Reframing Active Labor Market Policy: Field Experiments on Barriers to Program Participation

Lukas Lehner* Anna Schwarz†

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Abstract

Governments struggle to attract unemployed workers to their widely offered job training programs. In three randomized field experiments with 50,000 job seekers, we investigate the barriers to participation in job training programs by using informational interventions designed to encourage participation. Raising awareness about the availability of job training increased program enrollment by 18%. Signaling program cost with a voucher on top to reduce internalized stigma increased completion by 28%. Effects were sizable and concentrated among women and low-income job seekers. Notably, increased job training did not result in higher employment or wages. These findings indicate that while low-cost informational interventions effectively boost participation, the overall success of job training programs in enhancing employment prospects hinges on their fundamental design.

Keywords: job training, program participation, information friction, social stigma, field experiment
JEL codes: J64, J68, C93, D04, D83

*Department of Social Policy and Intervention, University of Oxford. lukas.lehner@spi.ox.ac.uk.
†Department of Economics, Vienna University of Economics and Business. anna-magdalena.schwarz@wu.ac.at..

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1 Introduction

"No one cares if a course fits your profile or not. The main focus is that you are no longer adding to the unemployed numbers."

– Job seeker in our survey, Austria (2021).

"I am not a blip on a computer screen or a national insurance number, I am a man."

– Job seeker in the Ken Loach's movie "I, Daniel Blake" (2016).

Modern welfare states provide comprehensive social support to disadvantaged people including to unemployed workers. However, take-up of benefits, public services, and social programs by eligible populations is incomplete (UN Special Rapporteur, 2022). As a main pillar of active labor market policies (ALMP), public employment services (PES) provide training to job seekers to improve their re-employment prospects. While governments spend large amounts of public budgets on these programs, many job seekers are hesitant to participate.[1] Information frictions from a lack of awareness and psychological frictions from social stigma attached to public training programs constitute potential barriers faced by job seekers to engage in training (Heckman and Smith, 2004) that recently received renewed attention (Dhia and Mbih, 2020; Leduc and Tojerow, 2023; Muller et al., 2023). Following one explanation, individuals eligible for benefits or social programs are simply not aware of their eligibility and face administrative burdens in accessing their benefits and services (Altmann et al., 2018; Barr and Turner, 2018; Belot et al., 2019; Haaland et al., 2023). Following another explanation, psychological frictions discourage eligible groups from accessing their entitlements even if they know about the programs (Bhargava and Manoli, 2015; Finkelstein and Notowidigdo, 2019; Goldin et al., 2022; Linos et al., 2022a). Such reasons are particularly likely to explain why unemployed people are often seen as failing to contribute, which results in shame and social stigma attributed to reliance on the welfare state (Goffman, 1963; Moffitt, 1983; Walker, 2014; Bursztyn and Jensen, 2017). It raises the question whether eligible individuals are simply not aware of the welfare support available to them, or whether psychological frictions associated with shame and stigma discourage them from accessing benefits, services, and programs including training for job seekers.

This paper investigates why unemployed workers are hesitant to participate in training programs. Job seekers can choose among a wide variety of training programs at no financial cost as long as their choices are deemed reasonable by the PES. However, caseworkers struggle to fill their training programs with unemployed job seekers who are hesitant to participate. We examine

[1] Governments spend on average 0.5% of GDP across OECD countries with up to 2% in European countries with the most developed active labor market programs (OECD, 2023). On average, less than 1% of the labor force participates annually in job training programs.

barriers for job seekers' hesitancy to engage in training including the role of information frictions and psychological frictions. Answering this question sheds light on how to overcome barriers to increase training to improve human capital and skills in the labor force. Further, it helps to understand whether and under what conditions training helps job seekers to get re-employed. In a broader context, it adds to the understanding of possible barriers that discourage disadvantaged groups from accessing public services resulting in non-take-up.

Experimental Design We answer this question with a set of three multi-armed field experiments at scale with 50,000 job seekers. This paper's current version reports the findings of the first of three experiments. The experiment consists of three treatment arms in which e-mails with varying content on job training were sent to unemployed job seekers. The intervention was implemented in the first quarter of 2021 by the PES of Lower Austria (*Arbeitsmarktservice Niederösterreich (AMS NÖ)*). The goal was to increase enrollment in training with the aim of increasing re-employment of job seekers.

We randomly allocate 11,000 unemployed job seekers in Lower Austria to three treatment groups and one control group. The first treatment group receives an e-mail with information on training programs offered by the PES; the second treatment group additionally receives a training voucher to be redeemed with the PES up to a value of €15,000; the third treatment group additionally receives information on which occupations have the most open vacancies. The intervention consists only of the variation in the information provided with all options and obligations kept constant for individuals in all four groups. The treatments are stacked and designed to separate out interacted effects of raising awareness (treatment 1), combined with signalling the training program's monetary value (treatment 2), and combined with providing information on the labor market (treatment 3). We observe training and employment from administrative records as our main outcomes. We link those to our own survey data of participants' training intentions, beliefs, and experiences to uncover mechanisms for assigning job seekers to job training. The current paper version includes results up to two years after the intervention.

Main findings Our main empirical findings can be divided in two areas: training and employment among unemployed workers. For average **training outcomes**, three sets of findings are noteworthy. First, **raising awareness** has a large positive effect on training enrollment; **signaling the monetary value** on top helps to improve program completion. The magnitude of an 18%-21% increase in training enrollment compared to baseline is striking given that the intervention consists of only one e-mail. The increase is sustained over a two-year period with recipients still 10% more likely to have participated in job training, which shows that the intervention does not only encourage job seekers to train earlier but increases overall training. The effect is stronger on training completion than enrollment indicating a positive effect on completion even among always-takers. This implies that unemployed workers who would have participated in training without the intervention are more likely to complete training programs due to the intervention. Signalling the monetary value of job training—highly stigmatized programs—increases program completion beyond its increase in participation. The increase in program completion amounts to 28% compared to baseline, and compares to a 19% increase for raising awareness.

Second, job training increases unevenly across programs and results in **spillovers** on other active labor market programs. The increase in job training is driven by a relative shift towards more rigorous programs. Both the e-mail and voucher increase enrollment in programs with longer duration, which are typically oriented toward acquiring job-related skills and human capital formation. Both treatments increase participation in examined programs, which are more rigorous and provide a certificate for successful completion. Signalling the monetary value leads to a larger increase in completion of ambitious programs beyond the increase in participation. Spillovers on other active labor market programs are not negligible. Increasing training drives a substitution of enrollment in other ALMPs, in particular for application courses and subsidized employment. Application course enrollment decreases by about half of the increase in job training. Subsidized employment also shows signs of decline. Raising awareness, thus, spurs substitution of job search and hiring subsidies with training programs.

The average results are driven by substantial **heterogeneity** across sub-groups. Effects are concentrated among disadvantaged groups: women and job seekers with lower income. Both groups are more likely to enroll in training programs at baseline and drive the training increase by a strong response to the information intervention.

Third, reducing information frictions on labor markets can have **unintended consequences**. Informing job seekers which occupations have the highest numbers of open job vacancies results in null effects, cancelling out any positive effects from raising awareness and signaling the monetary value of training programs (treatment 3: e-mail + voucher + occupation information). Those occupations with the highest number of open vacancies are viewed as unattractive, in particular to job seekers with better prospects, as the heterogeneity analysis and surveys reveal.

Turning to average **employment outcomes**, we find no positive effects of training programs on labor market outcomes. Using intention-to-treat (ITT) and instrumental variable (IV) estimation approaches, we find that training programs do not increase re-employment rates or wages of unemployed job seekers. The findings are robust to a number of variable definitions with no signs of meaningful heterogeneity across types of training programs or sub-groups of unemployed workers.

Implications The results demonstrate the potential of information provision in overcoming barriers for disadvantaged populations. Raising awareness to reduce information frictions (treatment 1: e-mail) and framing information to reduce psychological frictions (treatment 2: e-mail + voucher) increase training to foster human capital formation. Providing information does not always work in the same way. It can also have unintended consequences, such as discouraging unemployed workers from training (treatment 3: e-mail + voucher + occupation information).

Literature Job training is a key pillar of active labor market policies, widely studied in labor economics. Heckman and Smith (2004) suggested in a descriptive analysis that the **lack of**

awareness of program eligibility is a major determinant of job training participation. Experimental studies have shifted attention to studying the effect of messages to reduce information and psychological frictions as summarised by Haaland et al. (2023). Each of our treatment arms resembles interventions tested contemporaneously in separate experiments in different countries. Our study allows us to separate the interacted effects from addressing information frictions from a lack of awareness of training (compare treatment 1 to Leduc and Tojerow (2023) in Belgium), psychological frictions associated with training programs (compare treatment 2 to Dhia and Mbih (2020) in France), and information frictions on labor demand (compare treatment 3 to Muller et al. (2023) in the Netherlands). We compare results in Section 6. Contrary to our study, the shift in training intentions through information provision did not translate into training enrollment in Dhia and Mbih (2020) and Leduc and Tojerow (2023). Previously, Barr and Turner (2018) used quasi-experimental variation to show for the U.S. that letters sent from the PES informing job seekers about benefits and costs of training substantially increase training participation. Treatment 3 in our intervention contains information on labor demand by occupation, which parallels the experiment by Muller et al. (2023). In line with our study, they find no impact on received benefits and aggregate earnings. By separating out the interacted effects, our experiment further contributes to studies on the provision of job search information (Altmann et al., 2018; Belot et al., 2019; Briscese et al., 2020; Barbanchon Le et al., 2023).

We study job training as an archetypical social program thereby contributing to the public finance literature on barriers to social program take-up (Moffitt, 1983; Bertrand et al., 2000; Currie et al., 2001; Dahl et al., 2014; Finkelstein and Notowidigdo, 2019; Anders and Rafkin, 2022). **Psychological frictions** such as social stigma are suggested as important reasons for non-take-up of benefits (Bursztyn and Jensen, 2017; Friedrichsen et al., 2018; Celhay et al., 2022). A number of field experiments study social benefit take-up in the U.S. They find that provision of information to raise awareness, corresponding to our treatment 1, increases take-up (Bhargava and Manoli, 2015; Goldin et al., 2022), while framing interventions to overcome psychological frictions by reducing stigma, corresponding to our treatment 2, do not have an added benefit (Bhargava and Manoli, 2015; Linos et al., 2022a). The framing of messages, however, does matter in other contexts (Linos et al., 2020; Lasky-Fink and Linos, 2022; Linos et al., 2022b; Osman and Speer, 2023)[2] What differs is that most studied programs are entitlement programs in which participation primarily depends on the decisions of eligible individuals to apply. By contrast, participation in job training depends on the choices of both eligible individuals and caseworkers (Zweimüller and Winter-Ebmer, 1996; Heckman and Smith, 2004). This assignment mechanism is key for social stigma creation: a qualitative study shows that voluntary participation in ALMPs is positively evaluated by employers whereas mandatory assignment by the caseworker is negatively evaluated by employers (Fossati et al., 2021). We contribute by opening the blackbox of program assignment and uncovering mechanisms around autonomy of choice in program assignment. We trace the steps from job seekers' intention to train, the role of caseworkers, training enrollment,

[2]There is no guarantee that framing interventions would always increase program participation and depend on a behaviorally well-informed design (Hervelin, 2021).

and completion for the first time in an experimental study.

The **heterogeneous effects** in job training participation suggest a "Matthew Effect". Groups with higher enrollment at baseline disproportionally increase their training due to the intervention. This may be the result of "access bias" (Bonoli and Liechti, 2018) from training programs disproportionally targeting disadvantaged groups, such as unemployed women who return to the labor force after childbirth. Information interventions in other contexts find that heterogeneity in responses is driven disproportionately by disadvantaged groups, especially by income (Heffetz et al., 2022; Lasky-Fink and Linos, 2022) and education (Barbanchon Le et al., 2023), which corresponds to our results.

On the employment side, our study contributes to the rich body of **active labor market policy** evaluations. Overall, training programs for job seekers are found to have modest positive effects on re-employment and wages as summarized by the meta-analyses by Card et al. (2010, 2018) as well as by extensive reviews (Heckman et al., 1999; Kluve, 2010; Crépon and van den Berg, 2016). However, large differences between program types, context, and across sub-groups exist. Positive employment effects are more pronounced for disadvantaged groups in the labor market including women (Card et al., 2018) and low-wage workers (Katz et al., 2022). Explanations for why we do not find positive employment effects are discussed in Section 6, where we also compare our results to other studies, which, in Europe, are mostly non-experimental.

Our findings contribute to the understanding of **unintended consequences** of active labor market policies (Black et al., 2003; Crépon et al., 2013; Gautier et al., 2018). Unintended consequences may be understood by connecting labor market evaluations with insights from behavioral theory that shape our understanding of job search. Related to the results for our treatment 3, Bandiera et al. (2021) find in a different context that combining training and job search elements leads to worse outcomes than standalone job training. **Discouragement** emerges as the main mechanism behind the result: lower than expected call-back rates lead to negative effects of job search assistance. This form of discouragement stemming from overoptimism is in line with Spinnewijn (2015) and the burgeoning literature on duration dependence that has documented job seekers overoptimism about their employment prospects (Mueller et al., 2021; Maibom et al., 2023; Abebe et al., 2021; Miano, 2023; Adams-Prassl et al., 2023). Overoptimism has also been documented for job seekers in Austria (Böheim et al., 2011). Our results extend current understandings of discouragement as an unintended consequence of labor market interventions by showing that workers can become discouraged when learning about labor demand being concentrated in occupations below their skill level.

Roadmap The rest of this paper is structured as follows. Section 2 provides an overview of active labor market policies and the context of the study. Section 3, building on our pre-analysis plan, details our experimental design and analysis. Section 4 presents our empirical results, which include training, and employment. Section 5 investigates mechanisms behind the treatment effects, including training intentions, caseworker effects, and the relationship between job seekers and caseworkers. Section 6 discusses the results and Section 7 concludes.

Appendix A presents additional details on the design and Appendix B additional results of intervention 1. Appendix C provides details on the complementary survey including the questionnaire and additional results. Appendix D reports the design and results of intervention 2.

2 Background

This section provides an overview of the objectives, history, and types of of active labor market policies. It also discusses training programs in the Austrian context, and their assignment and eligibility criteria. Lastly, the impact of the Covid pandemic on the labor market is reviewed.

Objectives Active labor market policy has an economic policy and a social policy function with its dual objective of raising efficiency in labor markets while promoting equity among unemployed workers. Efficiency concerns have primarily centred around raising employment, improving job-worker matching, and increasing human capital, while equity concerns aim at levelling the risk distribution between unemployed job seekers and providing employment opportunities for disadvantaged groups (Clasen et al., 2016; Boeri and van Ours, 2021; Lehner and Tamesberger, ming). Thereby, ALMPs complement passive labor market policies such as unemployment benefits and early retirement schemes (Ebbinghaus, 2020).

History of ALMP Active labor market policy has a long history. In the 1950s, Sweden pioneered modern ALMP manpower programs in its notorious "Rehn-Meidner Plan" combining expansive macroeconomic policies with ALMPs with the objective of facilitating rapid labor reallocation and up-scaling to raise productivity while sustaining full employment (Weishaupt, 2011). In the late 1960s, Austria followed the Nordic examples and became one of the first countries to introduce far reaching training programs for unemployed workers (Hofer et al., 2013). The sustained increase in unemployment during the 1980s and 1990s resulted in a large up-scaling and convergence of ALMPs across high-income countries (Clasen and Clegg, 2011). Under the "activation" turn in the 1990s (see OECD (1994) for the landmark study at the time), PES introduced increasingly strict benefit conditionality that oblige job seekers to participate in ALMPs once assigned to be eligible for benefits (Bonoli, 2010; Knotz, 2020). Since the 2008 Great Recession, ALMPs continuously expanded the range of programs (OECD, 2018; Boeri and van Ours, 2021) with increasing convergence of activation requirements across high-income countries (Immervoll and Knotz, 2018).

