Training Jobseekers to Address Labour Shortages: An Experimental Study on Information Barriers*

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Preliminary draft. Please do not share.

Abstract

We explore whether better information can induce more jobseekers to reskill in occupations that are in shortage. We implement a large-scale field experiment in cooperation with a Public Employment Service, in which we inform 100,000 recently unemployed jobseekers about shortage occupations and available training programs. Using survey and administrative data, we estimate the effect of the information intervention on training intentions and enrolment, as well as job search and employment. Our findings show that although the treatment email generated interest in trainings and increased intentions to enrol, it had limited effects on actual training and job search behaviour, and no effect on employment.

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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 thus attempted to tackle this issue, namely through adopting targeted immigration laws, encouraging students to specialize in growing sectors, or retraining jobseekers to meet employers' needs. The latter policy is particularly attractive 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" training programs – i.e., training programs aiming at filling existing needs of local employers – to unemployed jobseekers.[1] In this paper, we focus on such policies and ask whether labour shortages can be addressed through a low-cost information intervention on shortage occupations and related trainings. Using a large-scale field experiment, we show that although our intervention affected perceptions and training intentions of unemployed jobseekers, these changes did not translate into actual behavioural changes in terms of training enrolment and subsequent employment six months after the intervention.

Our choice to study information frictions stems from a growing literature showing that jobseekers are not perfectly informed about the labour market in which they are searching for a job, but that simple, low-cost information interventions can help them to search more effectively. It has indeed been shown that unemployment insurance (UI) recipients tend to be overoptimistic about the speed at which they will find a new job (Spinnewijn, 2015; Mueller et al., 2021), and search in occupations with relatively few vacancies (Sahin et al., 2014; Patterson et al., 2016). But these biases can be addressed through improved (access to) information. Most notably in this vein, Belot et al. (2019) demonstrate that encouraging jobseekers to broaden their search towards similar jobs that are in higher demand increases the number of interviews they are invited to. Other papers also show that informing UI recipients about job search strategies and the consequences of unemployment (Altmann et al., 2018), supplementing their job search skills with editable resume and cover letter templates (Briscese et al., 2021), or training them to use LinkedIn (Wheeler et al., 2022) can also increase their job-finding rate and earnings.

[1] The most notorious might be New Orleans Office of Workforce Development's "Career Pathways", a program that offered training for unemployed and underemployed individuals relevant for medium-skilled jobs in the advanced manufacturing, health care, and information technology (IT) sectors.

The existing research on information frictions and job search thus provides a powerful finding: that improving information about the labour market and effective job search strategies can boost employment prospects of unemployed jobseekers at an extremely low cost. However, this is true only to the extent that jobseekers already possess the necessary skills to fill the available vacancies. Some individuals' skills might have become obsolete as a result of changing skill requirements, while others might simply not dispose of the appropriate qualifications to find a job on the current labour market. In this respect, Katz et al. (2020) show that sectoral training programs targeting low-wage workers generate substantial and persistent earnings gains for trained individuals. Similarly, Baird et al. (2022) show that a job training program, aimed at helping lower-skilled unemployed and underemployed individuals to enter skilled jobs in high-demand fields, had positive impacts on individuals' wage growth and job satisfaction (although it did not increase their employment probability). If unemployed jobseekers do not possess the necessary skills to fill the open vacancies, training could thus be a crucial step in bridging the gap between labour shortages and unemployment, while also improving future labour market outcomes of unemployed jobseekers.

This suggests that, in addition to knowing that some occupations are in higher demand than others, jobseekers also need to know about what training opportunities are available to them and under which conditions. Using a field experiment set up in cooperation with a Public Employment Service (PES) in Belgium, we explore information frictions that unemployed jobseekers might face in relation with the existence of shortage occupations, as well as related training opportunities. The experiment took place in the context of a satisfaction survey that is periodically carried out by the PES. Emails asking recent unemployed jobseekers to fill in the survey were sent to the entire population of individuals having enrolled as UI recipients between July 2021 and January 2022 (roughly 100,000 individuals), but only a randomly selected half of them received the information on shortage occupations and related trainings. The treatment email provided jobseekers with information about shortage occupations, the availability of trainings offered by the PES, and the attractive conditions under which they could enrol. First, it stated that searching for a job in shortage occupations yields higher chances of finding a job, and referred jobseekers to the list of occupations that suffer from such shortages. Second, it stated that the PES offers many training opportunities in these shortage occupations, to which jobseekers can enrol for free, and referred them to the relevant service if they wanted to get more information. Third, the email underlined the attractive conditions under which they could follow these trainings, namely that jobseekers who are enrolled in such trainings are exempted from job search requirements while keeping their entitlement to UI benefits, and are eligible for several benefits, including a financial

incentive of €350 or €2,000 (depending on the training) at completion.

Our findings show that the treatment information affected training intentions, as well as perceptions about how useful (shortage occupations-related) training programs are as a job search strategy. We find that individuals who received the treatment information were 3.97 percentage points (pp) more likely than the control group to state that they intended on enrolling in a training program in the upcoming year (which represents a 7.21% increase in relative terms). We show that the treatment effect on training intentions is likely the result of the email leading treated jobseekers to put more emphasis on trainings (namely those in shortage occupations) as an effective job search strategy. We indeed show that individuals who were sent the treatment email were 3.77 pp (7.74%) more likely than the control group to consider training as an effective job search strategy, and 2.76 pp (5.80%) more likely to consider training in a shortage occupation as an impactful strategy (although this last effect is not statistically significant). Interestingly, the treatment email did not affect perceptions about the importance of searching for a job in a shortage occupation, suggesting that the reader put greater emphasis on the information on trainings than on shortage occupations.

These effects on intentions and perceptions appear to only slightly increase training enrolment six months after the intervention. We indeed find that enrolment in long training programs increase by 0.36 pp, or 8.5% with respect to the mean enrolment in the control group. Participation in information sessions and shorter training programs seem to also increase, but less so. Interestingly, we find no effect on participation in training related to shortage occupations in particular.

Next, we document that treated jobseekers were 1pp (1.9%) more likely to list a shortage occupation in their job preferences. However, they experienced a decrease in the number of connections to their personal space on the PES website, which is probably driven by those who enrol in training programs following the email and who are thus not searching for a job during this time. These changes in job search behaviour do not appears to affect employment probability substantially six months after the intervention.

Finally, our survey dara indicates that the treatment information had some negative spillover effects on the importance attached to other job search strategies, consistent with recent research by Altmann et al. (2021) who show that policies that steer individuals' attention to a specific decision can lead to negative cognitive spillovers on the quality of choices in other domains. This underlies the importance of being cautious when implementing large-scale information interventions as these might backfire in non-treated domains.

This paper builds on a growing literature studying the impact of information frictions on the effectiveness of public policies and interventions that address these frictions (e.g., Chetty and Saez, 2013; Bhargava and Manoli, 2015; Belot et al., 2019; Van den Berg et al., 2020; Benghalem et al., 2021; Cairo and Mahlstedt, 2021). In particular, our research contributes to a set of recent papers studying information frictions faced by jobseekers in their job search process. This research has shown 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, Sahin et al. (2014) and Patterson et al. (2016) show that UI recipients tend to search in occupations with relatively few job openings, while other occupations that offer more employment opportunities attract little interest from jobseekers. Belot et al. (2019) examine how these frictions can be addressed through better information about the labour market, and evaluate a new low-cost and innovative tool to provide job search advice to jobseekers in the UK. They find that, although jobseekers might initially search too narrowly, providing them with occupational information broadens the set of jobs they consider and increases the number of interviews they get invited to. Relatedly, other researchers have focused on addressing information frictions in terms of job search skills. In Germany, Altmann et al. (2018) study the effects of sending recent UI recipients an informational brochure on job search strategies and the consequences of unemployment, including a motivational message encouraging recipients to actively look for a job. After one year, the authors find moderately positive (but insignificant) effects on employment and earnings overall, with more pronounced effects for individuals at risk of long-term unemployment. In Australia, Briscese et al. (2021) show that exposing jobseekers to a website supplementing job search skills via editable resume and cover letter templates increases the job-finding rate of jobseekers aged 35-50. Finally, in South Africa, Wheeler et al. (2022) show that training jobseekers to join and use LinkedIn increases their employment probability, by allowing individuals to acquire information about prospective employers and to access referrals and convey information to prospective employers on the platform. Our contribution to this literature is to focus on informing jobseekers about shortage occupations in particular and, crucially, available training opportunities they can seek if they do not possess the necessary skills to enter these jobs in high demand.

In this respect, our paper also relates to research on information frictions in the context of education and training decisions. In essence, this strand of the literature has mainly focused on addressing information frictions to influence educational choices of high school or graduate students before they enter the labour market (Bonilla-Mejia et al., 2019; Ganguli et al., 2020; Kerr et al., 2020). Two notable exceptions are Barr and Turner (2018) and Mbih and Ben

Dhia (2021), who both study the effects of an informational outreach on training choices of jobseekers. In the United States, Barr and Turner (2018) study the effects of sending a letter to UI recipients with information about the benefits and costs of post-secondary education, as well as the necessary steps and assistance available to facilitate such an investment. Exploiting the variation in the dissemination and timing of the information, they show that the intervention increased the probability that UI recipients enrol in community colleges by 4 pp, or 40% in proportional terms. The effects are strongest among older jobseekers and more vulnerable groups, and especially pronounced for shorter-term certificates or technical associate's programs. In France, Ben Dhia and Mbih (2021) implement a field experiment in which they randomly add information about training costs, registration procedures, and training returns, to emails advertising training opportunities. Their baseline survey reveals important information gaps about basic aspects of training costs and shows that many respondents are uncertain or sceptical about training returns. Their information intervention did not have any meaningful impact on training enrolment, however, suggesting that these information frictions did not impede jobseekers from following the studied trainings. Our key contribution to this nascent literature is to focus on trainings related to shortage occupations in particular and to combine information about these occupations in high demand, with information about the related trainings. We are also able to follow our sample for up to three years after the information intervention takes places, whereas Ben Dhia and Mbih (2021) and Barr and Turner (2018) can only follow their sample up to six months after the intervention. This means that we will be able to explore potential long-term effects on training enrolment, as well as effects on employment (as individuals have to time to enter the trainings, finish them, and transit to employment). Additionally, in contrast with Barr and Turner (2018), we implement a field experiment which allows a particularly precise identification. We also include much shorter trainings requiring no or little pre-qualifications, which could be of particular interest to the most vulnerable jobseekers. Compared with Mbih and Ben Dhia (2021), we include a much broader set of trainings (126 instead of seven), and a greater number of jobseekers (100,000 instead of 50,000) as we do not send emails to a selected sample of individuals interested in specific occupations. Our setting thus allows to study the effects of informing jobseekers about shortage occupations and related trainings, with a particularly high potential to ultimately lead them into (stable) employment.

Finally, because our experiment is implemented on a regional scale, our paper adds to a nascent literature on whether nudging and information interventions still work when scaled. In this respect, Bird et al. (2021) study the impact of a national and state-level campaign encouraging students to apply for financial aid for college. Their experiment reaches 800,000

students and concludes that there is no effect on aid receipt or college enrolment overall or for any subgroups, which suggest that nudging strategies that work locally may be hard to scale effectively. We contribute to this literature by studying a nudging intervention that affects the entire population of recently enrolled UI recipients, and show, as in Bird et al. (2021), that our large-scale information intervention did not substantially affect training behaviour. In this sense, our findings also suggest that smaller-scale field experiments or lab experiments studying the behaviour of jobseekers in specific contexts might not translate into actual behavioural changes in real life.

2 Institutional Setting

In Belgium the UI agency is composed of one federal and three regional bodies. The federal UI agency is in charge of the payment of unemployment benefits, while the regional PES are in charge of job search advice, job search monitoring and employment promotion activities inside their geographic territory. The Forem – the PES with whom we partnered for this evaluation – is in charge of all active labour market programmes in the region of Wallonia (one of Belgium's three regions, with a population of approximately 3.6 million inhabitants).

The PES's central mission is to "accompany all citizens (not only jobseekers, but also apprentices, students, workers, and teachers) and firms in their professional journey". In this context, one of the PES's key roles is to offer and promote professional trainings, with a special emphasis on professions for which firms installed 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 the working conditions offered by employers do not prompt jobseekers to apply or accept the jobs) 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 then actively promotes trainings for these jobs in particular.[2]

The PES identifies shortage occupations using two types of tools: statistical evidence from employer surveys and expert knowledge. The first tool consists in a yearly survey in which employers are asked about whether and how quickly they were able to fill job openings for different types of occupations.[3] The PES considers occupations to be in shortage if (i) employer satisfaction with the job filling for that occupation is below the median, and

[2]Note that the PES also offers a range of other trainings, including relatively broad or generic trainings (e.g., basic IT skills, presentation skills, etc.) and trainings for non-shortage occupations.

