What Prevents Job Seekers from Training? Barriers, Caseworkers, or Efficient Sorting

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Abstract

We study barriers to job training enrollment in a randomized field experiment with 11,000 Austrian job seekers. Two information treatments increase training enrollment and completion by 18 to 27 percent. Effects are concentrated among women, low-income individuals, and those assigned to low-productivity caseworkers. Treated individuals are more likely to select more ambitious programs. We find no overall employment effects but an increase in employment conditional on training completion. The findings uncover information and psychological frictions and caseworker heterogeneity as key sources of inefficient sorting into training and demonstrate that low-cost interventions can improve effectively matching job seekers to training programs.

Keywords: unemployment, job training, program participation, caseworker, efficient sorting, field experiment
JEL codes: J64, J68, C93, D04, D83

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1 Introduction

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

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

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

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

Job training is widely available in modern Public Employment Services, yet participation among eligible job seekers remains low.[1] This paper asks why. We study training take up in a setting where eligibility is broad and direct monetary costs are essentially zero, but program entry is jointly determined by job seekers and caseworkers. In this environment, low participation may reflect efficient sorting, or it may reflect frictions that keep high return participants out of training (see Heckman and Smith 2004, UN Special Rapporteur 2022, Bergman et al. 2024).

We focus on two mechanisms that can generate such frictions: information and psychological barriers. Informational barriers include limited awareness of program options or eligibility requirements, as documented for benefits, services, and job search assistance (Altmann et al., 2018; Barr and Turner, 2018; Belot et al., 2019; Haaland et al., 2023). Psychological barriers capture stigma or low perceived program value that may discourage engagement even when information is available (Goffman, 1963; Moffitt, 1983; Bhargava and Manoli, 2015; Finkelstein and Notowidigdo, 2019; Goldin et al., 2022; Linos et al., 2022). Because program participation requires caseworker approval, these frictions operate within an institutional setting where counselors influence access and assignment. Using a large scale field experiment, we test whether low cost information interventions reduce these barriers, improve sorting into training programs, and affect subsequent employment outcomes.

Experimental Design We study this question using a multi-armed field experiment involving 11,000 unemployed job seekers in Austria. 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 in partnership with the Public Employment Service (PES; Arbeitsmarktservice, AMS) in Lower Austria, Austria's second largest state. The goal was to increase enrollment in training and, ultimately, improve employment of job seekers.

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[1]Governments spend an average of 0.5 percent of GDP on active labor market policies across OECD countries, with some European countries allocating as much as 2 percent (OECD, 2023). On average, less than 1 percent of the labor force participates in job training annually.

We randomly allocate the job seekers 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 additionally receives a training voucher redeemable through the PES up to €15,000; and the third, in addition to the e-mail and voucher, receives information on high vacancy occupations, defined as those with the largest number of open positions. The intervention consists only of the variation in the information provided with all options and obligations kept constant for individuals in all four groups. The treatments are stacked on top of each other 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 up to three years after the intervention as our main outcomes. We link those to our own survey data of participants' training intentions, beliefs, and experiences to uncover mechanisms for program participation.

Main findings   Our main empirical findings can be divided into two areas: training and employment. For average training outcomes, five sets of findings are noteworthy. First, raising awareness and signaling the monetary value have large positive effects on training enrollment (a 2–2.4 pp increase or 18%–21% relative to baseline). The increase is sustained over a three-year period.

Second, signaling the monetary value of job training, highly stigmatized programs, increases program completion beyond its increase in participation, indicating a positive effect on completion even among always-takers. The increase in program completion amounts to 2.6 pp or 27% compared to baseline, which compares to a 1.8 pp or 19% increase for raising awareness.

Third, 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. Increasing training also drives a substitution of enrollment in other ALMPs, in particular a reduction in enrollment in application courses, a typical activation measure.

Fourth, 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.

Fifth, not all information framing is beneficial: informing job seekers about high-vacancy occupations offsets positive treatment effects, as many job seekers, particularly those with better prospects, perceive these high-vacancy occupations as unattractive.

Turning to the labor market, we find no differences in employment outcomes between treatment and control groups overall. However, among those who complete training, treated individuals are 6 percentage points more likely to be employed and spend 50 additional days in employment over a three-year period (a 14 percent increase). We interpret this improvement conditional on training completion as an indication that job seekers were not perfectly sorted into training at baseline. Extending our analysis with a matching approach, we find stronger lock-in effects, likely as a result of the unusually rapid post-COVID-19 labor market recovery.

The absence of positive employment effects overall, thus, appears to be driven by timing and labor market context rather than by the intervention itself.

Mechanisms   We track job seekers' training intentions and their translation into actual enrollment. The treatments increase stated intentions to participate in training, and these intentions translate into higher enrollment rates. This effect appears to be driven by shifts in perceptions: treated individuals view training more favorably, suggesting that the interventions reduce both information and psychological frictions.

We also examine the role of caseworkers in matching job seekers to training programs. Treatment effects are concentrated among job seekers assigned to low-productivity caseworkers, who tend to exert greater control while offering less guidance on available opportunities. The interventions offset these informational gaps, increasing awareness and take-up of training.

Implications   The findings demonstrate the potential of information provision to reduce barriers to program participation, particularly for disadvantaged groups. Raising awareness (Treatment 1: e-mail) reduces information frictions, while framing participation through a voucher (Treatment 2: e-mail plus voucher) mitigates psychological barriers by raising perceived value. Both treatments increase training uptake. However, information provision can also backfire: presenting high-vacancy occupations (Treatment 3) reduces enrollment among job seekers who are overqualified, suggesting that learning about unattractive vacancies can dampen human capital investment.

Our experiment isolates three margins that are typically bundled. First, raising awareness about available training increases enrollment. Second, framing access through a voucher that signals program value increases completion over and above enrollment, consistent with psychological barriers that limit enrollment. Third, providing vacancy information can reduce training take up among job seekers with stronger outside options, highlighting that information can worsen program take up when it shifts attention toward unattractive jobs. These behavioral responses translate into economically meaningful changes in program composition: treated job seekers move into longer and certified courses, while participation in short activation measures falls. Together with survey evidence on intentions and caseworker responses, the results point to a unified mechanism: informational and psychological frictions interact with gatekeeping by caseworker to generate inefficient sorting into training, and a simple outreach can partially correct it.

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 aimed at reducing information and psychological frictions, as summarized by Haaland et al. (2023). Our study allows us to separate the interacted effects from addressing information frictions from a lack of awareness of training and psychological frictions associated with training programs (see Ben Dhia and Mbih

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(2023); Leduc and Tojerow (2025)). Barr and Turner (2018) used quasi-experimental variation to show for the U.S. that letters sent from the PES informing job seekers about benefits and costs of training increase training participation. By separating out the effects of distinct information components, our experiment contributes to the literature on job search information provision, which has typically examined the bundled effects of single, composite treatments (Altmann et al., 2018; Belot et al., 2019; Briscese et al., 2020; Wheeler et al., 2022; Ben Dhia et al., 2022; 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; Hemmeter et al., 2025). Psychological frictions such as social stigma, first theorized by Goffman (1963), are suggested as important reasons for non-take-up of benefits (Bursztyn and Jensen, 2017; Friedrichsen et al., 2018; Celhay et al., 2022). A number of field experiments study social benefit take-up in the U.S. They find that provision of information to raise awareness, related to our treatment 1, increases take-up (Bhargava and Manoli, 2015; Goldin et al., 2022), while framing interventions to overcome psychological frictions, as done in our treatment 2, do not add benefits (Bhargava and Manoli, 2015; Linos et al., 2022). Pure framing of messages, however, does matter in other contexts (Linos et al., 2020, 2024; Lasky-Fink and Linos, 2022; Osman and Speer, 2024). As in Hopkins and Dorion (2024) and Hemmeter et al. (2025), a basic information letter generated substantial relative increases in take-up, and behaviorally informed content further amplified engagement. We extend this evidence by showing that such interventions not only raise participation but also improve sorting into higher-return programs, particularly for individuals underserved by caseworker guidance.

A crucial difference in our setting is that most previously studied programs are entitlement-based, where participation depends primarily on eligible individuals choosing to apply. By contrast, participation in job training depends on the decisions of both job seekers and caseworkers (Zweimüller and Winter-Ebmer, 1996; Heckman and Smith, 2004). Despite the central role of caseworkers in nearly all high-income countries, the assignment mechanism they mediate remains poorly understood. Existing evidence, largely from Switzerland, suggests that caseworkers are no more effective at matching job seekers to programs than random assignment (Lechner and Smith, 2007). Qualitative evidence further suggests that the assignment mechanism has labor market consequences: voluntary participation is viewed favorably by employers, whereas mandatory assignment can carry stigma (Fossati et al., 2021). We contribute by opening this black box. While caseworker discretion has been studied in the context of job search requirements (Arni and Schiprowski, 2019), we are, to our knowledge, the first to provide experimental evidence on the role of caseworkers in training assignment. We trace the full sequence from job seekers' intention to train, through caseworker responses, to eventual training enrollment, completion, and employment—extending the descriptive insights of Heckman and Smith (2004) with causal evidence.

With regard to employment, our study contributes to the extensive literature on active labor

market policies by emphasizing the underexplored role of efficient sorting into training programs as a determinant of average employment outcomes. Prior work has found modest average effects of training on employment, as summarized in meta-analyses (Card et al., 2010, 2018) and in comprehensive reviews (Heckman et al., 1999; Kluve, 2010; Crépon and van den Berg, 2016). At the same time, substantial heterogeneity across program types and subgroups has been documented (Card et al., 2018; Katz et al., 2022; Humlum et al., 2023). We build on this insight by showing that part of this heterogeneity reflects differential access to high return programs driven by informational frictions and counseling structures. In related work, Chakravorty et al. (2024) demonstrate that providing detailed job information within a vocational training improves job retention through better selection into jobs. In contrast, we study sorting at an earlier margin, namely entry into and assignment across training programs in an unemployment system where access is mediated by caseworkers. Our results suggest that average employment effects may obscure important selection dynamics at the program entry stage, and that low participation among individuals with high predicted returns contributes to muted aggregate impacts.

Moreover, existing evidence on training programs in German-speaking countries relies exclusively on observational analyses; our study provides the first experimental evidence in this context.

Roadmap The rest of this paper is structured as follows. Section 2 provides an overview of job training in Austria and the context of the study. Section 3, building on our pre-analysis plan, details our data, experimental design, and analysis strategy. Section 4 presents our results, which include training, and employment. Section 5 investigates mechanisms behind the treatment effects, including training intentions, caseworker effects, and unintended consequences. 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 an additional matching analysis. Appendix D provides details on the survey including the questionnaire and survey results. Appendix E provides additional evidence on mechanisms.

2 Background

This section provides an overview of active labor market policies (ALMPs) and job training programs in Austria, their assignment and eligibility criteria, and contextual information on the timing of the intervention.

Active labor market policies 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 levelling the risk distribution between unemployed job seekers and providing employment op-

portunities for disadvantaged groups (Boeri and Ours, 2021; Lehner and Tamesberger, 2024). ALMPs can be divided into four main categories: job search assistance, training, employment subsidies, and public employment (Card et al., 2018). Job search assistance includes counseling and "application courses", which guide participants through job search strategies and CV preparation. Training programs aim to enhance human capital, facilitate re-employment, and promote occupational mobility. Employment subsidies cover hiring incentives and startup support for job seekers. Public employment typically targets the most disadvantaged, including the long-term unemployed and those with health conditions (Kasy and Lehner, 2023). Our intervention focuses on job training but allows observation of spillovers across ALMP types.

Job training programs in Austria Austria's job training programs are internationally recognized and serve as a model for other countries, with the Austrian PES ranking among the highest in training expenditures (Figure 1). 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., 2024). Common programs include mechatronics, plumbing, information and communications technology, programming, restaurant management, hotel and catering assistance, and nursing. Program duration varies from a few days to 1.5 years with longer programs offering high quality training for job specific skills. Among training program participants, about 40% graduate with a certificate after successfully passing an exam. Programs with an exam are typically more rigorous. During training enrollment, individuals continue to receive unemployment benefits, which is topped up with a small amount of € 4 per day for training programs which take at least 4 months to account for an increase in expenditures during training participation. In contrast, application courses focus on job search strategies to quickly reintegrate job seekers into employment and typically last only a few weeks.

The role of caseworkers Caseworkers are an archetypal example of street-level bureaucrats employed by the PES. They provide job search assistance and monitor job search effort, while administering benefits and deciding on program assignment. Job seekers are typically assigned to caseworkers based on availability at the time of unemployment benefit registration; a process that is effectively random. Each caseworker is responsible for around 200 to 250 job seekers. Job seekers meet their caseworkers regularly for consultations to discuss training opportunities, benefits, and job search.

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 benefit cuts. Unemployed workers can express interest in a large number of ALMPs, but caseworkers have the final say for program assignment. Unlike application courses to which caseworkers occasionally assign job seekers with the aim of "restoring work morale", assignment to training programs is intended to follow job seekers' interest. In practice, 87% of caseworkers in Austria report that they assign job seekers to training programs of their choice, while only 6%

Figure 1: Job training expenditure in % of GDP, Austria and EU countries 1997–2023
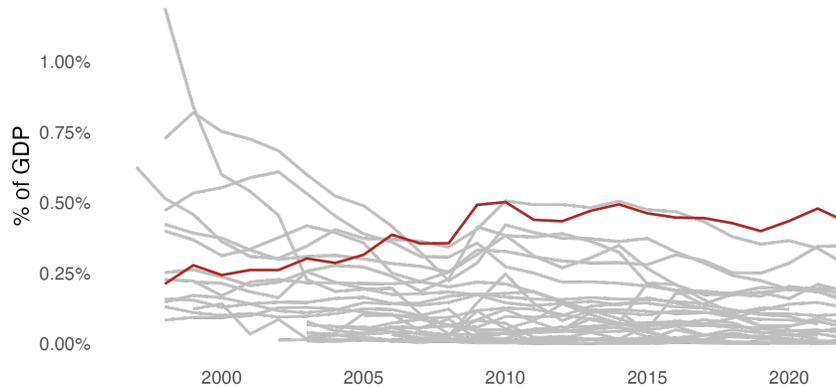
report "exercising pressure" when assigning training programs (Schönherr and Glaser, 2023). By contrast, application courses serve more frequently as a disciplining device, with 20% of caseworkers reporting they make assignments to "exercise pressure". Another motive for how caseworkers assign programs is "meeting their target", which is equally the case both for application courses and training programs.

