yield smaller values. Because we focus on unemployment-to-employment transitions, we set the elasticity to two-thirds and examine robustness below.

effort denoted by s_{gG}. By construction, group E searches primarily in $G = 1$. Search in one's primary occupation is likely to be more effective, so we assign each group one unit of 'primary search'. For group E, this primary search is always in $G = 1$, while for group O it can lie in either $G = 2$ or $G = 3$. Meetings from primary search convert into matches with occupation-specific probability A_G, whereas non-primary search converts at probability $A_G P$, where $P < 1$ captures the lower effectiveness of searching outside one's primary occupation.[38]

We calibrate the model using pre-intervention statistics summarized in Table 6. The key pattern is that selection occupations ($G = 1$) are much less tight than recommendation occupations ($G = 2$), while other occupations ($G = 3$) lie in between. Consistent with our sample construction, group E directs most search to $G = 1$, whereas group O searches mainly in $G = 2$ and $G = 3$. Primary search is therefore assigned fully to $G = 1$ for group E, and split between $G = 2$ and $G = 3$ for group O.

We do not observe A_G and P, so we calibrate them to match observed job finding outcomes. For $g = E$, we target the 18-month employment rate in the control group together with the observed distribution of job findings across occupation groups, which implies monthly job finding probabilities of $F_{E1} = 1.4\%$, $F_{E2} = 0.9\%$, and $F_{E3} = 2.3\%$. For $g = O$, we target the overall job finding probability $F_O = 9.1\%$ from public data. These moments identify $A_1 = 3.4\%$, $A_2 = 5.9\%$, $A_3 = 6.6\%$, and a non-primary search penalty parameter of $P = 0.72$.

The estimates imply that matching is least efficient in $G = 1$, even though differences in matching efficiency are smaller than differences in tightness. The estimate for P implies that search outside one's primary occupation is about 30% less effective, which is, reassuringly, close to the experimental evidence in Behaghel et al. (2024).

We then introduce the treatment by converting a fraction of group $g = E$ into a treated group $g = T$ with a different search allocation.[39] We calibrate the treated search vector using two targets: the experimental increase in employment after 18 months (approximately 2.9p.p.) and the distribution of jobs found across occupation groups in the treatment-group outflow survey. The implied search intensities ($s_{T1} = 0.93$, $s_{T2} = 0.38$, $s_{T3} = 0.87$) indicate that treated job seekers reduce search in $G = 1$ and shift it toward the tighter occupation groups $G = 2$

[38] An alternative modeling approach would be to penalize the likelihood of meetings. However, this would strictly improve efficiency compared to our current setup, as this reduces meeting congestion.

[39] We take this change in search behavior as given and model its spillovers, while abstracting from the underlying belief-updating process that generates it (see, e.g., Belot et al. (2019)).

Table 6: Key parameters and values (pre-treatment)

	Occupation group:		
	G = 1 (Selection)	G = 2 (Recommendations)	G = 3 (Other)
Tightness (θ_G)	3.6%	52.1%	27.4%
Search effort g = O (s_{OG})	0.21	0.60	1.81
Search effort g = E (s_{EG})	1.31	0.27	0.73
Primary search g = O (ρ_{OG})	0	0.46	0.54
Primary search g = E (ρ_{EG})	1	0	0
Job finding rate g = O (F_O)	———	9.1% ———	
Job finding rate g = E (F_{EG})	1.4%	0.9%	2.3%
Matching efficiency (A_G)	3.4%	5.9%	6.6%

Notes: The table reports key parameters by occupation group (G), used for *pre-treatment* calibration of the model. Tightness (θ_G) is defined as the number of vacancies in occupation (V_G) divided by the number of times job seekers list an occupation from this group as one of the (up to three) occupations they are interested in (U_G). Both are obtained from the 'Open Match Data' (OMD), provided by the PES (measured just before the start of the intervention). Search effort s for the study participants (g = E) is measured from our pre-intervention survey: each time an occupation in G is listed, it counts as one unit of search intensity in that occupation. Listings are capped to 3. Search effort for other job seekers (g=O) is obtained from OMD, after subtracting all s_{EG} from the aggregate search intensity in occupation G. Primary search (ρ) for group g = E is assumed to be fully targeted to G = 1 (as they were selected on the basis of searching for one of the selection occupations). Primary search for group g = O is divided across occupational groups 2 and 3, based on unemployment insurance receipts by occupational group. The job finding rates are observed in our study (for g = E) and in a separate dataset on unemployment-insurance outflows by occupation (for g = O, averaged over the observation period of our experimental group and scaled by the average conversion to work; see Appendix D for details). Matching efficiencies are pinned down by the model based on the above parameters.

and $G = 3$.

We also have to take a stance on vacancy creation and their survival. For vacancy creation, we consider two possibilities: vacancies are created according to free entry, or firms always post the same number of vacancies. For survival, we consider that vacancies can be short-lived (i.e., need to be re-posted every period) or long-lived (i.e., persist with the same survival probability $1-\delta$ as normal jobs). It turns out that efficiency is lowest under short-lived vacancies without free entry, which is why we report it here. Robustness checks for alternative assumptions are relegated to Appendix D.

7.2 Simulation results

We now use the calibrated model to answer two questions. First, how much do spillovers affect the estimate of our treatment effect? Second, how much of the gross employment gain in the experimental sample remains after accounting for employment losses and gains among other job seekers?

The first row in Table 7 simulates our intervention: 67% of the unemployed in group $g = E$ get treated (and thus move to $g = T$), and nobody else. As one might expect, the reallocation generates positive externalities for job seekers in both the control and treatment groups, as the treatment leads to less search in $G = 1$. Compared to the no externalities condition, the increase in employment is 0.11p.p. for the control group and 0.06p.p. for the treatment group. The positive externality for the treatment group is slightly smaller, as they no longer search as much in $G = 1$ as the control group. Our original estimate of a 2.9p.p. narrows by 0.05 p.p. (columns 6-7), constituting only a minimal SUTVA violation.

Job seekers who are not part of the experiment suffer, as the treated job seekers increase competition for jobs in $G = 2$ and $G = 3$. While the impact at the individual level is minimal for this group (a reduction of 0.02p.p., column 5), the size of this group means that a little over a third of the jobs gained in the control and treated groups come at the expense of jobs in this group. The total efficiency of the treatment (column 8), defined as the ratio of jobs created with moving tightness over jobs created under fixed tightness, ends up at 65%.[40] When rolling out the experiment at scale, employment offices will likely want to inform all job seekers in low-tightness occupations. We can simulate this in our model by increasing the share of unemployed job seekers treated at the time of our intervention (row 2), or additionally treating job seekers who become unemployed over the 18-month period of our treatment (row 3). We find that efficiency remains high.

7.3 Larger scale roll-outs and sensitivity to tightness

Our model also allows us to speak to the effectiveness of our intervention in alternative initial labor markets. We are particularly interested in two alternative scenarios. First, a scenario in which job seekers in occupations with poor prospects

[40] Appendix Table D1 shows a number of robustness checks. First, we show results when we change our assumptions on entry and survival of vacancies and show that it increases efficiency. Free entry of vacancies is a particularly realistic assumption for the long-term, as firms will likely adjust to the different search behavior. Second, we show the impact of reducing the elasticity parameter to 0.5. Efficiency also remains positive then.