ALMP types Programs can be divided into at least four categories: Job search assistance, training, employment subsidies, and public employment creation (Card et al., 2018). [3] Job search assistance includes one-on-one counseling as well as courses in which job seekers learn job search skills and apply for jobs. These typically focus on job search strategies and CV

[3]For alternative classifications see Vlandas (2013) and Ebbinghaus (2020).

preparation. Training refers to programs focused on sustaining, deepening, and acquiring skills to build human capital, facilitate re-employment, and spur occupational mobility. Employment subsidies incorporate hiring subsidies for employers as well as a smaller subset of funding support to job seekers who found a start up business. Public employment is typically targeted at the group of most disadvantaged job seekers, which includes those with long unemployment spells and health conditions (Kasy and Lehner, 2023). Our intervention is targeted at training programs, but we are able to observe spillovers on other ALMP types.

Job training programs in Austria Training programs in Austria are recognized as among the most developed in the world, and the Austrian PES has served as a role model for other countries. Expenditures for ALMP in Austria are among the highest as a share of GDP across high-income countries (OECD, 2023). Training programs constitute the largest pillar and receive 2/3 of the annual ALMP budget (Hofer et al., 2013). Training offered by the PES includes over 1,000 programs that cover advancing skills within an occupation as well as acquiring new skills to foster occupational mobility (Zweimüller and Winter-Ebmer, 1996; Eppel et al., 2022). Common programs include mechatronics, plumbing, ICT, programming, restaurant management, hotel and catering assistance, and nursing. Program duration varies from a few days to 1.5 years with longer programs offering high quality training for job specific skills. Among training program participants, about 40% graduate with a certificate after successfully passing an exam. Programs with an exam are typically more rigorous. During training enrollment, individuals continue to receive the same amount as their unemployment benefits, which is topped up with a small amount of €4 per day to account for an increase in expenditures during training participation.

The role of caseworkers Caseworkers are street-level bureaucrats employed by the PES as job counselors with several responsibilities. They provide job search assistance and monitor job search effort. They administer benefits and decide on program assignment. Job seekers meet their caseworkers regularly for consultations, where they discuss training opportunities as well as benefits and job search progress. The dual role of caseworkers reflects a deeper ideological divide about emphasizing welfare provision to unemployed workers versus making welfare contingent on demanding active job search and work availability.

Every unemployed job seeker is eligible to participate in training programs. While program participation comes at no financial cost to job seekers, attendance is mandatory and repeated no shows risk sanctions such as benefit cuts.

Unemployed workers can express interest in a large number of ALMPs, but caseworkers have the final say for program assignment. Unlike application courses to which caseworkers occasionally assign job seekers with the aim of "restoring work morale", assignment to training programs is intended to follow job seekers' interest. In practice, 87% of caseworkers in Austria report that they assign job seekers to training programs of their choice, while only 6% report "exercising pressure" when assigning training programs (Schönherr and Glaser, 2023). By contrast, application courses serve more frequently as a disciplining device, with 20% of caseworkers reporting they make assignments to "exercise pressure". Another motive for how caseworkers assign programs is "meeting their target", which is equally the case both for application courses and training programs.

Covid pandemic Our intervention took place in February 2021 as part of a broader PES campaign *Corona Joboffensive* to promote job training programs. The intention was to prepare job seekers for the recovery phase post-lockdown, given the low likelihood of immediate re-employment during the lockdown period. This lockdown extended from November 2020 to May 2021, with temporary easing occurring between February and March 2021. The PES received additional funding and increased training capacity massively from February 2021, which led to a virtually unlimited supply of training programs only constrained by the demand of job seekers (Leitner and Tverdostup, 2023). The majority of training programs took place in person with safety measures in place while some programs moved online. The type of training programs offered was not affected by the pandemic.

3 Study design

We designed a field experiment at scale in a natural context (Harrison and List, 2004) to test whether information provision increases training and employment of job seekers. Job seekers receive an e-mail from the PES with varying content by treatment group to inform them about training opportunities. In this section, we provide an overview of the data and sample selection in Section 3.1, experimental design in Section 3.2, identifying assumptions in Section 3.3, and our approach to estimation and inference in Section 3.4. Tables and figures to describe the treatment assignment are shown in Appendix A.

The study design was pre-registered and is documented in the pre-analysis plan (AEARCTR-0007141).[4] The experiment was approved by the Departmental Research Ethics Committee at the University of Oxford and by the Competence Center for Experimental Research at the Vienna University of Economics and Business.

3.1 Data

Administrative records We leverage a wide range of demographic, benefit, and job characteristics from administrative data including (i) the PES internal registry for administrative data on unemployed workers (AMS Data Warehouse); and (ii) the "occupational-career data" (Erwerbskarrierenmonitoring, EWKM), accessed via the AMS internal registry for social security registry data. Due to our reliance on administrative data, we face virtually zero attrition.

Surveys Additionally, we survey participants and link the data with the administrative records at the individual level. We collect detailed data on training intentions, experiences and perceptions,

[4]The code implementing the study design was uploaded prior to the implementation of the intervention to GitHub at https://github.com/lukaslehner/Vouchers.

interactions with caseworkers as well as job search behavior and reservation wages. The surveys are distributed via e-mail to all individuals in our sample. We send the e-mails as researchers, ensure respondents' anonymity, and communicate our independence from the PES. We design the questionnaire using Qualtrics following Stantcheva (2023). Section C.1 provides a sample of our survey questionnaire.[5] We achieve a response rate of 30%, which is relatively high compared to related studies (Dhia and Mbih, 2020; Leduc and Tojerow, 2023; Muller et al., 2023).

Sample The sample of our first experiment includes the population of unemployed workers in Lower Austria with an unemployment spell of either 2-3 or 6-12 months at the time of treatment.[6] Unemployed job seekers who are already enrolled in a training program or who have a job offer accepted at the time of the intervention are excluded from the sample. The sample is further restricted to people who are at least 25 years old.[7]

This leaves us with 11,050 unemployed workers (Table 1 column (3)).[8] Among them, 52% are women, 30% are younger than 35, and about 32% are older than 50. A third has no more educational attainment than compulsory schooling. Just over 1/5 has a foreign nationality and an equally large share has a health restriction preventing them from working in certain occupations. With respect to language, 14.5% speaks only limited or no German.

Overall, our sample is very similar to job seekers across Austria (Table 1 column (4)), despite Lower Austrian job seekers being more likely to have Austrian nationality. We also compare our sample to the population of job seekers before the pandemic (Table 1 column (1-2)). A high share of lay-offs took place at the start of the pandemic in March 2020, which explains the higher share of unemployed workers with a duration of 9-12 months in our sample. Among them, a higher share had minimum educational attainment and non-Austrian nationality. A smaller share of job seekers in the sample had a health restriction compared to unemployed job seekers before the pandemic. With regard to gender and age, the composition remained broadly the same.

[5] The full questionnaire was pre-registered on the AEA RCT Registry at https://www.socialscienceregistry.org/versions/87136/docs/version/file.

[6] All unemployed workers with a spell of 3 to 6 months received the information treatment 1 without control group two weeks prior to the experimental intervention and thus could not be included in the randomized experiment.

[7] The PES runs specific programs for younger job seekers.

[8] The sample for the analysis is reduced to 10,714 since observations with missing values are excluded. Missing values include mainly nationality and occupation as well as in few instances education and pre-unemployment income.

Table 1: Sample representativeness across time and states

	Lower Austria			Austria
	Feb.19	Feb.20	**Feb.21**	Feb.21
Total	5551	6540	**11050**	71487
Gender				
Women	53.4%	51.7%	**51.9%**	49.4%
Men	46.6%	48.3%	**48.1%**	50.6%
Age				
Below 35	30.3%	29.7%	**29.9%**	33.4%
35-50	37.0%	37.1%	**38.5%**	39.4%
Above 50	32.6%	33.1%	**31.5%**	27.1%
Education				
Compulsory education	29.5%	29.0%	**32.5%**	36.3%
Higher than compulsory	70.5%	71.0%	**67.5%**	63.7%
Nationality				
Austrian	82.8%	82.0%	**77.9%**	65.7%
Non-Austrian	17.2%	18.0%	**22.1%**	34.3%
Health				
Health restriction	24.0%	25.8%	**21.3%**	17.5%
No health restriction	76.0%	74.2%	**78.7%**	82.5%
Unemployment duration				
3-4 months	28.5%	30.9%	**24.3%**	28.8%
6-9 months	43.0%	40.0%	**33.9%**	28.9%
9-12 months	28.6%	29.1%	**41.8%**	42.3%
Language skills				
German speaking	89.0%	88.2%	**88.6%**	85.5%
Non-German speaking	11.0%	11.8%	**11.4%**	14.5%
Summary indicators				
Unemployment rate	8.9%	8.7%	**10.0%**	10.7%
In training	16.2%	15.3%	**13.5%**	16.5%

Note: All selection criteria as explained in the text are the same for our sample and the comparison samples.

Outcomes of interest We categorize our outcomes of interest into two main groups: training and employment outcomes. In our main specifications, training outcomes are measured within 12 months after the intervention, whereas employment responses are expected to materialize later, and we thus measure them within 24 months after the intervention.[9] We report descriptive statistics for these outcomes in Table 2. We measure training by enrollment and completion of respective training programs. Our training outcomes in the upper part of the table are all binary and take the value of 1 if the unemployed participated in the specific type of ALMP within 12 months after the intervention. The same holds for employment in the lower part of the table. Participation in job training counts as unemployed. We also measure days in employment and unemployment as well as the average daily wage when the person was employed and construct

[9]Naturally, we also report training outcomes 24 months after the intervention as well as employment outcomes 12 months after the intervention in the respective appendix sections.

an index for job quality. This index can take values between 0 and 1 and is an equally weighted combination of standardized average wage quality and employment continuity, measured as days in employment. We test a range of alternative definitions for robustness presented in Table B11.

Baseline data At baseline, 11% of job seekers enroll in a training program within 12 months after the intervention (column 1), while almost 10% also complete these programs (column 2). Among all the programs, 8% last for 40 days, which is the median duration, or longer (column 3). Longer programs have a stronger focus on equipping job seekers with new skills and human capital formation, while shorter programs often focus on refreshing existing knowledge or adding complementary skills. Close to 5% of job seekers participate in training programs that finish with an exam, which is another indicator for more demanding training programs (column 4). Besides training, the PES provides a range of active labor market programs discussed in Section 2. We present results for enrollment in application courses and subsidized employment to account for spillover effects on other ALMPs.[10] At baseline, 4.5% of job seekers participate in application courses (column 5), while 1 in 4 job seekers finds a job supported by employment subsidies (column 6) within 12 months of starting their unemployment spell.

Concerning employment outcomes in the lower part of the table of Table 2, 75% of job seekers in our sample have been in employment for at least one day within 24 months after the intervention (column 1). During that period, a job seeker is on average 350 days in employment (column 2) and 361 days in unemployment (column 3). Once in employment, their average wage amounts to 51 Euros gross per day (column 4).

[10]Public employment programs are targeted at a different sub-group: the most disadvantaged job seekers with very long unemployment spells and health conditions.

Table 2: Outcome variables descriptives

	Training outcomes (within 12 months after intervention)					
	Training	Training completion	Long training	Examined training	Application courses	Subsidized employment
Mean	0.112	0.094	0.078	0.047	0.045	0.257
SD	0.316	0.292	0.268	0.211	0.208	0.437
Range	0/1	0/1	0/1	0/1	0/1	0/1
Valid N.	11,050	11,050	11,050	11,050	11,050	11,050
	Employment outcomes (within 24 months after intervention)					
	Employment	Days in employment	Days in unemployment	Avg. daily wage	Job quality	
Mean	0.754	350.103	361.954	50.814	0.382	
SD	0.431	310.971	259.588	29.494	0.144	
Range	0/1	0-928	0-934	1.238-217.479	0-1	
Valid N.	11,050	11,050	11,050	7,938	7,527	

Note: The table shows mean, SD, and range of all outcome variables for the control group. Valid N. refers to all non-missing values in the whole sample (i.e., including the three treatment groups).

3.2 Experimental design

Treatment assignment We assigned study participants to one of three treatment groups and one control group using stratified randomization. We used the following covariates to construct the strata: gender, age, educational attainment, region, and unemployment duration. We constructed these variables from raw data for job seekers using the PES internal registry and the social security administrative data described above. All of these variables were used as they were available to the PES in February 2021.

For the stratified randomization, we first divided individuals into strata based on the variables described above. We constructed 145 strata for every possible combination of the values of the 5 strata variables ranging from 10 to 270 individuals per stratum as shown in Figure A1. We then assigned individuals randomly within the strata to one of the three treatment groups or the control group. The randomization procedure resulted in four equally-sized, balanced groups as shown in Appendix A.3. The pre-analysis plan contains further details on the treatment assignment(AEARCTR-0007141) (Lehner and Schwarz, 2021).

Intervention The intervention consists of e-mails sent by the PES with varying information on job training aimed at encouraging job seekers to participate in job training. Participants are not aware of the experiment as characteristic for a natural field experiment (List, 2022). The treatments are stacked on top of each other, i.e., treatment group 2 receives the same e-mail as treatment 1 complemented with a voucher; treatment group 3 receives the e-mail and voucher of groups 1 and 2 complemented with information regarding which occupations have open vacancies. The stacked treatment design allows us to interpret the outcomes as interacted treatment effects. The control group is not contacted but continues to have access to training and regular PES consultations. The formal training assignment mechanism remains the same for individuals of all four groups. The intervention was implemented in February 2021.

Treatment group 1 receives an e-mail with information on PES-provided training programs as shown in Figure A2. The intention is to raise job seekers' awareness of training programs to overcome information frictions that discourage them from participation.

Treatment group 2 includes a voucher for job training programs added to the e-mail as shown in Figure A3.[11] Although training program enrollment is costless to job seekers irrespective of which treatment group they are assigned to, the voucher indicates a value of €15,000.[12] The value was chosen as an upper bound for training program costs as it corresponds to the cost incurred to the PES by their most expensive training programs on offer. By signalling the monetary value of the programs, the treatment is intended to reduce psychological frictions that can discourage job seekers from program participation. These frictions may include internalized stigma about participating in job training (Fossati et al., 2021). The voucher is, thus, solely a way of framing access to training programs that are already available to job seekers.

Treatment group 3 receives a list of occupations with the highest number of open vacancies in addition to the e-mail and voucher as shown in Figure A4. This information is intended to encourage job seekers for training in occupations with high labor demand and broaden their job search beyond their previous occupation. As job seekers are found to search in occupations with relatively few vacancies (Sahin et al., 2014), improving access to information has been shown to broaden job seekers' search and increase the number of job interviews they are invited to (Belot et al., 2019).

E-mail clicks For intervention 2 and 3, we collect data on whether an e-mail was received and opened, and on clicks on hyperlinks in the e-mail to assess whether the intervention was successfully implemented. Figure A5 shows a graphic of the e-mail and hyperlink clicks observed.

3.3 Identifying assumptions

Training outcomes Due to the clean randomization of participants into control and treatment groups, it is possible to compare the relevant outcome variables directly between the 4 groups. This provides us with an unbiased estimate of the treatment effect that does not hinge on any assumptions other than the random assignment into groups. The results for training can thus be interpreted as intention-to-treat (ITT) generalizable to the entire population of unemployed job seekers in our sample (Imbens and Angrist, 1994).

[11] The stacked design is necessary since providing a voucher to signal the monetary value inherently raises awareness as well. While we cannot rule out interaction effects between the treatments, the stacked design allows us to keep the effect of raising awareness about training programs constant across the treatment groups to separate out the interacted effect of signaling the monetary value.

[12] The voucher also includes €3,000 for any training not provided via the PES.

With the additional assumption that all effects are mediated by opening the e-mail, these estimates can be scaled up by the effect of treatment on the probability of opening the e-mail. This yields instrumental variable estimates of the local average treatment effect (LATE) of having received the treatment. The effect of assignment on opening the e-mail is estimated to be around .91, so that the corresponding instrumental variable estimates of all treatment effects on training outcomes would be about 10% higher of the reported ITT effects.

