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# Closing the Mismatch: Encouraging Jobseekers to Reskill for Shortage Occupations

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# Closing the Mismatch: Encouraging Jobseekers to Reskill for Shortage Occupations<sup>\*</sup>

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## Abstract

We partner with a Public Employment Service to examine whether jobseekers can be encouraged to reskill for shortage occupations. In a large-scale field experiment involving 100,000 recently unemployed individuals, we provide information on shortage occupations and related training opportunities. The intervention increased participation in transversal training courses by 6%, but did not boost enrolment in occupational training for shortage jobs. Jobseekers also shifted their search towards high-demand occupations, yet employment remained unchanged. These findings suggest that while low-cost informational interventions can influence job search and training behaviour, different approaches are likely needed to drive substantial reskilling among jobseekers.

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# 1 Introduction

Labour shortages have been widespread across countries and industries for many years, and have further worsened since the Covid-19 pandemic (McGrath and Behan, 2017; Causa et al., 2022). Such shortages are problematic in that they constrain the production capacity of firms and economic growth (Le Barbanchon et al., 2023). Policymakers have attempted to tackle this issue, e.g., by adopting targeted immigration laws, encouraging students to specialize in growing sectors, or retraining jobseekers to meet employers’ needs. The latter is a particularly attractive policy because it carries the potential of tackling both skills shortages and unemployment at the same time.

As a result, a growing number of countries have begun to offer “demand-driven” vocational training programs – i.e., training aimed at filling existing needs of local employers – to unemployed jobseekers. These programs have demonstrated their effectiveness in improving the labour market prospects of participants (Grosz, 2020; Baird et al., 2022; Katz et al., 2022; Foged et al., 2022; Wood et al., 2024). Nevertheless, they tend to be chronically under-subscribed, namely due to behavioural and informational barriers faced by potential participants (Babcock et al., 2012, Dhia and Mbih, 2021).

In this paper, we implement a large-scale information intervention that aims at encouraging more unemployed jobseekers to reskill for shortage occupations. We do this following a growing literature showing that jobseekers are not well-informed about the labour market in which they are searching for a job (Sahin et al., 2014; Patterson et al., 2016), but that simple and low-cost job search recommendations can help them to search more effectively (e.g., Altmann et al., 2018; Belot et al., 2019, 2022; Briscese et al., 2021; Wheeler et al., 2022; Le Barbanchon et al., 2023; Behagel et al., 2024). Most notably in this vein, Belot et al. (2019) and Belot et al. (2022) demonstrate that encouraging jobseekers to broaden their search towards similar jobs that are in higher demand improves their labour market prospects, particularly for the long-term unemployed. However, changes in job search strategies will only facilitate a return to the labour market to the extent that jobseekers already possess the necessary skills to fill available vacancies. In some cases, it is possible that upskilling or reskilling might be needed. For example, some individuals’ skills may have become obsolete as a result of changing skill requirements, while others may simply lack the appropriate qualifications to find a job on the current labour market. Hence, informing jobseekers about training opportunities might be an essential step when steering them toward occupations that are in high demand by employers.

We implement a field experiment in partnership with a Public Employment Service (PES henceforth) in Belgium. The experiment took place in the context of a satisfaction survey that was sent to all individuals having enrolled at the PES between July 2021 and January 2022 (roughly 100,000 individuals). For a randomly selected half of individuals, the email inviting them to complete the survey also included information about shortage occupations, the availability of training programs offered by the PES, and the attractive conditions under which they could enrol. We use a combination of administrative records, data on email engagement, and survey data to estimate the effects of this intervention on beliefs, training participation, job search, and employment.

We first investigate whether the intervention affected beliefs about job search and training intentions using data collected in the satisfaction survey. Our findings show that receiving the information increased training intentions by 3.97 percentage points (pp), a 7.21% proportional increase relative to the mean intention of enrolling in a training program in the next year. This increase in training intentions is accompanied by a rise in the perceived usefulness of training participation. These first findings suggest that the intervention had a relatively strong “first stage”, in that it affected intentions to train and beliefs about their usefulness.

Next, using administrative records on training participation, we show that effects on intentions and perceptions only partly translate into actual increases in training enrolment. We find that training participation increases by 0.36 pp six months after the intervention took place, a 6.13% increase with respect to the mean enrolment in the control group. All effects on training enrolment materialize in the first six months after the intervention, with effects remaining at 0.36 pp after 12 months. Interestingly, this increase in training participation is entirely driven by enrolment in transversal training courses, i.e., programs that teach skills that are useful in a wide variety of jobs and sectors. In contrast participation in occupational training remained unaffected, as did participation in programs targeting specifically shortage occupations. These findings thus indicate that jobseekers can be encouraged to enrol in training, even with a low-cost and general information intervention. However, this low-cost intervention was not sufficient to steer them toward occupational training programs addressing shortage occupations.

Turning to effects on job search behaviour, we find that the information intervention was effective in steering jobseekers toward searching for jobs in occupations that are in high demand. We indeed find that receiving the information increased the likelihood of searching for a shortage occupation by 1 pp (1.14 pp) six (18) months after the intervention took

place, a 1.68% (1.86%) proportional increase. Hence, in line with previous findings from the literature, we find that jobseekers adjust their job search strategy in response to new information about employers' preferences and needs.

Heterogeneity analyses reveal that the information intervention fostered training participation among low-skilled individuals who were on the margin of participating, but were unlikely to find a job. When looking at effects on job search, we also find that those most affected are low-skilled jobseekers with a relatively low employment propensity. Women appear to be more affected than men, potentially because of their greater engagement with the information received. In short, the individuals most affected by the intervention were those for whom it was likely most needed, as well as women.

Ultimately, neither the increased participation in transversal training courses, nor the increased focus on shortage occupations in job search, led to an increase in employment probability. Hence, occupational training (in the form of reskilling and/or upskilling) may be needed if jobseekers are to be effectively steered toward filling shortage occupations.

This paper contributes novel findings on how to bridge the gap between the skills of unemployed jobseekers and the needs of employers. Our findings are primarily related to the literature studying the impact of information frictions occurring in the context of job search, and how best to address them. This literature shows that the unemployed greatly overestimate how quickly they will find work (Spinnewijn, 2015), and do not revise their (biased) beliefs downward when remaining unemployed (Mueller et al., 2021). Moreover, it appears that unemployed individuals tend to search in occupations with relatively few job openings, while other occupations that offer more employment opportunities attract little interest (Sahin et al., 2014; Patterson et al., 2016).

Given that jobseekers hold biased beliefs about the labour market, a growing body of literature has focused on interventions that aim at correcting erroneous beliefs and information gaps. For example, Belot et al. (2019) evaluate an innovative tool that provides jobseekers with tailored job search advice. They find that providing individuals with occupational recommendations broadens the set of jobs they consider, and increases the number of interviews they get invited to. In a follow-up (and scaling-up) project, the authors show that the long-term unemployed can particularly benefit from this type of advice (Belot et al., 2022). In a similar vein, Behagel et al. (2024) and Le Barbanchon et al. (2023) show that job search recommendation algorithms can reduce search frictions and increase aggregate employment. Altmann et al. (2018) further show that sending recently unemployed individuals an informational brochure on job search strategies and the consequences of unemployment

can increase their employment probability and earnings, with more pronounced effects for individuals at risk of long-term unemployment. Supplementing job search skills by providing editable resume and cover letter templates (Briscese et al., 2022) or LinkedIn courses (Wheeler et al., 2022) is another proven strategy that can help jobseekers to search for jobs more effectively, at a relatively low cost. Our contribution to this strand of the literature is to focus on informing jobseekers about *shortage occupations* in particular and, crucially, available *training opportunities* they can seek if they do not yet possess the necessary skills to enter these jobs in high demand.

In this respect, our paper also contributes to research on information frictions in the context of education and training decisions. This strand of the literature has mainly focused on addressing information frictions faced by high school or graduate students before they enter the labour market (e.g., Bonilla-Mejia et al., 2019; Conlon, 2021; Ganguli et al., 2022; Kerr et al., 2020). Two notable exceptions are Barr and Turner (2018) and Dhia and Mbih (2023), who both study the effects of an informational outreach on training choices of the unemployed. Barr and Turner (2018) show that informing unemployment insurance recipients about the benefits and costs of post-secondary education, as well as the necessary steps and assistance available to facilitate such an investment, increases the probability that they enrol in community colleges by 4 pp, or 40% in proportional terms. Dhia and Mbih (2023) test for the existence of misinformation about training costs and returns among French unemployment insurance recipients, and uncover important information gaps. They then go on to show that highlighting returns to training when inviting jobseekers to enrol more than doubles the likelihood that they call back the training center, but does not affect training enrolment. Our key contribution to this nascent literature is to combine information about shortage occupations with information on the available training opportunities, so that jobseekers know that they could acquire the necessary skills to enter these jobs in high demand if they wished to. Our setting thus allows us to study the effects of informing jobseekers about training programs with particularly high potential to lead them into stable jobs.

The remainder of the paper is structured as follows. Section 2 provides information on the institutional setting. Section 3 describes the intervention and how it was implemented. Section 4 explains the empirical strategy and data sources. Section 5 presents our main findings, while Section 6 discusses their robustness. Finally, Section 7 concludes.

## 2 Institutional Setting

The PES with which we partnered for this evaluation is responsible for all active labour market programs in the region of Wallonia, in Belgium. The region has an average of approximately 200,000 unemployed jobseekers at any point in time (12.6% of the active population); 120,000 of whom have been unemployed for more than a year.

The PES’s central mission is to “accompany all citizens (jobseekers, but also apprentices, students, workers, and teachers) and firms in their professional journey”. In this context, one of their key activities is to offer and promote training programs, with a special emphasis on vocational training for which firms operating in the region face skill shortages. These shortages can be of a qualitative nature (e.g., candidates who apply for the jobs do not have the qualifications that employers require) or of a quantitative nature (i.e., there are too few unemployed jobseekers to fill the job openings). Each year, the PES makes a list of jobs that are considered to be in shortage and attempts to direct particular efforts to alleviate these shortages, namely through training the unemployed.<sup>1</sup> While the PES offers a range of training programs (including transversal training courses and many occupational trainings), it directs the majority of its resources towards training programs for occupations that have been identified as being in shortage.

In 2021, out of 650 occupations, 121 (or almost 20%) were defined as being in shortage in Wallonia. The jobs in this list are quite diverse (e.g., healthcare sector, construction, service industry, logistics, industry) and require varying levels of education. The length of training required to enter these jobs ranges from five weeks (cleaner) to eight years (general practitioner). Overall, most unemployed individuals considering upskilling or reskilling should be able to find a shortage occupation that matches their education level and preferred sector of work.

But despite the large number of training options offered, shortage occupations typically remain understaffed for many years, and trainings tend to be chronically under-subscribed. This is somewhat surprising, not only given the high potential rewards from completing such programs – getting a job at the end of the training is very likely given that the occupation is, by definition, understaffed – but also because the conditions under which these trainings can be pursued are relatively attractive. Jobseekers who follow training programs for shortage occupations are indeed exempted from their job search requirements while keeping their

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<sup>1</sup>Appendix A.1 describes how the PES identifies shortage occupations and shows the full list of occupations that were considered in shortage in 2021 and 2022.



rights to unemployment benefits during the entire duration of the training program.<sup>2</sup> In addition, they are entitled to a travel allowance, extra childcare benefits, special interview coaching and a financial bonus ranging from €350 to €2,000 if they successfully complete the training.<sup>3</sup> In spite of all this, many jobseekers remain unemployed for long periods of time without ever enrolling in these trainings.

We explore to what extent participation in these training programs could be encouraged by reducing information frictions on the side of unemployed jobseekers. Although the PES’s website provides some information on shortage occupations and related training opportunities, one needs to actively search the website to find this information. In theory, caseworkers could also provide job search advice and information about training opportunities in this area, but they are not instructed to systematically focus on shortage occupations during their meetings with jobseekers. As a result, jobseekers in Wallonia are unlikely to be informed about shortage occupations and related training programs unless they have already expressed interest in one. In this context, an information intervention has the potential to encourage more jobseekers to enter shortage occupations and, if they currently lack the skills to do so, to enrol in the appropriate training programs.

## 3 Experimental Design

### 3.1 Intervention

We implemented a large-scale field experiment in partnership with the PES that took place between October 2021 and February 2022. The intervention consisted of sending information, by email, about shortage occupations and related trainings. All 98,844 jobseekers who registered at the PES between July and December 2021 and had an email address were included in the experiment.<sup>4</sup>

In our experiment, the PES sent participants an email invitation to complete a satisfaction survey regarding the services they had received while unemployed.<sup>5</sup> The email sent to

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<sup>2</sup>This is true under a few broad conditions listed in Appendix A.2

<sup>3</sup>Most training programs focused on shortage occupations offer a €350 bonus, but those in the construction sector offer a €2,000 bonus upon completion.