Table 7: Spillovers and efficiency under alternative treatment scales

Scenario	Roll-out	Spillover effect on employment after 18 months (pp)			Treatment - control gap (pp)		Efficiency
		$g = E$	$g = T$	$g = O$	fixed θ	moving θ	
Baseline experiment	67%, one-off	0.11	0.06	-0.02	2.9	2.85	0.65
Larger one-off roll-out	99%, one-off	0.16	0.09	-0.03	2.9	2.82	0.64
Continuous roll-out	99%, repeated	0.23	0.12	-0.04	2.9	2.79	0.61

Notes: Columns 3-5 show how the employment rate of each group changes when tightness is allowed to adjust in the model (as compared to a scenario where tightness remains fixed). Columns 6-7 quantify the SUTVA violation, which is the change in the experimental treatment effect when tightness is allowed to adjust. Efficiency in column 8 is defined as the ratio of jobs created with moving tightness over jobs created under fixed tightness.

represent a much larger share of the unemployed population, representing e.g. larger labor demand reductions due to technological change, which possibly increases congestion concerns when everyone is treated. Second, a scenario where the difference in tightness between selection occupations and transition occupations is smaller, potentially reducing the treatment's effectiveness. Below, we detail the main findings, with a more detailed exposition relegated to Appendix D.4.

Panels (a) and (b) of Figure D1 respectively show the overall and per-person impact of our treatment on jobs 'created' at different $g = E$ shares. Panel (a) shows that the impact of our treatment is de facto linear in the number of individuals treated, which suggests that congestion is not a major threat.[41] The reason for this is that tightnesses are highly dispersed initially, and since our treatment has only a moderate impact on search intensities it does not meaningfully close the gap. The largest relative jump is in $G = 2$, where tightness only barely drops below 0.4 even when $g = E$ makes up 99% of the unemployed; still over seven times as high as tightness in $G = 1$ (see Figure D2). In addition, the moderate amount of congestion caused in $G = 2$ and $G = 3$ is partly compensated by an increase in tightness in $G = 1$.

The results from Figure D1 raise the question what the maximum level of $G = 1$ tightness is under which the treatment is still be effective (assuming initial tightnesses in $G = 2$ and $G = 3$ do not change). Figure D3 shows this by share $g = E$. Two findings are interesting. First, the maximum tightness under which

[41] Panel (b) shows that the line in panel (a) is not truly linear. The marginal benefits (costs) from search reduction (increase) in $G = 1$ ($G = 2$ and $G = 3$) change as share $g = E$ grows. Initially, expanding the size of eligible job seekers therefore gains over-proportionally from freeing up vacancies in the congested occupation. Eventually as the share $g = E$ grows, this changes as vacancy groups $G = 2$ and $G = 3$ become congested more rapidly and benefits even for eligible job seekers $g = E$ who direct their search there decline. The non-monotonicity is relatively minor in magnitude, though, and the effect size per job seeker remains relatively stable.

the treatment is effective lands well above the true value of approximately 0.036 at every share $g = E$, suggesting that the group of occupations could be significantly expanded, conditional on there being enough transition occupations.[42] Second, the maximum $G = 1$ initial tightness is mostly *increasing* in share $g = E$. The reason is that at higher share $g = E$, the negative congestion externality in vacancy groups $G = 2$ and $G = 3$ is imposed on a smaller share of other job seekers $g = O$ while the positive decongestion in vacancy group $G = 1$ benefits a larger share of eligible job seekers $g = E$. The maximum non-detrimental $G = 1$ tightness drops slightly at the tail end of the graph for the more obvious reason: the congestion in the "good" occupations increases too fast due to the increase in search intensity of the very large treated group, so the maximal non-detrimental tightness difference slightly drops.

These results suggest that, as long as tightnesses are sufficiently dispersed across occupations and it is possible to find suitable transitions for job seekers, a large group of job seekers can be helped by an information treatment similar to ours. In contrast to Altmann et al. (2025), our method of scaling in this calibration focuses on providing information to (close to) all job seekers primarily looking for work in a specific occupation, rather than randomizing treatment assignment. Such a method of scaling may be more palatable to PES, as they may be opposed to or unable to randomly assign information to similar types of job seekers.

8 Conclusion

Changing labor demand across occupations can create some occupations with 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-seeker ratios are not averse to considering alternatives. On the contrary, they are

[42] Over 50% of total job search intensity is concentrated in occupations that have a tightness below the threshold.

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 find suggestive evidence 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 to all unemployed job seekers in occupations with poor prospects is warranted.

A key advantage of our intervention from a policy perspective is that it is easy to implement. It requires two things that are readily available or can be easily generated. First, it requires information about the labor market tightness by occupation, which likely is a basic piece of information at PES. Second, it requires that information on recent occupational trajectories of job seekers be available, which is the case in many countries around the world. Such information reflects the transferability of skills across occupations. If unavailable, such information could easily be constructed using skills measures of occupations (e.g., Gathmann and

Schönberg, 2010). Such information 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 PES in helping job seekers in slack labor markets.

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Appendix (for online publication)

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

Coefficients report differences relative to the selection occupation category. Educational attainment is similar across groups in the different categories of occupations. This fact is encouraging, as it suggests that lack of educational attainment is not a barrier to transitions. With regard 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. Moreover, job seekers in our selection occupations are younger on average, and less likely to be male than in transition and other occupations. Constant represents the mean for the selection group. Standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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

Coefficients report differences relative to the selection occupation category. Results from this Table are broadly similar to Table A2. Additionally, it 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. Constant represents the mean for the selection group. Standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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

Table A5: Balance table main analysis sample: 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	

Main sample (zero employment in March-2021, non-Covid occupations). 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	

Main sample (zero employment in March-2021, non-Covid occupations). This table is based on the individuals that received the survey invitations. 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	

Main sample (zero employment in March-2021, non-Covid occupations). 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

Main sample (zero employment in March-2021, non-Covid occupations). '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 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	

Main sample (zero employment in March-2021, non-Covid occupations). '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: Occupations of new jobs

	Control	Treatment	P-value
Primary occ. (4-digit)	21%	15%	0.021
Primary occ. (3-digit)	30%	19%	0.001
Primary occ. (2-digit)	38%	26%	<0.001
Primary occ. (1-digit)	45%	34%	0.003
Recommendation (4 digit)	12%	14%	0.28
Recommendation (3 digit)	25%	31%	0.060
Recommendation (2 digit)	32%	37%	0.16
Recommendation (1 digit)	40%	47%	0.070
Other occ. (4-digit)	67%	71%	0.24
Other occ. (3-digit)	45%	50%	0.22
Other occ. (2-digit)	30%	38%	0.034
Other occ. (1-digit)	15%	19%	0.10
Observations	274	624	

Note: this table shows the occupational groups in which job seekers found jobs (based on the outflow survey responses).The same statistics are displayed in Figure 6. The p-values reflect tests comparing the control and treatment percentages.

Table A13: 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	

Main sample (zero employment in March-2021, non-Covid occupations). P-values refer to tests for equality of the short and long UI duration columns. Remaining benefits and unemployment duration are measured in March 2021.

Table A14: 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	

Main sample (zero employment in March-2021, non-Covid occupations). P-values refer to tests for equality of the short and long UI duration columns. Remaining benefits and unemployment duration are measured in March 2021.

