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Abstract

Digitization has spurred interest in the potential of statistical profiling to improve the targeting of active labor market policies. Despite growing adoption, empirical evidence on the effectiveness of such profiling in program allocation is scarce. We evaluate a semi-automated statistical profiling model in Austria that aims to target policies based on predicted reemployment prospects (low, medium, high). Our analysis shows that a reallocation of resources from low-chance to medium-chance segments, as envisaged by the Public Employment Service, would not yield the desired efficiency gains. Employment programs have a stronger impact on jobseekers with low job prospects than on those with medium prospects, and training programs are not consistently less effective in the low-chance segment either. Our findings suggest that the focus should remain on the most disadvantaged, both from an efficiency and an equity perspective. They caution against relying on overly coarse profiling and stress the need for nuanced targeting strategies.

JEL classification: • J24 • J64 • J68

Keywords: Digitization, Public Employment Service, statistical profiling, targeting, active labor market policy, training, wage subsidies, direct job creation

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1 Introduction

Digitization and artificial Intelligence are fundamentally changing the design and delivery of social and labor market policies, and, subsequently, their impact on welfare recipients. However, the exact nature and extent of this transformation is far from being fully understood (Scarano & Colfer, 2022; van Gerven, 2022). At the forefront of this transformation is the digitization of public employment services (PES). In recent years, PES have increasingly adopted digital technologies, including artificial intelligence, with the aim of improving the quality, efficiency and cost-effectiveness of service delivery and active labor market policies (ALMPs) (Duell, 2023; Peromingo & Davem, 2023).

In particular, PES have begun to experiment with the statistical profiling of unemployed jobseekers. Traditionally, they have used rule-based approaches combined with caseworker discretion to segment their unemployed clients into groups and tailor services and allocate resources accordingly. However, the growing availability of large administrative sources of microdata, recent advances in machine learning, and budgetary pressures have led them to increasingly use data and statistical models to predict jobseekers' prospects of finding work and classify them on this basis (Desiere & Struyven, 2021; Körtner & Bonoli, 2023). This computerized segmentation can be used for a variety of purposes.

The basic idea is that unemployed individuals with different expected job prospects need different types and intensities of support (van den Berg et al., 2023). Profiling can be used as a means of allocating employment services and programs to jobseekers, i.e., targeting. So far, it has been used in most implemented applications to identify jobseekers at risk of becoming long-term unemployed and to better tailor services to the needs of this group (Desiere et al., 2019; Körtner & Bonoli, 2023; van Landeghem et al., 2021).

Statistical profiling based on informative microdata sources has the potential to improve tailoring, targeting, and efficiency. The hope is that resources and interventions can be allocated more effectively and efficiently by better prioritizing those who will benefit most (Bach et al., 2023). However, the potential for improvements of ALMP through statistical profiling depends on how it is designed and used. It is only successful if two conditions are met: First, the profiling is accurate in predicting the profiling variable, such as the duration of unemployment or the likelihood of reemployment, and second, it is used effectively to target services to those clients with the greatest benefit from participation compared to non-participation (Berger et al., 2001). Despite its promise and growing adoption in the OECD, empirical evidence on statistical profiling as a means to increase the efficiency of ALMP allocation is scarce. Very few studies shed light on its effectiveness in helping to better target programs to those who benefit most.

We contribute to filling this research gap by evaluating a new, semi-automated statistical profiling model called "Labor Market Chances Assistance System (AMAS)", which has been fully developed in Austria to target labor market policies, has been approved by all relevant bodies, including the Ministry of Labor, but has not yet been implemented due to concerns of the data protection authority. AMAS divides unemployed jobseekers into three groups based on their chances of reemployment as predicted by an algorithm. The Austrian PES plans to use this profiling to shift resources to jobseekers with medium job prospects. It believes that most clients with high job prospects will find work quickly anyway, so that targeting intensive ALMPs would create deadweight losses that should be avoided, and that those with low prospects are too far from reentering the labor market and therefore benefit too little from active policies compared to the group with medium job prospects. Given scarce resources, the Austrian PES plans to concentrate internal counseling, costly training, and employment programs on the medium-chance segment. For the low-chance segment, low-cost external counseling and support will be provided. The PES hopes that this shift will lead to gains in effectiveness and efficiency.

Whether these expectations are justified has not yet been evaluated empirically. It depends crucially on whether the segments derived from the estimated reemployment prospects are good predictors of the expected effectiveness of support. The planned shift of ALMP-resources from the low to the medium-chance segment promises efficiency gains only if the treatment effects for those with low predicted re-employment prospects are indeed lower than for those with medium prospects. Our analysis of potential treatment effects for each of the groups of unemployed considered is the first to systematically examine this relationship between profiling and program impact.

The Austrian setting provides a unique opportunity for causal analysis, as we can study the effects of a real-life example and benefit from the fact that profiling has not yet been applied and therefore has not influenced the prior allocation of ALMPs. We compare treatment effects across segments at a time when programs were allocated completely independently of the computerized segmentation that was later planned. This lends particular credibility to our empirical approach. We take the opportunity to analyze the potential of profiling to improve the effectiveness of ALMPs. Specifically, we test whether programs for the unemployed in the segment with low a priori chances of re-employment are indeed less effective than for those in the medium-chance segment, and whether shifting resources to the medium segment therefore promises efficiency gains. In addition, we examine the heterogeneity of effects across many subgroups of the unemployed within the segments. This shows how accurate the planned segmentation of clients into the three defined groups is.

In a first step, we simulate how the unemployed from 2014 to 2017 would have been segmented if statistical profiling had already been applied by replicating the predictive algorithm of the PES for the past. That is, we use an almost identical logistic model based on individual characteristics and information on previous labor market careers to predict job prospects and the intended thresholds used for AMAS to classify into the three respective groups with high, medium, and low reintegration prospects. We then apply a dynamic matching approach to estimate and compare the effects of the four most relevant types of ALMP programs on labor market integration in the medium and the low chance segments. These include (1) training provided by external providers on behalf of the PES, (2) course subsidies for participation in training on the open education market, (3) temporary wage subsidies in the private sector, and (4) direct job creation in the public and non-profit sectors.

We find that labor market policies are generally no less effective in the low-chance segment than in the medium-chance segment. Both employment programs – wage subsidies in the private sector and direct job creation – clearly improve integration into unsubsidized employment more in the low-chance segment. This relationship is less clear for training programs, but they are generally not more effective in the medium-chance segment either. These findings suggest that the new profiling and targeting strategy is unlikely to produce the desired efficiency gains, as resources would be allocated less than before to those who benefit more from them. Only in the case of training provided by external training providers could a shift potentially lead to some efficiency gains.

A reallocation of resources from the low-chance to the medium-chance segment risks creating a "Matthew effect" in access to and use of ALMPs (Bonoli & Liechti, 2018), as it would favor people who already have better chances of re-employment and disadvantage those most in need of support. Our findings highlight the importance of maintaining a focus on the most disadvantaged jobseekers, both from an efficiency and equity perspective. Moreover, they suggest that the classification into three groups based on job prospects is too coarse, as we observe significant differences in program impacts across subgroups, even within the segments created by the algorithm. In sum, our results suggest that while statistical profiling has the potential to improve the efficiency of ALMPs, this depends heavily on its intended use and specific configuration.

2 Previous evidence

The United States (US) and Australia introduced fully operational profiling systems based on statistical prediction for customer segmentation as early as the late 1990s. Over time, New Zealand and several countries in Europe have also tested or implemented profiling systems, some using machine learning techniques, including the Netherlands (Wijnhoven & Havinga, 2014), Flanders in Belgium (Desiere et al., 2019), Ireland (McGuinness et al., 2022; O'Connell et al., 2012), and Denmark (Desiere et al., 2019). Despite this growing adoption of statistical profiling, there is still a large research gap in evaluation.

Some studies analyze the accuracy of profiling methods in predicting profiling variables. For example, Bach et al. (2023) evaluate regression and machine learning models for predicting jobseekers' risk of becoming long-term unemployed using German administrative labor market data. van den Berg et al. (2023) compare three different types of predictions of the probability of finding a job within six months of entering unemployment: self-assessments by the unemployed, assessments by caseworkers, and predictions based on machine learning algorithms. They show that none of these three measures consistently outperforms the others in terms of information value. Instead, an integrated approach that combines insights from all three methods may be the most effective strategy for accurately identifying individuals at risk of long-term unemployment.

Other studies point to factors that should be taken more into account in profiling, such as psychological factors (Houssemand et al., 2014) or the accessibility of suitable job offers by private and public transport (Fransen et al., 2019), shed light on social and political-legal aspects such as transparency and possible discrimination (e.g. Allhutter et al., 2020; Desiere & Struyven, 2021; Niklas et al., 2015) or identify drivers of acceptance of profiling (Delpierre et al., 2024).

However, very few studies have explored the potential of profiling to contribute to efficiency gains in the allocation of labor market programs. Black et al. (2003) examine the impact of the US Worker Profiling and Reemployment Services (WPRS) system in the state of Kentucky. This program provides early mandatory employment and training services to Unemployment Insurance (UI) claimants with long predicted UI spells or a high predicted probability of benefit exhaustion. They find that imposing this requirement significantly reduces the average level and duration of UI benefits and increases average earnings. They also evaluate the use of profiling scores based on expected UI claim duration as a means of allocating treatment. A priori, the authors expect the effect of treatment to increase with the profiling score if it is an efficient method of allocating treatment. Instead, they find little evidence of a systematic relationship between the estimated treatment effect and the profiling score, suggesting that such profiling does not increase the efficiency of treatment allocation.

Caliendo et al. (2008) construct a target score that simply sums the number of individual labor market disadvantages and estimate the effect of participation in a German direct job creation program within each category of the target score. Their results tend to support the hypothesis that higher target scores are associated with larger impacts, but insignificant estimates for most groups preclude firm conclusions. O'Leary et al. (2005) estimate that targeting reemployment bonus offers to UI claimants identified through profiling models as most likely to exhaust benefits reduces benefit payments and thus can improve cost-effectiveness.