<sup>4</sup>In the first wave of the experiment, the email was sent to 60,485 individuals on October 19, 2021 (Monday around 9am). A reminder was sent on October 26 (Tuesday around 2pm) to those who had not opened the first email. In the second wave, the email was sent to 38,362 individuals on February 1, 2022 (Tuesday around 9am). A reminder was sent on February 8, 2022 (Tuesday around 2pm) to those who had not opened the first email. According to the PES, this time of week and day maximizes the rate of engagement with the email they send to jobseekers.

<sup>5</sup>This satisfaction survey is usually sent to a small sample of recent jobseekers but, for the purpose of

individuals in the control group solely included the invitation to complete the satisfaction survey. For a randomly selected half of individuals who were assigned to the treatment group, the email also contained information about shortage occupations and related training opportunities.

The treatment email informed jobseekers that shortage occupations systematically lacked candidates, and that focusing on these 121 occupations could allow them to increase their probability of (re-)entering the labour market. The number of shortage occupations was mentioned in order to signal that there were many options to choose from, without needing to add the full list to the email. The message also mentioned that jobseekers could follow trainings for shortage occupations, free of charge, and under relatively attractive conditions. In particular, it stated that they maintained rights to their unemployment benefits without having to actively search for a job during the training, that they could get additional childcare and travel allowances, and that they were entitled to a bonus ranging from €350 to €2,000 after completing a training for a shortage occupation. Interested jobseekers were then referred to their local “Customer Service” – agencies that are in charge of assisting jobseekers at different stages of their unemployment spell – whom they could contact for additional information. At the end of the email, there were three links leading to additional information on shortage occupations, namely: (i) a video explaining what shortage occupations are, (ii) the full list of shortage occupations, and (iii) additional information on the financial bonuses. A copy of the treatment and control emails are presented in Appendix A.3.

The treatment was thus intended to steer jobseekers towards more promising occupations and related training opportunities. It did so by informing them, in a salient and simple manner, about the existence of shortage occupations and the attractive conditions under which they could seek training for these jobs while unemployed. The emails were sent before the first meeting with a caseworker would have taken place.<sup>6</sup> This means that, even if jobseekers did not take immediate action by themselves after receiving the email, they had an opportunity to steer the conversation with their caseworkers towards the shortage occupation of their choice and could then obtain more tailored information (about job search or training) from their caseworker directly. In this sense, the timing of the intervention and the choice of the target population allowed to maximize the potential effectiveness of our low-cost, low-intensity, information intervention.

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our experiment, the PES sent the survey to all recently registered jobseekers.

<sup>6</sup>These meetings typically take place about four months after registering at the PES and jobseekers had been unemployed for less than four months at the time the emails were sent.

To summarize, we implement a low-cost information intervention, providing general information about shortage occupations and related training programs to a broad population of recently unemployed jobseekers. In turn, the information provided aims at encouraging jobseekers to seek additional information with their local Customer Service or during their upcoming meeting with a caseworker. As described in the next section, our data allows us to follow jobseekers for up to 18 months after the intervention took place. This means we are able to detect effects that would occur in a sequential way, i.e., after the jobseekers meet their caseworker.

## **3.2 Randomization**

Half of the individuals in our sample were randomly selected to receive the treatment email. Before sending the treatment and control emails, the PES sent us the anonymized identifiers of all individuals who had recently registered. We then performed the randomization based on the (meaningless) last digit of the anonymized individual identifier.

Table 1 shows descriptive statistics on the characteristics of the treated and control groups, before the treatment email was sent. Reassuringly, it shows that the characteristics of the two groups are well-balanced. None of the differences between the treatment and control groups is statistically significant, except that there are slightly (0.1 pp) more individuals who live outside the Walloon region in the control group. This difference is very small, and only marginally significant; it therefore does not threaten the validity of our experiment.

## **3.3 Engagement with the Email and its Content**

The advantage of using emails to communicate our treatment information is that we can reach a broad population of jobseekers at a very low cost. In terms of data, using emails allows us to observe who actually receives the treatment, i.e., who opens the emails and clicks on information links. The information on email engagement is linked to administrative data, which means we can also assess who engages with the email and its content. This allows us to distinguish jobseekers who were sent an email but did not open it from those who did open the email (and/or clicked on links), and were thus effectively treated.

Table 2 shows information on email engagement for the overall sample (column 1), the treatment group (column 2), the control group (column 3) and the difference between the treatment and control groups (column 4). It shows that approximately 54% of individuals opened the email they were sent by the PES. This share is the same in the treatment and control groups, which is reassuring as it indicates that there was no differential selection into

reading the email between the two groups. In our main regressions, we keep only those who opened the email, and were therefore actually affected by our information intervention. In the robustness section, we check the sensitivity of our findings to keeping non-openers.

Table 2 further shows that treated individuals were, in contrast, more likely to click on at least one link in the email (13.92% versus 11.31% of the controls) and clicked on more links on average, which likely reflects the fact there were more links they could potentially click on. Interestingly, the treatment group was less likely to click on the survey link (7.81% against 11.08%), suggesting that the information on shortage occupations and related training programs diverted jobseekers' attention away from the survey. Similarly, treated jobseekers were slightly less likely to click on a social media link (0.19% against 0.33%). Note that these differences illustrate that highlighting certain information (in our case: the attractiveness of shortage occupations and related trainings) can divert the attention away from other information and actions (in our case: answering the satisfaction survey). This differential selection in the survey, although not huge, suggests that our treatment group is likely negatively selected in the survey – those most interested in the treatment information are indeed those most likely to have clicked on the information links rather than the survey link.

Table 2 also provides some information on how the treatment group interacted with the information on shortage occupations and related trainings. It shows that treated jobseekers were most interested in the list of occupations in shortage, with 4.66% of them clicking on that link. The email also included a video which presented the occupations in shortage, which 1.65% of treated individuals clicked on. Finally, 1.47% clicked on the link referring them to the Customer Service while, maybe surprisingly, only 1.24% clicked on the link that provided them with more information on the financial incentives. Overall, 7.65% of treated individuals who opened the email clicked on at least one of the treatment information links, indicating a certain interest in the information they were being provided.

Another noteworthy point is that approximately 1.89% of treated individuals clicked on both the survey and one of the links on shortage occupations. This means that a little less than one quarter of survey respondents also clicked on a shortage occupation link, and over 75% of those who clicked on shortage occupation links (arguably the most interested in these training programs) are not included in our survey. This suggests that the estimated effects on training intentions (measured in the survey) could be somewhat underestimated, as it will not include individuals who are potentially the most affected by the treatment information.

Finally, in Appendix A.4, we explore to what extent individuals who opened the email and clicked on the survey differ from the overall sample. We find that women, older individuals,

non-Belgians and individuals who had already been unemployed are over-represented in the sample of email openers and of people who clicked to complete the survey.

## 4 Empirical Approach

### 4.1 Data

We make use of both administrative and survey data in this paper. The administrative data contains information on personal characteristics, training participation, employment, and job search. The survey data is used to construct outcomes relating to beliefs about job search and training intentions. The survey and administrative data cannot be linked to one another.

The administrative data contains highly reliable and extensive information on all individuals who were part of the experiment. In addition to data on personal characteristics – age, gender, nationality, education level, field of education, district of residence, or the existence of previous unemployment spells – it contains detailed information on training and employment spells. The training data includes all training activities undertaken by jobseekers, with the name of the training activity, its length, and the reason for ending the training. The employment data contains information on the start and end date of each spell, as well as the sector of activity in which the employer is active. Finally, this data contains a list of occupations for which each individual is listed as searching for a job in, as well as the number of times they have connected themselves to their personal space on the PES website.

Our survey data includes respondents’ answers to questions relating to their beliefs about job search and their training intentions. First, we asked respondents whether or not they had the intention to follow a training in the coming year. Second, we asked them how useful they considered different job search strategies to be. We asked their opinion about the following job search strategies: searching for a job in a shortage occupation, participating in training programs, participating in trainings for shortage occupations, having a good CV and motivation letter, using one’s network, meeting one’s caseworker, making use of employer subsidies, or searching for a job in which they already have experience. Respondents could choose from five options: “no impact”, “little impact”, “average impact”, “high impact”, or “very high impact”. In the survey, we distinguished between treated and control individuals by creating separate survey links for each group. More details on the survey and the questions it contains can be found in Appendix A.5.

In total, 3,049 individuals answered the survey, of which 1,252 (41%) belonged to the treat-

ment group and 1,797 (59%) belonged to the control group. In other words, 2.54% of individuals in the treatment group completed the survey, against 3.62% of the control group. Although the sample of individuals who completed the survey is a selected sub-sample of our overall target population, treated and control individuals do not appear to self-select into answering the survey differently (at least based on observable variables).

## 4.2 Outcomes of Interest

To investigate whether the information intervention affected training participation, we first consider whether individuals participate in any training activity six (short-term) and 12 (long-term) months after the intervention. To this end, we create a binary variable equal to one if the individual is observed taking part in at least one training activity, and zero if not. We consider pre-training activities (such as selection tests, information sessions, or screenings) separately from actual training activities, as pre-trainings are not focused on acquiring new skills but focus on selecting the right individuals for a given training program.

Since we are interested in understanding whether the intervention was effective in encouraging participation in trainings relating to shortage occupations, we subdivide training activities between transversal and occupational trainings. Transversal training courses include training activities that do not target a given occupation or sector, such as a language course, trainings for using IT tools, or orientation and coaching. In contrast, occupational training programs focus on acquiring skills specific to an occupation or sector. These categories are mutually exclusive and exhaustive, i.e., we classify all training activities as either transversal or occupational. In a second step, we subdivide occupational trainings between those that target occupations that are in shortage and trainings that target other, non-shortage, occupations.

Second, we are interested in understanding whether the information intervention affected job search behaviour. To this end, we define three job search-related outcomes. First, we construct a dummy equal to one if an individual lists a shortage occupation among their list of occupational preferences.<sup>7</sup> We also consider the total number of occupations that jobseekers list in their occupational preferences. Moreover, we observe the number of times individuals connect to their personal space, which we use as a proxy for job search intensity.<sup>8</sup>

Third, we are interested in estimating the effect of the intervention on the probability of

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<sup>7</sup>Jobseekers must list the occupations in which they would like to find employment in their personal space on the PES website. We can observe the list of these occupations six and 18 months after the intervention. Jobseekers can list as many occupations as they like, but typically have between one and six.

<sup>8</sup>This variable shows a picture six and 18 months after the email was sent.

finding a job. To this end, we use data on all employment spells that individuals experience over our period of analysis and construct a dummy equal to one if they experience any employment spell starting in the first six months (for short-term estimates) after the intervention took place. Long-term estimates consider all employment spells that start within the first 18 months after the intervention.

Finally, we use survey responses to construct outcomes relating to beliefs and intentions. This allows us to identify whether the information intervention had a “first-stage”, i.e., whether it affected individuals’ intentions to enrol in trainings and their beliefs about the usefulness of training. For intentions, we build a dummy variable equal to one when survey respondents answered that they had the intention to follow a training in the coming year. For beliefs about the usefulness of job search strategies, we build a dummy variable equal to one if the respondent considered a strategy as being “useful” or “very useful”.

### 4.3 Regression Specification

Because individuals were randomly assigned to treatment, we can estimate the following equation to recover intention-to-treat (ITT) effects:

$$Y_i = \alpha_1 + \beta_1 Treatment_i + \epsilon_i \quad (1)$$

A key advantage of delivering the information treatment by email is that we can observe who opens the email and who clicks on the information links, and thus who actually accesses the information. Therefore, we can also estimate a local-average-treatment-effect (LATE) for individuals who accessed the information on shortage occupations and related trainings, through clicking on at least one of the treatment links in our email. We estimate the following equation using 2SLS:

$$Y_i = \alpha_2 + \beta_2 Info_i + \epsilon_i \quad (2)$$

Where:

$$Info_i = \gamma + \delta Treatment_i + \eta_i \quad (3)$$

In these equations,  $Y_i$  is the outcome of interest of individual  $i$ ,  $Treatment_i$  is a dummy equal to one when individual  $i$  was assigned to the treatment group and zero otherwise, and  $Info_i$  indicates whether an individual has clicked on one of the treatment links. Our coefficients of interest are  $\beta_1$  and  $\beta_2$ , the effect on the outcome of receiving the information email or clicking on an information link, respectively. Throughout our analyses with administrative data, we restrict our sample to the population who opened the email and were thus effectively part of the experiment ( $N = 53,882$ ). In our analyses that require survey data, our sample is

composed of all respondents who, by definition, have also opened the email ( $N = 3,049$ ). The robustness section explores to what extent our estimates vary depending on the criteria used for inclusion in the sample.

The ITT estimates are indicative of the effects of such an intervention if generalized to the entire population of jobseekers. In contrast, the LATE provides insight into the impact of actually accessing information on shortage occupations and related training for individuals who were induced to do so by the intervention. Thus, it specifically informs about the effects of the information intervention on those who actively engaged with it.

