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Repackaging active labor market policy: Experimental evidence of training vouchers for unemployed

PRE-ANALYSIS PLAN

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This document describes the research design and analysis strategy of our field experiment. The aim is to boost skills and employment by repackaging training program provision in form of education vouchers to the unemployed. The intervention takes place in February and March 2021 during a Covid-19 induced partial lockdown in the region of Lower Austria (*Niederösterreich*) in Austria. We start with a detailed outline of the intervention. We designed multiple different treatment arms to separate out direct effects of raising awareness, supporting reciprocity, and strengthening autonomy. We provide a detailed discussion of our sample selection, variables used and the handling of the data to make the analysis as transparent and replicable as possible. Further, we state our hypotheses and outcomes of interest motivated by the active labour market policy evaluation literature. Finally, we conclude by specifying our statistical approach to inference.

1 Background

1.1 Description of the intervention

In February and March 2021, we launch a field experiment designed in cooperation and implemented by the Public Employment Service (PES) of Lower Austria (*Arbeitsmarktservice Niederösterreich (AMS NÖ)*). The aim is to increase skills and employment among the unemployed by increasing participation in and completion of training programs. The intervention consists of an email newsletter that invites unemployed, who have been registered as unemployed for 3 to 12 months, to voluntarily contact the PES to arrange a consultation on training programs. A more detailed description of the target population is provided below. The newsletters will be sent in three waves on 9th of February, 16th of February, and 9th of March. Individuals are assigned to the waves depending on their duration of unemployment. The newsletters were designed in cooperation with the regional PES.

Three different treatment arms vary the type of information provided and the autonomy that the unemployed have in choosing a training program. The unemployed in the sample will be randomly allocated to each of the 4 groups on an individual level. This randomization is conducted separately for each sending date, which ensures that unemployed with different unemployment durations are equally divided within the groups. Further information about the randomization procedure is provided below.

The different treatment conditions are as follows:

1. Control group
2. Treatment group with newsletter
3. Treatment group with newsletter with voucher
4. Treatment group with newsletter, voucher, and information prime

The newsletter consists of an invitation to a consultation to discuss potential training programs with the PES' job counselor and provides information about existing financial incentives to start a training program.
Treatment groups 3 and 4 further receive a voucher worth €15.000,-, which can be redeemed to take part in training programs provided by the PES. Alternatively, the voucher can be redeemed in consultation with the PES for any outside training for up to €2.000,-. The groups that receive the voucher further receive a list of typical training programs as part of the newsletter. This should motivate the unemployed in these two groups to already think about their preferred training program before the consultation with a job counselor. It is designed to increase autonomy of the unemployed in their choice of training programs.
Finally, group 4 additionally receives an information treatment consisting of the 10 occupations with the highest number of job vaccancies as of December 2020. This information treatment is intended to counteract a frequently mentioned concern related to training vouchers: unemployed allegedly do not have enough information to make an informed choice about their optimal training program (Strittmatter, 2016).

In general, all groups (including the control group) have access to the same training programs. The intervention, thus, consists of the information provided and variation in the type of information and additionally varies the actual and perceived autonomy that the unemployed have in choosing their courses.

Conditionality Conditionality is eased for the treatment groups receiving a training voucher (groups 3 and 4). Currently, by law (*Arbeitslosenversicherungsgesetz (AlVG §9)*), unemployed are assigned to labor market programs by the PES. This takes place after a consultation with the job counselor. In most cases, consultations with the job counselors and program assginment is obligatory, i.e. with conditionality attached. If an unemployed does not attend a PES appointment or assigned program - typically with no shows - her unemployment benefits can be cut or - in the most severe cases - blocked temporarily. Absences with a valid excuse, such as for sick leave, are exempted. As a result, assignment to a training program is often perceived negatively as a burden or punishment rather than positively as an opportunity and support.
In the context of our experiment, the consultation with the job counselor is open to all treatment and control groups. Attendance is voluntary and only takes place if an unemployed contacts the PES on her own initiative, i.e. as a response to the newsletter. Job counselors are instructed to conduct the consultations as usual with participants of the control group and the treatment receiving the standard newsletter (group 2). In practice, such consultations often imply limited discussion with unemployed about their preferences. By contrast, job counselors are instructed to *take serious* the voucher the unemployed assigned to group 3 and 4 received, and to actively encourage them to come up with suggestions for a training program on their own. This should increase autonomy of the unemployed about their choice of training program and consequently may increase reciprocity and motivation. By law, job counsellors are required to assess the adequacy of any training program and approve of it (*Arbeitsmarktpolitische Prüfung*). Thus for groups 3 and