COVID-19 pandemic Our intervention took place in February 2021 as part of a broader PES campaign *Corona Joboffensive* to promote job training programs. The intention was to prepare job seekers for the recovery phase post-lockdown, given the low likelihood of immediate re-employment during the lockdown period. This lockdown extended from November 2020 to May 2021, with temporary easing occurring between February and March 2021. The PES confirmed that training program places remained readily available, with no training supply constraints restricting participation. Training programs continued with the majority taking place in person with safety measures in place while some programs moved online. Training expenditure remained constant throughout the pandemic (Figure 1).

3 Study design

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

assignment are shown in Appendix A.

The study design was pre-registered (AEARCTR-0007141)[2] and approved by the Departmental Research Ethics Committee at the University of Oxford and by the Competence Center for Experimental Research at the Vienna University of Economics and Business.

3.1 Data, Outcomes, and Sample

Administrative records We leverage extensive administrative data on demographics, benefits, and employment histories from two key sources: (i) the PES internal registry for unemployed workers (AMS Data Warehouse) and (ii) the "occupational-career data" (Erwerbskarrierenmonitoring) from the AMS internal social security records. The AMS Data Warehouse provides detailed information on course participation, completion, and course types, along with sociodemographic details typically absent from Austrian social security records, such as education and occupation. The occupational-career data allow us to track individual employment trajectories and income from before the intervention until three years afterward. Our reliance on administrative records ensures virtually zero attrition.

Surveys Additionally, we survey participants and link the data with the administrative records at the individual level. We collect detailed data on training intentions, experiences and perceptions of job training, and interactions with caseworkers. The surveys are distributed via e-mail to all individuals in our sample approximately five weeks after the intervention. We send the e-mails as researchers, ensure respondents' anonymity, and communicate our independence from the PES. We achieve a response rate of 30%. We design the questionnaire using Qualtrics following Stantcheva (2023). The survey questions used are provided in Appendix Section D.2.[3] Likert scale responses are recoded as binary variables, equal to 1 if the respondent selected one of the top two categories (e.g., "very important" or "rather important"), and 0 otherwise.

Sample The sample of our first experiment includes the population of unemployed workers in Lower Austria with an unemployment spell of either 2–3 or 6–12 months at the time of treatment.[4] Unemployed job seekers who are already enrolled in a training program or who have a job offer accepted at the time of the intervention are excluded from the sample. The sample is further restricted to people who are at least 25 years old.[5]

This leaves us with 11,050 unemployed workers (Table 1 column (3)).[6] Among them, 52%

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

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

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

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

[6]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.

are women, 30% are younger than 35, and about 32% are older than 50. A third has no more educational attainment than compulsory schooling. Just over 1/5 has a foreign citizenship and an equally large share has a health restriction preventing them from working in certain occupations. With respect to language, 14.5% speaks only limited or no German.

Overall, our sample is very similar to the full population of job seekers across Austria (Table 1 column (4)), but Lower Austria's job seekers are more likely to have Austrian citizenship. We also compare our sample to the population of job seekers before the pandemic (Table 1 columns (1–2)). A high share of lay-offs took place at the start of the pandemic in March 2020, which explains the higher share of unemployed workers with a duration of 9–12 months in our sample. Among them, a higher share had minimum educational attainment and non-Austrian citizenship. A smaller share of job seekers in the sample had a health restriction compared to unemployed job seekers before the pandemic. With regard to gender and age, the composition remained broadly the same. In comparison, the sample of survey respondents are disproportionately women, older, and have higher educational attainment, while there are no differences in German language proficiency or health conditions (Table D1).

Table 1: Sample representativeness across time and states

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

Note: The table reports the size and characteristics of our sample as drawn in February 2021 (in bold) in Lower Austria, compared to samples from earlier years and to the full population of unemployed from Austria. The same selection criteria, described in the text, are applied to our sample and the comparison samples.

Outcomes of interest We categorize our outcomes of interest into two main groups: training and employment outcomes. In our main specifications, training outcomes are measured within 12 months after the intervention, whereas employment responses are expected to materialize later, and we thus measure them within 36 months after the intervention.[7] We report descriptive statistics for these outcomes in Table 2. We measure training by enrollment and completion of respective training programs. Our training outcomes in the upper part of the table are all binary and take the value of 1 if the unemployed participated in the specific type of ALMP within the 12 months following the intervention. In the lower panel of the table, we measure employment over the 36 months following the intervention, counting participation in job training as unemployed. Column 1 reports a binary indicator equal to 1 if the individual was employed at any point during this period. We also measure days in employment and unemployment, the average daily wage during employment spells, and construct a composite index of job quality. This index can take values between 0 and 1 and is an equally weighted combination of normalized wages and employment continuity, measured as days in employment. We test several alternative income and job quality definitions for robustness presented in Table B12.

Baseline data At baseline, 11.2% of job seekers enroll in a training program within 12 months after the intervention (column 1), while 9.4% complete these programs (column 2). The median program duration is 68 days, which we use as the threshold to classify long training programs (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 4.7% of job seekers participate in training programs that finish 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.[8] At baseline, 4.5% of job seekers participate in application courses (column 5), while 1 in 4 job seekers finds a job supported by employment subsidies (column 6) within 12 months of starting their unemployment spell.

Concerning employment outcomes in the lower part of Table 2, 79% of job seekers in our sample have been in employment for at least one day within 36 months after the intervention (column 1). During that period, a job seeker was on average 489 days in employment (column 2) and 427 days in unemployment (column 3). Once in employment, their average wage amounts to €50.6 gross per day (column 4). Job quality, measured as an index ranging from 0 to 1 that combines normalized wages and employment duration, averages 0.38 (column 5).

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[7]Additionally, we report training outcomes 24 and 36 months after the intervention and employment outcomes 12 and 24 months after the intervention in the appendix.

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

Table 2: Outcome variables descriptives for the control group

	Training outcomes (within 12 months after intervention)					
	Training	Training completion	Long training	Examined training	Application courses	Subsidized employment
Mean	0.112	0.094	0.055	0.047	0.045	0.257
SD	0.316	0.292	0.227	0.211	0.208	0.437
Range	0/1	0/1	0/1	0/1	0/1	0/1
Valid N.	2,769	2,769	2,769	2,769	2,769	2,769
	Employment outcomes (within 36 months after intervention)					
	Employment	Days in employment	Days in unemployment	Avg. daily wage	Job quality	
Mean	0.791	488.533	427.230	50.664	0.375	
SD	0.406	407.027	319.679	31.557	0.134	
Range	0/1	0-1,247	0-1,208	0.199-222.816	0.012-0.852	
Valid N.	2,769	2,769	2,769	2,105	2,032	

Note: The table reports means, standard deviations (SD), ranges, and valid observations (N) for the control group. Training outcomes in the upper panel are binary indicators equal to 1 if the unemployed participated in the specified ALMP within 12 months after the intervention. Employment outcomes in the lower panel are measured over 36 months, counting job training as unemployment. Column 1 shows a binary indicator equal to 1 if employed at any point during this period. We also report days in employment and unemployment, average daily wage during employment, and a composite job quality index. The index ranges from 0 to 1 and is an equally weighted average of normalized wages and days in continuous employment.

3.2   Experimental design

Treatment assignment   We assigned study participants to one of three treatment groups and one control group using stratified randomization. We used the following covariates, as available to the PES in February 2021 prior to treatment, to construct the strata: gender, age, educational attainment, region, and unemployment duration. 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 A1. The pre-analysis plan contains further details on the treatment assignment(AEARCTR-0007141, Lehner and Schwarz 2021). For comparison, survey respondents are also evenly distributed across groups, with balance on all covariates except educational attainment (Table D1).

Intervention   The intervention consists of e-mails sent by the PES with varying information on job training aimed at encouraging job seekers to participate in job training. Participants are not aware of the experiment as characteristic for a natural field experiment (Harrison and List, 2004). The treatments are stacked on top of each other, i.e., treatment group 2 receives the same e-mail as treatment 1 supplemented with a voucher; treatment group 3 receives the same e-mail

and voucher as in treatment groups 1 and 2, supplemented with information on high-vacancy occupations.

The stacked treatment design allows us to interpret the outcomes as interacted treatment effects. The control group is not contacted but continues to have access to training and regular PES consultations, as do the treatment groups. The intervention consists only of the variation in the information provided with all options and obligations kept constant for individuals in the four groups. The intervention was implemented in February 2021, with over 99% of the e-mails successfully delivered.

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

Treatment group 2 includes a voucher for job training programs added to the e-mail as shown in Figure A3.[9] Although training program enrollment is costless to job seekers irrespective of which treatment group they are assigned to, the voucher indicates a value of €15,000.[10] By signaling the monetary cost of the programs, the treatment is intended to convey the value of training. [11]. 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 vacancies in addition to the e-mail and voucher as shown in Figure A4. This information is intended to encourage job seekers for training in occupations with high labor demand and broaden their job search beyond their previous occupation. As job seekers are found to search in occupations with relatively few vacancies (Sahin et al., 2014), improving access to information has been shown to broaden job seekers' search (Belot et al., 2019). The same mechanism may also extend to training choice.

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 four groups. This provides us with an unbiased estimate of the treatment effect that does not hinge on any assumptions other than the random assignment into groups. The results for training can thus be interpreted as intention-to-treat (ITT) generalizable to the entire population of unemployed job seekers in our sample (Imbens and Angrist, 1994).

With the additional assumption that assignment to treatment only affects outcomes through

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

[10]The total value was chosen as an upper bound for training program costs, corresponding to the cost incurred by the PES for its most expensive training programs. The voucher also includes €3,000 for training programs offered by providers other than those contracted by the PES, but few job seekers took up this offer.

[11]Indeed, recipients report higher motivation for courses and tend to perceive them as more expensive (see Section 5.1)

opening the e-mail, the ITT estimates can be scaled up by the effect of assignment on the probability of opening. In this interpretation, opening the e-mail is the actual treatment, while being sent the e-mail is the instrument. The first stage, the effect of assignment on opening, is estimated at about 0.68. Hence, the corresponding instrumental variable estimates of the local average treatment effect (LATE) imply that treatment effects on training outcomes are roughly 47% larger than the ITT effects reported throughout the paper (i.e., the ITT effects divided by 0.68).

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

For the IV approach, we use the information intervention to instrument training. This gives us the LATE, which is representative for compliers, i.e., those job seekers at the margin of enrolling in training (Angrist et al., 1996). Our instrument is random by our experimental design. Our identification rests on the exclusion restriction: our instrument affects the dependent variable, employment outcomes, only through training enrollment. In other words, the information intervention itself does not affect employment.[12] Additionally, our instrument, the information intervention, is correlated with the endogenous variable, training. Our first stage regression model has an F statistic above 10, which is conventionally used as a threshold to qualify strong instruments. Nevertheless, our first stage is not particularly strong compared to other IV settings, which results in relatively imprecise IV effect estimates. We suggest interpreting the IV estimates with caution.

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:

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

where Y_i refers to the outcome variables for individual i. We use OLS models for continuous outcomes and logistic models for binary outcomes. 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

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[12]A potential concern for the IV approach is that the intervention may induce ex ante adjustments in job search prior to program entry, as documented in Black et al. (2003); van den Berg et al. (2009). In our setting, the information pertains to voluntary training opportunities rather than search assistance or activation measures, and we detect no employment responses before training starts.

we used stratified randomization, we include strata fixed effects s_i, following Athey and Imbens (2017). We additionally control for all socio-demographic variables as recorded before treatment $\mathbf{X_i}$ that were not used for stratification. This includes language skills, citizenship, occupation, marginal employment, previous wage, and within the past 10 years the days in employment and number of employment spells. Finally, we include caseworker fixed effects c_i. ε_i denotes the error term. For employment outcomes in our main specification, we maximize statistical power by pooling individuals in the treatment groups that increased training (treatment groups 1 and 2). The heterogeneity analysis is conducted via sub-group regressions of the equation above for the variables specified in the pre-analysis plan. We report heteroskedasticity-robust standard errors for linear models.

4 Results

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

Figures 2–4 and Tables 3–5 present our main results. Appendix B documents additional results for robustness using alternative estimation approaches and variable definitions, timing patterns, and heterogeneity.

4.1 Training

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

Main findings The e-mail and voucher treatments both lead to a significant increase in training enrollment. The increase is substantial in magnitude with 2–2.4 pp or 18%–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 2). The information on vacancies, by contrast, does not increase training. It is important to keep in mind that the information on vacancies is added to the e-mail and voucher as provided to treatment groups 1 and 2. We can interpret the null effect of treatment group 3, thus, as the vacancies information having a negative effect on aggregate training, which offsets the gains from treatment 1 and 2 in magnitude.

Figure 2: Training enrollment
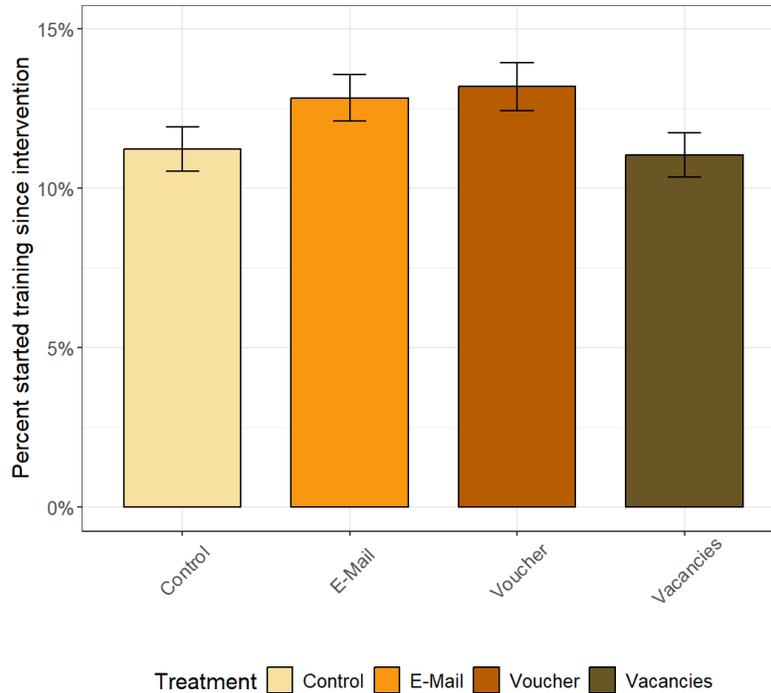
The e-mail and the voucher treatments both increase completion of training programs (column 2). While the increase for the e-mail is about the same magnitude as for participation (1.8 pp or 19%), the increase for the voucher (2.6 pp or 27%) exceeds the increase in enrollment and raises completion from 9.4% at baseline to 12% of eligible job seekers. The difference between the e-mail and the voucher is statistically significant, which indicates that the voucher increases completion even for job seekers who would have enrolled in a training program in the absence of the treatment. Table B1 shows that the increase in completion also holds for the different types of training programs.

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

completion of training programs, suggesting added benefits signaling monetary value.

