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

Lukas Lehner* Anna Schwarz†

December 1, 2023

Click here for the latest version.

Abstract

Governments struggle to attract unemployed workers to their widely offered job training programs. In a randomized field experiment with 11,050 job seekers, we investigate the potential of informational interventions to encourage participation in job training programs. Raising awareness about the availability of job training increases program enrolment by 18%. Signalling program cost with a voucher on top to reduce internalized stigma increases completion by 28%. Effects are sizeable and concentrated among women and low-income job seekers. Notably, increased job training does 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..

We thank Sven Hergovich former managing director of the *AMS NÖ* and the employees of the *AMS*, including Christian Bliem, Mario Danler, Petra Eisner, Gabriele Fälbl, Martina Fischlmayr, Karmen Frena, Andreas Kreiner, Günter Martinsich, Paul Reisenauer and Daniel Riegler, without whose support this study would not have been possible.

We thank Daron Acemoglu, David Autor, Sydnee Caldwell, David Card, Bart Cockx, Stefano DellaVigna, Lydia Dimitrakopoulou, Bernhard Ebbinghaus, Per Engzell, Chris Featherman, Simone Häkl, Christoph Henking, Simon Jäger, Casper Kaiser, Maximilian Kasy, Erin Kelly, Elisabeth Leduc, Rudolf Moser, Elias Naumann, Brian Nolan, Dylan Nelson, Patrick Nüß, Amanda Pallais, Jesse Rothstein, Georg Rilinger, Emmanuel Saez, Rupert Sausgruber, Susan Silby, Dennis Tamesberger, Simon Theurl, Nathan Wilmers, Janet Xu, Victoria Zhang, and Erza Zuckerman for valuable feedback and comments, and seminar participants at IHS, LSE, MIT, UC Berkeley, University of Chicago, University of Oxford, and WU Vienna. We acknowledge financial support from the AMS. Lukas Lehner acknowledges financial support from the Economic and Social Research Council [grant number ES/P000649/1], the Scatcherd European fund and the Saven European fund at the University of Oxford as part of his PhD funding, and the Austrian Economic Association through its Dissertation Fellowship.

The experiment was pre-registered as AEARCTR-0007141 Lehner and Schwarz (2021). 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.

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 prevent 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 for unemployed people often seen as failing to contribute, which encourages blame, results in shame and social stigma related to reliance on the welfare state (Goffman, 1963; Moffitt, 1983; Walker, 2014). 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 prevent 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 being hesitant to participate. We examine barriers

[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.

for job seekers' hesitancy of engaging 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 prevent disadvantaged groups from accessing public services resulting in non-take-up.

Experimental Design We answer this question with a field experiment involving 11,000 job seekers. 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 of all four groups. The treatments are stacked and designed to separate out interacted effects of raising awareness (treatment 1), combined with signaling 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, perceptions, and experiences to uncover mechanisms. 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**, four sets of findings are noteworthy. First, **raising awareness** has a large positive effect on training enrollment; **signaling the monetary value** on top helps to further improve program completion. The magnitude of an 18%-21% increase in training enrollment compared to baseline is striking given that the intervention consists only of one e-mail. Two years after treatment, recipients were still 10% more likely to having participated in job training, which shows that the intervention does not only encourage job seekers to train earlier but leads to a sustained increase in training (Table B2). Signaling 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 ambitious programs. Both, the e-mail and voucher increase enrollment in programs with longer duration, typically oriented toward acquiring job-related skills and human capital formation. Both treatments increase participation in examined programs, which are more ambitious and provide a certificate for successful completion. 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 a 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 canceling out any positive effects from raising awareness and signaling the monetary value of training programs (e-mail + voucher + information, treatment 3). 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.

Fourth, using our survey data, we are able to trace the steps from job seekers' intention to train, the role of caseworkers, training enrollment, and completion for the first time in an experimental study. We show that our treatments shift perceptions of courses in the way we intend to and also increase intentions to take up training. However, we also show that it leads to increased debate with caseworkers on training program choice. Further, caseworkers play an important role for whether such information interventions can be effective. Our positive treatment effects on training are mostly concentrated among job seekers assigned to caseworkers with lower re-employment successes. We discuss potential implications of this in Section 6.

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 info).

The results also reveal that government funded job training programs do not necessarily improve labor market prospects of unemployed workers. The absence of positive effects on employment or wages raises doubts about governments' intentions to nudge job seekers into widely offered training programs. Remaining hesitancy of job seekers' to enroll in job training may reflect actual preferences about how to allocate their time instead of barriers to access social programs.

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 summarized by Haaland et al. (2023). As we learned after the intervention was implemented, each of our treatment arms resembles interventions tested in separate experiments in different countries at about the same time. We compare results in Section 6. Our study allows 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). 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) use 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 us, 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). Evidence from the lab points to an important role of **psychological frictions** such as social stigma for non-take-up of benefits (Friedrichsen et al., 2018). 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). Framing of messages does matter in other contexts though (Linos et al., 2020; Lasky-Fink and Linos, 2022; Linos et al., 2022b; Osman and Speer, 2023).[2] What differs is

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

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). We contribute by opening the black box 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, 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 also 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). Austria is widely considered to have one of the most developed PES and job training programs and has served as a role model for other countries. Our null results on employment are thus surprising and we discuss potential explanations in Section 6, where we also compare our results to other studies which, in Europe, are mostly non-experimental.

Eventually, our findings contribute to the understanding of **unintended consequences** of active labor market policy interventions (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 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). While this literature largely finds positive employment effects of correcting misbeliefs, we document with treatment 3 negative consequences for job seekers' training and null effects on employment. Medium and high skilled workers get discouraged when learning about occupational labor demand concentrated in low-skill occupations.

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. Appendix C provides details on the complementary survey including the questionnaire and additional results.

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 centered around raising employment, improving job-worker matching, and increasing human capital, while equity concerns aim at leveling 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 but public works projects existed even before and were implemented at scale already during the 1920s in post-WWI Europe and during the 1930s Great Depression under the New Deal in the US (Vlandas, 2013). Sweden pioneered modern ALMP manpower programs in the 1950s 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). Following the Nordic examples, Austria was in the late 1960s 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 (application 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 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 targeted at a specific group of unemployed workers, the most disadvantaged job seekers including 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 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 up 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 exam constitute more ambitious programs. 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.

Assignment, eligibility and conditionality 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 about training opportunities besides benefits and job-search. The dual role of caseworkers reflects a deeper ideological divide about providing welfare support to unemployed workers versus demanding active job-search and work availability "in return".

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

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 for a large number of ALMPs but caseworkers have the last 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 intentioned 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 to "exercising pressure" for assignment. "Meeting their target" for program assignment is another motive for caseworkers, which is equal 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 lasted 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 implemented a field experiment at scale (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 in Section 3.1, our 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 our sample and treatment assignment are shown in Section A.

Further details on the study design are documented in the pre-registered 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.

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

3.1 Data

Administrative records We leverage a wide range of demographic-, benefit- and job characteristics from administrative data including (i) social security records; and (ii) additional data collected by the PES. The first allows us to construct whole employment (and unemployment) histories for the people in our sample and additionally follow their labor market outcomes after our intervention. The second provides additional socio-demographic information, which is not included in the social security records, such as education, language skills, and occupation. Additionally, the PES provides us with fine-grained information on job seekers' participation in all types of ALMPs. Due to our reliance on administrative data, we face virtually zero attrition.

Surveys Additionally, we designed our own survey, which we are able to link with the administrative records at the individual level. We collect detailed data on training intentions, experiences and perceptions of the PES, 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 around one month after the intervention. 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 an overview 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 2 to 3 and 6 to 12 months at the time of treatment.[6] The sample is restricted to people who are at least 25 years old.[7] Also, unemployed job seekers who are already enrolled in a training program or have a job offer accepted at the time of the intervention are excluded from the sample.

This leaves us with 11,050 unemployed people (Table 1 column (3)).[8] Among them, 52% are women, 30% are younger than 35 years and about 32% older than 50 years. 1/3 has no more 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. 14.5% speaks only limited or no German.

[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 were thus excluded from the sample.

[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 citizenship 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 years	30.3%	29.7%	**29.9%**	33.4%
35-50 years	37.0%	37.1%	**38.5%**	39.4%
Above 50 years	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%
Citizenship				
Austrian Nationality	82.8%	82.0%	**77.9%**	65.7%
Non-Austrian Nationality	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 unempl.	28.5%	30.9%	**24.3%**	28.8%
6-9 months unempl.	43.0%	40.0%	**33.9%**	28.9%
9-12 months unempl.	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.