Related to this are several studies that find evidence of substantial potential gains from introducing statistical treatment rules, i.e., using treatment effect estimates to allocate labor market programs to those individuals who are expected to benefit most. For example, in a Bayesian analysis of California's Greater Avenues to Independence (GAIN), a mandatory welfare-to-work program, Dehejia (2005) finds that statistical treatment rules can increase average program impacts and cost-effectiveness. For Switzerland, Lechner and Smith (2007) compare caseworker assignment to labor market programs and services with alternatives including random assignment and assignment via statistical treatment rules based on observable participant characteristics. They find that caseworkers achieved roughly the same post-program employment rates as random assignment, while statistical treatment rules performed significantly better. They thus come to a similar conclusion as Frölich et al. (2003) in their simulation for Switzerland that statistically assisted program assignment is potentially fruitful. A key feature of these examples is that profiling is not based on predicted outcomes (such as duration of unemployment or probability of reemployment) but on predicted impacts, i.e., the change in outcomes due to participation (see Smith, 2022).

Typically, statistical systems are designed to provide additional information to caseworkers, leaving the allocation decision largely up to them. Some studies show that caseworkers do not necessarily take up this offer. For example, a large field experiment in Swiss employment offices showed that access to a statistical support system did not significantly change their behavior (see Behncke et al., 2009). The authors conclude that strong incentives or coercion are needed for caseworkers to comply with statistical profiling and targeting systems. Bolhaar et al. (2020) find considerable heterogeneity in the way caseworkers allocate welfare-to-work programs in a field experiment in Amsterdam. Caseworkers did not appear to make optimal use of their discretion in assigning benefit recipients to the most effective programs. Even learning about the effectiveness of different programs does not lead them to focus more on the effective ones.

Overall, the available evidence on the effectiveness of profiling and targeting systems is scarce. There is a large gap in evaluations from the field that show whether it is an effective tool in terms of better targeting to those who benefit most.

3 Austria's statistical profiling model

Austria is an OECD country with traditionally high spending on active labor market policies and one of the lowest unemployment rates (Lauringson & Lüske, 2021; Miyamoto & Suphaphiphat, 2021). The Austrian PES (*Arbeitsmarktservice*, AMS), which is responsible for the implementation of labor market policies, started in 2015 to develop a new strategy for the segmentation of its clients based on statistical profiling. This was triggered by three perceptions: First, unemployment and long-term unemployment in Austria had risen unusually sharply in the wake of the financial market and economic crisis in 2009 (see Eppel et al., 2018). Second, long-term unemployment remained at historically high levels, despite a decline in the subsequent economic and labor market upswing. Third, the AMS, faced with scarce budgetary resources, perceived a decreasing effectiveness of the applied labor market measures, a mismatch between the costs and the success of its interventions.

Against this background, the PES commissioned an external research institute, *Synthesis Forschung*, to develop "AMAS", a statistical profiling model to predict the future labor market chances of its clients, to classify the unemployed on this basis into three segments with low, medium and high chances of reintegration, and thus to help decide which clients should receive which level of support and which programs they should be assigned to (see Marte-Huainigg, 2020).

The model calculates future employment probabilities for currently registered clients based on past clients: first, the probability of being in unsubsidized employment for at least three months in the following seven months (short-term indicator), and second, the probability of being in unsubsidized employment for at least six months in the following 24 months (long-term indicator), at the time of entering unemployment and at monthly intervals thereafter. Those with at least a 66% probability of reaching the short-term employment threshold are assigned to the segment with high reintegration chances, while those with less than a 25% probability of reaching the long-term employment threshold are assigned to the low segment. The rest remains in the medium segment.

More specifically, the currently registered clients are categorized into numerous small subgroups based on the unique combinations of thirteen characteristics. For each subgroup, the predicted likelihood of future reintegration into employment is derived from the empirically observed proportion of clients within that subgroup who have successfully achieved short- or long-term employment in the past (cf. Arbeitsmarktservice Österreich (AMS), 2020; Gamper et al., 2020). The characteristics, each with categorized specifications, include personal attributes (gender, age, nationality, education, health impairment, childcare responsibilities), characteristics of the previous employment history (occupation, days of employment, number and duration of unemployment spells and participation in ALMP measures in the four years before entering unemployment), the elapsed unemployment duration and the type of region in terms of labor market conditions (cf. Allhutter et al., 2020; Gamper et al., 2020; Marte-Huainigg, 2020) (for details see Table 3 in the online appendix).

The probabilities are calculated separately for four subpopulations – a base population with complete data and three subpopulations with incomplete data and therefore adjusted variables (young entrants, recent immigrants, and others with "fragmented data") – and for twelve unemployment duration groups. Too small subgroups are pooled by successive exclusion of characteristics.

In addition to the data-driven assignment, some special rules apply, making AMAS a statistical profiling with elements of rule-based profiling. Certain groups are assigned to certain segments from the outset. First, all persons with a job offer from an employer are a priori in the high segment. Second, young people under the age of 18 and all young people between the ages of 18 and 24 who would have been assigned to the low segment on the basis of their estimated reintegration chances are assigned to the medium segment[1] (see Gamper et al., 2020; Marte-Huainigg, 2020). The profiling is intended to provide the PES caseworkers with a "second opinion" when assessing the needs of their clients. They retain the final decision on segment and program allocation (see Arbeitsmarktservice Österreich (AMS), 2020).

The PES plans to provide less support and a very limited range of measures for jobseekers in the high segment, on the assumption that they will find work quickly anyway. In particular, the costly employment measures of private-sector wage subsidies, direct job creation, training and course subsidies (including vocational orientation) are not planned for this group.[2] Unemployed jobseekers with limited chances of reintegration (low segment) will primarily be offered a new counseling and support program tailored to those with multiple barriers to employment. Those who choose to participate will be referred to an external counseling and support facility that

[1] The reason for this rule is the "training guarantee until 25" introduced in Austria in 2017: Young people under 25 who register with the PES as apprenticeship seekers, have no more than compulsory schooling and are demonstrably unable to find a training place on the training market are guaranteed an in-company or supra-company training place.

[2] The offer of training is limited to training at the future workplace (job-related skills training, work foundations, work practice and preparation for work training, apprenticeship subsidies), job search training, the skilled workers' grant (*Fachkräftestipendium*), wage top-ups to compensate for low wages, subsidies for travel to work, subsidies for one-person businesses, and other subsidies such as childcare subsidies.

provides easy access to a wide array of services, including an ‘open space’, in-depth individual and group counseling on site, activating workshops, and support with skills and health issues. This program prioritizes personal stability over immediate job placement and aims to achieve comparable or even better labor market outcomes at reduced costs (see Böheim et al., 2024).

Most support will be given to unemployed people with medium job prospects. They will receive much more intensive counseling and placement service than jobseekers in the other two segments, and only they will continue to have full access to all active measures (see Arbeitsmarktservice Österreich (AMS), 2020; Marte-Huainigg, 2020).[3] This strategic focus on unemployed people in a medium segment of the labor market is unique internationally. In other countries, profiling has so far mostly been used as an “early warning system” to identify clients at high risk of long-term unemployment at an early stage (see Desiere et al., 2019).

The PES expects that this shift will lead to a more cost-effective use of scarce resources for a number of reasons. First, statistical profiling could help caseworkers to better assess the support needs of their clients. Second, focusing on the medium segment could make counseling more efficient and effective. Outsourcing the intensive in-house counseling of people in the low segment is expected to free up resources. These resources could be better used in the medium segment, for example to discuss the latest job offer or to prepare for a planned training course. Third, the PES hopes to increase the efficiency of the use of active measures. By providing less support to jobseekers with already high job prospects, it hopes to reduce deadweight effects. In the low segment, it hopes to save money by abandoning what it sees as expensive but not very effective employment and training measures and replacing them with low-cost external counseling and support. At the same time, the AMS hopes to increase cost-effectiveness by shifting resources to the medium segment, where it believes the greatest employment effect can be achieved (see Kopf, 2019a, 2019b; Marte-Huainigg, 2020; Szigetvari, 2018).

The PES planned to introduce statistical profiling nationwide in 2021. However, the data protection authority objected to the lack of a sufficient legal basis for the planned data processing. This objection was rejected by the Federal Administrative Court. Following a judicial review of the data protection authority, the decision was referred to the Supreme Administrative Court. However, the court decided that there was a need for further clarification. In the next step, the Federal Administrative Court will have to clarify whether the PES caseworkers’ decision on the allocation of jobseekers is largely determined by the automatically calculated reintegration chances or not. If this is the case, a separate legal basis will have to be created. It is therefore still unclear whether and when the Austrian PES will be able to implement the new profiling (see Allhutter et al., 2020; ORF, 2024).

4 The active labor market programs of interest

In Austria, a wide range of active measures aims at supporting the unemployed in their reintegration into the labor market (cf. Nagl & Moshammer, 2024). We evaluate the four most important and relatively costly training and employment measures that the PES intends to allocate mainly to the medium segment and much less to the unemployed in the low segment.

Training is provided by external training providers, commissioned, and funded by the PES. Participants acquire vocational knowledge and skills, often leading to recognized qualifications such as school-leaving certificates or certified apprenticeships. Some courses offer continuing education

[3] Individuals who belong to a specific target group, as defined in federal program guidelines, will remain eligible, regardless of their segment affiliation (see Marte-Huainigg, 2020)

without formal vocational qualifications, providing entry-level or supplementary skills. The PES covers the cost of training and provides income support to participants throughout.

Course subsidies cover up to 100% of the expenses for unemployed individuals who select courses from the open education market, excluding those funded by the PES. These include fees, materials, exams, clothing, textbooks, and sign language interpretation. Courses focus on specific skills such as office administration or health services, including driving licenses for vehicles and equipment and German language courses. Agreements between applicants and the PES ensure that courses are relevant to the needs of the labor market.

Temporary wage subsidies in the private sector ("integration subsidies") are granted for a maximum of three years to employers hiring long-term unemployed or those at risk of becoming long-term unemployed, covering up to 66.7% of gross monthly wages plus 50% of non-wage costs, potentially rising to 100% during the probationary period. Eligibility is targeted at older workers (women 45+, men 50+), those far from the labor market, the long-term unemployed (under-25-year-olds for at least six months, over-25-year-olds for at least twelve months), or those considered to be at high risk of long-term unemployment (e.g., women returning to the labor market, persons with obsolete labor market skills and persons with health problems). There are no post-subsidy obligations on employers.