Given that the vacancies information (treatment 3) does not affect training outcomes, one may wonder whether job seekers are more likely to enroll in training programs that prepare job seekers to work in those occupations presented in the vacancies information. Our analysis, however, does not support this claim (Table B2).

Table 3: Average treatment effects on active labor market programs

	Dependent variable:					
	Training enrollment	Training completion	Long training	Examined training	Application courses	Subsidized employment
	(1)	(2)	(3)	(4)	(5)	(6)
E-mail	0.020**	0.018**	0.013**	0.011*	−0.009*	−0.005
	(0.008)	(0.008)	(0.006)	(0.006)	(0.005)	(0.012)
Voucher	0.024***	0.026***	0.010*	0.008	−0.007	−0.019
	(0.008)	(0.008)	(0.006)	(0.006)	(0.005)	(0.012)
Vacancies	0.0005	0.006	−0.001	0.003	−0.005	−0.019
	(0.008)	(0.008)	(0.007)	(0.006)	(0.005)	(0.012)
Control Mean	0.112	0.094	0.055	0.047	0.045	0.257
Control SD	0.316	0.292	0.227	0.211	0.208	0.437
Observations	10,714	10,714	10,714	10,714	10,714	10,714

Note: The table reports average treatment effects on ALMP outcomes 12 months after the intervention. Long training (column 3) and examined training (column 4) are subsets of training (column 1) and measure enrollment. Application courses (column 5) and subsidized employment (column 6) are additional ALMP programs offered by the PES. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Standard errors are in parentheses: $^*p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Timing patterns To analyze timing patterns in the treatment effects of active labor market programs as suggested by Card et al. (2018), we investigate the temporal dimension of treatment effects on a monthly basis for 12 months following the treatment. Regarding outcomes, we consider whether job seekers have participated in a training program since the intervention took place. Figure 3 shows the treatment effect on training program enrollment per month.

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

training enrollment.

The treatment effects on training remain substantial for up to three years after the intervention (Table B3 and Figure B2). After two and three years, treated participants remain 11 to 16 percent more likely to have enrolled in and completed job training programs. The intervention's impact, thus, leads to a sustained increase in training enrollment and extends beyond merely prompting earlier training among job seekers.

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

Figure 3: Average treatment effects on monthly training enrollment by treatment arm over 13 months
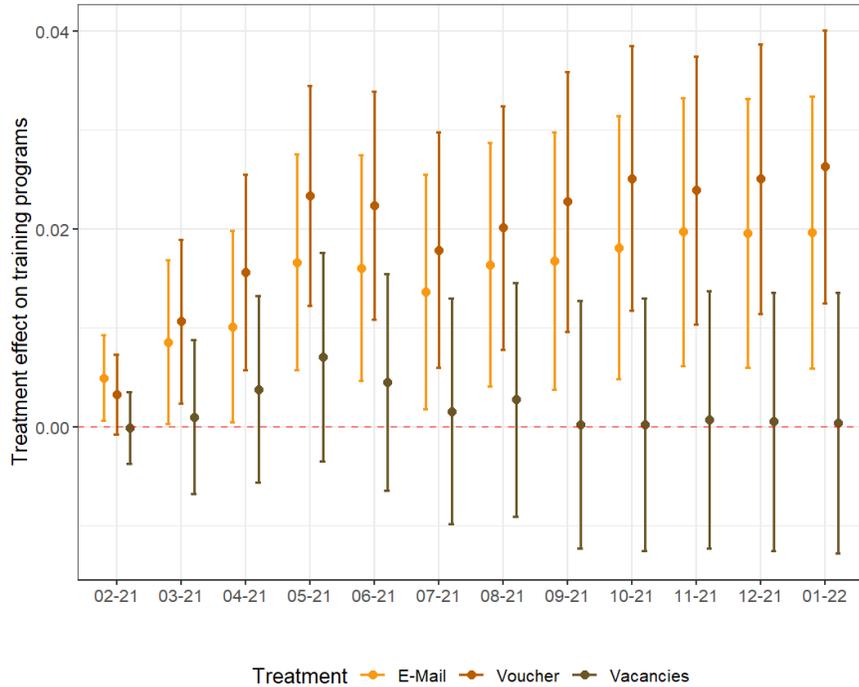
Note: The figure shows the average treatment effects on training enrollment in 12 monthly intervals for each of the three treatment arms. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Confidence intervals are reported at the 90% level.

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

The overall positive treatment effect is mostly driven by women and unemployed workers with

lower income in their previous job (Figure 4 and Table 4). Further, unemployed people older than 35, with Austrian citizenship, or white-collar occupations seem to contribute more to the effect (Table B4-Table B6). There are no clear patterns by education or language skills. Heterogeneous effects are similar between providing information (e-mail) and additionally signaling the monetary value (voucher).

Treatment 3 (e-mail + voucher + information) results in interesting diverging outcomes for different sub-groups (Table B6). Contrary to treatments 1 and 2, job seekers in blue-collar occupations react more positively than those in white-collar occupations. The same holds for low-skilled compared to high-skilled occupations. The estimates point in a negative direction for comparatively advantaged groups, such as men, higher income, and core age groups, albeit not significantly. In Section 5, we discuss the interpretation of these patterns.

Figure 4: Heterogeneity in average treatment effects on training enrollment by gender and income

19

Table 4: Heterogeneity in training enrollment by gender and income

	Women	Men	Below median income	Above median income
			Dependent variable:	
			Training take-up	
	(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
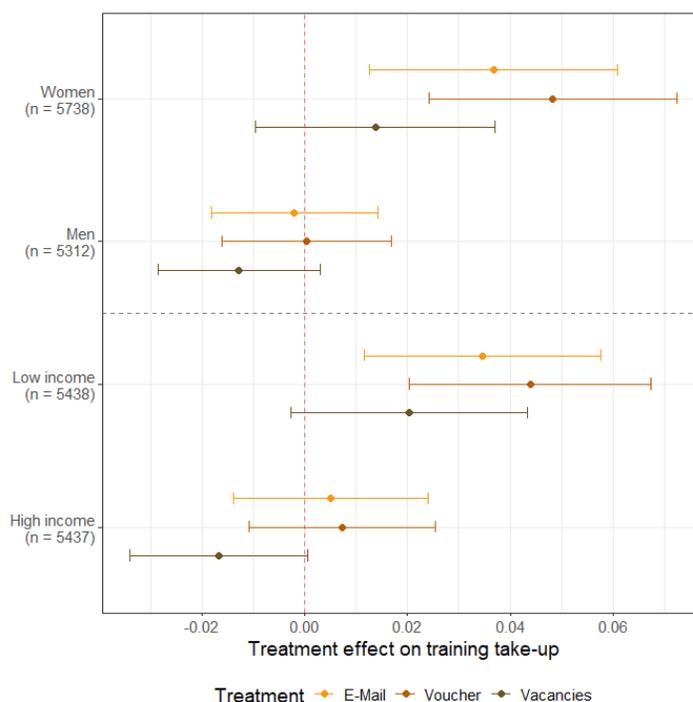
Note: The table shows average treatment effects on training enrollment by gender and by income above or below the median for each treatment arm, corresponding to Figure 4. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

4.2 Employment

To analyze treatment effects on employment, we first present baseline results on labor market outcomes and then discuss sub-group results. Table 5 presents the effects of the three treatment arms on employment 36 months post-intervention, with Panel A showing the main ITT effects and Panel B the effects conditional on training completion. To maximize statistical power in our main specification, we pool individuals in the treatment groups that increase training (treatment groups 1 and 2).

Main Findings We find no positive treatment effects when comparing all job seekers in the treatment and control groups using ITT or IV approaches (Table 5, Panel A). However, clear and consistent positive employment effects emerge when restricting the comparison to job seekers who completed training programs (Panel B).

The main effects (Panel A) are consistent for different time frames, specifications, and outcomes. Short-term effects one year after the intervention and medium-term effects after two years show the same pattern (Table B11). Instrumenting training program participation with the information intervention yields no statistically significant change in employment or unemployment (columns 2-4).[13] Neither wages nor job quality increases with training (columns 5 and

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

6). The findings are robust across alternative income definitions (Table B12), over time (Figure B5), when analyzing treatment groups separately (Table B13), and under our instrumental variable estimation strategy (Table B14). Though, the IV standard errors are substantial, reflecting the moderate first-stage strength and implying limited statistical power for detecting smaller employment effects.

Efficient sorting   Conditional on training completion (Panel B), the treatments increased the share of job seekers who found employment by approximately 6 percentage points (a 7% increase, column 1). Treated job seekers spent 50 more days in employment (a 14% increase) and 58 fewer days in unemployment (a 9% decline) (columns 3-4). We find no effects on wages or job quality (columns 5 and 6). The results are robust when estimating the effects separately for the e-mail and voucher treatment groups (Table B13).

While we cannot rule out selection on unobservables, the findings suggest that at baseline, sorting of job seekers into training programs was not efficient. Positive selection into program completion appears unlikely to explain the observed increase in employment conditional on completion, as the treatments primarily increased participation among women and low-income job seekers; disadvantaged groups in the labor market. Rather, the findings challenge the assumption of efficient sorting and underscore the potential for targeted interventions to improve efficient allocation of job training opportunities.

Matching extension   To assess whether training completion independently of the intervention affected employment outcomes, we implement nearest-neighbor propensity score matching for the control group (Appendix C). Using a comprehensive set of pre-intervention covariates, this procedure yields a matched sample of 245 treated individuals (i.e., those who participated in training) and 811 control individuals (i.e., those who did not), with strong covariate balance (Table C1).

The results reveal pronounced lock-in effects: training completion increases time spent in unemployment and reduces time in employment over a three-year follow-up period (Table C2). However, wages in subsequent employment rise. Compared to earlier studies applying the same methodology to evaluate PES training programs in Austria (Eppel et al., 2024), our findings indicate stronger lock-in effects, likely reflecting the rapid post-COVID-19 labor market recovery. This pattern suggests that the absence of positive employment effects stems from the timing of training within an unusually dynamic labor market rather than from the intervention itself.

Heterogeneity   We do not find significant heterogeneity in employment effects (Appendix B.4). Employment effects tend to be more negative for groups with the stronger increase in training, which suggests that lock-in effects drive the employment effects. This includes women (Table B15), those aged 35 to 50 (Table B16), those with Austrian citizenship (Table B17), and those who previously worked in medium-skilled occupations (Table B18). However, the heterogeneous effects are not statistically significant and thus have to be viewed with caution.

Table 5: Average treatment effects on employment

	Any employment		Days in employment	Days in unemployment	Avg. daily wage	Job quality
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Main effects						
E-mail + Voucher	−0.004		−6.262	−2.205	0.398	−0.002
	(0.009)		(8.847)	(7.193)	(0.807)	(0.003)
Training		0.008				
		(0.409)				
Control Group Mean	0.791	0.791	488.533	427.23	50.664	0.375
Control Group SD	0.406	0.406	407.027	319.679	31.557	0.134
Observations	10,714	10,714	10,714	10,714	8,212	7,909
Panel B: conditional on training completion						
E-mail + Voucher	0.059**		50.211**	−58.011***	−0.817	0.012
	(0.023)		(22.105)	(20.661)	(2.088)	(0.010)
Control Group Mean	0.800		356.277	637.448	52.226	0.304
Control Group SD	0.400		329.006	313.852	27.428	0.120
Observations	1,589		1,589	1,589	1,286	1,224

Note: The table reports average treatment effects on employment outcomes 36 months after the intervention. Treatment group 1 (e-mail) and treatment group 2 (voucher) are combined to maximize statistical power. The coefficient for treatment group 3 is small and non-significant for all outcomes and therefore omitted. Panel A shows results for the full sample; Panel B restricts to job seekers who completed a training program. Columns 1 and 3–6 in Panel A report intention to treat effects, while column 2 instruments training enrollment via assignment to treatment groups 1 and 2. Any employment (columns 1–2) is a binary indicator for employment at any time during the period. Days in employment (column 3) and unemployment (column 4) are measured as counts. Average daily wage (column 5) is the mean wage per day worked. Job quality (column 6) is an index from 0 to 1, defined as the equally weighted average of normalized daily wage and days in continuous employment. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

5 Mechanisms

We investigate mechanisms behind job seekers' training participation, the role of caseworkers, and the unintended consequences of the vacancies treatment.

5.1 Training intentions

First, we assess the treatments' effectiveness in shifting job seekers' intentions to train. We collect data on intentions with the survey detailed in Section 3 and Section D.2. We investigate whether the treatments affect job seekers' intentions, whether intentions translate into enrollment, and whether the treatments affect perceptions of job training. We do so to better understand the role information and psychological frictions play in preventing job seekers from participating in training.

Intentions   The treatments are successful in shifting job seekers' intentions to participate in training (Figure E1). Interest in courses offered by the PES increases after receiving the voucher. Plans to enroll in a program show signs of elevation for e-mail and voucher recipients but the effects are not statistically significant. By contrast, interest in and plans to enroll in a program appear to decline among recipients of the high-vacancy occupation treatment, though the differences are not statistically significant. Among treated individuals, job seekers who received the voucher and vacancies information reported stronger intentions to enroll compared to those who received only the e-mail. They were also more likely to recall the information provided and expressed greater motivation to participate in courses. Overall, these results demonstrate that the treatments are successful in shifting job seekers' stated preferences for training participation.

Intentions and enrollment   Intentions for training translate into program enrollment (Figure 5). Among job seekers who planned to take a course, 40%–50% eventually enroll in a program. By contrast, among those who did not plan to take a course, only 10%–20% eventually enroll.[14] Among treatment groups no relevant differences in the correlation of intentions and actual training enrollment are noticeable. Overall, job seekers' intentions are found to matter for training enrollment, which underscores job seekers' discretion in deciding whether to enroll in a program.

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[14]Note that 40% of program participants stated "being assigned to it" as a reason for enrolling (Figure D1).

Figure 5: Training enrollment by intentions

Perceptions The intervention tends to shift perceptions of job training reducing information and psychological frictions. In particular, the e-mail and voucher treatments raise awareness and signal the monetary value of training (Table E1). Recipients of the e-mail and voucher tend to report less often that they lack information on courses (column 5), which indicates the effectiveness of the treatment in raising awareness and informing job seekers about their training options. In parallel, recipients of the high-vacancy occupation information tend to report more often that they lack information, which could indicate that the information may have provided insufficient content to inform job seekers about their options. Job seekers who receive the voucher tend to report more often that courses are expensive (column 6), which indicates that the voucher is effective in signaling the monetary value of training programs. While the intervention seems to have shifted perceptions of job training in the way intended, the coefficients are not statistically significant, which may be related to the lower sample size among survey respondents.