We estimate treatment effects without controlling for personal characteristics, as these are balanced between the treatment and control groups. We nevertheless show in Section 6 that our findings are unaffected by their inclusion. In those analyses, control variables include age, gender, nationality, education level, province of residence and a dummy indicating whether an individual has experienced unemployment spells before.

## 5 Findings

### 5.1 Effects on Beliefs and Intentions

Figure 1 shows, using Equation 1, the effect of being sent the treatment information on the probability of having the intention to follow a training in the upcoming year. It also shows the effect of the treatment intervention on the likelihood of perceiving different job search strategies as being useful.

Our findings indicate that being sent the treatment information increases training intentions by 3.97 pp, or 7.2% relative to the mean control probability of 55%. This estimate is statistically significant at the 5% confidence level. Receiving information on shortage occupations, related trainings, and the conditions under which jobseekers can follow these training programs, thus increases jobseekers' intention of enrolling in the upcoming year. Our findings suggest that this occurred through an improved perception about the usefulness of training (namely in shortage occupations, but not exclusively). Treated individuals are indeed 3.77 pp (6.4%) more likely to deem training as an effective job search strategy than respondents from the control group, while this effect is 2.76 pp (5.8%, marginally statistically insignificant) when focusing on training programs for shortage occupations specifically. As shown in Appendix A.6, these estimates grow in magnitude (and significance in the case of trainings for shortage occupations) once we add control variables. The fact that the intervention increased jobseekers' intentions of enrolling in training is encouraging with respect to its



objective of increasing training participation and, ultimately, entry in shortage occupations. In the next section, we investigate to what extent these intentions materialize into actual training participation.

Interestingly, Figure 1 shows that the treatment email did not affect perceptions about the usefulness of searching for a job in a shortage occupation. On the one hand, this could indicate that email recipients put greater emphasis on the information about training, rather than the message about searching for jobs in shortage occupations. On the other hand, it could signal that jobseekers already know about the existence of shortage occupations and the fact that searching a job in such occupations is a good way of maximizing one’s chances of being hired. In Section 5.3, we explore whether treated jobseekers were more likely to change the occupations in which they were registered as searching for a job and confirm that treatment effects on job search behaviour were smaller in relative terms than effects on training enrolment.

Finally, Figure 1 suggests that the intervention had some negative spillovers on beliefs about the usefulness of meeting with caseworkers. This echoes recent findings from Altmann et al. (2022) showing that although policies that steer individuals’ attention to a specific decision usually lead to better choices in the targeted choice domain, they can also induce negative cognitive spillovers on the quality of choices in other domains. In our case, it is possible that by informing jobseekers about the importance of shortage occupations and related trainings in their job search efforts, the email led them to consider other strategies as relatively less useful in comparison. In exploratory analyses, we observe that this negative spillover is driven by individuals who had already been unemployed in the past. One possible explanation could be that the email made them realize they had never heard about these training programs from their caseworkers in the past, leading them to consider their services as less useful.

## 5.2 Effects on Training Participation

We now turn to explore whether increased intentions to train translate into actual changes in training behaviour. We estimate Equation 1 (ITT) and Equation 2 (LATE) to recover the treatment effects of the information intervention on various training activities, six and 12 months after the email was sent. Our estimates are shown in Table 3.

Columns (1) and (2) of Table 3 show the effect of the intervention on the probability of enrolling in pre-training activities and training programs. We find that receiving the treatment email increased the probability of enrolling in a training program by 0.36 pp, or 6.13% relative to the control mean. This effect subsists 12 months after treatment, although is

becomes marginally insignificant ( $p$ -value=0.1107). All of the effect on training enrolment materializes in the first six months after the intervention, as illustrated by the point estimate that is the same six and 12 months post-intervention.

Based on the LATE coefficient, we estimate that for individuals induced to click on an information link in the email, the intervention increased the likelihood of enrolling in a training program by 4.59 pp, a 78.19% increase relative to the control mean. This suggests that for individuals with some interest in the information provided, the email led to substantial changes in training behaviour.

In contrast, participation in pre-training activities such as information sessions, screenings, or selection tests, remained unaffected by the intervention. As discussed below, the absence of any effect on participation in pre-training is likely related to the absence of any effects on participation in occupational training, as pre-trainings are usually a first step to entering such programs.

Next, we explore whether the information treatment increased participation in training programs that are focused on shortage occupations. In columns (3) and (4), we first subdivide training activities from column (2) into trainings that offer participants the opportunity to learn general skills that can be used in many sectors and occupations (“Transversal Training”) versus programs that teach participants a specific occupation, or at least are specific to a sector of activity (“Occupational Training”). We find that the increased training participation found in column (2) is exclusively driven by increased enrolment in transversal training courses, whereas there is no effect on occupational training programs. This is true both in the short- and long-term.

The absence of an effect on occupational training could nevertheless hide important behavioural changes. Specifically, participants who would have pursued occupational training regardless might have shifted from their initial occupational preference to a shortage occupation. In this case, we would observe no change in overall participation in occupational training but an increase in enrollments for shortage occupations. Such a shift would be considered a success, as it would indicate that the information intervention effectively reoriented jobseekers toward higher-demand jobs.

Therefore, in columns (5) and (6), we differentiate occupational training programs targeting shortage occupations from those focused on other (“non-shortage”) occupations. Our findings in Table 3 do not suggest any shift from non-shortage to shortage occupation training programs. This is in line with our survey results showing that while perceptions of training

usefulness increased significantly due to the intervention, perceptions of training specifically for shortage occupations did not increase as much. It thus seems that treated jobseekers picked up the message relating to the attractiveness of training in general, rather than the attractiveness of shortage occupations *and related trainings*. As discussed in the conclusion, the absence of an effect on occupational trainings could also stem from potential supply-side constraints.

Together, our findings are quite encouraging as they suggest that even a low-touch information intervention was able to generate interest – and, to some extent, even enrolment – in training programs. This finding seems to be broadly in line with the literature on the effect of light-touch information interventions in the context of job search (e.g., Altmann et al., 2018; Belot et al., 2019; Wheeler et al., 2022). In the limited literature focusing on information interventions in the context of education decisions for the unemployed, results are more mixed. Dhia and Mbih (2023) show that an informational outreach highlighting the costs and returns of training, as well as the simplicity of registration procedures, did not significantly impact training enrolment of jobseekers in France six months after the intervention, although it did increase requests for more information. In contrast, Barr and Turner (2018), who study a more intensive informational campaign, show that after six months, a letter encouraging unemployment insurance recipients to enrol in a post-secondary program increased training take-up by 4 pp (40%). Larger effects are to be expected in their case, since the intervention included an “assistance dimension” (i.e., unemployment insurance recipients were also sent information that facilitated engagement with a number of intermediaries, including employment services offices and local financial aid administrators, who were prepared to assist individuals responding to the letter). Our results contribute to this literature by showing that while a low-cost information intervention can encourage training participation, promoting reskilling for shortage occupations may be more challenging.

Finally, a gap appears to exist between intentions and behaviours: while 55% of jobseekers express an intention to enrol in a training program, only 7.8% do so within 12 months. This is consistent with the existence of an intention-behaviour gap – defined as the disconnect between the intention to perform a particular behaviour and the enactment of such behaviour – in job search, as shown in Abel et al. (2019). Such a gap between intentions and perceptions underscores the importance of estimating effects using administrative records on actual behaviours rather than stated intentions or actions.

### 5.3 Effects on Job Search and Employment

The intervention intended to steer jobseekers toward occupations that are in high-demand by employers, in part through training. In this section, we investigate to what extent treated jobseekers adapt their job search strategy after receiving the email, and whether any of these changes in training and job search behaviour ultimately led to changes in employment probability.

First, Table 4 shows that the information intervention increased the likelihood that jobseekers would list a shortage occupation among their occupational preferences. The effect is approximately equal to 1pp six months after the intervention, which represents a 1.68% increase. The effect grows somewhat in subsequent months, reaching 1.14 pp (1.86%) after 18 months. Looking at the LATE coefficient, we see that the intervention increased the probability of searching for a shortage occupation by 0.13 pp (0.15 pp) six (18) months after the intervention, a 22% (24%) proportional increase. This effect on job search could come from jobseekers adding a shortage occupation to their list, or them changing one or more of their occupational preferences from a non-shortage to a shortage occupation. Interestingly, column (3) indicates that jobseekers do not necessarily list a larger number of occupational preferences; rather, when they make a change (modification or addition) to their list, they are more likely to list an occupation that is in high-demand by employers.

This finding contrasts with the lack of an effect on beliefs about the usefulness of searching for a job in a shortage occupation (see Section 5.1). One explanation could be that, while jobseekers are aware of the existence of shortage occupations, our intervention made these occupations more salient and, as a result, jobseekers became more likely to list a shortage occupation when they changed their occupational preferences.

The fact that the information intervention changed the types of jobs targeted by treated jobseekers is consistent with findings from Belot et al. (2019) and Belot et al. (2022). They indeed find that encouraging jobseekers to search for high-potential occupations that require similar skills to the jobs they are already targeting broadens the scope of their search. Our effects are smaller, which is likely due to the fact that our information is of a more general nature, and is not targeted to the individual situation and preferences of the jobseeker. Moreover, we expect that the time at which jobseekers receive this information may matter. In Belot et al. (2019) and Belot et al. (2022), jobseekers receive job search advice within the job search platform, whereas in our intervention, the information was provided by email. As discussed in the conclusion, the time at which job search (and training) advice is provided is likely to play a role in explaining the greater success of some information interventions over

others.

Next, column (4) of Table 4 shows the estimated effect of the intervention on the number of times an individual logs into their personal space on the PES website, a proxy for job search intensity. We find that receiving the information led to a 0.13 decrease in the number of connections to their personal space, a 7.57% decrease. The effect remains of similar magnitude after 18 months, though it loses statistical significance. This suggests that treated individuals exerted less job search effort, possibly due to the increased likelihood of being enrolled in a training program. As we show in our heterogeneity analyses, it indeed seems that the same groups that experienced the largest increases in training participation are the ones who decreased their job search efforts.

Finally, we examine the estimated effects of the intervention on employment, presented in column (1). Given what we have shown so far, our intervention could have affected employment prospects of treated jobseekers through increased training participation (potentially after the well-known “lock-in” period), as well as by making them more likely to target shortage occupations in their job search. The latter mechanism could only take place to the extent that (some) jobseekers already have skills that are marketable when applying to jobs for shortage occupations. This could happen if, for example, some shortage occupations are relatively similar or require comparable skills to the jobs individuals were already searching in (as in the tool implemented and tested in Belot et al., 2019). However, Table 4 shows that we find no effect on the probability of experiencing an employment spell six and 18 months after the intervention.<sup>9</sup>

On a positive note, the absence of a negative effect on employment suggests that the increased participation in transversal training courses induced by our intervention did not create significant lock-in effects for individuals who would otherwise have found a job. As shown in the next section, our heterogeneity analysis also indicates that any negative effects occurring from such lock-in are unlikely.

Ultimately, the intervention was thus not successful in increasing employment. Following transversal training programs and searching for jobs in shortage occupations (without completing any occupational training), is therefore not sufficient to get jobseekers into employ-

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<sup>9</sup>We find no evidence that changes in job search behaviour allow jobseekers to enter shortage occupations. Specifically, we checked whether treated jobseekers were more likely to find employment in a sector where there tends to be a higher share of occupations in shortage. This exercise relies on a number of assumptions, namely that jobs are spread evenly across occupations within a given sector. Since our employment data does not include information on occupation (only on the industry in which the employer is active), this is the closest we can get to exploring whether changes in job search behaviour leads to more employment in shortage occupations.

ment, and certainly not to place them in shortage occupations.<sup>10</sup> The fact that increased participation in transversal training courses does not translate into increased employment hints to evidence on such transversal training courses not being very effective in bringing the unemployed back to work (Stenberg and Westerlund, 2015; Woods et al., 2024). Occupational training programs targeting high-demand occupations have a better record of fostering returns to employment (e.g., Katz et al., 2022; Grosz, 2020), but it appears that different approaches will be needed to foster participation.

## 5.4 Heterogeneity

This section examines whether some groups were more affected by the intervention than others. Note that, given our limited power, none of the differences we discuss here are significant; however, examining what individuals appear to drive our results can be informative. We are particularly interested in understanding whether the intervention affected those most in need of support for re-integrating the labour market.

We first explore potential (pre-registered) heterogeneities along demographic characteristics, such as gender and education.<sup>11</sup> Figure 2 shows that all of our effects on training participation and job search are driven by women. This is consistent with Barr and Turner (2018) who also found that the Pell Letter had larger effects on training enrolment among women. A priori, it is unclear why women would be more affected by the information intervention. However, it is notable that women were also more likely to open the email and to complete the survey, suggesting that they might have been more attentive to the email sent by the PES.

