Peer Effects in Labor Market Training*

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This paper shows that group composition shapes the effectiveness of labor market training programs for jobseekers. Using rich administrative data from Germany and a novel measure of employability, we find that participants benefit from exposure to more employable peers through increased long-term employment and earnings. Effects vary by own employability: low-employability jobseekers gain somewhat more in the long run in terms of employment stability and cumulative earnings, while high-employability individuals benefit more through higher entry wages. An analysis of mechanisms suggests that within-group competition in job search attenuates part of the positive peer effects that operate through knowledge spillovers, and that peer effects vary with the tightness of the labor market.

Keywords: peer effects, labor market training, vocational training

JEL Codes: H52, J64, J68

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1 Introduction

Technological and demographic change requires continuous (re)training of the workforce. This involves labor market training programs that are partly or fully publicly funded. If implemented effectively, these programs may overcome structural mismatches and alleviate labor shortages by providing jobseekers with the skills that are in demand. One possible, but so far understudied determinant of the program effectiveness, is the group composition in these programs. Labor market training for unemployed workers, which is the focus of this study, combines two contexts for which positive peer effects have been documented: education and job search. A large literature shows that peer ability is a significant driver of student achievement and labor market outcomes (see Sacerdote, 2011 and Paloyo, 2020 for overviews). In job search, there is evidence that social networks facilitate information transmission, reduce search frictions, and improve match quality between firms and workers (e.g., Ioannides and Loury, 2004). However, in combination, these contexts might also give rise to negative peer effects, making the direction of the overall effect unclear. On the one hand, jobseekers might benefit from existing networks or skills of more employable peers and find jobs more easily or better ones. On the other hand, they might lose out against better peers and face more competition in job-search. Finally, peer effects might depend on the macroeconomic context within which the program takes place. The within-group competition might be fiercer if jobs are scarce.[1]

Designing effective training programs is of great economic relevance, as public spending on vocational training remains high in many industrialized countries (up to 1.5% of GDP, OECD, 2023). This paper shows that peer quality is an important feature of

[1]The literature quantifying spillover effects of labor market policies shows that such spillovers to non-participants are larger if labor markets are weak (e.g., Crépon et al., 2013). Lazear et al. (2018) show that competition between jobseekers shapes their labor market success when job slots are fixed.

labor market training programs, influencing individual labor market outcomes through different mechanisms. The findings suggest that policy makers might improve program effectiveness by strategically shaping group composition.

The setting for our analysis is publicly sponsored training for jobseekers in Germany. Germany provides an ideal setting for studying peer effects in labor market training programs, as these programs are widely used and similar to those in many other developed countries such as Austria, the Netherlands, Switzerland, and the United States. Specifically, we investigate how the labor market outcomes of program participants are affected by the employability of their peers in the same course. Employability is defined as the (ex-ante) predicted probability to find a stable employment (i.e. a job contract lasting for more than six months) within one year after program start. It is estimated using a lasso logit model and summarizes a rich set of individual and labor market characteristics in a single score. To establish causality, we rely on idiosyncratic variation in the group composition within courses of the same competence level, within the same occupation group, and offered by the same training provider over time. We control for dynamic sorting into peer groups with aggregate time trends that vary by occupation group. We further test the robustness of the results to various alternative specifications.

The analysis is based on rich administrative data on the universe of job-seeking individuals who participate in publicly sponsored training programs in the years 2007 to 2012. We distinguish between three program types – short training, long training, and retraining – but the assumptions underlying our identification strategy seem most plausibly satisfied for short training programs, which are also the most common, accounting for 60% of participants. While we report results for all program types for transparency, we focus our discussion on the results for short programs.

We document the following results. First, we find that attending training with more employable peers has no effects on the job search duration for the average program participant, but it moderately improves days spent in employment in the long-run. A one-standard-deviation increase in the average peer employability increases employment by around 13 days within five years after program start. We find more pronounced effects on earnings: earnings in the first job increase by 3% due to a one-standard-deviation increase in peer employability, and cumulative earnings over the course of five years increase by around 4-5% (or approximately 2,300 euros in 2010 terms).

Second, we find that jobseekers with an individual employability below the median draw higher long-term benefits from exposure to highly employable peers compared to jobseekers with an employability above the median. In particular, we document peer effects on employment only for less employable people, and peer effects on long-term earnings are about one percentage point higher for less employable jobseekers. Instead, in the short-term, peer effects on entry wages are four times larger for highly employable people.

Third, we disentangle the effect of peer employability through knowledge spillovers from the within-group competition effect in job search. We follow the recent peer-effects literature and add a variable measuring the ordinal rank within the group to our model. We identify knowledge spillovers from peers' mean employability and competition from a jobseeker's rank. Our results suggest that competition attenuates part of the peer effects that operate through knowledge spillovers. In particular, controlling for rank leads to estimates for the effect of peer employability on earnings that are 50% higher. Finally, we find that spillovers and competition effects are larger in non-tight labor markets, suggesting that peer effects play a more important role when jobs are scarce.