4, job counselors will only intervene to avoid course choice that seem purely for personal pleasure or have obvious negative effects on the employment prospects of the unemployed.

1.2 Timeline for the intervention

The intervention takes place in three waves in February and March 2021:

- February 2: all unemployed who have been unemployed for at least 3 months and up to 6 months (91-180 days) - *not part of the randomization - all are in Treatment group 1!*
- February 9: all unemployed with an unemployment spell of more than 6 and up to 9 months receive the newsletter (181-270 days)
- February 16: all unemployed with an unemployment spell of more than 9 up to 12 months receive the newsletter (271-365 days)
- March 9: all unemployed with an unemployment spell of at least 3 months adn up to 6 months and who did not receive a newsletter in the first wave (i.e. those who have been unemployed between 2 and 3 months at the beginning of February)

First analyses of short-term treatment effects will be carried out with outcome data delivered in April, June, and September 2021. Longer-term effects will be estimated with data delivered each year in February until 2026. This will allow us to estimate long-term effects up to 5 years after the intervention.

1.3 Covid-19 labor market crisis

The timing of the intervention is amidst the Covid-19 crisis, which affected the Austrian labor market severely. In December 2020 the absolute number of unemployed persons in Austria has reached nearly 500.000 people, which is around 30% higher than in December 2019 (AMS-Übersichtsbericht, 2021). The situation in Niederösterreich, the region we study, is slightly better with around 70.000 unemployed and an increase of 18% in comparison to the previous year (AMS-Übersichtsbericht, 2021). The most affected subgroups were young and blue collar workers. The impact is very uneven across sectors with tourism, hospitality and personal services hit hardest by a drop in employment due to government induced shutdowns (Bock-Schappelwein, Huemer, and Hyll, 2021).
When considering the literature about the effects of training programs for the unemployed the presence of so-called lock-in effects is undisputed. Training programs, thus, first lead to negative employment effects, as they take up a considerable amount of time for the unemployed, which reduces time spent on job search or even prevents take-up of available jobs (Vooren et al., 2019). Positive employment effects were found to materialize only in the long-term, i.e. at the earliest one year afterwards. However, these lock-in effects are strongly dependent on the current labor market situation. In times of high unemployment and dense labor markets, these lock-in effects are reduced, which, in turn, increases the effectiveness of training programs in recessions (Card, Kluve, and Weber, 2018). In light of this evidence, the timing of the intervention was deliberately set amidst the Covid-19 crisis to maximize it's effectiveness.

2 Study design

2.1 Overview

In this section, we describe our analytical approach in detail. Firstly, we specify the criteria for inclusion in our sample and related limitations. Secondly, we describe the data. Thirdly,

we present the randomisation procedure for our treatment assignment. Fourthly, we state our hypotheses regarding outcomes. Fifthly, we specify the details of our outcome variables. Finally, we discuss our estimation and inference approach.

2.2 Sample selection

In January 2021 there have been 73.621 persons registered as unemployed in Lower Austria. Our sample comprises of every person, who has been registered as unemployed with the PES for at least 6 and up to 12 months with the status "unemployed" as well as "in job search", meaning that all registered unemployed are included regardless of whether they receive unemployment benefits or not. Additionally, with the last wave, we also capture those, who have been unemployed at the start of the intervention for between 2 and 3 months. It is further restricted to people, who are at least 25 years old and do not have a pending job acceptance. Additionally, unemployed who are already enrolled in a training program *in Schulung* at the start of the intervention are excluded from the sample. Since the newsletter is sent via email only registered unemployed with a valid email can be contacted. Unemployed without a valid email are not included in the randomisation procedure and thus excluded from the study. We therefore have 11.210 people in wave 2, 11.428 in wave 3 and *number* in the last wave, adding up to *number* people in total. These restrictions have to be kept in mind when generalizing our results to a broader population of unemployed.