5.2 Caseworkers

The intervention raises job seekers' interest and their intention to take up training. However, enrollment still requires caseworker approval, which can limit translation from intentions to par-

ticipation. Increased training intentions can, therefore, lead either to higher enrollment or to greater rejection of those intentions. While the importance of caseworker discretion has been documented in the context of job search requirements (Arni and Schiprowski, 2019), we are, to our knowledge, the first to provide quantitative evidence on their role in training assignment.

Assignment   The treatments increase job seekers' self-assessed autonomy over program assignment, but this also leads to more rejections of training intentions by caseworkers (Table 6). Recipients of any treatment feel more in control over which course to choose (column 1). However, treated job seekers report less often that caseworkers consider their wishes for training program assignment (column 2), which suggests increased disagreement between job seekers and caseworkers about course enrollment. Consequently, caseworkers more often reject job seekers' training intentions (column 3). These outcomes suggest that while job seekers feel some autonomy over program assignment, that autonomy is constrained by the required caseworker approval. While training intentions of some job seekers are turned down, others cannot find suitable courses despite increased interest (column 4). Moreover, treated job seekers tend to report less often that program enrollment was due to assignment by a caseworker (column 5). Uncovering these mechanisms not only clarifies how reducing informational and psychological frictions can shift agency toward job seekers, but also reveals the persistent gatekeeping role of caseworkers and institutional assignment rules.

Table 6: Average treatment effects on job seekers' experiences (survey outcomes)

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

Note: The table reports average treatment effects on survey outcomes measured about five weeks after treatment. Sample sizes differ across columns: all survey respondents (columns 1–2), respondents not enrolled in training (columns 3–4), and respondents enrolled in training (column 5). Column 1 indicates whether job seekers reported being able to decide for themselves whether and which PES course to attend. Column 2 indicates whether their wishes and interests were considered in counseling. Column 3 indicates whether the PES refused their preferred course. Column 4 indicates whether they could not find a suitable course. Column 5 indicates whether assignment by a caseworker was the reason for enrolling in training. All outcomes are measured on a five item Likert scale and transformed into binary indicators equal to 1 if the respondent selected one of the top two categories (e.g., "very important" or "rather important"), and 0 otherwise. Full survey questions are provided in Appendix Section D.2. Regressions control for baseline covariates and, where indicated, caseworker fixed effects. Subsample analyses exclude caseworker fixed effects due to small sample size. Standard errors in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Heterogeneity by caseworkers   To further investigate the role of caseworkers, we analyze the interaction with treatment effects by categorizing caseworkers as either high or low in productivity. We use the fitted values of caseworker fixed effects from a regression with employment duration as the dependent variable to measure the desired outcome of PES counseling.[15] We control for all baseline covariates and the treatment group. We then construct a dummy that takes the value 1 if the fixed effect of the job seeker's caseworker is higher than the median (high productivity); otherwise the value is 0 (low productivity). Finally, we re-estimate our main analysis separately for the two sub-groups.

The treatment effect on training enrollment is largely driven by job seekers assigned to low-productivity caseworkers (Figure 6). Notably, in the absence of the intervention, low- and high-productivity caseworkers do not significantly differ in the types of programs to which they assign job seekers (Table E3, Panel B). This result remains robust across alternative dependent variables in the fixed effects estimation, including training completion and unemployment duration (Table E2). Accordingly, we conclude that information interventions primarily affect job seekers

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[15] A longer employment duration after the period of unemployment indicates a good match.

counseled by low-productivity caseworkers. Moreover, positive employment effects conditional on training completion appear to be driven by low-productivity caseworkers, though these effects are not statistically significant (Tables E5-E6).

Why are treatment effects driven by low-productivity caseworkers? Low-productivity caseworkers tend to exert tighter control over their clients, disproportionately assigning them to programs with limited regard for individual preferences. Their clients also report larger information gaps regarding available training options (Table E4, Panel A). This pattern contradicts the notion that low-productivity caseworkers are simply more lenient. Importantly, these patterns are not caused by the intervention: similar differences appear in the control group (Table E4, Panel B). These findings suggest a key mechanism behind the intervention's effectiveness: it helps close information gaps for clients of low-productivity caseworkers, supporting more informed choices about training. By addressing these frictions, the intervention acts as a substitute for caseworker guidance when the latter is weak or absent. We return to the interpretation of caseworker fixed effects in Section 6.

Figure 6: Average treatment effect on training enrollment by caseworker type
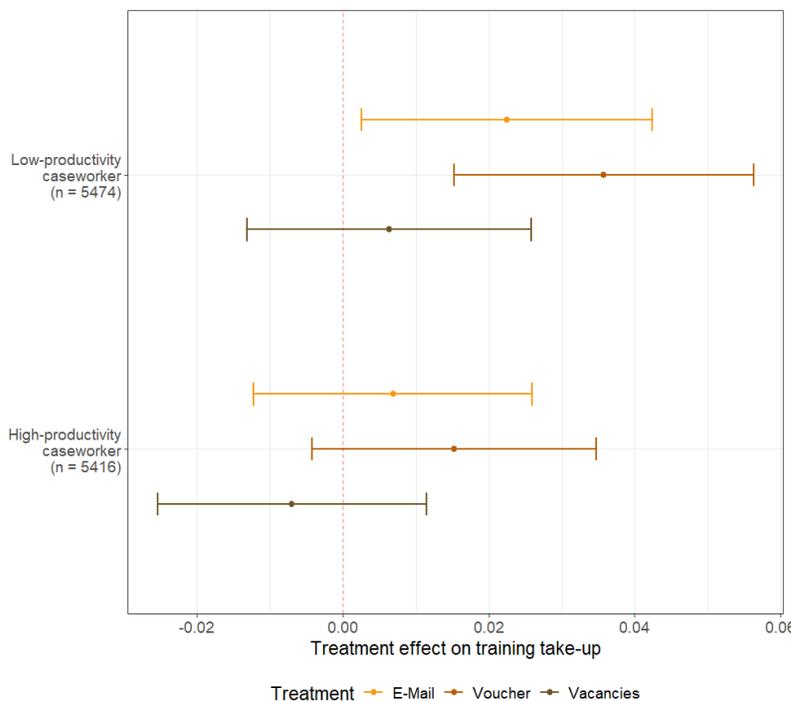
Note: The figure shows average treatment effects on training enrollment by treatment group, split by caseworker productivity. Productivity is measured using fixed effects from a regression of job seeker employment duration on caseworker fixed effects, controlling for our baseline covariates and treatment group. A dummy equals 1 if the caseworker's fixed effect is above the median (high productivity) and 0 otherwise (low productivity). Treatment effect estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Confidence intervals are reported at the 90% level.

5.3 Unintended consequences

Vacancies Sub-group analyses suggest that information on high-vacancy occupations may have discouraged job seekers who were overqualified for those roles from pursuing training. Our survey provides further suggestive evidence (Table E7). Among low educated survey participants, 55% find the information helpful and nearly 50% are willing to take a job in one of the included occupations. Among those educated above the minimum, only 35% find the information helpful and only 30% are willing to take a job in one of the presented occupations.

These patterns make it unlikely that the null effect of Treatment 3 reflects confusion from providing too much information at once. The information appears salient and relevant for low educated job seekers. At the same time, Table B2 shows no increase in enrollment or completion in programs linked to the highlighted occupations. Moreover, Treatment Group 3 exhibits no employment effects, making it implausible that reduced training enrollment is driven by direct employment take up in these occupations.

6 Discussion

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

6.1 Training

The findings appear remarkable in three aspects: their large magnitude given a one-off information intervention, the unintended consequences caused by the vacancies information, and the insights we provide into the job seeker-caseworker relationship. We further discuss spillovers, heterogeneity, and efficient sorting.

Magnitude An increase of 19%–20% from baseline is substantial for a one-off information intervention that consists only of an e-mail (treatment 1) but aligns with results from information interventions in other contexts. This includes an increase of up to 15% in compliance with municipal housing codes (Linos et al., 2020), an increase of up to 11% in student scholarships registrations (Linos et al., 2024), and an 11% increase in program applications for rental assistance (Lasky-Fink and Linos, 2022). Studies on social benefit take-up tend to find higher effects of letters including a 35-60% increase in social benefit applications (Bhargava and Manoli, 2015) and enrollments (Finkelstein and Notowidigdo, 2019). Hemmeter et al. (2025) even find an increase of 250% for Supplemental Security Income (SSI) take-up. In an observational study, Barr and Turner (2018) find that information letters increase college enrollment of U.S. job seekers by 40%, with treatment effects concentrated among vulnerable job seekers, similar to the patterns we observe in our study.

Unintended consequences   The negative effect of the high-vacancy occupation information (treatment 3) on training enrollment indicates the importance of targeting information to specific sub-groups. The purpose of the treatment was to provide additional information on the labor market to broaden job seekers' search and training choices towards occupations with high labor demand. Since most of the advertised jobs were in low-skilled occupations, job seekers with previous experience in high-skilled roles became discouraged from training, while those from low-skilled backgrounds saw an increase in training completion.

The role of caseworkers   Caseworkers play a crucial role in shaping job seekers' training enrollment, yet their influence remains somewhat opaque. Typically, job seekers express interest in training informally through repeated interactions with their caseworkers. As discussed in Section 2, 87% of Austrian caseworkers report assigning job seekers to training programs based on the job seekers' preferences (Schönherr and Glaser, 2023). This practice may explain why increased intentions to participate in training translated into actual enrollment.

However, our intervention altered this dynamic. Treated job seekers reported more frequent discussions about training with their caseworkers but also experienced a higher rate of rejection when requesting enrollment. This suggests that caseworkers act as gatekeepers, potentially constraining the effect of increasing job seekers' intentions on enrollment.

Additionally, we find that information gaps are more pronounced among job seekers assigned to low-productivity caseworkers in the control group. With the treatment, however, this disparity diminishes, indicating that the intervention serves as a substitute for high-quality caseworker counseling. This finding highlights the importance of both structured information provision and the quality of caseworker guidance in shaping job training.

Spillovers   We show that encouraging job seekers to engage in training shifts their participation away from other ALMPs, including application courses and subsidized employment. The shift is likely driven by job seekers' underlying preferences as documented in our survey. Application courses are more frequently perceived as a disciplining measure while training programs, in particular longer ones, usually involve an active choice of job seekers. Indeed, our findings are consistent with studies that have found stigma effects to be more severe for application courses and subsidized employment than for job training (Baert, 2016; Van Belle et al., 2019; Kübler et al., 2019; Gatta, 2023), especially when assignment is mandatory (Liechti et al., 2017). Our intervention helps align job seekers' preferences with training behavior by reducing frictions in the assignment process.

Heterogeneity   Disadvantaged groups, in particular women and job seekers with lower wages in their previous job, drive the aggregate increase in training. This pattern resembles other information interventions (Heffetz et al., 2022; Lasky-Fink and Linos, 2022; Barbanchon Le et al., 2023; Leduc and Tojerow, 2025) and reveals an interesting finding with regard to other studies on widening access to educational programs: Such studies typically identify a Matthew Effect,

originally established in the context of higher education, where expanding access disproportionately benefits those already likely to enroll, thereby widening inequalities. Our study follows this pattern, however, from a different starting point. Women and job seekers on lower income, who enroll disproportionally in job training, increase their enrollment disproportionally. Yet, by contrast to settings that have documented the Matthew Effect, these groups are disadvantaged. As such, their increased enrollment has the potential to reduce labor market inequalities. Indeed, studies have found women and lower income job seekers to benefit disproportionally from job training (Zweimüller and Winter-Ebmer, 1996; Card et al., 2018). Further, our finding might stem from an "access bias" that emerges through targeting particular groups for training programs (Bonoli and Liechti, 2018). For instance, a subset of programs are specifically aimed at unemployed women re-entering the workforce post-childbirth. A contextual factor may have contributed as well: Women experienced a sharper increase in unemployment than men during the pandemic (Leitner and Tverdostup, 2023).

Efficient sorting Our results show that lowering information and psychological barriers improves sorting of job seekers into training, particularly into longer, certified programs that are associated with stronger labor market returns. This underscores how even minor frictions can distort participation decisions and generate inefficient allocation. In this respect, our findings mirror the logic in Shepard and Wagner (2025), who show that increasing enrollment costs can raise adverse selection into public programs. We show the converse: reducing frictions can draw in high-return participants who would otherwise be excluded, especially those underserved by caseworker guidance. These findings highlight the importance of enrollment design in shaping both take-up and program effectiveness.

6.2   Employment

Lock-in effects and timing of training Job training programs can temporarily divert job seekers' time and attention from job search, thereby extending unemployment spells. These so-called "lock-in effects" are well-documented in the literature (Lechner and Wunsch, 2009; Lechner et al., 2011). Consistent with this, we find small, marginally significant negative employment effects 10 to 24 months after treatment, which dissipate over time (Figure B5).

While the intervention was designed to encourage training during the COVID-19 recession, when job vacancies were limited, a strong labor market recovery followed shortly after (Figure 7). As a result, training enrollment spiked in spring 2021 (Figure 3), coinciding with a sharp decline in unemployment and a doubling of job vacancies. It is plausible that participants prioritized training over job search during this period, missing employment opportunities in a rapidly improving labor market. This highlights the importance of aligning training with the economic cycle: focusing on skill development during downturns and prioritizing job search during recoveries (Lechner and Wunsch, 2009; Huemer et al., 2021).

Figure 7: Number of unemployed and posted vacancies in Lower Austria

7   Conclusion

Public employment services across high-income countries struggle to attract unemployed workers to voluntarily enroll in job training. Many job seekers are hesitant due to barriers from information and psychological frictions. Our multi-armed field experiment at scale demonstrates the benefits of raising awareness and signaling the monetary value. Raising awareness to reduce information frictions increases program enrollment and completion by 18–19%. Signaling the monetary value of job training increases training enrollment by 21% and completion even further by 27%. The effects are sizable and concentrated among women and low-income job seekers. They are also driven by job seekers counseled by low-productivity caseworkers, who tend to exert greater control while providing less information about training opportunities. We find no overall employment effects, which appears to reflect strong lock-in effects during the rapid post-COVID-19 labor market recovery. This suggests that the absence of gains is driven by timing and context rather than by the intervention itself. We do, however, find positive employment effects conditional on training completion, driven by the disproportionate uptake of more ambitious programs. This highlights the importance of effectively matching job seekers to high quality training opportunities. Overall, our findings suggest that information interventions can reduce barriers to program participation, increase efficient sorting into training, and thereby help job seekers get back into employment.