Direct job creation in the public and non-profit sectors is designed to provide a bridge to regular employment for disadvantaged people. Participants, who often have limited capacity and multiple barriers to placement, receive temporary subsidized employment in public or non-profit enterprises that operate in the market but serve social needs. A preparatory program precedes the maximum twelve-month transitional job, which can be extended in certain cases. Participants are paid, gain experience in a sheltered environment, and receive training and socio-pedagogical support with the aim of personal stabilization and eventual integration into the regular labor market (cf. Arbeitsmarktservice Österreich (AMS), 2021; Nagl & Moshammer, 2024).

On average, between 2013 and 2017, 3.7% of the eligible, registered unemployed individuals participated in training, 0.6% received course subsidies, 1.7% benefited from wage subsidies and 0.8% benefited from direct job creation (see Figure 7 in the online appendix). Despite declining participation rates, the focus of active labor market policy still lies on training measures. As Figure 3 in the appendix shows, older people are under-represented in training and course subsidies and over-represented in both employment programs. The low-skilled are much more likely to participate in training than in courses on the open labor market, as this program includes basic skills training tailored to people with no more than compulsory schooling, many of whom are foreigners (German courses, literacy courses, etc.). They are strongly overrepresented in direct job creation, but not in the wage subsidy scheme. The long-term unemployed participate relatively often in all programs, those with health restrictions rarely in training and often in the two employment programs, especially in direct job creation.

5 Empirical research design

5.1 Replication of the profiling model

To compare the impact of the four programs across segments, we approximate the division of unemployed clients into segments with high, medium, and low chances of reemployment, as envisaged by the profiling model. This is necessary because no PES calculations are available for research purposes. We simulate the segmentation of the unemployed from 2014 to 2017 by replicating the PES' predictive algorithm for the past.

Specifically, we assign all unemployed individuals in a calendar month to the segment with a high chance of reemployment if they have at least a 55% chance of being in unsubsidized employment for at least three months in the following seven months. Individuals with less than a 30% probability of being in unsubsidized employment for at least six months in the following 24 months are allocated to the low segment, with exceptions[4]: All individuals with a job offer from an employer are assigned to the high segment and individuals aged under age 18 to the medium segment. In addition, individuals aged 18-24 who would have been allocated to the low segment on the basis of their estimated chances of reemployment are moved to the medium segment.

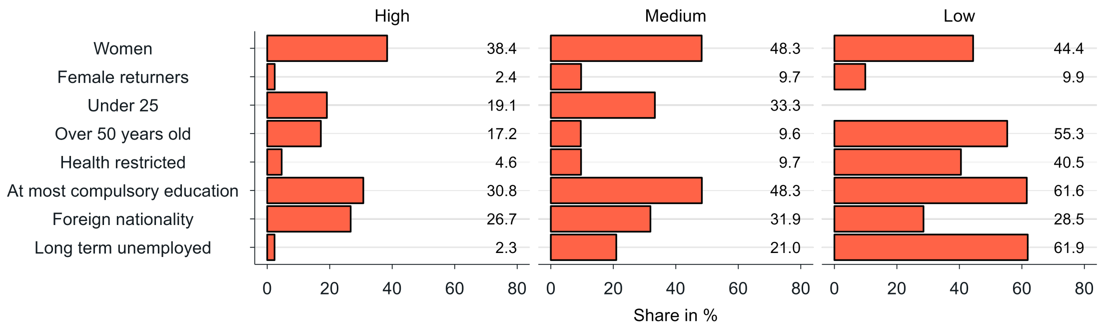
In the original profiling, probabilities are not estimated with regression models; instead, the unemployed are grouped into subpopulations based on their characteristics, and their probability values reflect the proportion of individuals within each subpopulation who have reached the respective employment threshold in the past.[5] We estimate binary logistic regression models based on the same or similar characteristics (see Table 2 in the appendix), separately for the four simulated populations (a basic population with complete data and three subpopulations with incomplete data and therefore adjusted variables). To replicate the separate calculations for different unemployment durations, we interact all explanatory variables with previous unemployment duration. As in the original model, estimates are not made for the unemployed themselves; instead, current jobseekers receive reemployment estimates derived from the previous year's unemployed with the same characteristics ("out-of-sample prediction").

Figure 4 in the appendix shows the distribution of the unemployed from 2014 to 2017 and the participants in the four programs according to our replication of the AMS profiling into the three segments. On average, 27.8% of all unemployed fall into the low segment, 58.8% into the medium segment and 13.3% into the high segment. In particular, the low segment includes many older people, those with health problems and the low-skilled unemployed, almost two-thirds of them are long-term unemployed. Those in the high segment often have education beyond compulsory schooling and rarely face health problems. The medium segment has the highest proportion of women and people under 25. Participants in the direct job creation scheme are over-represented among those in the low segment, while almost three-quarters of participants in training measures come from the medium segment. Unemployed people from the high segment participated relatively rarely in all four programs even before the planned policy change (see Figure 1).

[4] We adjust the selected threshold from 66% to 55% to better replicate the size and composition of the segments formed by AMAS.

[5] An advantage of regression estimates is that they do not depend on proportion values derived from a limited number of observations.

Figure 1: **Characteristics of the unemployed in the three segments according to AMS profiling replication, average 2014-2017**
Share of the respective sub-group in each segment (in %)



Source: Calculations based on AUR and ASSD. Female returners: mothers returning to work after a family-related career break.

5.2 Identification strategy

After replicating the profiling model, we turn to estimating program effects. We identify the causal impacts of participation in the four programs of interest from 2014 to 2017 on participants' labor market ntegration in the five years after program entry, for all participants and specifically for the low and medium segments.

Following the "potential outcomes framework" developed by Splawa-Neyman (1990), Fisher (1935) and Rubin (1974, 1978, 1980), our parameter of interest is the average treatment effect on the treated (ATT), which is the difference between the actual labor market outcomes of program participants and the counterfactual outcomes they would have experienced if they had not participated in the program (cf. Heckman et al., 1999). As the outcomes of non-participants remain unobservable, we use a control group design that compares outcomes between participants and comparable eligible non-participants to estimate the ATT.

To mitigate pre-treatment observable differences between treated and control groups, we apply a dynamic nearest neighbor propensity score matching approach (Rosenbaum & Rubin, 1983), which consists of three steps: First, we estimate the probability of participation in the measure (propensity score) using a logistic regression model that includes a wide range of individual characteristics. Second, we use the derived propensity score to match each participant with up to four similar non-participants ("statistical twins"). To ensure that only very similar individuals are matched, we impose a 'caliper' of 0.8, limiting participant-non-participant pairs to those with propensity score differences within this tolerance threshold. In a third step, we estimate the ATT by comparing outcomes between treated and matched non-treated individuals within the common support.

This matching procedure is applied independently to each of the four programs. We estimate a unique propensity score for each program using an individually tailored logit model and then use it to match program participants to their respective control groups of similar non-participants.

5.3 The counterfactuals

To precisely define the comparison groups, we stratify all program entries into monthly intervals from January 2014 to December 2017. Within each month, we compare unemployed individuals who entered the program (treatment group) with eligible unemployed individuals who did not enter the evaluated program or any other program in that month (control group). This stratification results in numerous subpopulations. We compute propensity scores separately for each stratum, using different sets of controls. We then aggregate all monthly samples to estimate program effects. Individuals in the control group may have participated in the program before or after the month being evaluated. We carefully control for prior participation in all relevant measures, while refraining from conditioning on future participation after the month in question, recognizing it as an outcome (e.g. Sianesi, 2004).

To assess treatment effects, we compare the labor market integration of participants and non-participants at annual cut-off dates in the five years following the month of (hypothetical) program entry. Thus, our follow-up period starts immediately after program entry. Its exact location varies according to the date of program entry, but all individuals are followed for the same length of time before and after this event. This moving window approach allows us to precisely control for the initial conditions at the time of program entry, both at the individual and the macroeconomic level.

Treatment is defined in terms of entry into the program, not necessarily completion, as a measure starts working from entry. Any lock-in effect during the program is thus accounted for in the impact (Sianesi, 2004). By using a dynamic control group design, we explicitly account for the potential future treatment of individuals who have not yet been treated, thus recognizing the non-randomness of treatment depending on the previous duration of unemployment (see Fredriksson & Johansson, 2008; Sianesi, 2004). We account for the timing of treatment in the unemployment spell by comparing only individuals who have been unemployed for the same duration (1-3 months, 4-6 months, 7-12 months, 13-24 months, 25 months and more). Additionally, we include the exact duration of prior unemployment in the matching as a control variable.

We control for a broad set of variables that might affect participation probability as well as outcomes, including sociodemographic and regional characteristics, employment status, participation, and benefit receipt status on the day preceding the month of (hypothetical) program entry. Furthermore, we adjust for differences in labor market history and previous program participation. In terms of outcomes, we compare labor market success up to five years after the (hypothetical) month of program entry between the treatment and control groups in order to measure treatment effects.

5.4 The special case of wage subsidies

In the case of the wage subsidy scheme, we compare participants not only with a control group chosen from all unemployed non-participants (scenario 1), but also with a control group of unemployed who took up a job in the same month, but not a subsidized one (scenario 2). [6] Limiting the analysis to the first scenario is likely to overestimate the true impact of the program for two reasons: First, participation requires a specific job, which raises uncertainty about whether the observable characteristics sufficiently correct for selection bias into employment. Therefore, as argued by Schünemann et al. (2015), some of the observed effects may still reflect the impact of getting a job relative to a control group with a lower probability of moving from unemployment to employment, rather than just the incremental impact of the wage subsidy itself. Second, scenario

[6] See Jaenichen and Stephan (2011) for a similar approach.

1 may overestimate the impact of the program if some jobs would have been created regardless of the incentive of the wage subsidy ("deadweight loss").