Next, Figure 3 shows that the effect of the information intervention on the probability of participating in training programs is driven by jobseekers with relatively lower levels of education (i.e., those with a secondary school degree or less). In contrast, those who react the most strongly in terms of job search behaviour are those with a tertiary education. This could reflect differences in training needs and/or existing labour market prospects between

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<sup>10</sup>In an exploratory analysis, we checked whether the increased (transversal) training participation and changes in job search strategy affected individuals' employment probability. Specifically, we estimated 2SLS regressions in which training participation and searching for a shortage occupation were instrumented by the treatment assignment. The estimates are too noisy to be interpreted with confidence, but they support our conclusion that the changes in training participation and job search induced by the intervention did not allow to significantly increase return into employment.

<sup>11</sup>Appendix A.7 also includes a heterogeneity analysis according to age and nationality. It shows that both young and older individuals were affected similarly in terms of training effects, although older individuals seem to be driving the effects on searching for a job in a shortage occupation. Regarding nationality, it appears that Belgians drive the effects on training, while non-Belgians are equally affected as Belgians in terms of changes in job search focus.

higher and lower educated jobseekers.

The fact that lower-educated jobseekers react more strongly in terms of training participation raises the question of whether jobseekers with different (expected) benefits from training reacted differently. In order to explore this, we perform a heterogeneity analysis where we divide our sample between those with a relatively high employment (respectively training) probability, versus those with a low probability of employment (respectively training). To do so, we use data from the control group to predict, using pre-treatment characteristics, the probability that each individual enters training or employment in the six months that follow the dispatch of the email. We then divide the sample between those with a low (below median) and a high (above median) probability of entering training or employment.

The results are shown in Figure 4 and Figure 5. They suggest that jobseekers who are induced to participate in training through our information intervention have a relatively high ex-ante training probability, but a low ex-ante employment probability. In other words, the information intervention fostered training participation among individuals who were on the margin of participating, but were unlikely to find a job. These are the same groups who reduce their job search intensity, possibly due to the fact that they are more likely to be enrolled in training and thus not actively searching for a job. The fact that training enrolment increased among those with a low employment probability is reassuring, as it indicates that the additional training participants are unlikely to have found a job in the counterfactual, and therefore that the intervention did not divert individuals from employment to training.

Moreover, when looking at effects on job search, we find that those most affected are jobseekers with a relatively low employment propensity. This reinforces our finding that those most affected by the information intervention are likely those who need this type of guidance the most in order to reintegrate the labour market.

Appendix A.7 shows additional heterogeneity analyses supporting our finding that the intervention fostered training participation among individuals who were the least likely to find employment. For example, we show that training effects are strongest among those who were still unemployed at the time of receiving the email and those who had already been unemployed in the past.

Finally, Appendix A.7 also shows a heterogeneity analysis according to whether or not jobseekers were receiving unemployment benefits at the time of treatment. We find that those receiving unemployment benefits are most affected, suggesting that those induced to participate in training are those with the lowest cost of doing so.

## 6 Robustness

This section examines the sensitivity of our findings to alternative sample selection criteria, specifications, and outcome definitions. First, in Table 5, we assess the robustness of our findings to the specific definition used for the training and employment outcomes. In particular, in columns (1) and (2), we exclude relatively short training activities (i.e., those lasting less than five or 30 days, respectively). Similarly, in column (6), we consider only employment spells that last more than five days. In column (3), we check that our estimates do not change depending on whether we consider training completion, rather than enrolment. Finally, columns (4) and (5) consider unemployment exit as an outcome, instead of employment. Across these tests, our findings remain largely consistent with the benchmark results, regardless of the outcome definition used.

Next, Table 6 presents how our estimated effects on training, job search, and employment change under different specifications and sample selections. The first column reports our benchmark results for ease of comparison. Column (2) shows that our benchmark findings remain stable when we include control variables. Column (3) shows that our estimated effects are larger when we limit our sample exclusively to individuals who were still unemployed at the time that the email was sent. This suggests, as expected, that the effects are driven by those for whom the information was most relevant. Next, column (4) presents our estimated effects when including individuals who did not open the email in our sample. The estimated effects become somewhat attenuated when including these individuals since, by definition, non-openers could not have accessed the information and they thus introduce uninformative noise to our estimates. Column (5) reports the result of estimating a LATE (in the fashion of Equation 2), but considering a dummy for opening the email as the instrumented treatment variable. Reassuringly, the estimated effects are very similar to our benchmark findings although they are less precise. Finally, in column (6), we estimate Equation 1, but using only the sample of individuals who did not open the email, and thus where no one was treated. Reassuringly, we find no statistically significant effects in this placebo test, with the exception of the long-term coefficient on employment which is negative (p-value=0.0900).

## 7 Conclusion and Discussion

In this paper, we conduct a large-scale field experiment in the labour market, with the aim of encouraging more unemployed jobseekers to enter occupations that are in shortage. We contact the entire population of nearly 100,000 individuals who enrolled at the Walloon PES



between July and December 2021, and send a randomly selected half of them information about shortage occupations and related training opportunities. Using a combination of survey and administrative data, we follow these individuals for up to 18 months after they receive the information, and investigate whether the intervention affected beliefs, training enrolment, job search, and employment.

We show that the intervention increased training participation, but only in transversal training courses rather than (shortage) occupational training programs. Treated jobseekers also become more likely to position themselves as searching for a job in a shortage occupation. However, neither the increased training participation, nor the change in job search focus, led to an increase in employment. Importantly, our heterogeneity analyses suggest that the individuals who reacted the most to receiving the intervention were those least likely to find employment quickly.

This paper contributes important new findings to the job search (assistance) literature. In particular, our findings demonstrate that jobseekers can be induced to participate in training programs, even through simple and low-cost information messages. Our findings also confirm previous findings indicating that the unemployed can be steered toward occupations that are in higher demand by employers. This is not only illustrated by the fact that our intervention increased the probability to search for jobs in shortage occupations, but also by the fact that many treated individuals clicked on the information links, suggesting some degree of interest in knowing more about shortage occupations. Together, these findings indicate that there is room to foster more reskilling among the unemployed; the question of how to achieve this, however, remains open.

In that respect, the growing body of evidence on job search recommendations offers insights into what interventions are likely to be most effective at steering jobseekers toward shortage occupations, namely through training, in the future. Our intervention was characterized by a non-tailored information intervention, sent by email. In terms of how jobseekers might have interacted with the information received (and acted upon it), the generality of the email meant that jobseekers needed to search through a long list of shortage occupations before they would find more specific information of direct interest to them. Then, they still needed to figure out how to sign up for the training and under what conditions they could do so. The fact that the information was transmitted by email also meant that jobseekers received the advice at a time when their attention was not necessarily focused on finding a job, and they could easily ignore or forget about it. These features are akin to, e.g., Altmann et al. (2018) and Barr and Turner (2018), but differ from Belot et al. (2019) or Le

Barbanchon et al. (2023), who provide jobseekers with tailored information during the job search process. Recommendation tools that are tailored and/or provided within job search platforms appear to have better results on average than those that are not tailored and/or sent by (e)mail; this should be kept in mind when designing future information interventions for jobseekers. A more tailored encouragement design, where jobseekers receive specific training recommendations during their job search process, may promote more substantial reskilling and yield the benefits of occupational training demonstrated in Katz et al. (2022) or Grosz et al. (2020).

In the end, steering job search behaviour might also simply be easier than encouraging jobseekers to enrol in training programs. For a jobseeker, shifting one's job search focus is not a particularly costly change; enrolling in a training lasting several months (or sometimes years), in contrast, is a more substantial investment. Moreover, the capacity to change training behaviour is constrained by the availability (or supply) of the chosen training program, whereas searching for a different kind of job does not rely on an external provider. In our case, assuming that jobseekers might have been interested in only one or two shortage occupations, it is very possible that a training for that specific occupation was not available directly. This might also explain why they chose to follow transversal training courses, which tend to be available on a more regular basis. More broadly speaking, this hints to the potential role of training programs' design (e.g., in terms of availability, or proximity) in explaining why jobseekers do or do not enrol in occupational training. If a relatively large-scale reskilling of the labour force is needed, it is crucial to further our understanding of how to encourage unemployed jobseekers to enrol in occupational training programs that would allow them to enter shortage occupations.

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## 9 Tables

Table 1: Summary Statistics

		Overall	Treated	Control	t-test
<b>Gender</b>	Woman	0.5218 (0.4995)	0.5229 (0.4995)	0.5206 (0.4996)	0.0023 (0.0032)
	Man	0.4782 (0.4995)	0.4771 (0.4995)	0.4794 (0.4996)	-0.0023 (0.0032)
<b>Age</b>	Age	32.6587 (11.2573)	32.6588 (11.2526)	32.6586 (11.2621)	0.0001 (0.0717)
<b>Province</b>	Hainaut	0.3844 (0.4865)	0.3838 (0.4863)	0.3851 (0.4866)	-0.0012 (0.0031)
	Liege	0.2809 (0.4494)	0.2813 (0.4496)	0.2805 (0.4492)	0.0008 (0.0029)
	Luxemburg	0.0823 (0.2749)	0.0819 (0.2743)	0.0827 (0.2755)	-0.0008 (0.0017)
	Namur	0.1489 (0.3560)	0.1494 (0.3564)	0.1485 (0.3556)	0.0008 (0.0023)
	Walloon Brabant	0.0961 (0.2947)	0.0967 (0.2956)	0.0955 (0.2939)	0.0013 (0.0019)
	Other	0.0073 (0.0854)	0.0069 (0.0828)	0.0078 (0.0879)	-0.0009* (0.0005)
<b>Education</b>	Max. Secondary	0.6014 (0.4896)	0.6016 (0.4896)	0.6011 (0.4897)	0.0005 (0.0031)
	Tertiary	0.2558 (0.4363)	0.2548 (0.4358)	0.2568 (0.4369)	-0.0020 (0.0028)
	Other	0.1428 (0.3499)	0.1436 (0.3507)	0.1420 (0.3491)	0.0016 (0.0022)
<b>First-time Unemployed</b>	No	0.7180 (0.4500)	0.7186 (0.4497)	0.7173 (0.4503)	0.0013 (0.0029)
	Yes	0.2820 (0.4500)	0.2814 (0.4497)	0.2827 (0.4503)	-0.0013 (0.0029)
<b>Nationality</b>	Belgian	0.8610 (0.3459)	0.8599 (0.3471)	0.8622 (0.3447)	-0.0023 (0.0022)
	Non-Belgian	0.1390 (0.3459)	0.1401 (0.3471)	0.1378 (0.3447)	0.0023 (0.0022)
<b># Obs</b>		98,844	49,251	49,593	

Note: This table shows the pre-treatment characteristics of individuals included in the experiment. It also shows whether there are any significant difference between the treatment and control group, pre-intervention. \*\*\*  $p < 0.01$  \*\*  $p < 0.05$  \*  $p < 0.1$

Table 2: Engagement with the Email and Treatment Information

	(1) Overall Sample	(2) Treatment Group	(3) Control Group	(4) Difference
Opened email	0.5445 (0.4980)	0.5440 (0.4981)	0.5450 (0.4980)	-0.0010 (0.0032)
Clicked on at least one link	0.1261 (0.3319)	0.1392 (0.3461)	0.1131 (0.3167)	0.0261 *** (0.0028)
Number of clicks	0.1439 (0.4221)	0.1726 (0.4938)	0.1155 (0.3341)	0.0571 *** (0.0036)
Clicked on survey link	0.0945 (0.2925)	0.0781 (0.2683)	0.1108 (0.3139)	-0.0327 *** (0.0025)
Clicked on social network link	0.0026 (0.0510)	0.0019 (0.0440)	0.0033 (0.0571)	-0.0013 *** (0.0004)
Clicked on “Client Service” link		0.0147 (0.1204)		
Clicked on video		0.0165 (0.1274)		
Clicked on list of shortage occupations		0.0466 (0.2108)		
Clicked on information about financial incentive		0.0124 (0.1108)		
Clicked on at least one treatment link		0.0765 (0.2659)		
Number of observations	98,844	49,251	49,593	

Note: This table shows descriptive statistics on email engagement for our overall sample, and the treatment and control groups separately. Statistics on email opening are shown for the entire sample, whereas clicking behaviour is shown for the sample of email openers. The last column shows the difference between treatment and control group means, with stars indicating the statistical significance of these differences. \*\*\*  $p < 0.01$  \*\*  $p < 0.05$  \*  $p < 0.1$