Table A15: Heterogeneous treatment effects: initial breadth of search and willingness to consider alternative occupations (survey)

	Narrow searchers	Broad searchers	Willing to explore	Unwilling to explore
Monthly hours worked	2.989 (1.849)	4.636*** (1.750)	9.810 (5.961)	-12.16** (5.340)
Monthly labour earnings	66.01* (33.84)	73.55** (33.35)	221.87* (113.39)	-215.83** (99.42)
Monthly UI benefits received	-24.05 (18.25)	15.97 (14.96)	11.54 (62.32)	80.00 (62.32)
Labour earnings above 0	0.023 (0.014)	0.033** (0.013)	0.054 (0.043)	-0.053 (0.040)
Observations	4880	5858	485	555

Each cell contains a treatment effect estimate from a separate regression that controls for demographics. All outcomes are measured 18 months after the intervention. Robust standard errors in parentheses. Samples vary across columns due to dependence on survey responses.

Table A16: 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
	(0.636)	(1.549)	(0.361)	(0.144)	(0.108)	(0.050)
Post	−0.956*	1.024	0.067	−0.257**	0.207**	0.053
	(0.541)	(1.668)	(0.197)	(0.129)	(0.090)	(0.050)
Treatment*Post	−0.354	−1.783	−0.088	−0.073	−0.164	−0.058
	(0.656)	(1.887)	(0.384)	(0.157)	(0.110)	(0.061)
Constant	5.801***	6.738***	0.533***	2.662***	3.806***	0.183***
	(0.525)	(1.264)	(0.189)	(0.117)	(0.088)	(0.039)
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. Main sample (zero employment in March-2021, non-Covid occupations). 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). Standard errors are clustered by individuals.

Table A17: 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**
	(0.038)	(0.080)	(0.033)	(0.041)	(0.033)
Post	0.122**	0.085	−0.010	−0.004	0.092***
	(0.058)	(0.091)	(0.024)	(0.031)	(0.030)
Treatment*Post	−0.048	−0.086	0.003	0.020	−0.017
	(0.073)	(0.114)	(0.029)	(0.037)	(0.035)
Constant	0.101***	0.162**	0.657***	0.660***	0.258***
	(0.033)	(0.066)	(0.027)	(0.034)	(0.026)
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. Main sample (zero employment in March-2021, non-Covid occupations). The dependent variables are: expected job offer rate per application in the primary search occupation (Column 1) and in the first alternative occupation (Column 2), the expected stability of a job found in the primary search occupation (Column 3) and in the first alternative occupation (Column 4), and the job seeker‘s expected overall job finding probability (Column 5). Standard errors are clustered by individuals.

B Additional Figures

Figure B1: Unemployment and vacancies in the Netherlands

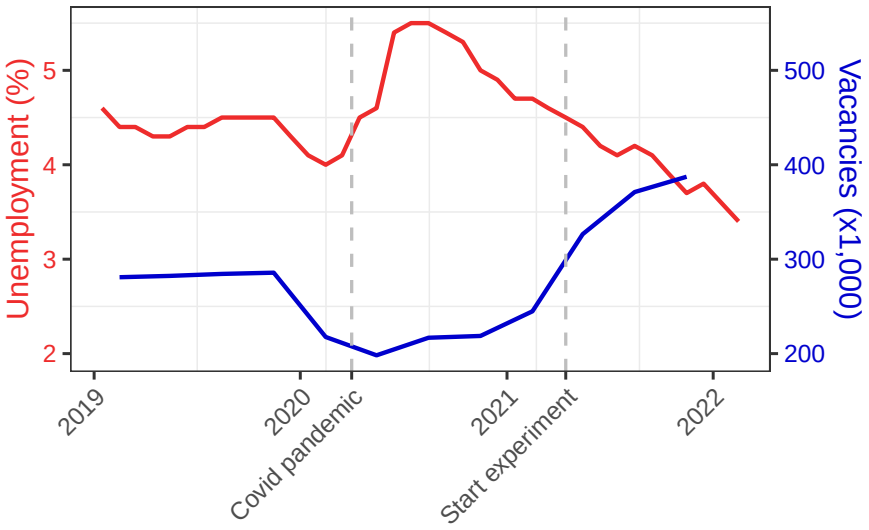


Figure B2: Job prospects of Covid and non-Covid occupations

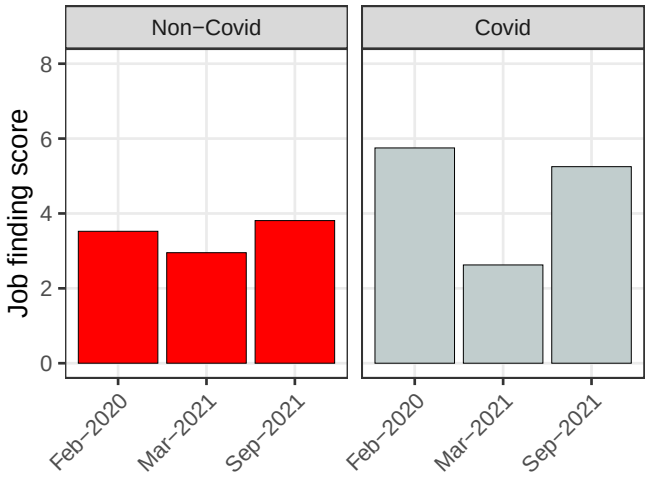


Figure B3: Number of search occupations

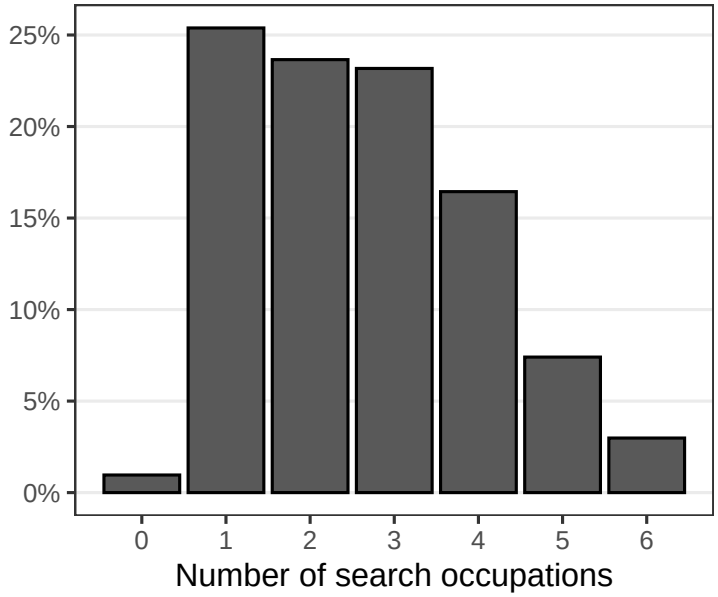


Figure B4: The number of suggested search occupations (form our intervention) that were already part of job seekers' initial set of search occupations