The magnitude of deadweight loss remains unobservable. However, by considering both scenarios, we can delineate the range of program effects: While scenario 1 assumes zero deadweight loss (implying that all jobs were created solely because of the subsidy) and assumes that observable characteristics fully capture selection into employment, scenario 2 assumes 100% deadweight loss (where all jobs would have been created regardless of the subsidy). Scenario 2 helps to determine whether the subsidy has affected subsequent labor market outcomes despite not directly inducing job take-up, and thus provides insights into the incremental effect of the wage subsidy over and above the effect on job take-up. Under the plausible assumption that deadweight loss is associated with some, but not all, subsidized jobs, the net program effects after accounting for deadweight should fall within the range established by scenario 1 as the upper bound and scenario 2 as the lower bound.[7]

5.5 Data and variables

Our matching methodology relies on two key identifying assumptions: first, the conditional independence assumption (CIA), which requires that, given the propensity score, treatment assignment and potential outcomes are independent, and second, the common support condition, which necessitates sufficient overlap in the distribution of covariates between the treatment and comparison groups.[8] As we have access to the entire unemployed population in Austria, we benefit from a sufficiently large pool of potential controls and sufficient overlap. In addition, we are confident that we are able to meet the CIA because we are able to combine multiple data sources, thus exploiting a wide range of factors potentially associated with participation and outcomes.

Our evaluation is based on the integration of two administrative data sources. The first is the Austrian Social Security Database (ASSD), a matched firm-employee dataset maintained by the Association of Austrian Social Security Institutions. This database provides comprehensive records of labor market histories on a daily basis going back to 1972, along with monthly earnings data, some demographic characteristics and employer attributes. The second source is the Austrian Unemployment Register (AUR), which provides detailed socio-economic information on all individuals registered with the PES, including participation in labor market programs, transfer payments received and counseling history with the PES. In addition, we include data from Statistics Austria on regional characteristics.

This rich database allows us to control for differences in many socio-demographic characteristics, including gender, age, education, health, marital status, and migrant background. We also adjust for variables such as previous duration of unemployment, time since last job, sector of activity, occupation, earnings from last job, detailed employment histories over 15 years (distinguishing between different employment statuses and economic inactivity), previous receipt of sickness ben-

[7]Eppel et al. (2011) conducted an assessment of the deadweight loss for the period 2003-2006 and found it to be around 50%, indicating that half of the jobs created would have been created anyway without the subsidy. This magnitude would imply that the impact of the program, taking into account the deadweight loss, is in the middle of the estimates derived from the two scenarios.

[8]The stratification by month and previous unemployment entails a large number of propensity score estimates and matchings. Several types of post-matching balancing tests confirm that the propensity score matching procedure chosen balances the distribution of covariates very well in all cases. For example, when estimating the short-term overall impact of training (without restricting the sample to specific program entry years), the median standardized bias suggested by Rosenbaum and Rubin (1985) is 0.0% after matching. The pseudo-R^2 of the logit estimation on the matched samples is 0.000 and the p-value of the likelihood ratio test of the joint significance of all regressors after matching is 1.000. The loss to common support is well below 1% in most cases.

efits, participation in active labor market programs (ALMPs) within specified time periods, PES interactions, current labor market status, receipt of unemployment insurance benefits, and various regional characteristics. These include province, type of economic region, unemployment rate, share of long-term unemployed, annual change in unemployment, benefit receipt rate, labor supply and employment growth, vacancy trends, share of foreign commuters, population density, regional gross domestic product (GDP) and average wage levels. In addition, we consider variables such as the share of seasonal unemployment, the share of older, sick and low-skilled unemployed, the average benefit level, the growth of foreign labor supply and the employment structure (including the respective shares of manufacturing, construction and tourism).

Finally, following Sianesi (2008), we add the local 'program rate', defined as the share of active labor market program (ALMP) participants out of all eligible unemployed without a job offer from an employer. This variable serves as a measure of local program capacity and aims to capture unobserved local dynamics. All covariates are measured prior to program participation. Regional characteristics are collected at the level of local labor market districts, each of which corresponds to one of the 101 regional employment offices. The only exception is gross regional product, which is measured at the NUTS 3 level. The stratification by month and duration of unemployment results in an extraordinarily large number of propensity score estimates.[9] For each estimate, the set of control variables is adjusted separately to ensure optimal matching. The standard variables used to estimate program effects are shown in Table 3 in the online appendix.

Our primary measure of program effectiveness is the proportion of individuals engaged in unsubsidized, dependent, active employment. This excludes individuals with valid employment relationships who are receiving maternity or childcare allowances or are temporarily absent due to reasons such as educational leave. Additionally, it excludes subsidized employment schemes such as wage subsidies, direct job creation, non-profit labor leasing, wage top-up programs, and apprenticeship subsidies. As a supplementary outcome measure, we also consider the proportion of individuals who are engaged in any form of employment, whether actively working or temporarily absent, either as employees or self-employed, and regardless of subsidy status. Moreover, we assess the proportions of individuals who are unemployed or economically inactive at annual cut-off dates. Unemployment is broadly defined to encompass all individuals registered with the PES, including those in PES training. Economically inactive individuals are those who are neither employed nor unemployed.

The duration of our outcome period varies depending on the year of program entry. With data available until 2019, entries from 2014 to 2017 are included in the estimation of one- and two-year effects. Three-year effects correspond to entries from 2014 to 2016, four-year effects to entries from 2014 to 2015, and five-year effects to entries in 2014. Our primary focus is on mid-term effects within a three-year timeframe, which allows us to capture the majority of program years.

5.6 Sample

Our sample includes – with minor exceptions – all persons aged 20 to 59 who were registered as unemployed with the Austrian PES for at least one day in the program start month, were looking for an apprenticeship or were participating in a relevant labor market program. We do not include persons with a job offer because this group is systematically treated differently by the PES. Persons granted asylum or subsidiary protection are not considered because it is too uncertain whether all the characteristics that determine their ex-ante labor market chances are fully and validly recorded in the PES data. Furthermore, we exclude persons who died during the outcome period, as well as

[9]Therefore, we refrain from presenting propensity score estimates.

the few individuals for whom key information is missing: gender, age, highest completed education and the current unemployment spell. We also remove the very few persons who participated in a company-based or supra-company-based subsidized apprenticeship training in the last six months. Due to the small number of cases, it is not possible to control for these program participations.

Each individual is considered only once per month. If there are multiple programs entries in the same month, we prioritize the most relevant one. Typically, we select the longest program episode. If durations are the same, we prioritize by program type, and if durations and program types are the same, we choose the one with the latest start date. For the sake of analysis, we do not include training or open market courses that last less than five days and cost less than €100, as they are unlikely to have a significant impact. In addition, direct job creation programs are only included if participants actually took up a transitional job after a preparatory period. To eliminate probationary periods, we exclude participation in direct job creation and wage subsidy programs that last less than one month.

Our final evaluation sample size varies depending on the program and the length of the follow-up period, as the latter determines which program entry years we can include. For example, our full pooled dataset for the evaluation of training covering all program entries in 2014-2017 encompasses 15,620,512 observations across all monthly subpopulations. Among these, 376,551 observations pertain to the treated individuals. Between just under 4,000 and over 13,000 individuals participated in training per month. The number of control observations ranges from 270,000 to 390,000 per month of hypothetical program start. Table 3 in the online appendix presents summary statistics for the example of training. We use our pooled dataset to compare covariate means between participants and non-participants before matching. The comparison shows, for example of training, that the treated are on average younger and more likely to be female and Austrian than the controls.

6 Results

As Table 1 shows, on average, all four programs increase the probability that the treated unemployed are in unsubsidized, dependent, active employment. Across all segments, training provided by external training providers ("TRAIN") increases the employment share on average by 4.1 percentage points or 10.3%, course subsidies by 6.0 percentage points or 14.3%, and direct job creation by 5.0 percentage points or 18.8% three years after program entry. For the wage subsidy scheme, scenario 1 shows a large increase in the probability of unsubsidized employment of 17.3 percentage points or 40.8%. The estimates for scenario 2, on the other hand, are very small (+0.4 percentage points or +0.6%) and statistically significant only at the 10% level, indicating strong similarities in subsequent labor market trajectories between program participants and non-participants who simultaneously take up a job. Assuming a level of deadweight loss of about 50%, as estimated by Eppel et al. (2011), and thus using the mean of the two scenarios, participation in the wage subsidy program increases the employment probability after three years by 8.8 percentage points, or 20.7% on average. Hence, even with significant deadweight loss, participation clearly improves employment opportunities.

We find no evidence to suggest that labor market policies are generally less effective in the low segment than in the medium segment. On the contrary, both employment programs on average have a stronger impact on jobseekers with low job prospects than on those with medium job prospects. Direct job creation increases the employment share after three years by 4.4 percentage points or 11.3% in the medium segment, while it boosts the employment share by 5.6 percentage points or 30.3% in the low segment. Similarly, under scenario 1, wage subsidies elevate the employment

Table 1: **Program effects on the probability to be in unsubsidized, dependent active employment after 3 years**

	All segments				Medium segment				Low segment			
	T	C	Difference		T	C	Difference		T	C	Difference	
	%	%	PP (SE)	%	%	%	PP (SE)	%	%	%	PP (SE)	%
TRAIN	43.9	39.7	+4.1*** (0.1)	+10.3	50.5	46.0	+4.5*** (0.1)	+9.9	23.8	21.5	+2.3*** (0.2)	+10.8
CS	48.4	42.3	+6.0*** (0.2)	+14.3	53.9	48.3	+5,7*** (0.2)	+11.7	27.6	21.1	+6.5*** (0.4)	+30.9
DJC	31.4	26.5	+5.0*** (0.4)	+18.8	43.1	38.7	+4.4*** (0.6)	+11.3	24.0	18.4	+5.6*** (0.4)	+30.3
WS1	59.5	42.3	+17.3*** (0.2)	+40.8	66.1	47.8	+18.3*** (0.3)	+38.4	49.0	25.1	+23.9*** (0.3)	+95.1
WS2	59.5	59.2	+0.4* (0.3)	+0.6	66.1	65.2	+1.0** (0.3)	+1.5	49.0	49.0	+0.0 (0.6)	+0.1