Employment outcomes We rely on the same ITT approach to estimate employment outcomes and additionally use an instrumental variable (IV) approach. Training is driven by those job seekers who enroll in training programs because of the treatment. While this is a small share of 2 percentage points who are shifted at the margin, we report our baseline estimations as ITT, which are generalizable to the entire population.

For the IV approach, we use the information intervention to instrument training. This gives us the LATE, which is representative for compliers, i.e., those job seekers at the margin of enrolling in training (Angrist et al., 1996). Our instrument, the information intervention, is correlated with the endogenous variable, training. Our IV estimation has an F statistic above 10, which is conventionally used as a threshold to qualify strong instruments. Our instrument is as good as random since we randomly assigned it. Our identification rests on the exclusion restriction: our instrument affects the dependent variable, employment outcomes, only through training. In other words, the information intervention itself does not affect employment.

3.4 Estimation and inference

First, we compare the simple means between the treatment and control groups. To increase precision, we estimate parametric regressions for the treatment effects using the following estimation regression:

$$Y_i = \beta_0 + \beta_1 T_1 + \beta_2 T_2 + \beta_3 T_3 + \mathbf{X_i} + s_i + \epsilon_i \tag{1}$$

where Y_i refers to the outcome variables for individual i. Depending on the scale of the outcome variable, an OLS (continuous) or a Logit (binary) regression is used. Our outcome variables are measured at different time periods and for each time period a separate regression is estimated to measure time-varying treatment effects. T_1 to T_3 refer to the treatment groups as described above. Further, as we used stratified randomization, we include strata dummies, following Athey and Imbens (2017). We additionally control for all socio-demographic variables as recorded before treatment $\mathbf{X_i}$ that were not used for stratification. This includes language skills, nationality, occupation, marginal employment, previous wage, and within the past 10 years the days in employment and number of employment spells. Finally, we include caseworker fixed effects.

For employment outcomes, we maximize statistical power by pooling individuals in the treatment groups that increased training (treatment groups 1 and 2). Table B13 presents the employment results for the three treatment groups separately for robustness.

The heterogeneity analysis is conducted via sub-group regressions of the equation above for the variables specified in the pre-analysis plan.

4 Results

Our results are structured around two groups of outcomes: training, in Section 4.1 and employment, in Section 4.2. The training analyses focus primarily on one year after the intervention, while the employment analyses apply to a two-year time frame.

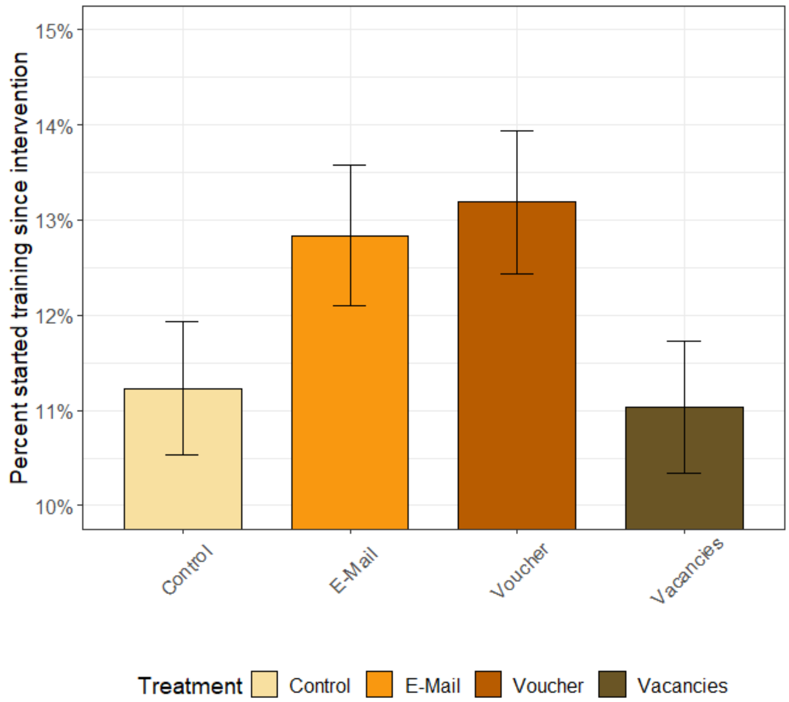
Figures 1-3 and Tables 3-5 present our main results. Appendix B documents additional figures with results for robustness using alternative estimation approaches and variable definitions, timing patterns, and heterogeneity.

4.1 Training

To analyze treatment effects on training, we first present baseline results on training behavior before proceeding to timing patterns and sub-group results.

Main findings The e-mail and voucher treatments both lead to a significant increase in training enrollment. The increase is substantial in magnitude with 20% and 24%, respectively, from baseline (Table 3 column 1), which results in around 13% of treated job seekers participating in training compared to 11% of untreated job seekers (Figure 1). The information on vacancies, by contrast, does not increase training. It is important to keep in mind that the information on vacancies is added to the e-mail and voucher as provided to treatment groups 1 and 2. We can interpret the null effect of treatment group 3, thus, as the vacancy information having a negative effect on aggregate training, which offsets the gains from treatment 1 and 2 in magnitude.

Figure 1: Training enrollment



Note: Confidence intervals are reported at the 90%-level.

The e-mail and the voucher treatments both also increase completion of training programs (column 2). The increase in completion is about the same magnitude as for participation (18% for the e-mail and 26% for the voucher), indicating that all those induced to take up training by the intervention also completed it. Table B.1 shows that this also holds for the different types of training. Additionally, the difference between the voucher and the e-mail is statistically significant for training completion, which indicates that the voucher has an additional positive effect on training.

The treatments also affect the type of training undertaken. Job seekers shift participation to more demanding training programs defined as longer in duration (column 3) and courses with an exam (column 4). At the same time, the increase in training for the e-mail and voucher treatments seems to have a spillover effect on enrollment in other active labor market programs. Around half of the increase in training of job seekers who receive the e-mail can be attributed to a decline in application course enrollment (column 5), which equals a 20% drop. Job seekers who receive the voucher tend find less subsidized employment, which equals the magnitude of the increase in training enrollment (column 6). The results demonstrate that reducing information frictions substantially increases training take-up. The voucher has an additional effect especially on the completion of training programs, suggesting added benefits from reducing psychological frictions.

Table 3: Average treatment effects on active labor market programs

	Dependent variable:					
	Training	Training completion	Long training	Examined training	Application courses	Subsidized employment
	(1)	(2)	(3)	(4)	(5)	(6)
E-mail	0.020**	0.018**	0.015**	0.011*	−0.009*	−0.005
	(0.008)	(0.008)	(0.007)	(0.006)	(0.005)	(0.012)
Voucher	0.024***	0.026***	0.014**	0.008	−0.007	−0.019
	(0.008)	(0.008)	(0.007)	(0.006)	(0.005)	(0.012)
Vacancies	0.0005	0.006	−0.003	0.003	−0.005	−0.019
	(0.008)	(0.008)	(0.007)	(0.005)	(0.005)	(0.012)
Control Mean	0.112	0.094	0.078	0.047	0.045	0.257
Control SD	0.316	0.292	0.268	0.211	0.208	0.437
Observations	10,714	10,714	10,714	10,714	10,714	10,714

Note: Standard errors are in parenthesis: *p<0.1; **p<0.05; ***p<0.01

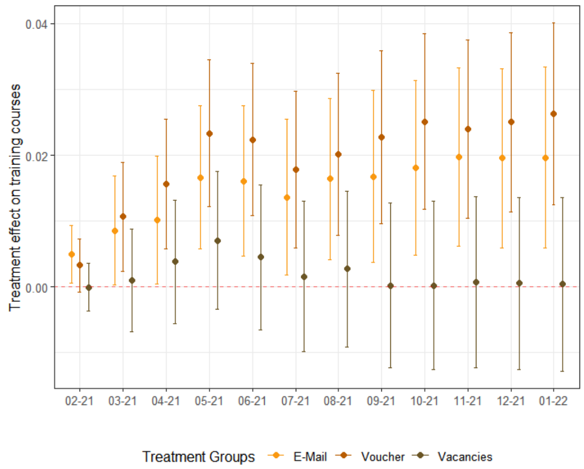
Timing patterns To analyze timing patterns in the treatment effects of active labor market programs as suggested by Card et al. (2018), we investigate the temporal dimension of treatment effects on a monthly basis for 12 months following the treatment. Regarding outcomes, we consider whether job seekers have participated in a training program since the intervention took place. Figure 2 shows the treatment effect on training program enrollment per month.

Findings Within the first 4 months, the voucher increases training enrollment by around 2.5 percentage points and the newsletter by around 1.5 percentage points compared to the control group. The treatment effect plateaus afterwards, as many job seekers who started training remain enrolled in their programs. The two treatments consequently lead to sustained higher training enrollment with no catch-up effect of the control group for the first 12 months after treatment, as can be seen in (Figure B1) on cumulative training enrollment within the first year. By contrast, the vacancies information shows no signs of a significant nor substantial increase in training enrollment.

The treatment effects on training remain substantial for two years after the intervention (Table B2 and Figure B2). After two years, treated recipients still exhibit a 10% higher likelihood of having participated in job training. The intervention's impact, thus, extends beyond merely prompting earlier training among job seekers; it also leads to a sustained increase in training enrollment.

The sustained increase in training goes hand-in-hand with a lasting reduction in other ALMPs' participation. The reduction in application course enrollment starts right after the intervention, reaches its strongest magnitude about 4 months after the intervention, and remains constant thereafter (Figure B3). Reductions in subsidized employment start to emerge only about 5 months after the intervention and intensify over time (Figure B4).

Figure 2: Average treatment effects on training enrollment over time



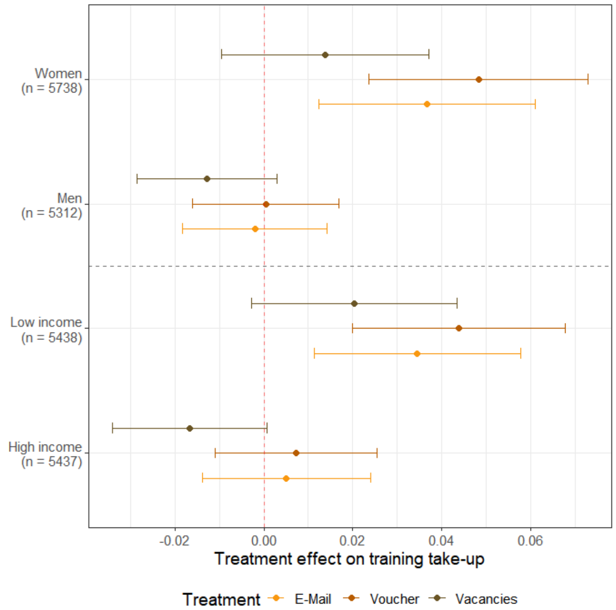
Note: Confidence intervals are reported at the 90%-level.

Heterogeneity To account for heterogeneity, we conducted sub-group regressions of the baseline equation for the main outcome variable. Additional analyses are shown in Section B.2.1.

The overall positive treatment effect is mostly driven by women and unemployed workers with lower income in their previous job (Figure 3 and Table 4). Further, unemployed people older than 35, with Austrian nationality, or white-collar occupations seem to contribute more to the effect. There are no clear patterns by education or language skills. Heterogeneous effects are similar between providing information (e-mail) and additionally signalling the monetary value (voucher).

Treatment 3 (e-mail + voucher + information) results in interesting diverging outcomes for different sub-groups (Table B5). Contrary to treatments 1 and 2, job seekers in blue-collar occupations react more positively than those in white-collar occupations. The same holds for low-skilled compared to high-skilled occupations. The estimates point in a negative direction for comparatively advantaged groups, such as men, higher income, and core age groups, albeit not significantly. In Section 5, we discuss the interpretation of these patterns more in-depth.

Figure 3: Heterogeneity in average treatment effects on training enrollment by gender and income



Note: Confidence intervals are reported at the 90%-level.

Table 4: Heterogeneity in training enrollment by gender and income

	Dependent variable:			
	Training take-up			
	Women	Men	Below median income	Above median income
	(1)	(2)	(3)	(4)
E-Mail	0.034***	−0.002	0.032***	0.005
	(0.013)	(0.010)	(0.012)	(0.011)
Voucher	0.046***	0.0004	0.040***	0.007
	(0.013)	(0.010)	(0.012)	(0.011)
Vacancies	0.012	−0.013	0.018	−0.016
	(0.013)	(0.010)	(0.012)	(0.011)
Control Group Mean	0.137	0.086	0.113	0.102
Control Group SD	0.344	0.28	0.317	0.302
Observations	5,523	5,191	5,363	5,351

Note: Standard errors are in parenthesis: *p<0.1; **p<0.05; ***p<0.01

4.2 Employment

To analyze treatment effects on employment, we first present baseline results on labor market outcomes and then discuss sub-group results. Table 5 reports the results of the 3 treatment arms on employment 24 months after the intervention. To maximize statistical power, we pool individuals in the treatment groups that increase training (treatment groups 1 and 2).

Main Findings Our intervention fails to improve employment status of job seekers within the 24-month period observed. We do not find statistically significant effects for any of the outcomes. However, the coefficients point in a negative direction across a range of outcomes and estimation approaches (Table 5). This pattern suggests negative consequences of training on employment status and wages. The short-term employment effects 1 year after the intervention show the same pattern (Table B10). The coefficient for being in employment at any point after the intervention is negative but not statistically significant. Instrumenting training program participation with the information intervention results also in a negative but non-significant coefficient for employment status (column 2). On average, job seekers in the treatment group spent 6 days less in employment. Days in unemployment also decreased marginally (column 4).[13] Neither wages nor job quality increases with training (columns 5 and 6). The findings are robust across different outcome definitions including income (Table B11) and estimation strategies including IV (Table B12) as well as when observing treatment groups separately (Table B13). Signs of negative employment effects start appearing from 4 months after the intervention and solidify, especially for the voucher group, over a two year period (Figure B5).

Table 5: Average treatment effects on employment

	Dependent variable:					
	Any employment		Days in employment	Days in unemployment	Avg. daily wage	Jobquality
	(1)	(2)	(3)	(4)	(5)	(6)
E-mail + Voucher	−0.008		−6.159	−3.263	−0.086	0.001
	(0.009)		(6.794)	(5.864)	(0.769)	(0.004)
Training		−0.314				
		(0.496)				
Control Group Mean	0.754	0.754	350.103	361.954	50.814	0.382
Control Group SD	0.431	0.431	310.971	259.588	29.494	0.144
Observations	10,714	10,714	10,714	10,714	7,723	7,323

Heterogeneity We do not find significant heterogeneity in employment effects (Appendix B.4). Employment effects tend to be more negative for groups with the stronger increase in training, which suggests that lock-in effects drive the employment effects. This includes women

[13]The categories employment, unemployment and out of labor force sum up to one.

(Table B14), those aged 35 to 50 (Table B15), those with Austrian nationality (Table B16), and those who previously worked in medium-skilled occupations (Table B17). However, the heterogeneous effects are not statistically significant and thus have to be viewed with caution.

5 Mechanisms

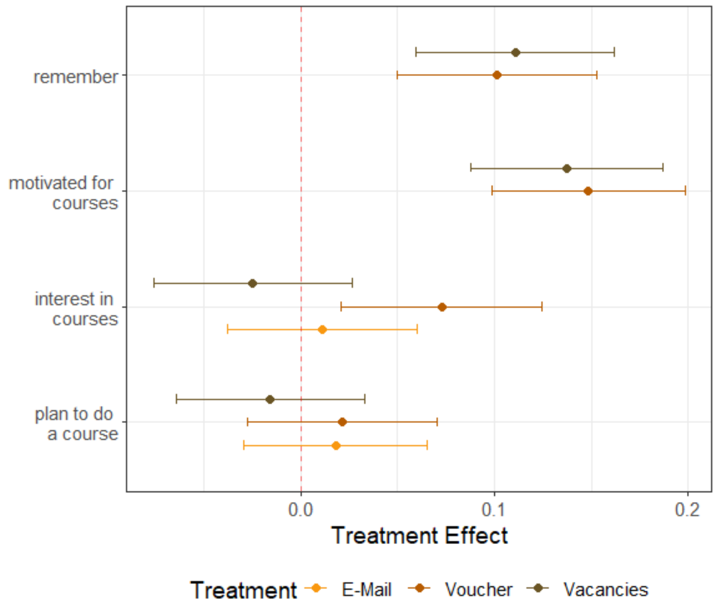
We investigate mechanisms behind engaging in training including job seekers' intentions to train, the role of caseworkers, and the unintended consequences of the vacancy treatment.