Overall, our sample is very similar to the whole Austrian sample (Table 1 column (4)), despite Lower Austrian job seekers being more likely to hold the Austrian citizenship. 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 with a duration of 9-12 months in our sample. Among them, a higher share had lower educational attainment and a non-Austrian citizenship, while a smaller share had a health restriction compared to the pool of unemployed job seekers before the pandemic. With regard to gender and age, the composition remained broadly the same.

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 only later and we thus measure them within 24 months after the intervention.[9] We report descriptive statistics for these outcomes in Table 2. Our training outcomes 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. However, we also measure days in employment and unemployment as employment outcomes, as well as the average daily wage when the person was employed and construct 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.

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

At baseline, 11% of job seekers enroll in a training program within 12 months after the intervention, while almost 10% also complete these programs. 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 complete with an exam, which is another indicator for more ambitious 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),

[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.

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

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, 75% of job seekers in our sample have been in employment for at least one day within 24 months after the intervention (column 1). On average, a job seeker is 350 days in employment (column 3) and 361 days in unemployment (column 4). Once in employment, their average wage amounts to 51 Euros gross per day (column 5).

3.2 Experimental design

Treatment assignment We assigned study participants to one of three treatments and one control group using stratified randomization. We used the following covariates to construct the strata: gender, age, education (i.e., more than the legally required minimum), 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 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 Our intervention consists of three treatment arms in which e-mails with varying types of information on job training were sent out. 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 on occupations with open vacancies. The control group is not contacted, but continues to have access to training and regular PES consultations, like the three treatment groups. 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 prevent 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

[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 direct effect of signaling the monetary value.

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 by the PES with their most expensive training programs on offer. By signaling the monetary value of the programs, the treatment is intended to reduce internalized stigma of job seekers that create psychological frictions and prevent job seekers from program participation. 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. Job seekers are found to search in occupations with relatively few vacancies (Sahin et al., 2014). This information is intended to encourage job seekers for training in occupations with high labor demand and broaden job search beyond their previous occupation. Previous studies have found that improved access to information can broaden their search and increase the number of job interviews (Belot et al., 2019).

E-mail clicks 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 the 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).

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, which yields instrumental variable estimates of the local average treatment effect (LATE) of actually having received the information treatment. The effect of assignment on opening the e-mail is estimated to be around .91 on average, so that the corresponding instrumental variable estimates of all treatment effects on training enrollment 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. Effects are thus driven by those job seekers who enroll into 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.

[12]The voucher also includes €3.000,- for any training not provided via the PES.

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 into 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. We know that 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 recorded before treatment $\mathbf{X_i}$ that were not used for stratification. This includes language skills, nationality, occupation, marginal employment, pre-unemployment income, and the days in employment and number of employment spells in the 10 years before. 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). However, Table B9 additionally presents the employment results for the three treatment groups separately for robustness.

The heterogeneity analysis is conducted via subgroup 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 on one year after the intervention, while the employment analyses apply to a two-year time frame.

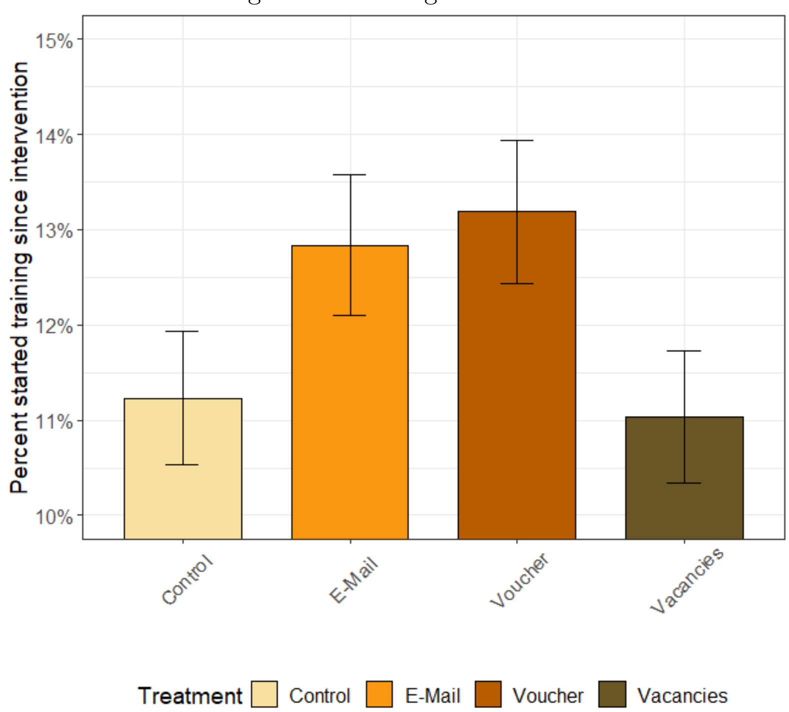
Figures 4-3 and Tables 3-5 present our main results. Additional Figures with results for robustness using alternative estimation approaches and variable definitions, timing patterns, and heterogeneity are documented in Appendix B.

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 18% and 21% 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. For the interpretation, 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 the same size as for participation (19% for the e-mail and 28% 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 a positive additional effect on training.

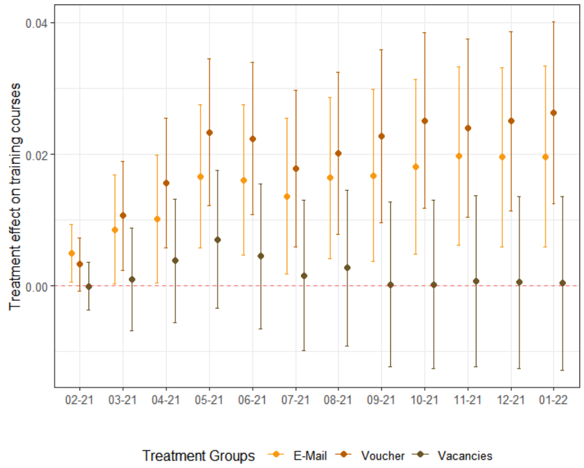
The treatments also tend to affect the type of training undertaken. Job seekers shift participation to more ambitious training programs defined as longer in duration (column 3) and courses with an examination on completion (column 4). At the same time, the increase in training for the e-mail and voucher treatments seems to have small spillover effects 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). This equals a 20% drop in application course enrollment. Job seekers who receive the voucher tend to take-up less subsidized employment, which equals the magnitude of the increase in training enrollment (column 6). Thus, information frictions play an important role in our setting and removing them has the biggest effect on training take-up of unemployed. However, we also find indications that the voucher has an additional effect especially on the motivation to complete training, which points at the role of 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

Figure 2: Average treatment effects on training enrollment over time



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

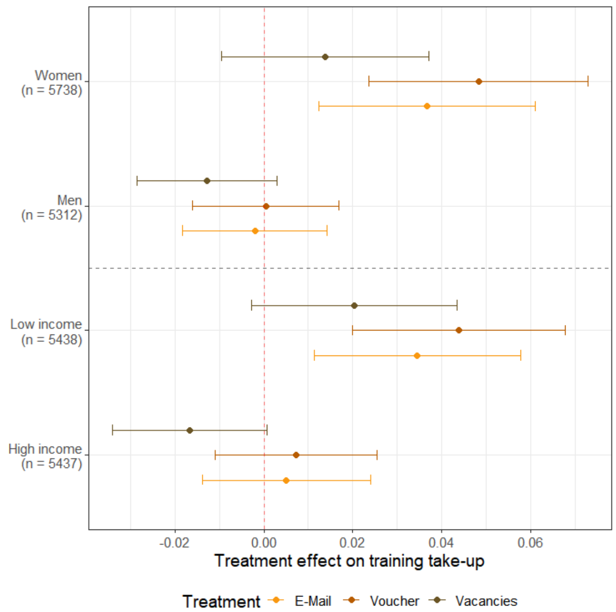
Timing patterns To analyze timing patterns in the treatment effects of active labor market programs as suggested by Card et al. (2018), we investigate the temporal dimension of treatment effects on a monthly basis for 12 months following the treatment. Regarding outcomes, we consider whether job seekers participate in a training program since the intervention took place. Figure 2 shows the treatment effect on training program enrollment per month. Within the first 4 months, the voucher treatment 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. This leads to sustained higher training enrollment in these two groups with no catch-up effect of the control group for the first 12 months after treatment. This can be seen in (Figure B1), which shows training enrollment by treatment group cumulatively over the first year. As before, the vacancies treatment shows no signs of a significant nor substantial increase in training enrollment.