The paper makes several contributions. It is the first to systematically analyze peer effects on labor market outcomes in large scale training programs using rich administrative data. While there is a large literature studying the direct effects of such programs (Card et al., 2010, 2018), the existing evidence on peer effects in active labor market policies (ALMP) is limited to a few experimental studies that come to different conclusions. Lafortune et al. (2018) analyze a training program for low-skilled Chilean women and find no robust evidence for peer effects. Van den Berg et al. (2019) evaluate a job search assistance program for young unemployed workers in France and find that average peer employability negatively affects the program's effectiveness. Baird et al. (2023) analyze a job training program for disadvantaged adults in the United States and find large positive peer effects on employment and earnings.[2] Compared to these papers, we study a more representative set of programs and workers which allows us to shed light on which contextual differences might drive outcomes. Furthermore, this paper is the first to systematically examine the mechanisms underlying peer effects in labor market training and how they depend on the macroeconomic environment.[3] Our findings highlight that the average peer effect in labor market training can mask potential counteracting effects. Negative effects from within group competition in job search can confound positive skill spillovers between peers.

Second, our findings inform optimal treatment assignment in labor market training and suggest that peer composition is an important design feature. Most of the existing literature investigates the efficient allocation of participants to programs based on individual characteristics, but does not take compositional aspects into account (Eberts et al., 2002, Lechner and Smith, 2007 Staghøj et al., 2010, Cockx et al., 2023). The evidence points

[2]Peer effects have also been found to matter for the adoption of labor market programs (Mora-García and Rau, 2023).

[3]The role of skill spillovers and competition effects between workers have been primarily studied at the workplace e.g. Cornelissen et al. (2025, 2017); Mas and Moretti (2009).

to potential benefits from introducing statistical treatment rules to assist caseworkers in assigning unemployed workers to program types. Nevertheless, the practical implementation of a functioning targeting system is challenging (see e.g., Behncke et al., 2009).[4] The only study to derive optimal allocation rules in the presence of peer effects is Baird et al. (2023). They show that in a setting where peer effects are positive and linear-in-means and programs have a limited capacity, allocating seats to relatively more able participants can significantly increase the impact of the program. Our findings suggest that these positive effects might be enhanced if programs are designed to maximize knowledge exchange between participants and reduce competition in job search.

Finally, we also contribute to the large literature on peer effects in education by considering educational programs for adults. For school-aged children, higher levels of peer ability have been shown to have a positive effect on individual learning outcomes, although there appear to be important non-linearities (again, see Sacerdote, 2011 or Paloyo, 2020 for an overview). For adolescents and university students, studies find at most small peer effects on performance from classmates and roommates (e.g., Brodaty and Gurgand, 2016; Booij et al., 2017) but large effects on social skills (Zárate, 2023) and social behavior like drinking, cheating and fraternity membership (e.g., Sacerdote, 2001 and Gaviria and Raphael, 2001). Our results suggest that spillovers are not limited to a young age.

The paper proceeds as follows. Section 2 presents the institutional background. Section 3 introduces the data and sample used. We then define the variables of interest, discuss our identification and estimation strategy in Section 4. The results are presented in Section 5, while Section 6 presents robustness checks. Section 7 concludes.

[4] For Germany, Doerr et al. (2017) and Huber et al. (2018) find that a voluntary assignment system using vouchers is less effective in the short-term compared to a system with caseworker assignment. In the long-run however, positive employment effects of voluntary assignment prevail.

2 Institutional Background

Publicly sponsored training for the unemployed has the objective to update and increase the human capital of jobseekers, to adjust their skills to technological changes, and facilitate a successful labor market integration. In Germany, it is typically organised in the form of courses where participants interact on a daily basis. On the one hand, the setting is thus similar to an educational setting where individuals are taught new skills in a group. On the other hand, participants in further vocational training courses are simultaneously looking for jobs, which means that their peers are also potential competitors on the labor market. Finally, they also compete for jobs with jobseekers outside of their training program.

Since 2003, the assignment of unemployed individuals to courses has been regulated by a voucher system.[5] Once an individual registers as unemployed, a caseworker reviews their labor market prospects in a profiling process. If a lack of qualifications is identified, she recommends participation in a training program and issues a voucher. The voucher specifies the program's planned duration, its occupational target, its geographical validity, and the maximum course fee to be reimbursed. Vouchers are valid for a period up to three months from the date of issuance. Training providers are independently organized and offer different types of courses repeatedly in varying intervals. All certified providers and courses are listed in an online tool of the employment agency (*Kursnet*). In addition, training providers may advertise their courses at local employment agencies. Caseworkers are instructed not to issue any course-specific recommendations. Once jobseekers obtain a voucher, they can choose between certified providers' courses within the period and area of validity of the voucher. If they decide to take up a course, participation is

[5] See e.g., Kruppe (2009) or Doerr et al. (2017) for a detailed description of the institutional details.

mandatory.[6] However, jobseekers can exit the program without further consequences before its planned end date if they find a job. In theory, courses are also open to workers who are not registered at the employment agencies. We do not observe these individuals in our data but argue in Appendix A.5.1 that this should be a small number of people.