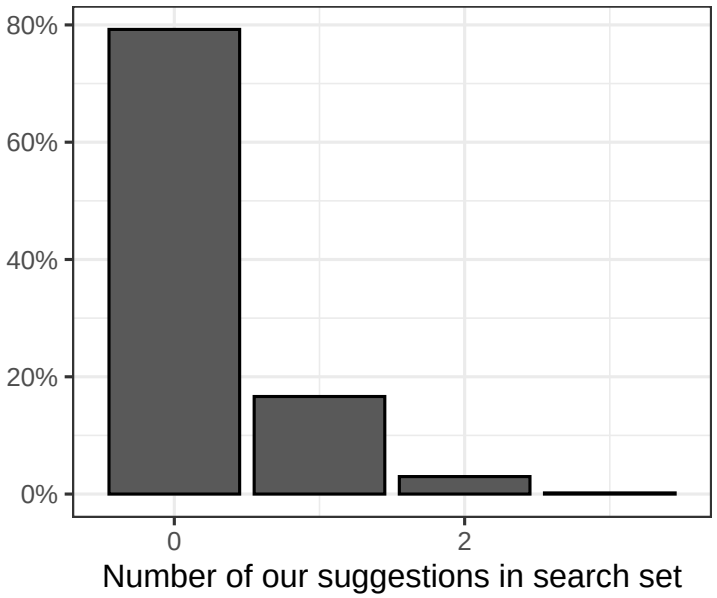


Figure B5: Cumulative distribution of remaining UI eligibility at the time of the intervention (main sample).

(a) Histogram (b) Cumulative density

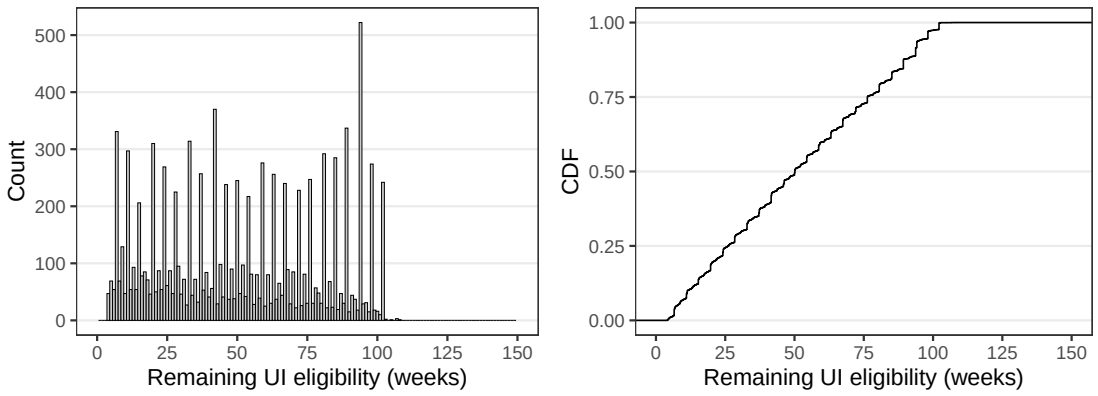
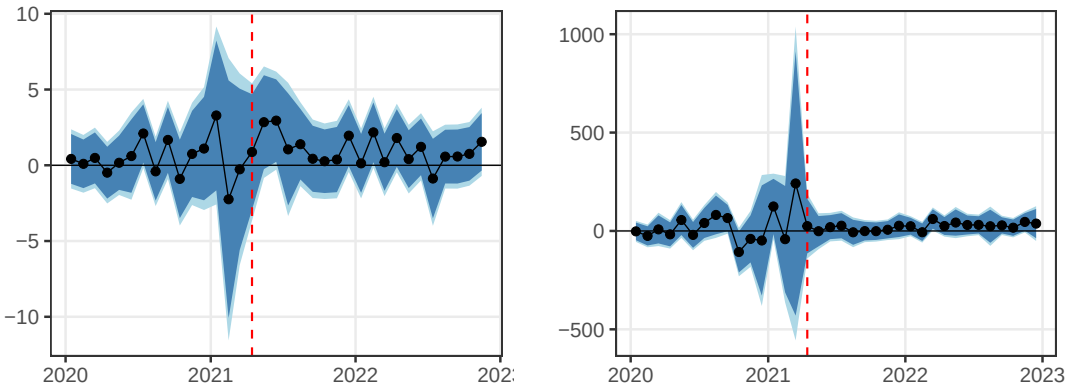


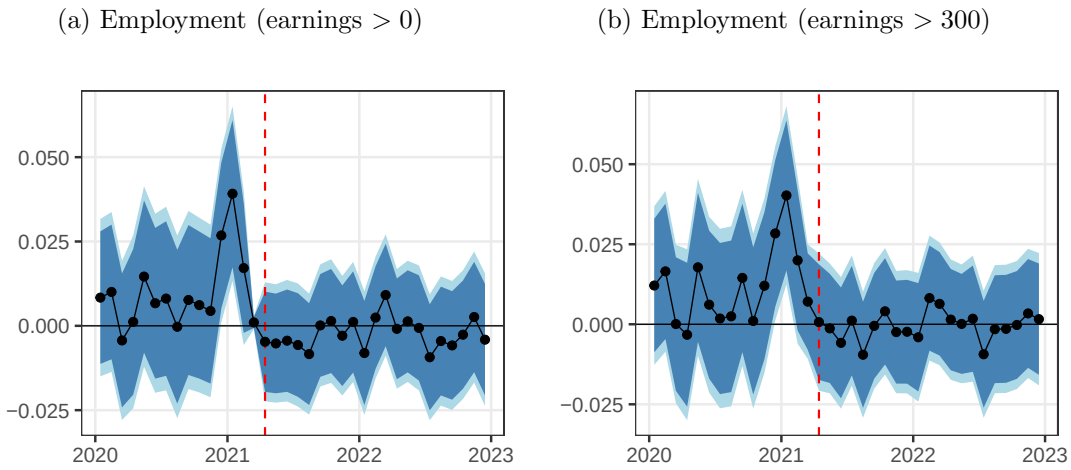
Figure B6: Intensive margin effects: treatment impacts conditional on employment

(a) Hours worked, conditional (b) Labor earnings, conditional



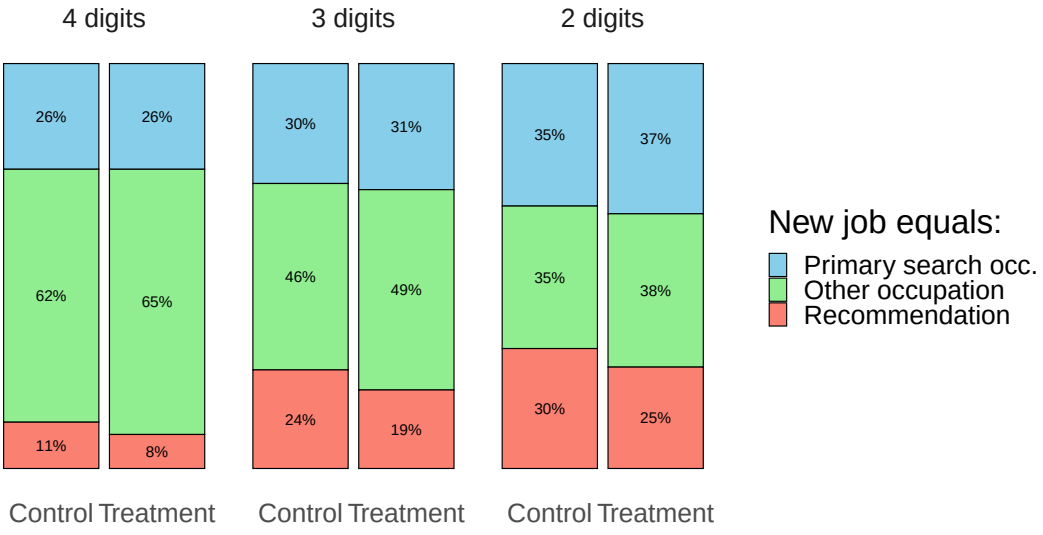
Note: The left panel considers only observations where the individual has positive working hours. The right panel considers only observations where the individual has positive labor earnings. Main sample (zero employment in March-2021, non-Covid occupations). The dashed red line indicates the first intervention 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 B7: Employment effects for job seekers working positive hours before treatment



Note: Sample with positive working hours in March-2021 ('partially unemployed'), non-Covid occupations. The dashed red line indicates the first intervention message.