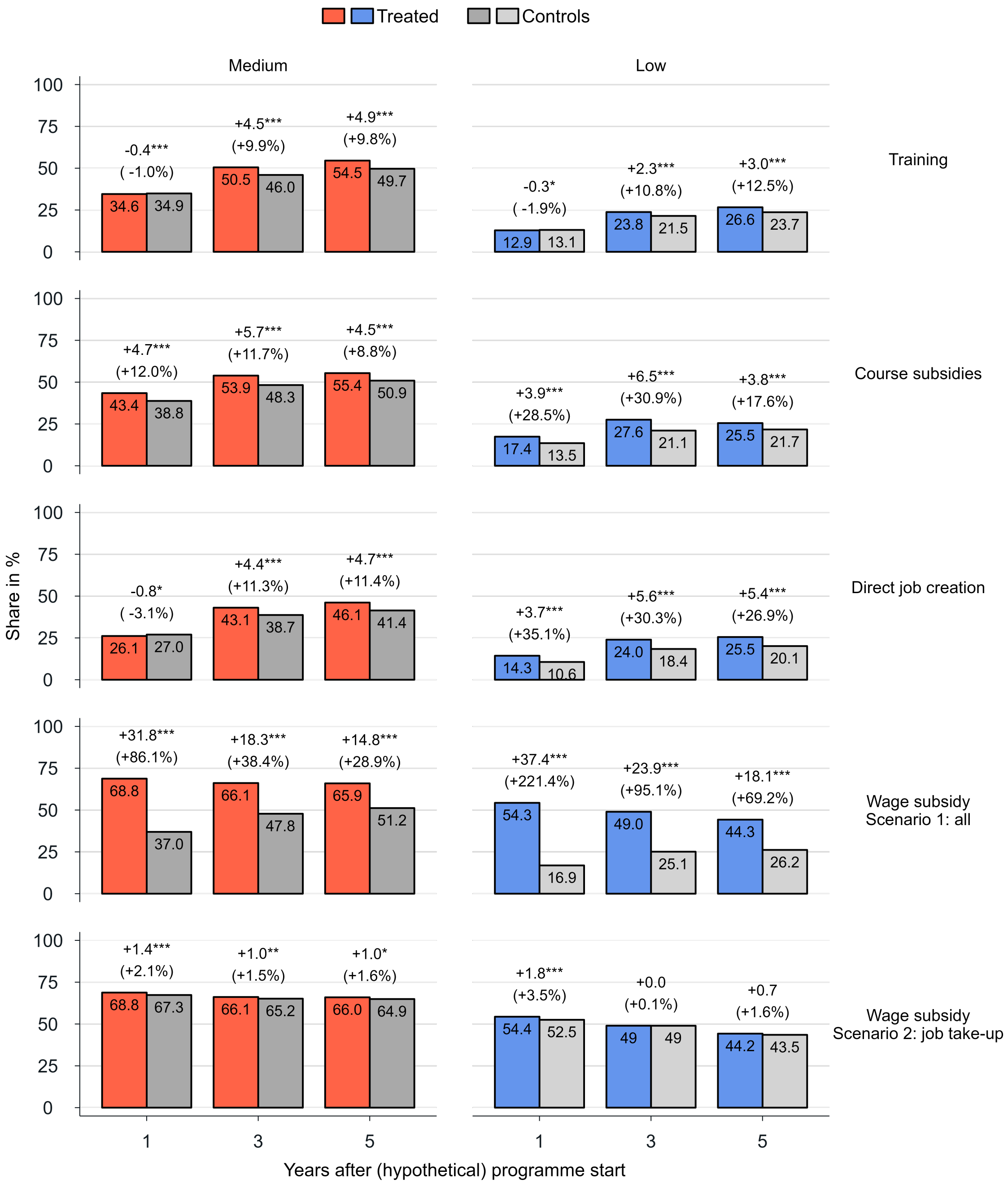
Source: AUR, ASSD. – T: treated. C: controls. Difference: Treatment effect as difference between treated and controls in percentage points (PP) and in %. SE: analytical standard errors as proposed by Abadie and Imbens (2006). TRAIN: Training. CS: Course subsidies. DJC: Direct job creation. WS1: Wage subsidies, scenario 1 (all unemployed). WS2: Wage subsidies, scenario 2 (unemployed with job take-up). *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

probability by 18.3 percentage points or 38.4% in the medium segment, compared to a higher increase of 23.9 percentage points or 95.1% in the low segment. Scenario 2 yields a marginal effect in the medium segment and none in the low segment. Theoretically, the deadweight loss should be higher in the medium segment, as individuals with higher ex-ante employment prospects are more likely to be hired even in the absence of the subsidy. Thus, our finding that wage subsidies are more effective in the low segment should hold regardless of deadweight effects.

The results for the two training programs are less clear, with no consistent evidence of lower effectiveness in the low segment either. External training shows a higher absolute effect in percentage points in the medium segment (4.5 vs. 2.3 percentage points after three years), while showing a slightly higher relative effect in percentage points in the low segment (9.9% vs. 10.8%). Overall, this suggests a slightly higher effectiveness in the medium segment. Subsidies for courses on the open education market yield similarly significant absolute effects in both segments, with the relative effect favoring the low segment. Figure 2 shows the development of the effects over time. It shows the employment shares of the participants (treatment group) and the control group of non-participants on annual cut-off dates after (hypothetical) program entry. The difference between the two groups, i.e., the ATT, is shown in absolute and relative terms. Notably, the absolute effect of course subsidies is slightly higher in the low segment after three years and remains comparable after five years. The relative effect consistently favors the low segment, regardless of the cut-off date.

Figure 5 in the appendix provides additional detail by comparing impacts on different labor market outcomes. It shows that the positive impact of all four programs on employment probability is primarily due to an increase in labor market participation rather than a reduction in unemployment. Participants are less likely to drop out of the labor force than those who do not participate. The figure also shows that training and participation in direct job creation have considerable initial lock-in effects (van Ours, 2004): As long as people participate in the programs, they are less likely to look for a job and are therefore less likely to be employed. Over time, this negative initial lock-in effect is overcompensated by improved employment opportunities.

Figure 2: **Program effects on the share of the treated in unsubsidized, dependent active employment by segment**



Source: AUR, ASSD, Statistics Austria, and own calculations. – In the bars: average share of treated and controls. Above the bars: Treatment effect as difference between treated and controls in percentage points and (in parentheses) in %. Statistical significance based on analytical standard errors as proposed by Abadie and Imbens (2006). *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

Finally, Figure 6 in the appendix compares program effects across many subgroups of the unemployed within the segments. It shows that the differences between the segments are very consistent across different subgroups of the unemployed. However, in terms of effect size, it also reveals a pronounced effect heterogeneity even within the two segments.

7 Conclusions

We have evaluated a profiling model in Austria that aims to target ALMPs based on predicted reemployment prospects (low, medium, high). Using a unique setting for causal analysis, we find that shifting resources from the low to the medium-chance segment, as envisaged by the PES, would not lead to the desired efficiency gains. Employment programs have a stronger impact on unemployed jobseekers with low job prospects than on those with medium prospects, and training programs are not consistently less effective in the low-chance segment either. These findings suggest that the new profiling and targeting strategy is unlikely to produce the desired efficiency gains, as resources would be allocated less than before to those who benefit more from them. Only in the case of training provided by external training providers could a shift potentially lead to some efficiency gains.

A reallocation of resources from the low-chance to the medium-chance segment would risk creating a "Matthew effect" in access to and use of ALMPs (Bonoli & Liechti, 2018), as it would favor people who already have better chances of re-employment and disadvantage those most in need of support. Instead, our findings highlight the importance of maintaining a focus on the most disadvantaged jobseekers, both from an efficiency and equity perspective. This would also be in line with the current trend in European countries to refocus ALMPs towards vulnerable groups, and to specifically enhance and innovate instruments for the long-term unemployed and individuals facing multiple obstacles to employment after the COVID-19 crises (Duell, 2023).

In sum, our results suggest that while statistical profiling has the potential to improve the efficiency of ALMPs, this depends heavily on its intended use and specific configuration. Specifically, our findings caution against over-simplistic profiling methods and underscore the need for nuanced targeting strategies. The classification into three groups appears to be too coarse, as we observe significant heterogeneity in program effects even within the two segments created by the algorithm. This observation is consistent with numerous evaluations of ALMPs in other OECD countries, which demonstrate substantial differences in program effects across various subgroups of the unemployed (see e.g. Card et al., 2010, 2018; Kluve, 2010, 2014). The unemployed population is inherently diverse, and even within subgroups, such as women or men, treatment effects are likely to vary.

Moreover, our analysis underlines the importance of distinguishing between profiling and targeting. Even if a profiling model accurately predicts labor market prospects and identifies risk groups, it is not necessarily suitable for targeting, but only if the profiling variable is also systematically related to the net effects of the treatment to be allocated. Efficiency targets can only be achieved effectively if both conditions are met (Berger et al., 2001; Körtner & Bonoli, 2023). Our findings thus highlight the importance of thoroughly investigating the relationship between profiling and program impacts prior to implementation. As predicted outcomes and treatment effects do not necessarily correlate, profiling and targeting systems that are directly based on program impacts may be more appropriate than those that rely solely on predicted outcomes (cf. McCall et al., 2016).