[3]Note that only employers who open vacancies (namely) on the PES website are contacted for this survey.

(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 "pure shortage occupations", defined as critical occupations for which there is a quantitative shortage of candidates with the appropriate qualifications.[4] In this paper, we group these two categories together and refer to them simply as shortage occupations. Ultimately, shortage occupations can thus be defined, in the context of this paper, as jobs for which employers experience difficulties in finding appropriate candidates, because of a quantitative and/or qualitative shortage of job applicants.

In 2021, 126 occupations were defined as being in shortage in Wallonia (the full list of occupations that were in shortage in 2021 can be found in Appendix A.1). The jobs covered by this list are quite diverse and require varying levels of education and lengths of training. The list includes jobs in the healthcare sector (e.g., general practitioner, nurse, healthcare assistant), service industry (e.g., domestic helper, cleaner, caregiver, waiter, cook, hairdresser, security guard, sales assistant but also office jobs like accountant, IT analyst, business analyst, financial analyst, sales representative, business engineer, or web developer), construction sector (e.g., carpenter, tiler, painter, plumber), logistics sector (e.g., coach and bus driver, truck driver, dispatcher, warehouse supervisor and worker, logistics manager) and industry (e.g., metal worker, industrial manager, production manager and many kinds of technicians). The length of training for these jobs ranges from five weeks (cleaner) to eight years (general practitioner). Overall, most unemployed individuals who are looking into retraining for the labour market should thus be able to find a shortage occupation that matches their preferred sector of occupation and professional aspirations.

Despite the large number of training options, shortage occupations typically remain understaffed over many years. This is somewhat surprising, not only given the high potential rewards from following such trainings (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 followed are relatively attractive. Jobseekers who follow trainings for shortage occupations are indeed exempted from their job search requirements while keeping their rights to UI benefits (under certain broad conditions listed in Appendix A.2) during the entire duration of the training program.[5] In addition, they are entitled to a

[4] A quantitative shortage is defined as less than 15 unemployed jobseekers with the appropriate qualification for every ten job openings in an occupation.

[5] The key condition is that the UI recipient must have finished his/her last studies (preceding the first

travel expense allowance, extra childcare allowances, special interview coaching and a bonus ranging from €350 to €2,000 if they successfully finish the training.[6] Surprisingly, despite the apparent attractiveness of following a shortage occupation training as a job search strategy, trainings are chronically under-subscribed, and many jobseekers remain unemployed for long periods of time without ever enrolling in these trainings.

Several factors could explain why unemployed jobseekers do not train in occupations that are in shortage. Such factors could namely include behavioural barriers (e.g., deficits in self-efficacy), information frictions (e.g., awareness about shortage occupations and training opportunities), time constraints (e.g., childcare), limited mobility, financial and administrative barriers, or simply a lack of willingness to work in these occupations (e.g., because of unattractive working conditions). For example, Ben Dhia and Mbih (2021) show that French jobseekers have biased beliefs about trainings costs. Barr and Turner (2018) show that providing information and administrative support to jobseekers can increase their enrolment in community colleges, suggesting informational and administrative barriers influence jobseekers' decision to participate in trainings. In terms of psychological barriers, Caliendo et al. (2020) show that individuals with and internal (as opposed to external) locus of control are more likely to undertake general trainings, suggesting that personality traits and other psychological factors could influence training decisions among jobseekers too. Recently, the Walloon PES has started to explore what factors prevent jobseekers from entering shortage occupations. To do so, they surveyed individuals who were "positioned" as searching for a job in a shortage occupation but remained unemployed nevertheless.[7] This survey allowed to explore perceived internal and external barriers to working in shortage occupations in the sectors of construction or hotels, restaurant and cafés. The key identified barriers include issues with the attractiveness of the jobs, training and (re-)skilling needs, and lack of work experience.[8] Note, however, that jobseekers could also be facing a number of barriers that they are unaware of, which would include barriers of an informational or behavioural nature

recipe of UI benefits) for at least 2 years and cannot already have benefited from the job search exemption in the past. See Appendix A.1 for a detailed description of the eligibility rules.

[6]Most trainings open a right to a €350 bonus at completion, but jobs in the construction sector, that suffer from even greater shortages, open a right to a €2,000 bonus at completion.

[7]For the PES "positioned" in a certain occupation is intended to mean that the individuals is interested in finding a job in that occupation. However, from the survey, it appears that only 70-74% of individuals positioned in an occupation are actually searching for a job in that occupation. This can occur if, for example, the occupation was listed on the individual's profile because they have work experience or a qualification in this occupation, but they are not interested in working in these jobs.

[8]More specifically, in the construction sector, the main self-reported barriers were: a lack of work experience, outdated skills, mobility constraints, and physical hardship. In the sector of hotels, restaurants and cafés, the key self-reported barriers to enter these jobs were: working hours, physical hardship, work/life balance, a lack of work experience, and outdated skills.

for example.

In this paper, we decide to focus in particular on the role of information frictions in explaining why jobseekers do not enrol in trainings related to shortage occupations. In fact, although jobseekers who are actively searching the PES's website might come across information on understaffed jobs, they are unlikely to receive information about them unless they explicitly express an interest in one of these occupations. Therefore, they might be searching in a closely related job, that is not in shortage, and be unaware that a similar job would yield a higher employment probability. This is illustrated by the fact that the PES sends out invitations to information sessions on trainings, but only to jobseekers who have expressed a possible interest. The PES's website also provides some information on shortage occupations and related training opportunities, but one needs to actively search the website to find this information. Finally, although caseworkers who accompany jobseekers can provide information about these training opportunities, they do not usually focus on this aspect in their job search assistance unless the jobseeker actively asks them to. Therefore, jobseekers are actually unlikely to be exposed to information about shortage occupations unless they are actively searching in these jobs already.

3 Experimental Design

To investigate information frictions faced by jobseekers about shortage occupations and related trainings, we implemented a large-scale field experiment in the Walloon labour market, in collaboration with the regional PES.[9] Our treatment intervention consisted in information that was added to an email sent by the PES to recently enrolled jobseekers. Specifically, the PES usually sends out a quarterly satisfaction survey to a random sample of recent jobseekers, in order to gather feedback on their experience with the PES's services. In the context of the experiment, no random sample was selected. The entire population of recent jobseekers who had communicated an email address to the PES, and who had not already followed a training with the PES, was sent a satisfaction survey email.

The experiment was conducted in October 2021 and February 2022, and included all jobseekers who had registered at the Walloon PES between July and December 2021. In the first wave of the experiment, which took place in October 2021, emails were sent to 60,485 individuals. In the second wave, which took place in February 2022, emails were sent to

[9]On average, in Wallonia, there are approximately 200,000 jobseekers at any point in time. This represents around 12.6% of the active population. Among these jobseekers, about 120,000 have been unemployed for over a year.

38,362 individuals. Together, the experiment was thus conducted on nearly 100,000 recently enrolled UI recipients.[10] Among these roughly 100,000 individuals, half were randomly allocated to the treatment group. The emails were sent through the PES's email address on the 19th of October 2021 and 1st of February 2022. One week after each mailing, a reminder was sent to individuals who had not yet opened the email (i.e., on the 25th of October and the 8th of February).

The "control email" was thus an email inviting jobseekers to complete a satisfaction survey. The "treatment email" was identical to the control email, except that information on shortage occupations and related trainings was added underneath. Specifically, the treatment email informed jobseekers about the fact that shortage occupations systematically lacked candidates, and that focusing on these 126 occupations could allow them to increase their probability of (re-)entering the labour market. The email 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 UI 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 (the amount of the bonus depends on the training). Interested jobseekers were then referred to their local "Customer Service" – agencies that are in charge of accompanying local jobseekers who come to them at different stages of their unemployment spell (enrolment, job search, certificates, skills identification, and information on trainings) – whom they could contact for additional information. At the end of the email, there were also three links on which jobseekers could click that led to (i) a video explaining what shortage occupations are, (ii) a list of understaffed jobs, and (iii) additional information on the financial bonuses. Ultimately, the aim of the treatment was to inform recent jobseekers, in a salient and very simple manner, about the existence of shortage occupations and the conditions under which they could seek training for these jobs while unemployed. By doing so, the intention was to steer them towards more promising occupations and training opportunities.[11] A copy of the treatment and control

[10] In the first wave, emails were sent on Tuesday the 19th of October 2021 around 9am and a reminder was sent on Tuesday the 26th of October around 2pm for those who had not opened the first email. In the second wave, the email was sent on Tuesday the 1st of February around 9am and a reminder was sent on Tuesday the 8th of February around 2pm for those who had not opened the first email. According to the PES, this time of week and day is the most likely to spark the attention of jobseekers when sending them an email.

[11] Note that the timing of the emails ensured that the vast majority of treated jobseekers received this information before their first meeting with a caseworker. This means that, even if jobseekers did not take immediate action by themselves after receiving the email, they then had the opportunity to steer the conversation with their caseworkers towards the shortage occupation of their choice and then obtain more tailored information for their caseworker directly. These meeting indeed take place about four months (two

emails are presented in Appendix A.3.

The experimental design as described above has a number of features which influence what effects we are able to measure, and how we should interpret our findings. A first key feature of our design is that the information is sent by email. 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. This namely allows us to distinguish between 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. This is actually important given that 45% of individuals who were sent an email did not open it, and it allows us to perform placebo tests using the sample of non-openers.

The drawback of sending the information by email, on the other hand, is that it is provided at a moment when individuals are not necessarily thinking about their job search, and the email can easily be ignored. This feature is akin to Almtann et al. (2018), Barr and Turner (2018), or Ben Dhia and Mbih (2021) but differs from Belot et al. (2019) or Briscese et al. (2021) who provide jobseekers with information or tools during the job search process. In our case, individuals could quickly skim through the information, forget about the email or delete it, or even not open the email at all (which was indeed the case for 45% of our sample).[12] In other words, the intensity of treatment in our experiment is potentially quite low.

This low treatment intensity is further reinforced by the second key feature of our design: that the information is not tailored to the situation of each jobseeker. Indeed, because one identical email was sent to all treated individuals, the information that was conveyed needed to be sufficiently general to apply to all email recipients. This is why the treatment information consists of general information on the existence of shortage occupations and the availability of trainings, as opposed to specific occupations or trainings as in Belot et al. (2019) or Ben Diah and Mbih (2021). 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. After doing so, they still needed to

months for those entering directly after their studies) after registering at the PES and jobseekers had an unemployment length of under four months at the time the emails were sent.

[12]We tried to minimize this risk by sending the information on a Tuesday, at different times of the day (once in the morning at 9am for the initial email, and once in the afternoon at 2 pm for the reminder), at moments when individuals might be less likely to be busy doing other things, e.g., parents were unlikely to be busy with childcare.

figure out how to sign up for the training and under what conditions they could do so.

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 because it took place right before jobseekers were due to meet with their caseworker. Indeed, a third important feature of our design is that it targets the population of recently enrolled jobseekers. The main drawback of this is that a significant share of individuals in our sample will have already returned to employment at the time that they receive the email and, for them, the information conveyed might not be very useful. But targeting the population of recently enrolled jobseekers also has several advantages. First, it allows us to insert the intervention in the PES's quarterly satisfaction survey, and thus to gather data on intermediate outcomes (training intentions and perceptions) for both the control and treatment group.[13] In addition, recently enrolled jobseekers are arguably those who are the least likely to have already been informed about shortage occupations and related trainings, and thus those on whom the information might have the largest impact. Because jobseekers received the email shortly before they were due to meet their caseworker for the first time, we expect that the treatment will have induced some to discuss shortage occupations and trainings with their caseworker, who would in turn be able to provide them with more detailed information and direct them to the appropriate resources.[14] In this sense, our information intervention is really intended to serve as a first step towards directing unemployed jobseekers into shortage occupations and related trainings. This means that we should expect treatment effects to take some time to materialize because, to occur, jobseekers need to have read the information, discussed it with their caseworker, and then signed up to the next available training.

Finally, all the information in our research design is provided through the PES. The email is sent through the PES's contact center (with the official PES's email address), and directs jobseekers to links on the PES's website (e.g., list of shortage occupations or information on financial bonuses) or its specialized services (e.g., the Customer Services which promote trainings locally). The advantage of relying on these formal information channels (rather than, for example, sending an email from a non-official address) is that it provides more credibility to our message. However, the extent to which this is true depends on how much jobseekers trust the PES and the information that it provides them. Unfortunately, we do

[13]Thanks to this, we we can estimate treatment effects on training intentions and perceptions about the importance of shortage occupations and trainings in the job search process.