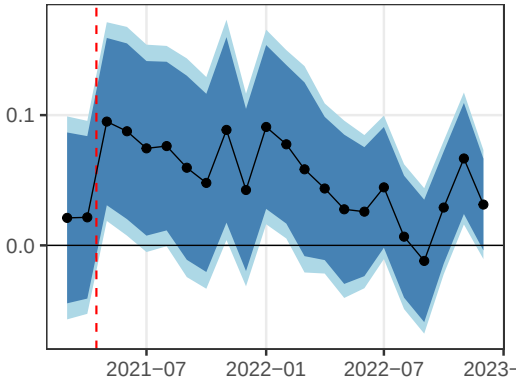
Figure B8: Comparison of occupations of new jobs with the primary search occupations and the set of recommendations (at different occupational digit levels): only individuals from Covid occupations.



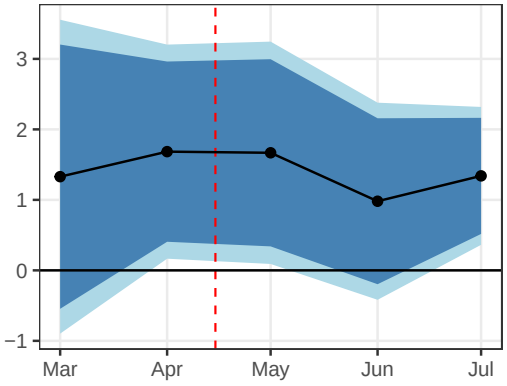
Note: 'Primary search occ' report the share of individuals who found work in the same occupation as their initial 'primary occupation of search' (registered at time of registration for UI benefits). 'Recommendation' reports the share of individuals who found work in one of the occupations recommended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

Figure B9: Treatment impact on monthly job search (treatment-control differences): registered activities and online viewed vacancies

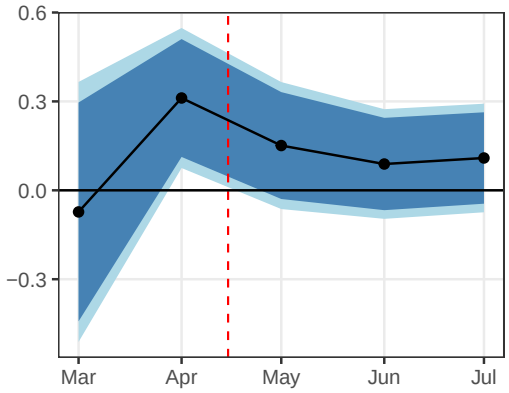
(a) Registered activities



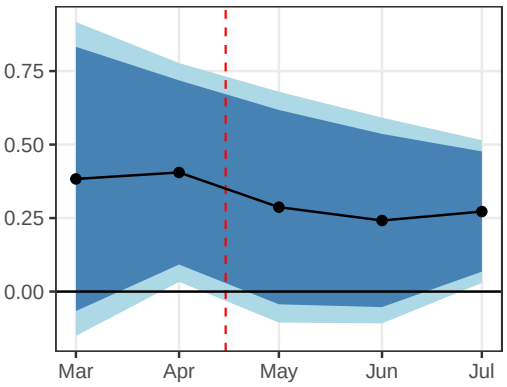
(b) Online search: viewed vacancies



(c) Online search: viewed vacancies in selection occupation



(d) Online search: viewed vacancies in recommended occupations



Note: Main sample (zero employment in March-2021, non-Covid occupations). Panel (a) excludes participants once they reach 40 working hours (as a proxy for having left job search). Panels (b)-(d) include only individuals that searched online at least once prior to the intervention. The dashed red line indicates the first intervention message.

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.

D Additional Calibration Details

In this appendix we provide further details on the model and particularly on the calibration and sources of data used.

D.1 Datasets

We use several sources released by the Dutch employment office, as well as data from our experiment, to discipline our calibration in Section 7. We use (i) 'Open Match Data' published by the Dutch employment 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 sep-

arate dataset by the Dutch employment office (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 also use this data to obtain average monthly outflow rates for the duration of our observation period (18 months). To this end, 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 (v) data from Statistics Netherlands regarding the size of the overall labor force in April 2021 (Statistics Netherlands, 2021).

D.2 Pre-treatment calibration

Our matching function (for each occupation group G) is $M_G(U_G, V_G) = A_G U_G^{\alpha} V_G^{1-\alpha}$ with $\alpha = 2/3$ as our benchmark. We obtain vacancy data V_G from source (i) by occupation group, see first three rows in Table D2. We consider each occupational listing by job seekers as a unit of search intensity. Search intensity of our experimental group ($g = E$) is therefore known. We observe $s_{E1} = 1.31, s_{E2} = 0.27, s_{E3} = 0.73$. Search intensity for jobs in G by "other" individuals $g = O$ equals total search intensity in the market minus the total search by group $g = E$ averaged across individuals in $g = O$: $s_{OG} = (U_G - s_{EG}u_E)/u_O$. This gives $s_{O1} = 0.21$, $s_{O2} = 0.60$, $s_{O3} = 1.81$. So the declared interest in a particular vacancy group in source (i) identifies search-weighted-unemployment U_G per occupation. See rows 4-6 in Table D2 (and rows 16-18 for the values of all s_{EG}). Together, these immediately give pre-intervention market tightnesses $\theta_G = V_G/U_G$: $\theta_1 = 3.6\%, \theta_2 = 52.1\%, \theta_3 = 27.4\%$.

As is standard in the literature, the probability that a unit of search intensity in occupation G leads to a meeting is given by increasing function $m(\theta_{Gt})$ which is directly derived from the matching function.[43] Similarly, pairwise meetings directly imply that the probability for a vacancy in G to meet a worker is given

[43]Subscript t denotes time, which we need when considering out-of-steady-state dynamics below.

by decreasing function $q(\theta_{Gt}) = m(\theta_{Gt})/\theta_{Gt}$. We assume that existing matches get destroyed with probability δ.