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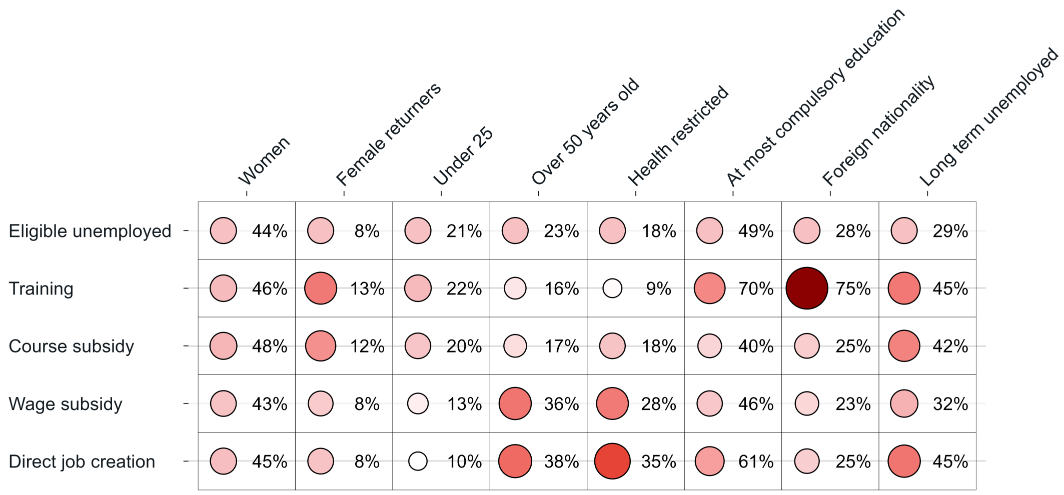
8 Appendix

Table 2: **Comparison of the characteristics used for AMAS and for its replication**

Characteristics	**Categories** **AMAS**	**Replication**
Personal characteristics		
Gender	Male, female	Like AMAS
Age group	Under 30 years, 30 to 49 years, 50 years and older	Like AMAS
Country group	Austria, other EU countries, third countries	5 instead of 3 groups: additionally EU15, EU28 and guest worker countries
Education	At most compulsory school, apprenticeship or vocational secondary school, school-leaving certificate and higher	5 instead of 3 groups: Apprenticeship and vocational secondary school separately, school-leaving certificate and university degree separately
Health restrictions	No, yes	Like AMAS
Responsibility for childcare	No, yes	Woman with child under 15: yes/no
Employment history characteristics		
Occupational group	Production occupation, service occupation	Like AMAS
Employment history	At least 75% employment days in the last four years, less than 75% employment days	Period of 5 instead of 4 years
Number of unemployment episodes	No episode in one of the last 4 years, one episode in one of the last 4 years, at least one episode in two of the last 4 years, at least one episode in three or four of the last four years	Like AMAS
Unemployment spell duration	No spell longer than six months/at least one spell longer than six months	Like AMAS
ALMP participation	No ALMP participation, participation in at least one program/at least one training program/at least one employment program	Like AMAS
Characteristics of current unemployment		
Region type	5 types based on local labor market conditions in the home labor market district	AMAS region types of 2017
Elapsed unemployment duration	3, 6, 9, 12, 15, 18, 21, 24, 30, 36 or 48 and more months	Like AMAS

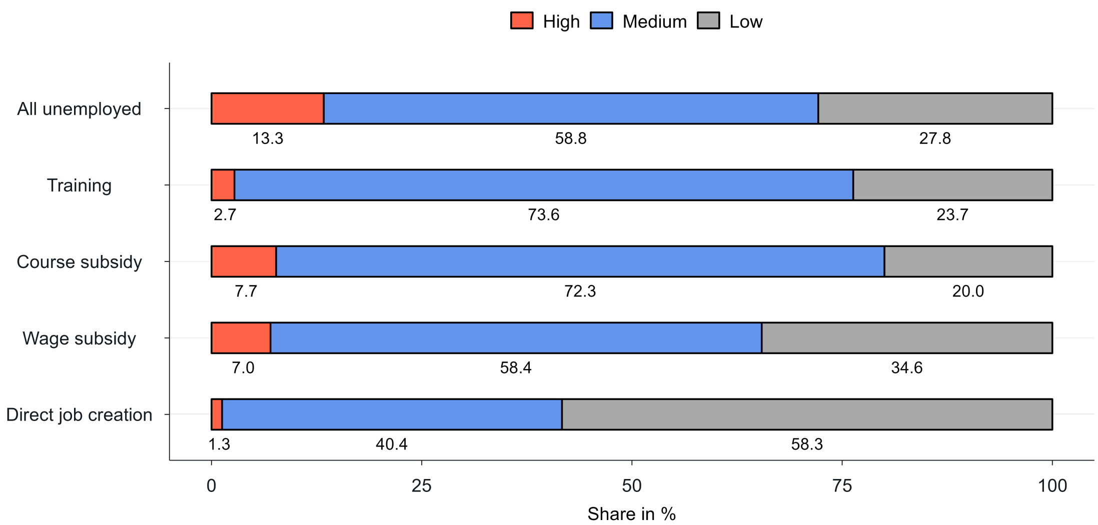
Source: Allhutter et al. (2020), Gamper et al. (2020), and Marte-Huainigg (2020). Characteristics measured at the end of the previous month, i.e., the day before the month of (hypothetical) program entry.

Figure 3: **Share of different population groups in the eligible unemployed and program participants (in %), 2014-2017**



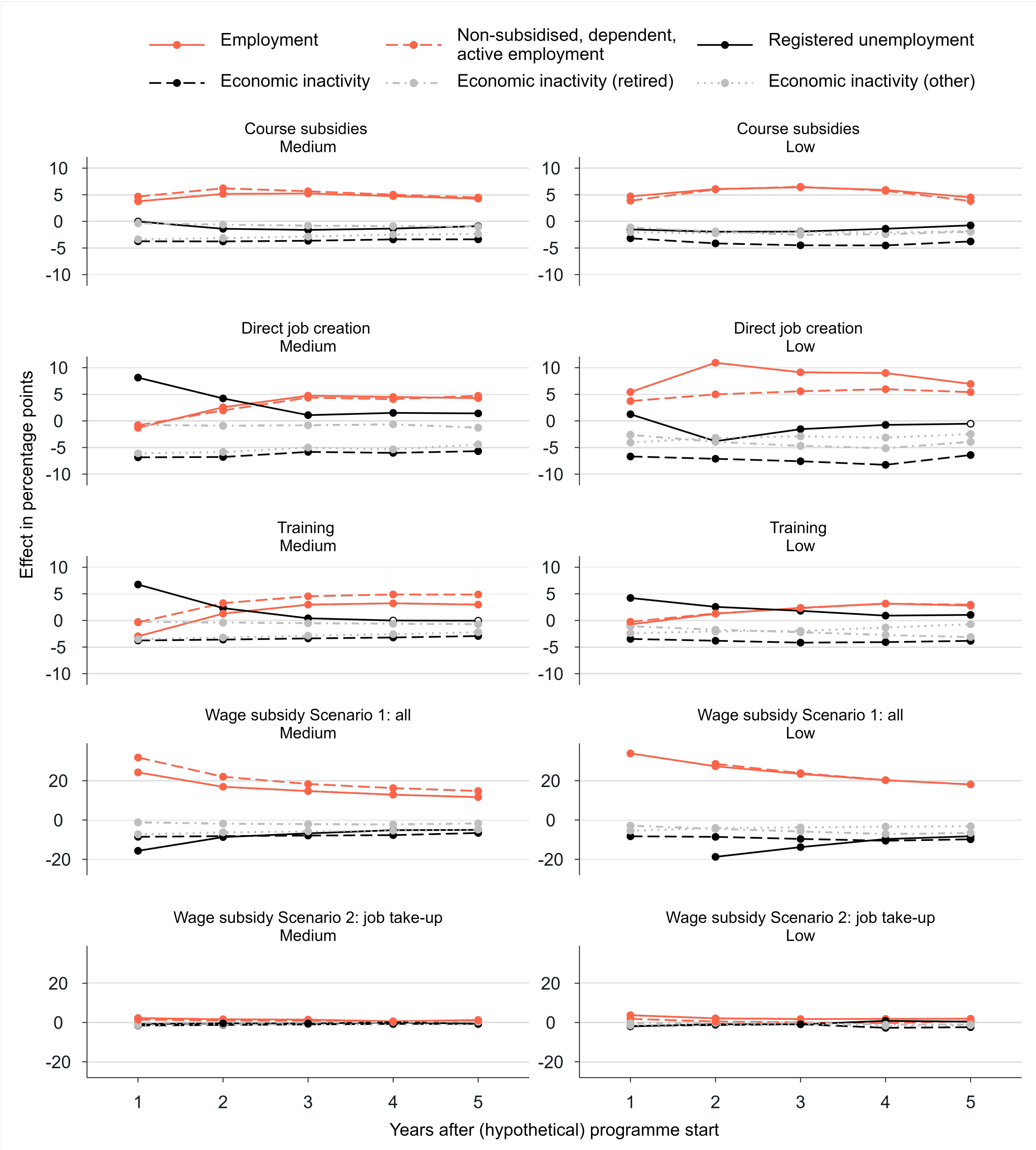
Source: Calculations based on AUR and ASSD. Eligible unemployed: Persons who were (for at least one day) registered as unemployed, looking for an apprenticeship, in PES training or a relevant ALMP measure for the unemployed in a given calendar month from 2014 to 2017. Program participants: Persons with program entry in the respective month. Female returners: mothers returning to work after a family-related career break. Average over all months.

Figure 4: **Distribution of unemployed and program participants by segment, 2014-2017**
Results of the profiling replication



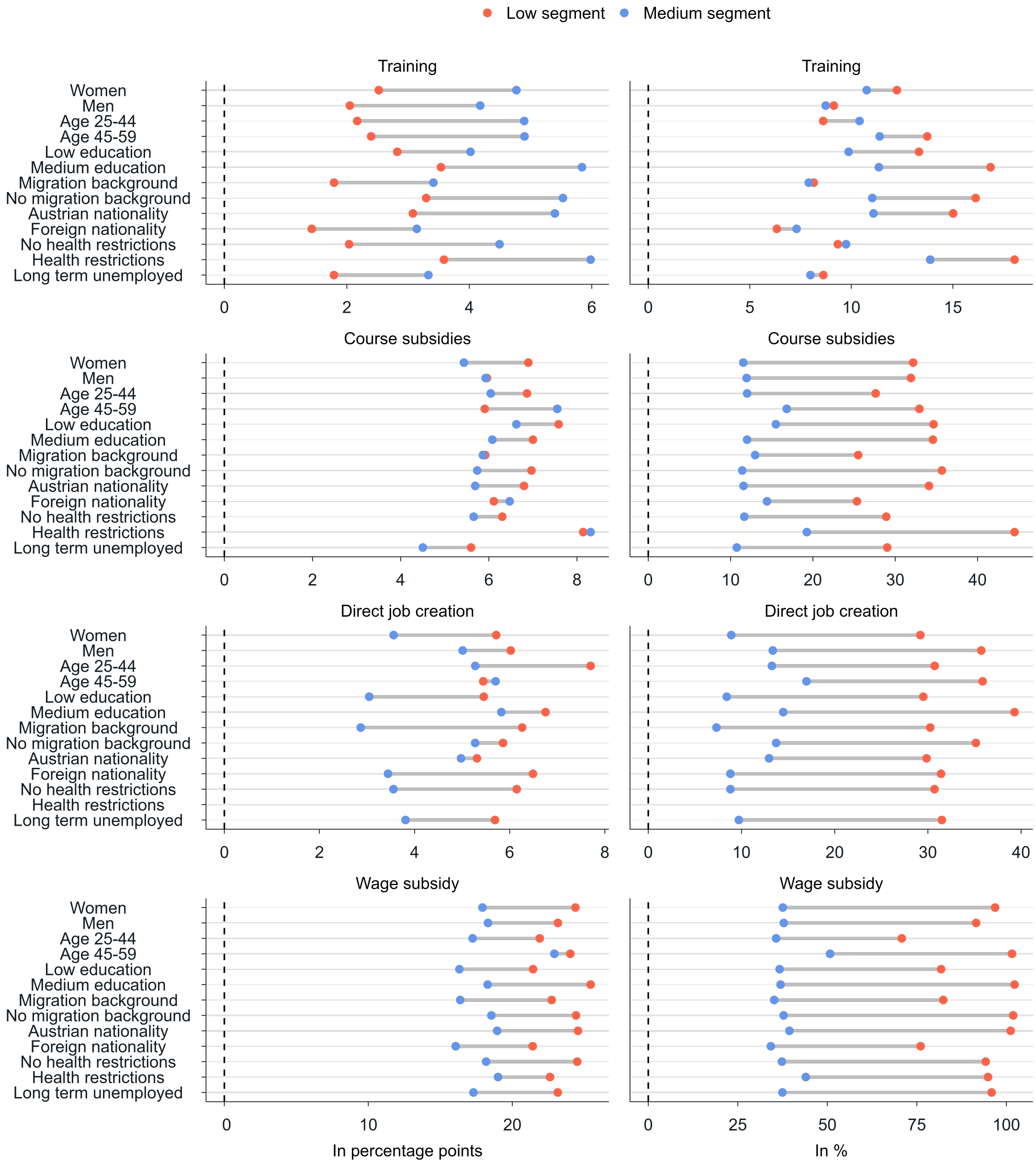
Source: Calculations based on AUR and ASSD.