5.1 Training intentions

First, we assess the treatments' effectiveness in shifting job seekers' intentions to train. We collect data on intentions with the survey detailed in Section 3 and Section C.1. We compare whether the treatments affect job seekers' intentions, whether intentions translate into enrollment, and whether treatments affect perceptions of job training.

Intentions Signalling the monetary value raises job training intentions (Figure 4). General interest in courses offered by the PES increases after receiving the voucher, especially for those receiving the voucher. Concrete plans to enroll in a program show signs of elevation for e-mail and voucher recipients but the effects are not statistically significant. By contrast, interest and plans for courses seems to decrease for recipients of the vacancies treatment though not statistically significant. Among those who were treated, recipients of the voucher and vacancies information were more likely to remember the information received. Voucher and vacancies information recipients show also higher motivation for courses compared to those who only receive the e-mail. Overall, these results demonstrate that the treatments are successful in shifting job seekers intentions to engage in training.

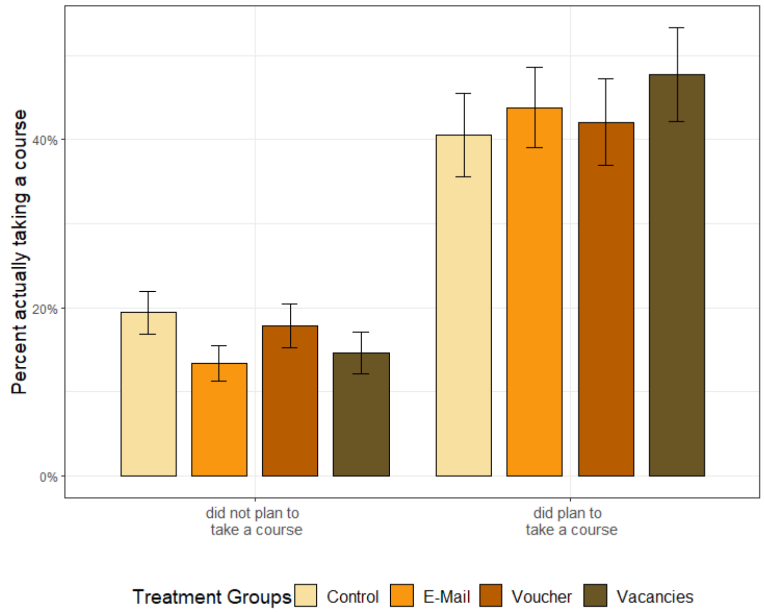
Figure 4: Averate treatment effect on intentions to train



Note: Confidence intervals are reported at the 90%-level.

Intentions and enrollment Intentions for training translate into program enrollment (Figure 5). Among job seekers who planned to take a course, 40%-50% eventually enroll in a program. By contrast, among those who did not plan to take a course, only 10%-20% eventually enroll in a training program. While some job seekers do not follow through on their intentions, job seekers own intentions are found to matter for actual training enrollment, which underscores job seekers' discretion in deciding whether to enroll in a program. Some of those who intend to enroll may not have been accepted by caseworkers, which we investigate further (Table 7). The 10%-20% job seekers who did not plan to enroll but eventually enrolled compare to the 40% who stated to have enrolled in a program because they were assigned to it (Figure D5). Among treatment groups no sizeable differences in the correlation of intentions and actual training enrollment are found. For the control group, a smaller share of those who planned to enroll follow through and enroll compared to the treatment groups. Reversely, the share of those who did not plan to enroll but eventually enroll is higher in the control group compared to the treatment groups. The comparison suggests that among survey respondents, about 5% of job seekers would have enrolled in job training regardless of whether their intention was shifted by the treatments.

Figure 5: Training enrollment by intentions



Note: Confidence intervals are reported at the 90%-level.

Information The intervention shifts perceptions of job training. In particular, the e-mail and voucher treatments raise awareness and signall the monetary value of training (Table 6). Recipients of the e-mail and voucher report less often that they lack information on courses (column 1), which indicates the effectiveness of the treatment in raising awareness and informing job seekers about their training options. In parallel, recipients of the vacancies information tend to report more often that they lack information, which could indicate that the information on occupations with job openings may have provided insufficient content to inform job seekers about their options or that this treatment involved too much information at once. Job seekers who receive the voucher tend to report more often that courses are expensive (column 2), which indicates that the voucher is effective in signalling the monetary value of training programs. While the intervention seems to have shifted perceptions of job training in the way intended, the coefficients are not statistically significant, which is likely related to the lower sample size in the survey data.

Table 6: Perceptions of courses

	Dependent variable:	
	Lack information	Courses are expensive
	(1)	(2)
E-mail	−0.030	0.017
	(0.038)	(0.030)
Voucher	−0.015	0.030
	(0.040)	(0.031)
Vacancies	0.054	0.035
	(0.040)	(0.031)
Reference Mean	0.425	0.64
Reference SD	0.495	0.48
Caseworker Fixed Effects	0	1
Observations	1,145	1,722

Note: Standard errors are in parenthesis: *p<0.1; **p<0.05; ***p<0.01.

5.2 Caseworkers

The intervention increases job seekers' interest in training and their intention to take up training. However, caseworkers need to approve job seekers' assignment to a training program. Increased interest in training from job seekers can, thus, either result in increased enrollment, or increased rejection of job seekers' training intentions. The important role of caseworkers' discretion has been documented in the context of stricter job search requirements (Arni and Schiprowski, 2019). To our knowledge, we are the first to provide evidence on their role in the context of training assignment in a quantitative study.

Assignment The treatments strengthen job seekers self-assessed autonomy over program assignment, which increases rejections of training intentions by caseworkers (Table 7). Recipients of any treatment feel more in control over which course to choose (column 1). However, treated job seekers report less often that their wishes for training program assignment are considered by caseworkers (column 2), suggesting increased discussion about course choice with caseworkers. In consequence, training intentions of treated job seekers are more often turned down (column 3). These outcomes suggest that job seekers feel more autonomy over program assignment but are confronted with the reality of requiring approval by caseworkers. While this indicates the boundaries of increasing perceived autonomy without changing the formal assignment rules, the treatments achieves actual impact on program assignment. Moreover, treated job seekers tend to report less often that assignment to a course by a caseworker is the reason for program enrollment (column 4). The increased interest of job seekers for training also increases the share of job seekers who cannot find a suitable course (column 5). Uncovering these mechanisms helps to understand the potential of information interventions and its limitations by caseworkers and rules.

Table 7: Training program assignment

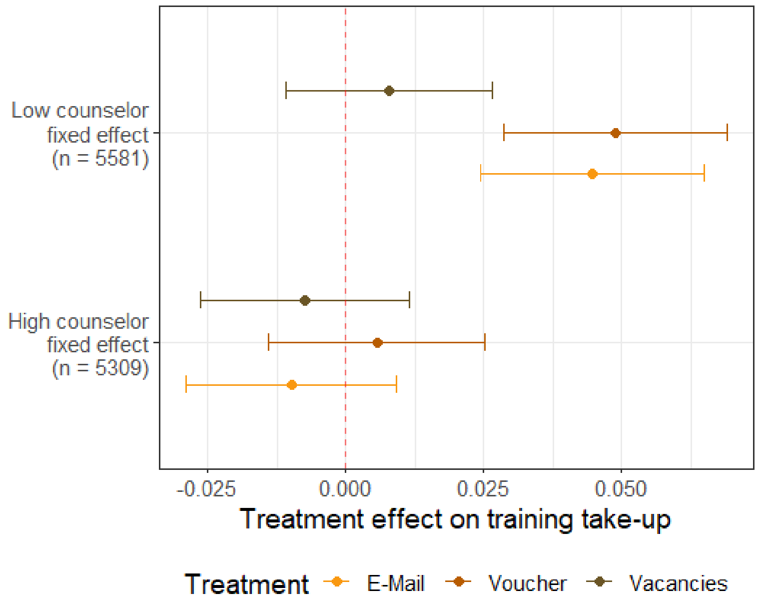
	Dependent variable:				
	Choose own courses	My wishes are considered	Course was turned down	Assigned to course	Could not find suitable course
	(1)	(2)	(3)	(4)	(5)
E-mail	0.068**	−0.054*	0.051	−0.161	0.235
	(0.031)	(0.029)	(0.033)	(0.054)	(0.039)
Voucher	0.069**	−0.068**	0.105***	−0.573	0.442**
	(0.032)	(0.030)	(0.036)	(0.055)	(0.041)
Vacancies	0.091***	−0.052*	0.036	−0.316	0.368*
	(0.032)	(0.030)	(0.034)	(0.059)	(0.041)
Reference Mean	0.362	0.741	0.225	0.465	0.454
Reference SD	0.481	0.439	0.419	0.501	0.499
Caseworker Fixed Effects	1	1	0	0	0
Observations	1,722	1,722	1,145	480	1,145

Note: Standard errors are in parenthesis: *p<0.1; **p<0.05; ***p<0.01.

Heterogeneity by caseworkers To further unravel the role of caseworkers, we analyze the interaction of the treatment effects with caseworkers. We use the fitted values of caseworker fixed effects from a regression on employment duration, as a desired outcome of PES counselling. A longer employment spell after the period of unemployment indicates a good match. We control for all baseline covariates and the treatment group. We then construct a dummy that takes the value 1 if the caseworker fixed effect of the job seeker's caseworker is higher than the median and 0 otherwise. Finally, we re-estimate our main analysis separately for the two subgroups.

The treatment effect on training enrollment is strongly driven by job seekers assigned to caseworkers with lower than median fixed effects on employment duration of their assigned job seekers (Figure 6). The result is robust to alternative variable definitions (training completion, and unemployment duration as the dependent variable in the fixed effects estimation, (Table B18). We conclude that information interventions affect job seekers counselled by caseworkers who achieve shorter re-employment spells of their clients. We further discuss the interpretation of caseworker fixed effects in Section 6.

Figure 6: Average treatment effect on training enrollment by caseworker



Note: Confidence intervals are reported at the 90%-level.

5.3 Unintended consequences

Vacancies While the vacancies information does not increase training enrollment, it increases perceived autonomy and the disconnect between training intentions and behavior. Sub-group analyses show that the vacancies information may have discouraged job seekers from training who are overqualified for the jobs with high labor demand. Our survey provides further suggestive evidence (Table B19). Among low educated survey participants, 55% find the information on job openings helpful and nearly 50% are willing to take a job in one of the included occupations. Among those educated above the minimum, only 35% find the information helpful and only 30% are willing to take a job in the listed occupations.

While the vacancies information does not affect aggregate training enrollment, one may wonder whether it changes the composition of programs in which job seekers enroll to those related to the vacancies. However, our analysis does not support this claim (Table B20).

6 Discussion

In this section, we compare the magnitude of our effects to related studies and discuss potential mechanisms and implications that could be drawn from our findings. We do this for training (Section 6.1) and employment (Section 6.2).

6.1 Training

The findings are remarkable in three aspects: their large magnitude given a one-off information intervention, the insights we provide into the job seeker-caseworker relationship, and unintended consequences caused by the vacancy information.

Magnitude An increase of 18% to 21% from baseline is substantial for a one-off information intervention that consists only of an e-mail. The closest related studies have found null effects of providing and framing information on training enrollment (Dhia and Mbih, 2020; Leduc and Tojerow, 2023) Like ours, both experiments took place as part of broader PES campaigns to promote job training. Our results are in line with information interventions outside the labor market, which have found larger effects of providing information in mailings. This includes a 35-60% increase in filing applications for social benefits (Bhargava and Manoli, 2015), an increase of up to 15% in compliance with municipal housing codes (Linos et al., 2020), an increase of up to 11% in registrations of high school students for state scholarships (Linos et al., 2022b), and an 11% increase in rental assistance program applications (Lasky-Fink and Linos, 2022). In an observational study, Barr and Turner (2018) find that information letters increase college enrollment of job seekers in the U.S. by 40%, particularly among vulnerable job seekers.

Of the various reasons that may explain, why our experiment was the first to be successful in shifting training enrollment of job seekers, differences in the approach of caseworkers seem most convincing. While the design of the e-mail may be more accessible and appealing to job seekers, our e-mail (treatment 1) is similar in design and content to Dhia and Mbih (2020); Leduc and Tojerow (2023). Similarly, contextual factors may have amplified the large effect on training. Indeed, the intervention was implemented during a large-scale expansion of training programs, which may have lowered the bar for enrollment for job seekers. However, the experiments in Dhia and Mbih (2020); Leduc and Tojerow (2023) took place during similar periods of training expansion-a time suitable for PES to collaborate on information campaigns. Therefore, it seems likely that differences in the approach of PES caseworkers could play a role.

The role of caseworkers We open the black box of caseworker relationships with job seekers. Job seekers subject to the intervention report an increase in rejections of their expressed wishes to enroll in job training after an increase in conversations about job training with their caseworkers. Treatment effects are concentrated among job seekers assigned to caseworkers, whose job seekers have shorter employment durations. Such low fixed effects could be interpreted as capturing less productive or more lenient caseworkers who exert less pressure for re-employment on job seekers. Leniency may translate into job training assignment, which is increased for job seekers assigned to caseworkers who are willing to follow job seekers expressions of interest. None of the previous studies collect data on rejection rates of job seekers' training intentions. They do, however, report increases in call-back rates (Dhia and Mbih, 2020) and intentions to enroll in trainings (Leduc and Tojerow, 2023), which did not translate into training enrollment. Activation requirements in France and Belgium are overall not more stringent than in Austria and even more lenient with regard to ALMP participation (OECD, 2023). The dynamic between job seekers and caseworkers could still play a role. Job seekers, for instance, typically express interest in training informally during repeated interactions with their caseworkers. As discussed in Section 2, 87% of caseworkers in Austria report that they assign job seekers to training programs of their choice (Schönherr and Glaser, 2023), which may explain why the intervention in Austria was successful.

Unintended consequences The negative effect of the vacancy information (treatment 3) on training enrollment indicates the importance of targeting information to specific sub-groups, which we test in a follow-up experiment. The purpose of the vacancy information was to provide additional information on the labor market to broaden job seekers' search and training choices towards occupations with high labor demand. As most of the jobs with many open vacancies are in low skill occupations, job seekers with educational attainment above the minimum got discouraged from training, while job seekers with minimum educational attainment responded positively.

Spillovers Encouraging job seekers to engage in training shifts some away from other ALMPs, such as application courses and subsidized employment. The shift may be driven by job seekers' preferences. Our survey documents that job seekers differentiate between different ALMP types in their attitudes. Application courses are more frequently perceived as a disciplining measure while training programs, in particular longer ones, usually involve an active choice of job seekers (cf. Vlandas, 2013). Indeed, our findings are consistent with studies that have found stigma effects to be more severe for application courses and subsidized employment than for job training (Baert, 2016; Van Belle et al., 2019; Kübler et al., 2019; Gatta, 2023). Mandatory assignment not ALMP participation itself causes stigma as it may lead employers to interpret ALMP participation as a sign of negative assessment by a caseworker (Liechti et al., 2017).