[14]Because caseworkers are instructed to provide assistance to jobseekers following their career wishes, this means that the email could orientate the discussion toward shortage occupations and related trainings. In contrast, if we had targeted the longer-term unemployed, they would have been likely to have already been engaged in a different path to employment.

not have a measure of jobseekers' trust towards the PES. We do, however, have a question in the survey that asks jobseekers how much tey feel supported by the PES in different sorts of job search endeavours.[15] On average, between 42.3% and 65.2% of survey respondents felt that the PES supported them in their job search and training endeavours (depending on the dimension they were asked about), which could hint to a certain scepticism towards the PES among jobseekers.

To wrap up, our experimental design is thus characterized as a low-cost email intervention, targeting a broad population of recently unemployed jobseekers with general information about shortage occupations and related trainings. In turn, the information provided aims at encouraging jobseekers to seek additional information with the Customer Services (link in the email) or during their upcoming meeting with their caseworker.

4 Data

We answer our research question using two sources of data: administrative data, which allows us to measure treatment effects on training activities and labour market outcomes, and survey data, which allows us to measure treatment effects on perceptions and training intentions. Additionally, we have access to data on email engagement (that can be linked to the administrative data), which allows us to observe to what extent individuals engaged with the information that was sent to them.

4.1 Administrative Data

Our key data source is individual-level administrative data, in which we follow individuals six months after having sent them the email.[16] This data includes a wide range of information, namely on training, employment, job search and personal characteristics. This data, which we use to construct our main outcomes of interest, is highly reliable and allows to precisely track the characteristics, training activities, and employment spells, before and after the intervention, without suffering from issues related to studies essentially based on voluntary surveys (namely attrition and misreporting).

[15]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) waver of job search requirements when enrolled in training, and (iv) number of job openings sent to them. Although these questions do not measure trust per se, they might provide some indication of how much jobseekers trust the PES is committed to helping them find a job.

[16]We will ultimately follow individuals up to three years after having sent them the email, but currently have data up to six months post-intervention.

Table 1: Summary Statistics - Overall Sample

		Overall	Treated	Control	t-test
Gender	Woman	0.5218	0.5229	0.5206	0.0023
		(0.4995)	(0.4995)	(0.4996)	(0.0032)
	Man	0.4782	0.4771	0.4794	-0.0023
		(0.4995)	(0.4995)	(0.4996)	(0.0032)
Age	Age	32	32	32	0.0023
		(11)	(11)	(11)	(0.0716)
Province	Brussels	0.0045	0.0040	0.0050	-0.0010**
		(0.0672)	(0.0633)	(0.0708)	(0.0004)
	Flanders	0.0028	0.0029	0.0027	0.0001
		(0.0530)	(0.0536)	(0.0523)	(0.0003)
	Hainaut	0.3844	0.3838	0.3851	-0.0012
		(0.4865)	(0.4863)	(0.4866)	(0.0031)
	Liege	0.2809	0.2813	0.2805	0.0008
		(0.4494)	(0.4496)	(0.4492)	(0.0029)
	Luxemburg	0.0823	0.0819	0.0827	-0.0008
		(0.2749)	(0.2743)	(0.2755)	(0.0017)
	Namur	0.1489	0.1494	0.1485	0.0008
		(0.3560)	(0.3564)	(0.3556)	(0.0023)
	Wal. Brabant	0.0961	0.0967	0.0955	0.0013
		(0.2947)	(0.2956)	(0.2939)	(0.0019)
Education	> Secondary	0.2252	0.2259	0.2245	0.0014
		(0.4177)	(0.4182)	(0.4173)	(0.0027)
	Secondary	0.4871	0.4881	0.4862	0.0019
		(0.4998)	(0.4999)	(0.4998)	(0.0032)
	Apprentice	0.0318	0.0312	0.0325	-0.0013
		(0.1756)	(0.1738)	(0.1773)	(0.0011)
	Tertiary	0.1784	0.1788	0.1779	0.0009
		(0.3828)	(0.3832)	(0.3824)	(0.0024)
	University	0.0775	0.0760	0.0789	-0.0029*
		(0.2673)	(0.2650)	(0.2696)	(0.0017)
First-time Unemployed	No	0.7180	0.7186	0.7173	0.0013
		(0.4500)	(0.4497)	(0.4503)	(0.0029)
	Yes	0.2820	0.2814	0.2827	-0.0013
		(0.4500)	(0.4497)	(0.4503)	(0.0029)
Nationality	Belgium	0.8610	0.8599	0.8622	-0.0023
		(0.3459)	(0.3471)	(0.3447)	(0.0022)
	Non-EU	0.0767	0.0779	0.0756	0.0024
		(0.2662)	(0.2681)	(0.2643)	(0.0017)
	EU	0.0622	0.0622	0.0622	-0.0000
		(0.2416)	(0.2415)	(0.2416)	(0.0015)
# Obs		98,844	49,251	49,593	

Note: This table shows a pre-treatment balance test of covariates. In particular, it shows summary statistics for our overall sample, and for treated and control groups separately. Differences between the treatment and control groups are shown in the last column, with stars indicating if these differences are statistically significant. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

First, our training data includes information on all training activities undertaken by jobseekers, with the name of the training activity, its length, and the reason for ending the training. Training activities cover a very wide range of subjects, including language tests, language courses, eligibility screenings, transversal trainings like basic IT skills or presentation skills, one-day certifications for the construction sector, trial days for certain occupations, and, crucially, occupational trainings.

We use this training data to create our outcomes of interest relating to training behaviour, which are dummy variables indicating whether an individual has engaged in different kinds of training activities. Specifically, we first consider whether jobseekers participated in any training activity (broadly defined) in the six months that followed the reception of the email. Second, because we are most interested in stimulating enrolment in shortage occupations trainings, we create a dummy variable equal to one if the individual participates in a training for a shortage occupation specifically. Finally, we explore whether jobseekers enrol in longer trainings, and create a dummy equal to one of they enrol in a training course that lasts more than 30 days.

In addition to data on training behaviour, we also study treatment effects on three intermediary outcomes to training: participating in an information session on a training program, undergoing a screening test for a training, and participating in trial days for trying out an occupation before enrolling in training. For these three intermediary outcomes, we code a dummy equal to one if the individual engages in the activity, and zero otherwise.

Second, our treatment information could have affected employment outcomes, either directly (through a change in job search), or indirectly (through training). To explore whether this is the case, we use data on employment spells to explore whether treated jobseekers are more likely to enter employment after receiving the information on shortage occupations (up to six months after reception of the email). We complement this outcome with information on job search behaviour included in our administrative data, which allows us to explore whether the intervention affected the job search behaviour of jobseekers. Specifically, we have access to information on the occupation(s) in which a jobseeker is registered as searching and the number of times they connect to their personal space on the PES's website. The outcomes of interest that we consider in terms of job search behaviour are thus: (i) the number of connections to one's personal space since reception of the email, and (ii) a dummy indicating whether an individual changed their occupation(s) of interest.

Finally, we have access to extensive socio-economic information on all individuals who were part of the experiment, including: age, gender, nationality, education level, field of education,

and district of residence. Our data on trainings and employment also include a three-year history, which means we can also look at characteristics of our sample in terms of past employment and training activities.[17]

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. Only two small differences appear: slightly less individuals from the treatment group have a university degree or live in Brussels. These differences are, however, very small in magnitude and do not threaten the validity of our experiment. We will nevertheless show the effects of the information treatment with and without using control variables.

4.2 Email Engagement

Because the information treatment is sent by email, we are able to gather data on whether individuals open the emails and click on any of the enclosed links. The information on email engagement is linked to administrative data, which means we can also assess who engages with the email. The main advantage of this is that we are able to estimate not only the intention-to-treat effects (i.e., for those who were sent the email), but also the average-treatment-on-the-treated effects (i.e., for those who opened the email). It also allows us to get a view of how these two measures differ, and what share of people being sent an email would actually be treated if other, similar, information campaigns were launched in the future.

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). In this Table, the statistics on email opening include the entire sample, whereas the other statistics are shown for the sub-sample of those who open the email. Table 2 first shows that the treatment group was as likely to open the email as the control group, with a little over 54% of individuals opening the email. This is reassuring as it suggests that the slight change in email object did not induce a differential take-up of the information treatment. Treated individuals were, in contrast, more likely to click on at least one link in the email (13.9% of the treated clicked on at least one link against 11.3% of the controls) and clicked on more links on average, which likely reflects the fact they had more links they could potentially click on. Interestingly, Table 2 indicates that the

[17]The PES records all training and employment information from the first time an individual registration at the PES. We thus have access to the training and employment history of individuals as of their first registration at the PES.

Table 2: Descriptive Statistics – Email Engagement

	(1) Overall Sample	(2) Treatment Group	(3) Control Group	(4) Difference
Open Email	0.5445	0.5440	0.5450	-0.0010
	(0.4980)	(0.4981)	(0.4980)	(0.0032)
Clicked on at least one link	0.1261	0.1392	0.1131	0.0261 ***
	(0.3319)	(0.3461)	(0.3167)	(0.0028)
Number of clicks	0.1439	0.1726	0.1155	0.0571 ***
	(0.4221)	(0.4938)	(0.3341)	(0.0036)
Click on survey link	0.0945	0.0781	0.1108	-0.0327 ***
	(0.2925)	(0.2683)	(0.3139)	(0.0025)
Click on social network link	0.0026	0.0019	0.0033	-0.0013 ***
	(0.0510)	(0.0440)	(0.0571)	(0.0004)
Click on "Client Service" link		0.0147		
		(0.1204)		
Click on video		0.0165		
		(0.1274)		
Click on list of shortage occupations		0.0466		
		(0.2108)		
Click on information about financial incentive		0.0124		
		(0.1108)		
Click on at least one treatment link		0.0765		
		(0.2659)		
Number of observations	96,846	49,251	49,593	

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

treatment group was significantly less likely to click on the survey link (7.8% against 11.1%), suggesting that the information on shortage occupations and related trainings diverted the attention of jobseekers away from the survey. Similarly, treated jobseekers were slightly less likely to click on a social media link (0.2% against 0.3%). Note that these differences illustrate that highlighting a certain information (in our case: the attractiveness of shortage occupations and related trainings) can divert the attention from other information and actions (in our case: answering the satisfaction survey). These elements could benefit from additional attention in the growing literature on information frictions and interventions that address them.

Table 2 also provides some information on how the treatment group interacted with the

information on shortage occupations and related trainings. It shows that the treatment information jobseekers were most interested in was the list of occupations in shortage, with 4.6% of them clicking on that link. The email also included a video which presented the occupations in shortage, which 1.7% of treated individuals clicked on. Finally, 1.5% clicked on the link referring them to the Customer Service (the agency in charge of helping local jobseekers who come to them, and who are in charge of promoting shortage occupation trainings) while, maybe surprisingly, only 1.2% clicked on the link that provided them with more information on the financial incentives. Overall, 7.7% of the treated individuals who opened the email clicked on at least one of the treatment information links.

Another noteworthy information is that approximately 1.89% of treated individuals click 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 click on a shortage occupation link, and over 75% of those who click on shortage occupation links – arguably the most interested in these trainings – are not included in our survey. This suggests that the estimated effects on training intentions could be somewhat underestimated in our survey as it will not include individuals who are potentially the most affected by the treatment information and were diverted from answering the survey.