Figure 5: **Program effects on the share of the treated in various labor market positions by segment**



Source: AUR, ASSD, Statistics Austria, and own calculations. Marker dots correspond to the average effect in percentage points (difference in average share between treated and controls). Without filling, if statistically insignificant at 10% error level. Statistical significance based on analytical standard errors as proposed by Abadie and Imbens (2006).

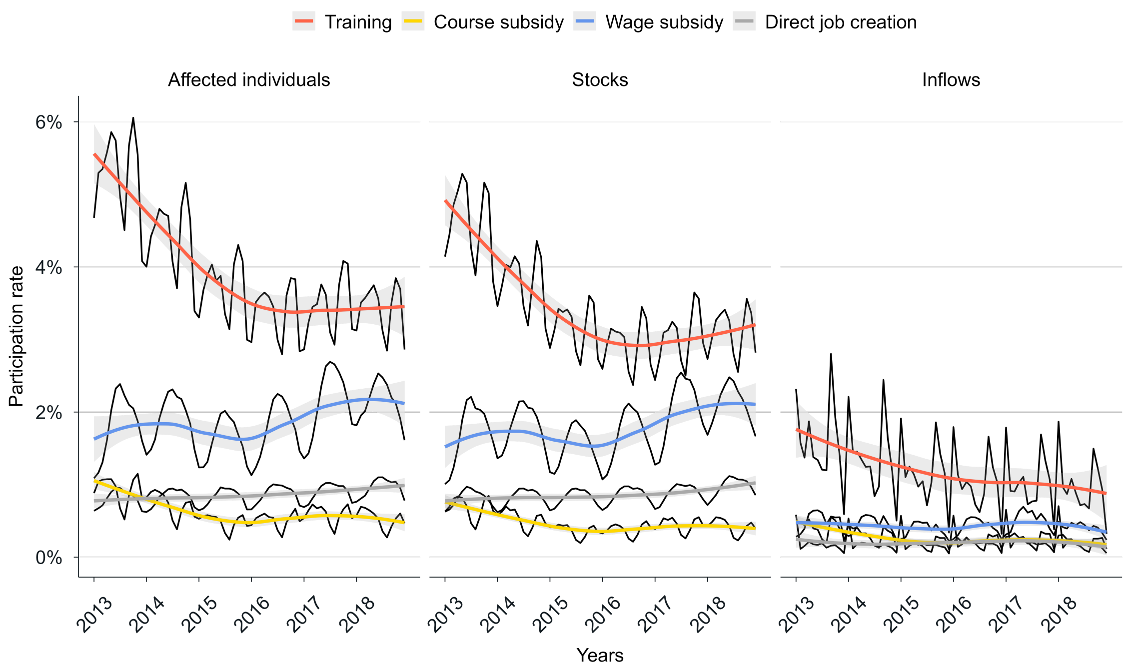
Figure 6: **Program effects on the share of the treated in unsubsidized, dependent active employment after 3 years, by population group within the segments**



Source: AUR, ASSD, Statistics Austria, and own calculations. Low education: at most compulsory school leaving certificate. Medium education: apprenticeship, vocational middle school. Wage subsidy: scenario 1. No estimates due to small sample size: high education, female returners, direct job creation by health restrictions. All effects are significant at the 1% level (different from zero). Statistical significance based on analytical standard errors as proposed by Abadie and Imbens (2006).

9 Online appendix

Figure 7: **Program participation rates**



Source: Calculations based on AUR and ASSD. Affected individuals: Number of individuals participating for at least one day in a calendar month, divided by the number of individuals unemployed or participating for at least one day in the month. Stocks: Number of individuals participating for at least one day in a calendar month divided by the number of individuals unemployed or participating for at least one day in that month, both weighted by the respective number of days. Inflows: Number of individuals with program entry in a calendar month, divided by the number of individuals unemployed or participating for at least one day in that month. Average over twelve calendar months. Light gray area indicates 95% confidence interval.

Table 3: **Descriptive sample characteristics by treatment status (before matching), training**

	Mean			t-test	
	T	C	Diff.	p>|t|	
Month of elapsed unemployment					
1^{st}	0.096	0.238	-0.142	0.000	***
2^{nd}	0.075	0.071	0.004	0.000	***
3^{rd}	0.079	0.066	0.012	0.000	***
4^{th}	0.070	0.052	0.018	0.000	***
5^{th}	0.078	0.049	0.029	0.000	***
6^{th}	0.058	0.037	0.021	0.000	***
7^{th}	0.050	0.034	0.016	0.000	***
8^{th}	0.047	0.033	0.015	0.000	***
9^{th}	0.038	0.027	0.012	0.000	***
10^{th}	0.036	0.025	0.011	0.000	***
11^{th}	0.032	0.022	0.010	0.000	***
12^{th}	0.031	0.021	0.010	0.000	***
13^{th}	0.025	0.018	0.007	0.000	***
14^{th}	0.022	0.016	0.005	0.000	***
15^{th}	0.021	0.017	0.004	0.000	***
16^{th}	0.017	0.014	0.003	0.000	***
17^{th}	0.017	0.014	0.003	0.000	***
18^{th}	0.015	0.012	0.002	0.000	***
19^{th}	0.013	0.012	0.002	0.000	***
20^{th}	0.013	0.012	0.001	0.000	***
21^{th}	0.011	0.010	0.001	0.006	***
22^{th}	0.010	0.010	0.000	0.446	
23^{th}	0.009	0.009	0.000	0.197	
24^{th}	0.009	0.009	0.000	0.561	
$\geq 25^{th}$	0.128	0.171	-0.043	0.000	***
Female	0.525	0.452	0.073	0.000	***
Age (in years)	35.063	39.309	-4.246	0.000	***
Formal education level					
At most compulsory school	0.506	0.463	0.043	0.000	***
Apprenticeship	0.247	0.314	-0.067	0.000	***
Intermediate vocational school	0.055	0.052	0.003	0.000	***
Higher academic or vocational school	0.121	0.102	0.019	0.000	***
Academic education	0.072	0.069	0.003	0.000	***
Single	0.527	0.552	-0.025	0.000	***
Family-related returner to workforce (only women)	0.123	0.099	0.024	0.000	***
Number of children (only women)					
0.000	0.811	0.776	0.035	0.000	***
1.000	0.094	0.101	-0.007	0.000	***
2.000	0.066	0.081	-0.015	0.000	***
$\geq$3	0.028	0.042	-0.014	0.000	***
Age of the youngest child (years)					
$\leq$2	0.029	0.027	0.002	0.000	***
3-7	0.060	0.055	0.005	0.000	***
8-10	0.020	0.022	-0.002	0.000	***
11-15	0.027	0.031	-0.004	0.000	***
$\geq$16	0.052	0.089	-0.036	0.000	***
Nationality					
Austria	0.547	0.731	-0.184	0.000	***
EU15 (without Austria), Switzerland	0.104	0.106	-0.002	0.004	***
EU2004/2007-member state	0.031	0.031	0.000	0.269	
Turkey, former Yugoslavia (without Slovenia)	0.129	0.079	0.050	0.000	***
Others	0.189	0.054	0.135	0.000	***
Migration background	0.578	0.408	0.170	0.000	***
Naturalized	0.112	0.121	-0.009	0.000	***