To ease derivations we denote by B_{gG} the probability that a meeting between worker in group $g \in \{E, O\}$ and a vacancy in occupation G converts into a match. For $(g, G) \in \{(E, 2), (E, 3), (O, 1)\}$, we parametrize $B_{gG} = PA_G$ so the penalty $P < 1$ applies when there is no primary occupation search effort. We specify $B_{E1} = \left(\frac{s_{E1}-1}{s_{E1}}P + \frac{1}{s_{E1}}\right) A_1$. Essentially, we only allow for one unit of search effort (i.e., listed occupation) to be primary. The rest of the search effort of $g = E$ in $G = 1$ is considered non-primary. Similarly, for group $g = O$ we specify $B_{OG} = \left(\frac{s_{OG}-\rho_{OG}}{s_{OG}}P + \frac{\rho_{OG}}{s_{OG}}\right) A_G$, 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 call C_G the probability that a random meeting in occupation G converts into a match. That is, B_{gG} is the probability of conversion conditional on job seeker type g, and C_G is the unconditional probability. If all search is primary, these equal matching efficiency A_G. If no search is primary, then these equal PA_G because of the penalty for search that is not primary. In general, it is a weighted average of these, depending on how much search is primary.

We calibrate parameters A_G and P to jointly match the job finding probability of control group individuals per occupational group. We measure this probability from the product of control group total job finding probability (F_E) and the share of new jobs in occupation group G (H_{EG}):

$$B_{EG} s_{EG} m(\theta_G) \;=\; H_{EG} F_E \text{ for } G \in \{1, 2, 3\} \tag{3}$$

For other job seekers ($g = O$) we only observe the total job finding rate F_O:

$$\sum\nolimits_G B_{OG} s_{OG} m(\theta_G) \;=\; F_O. \tag{4}$$

We set the number of unemployed individuals in group $g = E$ equal to the number of individuals in our experiment in non-Covid occupations (row 7). The number of all "other" individuals in group $g = O$ is set equal to the number of all other job seekers in source (ii) minus u_E (row 8). We obtain the job finding rate of the experimental group by targeting the employment rate of the control group after 18 months (55.9%). See row 9. The exit survey in (iv) identifies the fraction H_G of jobs found per occupational group G by job seekers in group $g = E$. This is similar to Figure 6, but there we used only the selection and recommendation occupations for the particular job seeker in question, while here

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.This ties down $F_{E1} = 1.4\%, F_{E2} = 0.9\%, F_{E3} = 2.3\%$. For the job finding F_O 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: $F_O = 9.1\%$. We use source (ii) to identify the fraction ρ_{OG} of "other" workers who have their primary occupation in occupational group G. See rows 11 and 12: $\rho_{O2} = 0.46 = 1 - \rho_{O3}$. This yields $A_1 = 3.4\%, A_2 = 5.9\%, A_3 = 6.6\%, P = 72\%$.

To evaluate out-of-steady-state treatment effects, we now embed these calibrated objects in a dynamic system for employment and vacancies. We define the evolution of vacancies and the labor force as follows and subsequently consider steady state values. At a given time t, the number of vacancies ($V_{G,t}$) and the number of unemployed $u_{g,t}$ determine tightness as well as future employment $e_{gG,t+1}$ of group g in occupation G.

$$\theta_{G,t} = V_{G,t}/(\sum_g s_{gG} u_{g,t}) \tag{5}$$

$$e_{gG,t+1} = B_{gG} s_{gG} m(\theta_{G,t}) u_{g,t} + (1-\delta) e_{gG,t} \tag{6}$$

with δ the job separation rate. The remainder remains unemployed: $u_{g,t+1} = \bar{e}_g - \sum_G e_{gG,t+1}$, where $\bar{e}_g$ is the total size of worker group g across both employment and unemployment (the labor force). Equation (6) states that employed workers remain employed at rate $(1-\delta)$ and unemployed workers find jobs at rate $B_{gG} s_{gG} m(\theta_{G,t})$. Vacancies evolve according to

$$V_{G,t+1} = V_{G,t}\left[1 - C_{G,t} q(\theta_{G,t})\right][1-\psi] + \mu_{G,t+1}, \tag{7}$$

where $\mu_{G,t+1}$ is the vacancy creation for each vacancy group and ψ is the probability that an unfilled vacancy is destroyed. Vacancies next period comprise unfilled vacancies this period that did not get destroyed, as well as newly created vacancies.

For our pre-treatment calibration we consider steady-state values. To identify

$\bar{e}_g$, we use the steady-state version of equation (6):

$$e_{gG} = \frac{B_{gG} s_{gG} m\left(\theta_G\right) u_g}{\delta},$$

All values on the right side are known, so that $\bar{e}_g = \sum_G e_{gG} + u_g$ and therefore $\bar{e}_E \approx 240,633$ and $\bar{e}_O \approx 9,357,367$. In steady state the number of employed people who lose their job equals the number of people who find jobs, so that $\sum_g (\bar{e}_g - u_g)\delta = \sum_g u_g F_g$ which gives $\delta \approx 0.2\%$. We can then identify μ_G via the steady-state version of (7) as

$$\mu_G = V_G - V_G \left[1 - C_G q\left(\theta_G\right)\right](1 - \psi),$$

where we need to use the θ_G that we already identified earlier and $C_{G,t} := \sum_g \left(B_{gG} s_{gG} u_g\right) / \sum_g \left(s_{gG} u_g\right)$.

D.3 Evaluation of SUTVA violations and externalities

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_{TG} of the treatment group such that we match the fraction that find a job in each occupational category, and that the employment rate of control group individuals is roughly 2.9 percentage points lower than that of a treatment group individual after 18 months (see Table D3). This gives $s_{T1} = 0.93$, $s_{T2} = 0.38$, and $s_{T3} = 0.87$.

Since this considers an out-of-steady-state scenario, we need to take a stance on whether vacancies are short-lived (if unfilled they disappear with probability $\psi = 1$ so that vacancies need to be re-posted every period) or are long-lived (unfilled vacancies survive at the same probability as jobs so that $\psi = \delta$). We also need to take a stance on the inflow of vacancies μ_{Gt}: it is either fixed at pre-intervention levels (e.g., if employers are not aware of the treatment or inflow is not affected for other reasons) or determined by free entry (assuming posting cost K_G, output y_G, Hall–Milgrom bargaining with bargaining weight γ_G, discounting at factor β). Under free entry, the value of posting a vacancy satisfies

$$K_G = q(\theta_{G,t+1})\, C_G\, Y_G, \quad \text{where } Y_G = \gamma_G \frac{y_G}{1 - \beta(1 - \delta)}.$$

Moreover, $U_{G,t+1}$ is determined by $\theta_{G,t}$, so $\theta_{G,t+1}$ pins down V_{t+1} and thus entry.

Table D1: Assessment of general equilibrium effects and SUTVA violations

Scenario	Roll-out	Spillover effect on employment after 18 months (pp)			Treatment - control gap (pp)		Efficiency
		$g = E$	$g = T$	$g = O$	fixed θ	moving θ	
Baseline (Fixed entry, short-lived)	67%, one-off	0.11	0.06	-0.02	2.9	2.85	0.65
Free entry, long-lived	67%, one-off	-0.06	-0.04	-0.01	2.9	2.92	0.76
Free entry, short-lived	67%, one-off	-0.06	-0.04	-0.01	2.9	2.92	0.76
Fixed entry, long-lived	67%, one-off	0.28	0.17	-0.02	2.9	2.78	0.74
Low α (0.5)	67%, one-off	0.16	0.08	-0.03	2.9	2.82	0.37

Note: all values in columns 3–7 are percentage points (pp). Efficiency in column 8 is defined as the ratio of jobs created under moving tightness over jobs created under fixed tightness.