Finally, because some of our analyses exploit survey data to investigate the effects of the information treatment on training intentions and perceptions, it is useful to explore to what extent individuals who answer the survey in the email are representative of the overall sample. To this end, Table 3 shows summary statistics for the sample of email openers and survey clickers, as well as that of the overall sample for means of comparison. 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 of university degree) are more likely to open the email than individuals with a lower level of education (apprenticeship and secondary degree or less) but, conditional on opening, the less-educated are somewhat more likely to answer the survey than the higher-educated. Fourth, immigrants (and in particular those from outside the EU) 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 less likely to open the email, but 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 email openers are slightly overrepresented in terms of women, higher-educated

Table 3: Summary Statistics – Email Openers and Clickers

		Overall Sample	Opened Email	Clicked on survey
Gender	Woman	0.5218	0.5403	0.5791
		(0.4995)	(0.4984)	(0.4938)
	Man	0.4782	0.4597	0.4209
		(0.4995)	(0.4984)	(0.4938)
Age	Age	32	33	38
		(11)	(12)	(13)
Province	Brussels	0.0045	0.0049	0.0041
		(0.0672)	(0.0697)	(0.0636)
	Flanders	0.0028	0.0032	0.0027
		(0.0530)	(0.0563)	(0.0519)
	Hainaut	0.3844	0.3739	0.3808
		(0.4865)	(0.4838)	(0.4856)
	Liege	0.2809	0.2851	0.2854
		(0.4494)	(0.4515)	(0.4516)
	Luxemburg	0.0823	0.0828	0.0786
		(0.2749)	(0.2756)	(0.2692)
	Namur	0.1489	0.1485	0.1463
		(0.3560)	(0.3556)	(0.3534)
	Wal. Brabant	0.0961	0.1016	0.1022
		(0.2947)	(0.3021)	(0.3029)
Education	> Secondary	0.2252	0.1986	0.1931
		(0.4177)	(0.3989)	(0.3948)
	Secondary	0.4871	0.4975	0.5399
		(0.4998)	(0.5000)	(0.4985)
	Apprentice	0.0318	0.0290	0.0237
		(0.1756)	(0.1679)	(0.1523)
	Tertiary	0.1784	0.1865	0.1614
		(0.3828)	(0.3895)	(0.3680)
	University	0.0775	0.0884	0.0819
		(0.2673)	(0.2839)	(0.2742)
First-time Unemployed	No	0.7180	0.7003	0.7494
		(0.4500)	(0.4581)	(0.4334)
	Yes	0.2820	0.2997	0.2506
		(0.4500)	(0.4581)	(0.4334)
Nationality	Belgium	0.8610	0.8386	0.7619
		(0.3459)	(0.3679)	(0.4259)
	Non-EU	0.0767	0.0946	0.1647
		(0.2662)	(0.2926)	(0.3709)
	EU	0.0622	0.0669	0.0734
		(0.2416)	(0.2498)	(0.2608)
# Obs		96,846	53,822	5,179

Note: This table shows summary statistics for our overall sample, and for the sub-samples of individuals who open the email and click on the survey. Its aim is to understand how the sub-samples of email openers and clickers differs from our overall target population.

and immigrants, whereas survey openers are overrepresented in terms of women, older and less-educated individuals, recurrent unemployed, and immigrants. Note that, as is the case for the overall sample, there does not seem to be any differences in observed characteristics between the treated and control individuals who open the email or click on the survey.

4.3 Survey

Our information treatment was included in an invitation to complete a satisfaction survey, which was therefore sent to both the treated and the control groups. In this survey, we added a series of questions that aimed at measuring perceptions of jobseekers about shortage occupations and related trainings. 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 topics unrelated to shortage occupations and related trainings.

Specifically, we added three questions to the survey, that aimed at testing whether the information treatment affected perceptions related to shortage occupations and trainings. 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 specially as to avoid inadvertently treating individuals from the control group, who would otherwise have been somewhat primed to think about shortage occupations. In our estimates, we coded this outcome as a 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 information treatment affected the treated group's perceptions about how useful focusing their job search strategy on shortage occupations and related trainings would be. We therefore 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 expect to be affected by our information treatment are options (iii), (vi), and (viii), i.e., those relating to shortage occupations and/or training activities. In our estimations, we consider whether jobseekers deem a given strategy impactful, and code this outcome as a dummy variable equal to one if the individual answered that they found a given strategy to have "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. The aim was to determine to what extent the information contained in the treatment emails on the advantageous conditions under which jobseekers could follow training (numerous training offers, keeping their rights to UI benefits, financial incentives), had been picked up by the individuals. 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) waver of job search requirements when enrolled in training, and (iv) number of job openings sent to them. The options we expect to be affected by our information treatment are options (i), (ii), and (iii), i.e., those relating to the advantages offered to jobseekers who enrol in a shortage occupation training.

Moreover, we asked two additional questions to help us interpret our findings. First, we asked what the individual's current professional situation was so that we could identify those who were still unemployed at the time of answering the survey. Second, we asked individuals of the treated group whether they had read the information on shortage occupations sent in the email inviting them to complete the survey. 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.

Across the two waves, 3,049 individuals ultimately answered the survey, of which 1,252 (41%) belonged to the treatment 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. As discussed in the previous section, although the sample of individuals who complete 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 on the basis of observable variables). Because the characteristics of the treated and

control individuals are the same, we believe our survey data on perceptions and training intentions can still be used in a causal framework, i.e., comparing the treated and control groups.

5 Empirical Approach

In this paper, we are interested in estimating the effects of receiving information on shortage occupations and related trainings, on training, job search and employment. Because individuals were randomly assigned to treatment, we estimate the following intention-to-treat (ITT) effect:

$$Y_{it} = \alpha_1 + \beta_1 Treatment_i + \epsilon_{it} \tag{1}$$

In addition, we estimate a local-average-treatment-effect for individuals who actually access more information on shortage occupations and related trainings. In the rest of the paper, we refer to this estimate as the local-average-treatment-effect (LATE) of clicking on a treatment link. We estimate:

$$Y_{it} = \alpha_2 + \beta_2 Info_i + \epsilon_{it2} \tag{2}$$

With:

$$Info_i = \gamma + \delta Treatment_i + \eta_i \tag{3}$$

Y_{it} are the outcomes of interest of individual i at time t, $Treatment_i$ is a dummy equal to one when individual i is assigned of the treatment group and zero otherwise, and $Info_i$ indicates whether an individual has clicked on one of the treatment links. Our coefficients of interet are β_1 and β_2, the effects of receiving the information email and clicking on an information link, respectively. Our main outcomes of interest Y_{it} are linked to: (i) perceptions and training intentions, (ii) training enrolment and completion, and (iii) labour market outcomes. Specifically, we start by using our survey data to estimate β, the treatment effect, on (i) the probability of having the intention to seek training in the following year, (ii) the probability that jobseekers consider job search strategies linked to shortage occupations and related trainings as relatively impactful, and (iii) how likely they are to feel supported by the PES in their training journey. Second, we use administrative data on training activities to estimate the effect of the information treatment on actual training activities (including training enrolment in different kinds of training programs, and participation in information sessions and trial days). Third, we explore treatment effects on job search behaviour and employment. Throughout our analyses on training and employment effects, we restrict our sample to the population who has opened the email ($N = 53,882$).

We estimate the treatment effects without controlling for personal characteristics as these are balanced between the treatment and control group. We nevertheless show in Appendix A.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.

6 Findings

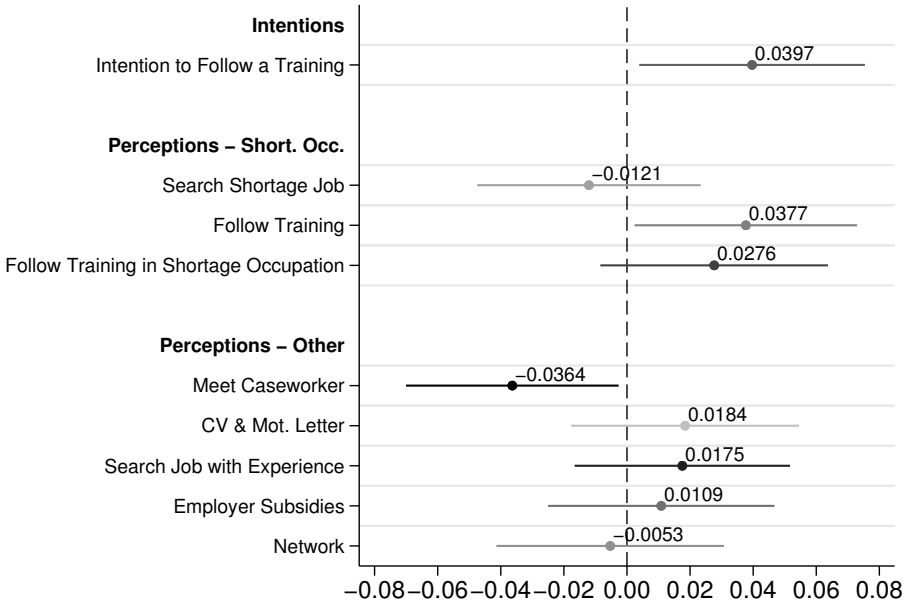
6.1 Effects on Training Intentions and Perceptions

Because our treatment information was included to a mailing campaign sent by the PES, both treated and control individuals received an invitation to complete the survey. This allows us to estimate treatment effects on training intentions, and perceptions about shortage occupations and related trainings. 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 having a high or very high impact on job finding probability (in contrast with a mixed, low, or no impact).

Our findings indicate that being sent the treatment information increases training intentions by 3.97 percentage points (pp), or 7.2% relative to the mean control probability (i.e., 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 trainings, thus increases jobseekers' intention of enrolling in the upcoming year. Our findings suggest that this occurred through an improved perception about the impact of following trainings (namely in shortage occupations, but not only). Treated individuals are indeed 3.77 pp (6.4%) more likely to deem trainings as an effective job search strategy than respondents from the control group, while this effect is of 2.76 pp (5.8%, non-statistically significant) when focusing on trainings in 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 information intervention increased jobseekers' intentions of enrolling in trainings is encouraging with respect to the aim of the treatment of increasing training enrolment and completion (with the ultimate goal of increasing employment in shortage occupations). We investigate to what extent these intentions materialize into actual training enrolment in the next section.

Interestingly, Figure 1 shows that the treatment email did not affect perceptions about

Figure 1: Treatment Effect on Training Intentions and Perceptions



Notes: This figure shows the estimated effect, using Equation 1, of being sent the treatment information 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 upcoming 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 (very) high impact on job finding probability. "Perceptions – Other Strategies" refer to the probability of considering job search strategies unrelated to shortage occupations (i.e., meeting one's caseworker, having a good CV and motivation letter, searching for jobs in which one has experience, making use of employer subsidies, or using one's network) as having a (very) high impact on job finding probability. The estimates are shown without control variables. Confidence intervals are shown at 95%. Point estimates are presented in Table A2

the usefulness of searching for a job in a shortage occupation. This indicates that email recipients put greater emphasis on the information about trainings, rather than the message about searching for jobs in shortage occupations. In Section 6.3, 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 limited. In addition, there appears to have been some negative spillovers of the treatment information on the importance attached to other job search strategies. Our findings indeed show that treated individuals are less likely to consider meeting their caseworker as an impactful job search strategy. This echoes recent findings from Altmann et al. (2021) showing that although

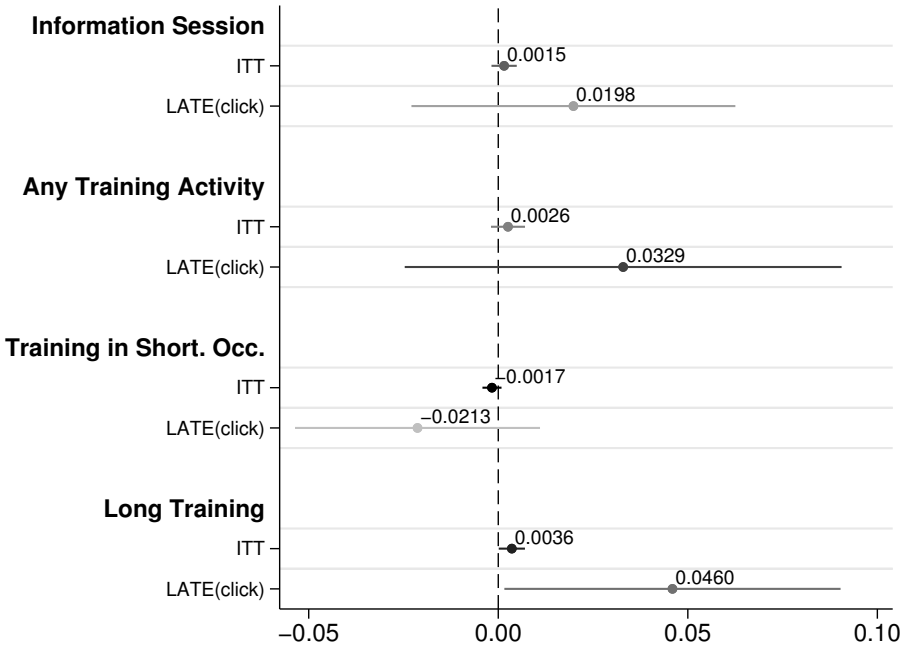
policies that steer individuals' attention to a specific decision usually lead to better choices in the targeted choice domain, they can also induce negative cognitive spillovers on the quality of choices in other domains. In our case, it is possible that by informing jobseekers about the importance 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 Appendix A.5, we show that this effect is somewhat driven by individuals who had already been unemployed in the past; one possible assumption could be that the email made them realize they had never heard about these trainings from their caseworkers in the past, leading them to consider their services as less useful.