2.3 Data

Administrative data All data used in the analysis is provided by the PES and consists of administrative data. No additional data will be collected. Table 1 shows a summary and categorisation of all variables used.

Attrition & Exclusions We will make an effort to keep attrition to a minimum. As we use solely administrative data, we expect attrition to be very low. It is, however, possible that participants move abroad or pass away. We will test, whether those who attrit are systematically different from the rest of the sample and report the results. Further, we can provide estimates of lower and upper bounds of our estimated effects depending on different assumptions we make about the part of our sample, that shows attrition.

Firstly, as mentioned above, all unemployed who do not have a valid email are excluded from the study by definition, as we cannot reach them. However, we have data about this subsample and can, as with attrition, check whether they differ systematically from our sample and discuss potential biases. Additionally, those who are already taking part in a training program are also not included in our sample, because they are, in some way, already treated. It would not make any sense to treat them with our intervention, as they cannot enrol in another training program in parallel. Our findings thus extend only to those unemployed who are not already in training programs and should be generalized to all unemployed only with caution.

2.4 Hypotheses

We have several hypotheses regarding the different treatment groups and different outcome variables:

1. Group 2-4 will achive higher training program take-up and completion rates than the control group, but groups 3 and 4 will have even higher take-up and completion rates than group 2.

2. We expect negative short-term effects (due to the so-called lock-in effects) on unemployment duration, which however will turn positive for all treatment groups in the long-term.

Table 1: Variables

Type	Variable name
Primary outcomes	Training completion Labor market status[1] Earnings (+ probability of having a high wage)
Secondary outcomes	Newsletter read + Clicks Training take-up Type of training[2] Job search: Matching attempts
Stratified Randomization	job counselor education unemployment duration (+ pre- and post Corona unemployed) income level age gender language proficiency care responsibilities health condition
Control Variables	demographic: education, gender, region, age, nationality, income level[3] job relevant: health condition, language proficiency, care responsibilities labor market: experience, occupation, sector, marginal employment AMS: unemployment duration, job counselor, PES branch office
Heterogeneity analysis	education unemployment duration (+ pre- and post Corona unemployed) income level age gender language proficiency care responsibilities health condition

[1] differentiated by employment and labor force exit.
[2] duration, type of certificate, occupation, sector.
[3] before unemployment spell.

3. Further, re-employment rates will be highest in group 4, followed by group 3, 2, and lastly the control group.

4. Finally, earnings will follow the same pattern as re-employment rates, but less pronounced.

The first hypothesis follows from studies, such as Doerr and Strittmatter (2018). They show that motivation to take-up and complete courses increases with voucher systems, due to increased autonomy, reciprocity and higher valuation of financial costs of such programs. In turn, voucher systems lead to less drop-out from courses. Finally, group 2 is expected to exhibit higher take-up rates than the control group, because they are informed about the positive effects of training and therefore nudged towards training program participation. As described above, negative short-term effects are relatively well-established in the literature and we therefore expect them as well

in our setting; however smaller in size due to limited job vaccancies as a result of the Covid-19 crisis. Further, these negative short-term effects are shown to become positive long-term effects on unemployment duration in most of the literature about effects of training for the unemployed with and without vouchers (Card, Kluve, and Weber, 2018; Doerr and Strittmatter, 2018). The third hypothesis follows directly from the variation in training participation between the groups. In addition, group 4 is expected to have higher re-employment rates because of the additional labour market information received. This information treatment is expected to counter the lack of information, well established in the literature (Perez-Johnson, Moore, Santillano, et al., 2011; Strittmatter, 2016), and could therefore lead to better targeted training choices. Earnings follow the same pattern as re-employment rates due to the variation in training participation and the additional information in group 4. However, the effects on earnings are often less pronounced and clear-cut than those on re-employment (Card, Kluve, and Weber, 2018).