Table 3: Effects on Training Activities

	(1)	(2)	(3)	(4)	(5)	(6)
	Pre-Training	Training	Transversal Training	Occupational Training	Short. Occ. Training	Non-Short. Occ. Training
<b>Short-term (6 months)</b>						
$\beta_1$ (ITT)	0.0012 (0.0020)	0.0036* (0.0021)	0.0036** (0.0016)	-0.0001 (0.0015)	-0.0006 (0.0013)	0.0005 (0.0009)
$\beta_2$ (LATE)	0.0149 (0.0254)	0.0459* (0.0264)	0.0457** (0.0210)	-0.0015 (0.0187)	-0.0080 (0.0162)	0.0059 (0.0110)
Constant	0.0554*** (0.0014)	0.0587*** (0.0015)	0.0358*** (0.0012)	0.0293*** (0.0010)	0.0222*** (0.0009)	0.0097*** (0.0006)
<b>Long-term (12 months)</b>						
$\beta_1$ (ITT)	-0.0008 (0.0026)	0.0036 (0.0022)	0.0035* (0.0018)	-0.0002 (0.0016)	-0.0010 (0.0014)	0.0008 (0.0009)
$\beta_2$ (LATE)	-0.0105 (0.0328)	0.0459 (0.0288)	0.0453* (0.0232)	-0.0026 (0.0206)	-0.0127 (0.0180)	0.0100 (0.0121)
Constant	0.0976*** (0.0018)	0.0711*** (0.0016)	0.0443*** (0.0013)	0.0359*** (0.0011)	0.0276*** (0.0010)	0.0118*** (0.0007)
Observations	53,822	53,822	53,822	53,822	53,822	53,822

Note: This table shows the estimated effect of the intervention, using Equation 1 (ITT) and Equation 2 (LATE), on training enrolment, six and 12 months after the intervention. “Pre-Training” includes any activity that takes place before an actual training, such as information sessions, selection tests, or screenings. “Training” covers any training activity that is not a pre-training activity, and can be subdivided into “Transversal” or “Occupational” training. “Transversal Training” includes training activities that do not target a given occupation or sector, while “Occupational Training” includes programs focusing on the acquisition of skills specific to an occupation or sector. Finally, “Short. Occ. Training” encompasses occupational trainings that target shortage occupations, while “Non-Short. Occ. Training” includes occupational trainings that target occupations that are not in shortage. \*\*\*  $p < 0.01$  \*\*  $p < 0.05$  \*  $p < 0.1$

Table 4: Effects on Employment and Job Search

	(1) Employment	(2) Search Short. Occ.	(3) Number Occ. Searched	(4) Connections to PES Website
<b>Short-term (6 months)</b>				
$\beta_1$ (ITT)	0.0024 (0.0043)	0.0099** (0.0042)	0.0221 (0.0189)	-0.1268** (0.0564)
$\beta_2$ (LATE)	0.0311 (0.0553)	0.1267** (0.0544)	0.2835 (0.2421)	-1.6276** (0.7247)
Constant	0.5161*** (0.0030)	0.5881*** (0.0030)	3.2871*** (0.0133)	1.6747*** (0.0398)
<b>Long-term (18 months)</b>				
$\beta_1$ (ITT)	-0.0009 (0.0039)	0.0114*** (0.0042)	0.0314 (0.0208)	-0.1573 (0.1331)
$\beta_2$ (LATE)	-0.0113 (0.0505)	0.1461*** (0.0539)	0.4034 (0.2664)	-2.0200 (1.7081)
Constant	0.7049*** (0.0028)	0.6120*** (0.0030)	3.6304*** (0.0146)	5.4415*** (0.0939)
Observations	53,822	53,822	53,822	53,822

Note: This table shows the estimated effect of the intervention, using Equation 1 (ITT) and Equation 2 (LATE), on employment and job search behaviour. “Employment” is defined as having experienced at least one employment spell since the intervention. “Search Short.Occ.” is defined as listing at least one shortage occupation in one’s occupational preferences. “Number Occ. Searched” is the total number of occupations listed as occupational preferences. “Connections to PES Website” is the number of times a jobseekers logs into their personal space on the PES website (a proxy of job search intensity). \*\*\*  $p < 0.01$  \*\*  $p < 0.05$  \*  $p < 0.1$

Table 5: Robustness Checks – Alternative Outcome Definition

	(1) Training (> 5 days)	(2) Training (> 30 days)	(3) Finish Training	(4) Leave UI	(5) Leave UI (> 5 days)	(6) Employment (> 5 days)
<b>Short-term estimates</b>						
$\beta_1$ (ITT)	0.0035* (0.0018)	0.0029* (0.0016)	0.0036** (0.0017)	0.0018 (0.0042)	0.0012 (0.0043)	-0.0014 (0.0042)
Constant	0.0459*** (0.0013)	0.0354*** (0.0011)	0.0405*** (0.0012)	0.6054*** (0.0030)	0.5631*** (0.0030)	0.3902*** (0.0030)
<b>Long-term estimates</b>						
$\beta_1$ (ITT)	0.0040** (0.0020)	0.0027 (0.0018)	0.0033* (0.0019)	-0.0010 (0.0034)	-0.0005 (0.0037)	-0.0023 (0.0042)
Constant	0.0561*** (0.0014)	0.0444*** (0.0013)	0.0496*** (0.0013)	0.8039*** (0.0024)	0.7604*** (0.0026)	0.6193*** (0.0030)
Observations	53,822	53,822	53,822	53,822	53,822	53,822

Note: This table shows the sensitivity of our benchmark ITT estimates to using alternative definition for our training and employment outcomes. “Training (> 5 days)” considers only training activities that are longer than five days, while “Training (> 30 days)” considers activities lasting over 30 days. “Finish Training” considers only training activities that were actually completed, as opposed to training enrolment used in our benchmark results. “Leave UI” and “Leave UI (> 5 days)” are alternative outcomes for employment, and are dummies equal to one if the individual is observed leaving unemployment (for at least five days in the latter case). Finally, “Employment (> 5 days)” considers only employment spells that lasted more than five days. \*\*\*  $p < 0.01$  \*\*  $p < 0.05$  \*  $p < 0.1$

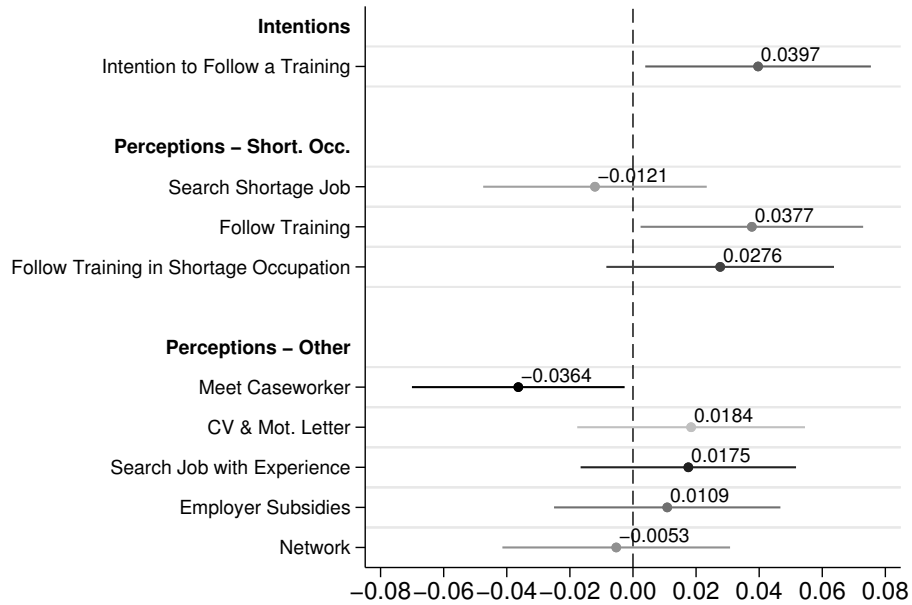
Table 6: Robustness Checks – Alternative Sample Selection and Specification

	(1)	(2)	(3)	(4)	(5)	(6)
	Benchmark	With controls	Unemployed in $t = 0$	Keeping non-openers	LATE of opening email	Placebo non-openers
<b>Short-term estimates</b>						
Pre-Training	0.0012 (0.0020)	0.0014 (0.0020)	0.0030 (0.0029)	0.0010 (0.0014)	0.0018 (0.0025)	0.0008 (0.0018)
Any Training	0.0036* (0.0021)	0.0038* (0.0021)	0.0051* (0.0029)	0.0018 (0.0014)	0.0033 (0.0026)	-0.0003 (0.0019)
Transversal Training	0.0036** (0.0016)	0.0036** (0.0016)	0.0048** (0.0024)	0.0015 (0.0011)	0.0027 (0.0020)	-0.0010 (0.0014)
Occupational Training	-0.0001 (0.0015)	0.0001 (0.0015)	-0.0003 (0.0020)	-0.0003 (0.0010)	-0.0005 (0.0018)	-0.0005 (0.0013)
SO Training	-0.0006 (0.0013)	-0.0004 (0.0013)	-0.0019 (0.0018)	-0.0008 (0.0009)	-0.0014 (0.0016)	-0.0009 (0.0012)
Non-SO Training	0.0005 (0.0009)	0.0005 (0.0009)	0.0007 (0.0012)	0.0005 (0.0006)	0.0009 (0.0011)	0.0005 (0.0007)
Employment	0.0024 (0.0043)	0.0021 (0.0043)	-0.0000 (0.0055)	0.0016 (0.0032)	0.0029 (0.0058)	0.0007 (0.0047)
Search Short. Occ.	0.0099** (0.0042)	0.0108*** (0.0039)	0.0090* (0.0054)	0.0065** (0.0031)	0.0119** (0.0057)	0.0023 (0.0046)
Number Occ. Searched	0.0221 (0.0189)	0.0269 (0.0172)	0.0284 (0.0241)	-0.0025 (0.0139)	-0.0045 (0.0256)	-0.0320 (0.0207)
Number connections	-0.1268** (0.0564)	-0.1227** (0.0564)	-0.1199 (0.0801)	-0.0499 (0.0363)	-0.0918 (0.0667)	0.0432 (0.0423)
<b>Long-term estimates</b>						
Pre-Training	-0.0008 (0.0026)	-0.0006 (0.0025)	-0.0014 (0.0035)	-0.0003 (0.0018)	-0.0005 (0.0033)	0.0004 (0.0024)
Any Training	0.0036 (0.0022)	0.0042* (0.0022)	0.0058* (0.0032)	0.0013 (0.0015)	0.0024 (0.0028)	-0.0013 (0.0021)
Transversal Training	0.0035* (0.0018)	0.0038** (0.0018)	0.0055** (0.0026)	0.0011 (0.0012)	0.0021 (0.0023)	-0.0017 (0.0016)
Occupational Training	-0.0002 (0.0016)	0.0003 (0.0016)	-0.0004 (0.0022)	-0.0005 (0.0011)	-0.0010 (0.0020)	-0.0009 (0.0015)
SO Training	-0.0010 (0.0014)	-0.0006 (0.0014)	-0.0021 (0.0020)	-0.0011 (0.0010)	-0.0020 (0.0018)	-0.0012 (0.0013)
Non-SO Training	0.0008 (0.0009)	0.0009 (0.0009)	0.0009 (0.0013)	0.0006 (0.0006)	0.0012 (0.0012)	0.0005 (0.0009)
Employment	-0.0009 (0.0039)	-0.0020 (0.0038)	-0.0015 (0.0053)	-0.0039 (0.0029)	-0.0072 (0.0054)	-0.0075* (0.0044)
Search Short. Occ.	0.0114*** (0.0042)	0.0121*** (0.0038)	0.0120** (0.0054)	0.0076** (0.0031)	0.0140** (0.0057)	0.0031 (0.0045)
Number Occ. Searched	0.0314 (0.0208)	0.0348* (0.0192)	0.0332 (0.0265)	0.0006 (0.0155)	0.0012 (0.0284)	-0.0362 (0.0231)
Number connections	-0.1573 (0.1331)	-0.1689 (0.1327)	-0.2646 (0.1900)	-0.0703 (0.0848)	-0.1292 (0.1558)	0.0374 (0.0960)
Observations	53,822	53,709	32,464	98,844	98,844	45,022

Note: This table shows the sensitivity of our benchmark ITT estimates to: the inclusion of control variables (column 2), the exclusion of individuals who were not unemployed anymore at the time of receiving the treatment (column 3), and the inclusion of individuals who did not open the email (column 4). Column 5 also shows the results of estimating a LATE (in the fashion of Equation 2), where the treatment is instrumented by a indicator for opening the email. Column 6 shows the results of a placebo test using only individuals who did not open the email. \*\*\*  $p < 0.01$  \*\*  $p < 0.05$  \*  $p < 0.1$