It turns out that short-lived vacancies and fixed entry is the most conservative assumption in terms of efficiency and we use it in the baseline calibration.

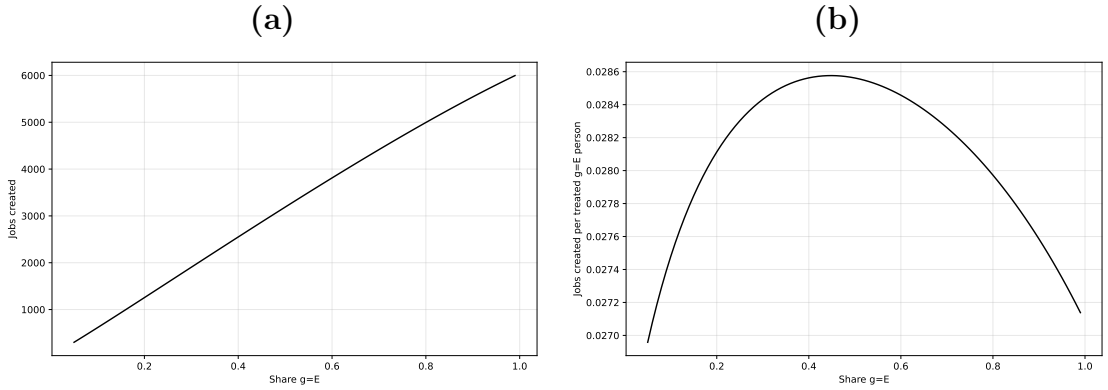
From here, we can assess the degree of SUTVA violation and its impact on efficiency as reported in Table 7 and D1. A note on the second and third row of Table D1 is warranted. Under free entry, all three groups ($g = E$, $g = T$, and $g = O$) suffer from the intervention. The reason for this is that the share of search that is penalized increases following the treatment, reducing the attractiveness of posting a vacancy conditional on total search intensity in the occupation since conversion probabilities conditional on meeting decrease. The reason efficiency is higher than in the fixed entry case is that the other group does not suffer as much as before. The reason for this is that from the firms' perspective, the reduced attractiveness conditional on search intensity is compensated by the increased search intensity in $G = 2$ and $G = 3$, meaning total vacancies posted there do go up compared to fixed entry.

D.4 Increasing $g = E$ share and maximum $G = 1$ tightness

Currently, even at a full-scale continuous roll-out, the experimental group only accounts for a small share of the total unemployed. A natural follow-up question is what happens when the experimental group is much larger (e.g., because of a labor market disruption that causes demand to decrease in all but a few occupations). While it is straightforward that the intervention will remain effective if policy makers can control the share of eligible individuals who receive the information (and thereby containing congestion risk), it is likely that employment offices will be unable or unwilling to do so out of fairness concerns. As such, we are particularly interested in how the full-scale continuous roll-out scales.

To estimate the impacts of the treatment when $g = E$ represents a larger share of total unemployed job seekers, we essentially convert $g = O$ job seekers to

Figure D1: Jobs created by share $g = E$ in economy



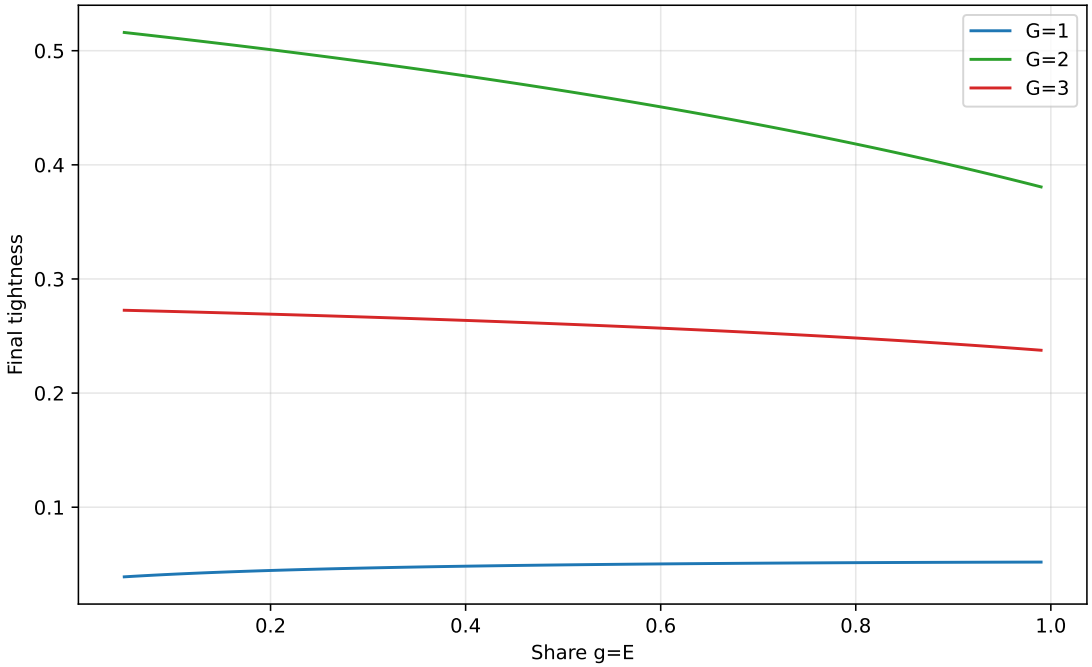
Note: Panel (a) shows the impact on total jobs created in the economy. Panel (b) shows jobs created per individual in g=E. Initial levels of tightness are fixed across occupations, but allowed to move within the calibration (i.e., when job seekers start searching more intensively in G=2 and G=3 following the treatment, tightnesses will change).

$g = E$ job seekers (i.e., they take over the $g = E$ search vector and primary search allocation) while keeping initial tightnesses fixed. We gradually increase the share of unemployed job seekers that $g = E$ represents from 5% (just above the baseline $g = E$ share) to 99%. After the change, we recalculate the steady state of the economy and depart from there. Note that because the $g = E$ group has worse job finding outcomes, the same number of unemployed job seekers now faces a lower inflow to employment, and to keep this as a steady state where it is balanced with the outflow from employment requires the number of employed individuals (and, at equal unemployment, the total labor force) to be lower. This exercise is therefore not a simple 'relabeling', but considers an economy that consist of many more job seekers looking for work in low-prospects occupations. We further assume the same things as in our baseline calibration (i.e., fixed entry and short-lived vacancies).

To treat job seekers in the counterfactual, we use the search intensities of the treated as calculated in Section D.3. We consider the full-scale continuous rollout (i.e., 99% of $g = E$ gets treated; both the unemployed and any employed individuals that lose their jobs). The value of 'jobs created' in panel (a) Figure D1 is the number of employed individuals at T=18 in the treatment scenario compared to a scenario where we do not impose the treatment. The value in panel (b) divides that by the number of treated individuals in $g = E$ but does include externalities (i.e., takes into account the per-person externality imposed).