6.2 Effects on Training Behaviour

This section explores whether the increased intentions to enrol in training actually materialize into increased training enrolment and completion. We estimate, using Equation 1 (ITT) and Equation 2 (LATE), the treatment effects of the information intervention on various training activities over the six months following the reception of the email. In particular, we show estimated treatment effects on the probability of participating in: (i) an information session about a training, (ii) any training activity, (iii) a training related to a shortage occupation and (iv) a long (> 30 days) training.

Figure 2 shows that the email intervention had overall (relatively small) positive effects on the probability that jobseekers take part in training activities. Importantly, jobseekers who see the treatment information are 0.358 pp more likely to participate in a long training program, which represents an 8.5% with respect to the mean participation of 4,2% among jobseekers in the control group. When focusing on the effect of clicking on one of the treatment links, we estimate that participation in long trainings increases by 4.6%, which represents more than a 100% increase with respect to the mean participation in the control group. However, we find no effect of the treatment intervention on participation in training programs that are related to shortage occupations in particular. This is also in line with our survey results on perceptions which showed that perceptions about the usefulness of training programs increased significantly as a result of the treatment information, more so than perceptions about the usefulness of trainings in shortage occupations. It thus seems that treated jobseekers picked up the message relating to the attractiveness of trainings in general, rather than the attractiveness of shortage occupations and *related* trainings. Finally, Figure 2 is also indicative that the information intervention might have increased participation in information sessions and overall training enrolment (including shorter programs), although estimates are not statistically significant.

Figure 2: Treatment Effect on Training Activities



Notes: This figure shows the estimated effect, using Equation 1, of being sent the treatment information on training behaviour. "Any training activity" refers to enrolling in any kind of training activity, including information sessions, trial days, languages courses and tests, etc. "Training in a Shortage Occupation" refers for enrolling in a training that is focused on shortage occupations in particular. "Long training (¿30 days)" refers to enrolling in a training program that lasts at least 30 days. "Information Session" refers to participating (in person) to an information session on a specific training. "Trial Days" refers to taking part in a few on-the-job days where jobseekers can learn more about the occupation they would train for, before signing up for the training. The estimates are shown without control variables. Confidence intervals are shown at 90% (thickest line,) and 95% (thinnest line).

It is noteworthy that a gap appears to exist between intentions and behaviours, illustrated by the fact that only 7,5% of jobseekers end up enrolling in a training program after 6 months, whereas 55% state to have the intention of doing so. 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).

Together, our findings are quite encouraging as they suggest that even very low-touch information intervention can generate interest (and, to some extent, even enrolment) in training programs. This is consistent with findings from the literature on job search, which shows that even light-touch information interventions can generate a shift in behaviour (e.g., Belot et al., 2019; Altmann et al., 2018). In terms of information interventions on training,

they appear to be in line with findings from the literature too. Indeed, Ben Dhia and Mbih (2021) show that an informational outreach highlighting the costs and returns of trainings, as well as the simplicity of registration procedures, did not significantly impact training enrolment of jobseekers in France six months after the intervention. Their sample size is quite small and thus we cannot rule out that effects of the magnitude we find would have also been found in that context. Moreover, our findings support the findings from Barr and Turner (2018) who study a more intensive informational campaign in the United States. The authors indeed show that, after six months, a letter encouraging UI recipients to enrol in a post-secondary program increased training take-up by 4 percentage points (40%) in the United States. Larger effects are to be expected in the case of the Pell Letter sutdied in Barr and Turner (2018), since it included an "assistance dimension" (i.e., UI 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).

In terms of the design of information interventions, it might be that mail interventions such as in Barr and Tuner (2018) and Altmann et al. (2018) are more effective than email interventions. If people receive much fewer letters than emails, they might pay more attention to letters and their content (this is particularly true given that the Pell Letter was formally endorsed by the White House). Additionally, many people might not read their emails or even have an email address, such that a letter would reach a broader population of UI recipients.

Finally, different effects of information interventions on training and education opportunities could also stem from the very different constraints disadvantaged individuals may face in the United States and in Europe. For example, differences in the cost of education could play an important role; it is likely that more individuals interested in pursuing higher education would have been unable to pay for it in the United States, whereas in Europe education is much more accessible. This might lead information interventions on costs and benefits of trainings to be less effective in Europe than in the US. Moreover, the United States and Europe differ quite strongly in terms of the social protection that is available for unemployed individuals. We shed some light on this aspect in Section 6.4, where we show that those most likely to enrol in training after receiving the information email are those who are not (yet) eligible to receive unemployment insurance benefits.

6.3 Effects on Job Search and Employment

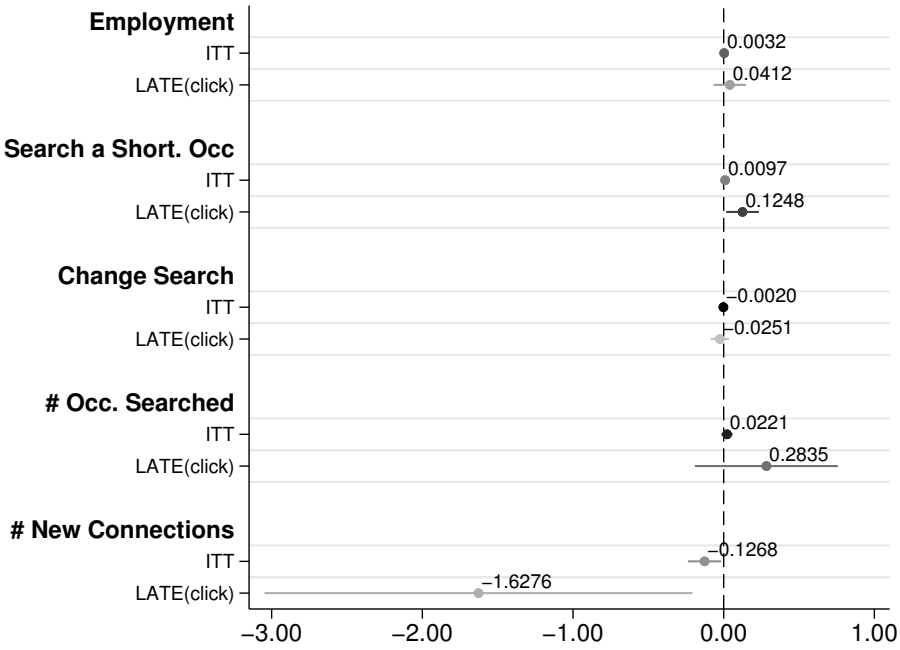
Our information intervention could impact employment outcomes through two channels. First, employment could increase following the increased enrolment of jobseekers into trainings. Second, the information intervention could affect the way in which jobseekers search for jobs and, in turn, their likelihood of finding employment. This could happen if, for example, some shortage occupations are relatively similar or require comparable skills to the jobs the jobseekers were already searching in. Such a channel would be akin to the search tool developed by Belot et al. (2019) which proposes alternative in-demand occupations to UI recipients when they search for jobs. In this case, we should observe employment effects already in the relatively short-term.

Figure 3 shows the estimated effects of our treatment information on employment outcomes and job search behaviour. Specifically, they show estimated treatment effects on: (i) the probability of experiencing at least one employment spell, (ii) the probability of searching for a shortage occupation, (iii) the probability of changing the occupations one searches a job in, (iv) the number of occupations searched, and (v) the number of time an individuals connects to their personal space of the PES website. These four latter outcomes are intended to study the effect of the information intervention on jobseekers' job search strategy.

Consistent with our finding in Section **??** that the treatment intervention did not impact the perceptions about how useful it is to search for a job in a shortage occupation, we do not observe any effect of the intervention on job search behaviour. There indeed seems to be no statistically significant effect on the probability that treated jobseekers change the occupations they search a job in, or the number of times they log into their personal space on the PES website. Consequently, we do not find any effect on employment in the short-term (six months after the reception of the email), which allows us to rule out the first potential mechanism through which our information intervention could have affected employment.

Although our main focus is on encouraging jobseekers to enter trainings (and only subsequently employment), it is somewhat surprising that we do not find any impact of the information intervention on job search behaviour or on short-term employment outcomes, because comparable information interventions did affect job search and employment (at least to some degree). For example, Belot et al. (2019) find that suggesting alternative, similar, occupations during the job search process leads jobseekers to broaden the scope of their search and to receive more invitations to interviews. In our case, it might be that the information on shortage occupations was too broad and not targeted enough to lead to changes in job search behaviour. It might also be that the tool developed by Belot et al. (2019)

Figure 3: Treatment Effects on Job Search and Employment



Notes: This figure shows the estimated effect, using Equation 1, of being sent the treatment information on job search and employment. "# of Logins to Personal Space" corresponds to the number of times an individual logs into their personal space on the PES website since receiving the email. "Change in Profile Positioning" corresponds to changes in the occupations an individuals has listed as being searching a job in. "Employment" refers to spending at least one day in salaried employment since receiving the email. The estimates are shown without control variables. Confidence intervals are shown at 90% (thickest line,) and 95% (thinnest line).

is particularly effective because it offers suggestions during the job search process, whereas jobseekers might have been busy doing something else when they received our email and were thus less attentive to its content. In this regard, our intervention is maybe more similar to that studied in Altmann et al. (2018), who investigate the effects of sending UI recipients a brochure containing information about job search strategies and the consequences of unemployment, as well as a motivational speech encouraging recipients to actively look for new employment. They show that the brochure (and its relatively broad content) increases cumulative earnings and days in employment, but mostly for individuals at risk of long-term unemployment. Effects on the overall sample are, however, only moderately positive and statistically insignificant. As discussed above, it is possible that the brochure being sent by mail rather than email made jobseekers pay more attention to it. Alternatively, their intervention might have worked through an increased motivation of jobseekers to find employment, a dimension which was not included in our setting.

In spite that we find limited effects of our intervention on job search behaviour and employment in the short-term, we cannot exclude that the intervention will affect employment in the longer-run. In particular, a second channel though which our treatment intervention could affect employment outcomes is through an increased take-up of trainings. Such effects would however take longer to materialize, as jobseekers would first need to go through some period of re-training before they can enter the jobs that newly become available to them. At this stage, we observe outcomes only up to six months after the email was sent and we therefore cannot yet tell whether job finding changes as a result of an increase in training take-up. We will use subsequent waves of the data to explore the existence of this mechanism in the next few months.

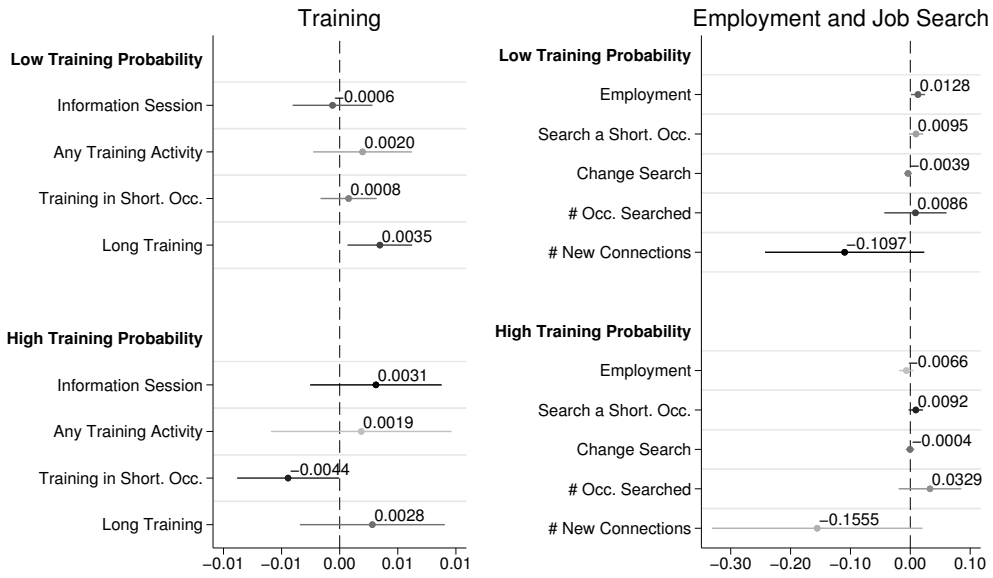
6.4 Heterogeneity

Although we find no aggregate effect on training enrolment, it is possible that the intervention affected some groups of the target population differently. We thus turn to a heterogeneity analysis in which we distinguish our benchmark findings according to several personal characteristics in **??** and ex-ante probability of entering training or employment in **??**. First, in **??**, we estimate treatment effects of the information intervention for individuals who were relatively more or less likely to enter training (left panel) or employment (right panel). 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 mean) and a high (above mean) probability of entering training or employment.[18] **??** shows that the information treatment was most effective for individuals with a low ex-ante probability of enrolling in training or entering employment. This is encouraging as it suggests that the email allowed to stimulate training enrolment among individuals who would have been unlikely to do so in the absence of the email. Moreover, the fact that training enrolment increased among those with a low employment probability is reassuring as it indicates that the additional training enrollees are unlikely to have found a job, and therefore that the email is not diverting individuals from employment to training. All in all, this heterogeneity analysis suggests that the information intervention allowed to enrol into training individuals who are likely to have remained unemployed (and outside of trainings) otherwise.