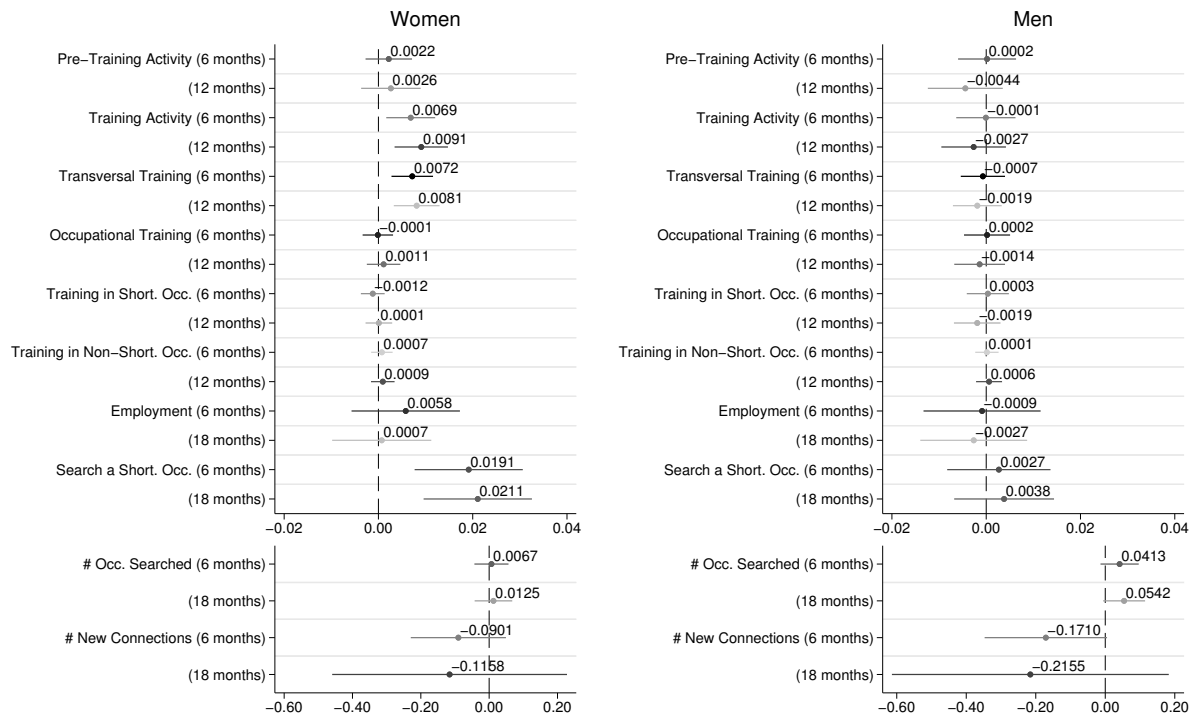
# 10 Figures

Figure 1: Effects on Beliefs and Intentions



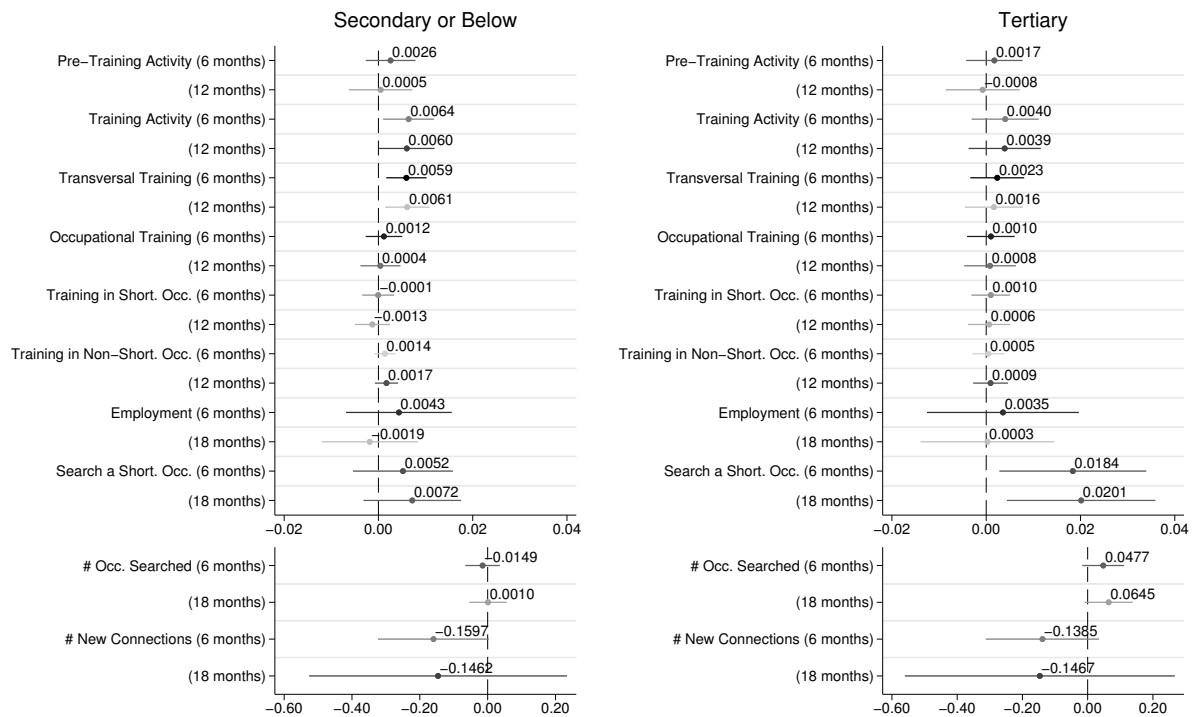
Notes: This figure shows the estimated effect, using Equation 1, of being sent the information treatment on training intentions and perceptions about the usefulness of various job search strategies. “Intentions” show treatment effects on the probability of answering “yes” when asked about having the intention of enrolling in a training program in the coming year. “Perceptions – Shortage Occupations” refer to the probability of considering job search strategies related to shortage occupations (i.e., searching for a job in a shortage occupation, following a training, and following a training in a shortage occupation) as having a high or very high impact on job finding probability. “Perceptions – Other Strategies” refer to the probability of considering job search strategies unrelated to shortage occupations as having a high or very high impact on job finding probability. The estimates are shown without control variables. Confidence intervals are shown at 95%.

Figure 2: Heterogeneous Effects - Gender



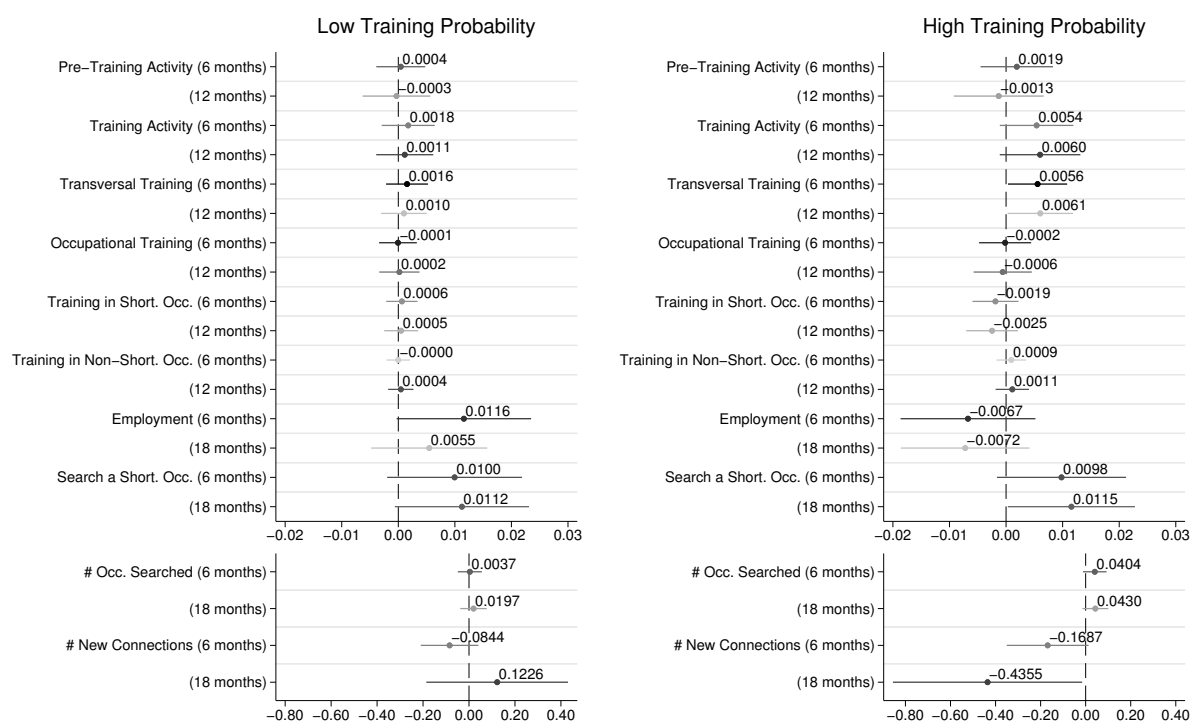
Notes: This figure shows the estimated effects of the intervention, using Equation 1, for women (left panel) and men (right panel) separately. Confidence intervals are shown at 95%.

Figure 3: Heterogeneous Effects - Education



Notes: This figure shows the estimated effects of the intervention, using Equation 1, for individuals with different levels of education. “Secondary or Below” includes all individuals who have completed at most upper secondary education. “Tertiary” includes all individuals who have completed post-secondary education (university or not). Confidence intervals are shown at 95%.

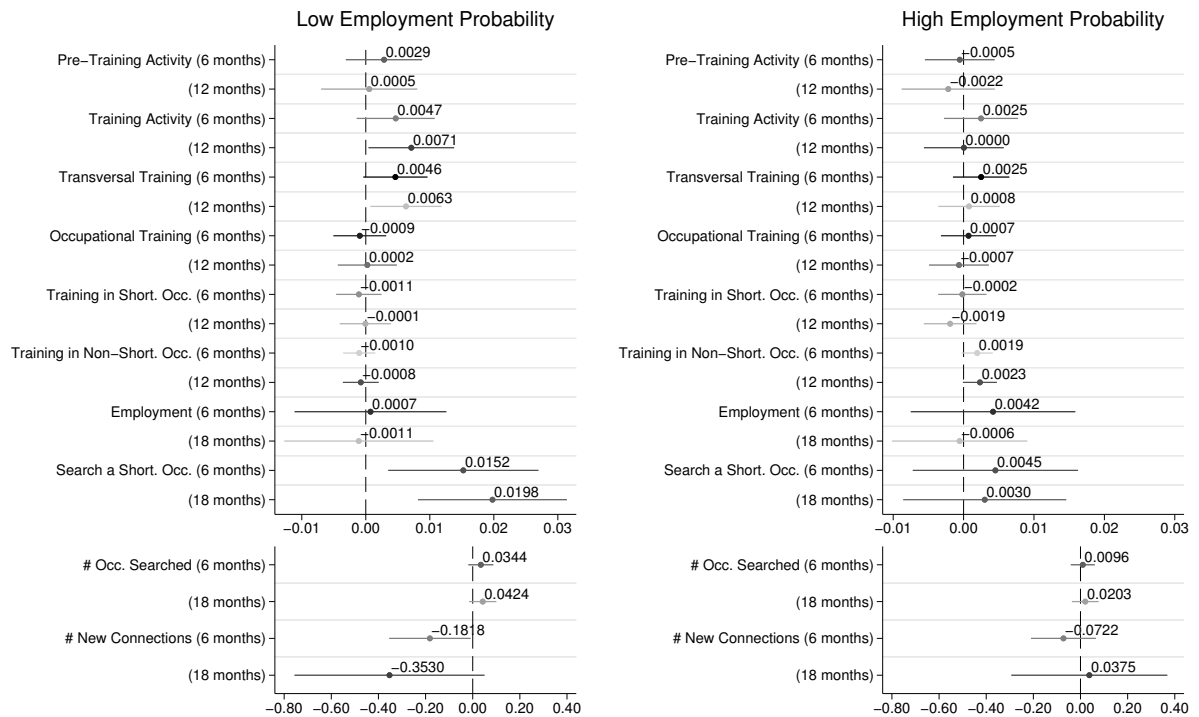
Figure 4: Heterogeneous Effects - Ex-ante Training Probability



Notes: This figure shows the estimated effects of the intervention, using Equation 1, for individuals with an ex-ante low (left panel) versus high (right panel) probability of enrolling in training. Confidence intervals are shown at 95%.



Figure 5: Heterogeneous Effects - Ex-ante Employment Probability



Notes: This figure shows the estimated effects of the intervention, using Equation 1, for individuals with an ex-ante low (left panel) versus high (right panel) employment probability. Confidence intervals are shown at 95%.

# Appendix

## A.1 Shortage Occupations in Wallonia

Every year, the Walloon PES identifies the list of occupations that are currently in shortage in the region. This list is made using two types of tools: statistical evidence from employer surveys and qualitative expert knowledge. The first tool consists in a yearly survey in which employers are asked about whether or not (and how quickly) they were able to fill job openings for different types of occupations. For this survey, the PES exclusively contacts employers who have published at least one vacancy during that year. The PES considers occupations to be in shortage if (i) employer satisfaction with the job filling for that occupation is below the median, and (ii) the average time to fill the opening is above the median. The list that is made on the basis of the employer survey is then submitted to the opinion of experts who work within the PES or in training centres. Experts can then add or remove occupations from the list based on their knowledge of the field.

The PES also makes a distinction between “critical occupations”, defined as those for which employers face strong hiring difficulties, and “shortage occupations”, defined as critical occupations for which there is a quantitative shortage of candidates with the appropriate qualifications. A quantitative shortage is defined as there being less than 15 unemployed jobseekers with the appropriate qualification for every ten job openings in an occupation. In the paper, we grouped “critical occupations” and “shortage occupations” together, and simply referred to them as shortage occupations.