2.5 Outcomes of interest

All data on outcomes of interest are available from administrative data sources, provided by the PES.

Primary outcomes Our three primary outcomes are training completion, labor market status, and earnings. The first one refers to whether or not a course/training program is completed. The second one differentiates between: unemployed, employed, out-of-labor force. We can, thus, differentiate between people exiting unemployment because they found a job and those exiting unemployment, because they dropped out of the labor force. This differentiation is important for drawing conclusions from the findings. Finally, earnings are important because they provide evidence on the quality of the job-match after unemployment. However, earnings are only observed for those who actually found a job. Therefore, we provide estimates for effects on earnings conditional on finding a job, but also for effects on the probability of being in high-paid employment. The latter can account for this inherent endogeneity (Rothstein and Von Wachter, 2017).

Secondary outcomes The following secondary outcomes are not per se desired outcomes, but can be seen as mechanisms leading to the primary outcomes described above. In this context we will look at whether or not the email was read, newsletter clicks, course take-up, and job search. We will differentiate by the type of course, the duration, and the occupation to check whether the intervention also changes training choices. Further, for the short-term effects on unemployment duration, it is important to monitor job search effort. However, we cannot perfectly observe job search, but we have data on attempts of the PES to match unemployed to jobs through job interviews etc. This can act as a proxy for job search to understand the mechanism behind negative short-term effects.

2.6 Estimation and inference

Due to the clean randomization of participants into control and treatment groups it is possible to, in a first step, compare the relevant outcome variables directly between the 4 groups via a two-sided test, such as a T-test or Mann-Whitney test or others, which will be chosen depending on the distribution of the outcome variables (Moffatt, 2019). Throughout the whole study, we will infer statistical significance via a p-value of 0.05 or below, thus using a 95% confidence interval.
To increase precision and robustness we will estimate parametric regressions for the treatment effects using the following estimation regression:

$$Y_{it} = \beta_0 + \beta_1 T_{NL} + \beta_2 T_V + \beta_3 T_{V+I} + \mathbf{X_{it}} + \mathbf{X_i} + \tau_t + \pi_j + \epsilon_{it} \tag{1}$$

where Y_{ijt} refers to the interesting outcome variables for individual i at time t. Depending on the scale of the outcome variable, an OLS(continuous) or a Logit(binary) regression is used. In the baseline specification the control group is the reference group, but depending on which difference between groups has to be estimated, the reference group will be chosen accordingly. T_{NL} is the second group with only the newsletter, T_V is the third group with the voucher, and T_{V+I} is the fourth group with voucher and information prime. Further, we include all control variables specified in table 1. $\mathbf{X_{it}}$ refers to the subset of these, which are not stable over time and would change independently of the treatment, which only includes health problems and care responsibilities. As education, marginal employment, or language proficiency could be direct outcomes of the intervention, these are only included for the pre-treatment period. Additionally, all other control variables specified in table 1 are as well included with their pre-intervention values and are represented by $\mathbf{X_i}$. Further, we apply time fixed effects τ_t and job counselor fixed effects π_j to control for time trends and unobservable differences between the respective job counselors consulting the unemployed. Finally, if necessary standard errors will be adjusted to be robust to heteroskedasticity as well as serial correlation, if necessary (using appropriate statistical tests) (Wooldridge, 2016).

The heterogeneity analysis will be conducted via subgroup regressions of the equation above for the variables specified in table 1. Additionally, the treatment dummies in the equation above can be interacted with the pre-specified variables in table 1 and estimated for both relevant heterogeneity variables separately.

3 Publication agreement

This evaluation is based on an agreement between the researchers (i.e. Lukas Lehner and Anna Schwarz) and the *AMS NÖ*. Two key components of this agreement are that (1) no payment will be made from the *AMS NÖ* to the researchers, and (2) the researchers are guaranteed to be entitled to publish the findings of their study in academic outlets without any interference by the *AMS NÖ*.

4 IRB approval

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