These treatment effects on training remain substantial also two years after the intervention, as can be seen in Table B2 and Figure B2. Two years following the intervention, treated recipients still showed a 10% higher likelihood of having participated in job training. This demonstrates that the intervention's impact extends beyond merely prompting earlier training among job seekers; it also leads to a sustained increase in training participation.

Eventually, Figure B3 shows the substitution of application courses and Figure B4 shows the substitution of subsidized employment per month. 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).

Heterogeneity To account for heterogeneity, we conducted subgroup 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 with lower income in their previous job (Figure 3 and Table 4). Further, unemployed people older than 35 years, with Austrian citizenship 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 signaling the monetary value on top (voucher).

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



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

Treatment 3 (e-mail + voucher + information) results in interesting diverging outcomes for different subgroups. Contrary to treatments 1 and 2, job seekers in blue-collar occupations react much more positively than those in white-collar occupations. The same holds for low-skilled vs. high-skilled occupations. These effects are shown in Table B5. Additionally, the point estimates point in a negative direction for more advantaged groups, such as men, higher income, and core age groups, albeit not significantly so. In Section 5, we will discuss more in depth how these patterns can be interpreted.

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.

Main Findings 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). 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 slightly 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 B6). 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 nonsignificant 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 for income (Table B7), estimation

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

strategies including IV (Table B8), and for observing treatment groups separately (Table B9). 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

Table 6:

	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

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

Heterogeneity We do not find significant heterogeneity in employment(Appendix B.4). Employment effects tend to be more negative for those groups that show the strongest increase in training, i.e. women (Table B10), people aged 35 to 50 years (Table B11), those with Austrian citizenship(Table B12), and job seekers who previously worked in medium-skilled occupations (Table B13). This suggests that lock-in effects drive the employment effects. However, none of these are statistically significant and thus have to be interpreted with care.

5 Mechanisms

5.1 First stage

To better understand the relatively large effects of our intervention on training, we leverage our survey data, described in Section 3 and Section C.1, to analyze shifts in perceptions and intentions that precede actual training enrollment.

Perceptions Recipients of the e-mail and voucher report less often that they lack information on courses (column 3), 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. This could either 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 received the voucher tend to report more often that courses are expensive (column 2), which suggests that the voucher was effective in signaling the monetary value of training programs. Thus, it seems that our intervention actually shifted perceptions of courses in the way we intended to. However, the coefficients are not statistically significant, which is likely related to the lower sample size in the survey data.

Table 7: Perceptions of courses

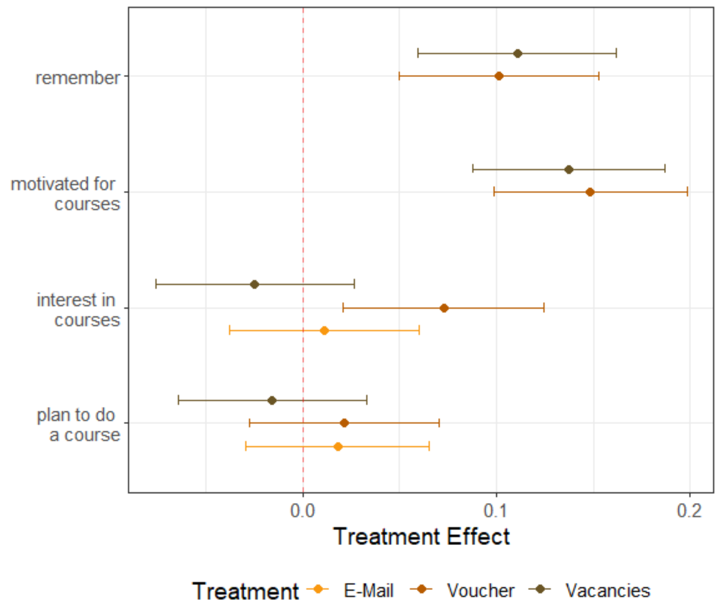
	Dependent variable:	
	Lack information	Courses are expensive
	(1)	(2)
E-mail	−0.030	0.017
	(0.038)	(0.030)
Voucher	−0.015	0.030
	(0.040)	(0.031)
Vacancies	0.054	0.035
	(0.040)	(0.031)
Reference Mean	0.425	0.64
Reference SD	0.495	0.48
Caseworker Fixed Effects	0	1
Observations	1,145	1,722

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

Intentions We first compare interest and plans for course participation between the treatment and control groups (Figure 4). We also analyze whether the type of treatment affects respondents memory of the treatment and motivation for courses.

The voucher treatment significantly raises job training intentions (Figure 4). General interest in courses offered by the PES increases after receiving the voucher. Specific plans to enroll in a course show signs of elevation on average for e-mail and voucher recipients but the effects are not statistically significant. Interest and plans for courses seem to decrease for recipients of the vacancies treatment though not statistically significant. Among those who were treated, recipients of the voucher and vacancies information are more likely to remember the information received. Voucher and vacancies information recipients also show higher motivation for courses compared to those who only received the e-mail. Overall, these results suggest that the treatments are successful in shifting job seekers intentions to engage in training.

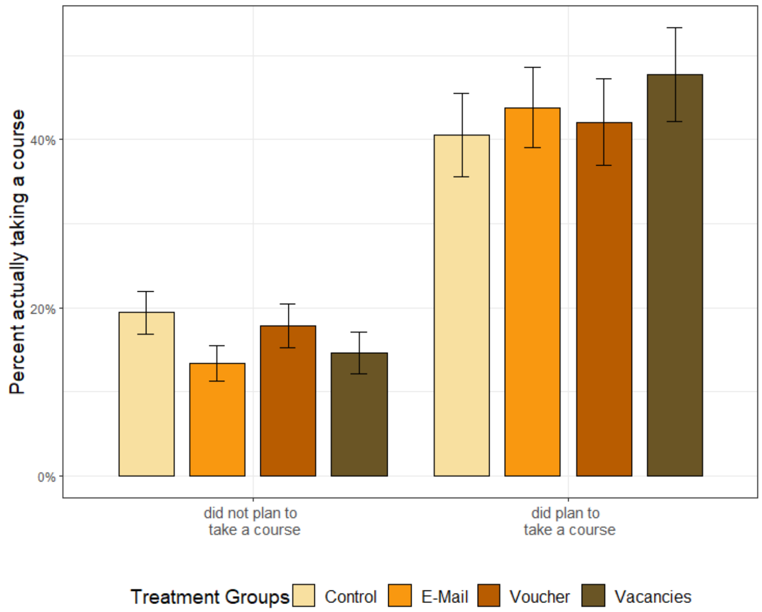
Figure 4: Averate treatment effect on intentions to train



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

Additionally, intentions for training translate into actual program enrollment (Figure 5). Among job seekers who had plans to take a course, 40%-50% eventually enroll in a program. By contrast, among those who do not plan to take a course, only 10%-20% end up enrolled in a training program. Some job seekers may have changed their mind or do not follow through on their intentions, which creates some noise around the correlation of intentions and enrollment. Still, job seekers' own intentions are found to matter for actual training, which underscores to the discretion on the side of job seekers in deciding whether to enroll in a program. Some of those who intended to enroll may have not been accepted by caseworkers, which we investigate in Table 8. At the same time, the 10%-20% of job seekers who did not intend to train but end up in training may correspond to job seekers who were assigned to their course. There are no sizable treatment differences in the correlation of intentions and actual training enrollment. However, one may argue that the correlation is lower for the control group, i.e. the share of those who planned to participate and actually participated is slightly lower while the share of those who did not plan to enroll but eventually enrolled is slightly higher than for the other groups.

Figure 5: Training intentions and enrollment



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

5.2 Caseworker's role

Thus, our intervention actually increased 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 could thus either result in increased enrollment, or increased rejection of job seekers' course wishes. The importance of caseworkers' discretion has for example 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 an experimental study.