The table below lists all the occupations that were considered to be in shortage in Wallonia in 2021 and 2022, when our experiment took place.

Table A1: List of Shortage Occupations

Occupation	Shortage	Shortage
	2021	2022
Accountant	X	X
Accounting Expert	X	X
Agricultural and Technical Mechanic	X	X
Air Conditioning/Ventilation Fitter	X	X
Aircraft Maintainer	X	
Architect	X	
Army Officer	X	
Automation Technician	X	X
Automotive Maintenance and Diagnostic Technician (MDA)	X	X
Baker	X	X

Barman		X
Building Maintenance Worker	X	
Bus Driver	X	X
Business Analyst	X	
Business Engineer	X	X
Butcher	X	X
Car Repairer	X	X
Caregiver	X	X
Carpenter	X	X
Cementer	X	X
Chef/Cook	X	X
Civil Engineering Mechanic	X	X
Coach Driver	X	X
Commercial Agent (Real Estate Sector)	X	X
Construction and Road Maintenance Equipment Operator	X	X
Construction Draftsman	X	X
Construction Supervisor/Manager	X	X
Construction Worker		X
Customs Agent	X	X
Data Scientist / Analyst		X
Dispatcher in Transport and Logistics	X	X
Domestic Helper	X	X
Driving Instructor		X
Earthmoving Equipment/Machine Operator	X	X
Electrical Installer/Maintenance Technician	X	X
Electromechanic	X	X
Electronics Maintenance Technician	X	X
Energy Consultant		X
Facilities Manager in Food Industry	X	X
Fast-food Restaurant Manager		X
Financial/Credit Analyst/Advisor	X	X
Florist		X
Gardener	X	X
General Practitioner (GP)	X	X
General/Specialist Nurse	X	X
Glassmaker	X	X
Hairdresser	X	X
Healthcare Assistant	X	X
Heating Maintenance and Operation Technician	X	X
Industrial and Logistics Manager	X	X
Industrial Cleaner		X
Industrial Installation and Maintenance Technician/Manager	X	X
Industry Planning Officer		X

Information Systems Administrator	X	X
Insulation Worker	X	X
Insurance Advisor	X	X
IT Analyst	X	X
IT Developer	X	X
IT Project Manager	X	X
Kitchen Installer	X	X
Laboratory Control Technician	X	X
Logistics Operations / Warehouse Manager	X	X
Logopedist	X	X
Machines Technician (Metal)	X	X
Manufacturing Operator in the Chemical Industry		X
Mason	X	X
Mechanical Fitter	X	
Medical Imaging Technologist		X
Metal Worker	X	X
Motion Designer	X	
Mover		X
Multi-skilled/Truck mechanic	X	X
(Industrial) Painter	X	X
Partitions/False Ceilings Fitter	X	X
Pastry Chef	X	X
Physics or Chemical Transformation Equipment Operator	X	X
(Industrial) Pipe Fitter	X	X
Plasterer	X	X
Plumbing and Heating Fitter	X	X
Police Inspector		X
Prevention Advisor	X	
Production line operator (food industry)	X	X
Production Manager/Technician	X	X
Production/Quantity Planner	X	X
Property/Buildng Manager	X	X
Quality and Regulations Manager	X	X
R&D Manager	X	X
(Hotel) Receptionist		X
Refrigeration Technician	X	X
Repointer/Brick Sealer	X	
Research Laboratory Technician	X	X
Retail Store Manager		X
Road Worker	X	X
Roof Builder	X	X
Sales Assistant	X	
Sales Manager	X	X

Sales Representative for consumer goods	X	X
Sales Representative for professional equipment goods	X	X
Sales Representative in business services		X
Scaffolder	X	
Security Guard	X	X
Security System Installer	X	X
Slaughterer / Meat Processing Worker	X	X
Sports Instructor		X
Swimming Pool Installer		X
Technical Compliance Officer	X	
Technical Sales Representative	X	X
Technician in the Performing Arts and Events		X
Textile Manufacturing Machine Operator	X	X
Tiler	X	X
Tourism Product Manager		X
Tower Crane Operator	X	X
Travel Agent		X
Truck Driver	X	X
Valet / Chambermaid / Housekeeper		X
(Head) Waiter	X	X
Warehouse Worker/Manager	X	X
Waste Sorter		X
Waterproofing Contractor	X	X
Web Developer	X	X
Welder	X	X

## A.2 Conditions for Job Search Exemptions While in Training

Jobseekers are exempted from their job search obligations when they follow a training. To receive this exemption, they must fulfil the following obligations:

1. Be a UI recipient living in Wallonia.
2. Have finished their last studies (preceding the first receipt of UI benefits) for at least 2 years.
3. Have been unemployed for at least 312 days over the past two years, except if the training considered is for a shortage occupation.
4. Not have a higher education (post-secondary) degree, except if this degree offers few opportunities on the labour market.
5. Not have already benefited from an exemption to pursue a training (except for prolongation requests).

On top of the fact that jobseekers are exempted from their job search obligations during the duration of their training and therefore keep the right to receive UI benefits, they are also entitled to a travel expense allowance and extra childcare allowances. Moreover, if they succeed the training, they are offered a €350-2000 bonus and receive special coaching to prepare them for interviews.

During the period of the experiment, unemployment benefits in Belgium were largely unlimited in time. Therefore, enrolling in training is not a potential way of extending benefit duration in our context.

### A.3 Treatment and Control Emails

Figure A1: Control Group Email



Figure A2: Treatment Group Email





## A.4 Selection into the Survey

Table A2: Summary Statistics – Email Openers and Survey Clickers

		Overall Sample	Opened Email	Clicked on survey
<b>Gender</b>	Woman	0.5218 (0.4995)	0.5403 (0.4984)	0.5791 (0.4938)
	Man	0.4782 (0.4995)	0.4597 (0.4984)	0.4209 (0.4938)
<b>Age</b>	Age	32.6587 (11.2573)	33.0565 (11.5651)	37.9538 (12.7970)
<b>Province</b>	Hainaut	0.3844 (0.4865)	0.3739 (0.4838)	0.3808 (0.4856)
	Liege	0.2809 (0.4494)	0.2851 (0.4515)	0.2854 (0.4516)
	Luxemburg	0.0823 (0.2749)	0.0828 (0.2756)	0.0786 (0.2692)
	Namur	0.1489 (0.3560)	0.1485 (0.3556)	0.1463 (0.3534)
	Walloon Brabant	0.0961 (0.2947)	0.1016 (0.3021)	0.1022 (0.3029)
	Other	0.0073 (0.0854)	0.0081 (0.0894)	0.0068 (0.0820)
<b>Education</b>	Max. Secondary	0.6014 (0.4896)	0.5658 (0.4957)	0.5182 (0.4997)
	Tertiary	0.2558 (0.4363)	0.2749 (0.4465)	0.2433 (0.4291)
	Other	0.1428 (0.3499)	0.1593 (0.3659)	0.2385 (0.4262)
<b>First-time Unemployed</b>	No	0.7180 (0.4500)	0.7003 (0.4581)	0.7494 (0.4334)
	Yes	0.2820 (0.4500)	0.2997 (0.4581)	0.2506 (0.4334)
<b>Nationality</b>	Belgian	0.8610 (0.3459)	0.8386 (0.3679)	0.7619 (0.4259)
	Non-Belgian	0.1390 (0.3459)	0.1614 (0.3679)	0.2381 (0.4259)
<b># Obs</b>		98,844	53,822	5,179

Table 2 shows the characteristics of individuals who opened the email (column 2) and clicked on the survey (column 3) compared to the overall sample (column 1). First, it shows that women are more likely to open the email than men and, conditional on opening, even more likely to answer the survey. Second, older people are slightly more likely to open the email, but much more likely to answer the survey than younger individuals, conditional on opening. Third, people with a higher level of education (tertiary or university degree) are slightly more likely to open the email than individuals with a lower level of education (secondary degree or less). Those most likely to open and, especially, click on the survey are those with an “Other” degree, i.e., those with an apprentice degree or a diploma from outside the EU. Fourth, non-Belgians appear to be overrepresented among email openers, and even more so among survey clickers. Fifth, individuals who had already been unemployed in the past are slightly less likely to open the email but, conditional on opening, they are relatively more likely to answer the survey than the first-time unemployed. Finally, there does not seem to be any selection of email openers and survey clickers on the basis of where individuals live. Overall, we can thus conclude that women, non-Belgians, and older individuals are generally overrepresented among those who open the email and click on the survey compared to the overall sample.

## A.5 Description of Survey Questions

The intervention took place in the context of a satisfaction survey which was sent to recently unemployed jobseekers. The invitation to complete the survey was sent to both the treated and the control groups, meaning we were able to add several questions to the survey in order to measure “first-stage” effects on beliefs and intentions about training and job search. A challenge here was to obtain information on perceptions about shortage occupations and related trainings from both the treated and control groups, while avoiding as much as possible to inadvertently prime the control group towards thinking about shortage occupations. With this in mind, we attempted to dilute questions on shortage occupations with questions on unrelated topics.

Specifically, we added three questions to the survey. First, we asked jobseekers whether they had the intention of enrolling in any training in the coming year, to which they could answer yes or no. This question was intended to determine to what extent the treatment email had affected the training intentions of treated jobseekers; we expect treated jobseekers to be relatively more interested in training than those not treated. We purposefully did not ask about training for shortage occupations to avoid inadvertently treating individuals from the control group, who would otherwise have been primed to think about shortage occupations. We coded this outcome as a dummy variable equal to one if the individual answered that they had the intention of following a training, and zero otherwise.

Second, we wanted to determine whether the intervention had affected perceptions (or beliefs) about how useful it is to focus one’s job search strategy on shortage occupations and (related) trainings. We asked jobseekers how big of an impact they thought a given search strategy would have on their chances of finding a job. They could choose between “no impact”, “little impact”, “average impact”, “high impact”, and “very high impact”. We asked them to evaluate the following job search strategies: (i) frequently meeting with one’s caseworkers, (ii) following a coaching session to have a good CV and motivation letter, (iii) focusing one’s job search on shortage occupations, (iv) using employer subsidies, (v) focusing one’s job search on occupations in which they have experience, (vi) following trainings, (vii) exploiting one’s professional network, and (viii) following a training in a shortage occupation. The options we expected to be affected by our information treatment were options (iii), (vi), and (viii), i.e., those relating to shortage occupations and/or training activities. When building these outcomes variables, we coded dummy variables equal to one if the respondent considered a given strategy to have a “high impact” or “very high impact” on their chances of finding a job, and zero if they answered it had “no impact”, “little impact” or an “average impact”.

Third, we asked jobseekers to mark their degree of agreement with a series of statements on how supported by the PES they felt. Specifically, we asked them to state whether they “completely disagreed”, “disagreed”, “had mixed views”, “agreed” or “agreed strongly” with four statements. The statements related to the support offered to jobseekers by the PES in terms of (i) number of training opportunities offered, (ii) financial help when seeking training, (iii) waiver of job search requirements when enrolled in training, and (iv) number of job openings sent to them.

Finally, we also asked respondents from the treatment group whether they had read the information on shortage occupations in the email. Our survey showed that, among those who answer the survey, only 56.7% of individuals assigned to the treatment group had actually read the treatment information before answering the survey. This could be due to the fact that the survey link was located above the treatment information in the email; respondents might have therefore clicked on the survey before looking further in the content of the email.