As the number of treated individuals increases, tightnesses are more strongly affected compared to their imposed steady state values. To illustrate this, Figure

Figure D2: Tightness at $t = 18$ by share $g = E$

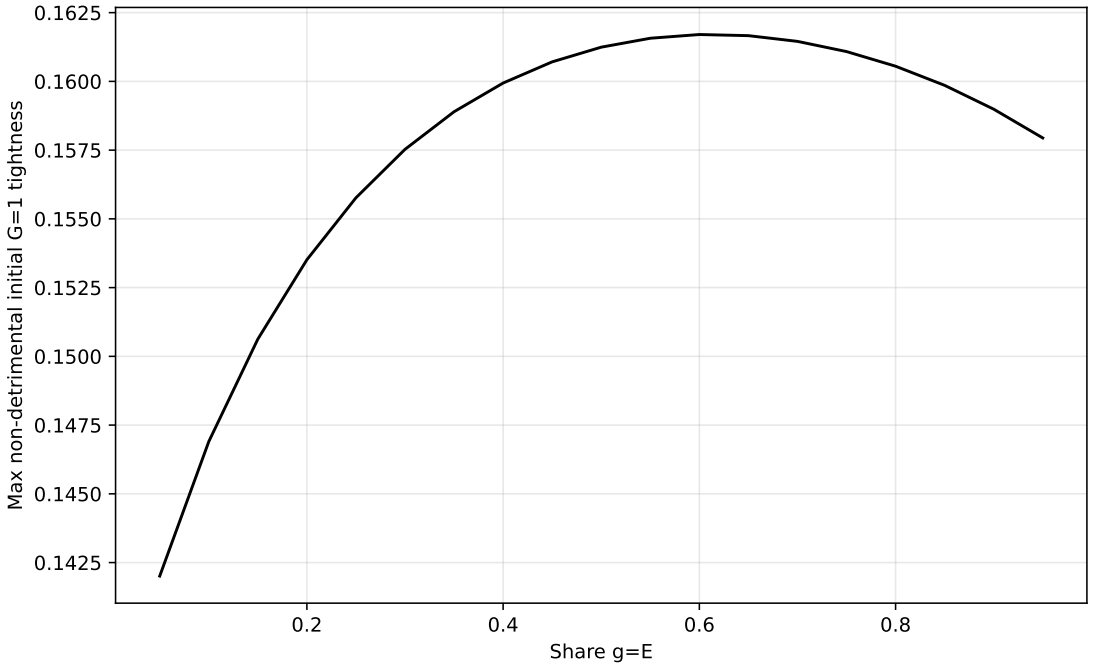


Note: Graph shows the tightness at t=18 in all occupations G, assuming short-lived vacancies with fixed entry and 99% of $g = E$ job seekers treated.

D2 shows the tightness at t=18 for all occupations under the treatment condition. It indicates that the treatment group indeed imposes substantial tightness externalities. It also shows that the impact on $G = 1$ tightness changes most drastically at low shares of $g = E$, and stabilizes at higher values of $g = E$, where the initial total search in $G = 1$ is much larger. The opposite holds for $G = 3$ and (in particular) $G = 2$, where total search intensity decreases as the size of the $g = E$ group increases and therefore becomes more sensitive to the movement of treated job seekers.

Beyond studying the tightness externalities imposed by treated job seekers, it is interesting to analyze how large the initial tightness difference between $G = 1$ and the other occupations ($G = 2$ and $G = 3$) needs to be for the intervention to still be effective. We analyze this by finding for each share $g = E$ the $G = 1$ tightness value for which the treatment is barely effective (i.e., total jobs created is 0). These values are plotted in Figure D3. Interestingly, the value is lowest (i.e., the treatment only works when $G = 1$'s tightness is relatively low) for small $g = E$ shares. The reason for this is that the externalities this group imposes are borne by a large group of $g = O$ job seekers. So, while the direct positive impact of the treatment is large here, so is the total externality imposed on others.

Figure D3: Maximum $G = 1$ tightness such that jobs created > 0



Note: Graph shows the maximum tightness in $G = 1$, assuming initial tightnesses in $G = 2$ and $G = 3$ are as we observe in the data. Calibration assumes short-lived vacancies with fixed entry and 99% of $g = E$ job seekers treated.

D.5 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 as the Dutch employment office 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 they do 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_{E1} 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 ($g = O$), 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 D2; panels c) and d) in Figure 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 short-lived: they need to be reposted every period. Entry is set to be exogenous. In models of fully directed search with free entry, the market tightness adjusts to keep the free entry condition satisfied. 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 tightnesses 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 D2: Calibration moments 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	$\sum_g s_{g3} u_g = U_3 = 395696$	Search-weighted unemployment in $G = 3$: Number of times occupation $G = 3$ is listed as occupation of interest, source (i).
7	$u_E = 10738$	Job seekers in group $g = E$: Experimental subjects, non-Covid occupations, source (iv).
8	$u_O = 214711$	Job seekers in group $g = O$: All job seekers in source (ii), minus u_E.
9	$\sum_G B_{EG} s_{EG} m(\theta_G) = F_E := 4.6\%$	Job finding rate for group $g = E$, source (iv).
10	$\sum_G B_{OG} s_{OG} m(\theta_G) = F_O := 9.1\%$	Job finding rate for group $g = O$, source (ii) and (iii).
11	$\rho_{O2} = 45.5\%$	Fraction of job seekers in group $g = O$ with primary search occupation in $G = 2$: All job seekers in source (ii).
12	$\rho_{O3} = 54.5\%$	Fraction of job seekers in group $g = O$ with primary search occupation in $G = 3$: All job seekers in source (ii).
13	$\frac{B_{E1} s_{E1} m(\theta_1)}{\sum_G B_{EG} s_{EG} m(\theta_G)} = H_{E1} := 30.3\%$	% of jobs found in $G = 1$ for $g = E$, source (iv).
14	$\frac{B_{E2} s_{E2} m(\theta_2)}{\sum_G B_{EG} s_{EG} m(\theta_G)} = H_{E2} := 20.1\%$	% of jobs found in $G = 2$ for $g = E$, source (iv).
15	$\frac{B_{E3} s_{E3} m(\theta_3)}{\sum_G B_{EG} s_{EG} m(\theta_G)} = H_{E3} := 49.6\%$	% of jobs found in $G = 3$ for $g = E$, source (iv).
16	$s_{E1} = 1.31$	Search effort group of $g = E$ in occupation $G = 1$: Pre-intervention survey, source (iv).
17	$s_{E2} = 0.27$	Search effort group of $g = E$ in occupation $G = 2$: Pre-intervention survey, source (iv).
18	$s_{E3} = 0.73$	Search effort group of $g = E$ in occupation $G = 3$: Pre-intervention survey, source (iv).
19	$\sum_g \bar{e}_g = 9598000$	Total labor force, source (v).

Table D3: Intervention moments

Row	**Target**	**Description and Source**
i)	$\frac{B_{T2}s_{T2}m(\theta_2)}{\sum B_{TG}s_{TG}m(\theta_G)} = \tilde{H}_2 = 26.3\%$	Source (iv).
ii)	$\frac{B_{T3}s_{T3}m(\theta_3)}{\sum B_{TG}s_{TG}m(\theta_G)} = \tilde{H}_3 := 54.5\%$	Source (iv).
iii)	2.9pp	Source (iv), Figure 5 panel (c).