Assignment In Table 8, we show related survey outcomes. First, all three treatments enhance job seekers self-assessed autonomy in course assignment. Recipients of any treatment feel more in control over which course to choose (column 1). This increase in perceived autonomy over training enrollment among job seekers leads to more discussion about course choice with caseworkers. Treated job seekers report less often that their wishes for training program assignment were considered by caseworkers (column 2). Reversely, course plans of treated job seekers are more often turned down (column 3). Thus, job seekers feel more autonomy over program choice but were confronted with the reality of required approval by job caseworkers. While this indicates the boundaries of increasing perceived autonomy without changing the formal assignment rules, the treatments suggest to have some impact on course assignment. Treated job seekers tend to report less often that assignment to a course by a caseworker is a reason for program enrollment (column 4). The increased interest of job seekers for training also results in a higher share of job seekers reporting that they could not find a suitable course (column 5). These mechanisms help to understand the role of caseworkers in the context of training program assignment.

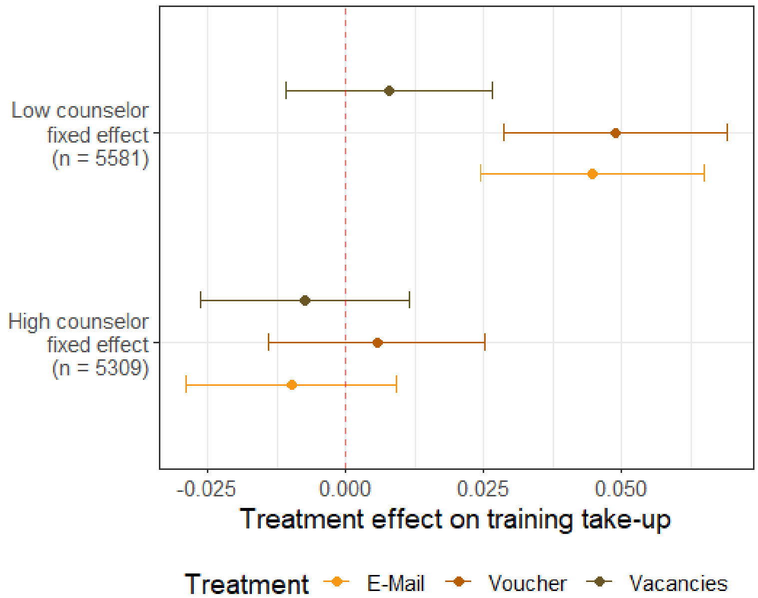
Table 8: Training program assignment

	Dependent variable:				
	Choose own courses	My wishes are considered	Course was turned down	Assigned to course	Couldn't 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.

Treatment effect heterogeneity Additionally, we analyze the interaction of our treatment effects with the caseworkers. Therefore, we use the fitted values of caseworker fixed effects in a regression on employment duration controlling for all baseline covariates and the treatment group. We choose employment duration, as we deem this as the desired outcome of PES counseling (longer employment duration after the unemployment spell indicates a good match). 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. We then re-estimate our main analysis separately in these two subgroups.

Figure 6: Average treatment effect on training enrollment by caseworker



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

We show that our treatment effect on training enrollment is strongly driven by job seekers assigned to caseworkers with lower than median fixed effects (Figure 6). This also holds for training completion and when using unemployment duration as the dependent variable in the fixed effects estimation (Table B14). Thus, general information interventions that are sent out centrally seem to mostly affect those job seekers assigned to caseworkers that achieve shorter re-employment of their clients. We further discuss the interpretation of caseworker fixed effects in Section 6.

5.3 Vacancies

While the vacancies information don't affect aggregate training behavior, it did shift some of our survey outcomes, such as perceived value of the courses, or self-assessed autonomy. Most importantly, sub-group analyses show that the vacancies information may have discouraged job seekers from training who are overqualified for the jobs in high demand. Our survey provides further suggestive evidence for this (Table B15). Among survey participants with previous occupations that were low-skilled nearly 50% deem the information as important and 41% would consider working in one of these jobs, whereas only around 30% among those with high-skilled occupations do the same. A similar pattern emerges for education, as well as age groups, where the more advantaged rate the information as less important.

In addition, we check whether the vacancy treatment potentially only had an effect on specific training programs that are related to the vacancies that were advertised in the e-mail job seekers received (Table B16). However, our results do not support this claim.

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 separately for our results on 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 the negative effect of vacancy information. Investigating these aspects helps to understand the underlying mechanisms.

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) Alike 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 up to 15% in compliance with municipal housing codes (Linos et al., 2020), an increase 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%, driven by more vulnerable job seekers. However, the setting is not completely comparable to ours, as the information refers to financial aid programs for post-secondary education rather than PES-specific training courses.

One may think of several reasons why our experiment was so successful in shifting training enrollment of job seekers. First, the design of the e-mail may be more accessible and appealing to job seekers. However, our e-mail (treatment 1) is similar in design and content to Dhia and Mbih (2020); Leduc and Tojerow (2023). Nonetheless, we see indications that adding the voucher (treatment 2) had some additional effects, albeit more along the margin of actual training completion than take-up. Thus, this could be one driving factor but may not explain the full difference to other studies. Second, contextual factors may have amplified the large effect on training. Indeed, the intervention was implemented during a large-scale expansion of training program supply, which could 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.

Finally, differences in the approach of PES caseworkers could play a role.

Caseworkers' role Our detailed administrative data in combination with our survey data allows us to provide first insights into the job seeker-caseworker dynamic. In our survey, we find that the intervention leads to increased discussions with caseworkers and even rejections of courses. Additionally, we show that our treatment effects are strongly concentrated among job seekers assigned to caseworkers with low fixed effects (i.e. those where their assigned unemployed have lower employment duration, all else equal). These low fixed effects could be interpreted as capturing more lenient caseworkers who exert less pressure on job seekers to find a job quicker, which leads to their lower employment duration. This is in line with the argument that, for such a treatment to be effective in raising training take-up, caseworkers have to be willing to approve additional courses. None of the 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 training (Leduc and Tojerow, 2023), which did not translate into training enrollment. In general, activation requirements in France and Belgium do not seem to be more stringent than in Austria and even more lenient with regard to ALMP participation (OECD, 2023). However, 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.
Moreover, the fact that our treatment effects are strongly dependent on caseworkers' fixed effects can also have political implications. A related study in Austria shows that caseworkers are mostly overworked and have too little time to engage with each of their assigned job seekers (Böheim et al., 2022). If we interpret those caseworkers with low fixed effects as those who are overworked, this indicates that very general information interventions can substitute for caseworkers' time and thus reduce the burden on them. Naturally, this can only accompany measures to increase the number of caseworkers and thus reduce caseload. Further, in the next paragraph, we also discuss potential pitfalls of some forms of information interventions.

Unintended consequences The negative effect of the vacancy information (treatment 3) on training enrollment shows 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 direct 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, especially job seekers with educational attainment above compulsory schooling get discouraged from training, while job seekers with low educational attainment respond positively.

6.2 Employment

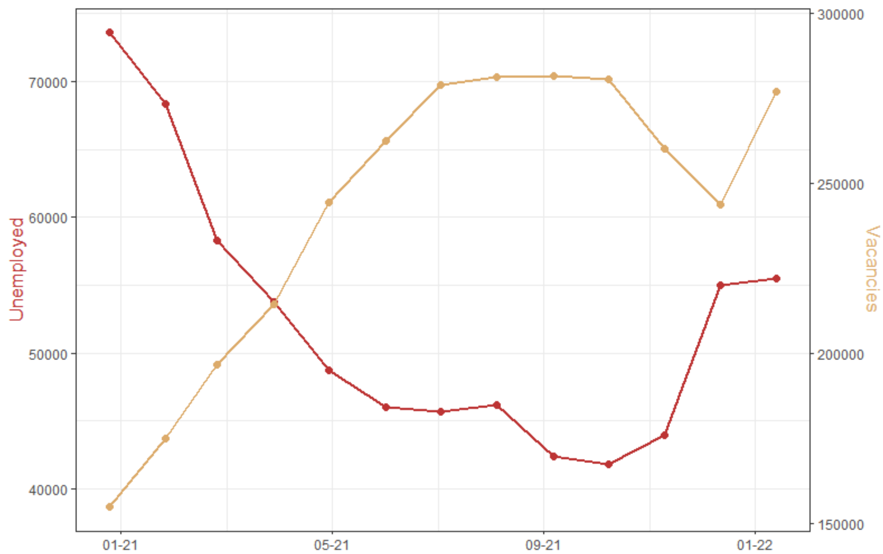
The absence of positive employment effects of training is surprising. We compare our results to related studies and discuss reasons that help understand our findings including possible lock-in effects, and contextual factors from the macro environment.