Heterogeneity Disadvantaged groups, in particular women and job seekers on lower income, drive the aggregate increase in training. Already at baseline, they participate disproportionally. On the one hand, this reveals a Matthew Effect for job training, where those with better access at the outset benefit disproportionally from expansion in access to educational programs. While the Matthew effect, first established for higher education, typically increases inequalities, women and job seekers on lower income are disadvantaged in the labor market and are found to benefit disproportionally from job training, which should reduce inequalities (Zweimüller and Winter-Ebmer, 1996; Card et al., 2018). This trend might stem from an "access bias" that emerges through particular target groups for trainings (Bonoli and Liechti, 2018). For instance, a subset of training programs are specifically aimed at unemployed women re-entering the workforce post-childbirth. A contextual factor may have contributed as well. Women experienced a sharper increase in unemployment than men during the pandemic (Leitner and Tverdostup, 2023).

6.2 Employment

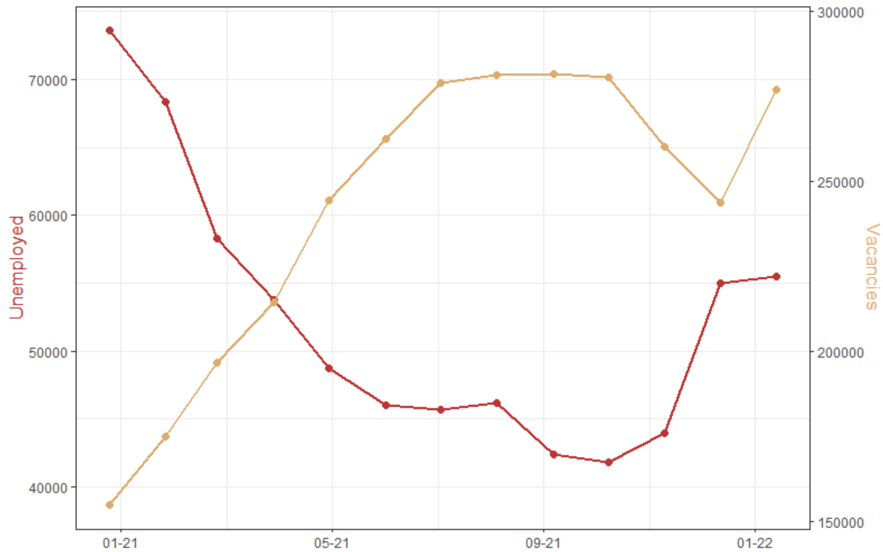
The absence of positive employment effects of training is surprising. We compare our results to related studies and discuss reasons that help understand our findings including possible lock-in effects and the macroeconomic context.

Comparison We do not have directly comparable estimates for employment from similar experiments on training take-up, since those did not shift program enrollment as described in Section 6.1. However, we can compare our employment effects to those in Barr and Turner (2018), given their strong increase of enrollment in post-secondary education. In line with our findings, they do not find any effects on earnings three years after the letter, suggesting that the negative immediate earnings effects of enrolling in post-secondary education instead of active job search (lock-in effects) are offset by the returns to increased education. Experiments that provide information treatments to improve job search have delivered mixed results on employment. Providing access to a website targeted to broaden the set of jobs considered delivers null results (Belot et al., 2019). Providing a brochure with job search advice increases employment by 1-4% (Altmann et al., 2018). Providing access to a website with resume and cover letter templates increases employment by 8% (Briscese et al., 2020) and instructing job seekers on how to use the career network website LinkedIn by 10% (Wheeler et al., 2022). Magnitudes measured in days remain small where reported similar to our results. During the year after the intervention, job seekers who receive the job search brochure, on average, increase their employment for about 1.2 days (Altmann et al., 2018). Observational evaluations of job training tend to find small but positive employment effects, though only in the medium-or long-run (Card et al., 2010, 2018). However, our estimates are not directly comparable since employment effects in our study are driven by the subset of participants responsive to the information intervention. Some have shifted to job training from enrolling in application course, which may lower the employment effect.

Lock-in effects Training program participation can divert job seekers' time and attention temporarily from job search and thereby lengthen unemployment spells. Such lock-in effects of job training programs are widely documented (Lechner and Wunsch, 2009; Lechner et al., 2011). Indeed, we find signs of negative employment effects in the short-run (Figure B5), which are offset after a year. Lock-in effects are found to be smaller during recessions (Lechner and Wunsch, 2009), which corresponds to our case.

Macroeconomic context Job training participants may have missed out on job opportunities during the rapid labor market recovery in spring 2021 prioritizing training over job search. While the timing of the intervention coincided with the Covid pandemic to minimize lock-in effects, a strong labor market recovery followed soon after (Figure 7). The increase in training enrollment was concentrated in spring 2021 (Figure 1), a recovery period, which saw sharply falling unemployment and a doubling number of vacancies. Following the intervention, participants in job training may have missed out on job opportunities during the recovery prioritizing training instead of job search.

Figure 7: Labor market context



Note: Number of unemployed and posted vacancies in Lower Austria in 2021.

Source: AMS DataWarehouse.

To compare interactions with contextual factors, we investigate the effects of training over an entire year after the Covid-induced lockdowns in our follow-up experiment Lehner and Schwarz (2022). The treatment period (2022-2023) covers times of high and low unemployment-to the best of our knowledge the first time in an experimental setting.

7 Conclusion

PES across high income countries struggle to attract unemployed workers to voluntary enroll in job training. Many job seekers are hesitant due to barriers from information frictions and psychological frictions. Our multi-armed field experiments at scale demonstrate the benefits of raising awareness and signaling the monetary value. Raising awareness to reduce information frictions increases program enrollment by 18%. Signaling the monetary value of job training to reduce psychological frictions, for instance from internalized stigma, increases training enrollment by 21% and completion even by 28%. However, providing information on labor demand can discourage job seekers from enrolling in training programs, in particular those who feel overqualified for jobs with open vacancies. Overall, our findings suggest that information interventions can be effective in reducing barriers to training. However, we do not find positive effects of job training on employment or wages.

Outlook Based on the positive effects on training enrollment, the PES has implemented the most effective treatment on a permanent basis. Further evaluations should be carried out in other countries and time periods to investigate the surprising absence of positive effects of training on employment. As part of the permanent implementation, we continue to use random assignment of the most effective intervention (treatment 2, voucher) and targeted information on job vacancies by education (modified version of treatment 3) to investigate the effects of targeted information on training. This follow up field experiment spans an entire year post-pandemic to examine whether job training as varying consequences during times of low and high unemployment, and account for possible distortions due to seasonality and the Covid pandemic.

Implications Our study contributes to the literature on information frictions and psychological frictions as barriers to incomplete take-up of social programs. Disadvantaged people often lack awareness of social programs and experience social stigma related to participation. The results provide evidence on the effectiveness of information interventions in reducing such barriers to increase program take-up. The study also contributes to the active labor market policy evaluation literature. The employment results raise questions about the rationale of encouraging job seekers to participate in job training. The findings strengthen the evidence base to design and implement effective training programs for unemployed workers. Overall, our study shows that information provision can help overcome barriers to program participation but governments should prioritize making social programs effectively work for disadvantaged people.

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Appendix

Table of Contents

A Intervention 1: Design

A.1 Background

Data and ALMP evaluations in Austria Austria's PES has access to high-quality data from longitudinal administrative records. Observational evaluations have found training to increase job seekers' re-employment stability (Zweimüller and Winter-Ebmer, 1996). However, no randomized evaluations of training programs have been carried out.[14]

[14] The few experimental evaluations of ALMPs in Austria have focused on job search assistance (Mühlböck et al., 2022; Böheim et al., 2022) and public employment programs (Kasy and Lehner, 2023).

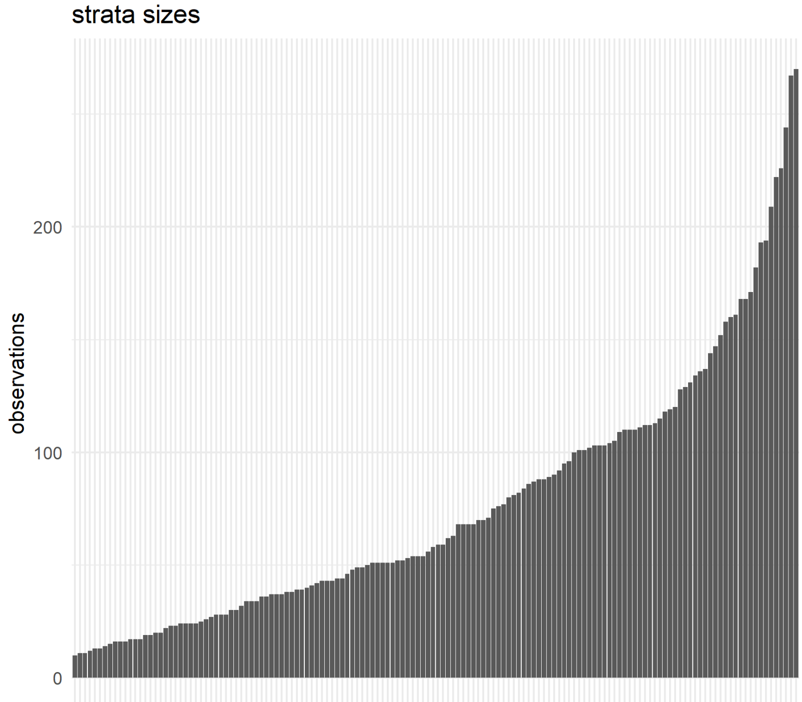
A.2 Sample

A.3 Treatment assignment

Table A1: Balance table

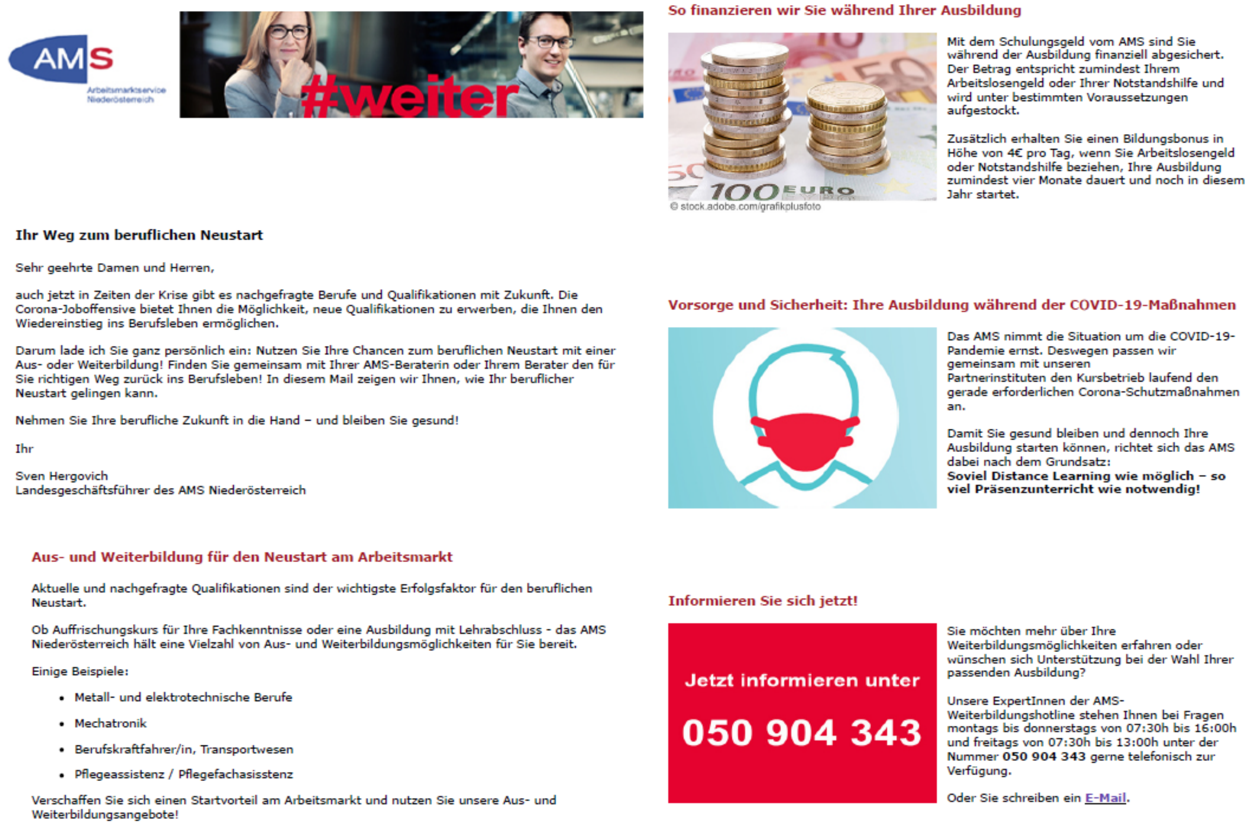
	T1 (N=2769)	T2 (N=2766)	T3 (N=2760)	T4 (N=2755)	Total (N=11050)	p value
Gender						0.999
Women	1437 (51.9%)	1434 (51.8%)	1433 (51.9%)	1434 (52.1%)	5738 (51.9%)	
Men	1332 (48.1%)	1332 (48.2%)	1327 (48.1%)	1321 (47.9%)	5312 (48.1%)	
Age group						1.000
Below 35 years	831 (30.0%)	828 (29.9%)	826 (29.9%)	823 (29.9%)	3308 (29.9%)	
35 - 50 years	1062 (38.4%)	1067 (38.6%)	1064 (38.6%)	1063 (38.6%)	4256 (38.5%)	
Over 50 years	876 (31.6%)	871 (31.5%)	870 (31.5%)	869 (31.5%)	3486 (31.5%)	
Education						1.000
Missing	10	9	8	9	36	
Primary	897 (32.5%)	898 (32.6%)	896 (32.6%)	891 (32.4%)	3582 (32.5%)	
Higher than primary	1862 (67.5%)	1859 (67.4%)	1856 (67.4%)	1855 (67.6%)	7432 (67.5%)	
Region						1.000
Industrieviertel	1222 (44.1%)	1225 (44.3%)	1227 (44.5%)	1219 (44.2%)	4893 (44.3%)	
Mostviertel	741 (26.8%)	731 (26.4%)	732 (26.5%)	732 (26.6%)	2936 (26.6%)	
Waldviertel	243 (8.8%)	245 (8.9%)	239 (8.7%)	241 (8.7%)	968 (8.8%)	
Weinviertel	563 (20.3%)	565 (20.4%)	562 (20.4%)	563 (20.4%)	2253 (20.4%)	
Unemp. dur.						1.000
3 - 4 Months	676 (24.4%)	675 (24.4%)	671 (24.3%)	668 (24.2%)	2690 (24.3%)	
6 - 9 Months	937 (33.8%)	937 (33.9%)	937 (33.9%)	934 (33.9%)	3745 (33.9%)	
9 - 12 Months	1156 (41.7%)	1154 (41.7%)	1152 (41.7%)	1153 (41.9%)	4615 (41.8%)	
Nationality						0.778
Missing	1	2	3	1	7	
Austria	2147 (77.6%)	2146 (77.6%)	2150 (78.0%)	2165 (78.6%)	8608 (77.9%)	
Other	621 (22.4%)	618 (22.4%)	607 (22.0%)	589 (21.4%)	2435 (22.1%)	
Health						0.991
No health restriction	2185 (78.9%)	2177 (78.7%)	2168 (78.6%)	2169 (78.7%)	8699 (78.7%)	
Health restriction	584 (21.1%)	589 (21.3%)	592 (21.4%)	586 (21.3%)	2351 (21.3%)	
Marg. empl.						0.733
No	2457 (88.7%)	2479 (89.6%)	2467 (89.4%)	2463 (89.4%)	9866 (89.3%)	
Yes	312 (11.3%)	287 (10.4%)	293 (10.6%)	292 (10.6%)	1184 (10.7%)	
German						0.456
Partial or non	404 (14.6%)	403 (14.6%)	377 (13.7%)	418 (15.2%)	1602 (14.5%)	
Proficient or native	2365 (85.4%)	2363 (85.4%)	2383 (86.3%)	2337 (84.8%)	9448 (85.5%)	

Figure A1: Strata size



A.4 Treatment (Intervention 1)

Figure A2: E-mail for treatment groups 1, 2 and 3



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#weiter

Ihr Weg zum beruflichen Neustart

Sehr geehrte Damen und Herren,

auch jetzt in Zeiten der Krise gibt es nachgefragte Berufe und Qualifikationen mit Zukunft. Die Corona-Joboffensive bietet Ihnen die Möglichkeit, neue Qualifikationen zu erwerben, die Ihnen den Wiedereinstieg ins Berufsleben ermöglichen.