Implications   Our study advances understanding of the barriers that limit take-up of social programs. Disadvantaged individuals often lack awareness of available opportunities and face psychological frictions that inhibit participation. We provide evidence that a targeted information intervention can reduce such barriers and increase program take-up. This study also contributes to the understanding of active labor market policy by shedding light on the role of caseworkers. We show that the intervention is particularly effective for job seekers assigned to low-productivity caseworkers, which suggests that the intervention acts as a substitute for caseworker guidance when the latter is weak. The findings also point to inefficient sorting in the assignment process: job seekers with high potential to benefit from training are not consistently matched with appropriate programs. Improving information can help enhance efficient allocation of job training opportunities. Overall, this study demonstrates that providing targeted and relevant information can reduce participation barriers, improve the matching of workers to training opportunities, and, thereby, boost employment of job seekers.

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Online Appendix

Table of Contents

A Intervention 1: Design

A.1 Treatment assignment

Table A1: Covariate balance between treatment and control groups

	Control (1)	T1 (2)	T2 (3)	T3 (4)	Total (5)	p value (6)
Observations	2769	2766	2760	2755	11050	
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%)	
Citizenship						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%)	
Pre-unemployment income						0.182
Missing	38	50	41	46	175	
Lower than median	1403 (51.4%)	1351 (49.7%)	1371 (50.4%)	1313 (48.5%)	5438 (50.0%)	
Higher than median	1328 (48.6%)	1365 (50.3%)	1348 (49.6%)	1396 (51.5%)	5437 (50.0%)	

Note: The table reports the number of observations per category measured at the time of treatment assignment. Shares within each group are reported in brackets. Columns 1–4 show the control and three treatment arms. Column 5 reports totals for the full sample. Column 6 reports p values from joint tests of equality across the four control and treatment groups.

Figure A1: Strata sizes
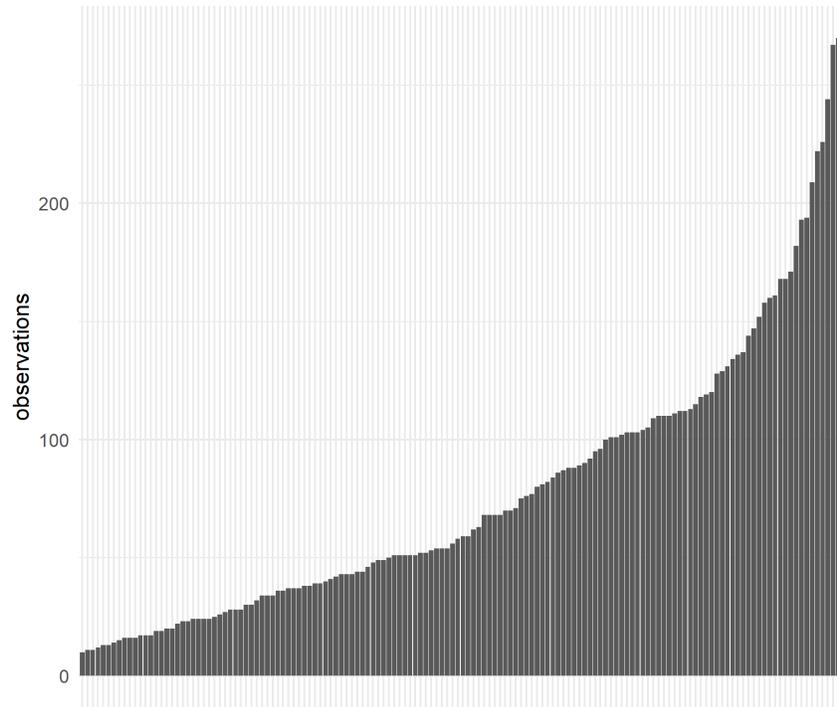
Note: The figure shows the 145 strata used for treatment assignment, defined by all possible combinations of five stratification variables: gender, age, education, region, and unemployment duration at the time of treatment assignment. Stratum sizes range from 10 to 270 individuals.

A.2   Treatment

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

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

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

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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

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:
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Aus- und Weiterbildung für den Neustart am Arbeitsmarkt

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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!

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.

E-mail text translated:

Your Path to a Fresh Career Start

Dear Madam or Sir,
Even in times of crisis, there are in-demand professions and future-oriented qualifications. The Corona Job Initiative offers you the opportunity to acquire new qualifications that will help you re-enter the workforce.
That is why I am personally inviting you: Take advantage of your opportunity for a fresh career start with training or further education! Together with your AMS advisor, find the right path back into professional life! In this e-mail, we will show you how your career restart can succeed.
Take your professional future into your own hands—and stay healthy!
Yours sincerely,
Sven Hergovich
Managing Director of AMS Lower Austria

Training and Further Education for a Fresh Start in the Job Market

Current and popular qualifications are the key success factor for a career restart.
Whether it is a refresher course for your professional skills or a training program leading to a formal qualification—AMS Lower Austria offers a wide range of training and further education opportunities.
Some examples:

- Metal and Electrical Engineering Professions

- Mechatronics

- Truck Drivers, Transport Sector

- Nursing Assistance / Advanced Nursing Assistance

Gain a competitive advantage in the job market and take advantage of our training and further education programs!

How We Finance You During Your Training

With AMS training support, you are financially secured during your education. The amount corresponds at least to your unemployment benefits or unemployment assistance and may be topped up under certain conditions.
Additionally, you will receive an educational bonus of € 4 per day if you receive unemployment benefits or unemployment assistance, your training lasts at least four months, and it starts within this year.

Precaution and Safety: Your Training During the COVID-19 Measures

The AMS takes the situation regarding the COVID-19 pandemic seriously. Therefore, together with our partner institutes, we continuously adjust course operations to the necessary COVID-19 protective measures.
To ensure that you remain healthy while still being able to start your training, the AMS follows the principle: **As much distance learning as possible—as much in-person instruction as necessary!**

Get informed now!

Find out more at 050 904 343
Would you like to learn more about your continuing education opportunities or need support in choosing the right training?
Our experts at the AMS Continuing Education Hotline are available to answer your questions by phone from Monday to Thursday between 07:30 and 16:00 and on Friday from 07:30 to 13:00 at the number **050 904 343**.
Or you can send an **e-mail**.

Figure A3: Voucher for treatment groups 2 and 3

Voucher text translated:

Now continue training

VOUCHER*

Worth up to € 15,000 for an investment in your professional future!

YES, I am in. The voucher* is worth up to € 15,000 if you participate in a training or further education program through AMS. You can also choose a training or further education program on the open education market that enhances your chances of finding new employment. In this case, the voucher* is worth up to € 3,000.

In any case, you must contact AMS IN ADVANCE to check your eligibility for funding!

First Name	Last Name
E-mail Address	Phone Number
ZIP Code	City

Fill out the fields above online and submit the form by clicking the "Submit" button. We will get in touch with you as soon as possible.

Alternatively, you can print out the voucher, complete it, and send it by e-mail to **mailservice.selnoe@ams.at**.

Please note that there is no legal entitlement to funding. This voucher can be redeemed until December 31, 2021. No cash redemption possible.

Figure A4: Occupations with the highest number of open vacancies (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**

Occupations text translated:

The Current Top Jobs in the Lower Austrian Labor Market
- **Electrical Installers and Fitters**

  Open positions registered with AMS NÖ in January: **343**
- **Registered Nurses**

  Open positions registered with AMS NÖ in January: **229**
- **Truck Drivers (all sectors)**

  Open positions registered with AMS NÖ in January: **228**
- **Bricklayers**

  Open positions registered with AMS NÖ in January: **170**
- **IT Technicians**

  Open positions registered with AMS NÖ in January: **159**
- **Pipe Installers and Fitters**

  Open positions registered with AMS NÖ in January: **157**
- **Hotel and Catering Professions**

  Open positions registered with AMS NÖ in January: **132**
- **Mechanical Engineers**

  Open positions registered with AMS NÖ in January: **117**
- **Nursing Assistants**

  Open positions registered with AMS NÖ in January: **110**
- **Medical and Technical Specialists (m/f)**

  Open positions registered with AMS NÖ in January: **81**

B   Intervention 1: Results

B.1   Training outcomes

Table B1: Average treatment effects on training completion

	Completion			
	Long training	Examined training	Application courses	External courses
	(1)	(2)	(3)	(4)
E-Mail	0.011*	0.010**	−0.009*	−0.002
	(0.006)	(0.005)	(0.005)	(0.004)
Voucher	0.014**	0.009*	−0.006	0.005
	(0.006)	(0.005)	(0.005)	(0.005)
Vacancies	0.002	0.004	−0.003	0.0001
	(0.006)	(0.005)	(0.005)	(0.005)
Control Mean	0.055	0.033	0.042	0.029
Control SD	0.227	0.177	0.2	0.169
Observations	10,714	10,714	10,714	10,714

Note: The table reports average treatment effects on program completion 12 months after treatment. Long training (column 1), examined training (column 2), and external courses (column 4) are subsets of training. Application courses (column 3) are additional ALMP programs offered by the PES. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Table B2: Treatment effects on training programs to prepare job seekers for vacancies in the vacancies information

	Dependent variable:	
	Training enrollment	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: The table reports average treatment effects on training outcomes 12 months after the intervention. Training enrollment (column 1) and training completion (column 2) refer to programs to prepare job seekers for vacancies presented in the vacancies information. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Table B3: Long-term average treatment effects on active labor market programs

	Training enrollment	Training completion	Long training	Examined training	Application courses	Subsidized employment
	Dependent variable:					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 2-year observation window						
E-mail	0.015*	0.011	0.011*	0.010	−0.013**	0.003
	(0.009)	(0.009)	(0.007)	(0.006)	(0.006)	(0.012)
Voucher	0.013	0.016*	0.010	0.004	−0.007	−0.012
	(0.009)	(0.009)	(0.007)	(0.006)	(0.006)	(0.012)
Vacancies	−0.005	−0.004	0.002	0.005	−0.006	−0.018
	(0.009)	(0.009)	(0.007)	(0.006)	(0.006)	(0.012)
Control Mean	0.149	0.13	0.069	0.061	0.062	0.319
Control SD	0.356	0.336	0.253	0.24	0.241	0.466
Observations	10,714	10,714	10,714	10,714	10,714	10,714
Panel B: 3-year observation window						
E-mail	0.015	0.012	0.006	0.012*	−0.014**	−0.011
	(0.010)	(0.009)	(0.007)	(0.006)	(0.006)	(0.013)
Voucher	0.014	0.015*	0.003	0.007	−0.008	−0.025**
	(0.010)	(0.009)	(0.007)	(0.006)	(0.006)	(0.013)
Vacancies	−0.007	−0.004	0.0001	0.008	−0.005	−0.027**
	(0.010)	(0.009)	(0.007)	(0.006)	(0.006)	(0.013)
Control Mean	0.171	0.148	0.083	0.062	0.063	0.343
Control SD	0.377	0.356	0.275	0.241	0.243	0.475
Observations	10,714	10,714	10,714	10,714	10,714	10,714

Note: The table reports average treatment effects on ALMP outcomes 24 months after treatment (Panel A) and 36 months after treatment (Panel B). Long training (column 3) and examined training (column 4) are subsets of training (column 1) and measure enrollment. Application courses (column 5) and subsidized employment (column 6) are additional ALMP programs offered by the PES. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Figure B1: Cumulative monthly training enrollment by treatment arm over two years
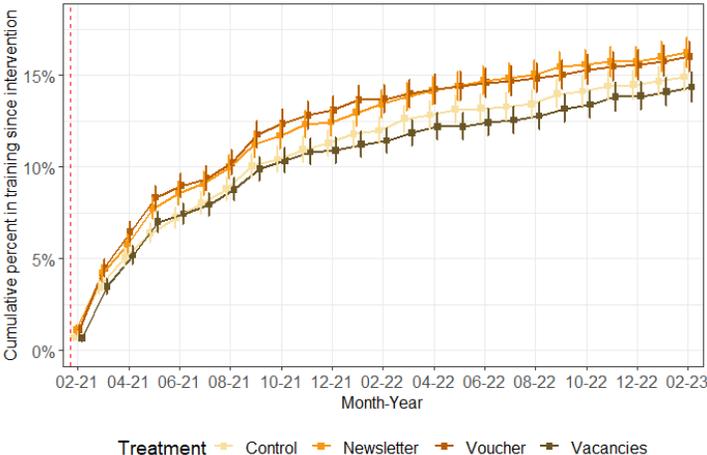
Note: The figure shows the cumulative share of job seekers enrolled in training by treatment arm, measured monthly over two years. Confidence intervals are reported at the 90% level.

Figure B2: Average treatment effects on monthly training enrollment by treatment arm over two years
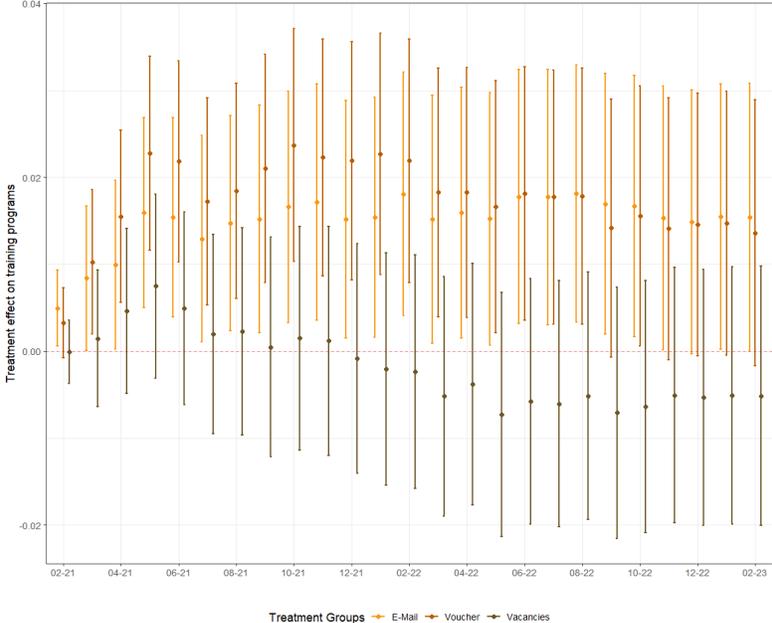
Note: The figure shows average treatment effects on training enrollment by treatment arm, estimated monthly over two years. Training enrollment is a binary variable. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Confidence intervals are reported at the 90% level.