Comparison We do not have directly comparable estimates for employment from other similar experiments on training take-up, since those did not shift program enrollment as described above. However, we can compare our employment effects to those in Barr and Turner (2018), given the strong increase of enrollment in post-secondary education that they found. They also do not find any effects on earnings three years after the letter, indicating that the negative immediate earnings effects (lock-in effects) of post-secondary education are exactly 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 (Altmann et al., 2018) increases employment by 1-4%. 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 received the job search brochure worked for about 1.2 days more on average than individuals who did not (Altmann et al., 2018). When comparing our estimates to observational studies on the effects of ALMP on employment (without any information treatments), it seems that our estimates are smaller than would be expected. Most studies find small, but positive employment effects, albeit only in the medium-or long-run. However, our estimates do not directly compare to these, because our employment effects only reflect the increase in training induced by our intervention and the related shift to more demanding courses. These shifts may be too small to produce substantial quantifiable employment effects within two years after the intervention.

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). We see some indications of negative short-run employment effects in Figure B5, but these are limited and seem to be offset on average. This makes sense, as the literature suggests that lock-in effects will be smaller in times of recessions (Lechner and Wunsch, 2009). Our intervention took place amidst the Covid pandemic in Feburary 2021, which could be the reason for the limited lock-in effects. However, as discussed in the next paragraph, the influence of the macroeconomic situation is less straightforward as it may seem.

Macroeconomic context As already mentioned, the intervention was placed amidst the Covid pandemic to minimize potential lock-in effects of increased training. However, the recovery of the Austrian labor market started sooner as expected, which can be seen in Figure 7. Unemployment fell drastically in the first half of 2021, while the number of vacancies doubled. Most of the training effects we observe also occurred during this time (see Figure 1). Thus, job seekers that were induced to take up training essentially missed this first recovery phase in 2021, because they were prioritizing training instead of job search. This could explain our muted employment effects.

Figure 7: Labor market context



Note: Number of unemployed and posted vacancies in Lower Austria in 2021. Data derived online from AMS DataWarehouse.

To further 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 job seekers to voluntary enroll into training programs. Many are hesitant due to barriers from information frictions and psychological frictions. Our multi-armed field experiment demonstrates 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 internalized stigma as a psychological friction 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 program enrollment. However, we do not find positive effects of job training on employment or wages.

Outlook Further evaluations should be carried out in other countries and time periods to investigate the surprising absence of positive effects of the observed increase in training on employment. Based on the positive effects on training enrollment, the PES has implemented the most effective treatment on a permanent basis. As part of this 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. The follow-up field experiment spans an entire year post-pandemic to account for possible distortions from business cycle dynamics, 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. Our 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.

References

Abebe, G., Caria, S., Fafchamps, M., Falco, P., Franklin, S., Quinn, S., and Shilpi, F. (2021). Matching Frictions and Distorted Beliefs: Evidence from a Job Fair Experiment. page 83.

Adams-Prassl, A., Boneva, T., Golin, M., and Rauh, C. (2023). Perceived returns to job search. *Labour Economics*, 80:102307.

Altmann, S., Falk, A., Jäger, S., and Zimmermann, F. (2018). Learning about job search: A field experiment with job seekers in Germany. *Journal of Public Economics*, 164:33–49.

Anders, J. and Rafkin, C. (2022). The Welfare Effects of Eligibility Expansions: Theory and Evidence from SNAP.

Angrist, J. D., Imbens, G. W., and Rubin, D. B. (1996). Identification of Causal Effects Using Instrumental Variables. *Journal of the American Statistical Association*, 91(434):444–455.

Arni, P. and Schiprowski, A. (2019). Job search requirements, effort provision and labor market outcomes. *Journal of Public Economics*, 169:65–88.

Athey, S. and Imbens, G. W. (2017). The Econometrics of Randomized Experiments. In *Handbook of Economic Field Experiments*, volume 1, pages 73–140. Elsevir.

Bandiera, O., Bassi, V., Burgess, R., Rasul, I., Sulaiman, M., and Vitali, A. (2021). The Search for Good Jobs: Evidence from a Six-year Field Experiment in Uganda. *SSRN Electronic Journal*.

Barbanchon Le, T., Hensvik, L., and Rathelot, R. (2023). How can AI improve search and matching? Evidence from 59 million personalized job recommendations.

Barr, A. and Turner, S. (2018). A letter and encouragement: Does information increase postsecondary enrollment of UI recipients? *American Economic Journal: Economic Policy*, 10(3):42–68.

Belot, M., Kircher, P., and Muller, P. (2019). Providing advice to jobseekers at low cost: An experimental study on online advice. *The Review of Economic Studies*, 86(4):1411–1447.

Bertrand, M., Luttmer, E. F. P., and Mullainathan, S. (2000). Network Effects and Welfare Cultures. *The Quarterly Journal of Economics*, 115(3):1019–1055.

Bhargava, S. and Manoli, D. (2015). Psychological Frictions and the Incomplete Take-Up of Social Benefits: Evidence from an IRS Field Experiment. *American Economic Review*, 105(11):3489–3529.

Black, D. A., Smith, J. A., Berger, M. C., and Noel, B. J. (2003). Is the Threat of Reemployment Services More Effective than the Services Themselves? Evidence from Random Assignment in the UI System. *The American Economic Review*, 93(4):1313–1327.

Boeri, T. and van Ours, J. C. (2021). Active Labor Market Policy. In *The Economics of Imperfect Labor Markets*. Princeton University Press, Princeton, third edition edition.

Böheim, R., Eppel, R., and Mahringer, H. (2022). More Caseworkers Shorten Unemployment Durations and Save Costs. Results from a Field Experiment in an Austrian Public Employment Office. *Working Paper*.

Böheim, R., Horvath, G. T., and Winter-Ebmer, R. (2011). Great expectations: Past wages and unemployment durations. *Labour Economics*, 18(6):778–785.

Bonoli, G. (2010). The Political Economy of Active Labour Market Policy. *SSRN Electronic Journal*.

Bonoli, G. and Liechti, F. (2018). Good intentions and Matthew effects: Access biases in participation in active labour market policies. *Journal of European Public Policy*, 25(6):894–911.

Briscese, G., Zanella, G., and Quinn, V. (2020). Improving Job Search Skills: A Field Experiment on Online Employment Assistance.

Böheim, R., Eppel, R., and Mahringer, H. (2022). More Caseworkers Shorten Unemployment Durations and Save Costs. Results from a Field Experiment in an Austrian Public Employment Office. WIFO Working Papers 647, WIFO.

Card, D., Kluve, J., and Weber, A. (2010). Active labour market policy evaluations: A meta-analysis. *The Economic Journal*, 120(548):F452–F477.

Card, D., Kluve, J., and Weber, A. (2018). What works? A meta analysis of recent active labor market program evaluations. *Journal of the European Economic Association*, 16(3):894–931.

Clasen, J. and Clegg, D. (2011). *Regulating the Risk of UnemploymentNational Adaptations to Post-Industrial Labour Markets in Europe*. Oxford University Press.

Clasen, J., Clegg, D., and Goerne, A. (2016). Comparative Social Policy Analysis and Active Labour Market Policy: Putting Quality before Quantity. *Journal of Social Policy*, 45(1):21–38.

Crépon, B., Duflo, E., Gurgand, M., Rathelot, R., and Zamora, P. (2013). Do Labor Market Policies have Displacement Effects? Evidence from a Clustered Randomized Experiment *. *The Quarterly Journal of Economics*, 128(2):531–580.

Crépon, B. and van den Berg, G. J. (2016). Active Labor Market Policies. *Annual Review of Economics*, 8(1):521–546.

Currie, J., Grogger, J., Burtless, G., and Schoeni, R. F. (2001). Explaining Recent Declines in Food Stamp Program Participation [with Comments]. *Brookings-Wharton Papers on Urban Affairs*, pages 203–244.

Dahl, G. B., Løken, K. V., and Mogstad, M. (2014). Peer Effects in Program Participation. *American Economic Review*, 104(7):2049–2074.

Dhia, A. B. and Mbih, E. (2020). Do informational frictions affect enrollment in public-sponsored training? Results from an online experiment.

Ebbinghaus, B. (2020). Changing work and welfare: Unemployment and labour market policies. In *Handbook on Society and Social Policy*, chapter Handbook on Society and Social Policy, pages 291–305. Edward Elgar Publishing.