Second, we also investigate whether some personal characteristics are associated with larger treatment effects on training behaviour in Figure 6. Because this analysis splits our sam-

[18] In our sample, the mean predicted probability of entering training is 0.0648 and the mean predicted probability of entering employment is 0.5542.

Figure 4: Heterogeneous Effects - Ex-ante Participation Probability



Notes: This figure shows the estimated effect, using Equation 1, of being sent the treatment information for individuals with a low versus high probability of entering a training (left panel). The estimates are shown without control variables and confidence intervals are shown at 95%.

ple into several different groups, it somewhat lacks power to distinguish strong differences, but we believe it is still interesting. The first finding that stands out from the heterogeneity analysis presented in Figure 6 is that women seem to have been particularly affected, whereas effects on men are close to zero or even negative. This is consistent with Barr and Turner (2018) who also find that the Pell Letter had larger effects on training enrolment among women. Second, we observe that older jobseekers were relatively more affected by the treatment intervention; for them, receiving the email increases their probability of engaging in all the training activities under study. Similarly, Altmann et al. (2018) find larger effects of their information brochure for older individuals. For both groups (women and older jobseekers), these characteristics correspond to those who were more likely to open the email and especially click on the survey link, indicating either greater interest in information sent by the PES and/or greater attention. Next, the intervention seems to have had more positive effects for Belgian than foreign jobseekers; surprisingly, for foreign jobseekers, point estimates are in fact negative (although confidence intervals are quite large). Finally, differences across educational attainment or previous UI spells seem to be negligible, whereas differences according to UI benefit receipt are larger but do not yield systematic conclusions e.g., effects on information sessions is larger for those receiving UI benefits, whereas the

Figure 5: Heterogeneous Effects - Ex-ante Employment Probability

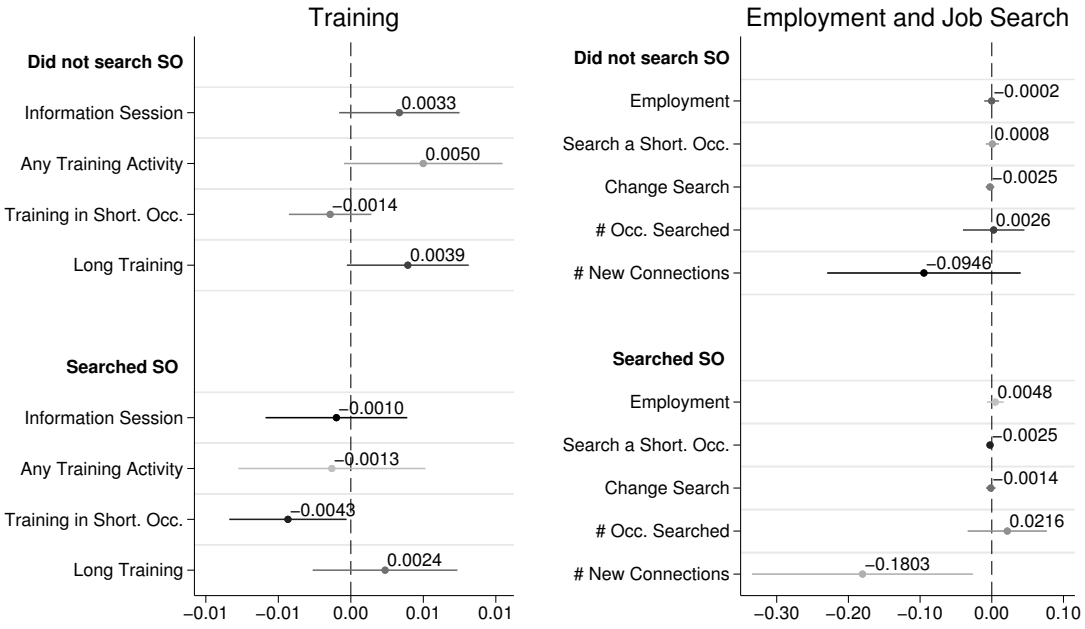
Notes: This figure shows the estimated effect, using Equation 1, of being sent the treatment information for individuals with a low versus high probability of entering a training (left panel). The estimates are shown without control variables and confidence intervals are shown at 95%.

effect on long trainings is larger for those who do not receive UI benefits).

Figure 6: Heterogeneous Effects - Gender

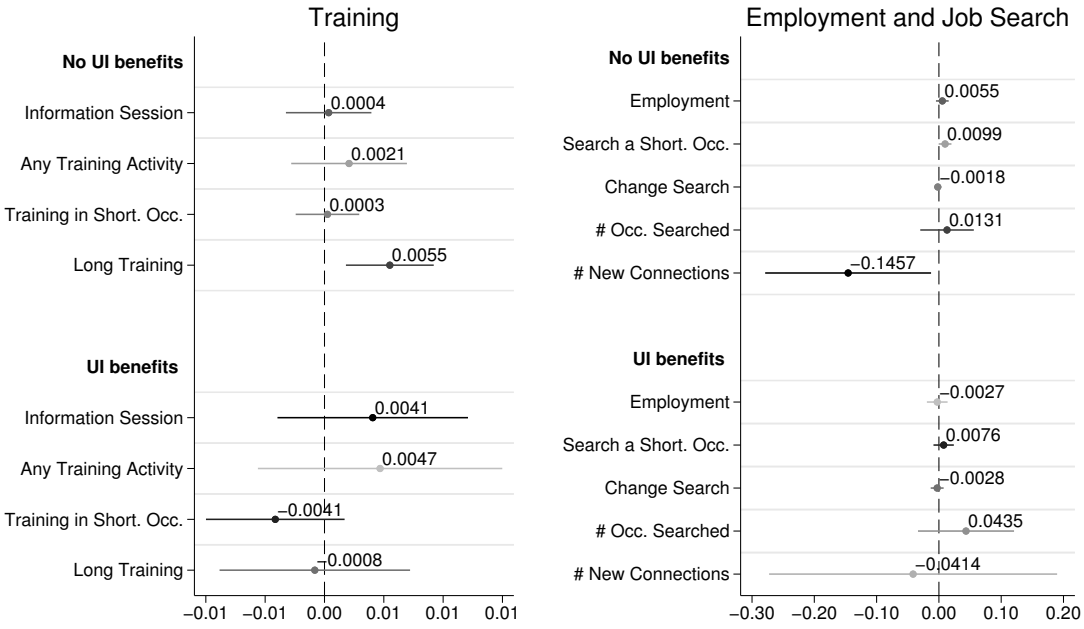
Notes: This figure shows the estimated effect, using Equation 1, of being sent the treatment information on training, employment, and job search behavior. The estimates are shown without control variables and confidence intervals are shown at 95%.

Figure 7: Heterogeneous Effects - Searched Short. Occ.



Notes: This figure shows the estimated effect, using Equation 1, of being sent the treatment information on training, employment, and job search behavior. The estimates are shown without control variables and confidence intervals are shown at 95%.

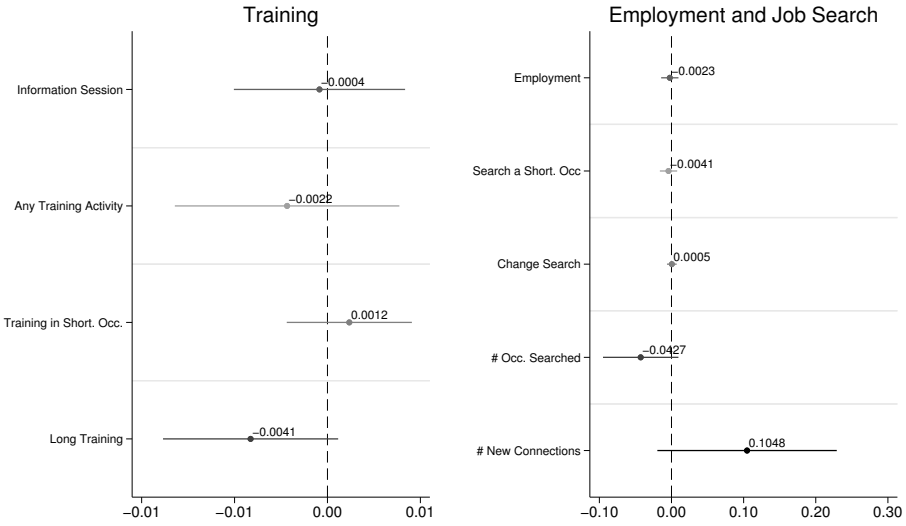
Figure 8: Heterogeneous Effects - UI Benefits



Notes: This figure shows the estimated effect, using Equation 1, of being sent the treatment information on training, employment, and job search behavior. The estimates are shown without control variables and confidence intervals are shown at 95%.

7 Robustness

Figure 9: Placebo Test



Notes: This figure shows the results of a placebo test. Specifically, we estimate the effects, using Equation 1, of the treatment information on training behaviour, but for the sub-sample of individuals who do not open the email. The estimates are shown without control variables. Confidence intervals are shown at 90% (thickest line,) and 95% (thinnest line).

In Figure 9, we run a placebo test on the sub-sample of individuals who do not open the email. Among these individuals, neither treated or control individuals should have read the information on shortage occupations and related trainings and we should therefore not find any differences in training enrolment between individuals assigned to the treatment and control group among non-openers. Reassuringly, Figure 9 shows that we find no significant effect of our intervention campaign on this placebo group.

8 Conclusion and Discussion

In this paper, we conduct a large-scale field experiment in the labour market to explore whether labour shortages can be addressed through a low-cost information intervention on shortage occupations and related trainings. We focus on the entire population of nearly 100,000 individuals who enrolled at the Walloon PES between July and December 2021. The treatment in our experiment consists in sending UI recipients information about the existence of shortage occupations and the availability and advantages of related training programs.

Using survey and administrative data, we estimate effects of the information treatment on perceptions about training programs in shortage occupations, the likelihood of enrolling and finishing such a training, and subsequent employment. Our findings show that the treatment email increased intentions to enrol in trainings, as well as the relative importance jobseekers attributed to trainings as an efficient job search strategy. However, we do not observe any change in training enrolment at this stage. The absence of any short-term effect on training enrolments six months after the dispatch of the email does not mean that there will be no effect in the longer-term, however. In Belgium, many training programs begin in September or October, whereas our information intervention took place in late October (wave 1) and early February (wave 2). This means that the outreach might impact jobseekers' training enrolment, but that we have not yet had time to observe these effects. Moreover, although the email itself might not have increased training take-up, it might have increased the likelihood that jobseekers mention these trainings (and shortage occupations) in their discussions with their caseworker. Because jobseekers meet with their caseworkers every four months during their first year on UI, enrolment in training might be somewhat delayed and training effects could therefore take some time to materialize. Future versions of this paper will investigate whether we observe such longer-term effects on training.

Our findings also show that the information intervention did not affect job search behaviour and, therefore, employment probability. Our survey results indeed show that the intervention did not impact the perceptions about how useful it is to search for a job in a shortage occupation. Our administrative data is consistent with this finding; it shows that the treatment email had no effect on the probability that treated jobseekers change the occupations they search a job in, or the number of times they log into their personal space on the PES website. Ultimately, we thus find no short-term effect on employment through a change in focus in treated individuals' job search. Analyses of further waves of the data will allow us to explore whether employment probability is affected through an increase in training enrolment.

Appendix

A.1 List of Shortage Occupations

Table A1: List of Shortage Occupations

Occupation	**Shortage 2021**	**Shortage 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/Builidng 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 Eligibility Conditions for Job Search Exemptions

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 his/her last studies (preceding the first recipe 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 e 350-2000 bonus and receive special coaching to prepare them for interviews.