	T	Mean C	Diff.	t-test p>|t|	
Health-related placement restriction					
Legal disability status	0.112	0.166	-0.054	0.000	***
Other health-related employment limitation	0.019	0.036	-0.017	0.000	***
Economic sector of last employment					
Agriculture, mining	0.005	0.006	-0.001	0.000	***
Manufacturing	0.075	0.089	-0.015	0.000	***
Energy and water supply	0.003	0.004	-0.002	0.000	***
Construction	0.049	0.083	-0.034	0.000	***
Trade	0.153	0.155	-0.002	0.000	***
Transport and logistics	0.034	0.048	-0.014	0.000	***
Accommodation and gastronomy	0.117	0.125	-0.008	0.000	***
Information and communication, financial and insurance service provider, real estate and housing	0.031	0.039	-0.008	0.000	***
Freelance, academic, technological services	0.030	0.036	-0.006	0.000	***
Other economical service	0.184	0.192	-0.008	0.000	***
Public service	0.108	0.135	-0.027	0.000	***
Other services	0.040	0.045	-0.005	0.000	***
Others, unknown	0.172	0.043	0.129	0.000	***
Last occupation					
Professionals					
Academic professions	0.052	0.057	-0.006	0.000	***
Armed forces occupations	0.001	0.001	0.000	0.074	*
Plant and Machine Operators and Assemblers	0.044	0.068	-0.024	0.000	***
Office workers and related occupations	0.106	0.089	0.017	0.000	***
Services and Sales Workers	0.231	0.219	0.012	0.000	***
Skilled Agricultural, Forestry and Fishery Workers	0.004	0.005	-0.001	0.000	***
Managers	0.021	0.032	-0.010	0.000	***
Craft and Related Trades Workers	0.112	0.138	-0.026	0.000	***
Elementary Occupations	0.328	0.289	0.039	0.000	***
Technicians and Associate Professionals	0.083	0.094	-0.011	0.000	***
In PES training at end of previous month	0.065	0.014	0.051	0.000	***
Unemployment insurance benefit receipt					
Unemployment benefit	0.322	0.273	0.049	0.000	***
Unemployment assistance	0.344	0.398	-0.054	0.000	***
Other benefit	0.040	0.026	0.015	0.000	***
Unemployment insurance benefit level (per day in €)					
$\leq$5	0.031	0.021	0.010	0.000	***
$\leq$10	0.030	0.021	0.009	0.000	***
$\leq$20	0.125	0.111	0.014	0.000	***
>20	0.520	0.544	-0.024	0.000	***
No benefit	0.294	0.303	-0.009	0.000	***
Employment history: days in last 2 years					
Active unsubsidized dependent employment	202.612	236.288	-33.676	0.000	***
Active subsidized dep. employment 1st labor market	4.715	5.992	-1.277	0.000	***
Active subsidized dep. employment 2nd labor market	9.587	8.865	0.722	0.000	***
Temporary absence	24.695	21.000	3.695	0.000	***
Self-employment	6.525	9.402	-2.877	0.000	***
Registered unemployment	261.103	304.096	-42.993	0.000	***
PES training	68.165	34.063	34.103	0.000	***
Other unemployment status	5.100	3.140	1.960	0.000	***
Out of labor force and not socially insured	51.611	29.325	22.285	0.000	***
Employment history: days in last 5 years					
Dependent employment	710.000	799.243	-89.243	0.000	***
Self-employment	25.000	34.563	-9.563	0.000	***
Unemployment (incl. PES training, apprenticeship search)	540.000	641.036	-101.036	0.000	***
Other unemployment status	9.746	7.390	2.356	0.000	***
Out of labor force and not socially insured	260.000	112.661	147.339	0.000	***

	Mean			t-test	
	T	C	Diff.	p>|t|	
Employment history: days in last 15 years					
Dependent employment	1,782.284	2,358.286	-576.002	0.000	***
Self-employment	73.226	119.262	-46.037	0.000	***
Unemployment (incl. PES training, apprenticeship search)	850.686	1,185.289	-334.603	0.000	***
Other unemployment status	19.115	20.238	-1.124	0.000	***
Out of labor force and not socially insured	1,578.298	827.642	750.656	0.000	***
Employed at cut-off dates					
3 months ago	0.173	0.291	-0.118	0.000	***
6 months ago	0.297	0.361	-0.064	0.000	***
1 year ago	0.396	0.416	-0.020	0.000	***
2 years ago	0.442	0.468	-0.026	0.000	***
Unemployed at cut-off dates (incl. PES training, apprenticeship search)					
3 months ago	0.710	0.589	0.121	0.000	***
6 months ago	0.538	0.508	0.030	0.000	***
1 year ago	0.388	0.435	-0.047	0.000	***
2 years ago	0.270	0.352	-0.082	0.000	***
Past sick pay receipt (days)					
During dependent employment in last 2 years	6.086	7.859	-1.773	0.000	***
During dependent employment in last 15 years	11.829	18.164	-6.335	0.000	***
During unemployment in last 2 years	14.355	26.106	-11.750	0.000	***
During unemployment in last 15 years	28.364	59.675	-31.312	0.000	***
Time since last job					
0	0.030	0.108	-0.078	0.000	***
≤90	0.158	0.193	-0.035	0.000	***
≤180	0.149	0.108	0.041	0.000	***
≤366	0.154	0.116	0.038	0.000	***
>366	0.294	0.364	-0.070	0.000	***
No job	0.215	0.110	0.105	0.000	***
Income in last job (in €)					
≤1,000	0.306	0.305	0.001	0.202	
1,000-1,500	0.201	0.210	-0.009	0.000	***
1,500-2,000	0.142	0.169	-0.027	0.000	***
2,000-2,500	0.081	0.110	-0.029	0.000	***
>2,500	0.054	0.096	-0.041	0.000	***
None	0.215	0.110	0.105	0.000	***
Active labor market policy participation in last quarter					
Job search training	0.020	0.018	0.001	0.000	***
Vocational orientation	0.061	0.015	0.046	0.000	***
Vocational training	0.110	0.032	0.078	0.000	***
Support measure	0.262	0.125	0.137	0.000	***
Active labor market policy participation in penultimate quarter					
Job search training	0.029	0.020	0.009	0.000	***
Vocational orientation	0.044	0.018	0.026	0.000	***
Vocational training	0.126	0.045	0.081	0.000	***
Support measure	0.173	0.107	0.066	0.000	***
Active labor market policy participation in last half-year					
Private-sector wage subsidies or wage top-up scheme	0.016	0.021	-0.005	0.000	***
Direct job creation or non-profit labor leasing	0.022	0.025	-0.003	0.000	***
Course subsidies	0.030	0.023	0.007	0.000	***
Active labor market policy participation in last two years					
Private-sector wage subsidies or wage top-up scheme	0.054	0.066	-0.012	0.000	***
Direct job creation	0.029	0.035	-0.006	0.000	***
Non-profit labor leasing	0.046	0.047	-0.001	0.007	***
Job search training	0.104	0.096	0.008	0.000	***
Vocational orientation	0.140	0.071	0.069	0.000	***
Vocational training	0.307	0.154	0.153	0.000	***
Course subsidies	0.082	0.068	0.015	0.000	***

	T	Mean C	Diff.	t-test p>|t|	
External counseling	0.391	0.247	0.144	0.000	***
Active labor market policy participation in last four years (days)					
Private-sector wage subsidies or wage top-up scheme	8.995	12.647	-3.652	0.000	***
Direct job creation	6.747	8.406	-1.659	0.000	***
Non-profit labor leasing	6.881	7.074	-0.193	0.000	***
Job search training	6.568	7.078	-0.510	0.000	***
Vocational orientation	10.193	6.857	3.336	0.000	***
Vocational training	55.427	29.434	25.993	0.000	***
Course subsidies	7.864	7.380	0.484	0.000	***
External counseling and support	74.371	60.435	13.936	0.000	***
PES meetings in last half-year					
0.000	0.053	0.172	-0.119	0.000	***
1.000	0.146	0.151	-0.005	0.000	***
2.000	0.219	0.207	0.012	0.000	***
≥2	0.582	0.471	0.111	0.000	***
PES meetings in last 2 years					
0	0.025	0.092	-0.066	0.000	***
1-4	0.299	0.254	0.045	0.000	***
5-8	0.275	0.231	0.044	0.000	***
>8	0.401	0.423	-0.022	0.000	***
PES placement offer in last half-year	0.486	0.445	0.041	0.000	***
PES placement offer in last 2 years					
0.000	0.355	0.362	-0.007	0.000	***
1	0.125	0.119	0.006	0.000	***
2-5	0.256	0.247	0.009	0.000	***
6-10	0.132	0.132	0.000	0.414	
>10	0.132	0.140	-0.008	0.000	***
Federal state (Bundesland)					
Burgenland	0.022	0.028	-0.006	0.000	***
Carinthia	0.061	0.068	-0.008	0.000	***
Lower Austria	0.104	0.169	-0.065	0.000	***
Upper Austria	0.123	0.117	0.006	0.000	***
Salzburg	0.029	0.042	-0.014	0.000	***
Styria	0.086	0.125	-0.039	0.000	***
Tyrol	0.035	0.060	-0.025	0.000	***
Vorarlberg	0.020	0.030	-0.010	0.000	***
Vienna	0.521	0.361	0.160	0.000	***
Regional characteristics at labor market district level (monthly data)					
Economic region type					
Metropolitan area	0.521	0.361	0.160	0.000	***
City	0.107	0.158	-0.051	0.000	***
Suburban	0.054	0.088	-0.034	0.000	***
Medium sized town	0.085	0.106	-0.021	0.000	***
Intensive industrial region	0.075	0.090	-0.015	0.000	***
Intensive touristic region	0.027	0.042	-0.015	0.000	***
Extensive industrial region	0.069	0.073	-0.005	0.000	***
Touristic periphery	0.020	0.032	-0.012	0.000	***
Industrial periphery	0.043	0.049	-0.007	0.000	***
Unemployment rate	0.115	0.105	0.010	0.000	***
Share of long-term unemployed in the unemployed	0.355	0.342	0.013	0.000	***
Share of unemployed with hiring promise among the unemployed	0.100	0.118	-0.018	0.000	***
Relative change in unemployment to previous year	0.051	0.039	0.012	0.000	***
Share of unemployed with unemployment insurance benefit	0.872	0.880	-0.008	0.000	***
Population density (inhabitants per square kilometer)	2,383.647	1,710.120	673.527	0.000	***
Relative change in employment to previous year	0.013	0.013	-0.001	0.000	***
Growth of labor supply	0.068	0.069	-0.001	0.000	***

	Mean T	C	Diff.	t-test p>|t|	
Share of commuters from abroad in the workforce	0.035	0.039	-0.003	0.000	***
Average annual gross salary of year-round full-time employees (in €)	47,892.340	47,690.700	201.640	0.000	***
Gross regional product (GRP) per inhabitant (in €)	43,559.980	42,252.340	1,307.640	0.000	***
Program rate	32.133	30.280	1.854	0.000	***

Source: AUR, ASSD, Statistics Austria and own calculations. Share of commuters from abroad in the active workforce with place of work in the respective region. Gross regional product (GRP) per inhabitant (in €) at current prices. Program rate: persons with at least one day of participation in a relevant ALMP measure as a proportion of all persons with at least one day of unemployment or program participation and no job promise from an employer in the respective month. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.