Eppel, R., Huemer, U., Mahringer, H., and Schmoigl, L. (2022). Evaluierung der Effektivität und Effizienz von Qualifizierungsförderungen des Arbeitsmarktservice Österreich.

Finkelstein, A. and Notowidigdo, M. J. (2019). Take-Up and Targeting: Experimental Evidence from SNAP. *Quarterly Journal of Economics*, 134(3):1505–1556.

Friedrichsen, J., König, T., and Schmacker, R. (2018). Social image concerns and welfare take-up. *Journal of Public Economics*, 168:174–192.

Gautier, P., Muller, P., van der Klaauw, B., Rosholm, M., and Svarer, M. (2018). Estimating Equilibrium Effects of Job Search Assistance. *Journal of Labor Economics*, 36(4):1073–1125.

Goffman, E. (1963). *Stigma: Notes on the Management of Spoiled Identity*. Spectrum Book. Prentice-Hall, Englewood Cliffs.

Goldin, J., Homonoff, T., Javaid, R., and Schafer, B. (2022). Tax filing and take-up: Experimental evidence on tax preparation outreach and benefit claiming. *Journal of Public Economics*, 206:104550.

Haaland, I., Roth, C., and Wohlfart, J. (2023). Designing Information Provision Experiments. *Journal of Economic Literature*, 61(1):3–40.

Harrison, G. W. and List, J. A. (2004). Field Experiments. *Journal of Economic Literature*, 42(4):1009–1055.

Heckman, J. J., Lalonde, R. J., and Smith, J. A. (1999). The Economics and Econometrics of Active Labor Market Programs. In *Handbook of Labor Economics*, volume 3, pages 1865–2097. Elsevier.

Heckman, J. J. and Smith, J. A. (2004). The Determinants of Participation in a Social Program: Evidence from a Prototypical Job Training Program. *Journal of Labor Economics*, 22(2):243–298.

Heffetz, O., O'Donoghue, T., and Schneider, H. S. (2022). Reminders Work, but for Whom? Evidence from New York City Parking Ticket Recipients. *American Economic Journal: Economic Policy*, 14(4):343–370.

Hervelin, J. (2021). Directing young dropouts via SMS: Evidence from a field experiment. *IZA Journal of Labor Policy*, 12(1).

Hofer, H., Weber, A., and Winter-Ebmer, R. (2013). Labor market policy in Austria during the Crises. Technical report.

Imbens, G. W. and Angrist, J. D. (1994). Identification and Estimation of Local Average Treatment Effects. *Econometrica*, 62(2):467.

Immervoll, H. and Knotz, C. (2018). How Demanding Are Activation Requirements for Jobseekers? Technical report.

Kasy, M. and Lehner, L. (2023). Employing the Unemployed of Marienthal: Evaluation of a Guaranteed Job Program. *Working Paper*.

Katz, L. F., Roth, J., Hendra, R., and Schaberg, K. (2022). Why Do Sectoral Employment Programs Work? Lessons from WorkAdvance. *Journal of Labor Economics*, 40(S1):S249–S291.

Kluve, J. (2010). The effectiveness of European active labor market programs. *Labour Economics*, 17(6):904–918.

Knotz, C. M. (2020). Does Demanding Activation Work? A Comparative Analysis of the Effects of Unemployment Benefit Conditionality on Employment in 21 Advanced Economies, 1980-2012. *European Sociological Review*, 36(1):121–135.

Lasky-Fink, J. and Linos, E. (2022). It's Not Your Fault: Reducing Stigma Increases Take-up of Government Programs.

Lechner, M., Miquel, R., and Wunsch, C. (2011). LONG-RUN EFFECTS OF PUBLIC SECTOR SPONSORED TRAINING IN WEST GERMANY. *Journal of the European Economic Association*, 9(4):742–784.

Lechner, M. and Wunsch, C. (2009). Are Training Programs More Effective When Unemployment Is High? *Journal of Labor Economics*, 27(4):653–692.

Leduc, E. and Tojerow, I. (2023). Training Jobseekers to Address Labour Shortages: An Experimental Study on Information Barriers.

Lehner, L. and Schwarz, A. (2021). Reframing active labor market policy: Experimental evidence of training vouchers for unemployed. Technical report, American Economic Association.

Lehner, L. and Schwarz, A. (2022). What prevents job-seekers from training? Reframing training access to reduce non-take up. Technical report, American Economic Association.

Lehner, L. and Tamesberger, D. (forthcoming). Unemployment and Labor Market Policy. In *Handbook of Social Infrastructure*. Edward Elgar Publishing.

Leitner, S. and Tverdostup, M. (2023). Auswirkungen der Corona- Pandemie auf den niederösterreichischen Arbeitsmarkt. *The Vienna Institute for International Economic Studies (wiiw) report.*

Linos, E., Prohofsky, A., Ramesh, A., Rothstein, J., and Unrath, M. (2022a). Can Nudges Increase Take-Up of the EITC? Evidence from Multiple Field Experiments. *American Economic Journal: Economic Policy*, 14(4):432–452.

Linos, E., Quan, L. T., and Kirkman, E. (2020). Nudging Early Reduces Administrative Burden: Three Field Experiments to Improve Code Enforcement. *Journal of Policy Analysis and Management*, 39(1):243–265.

Linos, E., Reddy, V., and Rothstein, J. (2022b). Demystifying college costs: How nudges can and can't help. *Behavioural Public Policy*, pages 1–22.

Maibom, J., Harmon, N., Glenny, A., and Fluchtmann, J. (2023). Unemployed Job Search across People and over Time: Evidence from Applied-for Jobs. *Journal of Labor Economics*, page 725165.

Miano, A. (2023). Search Costs, Outside Options, and On-the-Job Search.

Moffitt, R. (1983). An Economic Model of Welfare Stigma. *American Economic Review*, 73(5):1023.

Mueller, A. I., Spinnewijn, J., and Topa, G. (2021). Job Seekers' Perceptions and Employment Prospects: Heterogeneity, Duration Dependence, and Bias. *American Economic Review*, 111(1):324–363.

Mühlböck, M., Kalleitner, F., Steiber, N., and Kittel, B. (2022). Information, reflection, and successful job search: A labor market policy experiment. *Social Policy & Administration*, 56(1):48–72.

Muller, P., Kircher, P., Belot, M., and De Koning, B. (2023). Occupational advice for job seekers in the Netherlands.

OECD (1994). The OECD Jobs Study: Facts, Analysis, Strategies. Technical report, Organisation for Economic Co-operation and Development, Paris.

OECD (2018). Good Jobs for All in a Changing World of Work. The OECD Jobs Strategy. Technical report, Organisation for Economic Co-operation and Development, Paris.

OECD (2023). OECD Employment Database. Public expenditure and participant stocks on LMP. https://stats.oecd.org//Index.aspx?QueryId=123308.

OECD (2023). OECD Strictness of activation requirements database: Item 1. Availability requirements: ALMP participation. https://stats.oecd.org//Index.aspx?QueryId=125823.

Osman, A. and Speer, J. D. (2023). Stigma and take-up of labour market assistance: Evidence from two field experiments. *Economica*, n/a(n/a).

Sahin, A., Song, J., Topa, G., and Violante, G. L. (2014). Mismatch Unemployment. *The American Economic Review*, 104(11):3529–3564.

Schönherr, D. and Glaser, H. (2023). Zwischen Fördern und Fordern: Auswirkungen individueller Beratungsund Vermittlungsstrategien auf die Beschäftigungschancen arbeitsuchender Menschen. Technical report, SORA Institute for Social Research & Consulting., Wien.

Spinnewijn, J. (2015). Unemployed but Optimistic: Optimal Insurance Design with Biased Beliefs. *Journal of the European Economic Association*, 13(1):130–167.

Stantcheva, S. (2023). How to Run Surveys: A Guide to Creating Your Own Identifying Variation and Revealing the Invisible.

UN Special Rapporteur, D. S. O. (2022). Non-take-up of rights in the context of social protection. Report of the Special Rapporteur on extreme poverty and human rights, Olivier De Schutter. United Nations General Assembly. Human Rights Council, Fiftieth session. Technical report.

Vlandas, T. (2013). Mixing apples with oranges? Partisanship and active labour market policies in Europe. *Journal of European Social Policy*, 23(1):3–20.

Walker, R. (2014). *The Shame of Poverty*. Oxford University Press.