Figure B3: Average treatment effects on monthly application course enrollment by treatment arm over two years
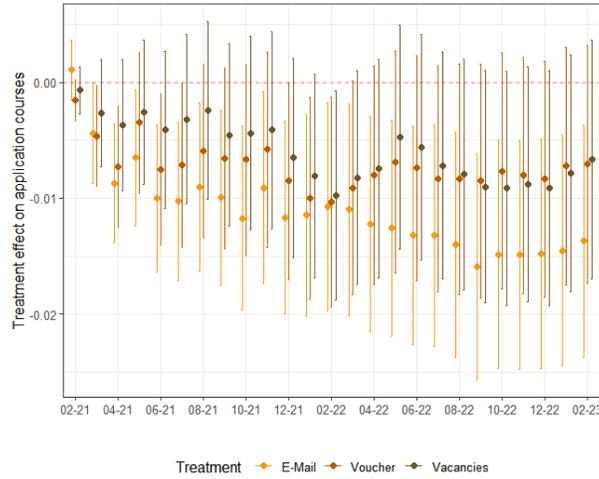
Note: The figure shows average treatment effects on application course enrollment by treatment arm, estimated monthly over two years. Application course enrollment is a binary variable. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Confidence intervals are reported at the 90% level.

Figure B4: Average treatment effects on monthly take-up in subsidized employment by treatment arm over two years
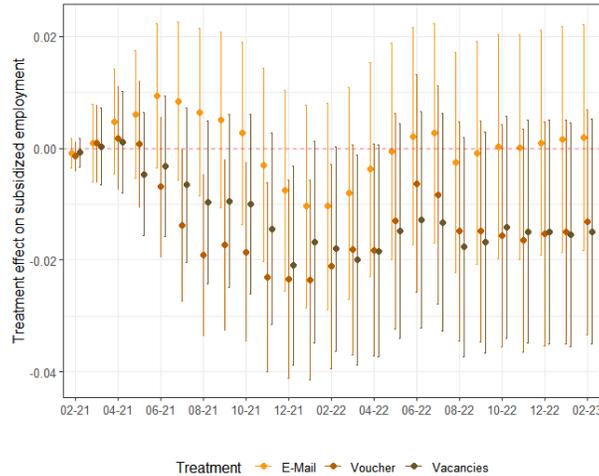
Note: The figure shows average treatment effects on take-up of subsidized employment by treatment arm, estimated monthly over two years. Subsidized employment is measured as a binary outcome equal to 1 if in subsidized employment at any time during the month. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Confidence intervals are reported at the 90% level.

B.2 Heterogeneity in training outcomes

B.2.1 Training enrollment

Table B4: Average treatment effects on training enrollment by age and education

	Dependent variable:				
	Training take-up				
	Below 35 years	35 to 50 years	Above 50 years	Compulsory education	Higher than compulsory education
	(1)	(2)	(3)	(4)	(5)
E-mail	0.003	0.028*	0.021*	0.013	0.022**
	(0.017)	(0.015)	(0.011)	(0.016)	(0.010)
Voucher	0.013	0.036**	0.021*	0.024	0.022**
	(0.017)	(0.015)	(0.011)	(0.016)	(0.010)
Vacancies	0.005	−0.013	0.010	0.018	−0.009
	(0.017)	(0.014)	(0.011)	(0.016)	(0.009)
Control Group Mean	0.153	0.132	0.05	0.144	0.097
Control Group SD	0.36	0.338	0.219	0.351	0.296
Observations	3,169	4,116	3,429	3,460	7,254

Note: The table reports average treatment effects on training enrollment 12 months after the intervention by age group and educational attainment. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Table B5: Average treatment effects on training enrollment by citizenship 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: The table reports average treatment effects on training enrollment 12 months after the intervention by citizenship and language. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Table B6: Average treatment effects on 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: The table reports average treatment effects on training enrollment 12 months after the intervention by occupation. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

B.2.2 Training completion

Table B7: Average treatment effects on training completion by gender and income

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

Note: The table reports average treatment effects on training completion 12 months after the intervention by gender and by income above or below the median. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Table B8: Average treatment effects on training completion by age and education

	Dependent variable:				
	Training completion				
	Below 35 years	35 to 50 years	Above 50 years	Compulsory education	Higher than compulsory education
	(1)	(2)	(3)	(4)	(5)
E-mail	0.014	0.017	0.019*	0.012	0.020**
	(0.016)	(0.013)	(0.011)	(0.015)	(0.009)
Voucher	0.017	0.039***	0.017	0.026*	0.024***
	(0.016)	(0.014)	(0.011)	(0.015)	(0.009)
Vacancies	0.010	−0.005	0.011	0.016	−0.001
	(0.015)	(0.013)	(0.010)	(0.015)	(0.009)
Control Group Mean	0.123	0.113	0.045	0.12	0.081
Control Group SD	0.328	0.317	0.206	0.326	0.273
Observations	3,169	4,116	3,429	3,460	7,254

Note: The table reports average treatment effects on training completion 12 months after the intervention by age group and education. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Table B9: Average treatment effects on training completion by citizenship and language

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

Note: The table reports average treatment effects on training completion 12 months after the intervention by citizenship and language. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Table B10: Average treatment effects on training completion by occupation

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

Note: The table reports average treatment effects on training completion 12 months after the intervention by occupation. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Table B11: Employment effects short-term

	Any employment		Days in employment	Days in unemployment	Avg. daily wage	Jobquality
	Dependent variable:					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 1-year observation window						
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
Panel B: 2-year observation window						
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: The table reports average treatment effects on employment outcomes 12 months after the intervention (Panel A) and 24 months after the intervention (Panel B). Columns 1 and 3–6 in Panel A report intention to treat effects, while column 2 instruments training enrollment via assignment to treatment group 1 and 2. Any employment (columns 1–2) is a binary indicator for employment at any time during the period. Days in employment (column 3) and unemployment (column 4) are measured as counts. Average daily wage (column 5) is the mean wage per day worked. Job quality (column 6) is an index from 0 to 1, defined as the equally weighted average of normalized daily wage and days in continuous employment. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Table B12: Alternative definitions of wages and job quality

	Dependent variable:			
	Daily wage in first job	Cumulative earnings	Higher than median avg. daily wage	Higher than median job quality
	(1)	(2)	(3)	(4)
E-mail + Voucher	−0.319	−778.617	0.019	−0.008
	(0.898)	(805.559)	(0.013)	(0.011)
Control Group Mean	59.181	34764.17	0.415	0.372
Control Group SD	34.979	36483.984	0.493	0.483
Observations	8,068	10,714	8,212	10,714

Note: The table reports average treatment effects 36 months after the intervention on alternative wage definitions. Column 1 refers to the average daily wage in the first job after unemployment. Column 2 refers to cumulative earnings over the period. Column 3 refers to a binary indicator for receiving an average daily wage above the sample's median. Column 4 refers to a binary indicator for job quality above the sample's median. Job quality is an index from 0 to 1 defined as the equally weighted average of normalized daily wage and days in continuous employment. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

B.3 Employment outcomes

Figure B5: Average treatment effects on monthly employment by treatment arm over 36 month

Note: The figure shows average treatment effects on employment by treatment arm, estimated monthly over 36 months. Employment is measured as a binary indicator equal to 1 if employed at any time during the month. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Confidence intervals are reported at the 90% level.

Table B13: Employment outcomes with separate treatment groups

	Any employment	Days in employment	Days in unemployment	Avg. daily wage	Jobquality
	Dependent variable:				
	(1)	(2)	(3)	(4)	(5)
Panel A: Main effects					
E-mail	−0.001	−2.141	−1.703	0.282	−0.002
	(0.010)	(10.221)	(8.284)	(0.934)	(0.004)
Voucher	−0.008	−10.382	−2.708	0.515	−0.002
	(0.010)	(10.286)	(8.320)	(0.937)	(0.004)
Vacancies	−0.007	−5.716	2.500	0.296	0.0004
	(0.010)	(10.323)	(8.387)	(0.939)	(0.004)
Control Group Mean	0.791	488.533	427.23	50.664	0.375
Control Group SD	0.406	407.027	319.679	31.557	0.134
Observations	10,714	10,714	10,714	8,212	7,909
Panel B: conditional on training completion					
E-mail	0.065**	52.357**	−60.799**	−0.927	0.005
	(0.026)	(25.168)	(23.949)	(2.369)	(0.011)
Voucher	0.052**	48.324*	−55.560**	−0.718	0.018
	(0.027)	(25.162)	(23.182)	(2.378)	(0.011)
Vacancies	0.023	17.659	−5.993	−1.356	0.010
	(0.028)	(26.800)	(24.891)	(2.508)	(0.011)
Control Group Mean	0.800	356.277	637.448	52.226	0.304
Control Group SD	0.400	329.006	313.852	27.428	0.120
Observations	1,589	1,589	1,589	1,286	1,224

Note: This table estimates the employment outcomes in Table 5 with separate indicators for each treatment group. Panel A shows results for the full sample; Panel B restricts to job seekers who completed a training program. Columns 1 and 3–6 in Panel A report intention to treat effects, while column 2 instruments training enrollment via assignment to treatment group 1 and 2. Any employment (columns 1–2) is a binary indicator for employment at any time during the period. Days in employment (column 3) and unemployment (column 4) are measured as counts. Average daily wage (column 5) is the mean wage per day worked. Job quality (column 6) is an index from 0 to 1, defined as the equally weighted average of normalized daily wage and days in continuous employment. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.
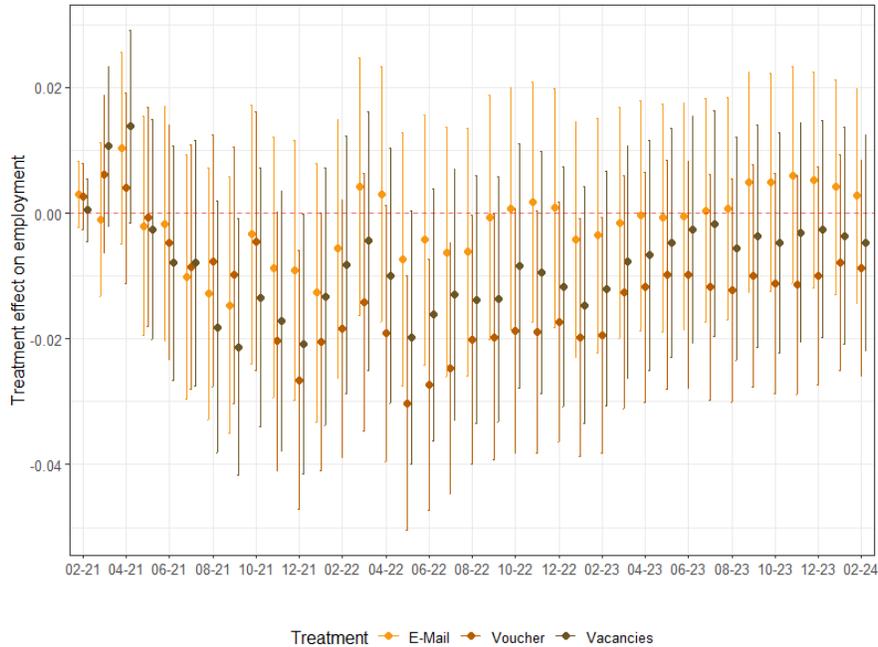
Table B14: Local average treatment effects on employment outcomes with instrumental variable approach

	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	−185.227	−187.522	10.274	−0.094
	(398.440)	(352.665)	(26.723)	(0.118)
Control Group Mean	488.533	427.23	50.664	0.375
Control Group SD	407.027	319.679	31.557	0.134
Observations	10,714	10,714	8,212	7,909

Note: *Note:* The table reports average treatment effects on employment outcomes 36 months after the intervention, using the instrumental variable approach. The outcomes correspond to those in Table 5 and follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

B.4 Heterogeneity in employment outcomes

Table B15: Average treatment effects on 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	−9.130	−4.761	−5.404	−3.306
	(12.594)	(12.690)	(12.516)	(13.114)
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: The table reports average treatment effects on days in employment 36 months after the intervention by gender and by income above or below the median. Treatment group 1 (e-mail) and treatment group 2 (voucher) are combined to maximize statistical power. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Table B16: Average treatment effects on employment by age and education

	Dependent variable:				
	Days in employment				
	Below 35 years	35 to 50 years	Above 50 years	Compulsory education	Higher than compulsory education
	(1)	(2)	(3)	(4)	(5)
E-Mail + Voucher	18.315	−19.422	5.346	−4.227	−6.699
	(16.154)	(14.541)	(15.726)	(15.684)	(10.913)
Control Group Mean	115.4	110.696	55.436	91.117	96.272
Control Group SD	123.211	120.787	92.293	112.4	118.168
Observations	3,169	4,116	3,429	3,460	7,254

Note: The table reports average treatment effects on days in employment 36 months after the intervention by age groups and educational attainment. Treatment group 1 (e-mail) and treatment group 2 (voucher) are combined to maximize statistical power. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Table B17: Average treatment effects on employment by citizenship and language

	Dependent variable:			
	Days in employment			
	Non-Austrian	Austrian	Non-German speaking	German speaking
	(1)	(2)	(3)	(4)
E-mail + Voucher	0.326	−10.934	44.382*	−14.601
	(20.592)	(9.995)	(26.486)	(9.549)
Control Group Mean	513.82	481.273	488.762	488.493
Control Group SD	390.291	411.626	385.433	410.68
Observations	2,270	8,444	1,460	9,254

Note: The table reports average treatment effects on days in employment 36 months after the intervention by citizenship and language. Treatment group 1 (e-mail) and treatment group 2 (voucher) are combined to maximize statistical power. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Table B18: Average treatment effects on 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	−6.522	−6.692	−1.622	−22.889*	14.688
	(15.213)	(11.278)	(21.204)	(12.241)	(18.942)
Control Group Mean	461.427	504.243	513.039	445.671	493.858
Control Group SD	388.107	415.756	422.667	376.908	408.505
Observations	3,775	6,939	2,132	5,694	2,888

Note: The table reports average treatment effects on days in employment 36 months after the intervention by occupation. Treatment group 1 (e-mail) and treatment group 2 (voucher) are combined to maximize statistical power. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

C   Matching analysis

To gain a better understanding of the downstream employment effects of the intervention, we estimate the causal impact of training completion among control group participants using a matching approach. The treatment variable is an indicator for completing any training within one year of the intervention start. Outcomes are defined as in Table 5, capturing our main employment outcomes in the three years after the intervention.

Method   We employ nearest-neighbor propensity score matching, using up to four control units per treated unit without replacement. The matching procedure relies on a rich set of covariates capturing demographic characteristics, labor market history, educational attainment, and region.[16] Variable selection closely follows Eppel et al. (2022). We successfully match 245 of the 257 control group participants who completed training within one year of the intervention to 811 out of 2251 control group participants who did not enter training.[17] Table C1 displays the covariate balance before and after matching, restricted to the variables shown in the main analysis balance table. Standardized mean differences between treated and control units fall below 0.1 for all matched covariates (difference in means divided by pooled standard deviation), consistent with established thresholds for adequate balance in matching approaches.