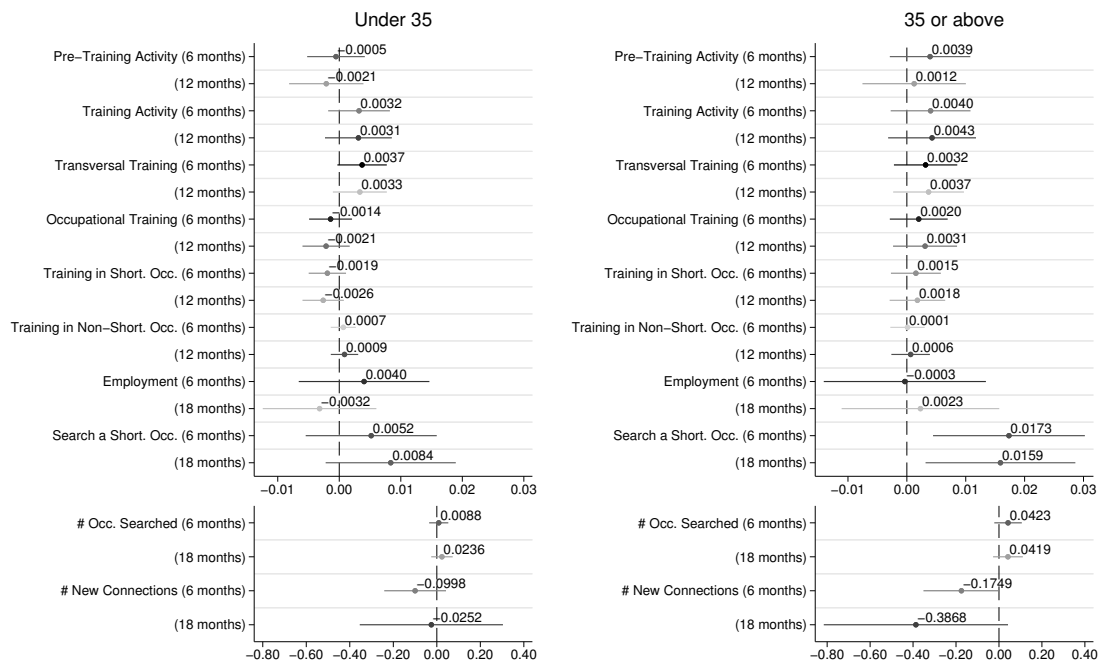
## A.6 Additional Robustness

Table A3: Effects on Training Intentions and Perceptions – With Control Variables

	<b>Intentions</b>	<b>Perceptions</b>							
	Follow a training	(1) Search Shortage Occupation	(2) Training	(3) Training in Shortage Occupation	(4) Meet with Caseworker	(5) CV and Mot. Letter Coaching	(6) Search Job Experienced	(7) Use Employer Subsidies	(8) Use Network
$\beta_1$	0.0334* (0.0178)	-0.0094 (0.0181)	0.0413** (0.0180)	0.0315* (0.0184)	-0.0343** (0.0172)	0.0207 (0.0184)	0.0215 (0.0174)	0.0118 (0.0183)	0.0000 (0.0182)
Constant	0.550*** (0.0117)	0.409*** (0.0116)	0.589*** (0.0115)	0.476*** (0.0118)	0.337*** (0.0110)	0.486*** (0.0118)	0.655*** (0.0112)	0.440*** (0.0117)	0.518*** (0.0118)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,049	3,049	3,049	3,049	3,049	3,049	3,049	3,049	3,049

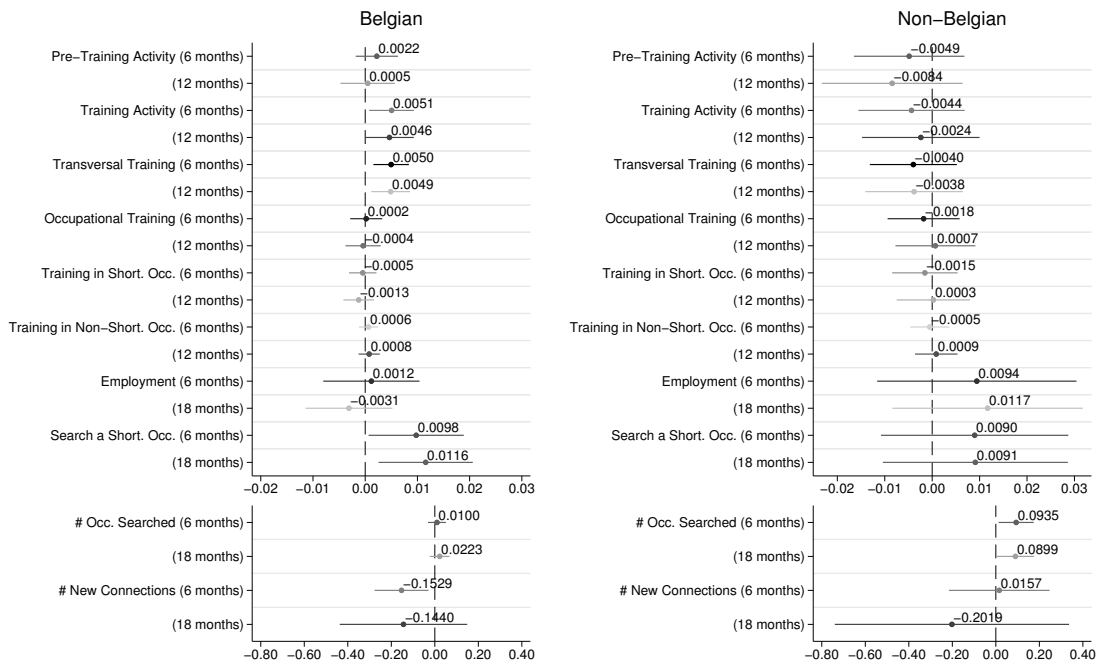
## A.7 Additional Heterogeneity Analyses

Figure A3: Heterogeneous Effects - Age



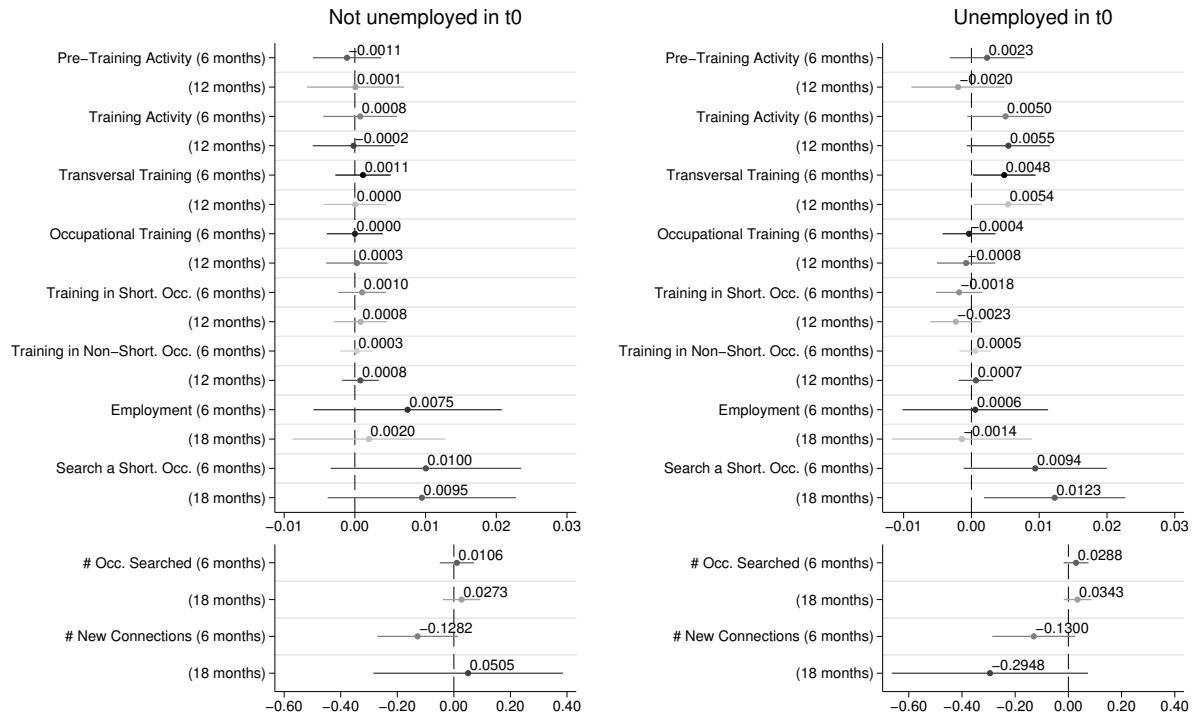
Notes: This figure shows the estimated effects of the intervention, using Equation 1, for individuals aged under 35 (left panel) and those aged 35 or above (right panel). Confidence intervals are shown at 95%.

Figure A4: Heterogeneous Effects - Nationality



Notes: This figure shows the estimated effects of the intervention, using Equation 1, for individuals with the Belgian nationality (left panel) and those with a non-Belgian nationality (right panel) separately. Confidence intervals are shown at 95%.

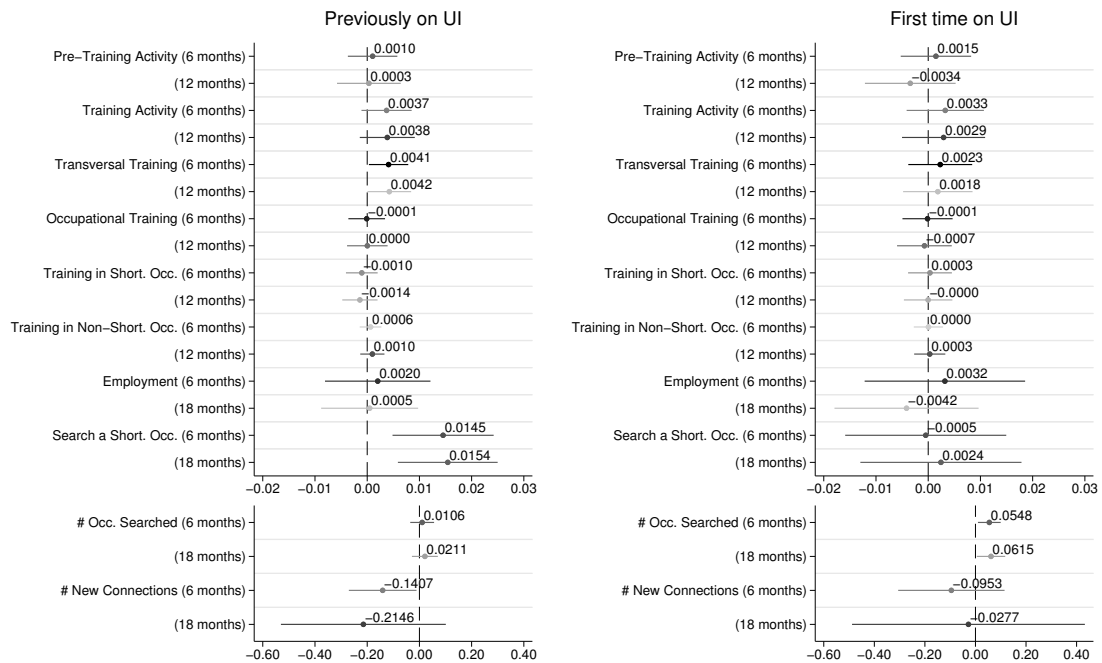
Figure A5: Heterogeneous Effects - Labour market position in  $t = 0$



Notes: This figure shows the estimated effects of the intervention, using Equation 1, for individuals who were not unemployed anymore in  $t = 0$  (left panel) and those who were unemployed in  $t = 0$  (right panel). Confidence intervals are shown at 95%.

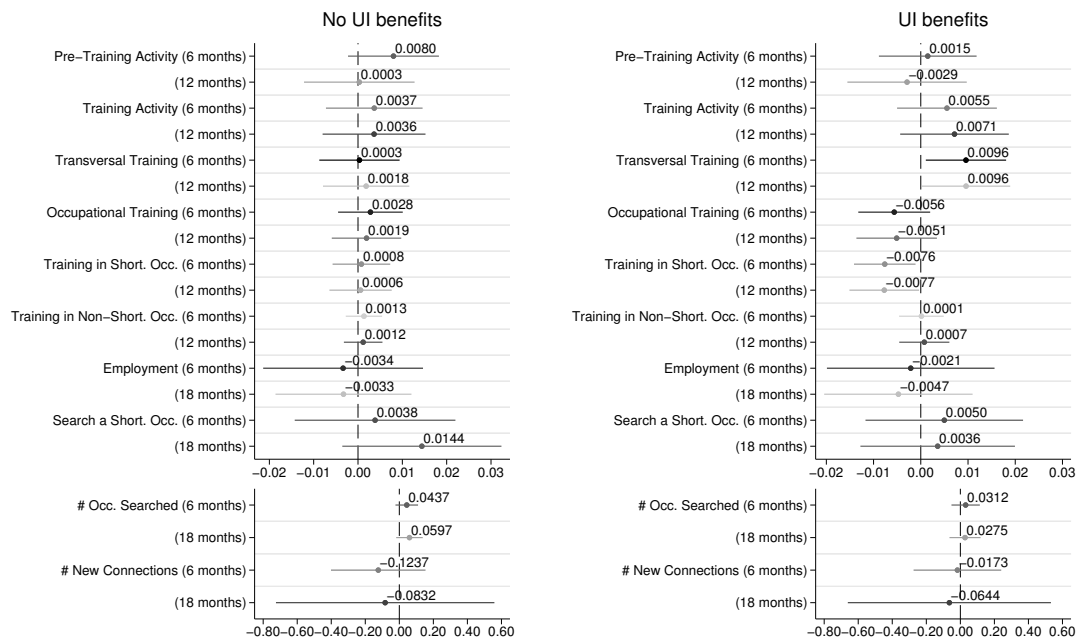


Figure A6: Heterogeneous Effects - Previously unemployed



Notes: This figure shows the estimated effects of the intervention, using Equation 1, for individuals who had already been unemployed in the past (left panel) and those who became unemployed for the first time (right panel). Confidence intervals are shown at 95%.

Figure A7: Heterogeneous Effects - Unemployment Benefits



Notes: This figure shows the estimated effects of the intervention, using Equation 1, for individuals who were receiving unemployment benefits in  $t = 0$  (left panel) and those who were not receiving unemployment benefits in  $t = 0$  (right panel). Confidence intervals are shown at 95%.