Weishaupt, J. T. (2011). *From the Manpower Revolution to the Activation Paradigm: Explaining Institutional Continuity and Change in an Integrating Europe*. Amsterdam University Press.

Wheeler, L., Garlick, R., Johnson, E., Shaw, P., and Gargano, M. (2022). LinkedIn(to) Job Opportunities: Experimental Evidence from Job Readiness Training. *American Economic Journal: Applied Economics*, 14(2):101–125.

Zweimüller, J. and Winter-Ebmer, R. (1996). Manpower Training Programmes and Employment Stability. *Economica*, 63(249):113–130.

Appendix

Table of Contents

A 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]

A.2 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.[15]

[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).

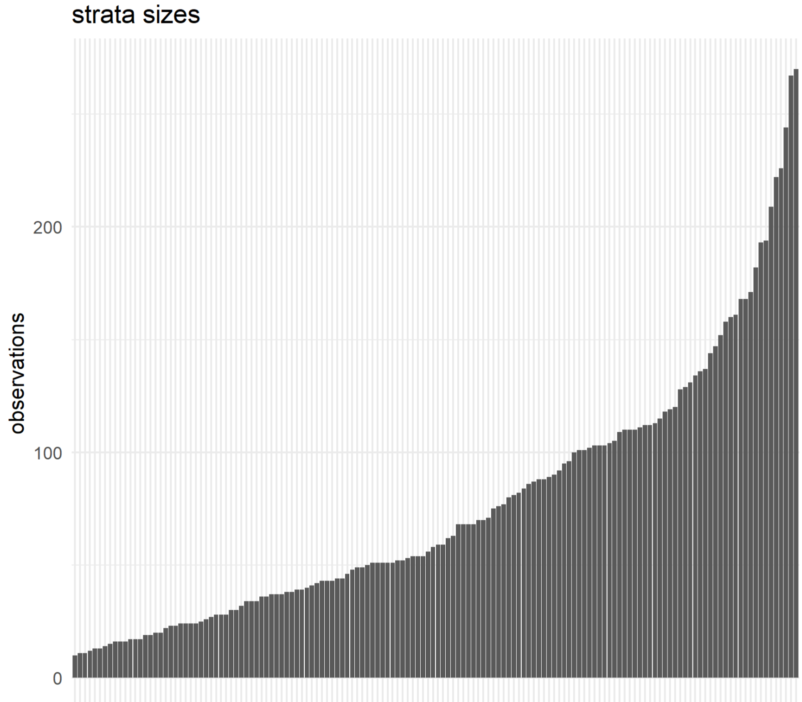
[15] The few experimental evaluations of ALMPs in Austria have focused on job-search assistance (Mühlböck et al., 2022; Böheim et al., 2022) and public employment programs (Kasy and Lehner, 2023).

A.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

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

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

© stock.adobe.com/grafikplusfoto

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

JETZT
#weiter bilden

GUTSCHEIN*

im Wert von bis zu **€ 15.000,-** für eine Investition in Ihre berufliche Zukunft!

JA, ich mache mit. Der Gutschein* hat einen Wert von bis zu € 15.000,-, wenn Sie eine Aus- oder Weiterbildung über das AMS machen. Ebenso können Sie sich am freien Bildungsmarkt selbst eine Aus- oder Weiterbildung aussuchen, die Ihre Chancen auf eine neue Beschäftigung erhöht. In diesem Fall hat der Gutschein* einen Wert von bis zu € 3.000,-.
In jedem Fall gilt: VORHER mit dem AMS Kontakt aufnehmen und die Förderbarkeit prüfen lassen!

Vorname	Nachname
E-Mail-Adresse	Telefonnummer
PLZ	Ort

Füllen Sie obenstehende Felder gleich online aus und übermitteln Sie uns das Formular, indem Sie auf den „Absenden"-Button klicken. Wir setzen uns dann so rasch wie möglich mit Ihnen in Verbindung. Gerne können Sie den Gutschein auch ausdrucken, ausfüllen und per E-Mail an **mailservice.selnoe@ams.at** schicken.

* Bitte beachten Sie, dass auf Förderungen kein Rechtsanspruch besteht. Dieser Gutschein kann bis 31.12.2021 eingelöst werden. Keine Barablöse möglich.

AMS
Arbeitsmarktservice Niederösterreich

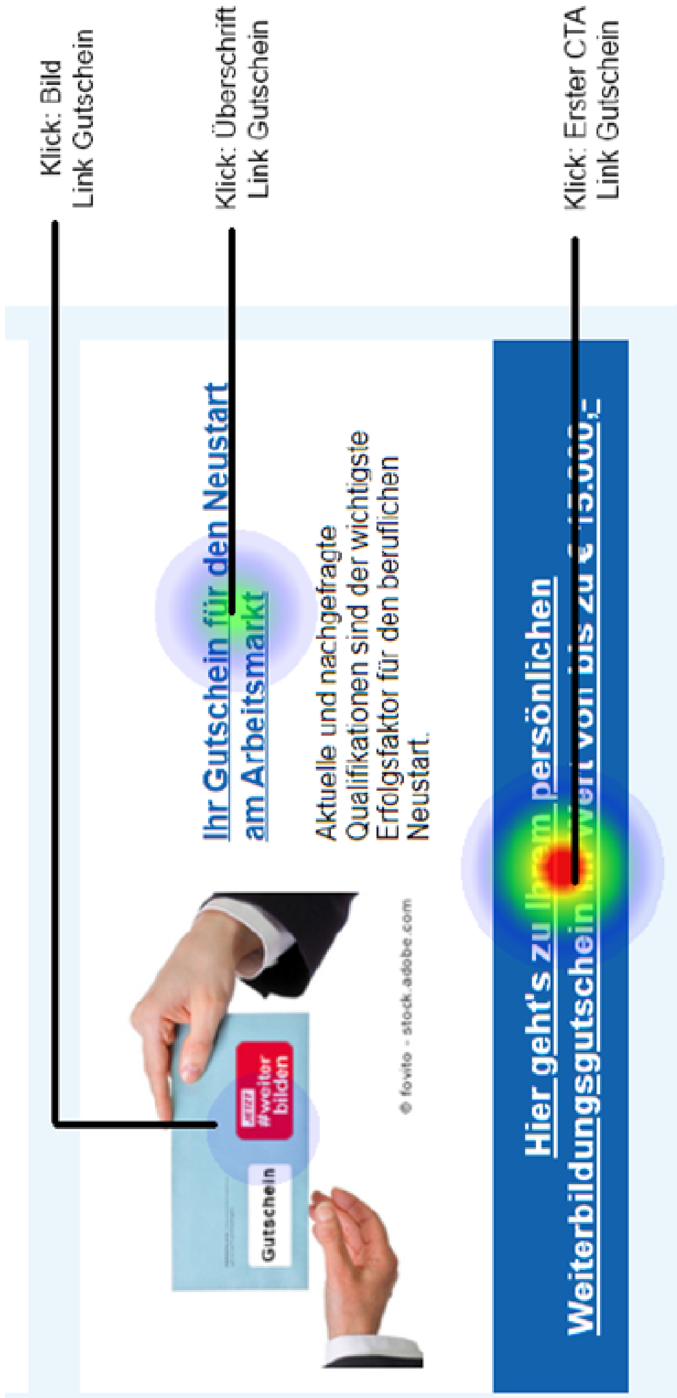
Figure A4: Occupations with the highest number of open vacancies for treatment group 3