Darum lade ich Sie ganz persönlich ein: Nutzen Sie Ihre Chancen zum beruflichen Neustart mit einer Aus- oder Weiterbildung! Finden Sie gemeinsam mit Ihrer AMS-Beraterin oder Ihrem Berater den für Sie richtigen Weg zurück ins Berufsleben! In diesem Mail zeigen wir Ihnen, wie Ihr beruflicher Neustart gelingen kann.

Nehmen Sie Ihre berufliche Zukunft in die Hand – und bleiben Sie gesund!

Ihr

Sven Hergovich
Landesgeschäftsführer des AMS Niederösterreich

Aus- und Weiterbildung für den Neustart am Arbeitsmarkt

Aktuelle und nachgefragte Qualifikationen sind der wichtigste Erfolgsfaktor für den beruflichen Neustart.

Ob Auffrischungskurs für Ihre Fachkenntnisse oder eine Ausbildung mit Lehrabschluss - das AMS Niederösterreich hält eine Vielzahl von Aus- und Weiterbildungsmöglichkeiten für Sie bereit.

Einige Beispiele:

- Metall- und elektrotechnische Berufe
- Mechatronik
- Berufskraftfahrer/in, Transportwesen
- Pflegeassistenz / Pflegefachasisstenz

Verschaffen Sie sich einen Startvorteil am Arbeitsmarkt und nutzen Sie unsere Aus- und Weiterbildungsangebote!

So finanzieren wir Sie während Ihrer Ausbildung

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Mit dem Schulungsgeld vom AMS sind Sie während der Ausbildung finanziell abgesichert. Der Betrag entspricht zumindest Ihrem Arbeitslosengeld oder Ihrer Notstandshilfe und wird unter bestimmten Voraussetzungen aufgestockt.

Zusätzlich erhalten Sie einen Bildungsbonus in Höhe von 4€ pro Tag, wenn Sie Arbeitslosengeld oder Notstandshilfe beziehen, Ihre Ausbildung zumindest vier Monate dauert und noch in diesem Jahr startet.

Vorsorge und Sicherheit: Ihre Ausbildung während der COVID-19-Maßnahmen

Das AMS nimmt die Situation um die COVID-19-Pandemie ernst. Deswegen passen wir gemeinsam mit unseren Partnerinstituten den Kursbetrieb laufend den gerade erforderlichen Corona-Schutzmaßnahmen an.

Damit Sie gesund bleiben und dennoch Ihre Ausbildung starten können, richtet sich das AMS dabei nach dem Grundsatz:
Soviel Distance Learning wie möglich – so viel Präsenzunterricht wie notwendig!

Informieren Sie sich jetzt!

Jetzt informieren unter
050 904 343

Sie möchten mehr über Ihre Weiterbildungsmöglichkeiten erfahren oder wünschen sich Unterstützung bei der Wahl Ihrer passenden Ausbildung?

Unsere ExpertInnen der AMS-Weiterbildungshotline stehen Ihnen bei Fragen montags bis donnerstags von 07:30h bis 16:00h und freitags von 07:30h bis 13:00h unter der Nummer **050 904 343** gerne telefonisch zur Verfügung.

Oder Sie schreiben ein E-Mail.

Figure A3: Voucher for treatment groups 2 and 3

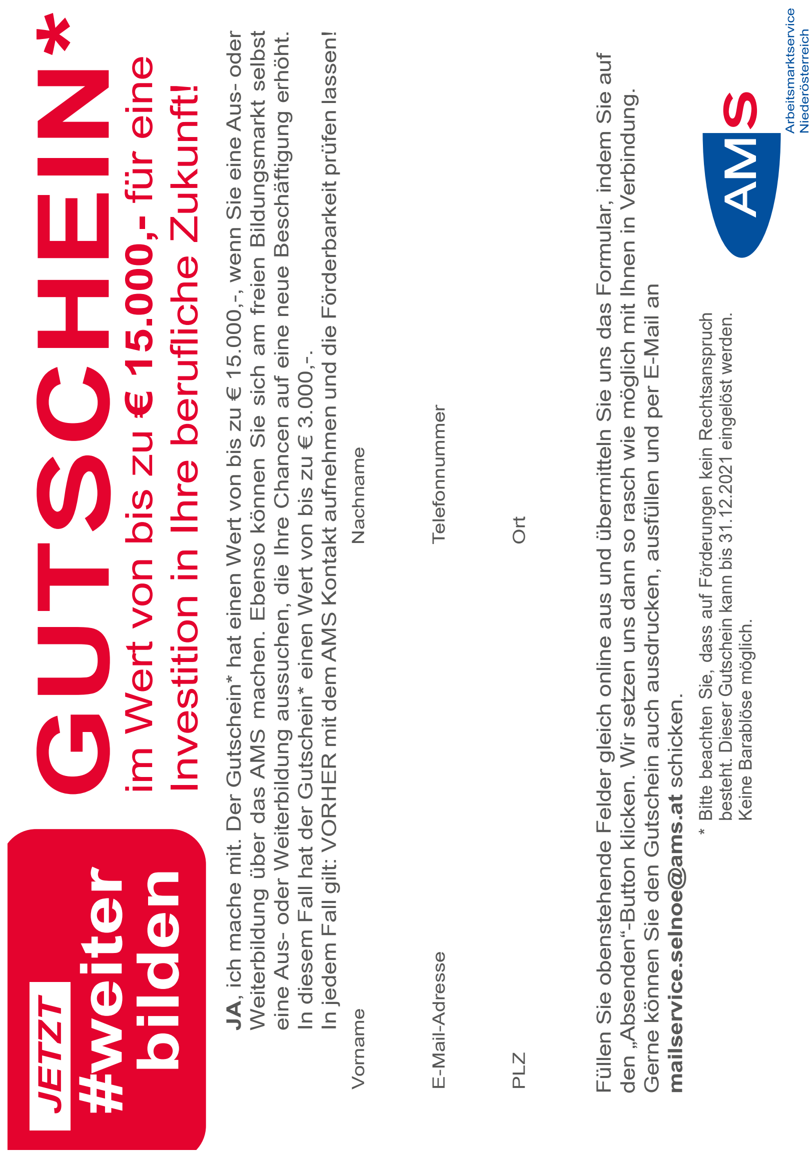


Figure A4: Occupations with the highest number of open vacancies for treatment group 3

Die aktuellen Top Jobs am niederösterreichischen Arbeitsmarkt

- **Elektroinstallateur(e)innen, -monteur(e)innen**
  beim AMS NÖ gemeldete offene Stellen im Jänner: **343**
- **Dipl. Krankenpfleger, -schwestern**
  beim AMS NÖ gemeldete offene Stellen im Jänner: **229**
- **Kraftfahrer/innen (alle Bereiche)**
  beim AMS NÖ gemeldete offene Stellen im Jänner: **228**
- **Maurer/innen**
  beim AMS NÖ gemeldete offene Stellen im Jänner: **170**
- **Techniker/innen für Datenverarbeitung**
  beim AMS NÖ gemeldete offene Stellen im Jänner: **159**
- **Rohrinstallateur(e)innen, -monteur(e)innen**
  beim AMS NÖ gemeldete offene Stellen im Jänner: **157**
- **Hotel- und Gaststättenberufe**
  beim AMS NÖ gemeldete offene Stellen im Jänner: **132**
- **Techniker/innen für Maschinenbau**
  beim AMS NÖ gemeldete offene Stellen im Jänner: **117**
- **Pflegeassistent/in**
  beim AMS NÖ gemeldete offene Stellen im Jänner: **110**
- **Medizinisch-technische Fachkräfte (m./w.)**
  beim AMS NÖ gemeldete offene Stellen im Jänner: **81**

A.5 Tracking e-mail responses

Figure A5: Measurement of e-mail openings and clicks

B Intervention 1: Results

B.1 Training

Table B1: Training completion

	Completion			
	Long training	Examined training	Application courses	External courses
	(1)	(2)	(3)	(4)
E-Mail	0.018**	0.010**	−0.009*	−0.002
	(0.008)	(0.005)	(0.005)	(0.004)
Voucher	0.026***	0.009*	−0.006	0.005
	(0.008)	(0.005)	(0.005)	(0.005)
Vacancies	0.006	0.004	−0.003	0.0001
	(0.008)	(0.005)	(0.005)	(0.005)
Control Mean	0.094	0.033	0.042	0.029
Control SD	0.292	0.177	0.2	0.169
Observations	10,714	10,714	10,714	10,714

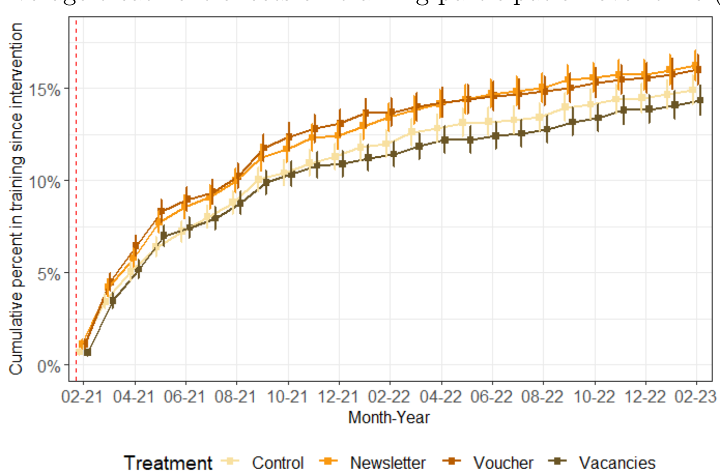
Note: Standard errors are in parenthesis: *p<0.1; **p<0.05; ***p<0.01

Table B2: Long term average treatment effects on active labor market programs

	Dependent variable:					
	Training	Training completion	Long training	Examined training	Application courses	Subsidized employment
	(1)	(2)	(3)	(4)	(5)	(6)
E-mail	0.015*	0.011	0.017**	0.010	−0.013**	0.003
	(0.009)	(0.009)	(0.008)	(0.006)	(0.006)	(0.012)
Voucher	0.013	0.016*	0.013*	0.004	−0.007	−0.012
	(0.009)	(0.009)	(0.008)	(0.006)	(0.006)	(0.012)
Vacancies	−0.005	−0.004	−0.003	0.005	−0.006	−0.018
	(0.009)	(0.009)	(0.008)	(0.006)	(0.006)	(0.012)
Control Mean	0.149	0.13	0.1	0.061	0.062	0.319
Control SD	0.356	0.336	0.301	0.24	0.241	0.466
Observations	10,714	10,714	10,714	10,714	10,714	10,714

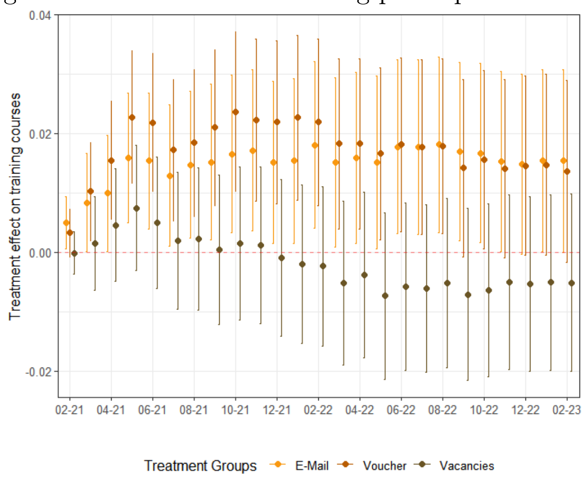
Note: Long term refers to 2 years after the intervention. Standard errors are in parenthesis: *p<0.1; **p<0.05; ***p<0.01

Figure B1: Average treatment effects on training participation over time (cumulative)



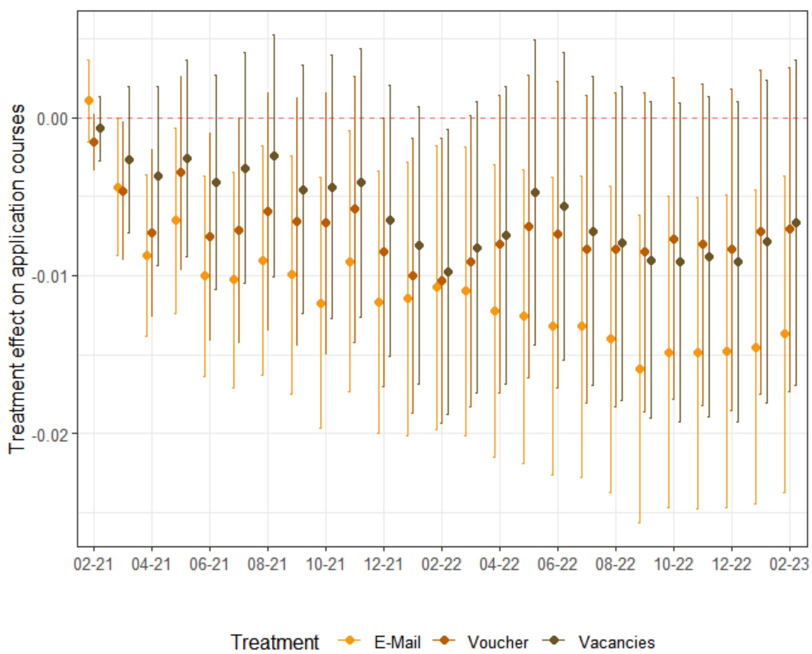
Note: Confidence intervals are reported at the 90%-level.

Figure B2: Average treatment effects on training participation over time (long-term)



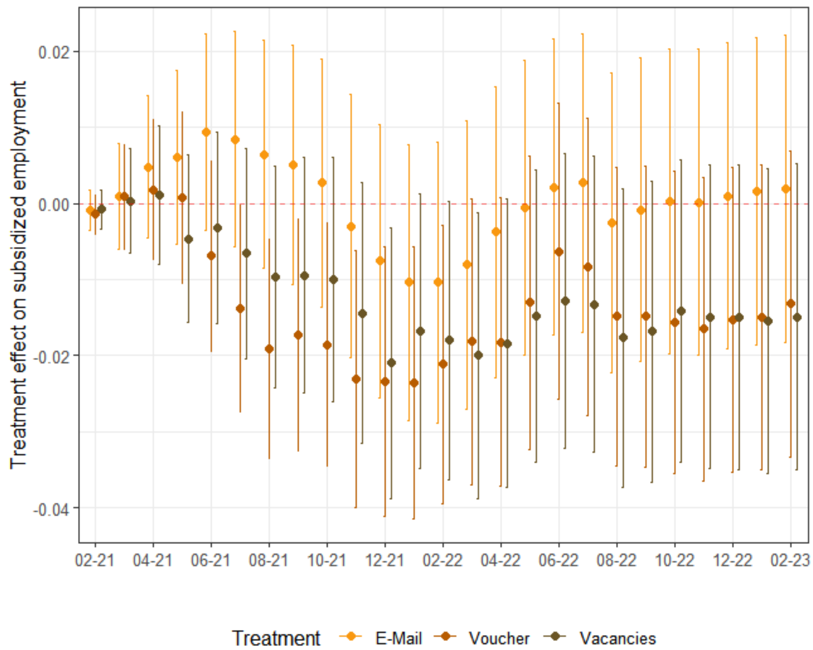
Note: Confidence intervals are reported at the 90%-level.

Figure B3: Average treatment effects on application course enrollment over time



Note: Confidence intervals are reported at the 90%-level.

Figure B4: Average treatment effects on subsidized employment over time



Note: Confidence intervals are reported at the 90%-level.