We then use the matched dataset to estimate the causal effect of training completion on our main employment outcomes. Because treated and control individuals are matched on observed characteristics, identification relies on the assumption that they are also comparable in unobserved characteristics. Under this assumption, causal effects can be recovered through mean comparisons. To ensure equal contribution of each matched set, control unit weights are normalized by the number of controls per treated unit. These weights are used in all regression models. We estimate models both with and without the standard control variables to assess robustness. As expected, the inclusion of controls does not affect the results (Table C2, Panel B).

Findings   The results indicate sizable lock-in effects of training over the three year follow up period (Table C2). Training completion reduces time spent in employment and increases time spent in unemployment, including training participation. While it does not significantly affect the probability of finding a job, it increases wages in subsequent employment. The job quality indicator shows a negative effect, driven by shorter employment durations. Compared to Eppel et al. (2022), who use a similar matching approach for the years 2013 to 2017 in Austria, we find substantially stronger lock-in effects. This likely reflects the timing of our intervention, which coincided with an unusually swift labor market recovery following the COVID-19 crisis, as discussed

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[16]More specifically, we include: gender, age, educational attainment (5 levels), health condition, duration of the current unemployment spell at the time of the intervention, German level, Austrian nationality, an indicator for marginal employment at the start of the intervention, regional employment office, previous occupation (8 levels), 1-digit NACE sector of the last job, level and type of unemployment benefits at the start of the intervention, time since last job, employment status at reference dates 3, 6, 12, and 24 months prior to the intervention, number of days spent in different employment statuses (employment, subsidized employment, training, out of the labor force) over the past 2, 5, and 10 years, and earnings in the previous job.

[17]Among matched treated units, 162 were matched to four controls, 25 to 3, 30 to 2, and 28 to 1.

in Section 6. The absence of positive employment effects should therefore not be attributed to the intervention itself, but rather to the economic context in which it was implemented. Under different labor market conditions, the same intervention may have produced clear employment gains.

Table C1: Covariate balance before and after matching

	Unmatched Control Group			Matched Control Group		
	No Training (1)	Training (2)	p value (3)	No Training (4)	Training (5)	p value (6)
Observations	2251	257		811	245	
Gender			< 0.001			0.551
Women	1115 (49.5%)	166 (64.6%)		506 (62.4%)	158 (64.5%)	
Men	1136 (50.5%)	91 (35.4%)		305 (37.6%)	87 (35.5%)	
Age group			< 0.001			0.565
Below 35 years	663 (29.5%)	103 (40.1%)		314 (38.7%)	98 (40.0%)	
35 - 50 years	837 (37.2%)	114 (44.4%)		340 (41.9%)	107 (43.7%)	
Over 50 years	751 (33.4%)	40 (15.6%)		157 (19.4%)	40 (16.3%)	
Education			< 0.001			0.198
Primary	693 (30.8%)	106 (41.2%)		291 (35.9%)	99 (40.4%)	
Higher than primary	1558 (69.2%)	151 (58.8%)		520 (64.1%)	146 (59.6%)	
Region			0.265			0.932
Industrieviertel	1015 (45.1%)	105 (40.9%)		337 (41.6%)	96 (39.2%)	
Mostviertel	595 (26.4%)	79 (30.7%)		247 (30.5%)	78 (31.8%)	
Waldviertel	193 (8.6%)	27 (10.5%)		83 (10.2%)	26 (10.6%)	
Weinviertel	448 (19.9%)	46 (17.9%)		144 (17.8%)	45 (18.4%)	
Unemp. dur.			0.739			0.607
3 - 4 Months	564 (25.1%)	61 (23.7%)		195 (24.0%)	60 (24.5%)	
6 - 9 Months	744 (33.1%)	91 (35.4%)		261 (32.2%)	86 (35.1%)	
9 - 12 Months	943 (41.9%)	105 (40.9%)		355 (43.8%)	99 (40.4%)	
Citizenship			< 0.001			0.236
Austria	1795 (79.7%)	166 (64.6%)		575 (70.9%)	164 (66.9%)	
Other	456 (20.3%)	91 (35.4%)		236 (29.1%)	81 (33.1%)	
Health			0.811			0.929
No health restriction	1790 (79.5%)	206 (80.2%)		650 (80.1%)	197 (80.4%)	
Health restriction	461 (20.5%)	51 (19.8%)		161 (19.9%)	48 (19.6%)	
Marg. empl.			0.770			0.909
No	1983 (88.1%)	228 (88.7%)		727 (89.6%)	219 (89.4%)	
Yes	268 (11.9%)	29 (11.3%)		84 (10.4%)	26 (10.6%)	
German			< 0.001			0.081
Partial or non	279 (12.4%)	72 (28.0%)		166 (20.5%)	63 (25.7%)	
Proficient or native	1972 (87.6%)	185 (72.0%)		645 (79.5%)	182 (74.3%)	
Pre-unemployment income			0.569			0.656
Lower than median	1114 (49.5%)	132 (51.4%)		417 (51.4%)	122 (49.8%)	
Higher than median	1137 (50.5%)	125 (48.6%)		394 (48.6%)	123 (50.2%)	

Note: The table reports covariate balance before and after nearest neighbor propensity score matching (with up to four controls per treated unit, without replacement). Matching variables are listed in Appendix Section C, footnote 14. Observations are measured at treatment assignment; group shares are shown in brackets. Columns 1–2 report unmatched non enrollees and enrollees in the control group, with p values in column 3. Columns 4–5 report the matched samples, with p values in column 6.

Table C2: Average treatment effects on employment based on matching

	Dependent variable:				
	Any employment	Days in employment	Days in unemployment	Avg. daily wage	Jobquality
	(1)	(2)	(3)	(4)	(5)
Panel A: Without controls					
Training completion	0.020	−83.497***	171.903***	5.760**	−0.040***
	(0.029)	(28.913)	(22.874)	(2.252)	(0.011)
Control Group Mean	0.82	529.276	391.411	46.461	0.383
Control Group SD	0.384	411.98	311.466	29.542	0.138
Observations	1,056	1,056	1,056	850	818
Panel B: With controls					
Training completion	0.010	−92.196***	185.062***	5.563**	−0.042***
	(0.028)	(28.337)	(22.539)	(2.196)	(0.011)
Control Group Mean	0.82	529.276	391.411	46.461	0.383
Control Group SD	0.384	411.98	311.466	29.542	0.138
Observations	1,043	1,043	1,043	841	809

Note: The table shows the average treatment effects on employment outcomes based on the comparison between the matched treatment and control groups 36 months after the intervention. Our matching approach employs nearest neighbor propensity score matching (with up to four controls per treated unit, without replacement). Matching variables are listed in Appendix Section C footnote 14. Estimates for Panel A are without controls. Estimates for Panel B include strata fixed effects and our baseline controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Any employment (column 1) is a binary indicator for employment at any time post treatment. Days in employment (column 2) and unemployment (column 3) are measured as counts. Average daily wage (column 4) is the mean wage per day worked. Job quality (column 5) is an index from 0 to 1, defined as the equally weighted average of normalized daily wage and days in continuous employment. Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

D Survey

D.1 Survey sample

Table D1: Survey Sample Comparison

	Full (1)	Survey (2)	p value (3)	Control (4)	E-Mail (5)	Voucher (6)	Vacancies (7)	p value (8)
Observations	11050	2180		533	555	555	537	
Gender			0.001					0.139
Women	51.9	56.2		55.0	53.7	55.9	60.3	
Men	48.1	43.8		45.0	46.3	44.1	39.7	
Age			0.001					0.295
Below 35	29.9	24.3		24.2	21.8	26.5	24.6	
35-50	38.5	38.3		38.6	40.9	34.1	39.5	
Above 50	31.5	37.5		37.1	37.3	39.5	35.9	
Education			0.001					0.035
Compulsory educ.	32.5	27.3		27.3	23.6	31.5	26.9	
Higher than comp.	67.5	72.7		72.7	76.4	68.5	73.1	
Citizenship			0.115					0.852
Non-Austrian	22.1	23.6		22.3	24.5	23.5	24.0	
Austrian	77.9	76.4		77.7	75.5	76.5	76.0	
Health			0.529					0.818
Health restriction	21.3	21.9		23.3	21.3	21.1	22.0	
No health restriction	78.7	78.1		76.7	78.7	78.9	78.0	
German			0.246					0.776
Partial or non	14.5	15.5		14.4	15.9	15.0	16.6	
Proficient or native	85.5	84.5		85.6	84.1	85.0	83.4	
Marginal Empl.			0.150					0.097
Yes	10.7	9.7		88.4	90.1	92.8	89.9	
No	89.3	90.3		11.6	9.9	7.2	10.1	
Unemployment			0.683					0.928
3-4 months	24.3	25.0		24.6	23.2	25.9	26.1	
6-9 months	33.9	33.0		32.6	33.9	32.1	33.3	
9-12 months	41.8	42.1		42.8	42.9	42.0	40.6	
Region			0.002					0.918
Industrieviertel	44.3	48.8		50.3	48.8	46.8	49.2	
Mostviertel	26.6	24.2		24.8	22.5	24.7	25.0	
Waldviertel	8.8	8.1		7.7	8.3	8.3	8.0	
Weinviertel	20.4	18.9		17.3	20.4	20.2	17.9	

Note: The table reports differences between the full sample and survey respondents (columns 1–3) and across survey respondents in the treatment arms (columns 4–8). Observations are shown as shares by characteristic measured at treatment assignment. Column 1 shows the full sample and column 2 the survey respondents; column 3 reports p values testing equality across columns 1 and 2. Columns 4–7 show survey respondents by treatment arm; column 8 reports p values from joint tests of equality across columns 4–7.

D.2 Survey questionnaire

This section includes the questions used to survey participants in the treatment and control groups for mechanisms. The questions are structured by order of exhibits. First-level numbered bullet points correspond to the questions. Second-level alphabetically listed bullet points correspond to the answer categories provided in the survey. Some questions are repeated where they appear in multiple exhibits. The full questionnaire for the survey was pre-registered and is available at https://www.socialscienceregistry.org/trials/7141.

Figure 5: Training enrollment by intentions

1. **Plan to do a course:** Are you planning to use the voucher to participate in a training course?

   (a) Yes, in any case!

   (b) Yes, more likely

   (c) Neither nor

   (d) No, not really

   (e) No, definitely not!

Table 6: Training program assignment

To what extent do the following statements apply to your experience with the AMS?

1. **Choose own courses:** I can decide for myself whether and which AMS course I want to attend.

2. **My wishes are considered:** I can bring my wishes and interests into the counseling sessions.

   (a) Very much true

   (b) Rather applies

   (c) Neither nor

   (d) Tends not to apply

   (e) Strongly disagree

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

3. **Course was turned down:** The AMS refused my preferred course.

4. **Could not find suitable course:** I haven't found a suitable course for me.

   (a) Very important

   (b) Rather important

   (c) Neither nor

   (d) Not that important

   (e) Not important at all

   How decisive were the following factors for you in your decision to attend a course?

5. **Assigned to course:** The course was assigned to me by my AMS caseworker.

   (a) Very important

    (b) Rather important

    (c) Neither nor

    (d) Not that important

    (e) Not important at all

Figure D1: Motivation for training enrollment

How decisive were the following factors for you in your decision to attend a course?

1. **Increase own employability:** To improve my re-employability.

    (a) Very important

    (b) Rather important

    (c) Neither nor

    (d) Not that important

    (e) Not important at all

2. **Professional reorientation:** Professional reorientation.

    (a) Very important

    (b) Rather important

    (c) Neither nor

    (d) Not that important

    (e) Not important at all

3. **Training within occupation:** Further training in my previous occupation.

    (a) Very important

    (b) Rather important

    (c) Neither nor

    (d) Not that important

    (e) Not important at all

4. **Bridge time until next job:** To bridge the time until the next job.

    (a) Very important

    (b) Rather important

    (c) Neither nor

    (d) Not that important

    (e) Not important at all

5. **Assigned to course:** The course was assigned to me by my AMS caseworker.

   (a) Very important

   (b) Rather important

   (c) Neither nor

   (d) Not that important

   (e) Not important at all

Figure E2: Average treatment effect on intentions to participate in training

1. **Recollection of treatment:** About two months ago you received the following newsletter from the AMS on further training: (please scroll down)
   Photo of e-mail
   Do you remember it?

   (a) Yes

   (b) No

2. **Motivation for courses:** Did the newsletter motivate you to take an AMS course?

   (a) Yes, very

   (b) Yes, rather

   (c) Neither nor

   (d) No, rather not

   (e) No, not at all

Table E2: Perceptions of courses

1. **Recollection of treatment:** About two months ago you received the following newsletter from the AMS on further training: (please scroll down)
   Photo of e-mail
   Do you remember it?

   (a) Yes

   (b) No

2. **Motivation for courses:** Did the newsletter motivate you to take an AMS course?

   (a) Yes, very

   (b) Yes, rather

(c) Neither nor

(d) No, rather not

(e) No, not at all

3. **Interest in courses:** Are you generally interested in AMS courses?

   (a) Yes, very

   (b) Yes, rather

   (c) Neither nor

   (d) No, rather not

   (e) No, not at all

4. **Plan to do a course:** Are you planning to use the voucher to participate in a training course?

   (a) Yes, in any case!

   (b) Yes, more likely

   (c) Neither nor

   (d) No, not really

   (e) No, definitely not!

   How decisive were the following factors for you in your decisionnot to attend a course?

5. **Lack of information:** I don't have enough information about the AMS courses.

   (a) Very important

   (b) Rather important

   (c) Neither nor

   (d) Not that important

   (e) Not important at all

   To what extent do the following statements apply to yourexperience with the AMS?

6. **Courses are expensive:** AMS courses are expensive for the AMS.

   (a) Very much true

   (b) Rather applies

   (c) Neither nor

   (d) Tends not to apply

   (e) Strongly disagree

Table E5: Survey responses on PES perception by caseworker

To what extent do the following statements apply to your experience with the AMS?

1. **My wishes are considered:** I can bring my wishes and interests into the counseling sessions.

2. **Apply for irrelevant jobs:** I have to apply for positions that do not match my interests and skills.

3. **More control than consultation:** I have the feeling that the AMS is more there to control me than to help me find a job.

4. **Choose own courses:** I can decide for myself whether and which AMS course I want to attend.

5. **Courses are expensive:** AMS courses are expensive for the AMS.

6. **Courses are high quality:** AMS courses are of high quality.

   (a) Very much true
   (b) Rather applies
   (c) Neither nor
   (d) Tends not to apply
   (e) Strongly disagree

   How decisive were the following factors for you in your decision to attend a course?

7. **Assigned to course:** The course was assigned to me by my AMS caseworker.

   (a) Very important
   (b) Rather important
   (c) Neither nor
   (d) Not that important
   (e) Not important at all

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

8. **Lack of information:** I don't have enough information about the AMS courses.