A.3 Treatment and Control Emails

Figure A1: Control Group Email

A.4 Point Estimates Presented in Graphical Evidence

Figure A2: Treatment Group Email

Table A2: Effects on Training Intentions and Perceptions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	8)	(9)
	Intentions	**Perceptions**							
	Follow a training	Search Shortage Occupation	Training	Training in Shortage Occupation	Meet with Caseworker	CV and Mot. Letter Coaching	Search Job Experienced	Use Employer Subsidies	Use Network
Treatment effect β	0.0397**	-0.0121	0.0377**	0.0276	-0.0364**	0.0184	0.0175	0.0109	-0.0053
	(0.0182)	(0.0181)	(0.0180)	(0.0184)	(0.0172)	(0.0184)	(0.0174)	(0.0183)	(0.0184)
Constant	0.550***	0.409***	0.589***	0.476***	0.337***	0.486***	0.655***	0.440***	0.518***
	(0.0117)	(0.0116)	(0.0115)	(0.0118)	(0.0110)	(0.0118)	(0.0112)	(0.0117)	(0.0118)
Control variables	No	No	No	No	No	No	No	No	No
Observation	3,049	3,049	3,049	3,049	3,049	3,049	3,049	3,049	3,049
R-squared	0.002	0.000	0.001	0.001	0.001	0.000	0.000	0.000	0.000

Note: This table shows the estimated effect, using Equation 1, of being sent the treatment information 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 training intentions in the upcoming year. "Perceptions" refer to the probability of associating job search strategies with a high or very high impact on job finding, in contrast with associating it with a mixed, low or no impact. Standard errors in parentheses are clustered at the individual level. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Table A3: Effects on Training Activities

	(1) Information Session	(2) Any Training	(3) Training in Short. Occ.	(4) Long Training
β_1 (ITT)	0.00154	0.00257	-0.00166	0.00358**
	(0.00170)	(0.00229)	(0.00128)	(0.00176)
β_2 (LATE)	0.0198	0.0329	-0.0213	0.0460**
	(0.0218)	(0.0294)	(0.0165)	(0.0226)
Mean outcome control group	0.0397***	0.0752***	0.0235***	0.0420***
	(0.00120)	(0.00162)	(0.000905)	(0.00124)
Observations	53,822	53,822	53,822	53,822

Note: This table shows the estimated effect, using Equation 1, of being sent the treatment information on training behaviour. "Any training activity" refers to enrolling in any kind of training activity, including information sessions, trial days, languages courses and tests, etc. "Training in a Shortage Occupation" refers for enrolling in a training that is focused on shortage occupations in particular. "Long training (¿30 days)" refers to enrolling in a training program that lasts at least 30 days. "Information Session" refers to participating (in person) to an information session on a specific training. "Trial Days" refers to taking part in a few on-the-job days where jobseekers can learn more about the occupation they would train for, before signing up for the training. Standard errors in parentheses are clustered at the individual level. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Table A4: Effects on Employment and Job Search

	(1) Employment	(2) Search Short. Occ.	(3) Change Search	(4) Number Occ. Searched	(5) Number New Access
β_1 (ITT)	0.0032	0.00973**	-0.00195	0.0221	-0.127**
	(0.00431)	(0.00431)	(0.00241)	(0.0189)	(0.0564)
β_2 (LATE)	0.0412	0.125**	-0.0251	0.284	-1.628**
	(0.0553)	(0.0553)	(0.0309)	(0.242)	(0.725)
Mean outcome control group	0.508***	0.513***	0.0864***	3.287***	1.675***
	(0.00304)	(0.00304)	(0.00170)	(0.0133)	(0.0398)

Note:

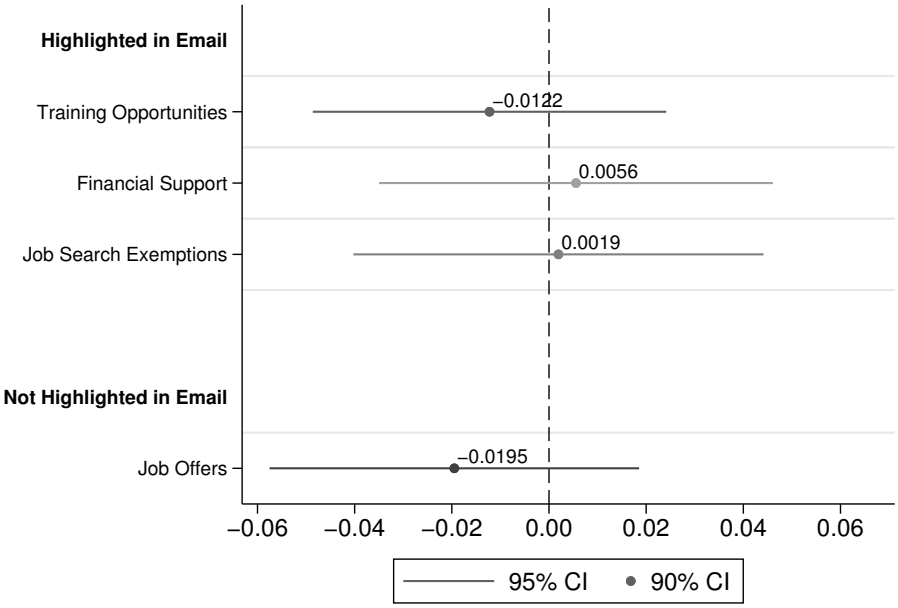
A.5 Additional Findings

Effects on Training Intentions and Perceptions

Figure A3 shows that the treatment information did not affect how supported jobseekers felt in their training endeavours. There is indeed no effect of the treatment information on the

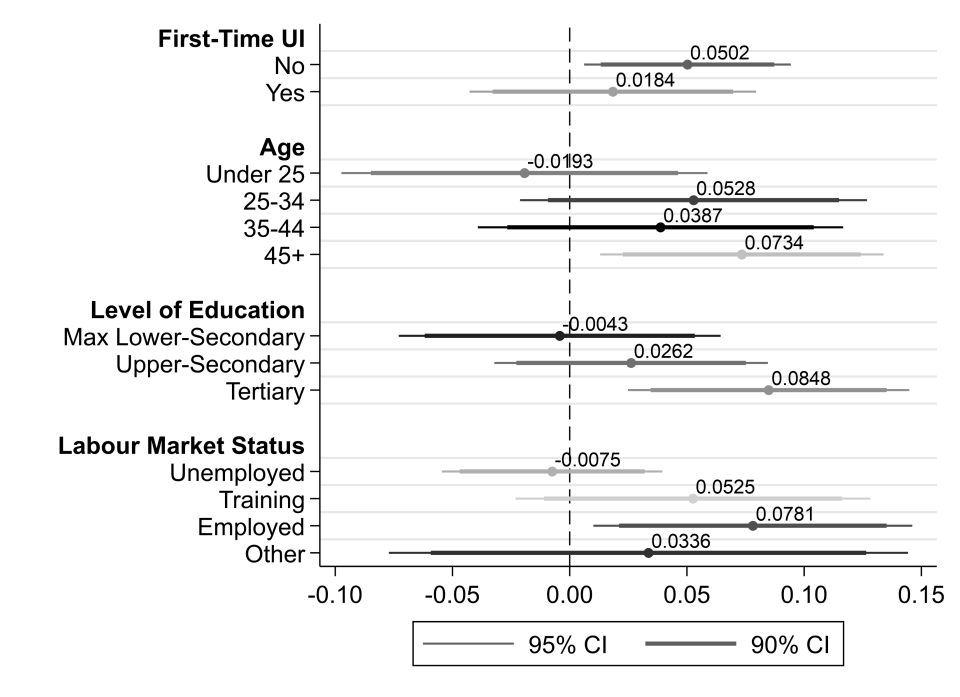
likelihood that individuals answer positively to feeling supported by the PES in terms of the number of training opportunities offered, financial support for training, or exemptions from job search obligations while enrolled in training.

Figure A3: Treatment Effect on Perceptions About Support from PES to Follow Trainings



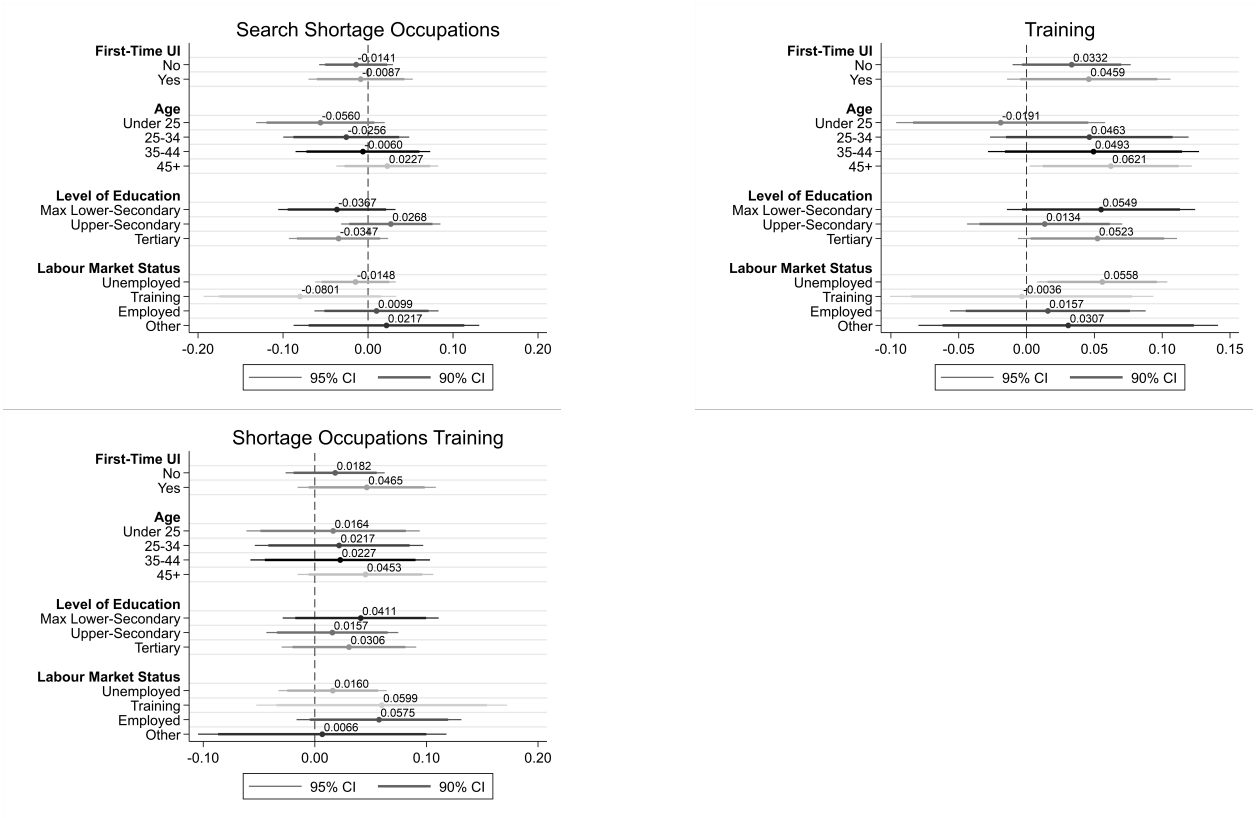
Notes: This figure shows the estimated effect, using Equation 1, of being sent the treatment information (ITT), and of reading it, on the probability of agreeing with statements on the support provided by the PES to follow trainings. The outcome of interest "Training Opportunities" refers to the likelihood of agreeing with a statement that the PES offers many training opportunities. The outcome of interest "Financial Support" refers to the likelihood of agreeing with a statement of feeling supported in their training aspirations thanks to the financial support provided by the PES. The outcome of interest "Job Search Exemptions when in Training" refers to the likelihood of agreeing with a statement of feeling supported in their training aspirations thanks to the job search exemption they benefit from when they are enrolled in a training program. Finally, the outcome of interest "Job Offers" refers to the likelihood of agreeing with a statement that the PES offers them many job openings and serves as a placebo as this dimension was not addresses in the treatment emails. Confidence intervals are shown at 90% (thickest line,), 95% (regular line) and 99% (thinnest line) levels. The estimates are shown using control variables but do not change significantly when these are removed.