B.2 Heterogeneity in training

B.2.1 Training enrollment

Table B3: Heterogeneity in training enrollment by age and education

	Dependent variable:					
				Training Take-up		
	below 35 years	35 to 50 years	Above 50 years	Up to secondary education	Vocational education	More than secondary education
	(1)	(2)	(3)	(4)	(5)	(6)
E-mail	0.003	0.028*	0.021*	0.015	0.026**	0.021**
	(0.017)	(0.015)	(0.011)	(0.014)	(0.013)	(0.013)
Voucher	0.013	0.036**	0.021*	0.027*	0.030**	0.018**
	(0.017)	(0.015)	(0.011)	(0.014)	(0.013)	(0.013)
Vacancies	0.005	−0.013	0.010	0.012	−0.004	−0.001
	(0.017)	(0.014)	(0.011)	(0.014)	(0.012)	(0.012)
Control Group Mean	0.132	0.05	0.153	0.108	0.137	0.086
Control Group SD	0.338	0.219	0.36	0.311	0.344	0.28
Observations	3,169	4,116	3,429	4,350	3,995	2,369

Note: Standard errors are in parenthesis: *p<0.1; **p<0.05; ***p<0.01

Table B4: Heterogeneity in training enrollment by nationality and language

	Dependent variable:			
	Training Take-up			
	Non-Austrian	Austrian	Non-German speaking	German speaking
	(1)	(2)	(3)	(4)
E-mail	0.031	0.018**	0.058**	0.016*
	(0.022)	(0.009)	(0.029)	(0.008)
Voucher	0.004	0.025***	0.036	0.022***
	(0.022)	(0.009)	(0.030)	(0.008)
Vacancies	0.003	−0.0003	0.029	−0.001
	(0.021)	(0.008)	(0.028)	(0.008)
Control Group Mean	0.196	0.088	0.243	0.09
Control Group SD	0.398	0.283	0.429	0.286
Observations	2,270	8,444	1,460	9,254

Note: Standard errors are in parenthesis: *p<0.1; **p<0.05; ***p<0.01

Table B5: Heterogeneity in training enrollment by occupation

	Dependent variable:				
	Training Take-up				
	Blue-collar occupation	White-collar occupation	Low-skilled occupation	Medium-skilled occupation	High-skilled occupation
	(1)	(2)	(3)	(4)	(5)
E-mail	0.018	0.020**	0.034	0.035***	−0.016
	(0.015)	(0.010)	(0.021)	(0.011)	(0.014)
Voucher	0.010	0.027***	0.005	0.044***	−0.004
	(0.014)	(0.010)	(0.021)	(0.011)	(0.015)
Vacancies	0.024*	−0.012	0.033	0.012	−0.031**
	(0.014)	(0.010)	(0.020)	(0.011)	(0.014)
Control Group Mean	0.121	0.103	0.101	0.155	0.097
Control Group SD	0.326	0.304	0.301	0.362	0.295
Observations	3,775	6,939	2,132	5,694	2,888

Note: Standard errors are in parenthesis: *p<0.1; **p<0.05; ***p<0.01

B.2.2 Training completion

Table B6: Heterogeneity in training completion by gender and income

	Dependent variable:			
	Training Completion			
	Women	Men	Below median income	Above median income
	(1)	(2)	(3)	(4)
E-Mail	0.030**	0.001	0.035***	−0.001
	(0.012)	(0.009)	(0.011)	(0.011)
Voucher	0.040***	0.012	0.042***	0.009
	(0.013)	(0.009)	(0.012)	(0.011)
Vacancies	0.012	−0.0002	0.026**	−0.014
	(0.012)	(0.009)	(0.011)	(0.010)
Control Group Mean	0.123	0.063	0.094	0.084
Control Group SD	0.329	0.243	0.292	0.278
Observations	5,523	5,191	5,363	5,351
Note:	Standard errors are in parenthesis: *p<0.1; **p<0.05; ***p<0.01			

Table B7: Heterogeneity in training completion by age and education

	Dependent variable:					
	Training Completion					
	Below 35 years	35 to 50 years	Above 50 years	Up to secondary education	Vocational education	More than secondary education
	(1)	(2)	(3)	(4)	(5)	(6)
E-mail	0.014	0.017	0.019*	0.015	0.019	0.026
	(0.016)	(0.013)	(0.011)	(0.013)	(0.012)	(0.012)
Voucher	0.017	0.039***	0.017	0.030**	0.026**	0.027**
	(0.016)	(0.014)	(0.011)	(0.013)	(0.012)	(0.012)
Vacancies	0.010	−0.005	0.011	0.013	−0.001	0.017
	(0.015)	(0.013)	(0.010)	(0.013)	(0.011)	(0.011)
Control Group Mean	0.113	0.045	0.123	0.09	0.116	0.071
Control Group SD	0.317	0.206	0.328	0.287	0.32	0.257
Observations	3,169	4,116	3,429	4,350	3,995	2,369
Note:	Standard errors are in parenthesis: *p<0.1; **p<0.05; ***p<0.01					

Table B8: Heterogeneity in training completion by nationality and language

	Dependent variable:			
	Training Completion			
	Non-Austrian	Austrian	Non-German speaking	German speaking
	(1)	(2)	(3)	(4)
E-mail	0.058	0.013*	0.032**	0.016*
	(0.021)	(0.008)	(0.028)	(0.008)
Voucher	0.024	0.026***	0.010	0.027***
	(0.020)	(0.008)	(0.028)	(0.008)
Vacancies	0.041	0.003	0.022	0.002
	(0.020)	(0.008)	(0.027)	(0.007)
Control Group Mean	0.169	0.073	0.213	0.074
Control Group SD	0.375	0.26	0.41	0.262
Observations	2,270	8,444	1,460	9,254
Note:	Standard errors are in parenthesis: *p<0.1; **p<0.05; ***p<0.01			

Table B9: Heterogeneity in training completion by occupation

	Dependent variable:				
	Training Completion				
	Blue-collar occupation	White-collar occupation	Low-skilled occupation	Medium-skilled occupation	High-skilled occupation
	(1)	(2)	(3)	(4)	(5)
E-mail	0.020	0.017*	0.032*	0.031***	−0.016
	(0.013)	(0.010)	(0.019)	(0.011)	(0.014)
Voucher	0.017	0.028***	0.009	0.043***	−0.004
	(0.013)	(0.010)	(0.019)	(0.011)	(0.014)
Vacancies	0.035***	−0.008	0.045**	0.013	−0.027**
	(0.013)	(0.009)	(0.019)	(0.010)	(0.013)
Control Group Mean	0.099	0.088	0.09	0.125	0.08
Control Group SD	0.299	0.283	0.286	0.332	0.272
Observations	3,775	6,939	2,132	5,694	2,888
Note:	Standard errors are in parenthesis: *p<0.1; **p<0.05; ***p<0.01				

B.3 Employment

Table B10: Employment effects short-term (1 year)

	Dependent variable:					
	Any employment		Days in employment	Days in unemployment	Avg. daily wage	Jobquality
	(1)	(2)	(3)	(4)	(5)	(6)
E-mail + Voucher	−0.007		−3.049	−0.317	−0.017	−0.004
	(0.011)		(2.576)	(2.738)	(0.860)	(0.005)
Training		−0.083				
		(0.436)				
Control Group Mean	0.548	0.548	94.625	211.497	48.76	0.348
Control Group SD	0.498	0.498	116.412	119.17	30.172	0.155
Observations	10,714	10,714	10,714	10,714	6,441	5,403

Note: Standard errors are in parenthesis: *p<0.1; **p<0.05; ***p<0.01

Table B11: Income with alternative definitions

	Dependent variable:			
	Daily wage in first job	Cumulative earnings	Higher than median avg. daily wage	Higher than median jobquality
	(1)	(2)	(3)	(4)
E-mail + Voucher	0.405	−553.181	0.002	−0.013
	(0.937)	(531.232)	(0.013)	(0.011)
Control Group Mean	55.91	21729.99	0.447	0.348
Control Group SD	34.933	23900.172	0.497	0.476
Observations	7,544	10,714	7,723	10,714

Note: Standard errors are in parenthesis: *p<0.1; **p<0.05; ***p<0.01

Table B12: Employment outcomes with instrumental variable approach

	Dependent variable:			
	Days in employment IV reg	Days in unemployment IV reg	Avg. daily wage IV reg	Jobquality IV reg
	(1)	(2)	(3)	(4)
Training	−243.832	−245.424	−15.672	−0.010
	(344.317)	(335.887)	(42.283)	
Control Group Mean	350.103	361.954	50.814	0.382
Control Group SD	310.971	259.588	29.494	0.144
Observations	10,714	10,714	7,723	7,323

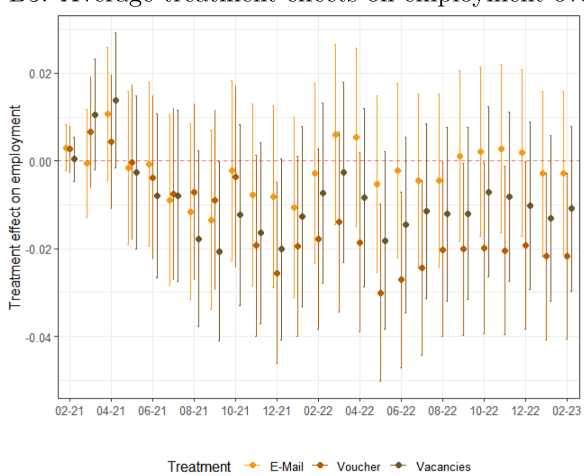
Note: *p<0.1; **p<0.05; ***p<0.01

Table B13: Employment outcomes with separate treatment groups

	Dependent variable:				
	Any employment	Days in employment	Days in unemployment	Avg. daily wage	Jobquality
	(1)	(2)	(3)	(4)	(5)
E-mail	−0.001	−1.937	−3.405	−0.456	−0.0003
	(0.011)	(7.845)	(6.755)	(0.882)	(0.004)
Voucher	−0.014	−10.381	−3.122	0.288	0.001
	(0.011)	(7.905)	(6.796)	(0.903)	(0.005)
Vacancies	−0.004	−4.266	1.590	0.324	0.001
	(0.011)	(7.904)	(6.787)	(0.900)	(0.005)
Control Group Mean	0.754	350.103	361.954	50.814	0.382
Control Group SD	0.431	310.971	259.588	29.494	0.144
Observations	10,714	10,714	10,714	7,723	7,323

Note: Standard errors are in parenthesis: *p<0.1; **p<0.05; ***p<0.01

Figure B5: Average treatment effects on employment over time



Note: Confidence intervals are reported at the 90%-level.

B.4 Heterogeneity in employment

Table B14: Heterogeneity in employment by gender and income

	Dependent variable:			
	Days in employment			
	Women	Men	Below median income	Above median income
	(1)	(2)	(3)	(4)
E-Mail + Voucher	−9.725	−3.420	−4.168	−5.819
	(9.641)	(9.770)	(9.580)	(10.078)
Control Group Mean	87.566	102.242	90.122	100.42
Control Group SD	115.361	117.101	113.099	120.086
Observations	5,523	5,191	5,363	5,351
Note:	Standard errors are in parenthesis: *p<0.1; **p<0.05; ***p<0.01			

Table B15: Heterogeneity in employment by age and education

	Dependent variable:					
	Days in employment					
	Below 35 years	35 to 50 years	Above 50 years	Up to secondary education	Vocational education	More than secondary education
	(1)	(2)	(3)	(4)	(5)	(6)
E-Mail + Voucher	10.039	−14.986	4.714	−1.551	−4.470	−10.781
	(12.669)	(11.367)	(11.634)	(10.761)	(11.270)	(15.464)
Control Group Mean	115.4	110.696	55.436	91.493	97.798	95.066
Control Group SD	123.211	120.787	92.293	113.22	118.382	118.629
Observations	3,169	4,116	3,429	4,350	3,995	2,369
Note:	Standard errors are in parenthesis: *p<0.1; **p<0.05; ***p<0.01					

Table B16: Heterogeneity in employment by nationality and language

	Dependent variable:			
	Days in employment			
	Non-Austrian	Austrian	Non-German speaking	German speaking
	(1)	(2)	(3)	(4)
E-mail + Voucher	0.778	−9.885	33.326*	−12.323*
	(15.796)	(7.669)	(20.235)	(7.329)
Control Group Mean	103.229	92.144	99.391	93.811
Control Group SD	114.68	116.843	111.774	117.189
Observations	2,270	8,444	1,460	9,254
Note:	Standard errors are in parenthesis: *p<0.1; **p<0.05; ***p<0.01			

Table B17: Heterogeneity in employment by occupation

	Dependent variable:				
	Days in employment				
	Blue-collar occupation	White-collar occupation	Low-skilled occupation	Medium-skilled occupation	High-skilled occupation
	(1)	(2)	(3)	(4)	(5)
E-mail + Voucher	−7.006	−6.333	0.290	−18.151*	6.763
	(11.703)	(8.658)	(16.135)	(9.403)	(14.533)
Control Group Mean	94.353	94.935	97.555	90.293	95.004
Control Group SD	114.945	117.265	119.044	112.342	116.676
Observations	3,775	6,939	2,132	5,694	2,888

Note: Standard errors are in parenthesis: *p<0.1; **p<0.05; ***p<0.01

B.5 Mechanisms

Table B18: Average treatment effect on training by caseworker

	Dependent variable:					
	Training enrollment		Training completion		Training enrollment	
	Low caseworker	High caseworker	Low caseworker	High caseworker	Low caseworker	High caseworker
	(1)	(2)	(3)	(4)	(5)	(6)
E-Mail	0.045^{***}	-0.010	0.019^{*}	0.003	0.029^{***}	0.003
	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)	(0.013)
Voucher	0.049^{***}	0.006	0.035^{***}	0.010	0.036^{***}	0.011
	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)	(0.013)
Vacancies	0.008	-0.007	0.002	0.002	0.007	-0.008
	(0.011)	(0.012)	(0.011)	(0.011)	(0.011)	(0.012)
Fixed effect outcome	empl. duration	empl. duration	empl. duration	empl. duration	unempl. duration	unempl. duration
Control Group Mean	0.113	0.112	0.097	0.092	0.098	0.129
Control Group SD	0.316	0.316	0.296	0.289	0.297	0.335
Observations	5,385	5,176	5,646	5,059	5,489	5,216

Note: Standard errors are in parenthesis: *p<0.1; **p<0.05; ***p<0.01

Table B19: Evaluation of vacancy information by socio-economic characteristics

	Percent at least rather agreeing	
	Information is important for me	Would consider working in one of these jobs
Occupation		
blue-collar | A	45.21%	38.26%
white-collar | B	38.58%	31.46%
Occupation skill-level		
low-skilled | A	48.00% C	41.33% C
medium skilled | B	42.93%	34.03%
high-skilled | C	31.90%	27.59%
Education		
up to secondary education | A	46.51% C	39.54% C
vocational education | B	43.26% C	36.17% C
more than secondary education | C	30.97%	23.89%
Age group		
below 35 years | A	51.14% B	44.32%
35-50 years | B	35.40%	32.30%
above 50 years | C	40.74%	28.89%
Gender		
Women | A	44.02%	31.20%
Men | B	36.00%	38.00%
Pre-unemployment income		
below median income|A	45.30%	37.02%
above median income | B	36.00%	30.00%

Table B20: Treatment effects on specific courses related to vacancy information

Table B21:

	Dependent variable:			
	Training	Training completion	Training	Training completion
	(1)	(2)	(3)	(4)
E-mail	0.004	0.002	0.004	0.002
	(0.003)	(0.003)	(0.003)	(0.003)
Voucher	0.005	0.005	0.003	0.003
	(0.003)	(0.003)	(0.003)	(0.003)
Vacancies	−0.001	0.0001	−0.001	−0.0005
	(0.003)	(0.003)	(0.003)	(0.002)
Control Mean	0.014	0.01	0.013	0.009
Control SD	0.119	0.1	0.115	0.095
Observations	10,714	10,714	10,714	10,714

Note: Standard errors are in parenthesis: *p<0.1; **p<0.05; ***p<0.01

C Survey

C.1 Survey questionnaire

Figure D1: Survey questionnaire: intro

English (United Kingdom)

Intro

Let us know what you think about AMS courses!