9. **Course was turned down:** The AMS refused my preferred course.

   (a) Very important
   (b) Rather important
   (c) Neither nor
   (d) Not that important
   (e) Not important at all

Table E8: Evaluation of vacancies information by respondent characteristics

1. **This information is important for me:** How important is this information for your personal return to work?

   (a) Very important

   (b) Rather important

   (c) Neither nor

   (d) Not that important

   (e) Not important at all

2. **Would be willing to work in one of these jobs:** I would be willing to work in one of the above areas.

   (a) Very much true

   (b) Rather applies

   (c) Neither nor

   (d) Tends not to apply

   (e) Strongly disagree

D.3   Descriptive survey results

Motivation to train   What motivates job seekers to engage in training? Desires such as increasing one's employability drive most job seekers enrollment while external constraints such as being assigned to a course drive a sizeable minority. 9 out of 10 job seekers enroll in training to increase their employability (Figure D1). 80% consider professional re-orientation as a motive while for 70% training within their occupation is important. About half of job seekers simply intend to bridge the time until their next job. Assignment by the caseworker as an external factor matters for around 40% of job seekers.

Figure D1: Motivation for training enrollment
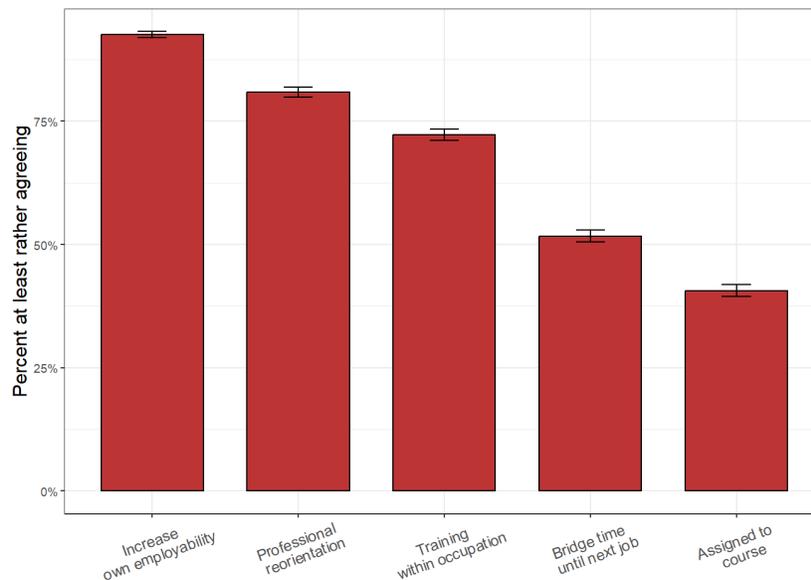
Training course assignment suffers from perverse incentives.

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

- *"All pointless mass processing so that some unemployed fall out of the statistics."*

- *"One should be listened to and not just thrown into a course to make the labor market statistics look better."*

Job seekers demand more autonomy.

- *"It would be nice if people's wishes and needs were taken into account."*

- *"Be more responsive to the needs of the unemployed to provide relevant training."*

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

E   Mechanisms

Figure E1: Average treatment effects on recollection of treatment and intentions to participate in training
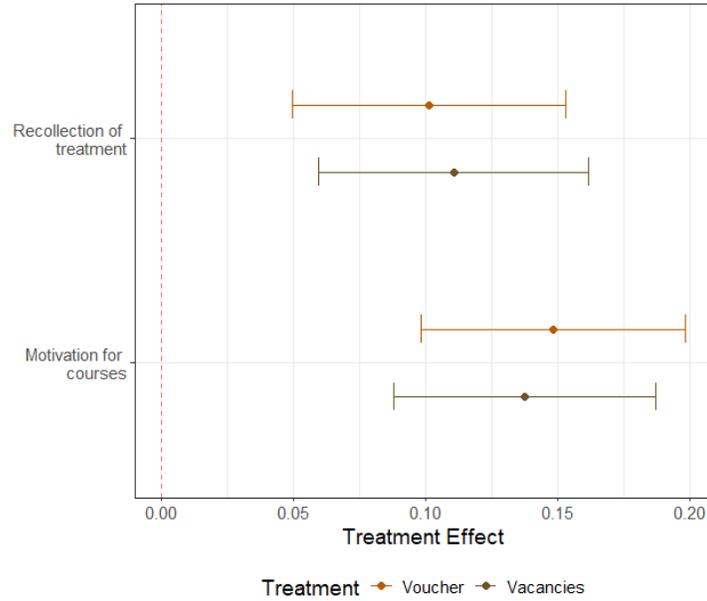
Note: The figure shows average treatment effects on recollection of the treatment and motivation to participate in training for the voucher and vacancies groups (T2 and T3) relative to the e-mail group (T1). Survey outcomes are measured about five weeks after treatment. Likert scale responses are recoded as binary variables, equal to 1 if the respondent selected one of the top two categories (e.g., "very important" or "rather important"), and 0 otherwise. Full survey questions are provided in Appendix Section D.2. Estimates include strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Confidence intervals are reported at the 90% level.

Table E1: Perceptions of courses

	Recollection of treatment (1)	Motivation for courses (2)	Interest in courses (3)	Plan to do a course (4)	Lack of information (5)	Courses are expensive (6)
E-mail			0.011 (0.030)	0.018 (0.029)	−0.030 (0.038)	0.017 (0.030)
Voucher	0.101*** (0.031)	0.148*** (0.030)	0.073** (0.031)	0.022 (0.030)	−0.015 (0.040)	0.030 (0.031)
Vacancies	0.111*** (0.031)	0.137*** (0.030)	−0.025 (0.031)	−0.016 (0.030)	0.054 (0.040)	0.035 (0.031)
Reference Mean	0.587	0.277	0.32	0.287	0.425	0.64
Reference SD	0.493	0.448	0.467	0.453	0.495	0.48
Caseworker Fixed Effects	0	0	1	1	0	1
Observations	1,337	1,337	1,703	1,703	1,145	1,722

Note: The table reports average treatment effects on survey outcomes measured about five weeks after treatment. Columns 1–2 compare the voucher and vacancies groups (T2 and T3) to the e-mail group (T1). Estimates include strata fixed effects, baseline controls described in Section 3.4, and, where indicated, caseworker fixed effects. Columns (3)–(4) and (6), which use the full survey sample, include caseworker fixed effects; columns (1)–(2) and (5), based on subsamples, do not due to limited statistical power. Sample sizes vary: treatment groups excluding the control group (columns 1–2), all respondents (columns 3–4 and 6), and respondents not enrolled in training (column 5). Likert scale responses are recoded as binary variables, equal to 1 if the respondent selected one of the top two categories (e.g., "very important" or "rather important"), and 0 otherwise. Full survey questions are provided in Appendix Section D.2. Standard errors in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Table E2: Average treatment effect on training by caseworker productivity

	Dependent variable:					
	Training take-up		Training completion		Training take-up	
	Low-productivity caseworker	High-productivity caseworker	Low-productivity caseworker	High-productivity caseworker	Low-productivity caseworker	High-productivity caseworker
	(1)	(2)	(3)	(4)	(5)	(6)
E-Mail	0.022*	0.007	0.021*	0.003	0.029***	0.003
	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)	(0.013)
Voucher	0.036***	0.015	0.034***	0.013	0.036***	0.011
	(0.012)	(0.012)	(0.012)	(0.011)	(0.011)	(0.013)
Vacancies	0.006	−0.007	0.005	−0.001	0.007	−0.008
	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)	(0.012)
FE outcome	empl. duration	empl. duration	empl. duration	empl. duration	unempl. duration	unempl. duration
Reference Mean	0.118	0.107	0.1	0.089	0.098	0.129
Reference SD	0.322	0.31	0.3	0.285	0.297	0.335
Observations	5,290	5,271	5,380	5,325	5,489	5,216

Note: The table reports average treatment effects on training enrollment and completion by treatment group, split by caseworker productivity. In columns 1–4, productivity is based on caseworker fixed effects from a regression of job seeker employment duration on caseworker fixed effects, controlling for baseline covariates and treatment group. In columns 5–6, productivity is based on fixed effects from the same regression using unemployment duration as dependent variable. A dummy equals 1 if the caseworker's fixed effect is above the median (high productivity) and 0 otherwise. Columns 1–2 and 5–6 report training enrollment; columns 3–4 report training completion. Estimates follow the baseline specification in Section 3.4, which includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Table E3: Uptake of active labor market programs by job seekers, separated by caseworker productivity

	Total	Low-productivity caseworker|A	High-productivity caseworker |B
	(1)	(2)	(3)
Panel A: Full sample			
Training	0.121	0.132 B	0.110
Examined	0.051	0.054 B	0.047
External courses	0.032	0.032	0.032
Application courses	0.040	0.039	0.040
Any program	0.193	0.206 B	0.179
Subsidized employment	0.229	0.231	0.227
Observations	10,890	5,474	5,416
Panel B: Control Group			
Training	0.113	0.118	0.107
Examined	0.047	0.049	0.045
External courses	0.032	0.031	0.033
Application courses	0.045	0.044	0.047
Any program	0.194	0.202	0.185
Subsidized employment	0.242	0.231	0.254
Observations	2,718	1,376	1,342

Note: The table reports mean uptake of active labor market programs by job seekers, separated by caseworker productivity. Column 1 shows the full sample, column 2 job seekers with low-productivity caseworkers, and column 3 job seekers with high-productivity caseworkers. Panel A reports results for the full sample; Panel B restricts to the control group. Pairwise t-tests are conducted within groups at the 5 percent significance level. "A" denotes significant differences relative to column (2) and "B" denotes significant differences relative to column (3).

Table E4: Survey responses on PES perception by caseworker productivity

	Total (1)	Low-productivity caseworker|A (2)	High-productivity caseworker |B (3)
Panel A: Full sample			
My wishes are considered	0.693	0.705	0.682
Apply for irrelevant jobs	0.472	0.479	0.461
More control than consultation	0.357	0.384 B	0.332
Choose own courses	0.409	0.407	0.407
Courses are expensive	0.658	0.665	0.653
Courses are high quality	0.429	0.433	0.424
Observations	1,767	875	874
Assigned to course	0.406	0.436	0.370
Observations	497	259	235
Course was turned down	0.255	0.282 B	0.230
Lack of information	0.405	0.416	0.392
Observations	1,167	574	579
Panel B: Control Group			
My wishes are considered	0.741	0.756	0.727
Apply for irrelevant jobs	0.458	0.421	0.492
More control than consultation	0.349	0.389	0.318
Choose own courses	0.362	0.362	0.355
Courses are expensive	0.640	0.665	0.620
Courses are high quality	0.433	0.471	0.397
Observations	467	221	242
Assigned to course	0.465	0.526	0.414
Observations	127	57	70
Course was turned down	0.225	0.250	0.209
Lack of information	0.425	0.479 B	0.373
Observations	306	144	158

Note: The table reports mean survey outcomes measured about five weeks after treatment. Column 1 shows the full sample, column 2 job seekers with low-productivity caseworkers, and column 3 job seekers with high-productivity caseworkers. Panel A reports results for the full sample; Panel B restricts to the control group. Pairwise *t*-tests are conducted within groups at the 5 percent significance level. "A" denotes significant differences relative to column (2) and "B" denotes significant differences relative to column (3). Full survey questions are provided in Appendix Section D.2. Likert scale responses are re-coded as binary variables, equal to 1 if the respondent selected one of the top two categories (e.g., "very important" or "rather important"), and 0 otherwise.

Table E5: Average treatment effects on employment by caseworker productivity I

	Dependent variable:			
	Any employment		Days in employment	
	Low-productivity caseworker	High-productivity caseworker	Low-productivity caseworker	High-productivity caseworker
	(1)	(2)	(3)	(4)
E-Mail + Voucher	−0.007	0.002	4.043	−12.660
	(0.013)	(0.012)	(12.530)	(12.924)
Control Group Mean	0.77	0.81	440.53	535.77
Control Group SD	0.01	0.01	10.77	11.17
Observations	5,290	5,271	5,290	5,271

Note: The table reports average treatment effects on employment outcomes split by caseworker productivity. Treatment group 1 (e-mail) and treatment group 2 (voucher) are combined to maximize statistical power. Any employment (columns 1–2) is a binary indicator for employment at any time during the period. Days in employment (column 3–4) is measured as counts. Caseworker productivity is measured using fixed effects from a regression of job seeker employment duration on caseworker fixed effects, controlling for baseline covariates and treatment group. A dummy equals 1 if the caseworker's fixed effect is above the median (high productivity) and 0 otherwise (low productivity). Estimates includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Standard errors are in parentheses: *p<0.1; **p<0.05; ***p<0.01.

Table E6: Average treatment effects on employment by caseworker productivity II

	Dependent variable:			
	Days in unemployment		Avg. daily wage	
	Low-productivity caseworker	High-productivity caseworker	Low-productivity caseworker	High-productivity caseworker
	(1)	(2)	(3)	(4)
E-Mail + Voucher	−9.104	3.573	0.657	0.051
	(10.529)	(10.170)	(1.120)	(1.207)
Control Group Mean	454.45	400.54	50.57	50.27
Control Group SD	8.84	8.42	0.81	0.89
Observations	5,290	5,271	3,931	4,158

Note: The table reports average treatment effects on employment outcomes split by caseworker productivity. Treatment group 1 (e-mail) and treatment group 2 (voucher) are combined to maximize statistical power. Days in unemployment (column 1–2) is measured as counts. Average daily wage (columns 3–4) is the mean wage per day worked. Caseworker productivity is measured using fixed effects from a regression of job seeker employment duration on caseworker fixed effects, controlling for baseline covariates and treatment group. A dummy equals 1 if the caseworker's fixed effect is above the median (high productivity) and 0 otherwise (low productivity). Estimates includes strata fixed effects, caseworker fixed effects, and controls for language skills, citizenship, occupation, marginal employment, previous wage, days in employment during the past ten years, and the number of employment spells during the past ten years. Standard errors are in parentheses: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table E7: Evaluation of vacancies information by respondent characteristics

	Percent at least rather agreeing	
	This information is important for me	Would be willing to work 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%

Note: The table reports the share of respondents in the vacancies information treatment group (T3) who expressed agreement, measured about five weeks after treatment. Column 1 shows the share rating the vacancies information as "very important" or "rather important." Column 2 shows the share indicating they would "very much" or "rather" consider working in one of the jobs referenced in the vacancies information. Pairwise *t*-tests are conducted within groups at the 5 percent significance level. "B" and "C" denote significant differences relative to the respective sub-group. Likert scale responses are recoded as binary variables, equal to 1 if the respondent selected one of the top two categories (e.g., "very important" or "rather important"), and 0 otherwise. Full survey questions are provided in Appendix Section D.2.