Figure A4: Treatment Effects on Training Intentions – Heterogeneity



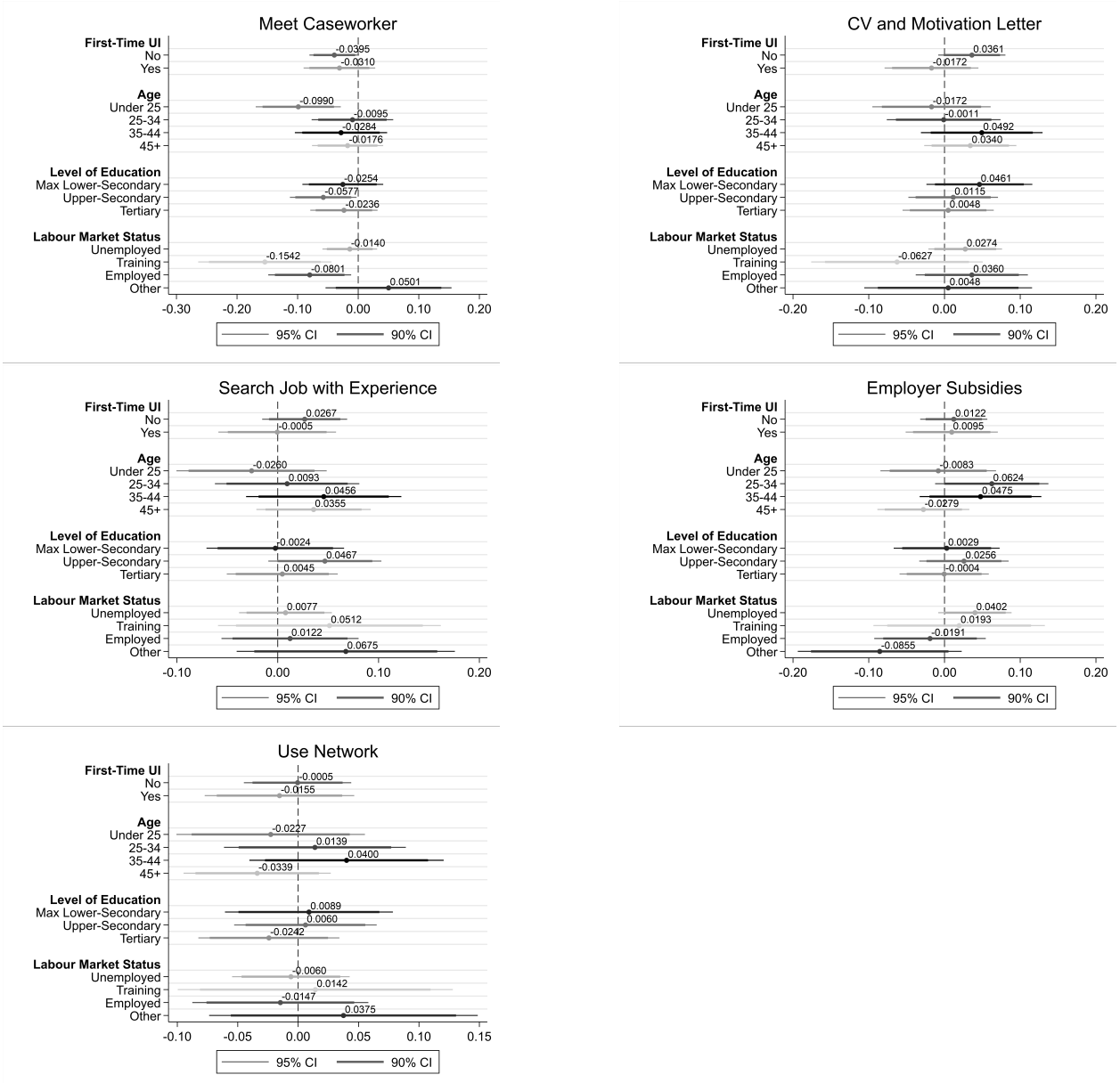
Notes: This figure shows the estimated effect, using Equation 1, of being sent the treatment information on training intentions for different sub-groups of survey respondents. Estimates are shown without control variables. Confidence intervals are shown at 90% (thickest line,) and 95% (thinnest line).

Figure A5: Effect on Perceptions About Job Search Strategies – Heterogeneity (1)



Notes: This figure shows the estimated effect, using Equation 1, of being sent the treatment information on perceptions about the usefulness of different job search strategies, for different sub-groups of survey respondents. Estimates are shown without control variables. Confidence intervals are shown at 90% (thickest line,) and 95% (thinnest line).

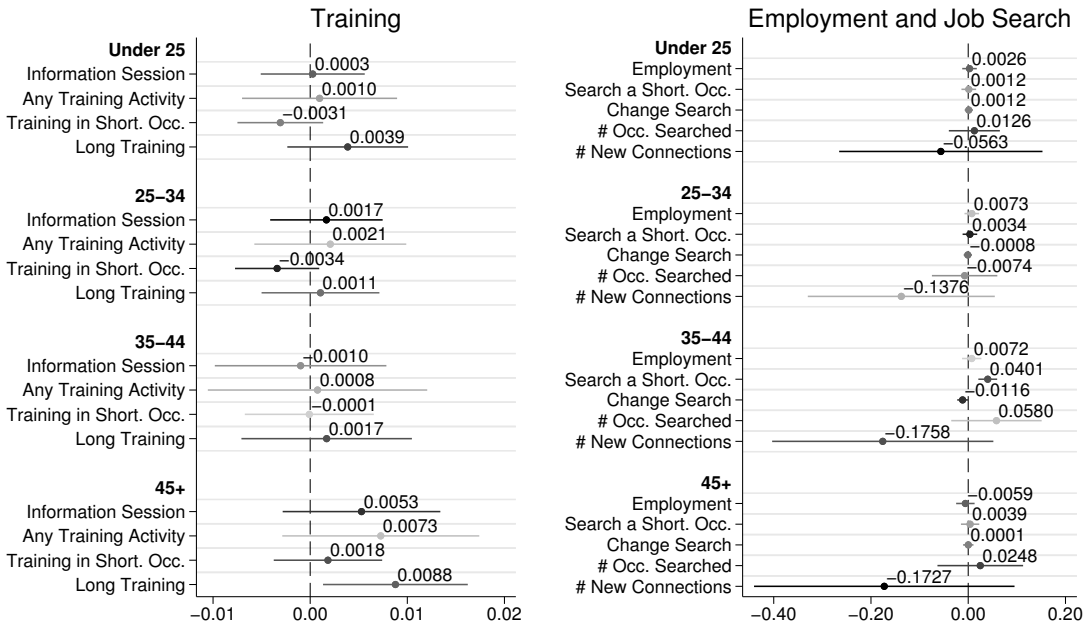
Figure A6: Effect on Perceptions About Job Search Strategies – Heterogeneity (2)



Notes: This figure shows the estimated effect, using Equation 1, of being sent the treatment information on perceptions about the usefulness of different job search strategies, for different sub-groups of survey respondents. Estimates are shown without control variables. Confidence intervals are shown at 90% (thickest line,) and 95% (thinnest line).

Effects on Training, Job Search and Employment

Figure A7: Heterogeneous Effects - Age

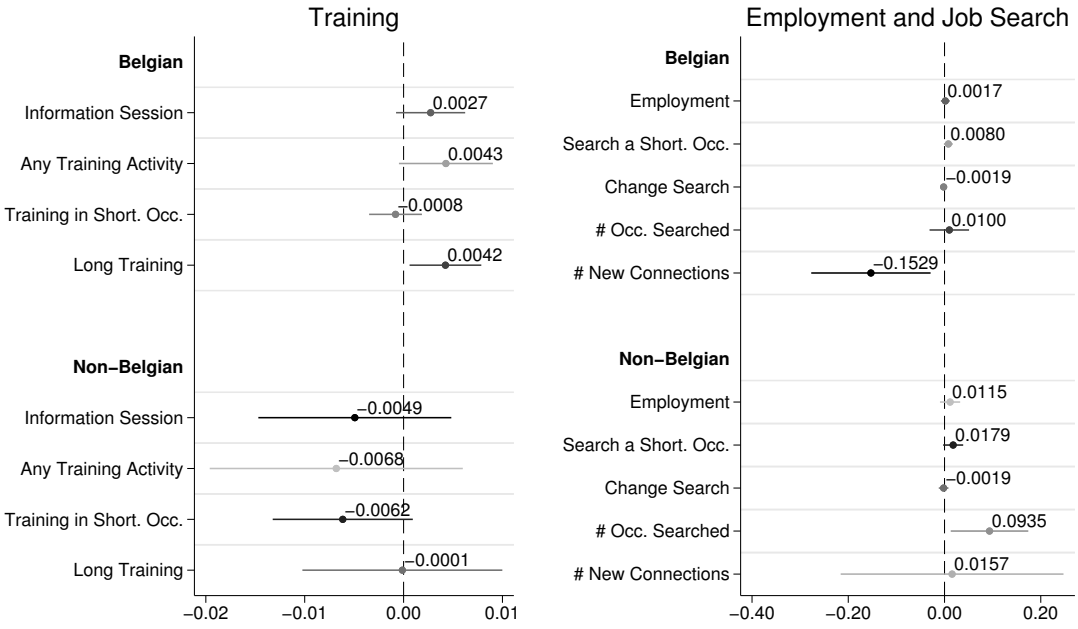


Notes: This figure shows the estimated effect, using Equation 1, of being sent the treatment information on training, employment, and job search behavior. The estimates are shown without control variables and confidence intervals are shown at 95%.

Figure A8: Heterogeneous Effects - Education

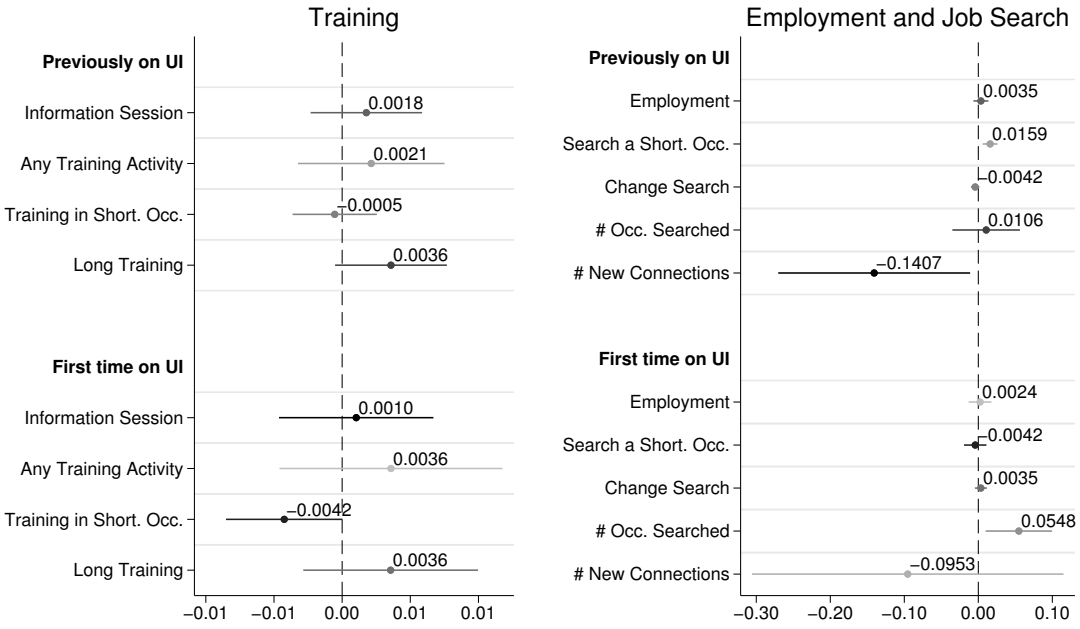
Notes: This figure shows the estimated effect, using Equation 1, of being sent the treatment information on training, employment, and job search behavior. The estimates are shown without control variables and confidence intervals are shown at 95%.

Figure A9: Heterogeneous Effects - Citizenship



Notes: This figure shows the estimated effect, using Equation 1, of being sent the treatment information on training, employment, and job search behavior. The estimates are shown without control variables and confidence intervals are shown at 95%.

Figure A10: Heterogeneous Effects - Previously on UI



Notes: This figure shows the estimated effect, using Equation 1, of being sent the treatment information on training, employment, and job search behavior. The estimates are shown without control variables and confidence intervals are shown at 95%.

A.6 Additional Robustness

Table A5: Treatment Effects on Training Intentions and Perceptions – With Control Variables

	Intentions	**Perceptions**							
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Follow a training	Search Shortage Occupation	Training	Training in Shortage Occupation	Meet with Caseworker	CV and Mot. Letter Coaching	Search Job Experienced	Use Employer Subsidies	Use Network
Treatment effect β	0.0334*	-0.0094	0.0413**	0.0315*	-0.0343**	0.0207	0.0215	0.0118	0.0000
	(0.0178)	(0.0181)	(0.0180)	(0.0184)	(0.0172)	(0.0184)	(0.0174)	(0.0183)	(0.0182)
Constant	0.550***	0.409***	0.589***	0.476***	0.337***	0.486***	0.655***	0.440***	0.518***
	(0.0117)	(0.0116)	(0.0115)	(0.0118)	(0.0110)	(0.0118)	(0.0112)	(0.0117)	(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
R-squared	0.050	0.007	0.007	0.006	0.009	0.007	0.012	0.009	0.030