Welcome to this short survey on AMS courses at the Vienna University of Economics and Business on behalf of AMS Niederösterreich. In order to be able to tailor the course offer to your interests, please fill out our short survey. Your opinion counts!

The survey only takes 3 minutes. All answers remain completely anonymous. The answers are evaluated by the Vienna University of Economics and Business on behalf of the AMS Niederösterreich and are incorporated into a research project to improve the AMS offer.

Would you like to participate in the survey?

○ Yes, I have been informed of the purpose of the survey and would like to take part.

If you have any questions or comments about the survey or the research project, you can contact me at any time: anna-magdalena.schwarz@wu.ac.at

Figure D2: Survey questionnaire: reminder of treatment

About two months ago you received the following newsletter from the AMS on further training: (please scroll down)

AMS
Arbeitsmarktservice
Niederösterreich

Ihr Weiterbildungsgutschein im Wert von bis zu 15.000,- Euro

Sehr geehrte Damen und Herren,

Nutzen Sie die Chance zum beruflichen Neustart mit einer Qualifizierung im Rahmen der Corona-Joboffensive! Bis zu 15.000,- Euro sind beim AMS Niederösterreich für Ihre zukunftssichere Aus- und Weiterbildung für Sie reserviert.

Finden Sie gemeinsam mit Ihrer AMS-Beraterin oder Ihrem Berater den für Sie richtigen Weg zurück ins Berufsleben und lösen Sie Ihren Weiterbildungsgutschein ein! In diesem Mail zeigen wir Ihnen, wie Ihr beruflicher Neustart gelingen kann.

Nehmen Sie Ihre berufliche Zukunft in die Hand – und bleiben Sie gesund!

Ihr

Sven Hergovich
Landesgeschäftsführer des AMS Niederösterreich

Figure D3: Survey questionnaire: treatment mechanisms

Do you remember that?

Yes	No

Did the newsletter motivate you to take an AMS course?

Yes very	Yes, rather	Neither nor	No, rather not	no not at all

Would you take advantage of this offer?

Yes, in any case!	Yes, more likely	Neither nor	No, not really	No, definitely not!

Would you rather attend an AMS course or a course on the independent education market?

AMS course	More like AMS course	Both	Rather course on the free education market	Course on the free education market

How did you find the newsletter?

Figure D4: Survey questionnaire: course participation

How decisive were the following factors for you in your decision not to attend a course?

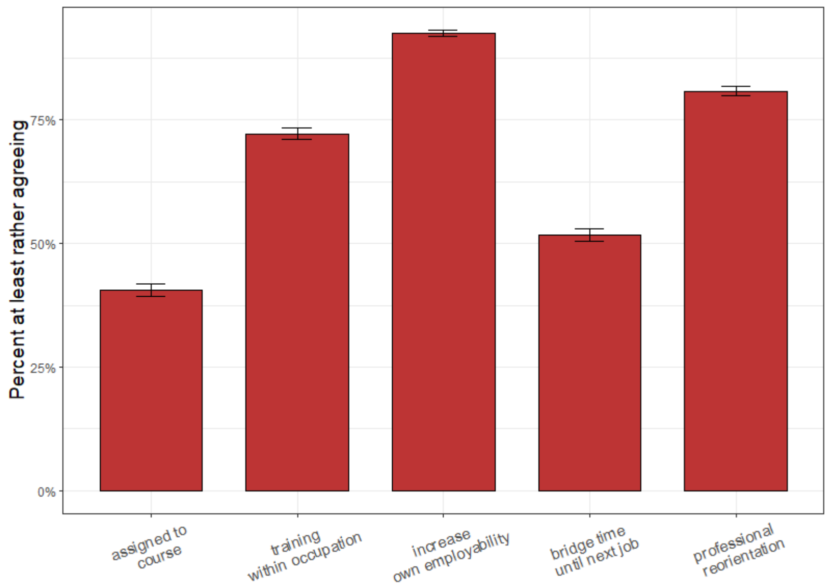
	very important	rather important	Neither nor	not that important	not important at all
The AMS refused my preferred course	○	○	○	○	○
I am too old to do advanced training.	○	○	○	○	○
I haven't found a suitable course for me	○	○	○	○	○

	very important	rather important	Neither nor	not that important	not important at all
I don't have enough information about the AMS courses	○	○	○	○	○
I cannot afford to attend a course for financial reasons	○	○	○	○	○
I am prevented by other obligations (e.g. childcare or caring for relatives)	○	○	○	○	○

C.2 Additional survey results

Motivation to train Why do job seekers enroll in training? Desires such as increasing one's employability drive most job seekers enrollment while external constraints such as being assigned to a course drive a sizeable minority. 9 out of 10 job seekers enroll in training to increase their employability (Figure D5). 80% consider professional re-orientation as a motive while for 70% training within their occupation is important. About half of job seekers simply intend to bridge the time until their next job. Assignment by the caseworker as an external factor matters for around 40% of job seekers.

Figure D5: Motivation for training enrollment



Note: Confidence intervals are reported at the 90%-level.

Training course assignment suffers from perverse incentives.

- *"No one cares if a course fits your profile or not. The main focus is that you are no longer adding to the unemployed numbers."*
- *"All pointless mass processing so that some unemployed fall out of the statistics."*
- *"One should be listened to and not just thrown into a course to make the labor market statistics look better."*

Job seekers demand more autonomy.

- *"It would be nice if people's wishes and needs were taken into account."*
- *"Be more responsive to the needs of the unemployed to provide relevant training."*

- *"The PES should provide us with a targeted offer of courses with self-selection under a certain budget, so that we can make our own choices."*

D Intervention 2

This section reports the design and results of intervention 2.

D.1 Design (Intervention 2)

Table D1: Balance table intervention 2

	T1 (N=3440)	T2 (N=3438)	T3 (N=3441)	T4 (N=3445)	Total (N=13764)	p value
Gender						0.999
Women	1656 (48.1%)	1649 (48.0%)	1656 (48.1%)	1655 (48.0%)	6616 (48.1%)	
Men	1784 (51.9%)	1789 (52.0%)	1785 (51.9%)	1790 (52.0%)	7148 (51.9%)	
Age group						1.000
Below 35 years	606 (17.6%)	618 (18.0%)	615 (17.9%)	616 (17.9%)	2455 (17.8%)	
35 - 50 years	1169 (34.0%)	1159 (33.7%)	1163 (33.8%)	1166 (33.8%)	4657 (33.8%)	
Over 50 years	1665 (48.4%)	1661 (48.3%)	1663 (48.3%)	1663 (48.3%)	6652 (48.3%)	
Education						1.000
Missing	10	8	10	12	40	
Primary	1749 (51.0%)	1753 (51.1%)	1752 (51.1%)	1752 (51.0%)	7006 (51.0%)	
Higher than primary	1681 (49.0%)	1677 (48.9%)	1679 (48.9%)	1681 (49.0%)	6718 (49.0%)	
Region						1.000
Industrieviertel	1481 (43.1%)	1481 (43.1%)	1483 (43.1%)	1489 (43.2%)	5934 (43.1%)	
Mostviertel	904 (26.3%)	908 (26.4%)	903 (26.2%)	901 (26.2%)	3616 (26.3%)	
Waldviertel	333 (9.7%)	332 (9.7%)	332 (9.6%)	334 (9.7%)	1331 (9.7%)	
Weinviertel	722 (21.0%)	717 (20.9%)	723 (21.0%)	721 (20.9%)	2883 (20.9%)	
Unemp. dur.						0.934
3 - 4 Months	477 (13.9%)	468 (13.6%)	479 (13.9%)	472 (13.7%)	1896 (13.8%)	
6 - 9 Months	278 (8.1%)	264 (7.7%)	272 (7.9%)	253 (7.3%)	1067 (7.8%)	
9 - 12 Months	299 (8.7%)	316 (9.2%)	307 (8.9%)	334 (9.7%)	1256 (9.1%)	
More than 12 months	2386 (69.4%)	2390 (69.5%)	2383 (69.3%)	2386 (69.3%)	9545 (69.3%)	
Contact						0.998
Post	1775 (51.6%)	1770 (51.5%)	1777 (51.6%)	1781 (51.7%)	7103 (51.6%)	
Email	1665 (48.4%)	1668 (48.5%)	1664 (48.4%)	1664 (48.3%)	6661	
Nationality						0.205
Missing	9	3	7	8	27	
Austria	2514 (73.3%)	2581 (75.1%)	2533 (73.8%)	2578 (75.0%)	10206 (74.3%)	
Other	917 (26.7%)	854 (24.9%)	901 (26.2%)	859 (25.0%)	3531 (25.7%)	
Health						0.839
No health restriction	2202 (64.0%)	2188 (63.6%)	2185 (63.5%)	2221 (64.5%)	8796 (63.9%)	
Health restriction	1238 (36.0%)	1250 (36.4%)	1256 (36.5%)	1224 (35.5%)	4968 (36.1%)	
Marg. empl.						0.493
No	3088 (89.8%)	3058 (88.9%)	3081 (89.5%)	3102 (90.0%)	12329 (89.6%)	
Yes	352 (10.2%)	380 (11.1%)	360 (10.5%)	343 (10.0%)	1435 (10.4%)	
German						0.329
Partial or non	857 (24.9%)	818 (23.8%)	821 (23.9%)	793 (23.0%)	3289 (23.9%)	
Proficient or native	2583 (75.1%)	2620 (76.2%)	2620 (76.1%)	2652 (77.0%)	10475 (76.1%)	

D.2 Treatment (Intervention 2)

Figure D1: Control group: standard text

Bis zu € 15.000,- sind für Ihre Weiterbildung reserviert!

Sehr geehrte Damen und Herren,

Nutzen Sie die Chance zum beruflichen Neustart mit einer Qualifizierung im Rahmen der Corona-Joboffensive! Bis zu € 15.000,- sind beim AMS Niederösterreich für Ihre zukunftssichere Aus- und Weiterbildung für Sie reserviert.

Finden Sie gemeinsam mit Ihrer AMS-Beraterin oder Ihrem Berater den für Sie richtigen Weg zurück ins Berufsleben und lösen Sie Ihren Weiterbildungsgutschein ein! In diesem Mail zeigen wir Ihnen, wie Ihr beruflicher Neustart gelingen kann.

Figure D2: Treatment 1: financial benefits

Bis zu € 15.000,- sind für Ihre Weiterbildung beim AMS für Sie reserviert - machen Sie eine Ausbildung und erhalten Sie bis zu € 1.000,- * Schulungsgeld pro Monat vom AMS!

Sehr geehrte Damen und Herren,

Nutzen Sie die Chance zum beruflichen Neustart mit einer Qualifizierung im Rahmen der Corona-Joboffensive! Bis zu € 15.000,- sind beim AMS Niederösterreich für Ihre zukunftssichere Aus- und Weiterbildung für Sie reserviert.

Bilden Sie sich weiter, erhalten Sie bis zu € 1.000,- * Schulungsgeld pro Monat vom AMS!
Finden Sie gemeinsam mit Ihrer AMS-Beraterin oder Ihrem AMS-Berater den für Sie richtigen Weg zurück ins Berufsleben, lösen Sie den beiliegenden Weiterbildungsgutschein ein und erfahren Sie, wie wir Sie monatlich mit mindestens € 1.000,- * Ausbildungsgeld bei längeren Vollzeitausbildungen unterstützen können.

Figure D3: Treatment 2: training returns

Bis zu € 15.000,- sind für Ihre Weiterbildung beim AMS für Sie reserviert - machen Sie eine Ausbildung und steigern Sie so Ihre Chance auf einen neuen Job um 50%!

Sehr geehrte Damen und Herren,

Nutzen Sie die Chance zum beruflichen Neustart mit einer Qualifizierung im Rahmen der Corona-Joboffensive! Bis zu 15.000,- Euro sind beim AMS Niederösterreich für Ihre zukunftssichere Aus- und Weiterbildung für Sie reserviert.

Mit der richtigen Ausbildung steigern Sie Ihre Chance auf einen neuen Job um 50%!
Finden Sie gemeinsam mit Ihrer AMS-Beraterin oder Ihrem Berater den für Sie richtigen Weg zurück ins Berufsleben und lösen Sie Ihren Weiterbildungsgutschein ein! **Denn mit AMS-Ausbildungen in nachgefragten Berufen steigen Ihre Beschäftigungschancen um 50%**. Gleichzeitig reduziert sich das Risiko, erneut arbeitslos zu werden.

Figure D4: Treatment 3: tailored support

Bis zu € 15.000,- sind für Ihre Weiterbildung beim AMS für Sie reserviert - finden Sie gemeinsam mit Ihrer AMS-Beraterin oder Ihrem AMS-Berater das passende Angebot!

Sehr geehrte Damen und Herren,

Nutzen Sie die Chance zum beruflichen Neustart mit einer Qualifizierung im Rahmen der Corona-Joboffensive! Bis zu 15.000,- Euro sind beim AMS Niederösterreich für Ihre zukunftssichere Aus- und Weiterbildung für Sie reserviert.

Maßgeschneiderte Beratung für eine Weiterbildung, die zu Ihren Fähigkeiten passt.
Sie sind noch nicht sicher, welchen Weg Sie einschlagen möchten? Finden Sie gemeinsam mit Ihrer AMS-Beraterin oder Ihrem AMS Berater den für Sie richtigen Weg zurück uns Berufsleben und lösen Sie den beiliegenden Weiterbildungsgutschein ein.

Nutzen Sie die Möglichkeit einer maßgeschneiderten Beratung, um abgestimmt auf Ihre Wünsche und Fähigkeiten eine für Sie passende Weiterbildung zu finden! In diesem Mail zeigen wir Ihnen, wie Ihr beruflicher Neustart gelingen kann.

D.3 Results (Intervention 2)

Table D2: Training results intervention 2

	Dependent variable:					
	Training	Training completion	Long training	Examined training	Application courses	Subsidized employment
	(1)	(2)	(3)	(4)	(5)	(6)
+ fin. support	−0.008	−0.008	−0.006	0.0002	−0.001	−0.018*
	(0.006)	(0.006)	(0.005)	(0.004)	(0.004)	(0.010)
+ training benefits	−0.003	−0.005	−0.002	0.004	0.001	0.010
	(0.006)	(0.006)	(0.005)	(0.004)	(0.004)	(0.010)
+ ind. support	−0.001	0.0004	0.003	0.001	0.005	0.012
	(0.006)	(0.006)	(0.005)	(0.004)	(0.004)	(0.010)
Control Mean	0.096	0.081	0.065	0.035	0.026	0.224
Control SD	0.295	0.273	0.246	0.185	0.16	0.417
Observations	13,131	13,131	13,131	13,131	13,131	13,131

Note: Standard errors are in parenthesis: *p<0.1; **p<0.05; ***p<0.01

Table D3: Employment results intervention 2

	Dependent variable:				
	Any employment	Days in employment	Days in unemployment	Avg. daily wage	Jobquality
	(1)	(2)	(3)	(4)	(5)
+ fin. support	−0.014	1.261	0.096	−1.611	0.012**
	(0.011)	(2.641)	(3.327)	(1.058)	(0.006)
+ training benefits	−0.022**	−3.456	2.404	−1.369	−0.002
	(0.011)	(2.567)	(3.306)	(1.047)	(0.006)
+ ind. support	−0.007	−0.652	−1.520	−0.339	0.004
	(0.011)	(2.605)	(3.361)	(1.049)	(0.006)
Control Group Mean	0.416	66.94	265.334	48.435	0.32
Control Group SD	0.493	111.47	139.596	28.823	0.136
Observations	13,131	13,131	13,131	5,620	4,446

Note: Standard errors are in parenthesis: *p<0.1; **p<0.05; ***p<0.01