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 Results

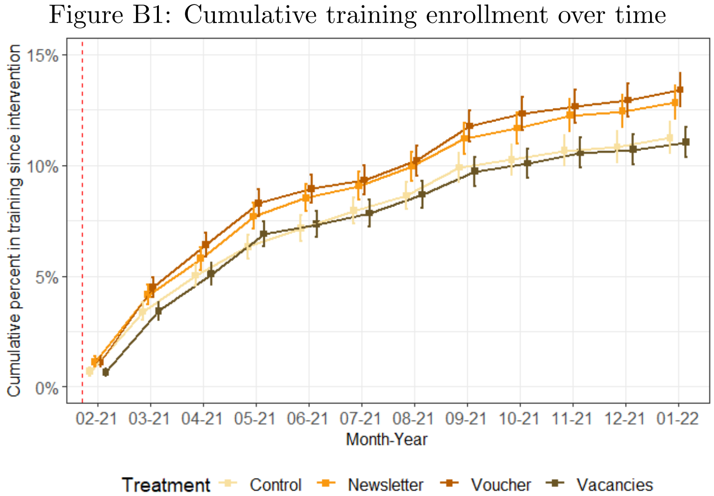
B.1 Training

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

Figure B1: Cumulative training enrollment over time



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

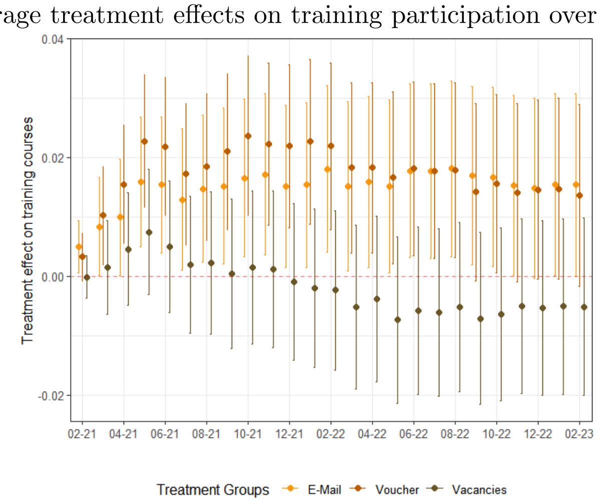
Table B2: Long term average treatment effects on active labor market programs

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

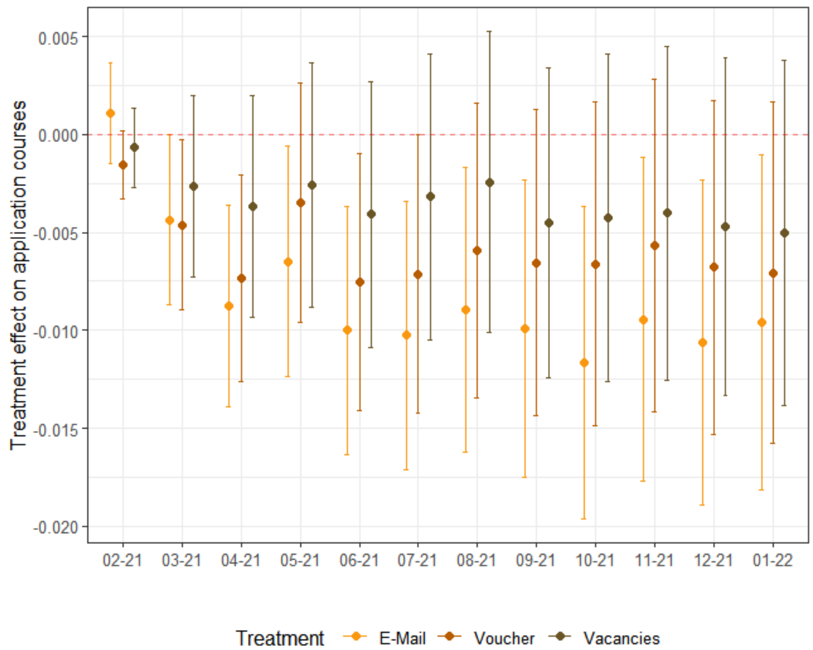
Note: Long term refers to 2 years after the intervention. Standard errors are in parenthesis: *p<0.1; **p<0.05; ***p<0.01

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



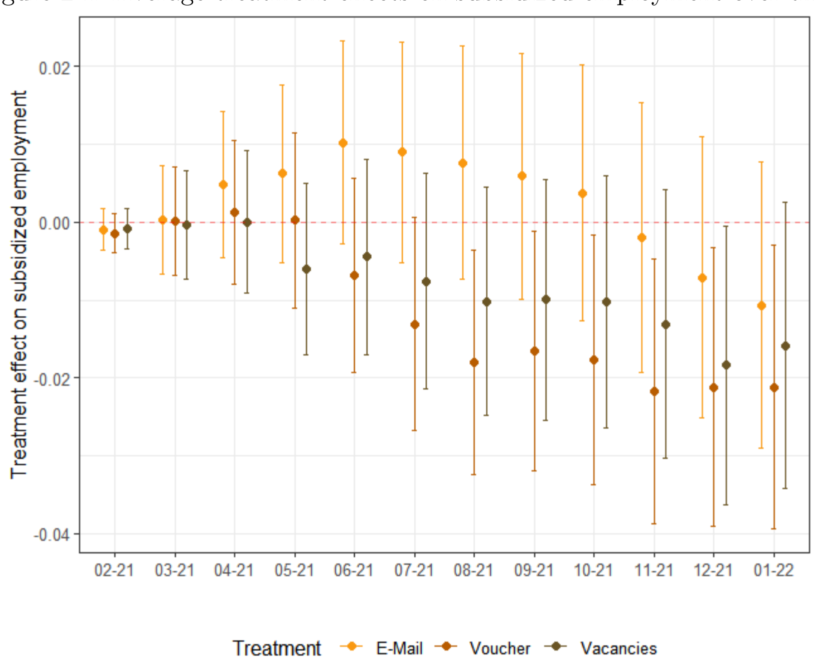
Note: Confidence intervals are reported at the 90%-level.

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.3 Employment

Table B6: 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 B7: 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 B8: 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

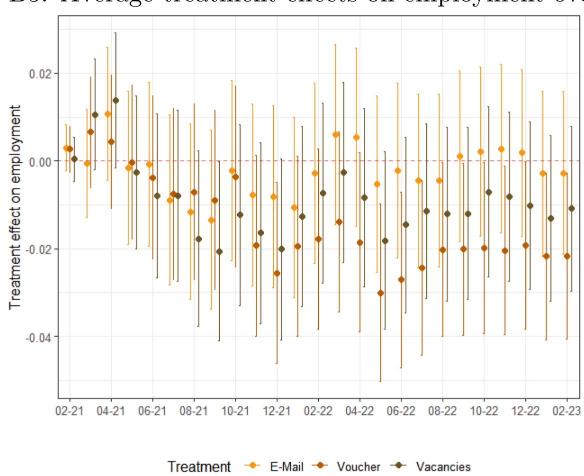
Note: *p<0.1; **p<0.05; ***p<0.01

Table B9: 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 B10: 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 B11: 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 B12: 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 B13: 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 B14: 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.	empl.	empl.	empl.	unempl.	unempl.
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 B15: 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 B16: Treatment effects on specific courses related to vacancy information

	Dependent variable:	
	Training	Training completion
	(1)	(2)
E-mail	0.004	0.002
	(0.003)	(0.003)
Voucher	0.005	0.005
	(0.003)	(0.003)
Vacancies	−0.001	0.0001
	(0.003)	(0.003)
Control Mean	0.014	0.01
Control SD	0.119	0.1
Observations	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.

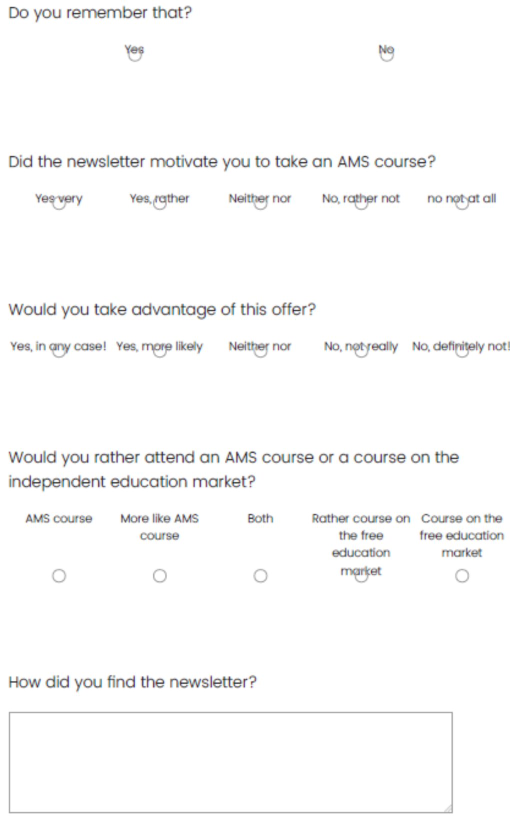
Finden Sie gemeinsam mit Ihrer AMS-Beraterin oder Ihrem Berater den für Sie richtigen Weg zurück ins Berufsleben und lösen Sie Ihren Weiterbildungsgutschein ein! In diesem Mail zeigen wir Ihnen, wie Ihr beruflicher Neustart gelingen kann.

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)	○	○	○	○	○