For our analysis, we aggregate different public sponsored training programs into groups according to their homogeneity with respect to educational contents and organization. We distinguish between short and long classic vocational training and retraining. All three types of training are offered for a wide range of fields, require full-time participation and combine classroom training with practical elements such as on-the-job training. Short vocational training programs (short training) are defined as programs with a maximum planned duration of 6 months. They have an average planned duration of about 3.6 months and offer minor improvements of skills. An example for such a course is "Financial accounting with SAP". Long vocational training programs (long training) have a planned duration of over 6 months and, on average, last around 9 months. They provide more intensive updating and extending of occupational skills. Such courses involve, for example, training on software skills, operating construction machines or in marketing and sales strategies. Some of the courses offer the possibility to obtain partial degrees. Retraining courses have the longest duration, lasting up to three years, and provide standardized training leading to a recognized vocational qualification within the German vocational education system. They are intended for jobseekers who either have no vocational qualification or have not worked in the occupation they were trained for.[7]

[6] Jobseekers *might* obtain a new voucher if the old one expires, but there is no legal claim for it. Doerr et al. (2017) finds that roughly 22% of vouchers are not redeemed and 25% of jobseekers who do not redeem obtain a new voucher. Once a voucher is redeemed, non-attendance can lead to benefit sanctions.

[7] For those workers, retraining courses provide a first occupational qualification. The full terminology is "programs with a qualification in a recognized apprenticeship occupation".

3 Data and Sample Selection

The analysis is based on administrative data provided by the Institute for Employment Research (IAB). It covers the universe of individuals participating in publicly sponsored labor market programs between 2007 and 2012 (Database of Program Participants, MTH) and is linked to the Database of Registered Job-Seekers (ASU and XASU) and the Integrated Employment Biographies (IEB). In combination, the data contain detailed information on the training programs attended (e.g., a course and provider identifier, the timing and planned duration, the "target occupation", information on course intensity and costs) as well as a wide range of characteristics of the jobseekers (demographic characteristics, labor market histories with daily accuracy and the region of residence). The "target occupation" is a variable based on the German Classification of Occupations 2010 (KldB 2010). The KldB is a hierarchical 5-digit classification that allows us to distinguish both the occupation group and competence level associated with a course. Our identification approach makes use of both dimensions. We distinguish between 10 occupation groups. Table A.4 shows the distribution of these occupation groups across program types ("agriculture and forestry" as well as "military" groups are not represented in our sample, hence we effectively have only 8 groups). We then distinguish three categories of competence levels: low-skilled, skilled, and high-skilled. We identify peer groups as individuals who attend the same course together.

We focus our analysis on courses for which a peer effects analysis is sensible given the group size and the organization of the training.[8] We restrict the sample to courses where the number of participants lies between 5 and 30.[9] Furthermore, we focus on courses

[8]For the construction of the relevant peer variables (see Section 4.1) it is important that we have information on everyone in the group. We thus exclude courses where some individuals have, for example, missing labor market histories.

[9]Larger group sizes are rare and represent only 0.1% of courses.

where individuals start within the same month, overlap for at least one day, and exclude self-learning programs and special programs.[10] For reasons related to our identification strategy (see Section 4.2), we only consider providers which offer courses once per month but multiple times in the observation period. Further, we exclude courses where the target occupation is missing.[11] Once the peer variables are constructed, we confine the estimation sample to individuals doing their first vocational training program within our observation window.[12] This yields a final sample of 56,134 program participants.

Table 1 summarizes selected course and individual characteristics as well as outcome variables by program type. The majority of individuals are in short vocational training (33,160 people in 3,376 courses), followed by long vocational training (11,513 people in 1,193 courses) and retraining programs (11,461 people in 1,287 courses). Panel A reports the course characteristics. The average number of courses per provider ranges from 5 to 6 in our sample. The average number of courses within a provider–competence level–occupation group cell is only slightly lower. On the one hand, this reflects the fact that the sample is constrained to providers who offer one course per month, i.e. small providers. On the other hand, providers have an incentive to specialize in specific topics, in particular, if they offer long programs which require more infrastructure and know-how. The largest share of courses falls within the "Health care, social and educational sector" occupation group, the "Traffic, logistics, safety and security" occupation group,

[10]Special programs usually target a particular sub-population of the labor market, such as the program *WeGebAU*, *Perspektive 50plus*, *Gute Arbeit für Alleinerziehende*. We exclude courses if all participants are funded via such special programs. The programs are too small in order to allow for a separate estimation of peer effects in our setting. A number of vocational courses offered in Germany allow for continuous entry. We do not consider those courses as they are characterized by partially overlapping peer groups. Identifying peer effects in these types of courses requires a different identification strategy.

[11]The target occupation is almost never missing in retraining courses (which are usually occupation-specific) and is missing for around 10 percent of courses in short and long vocational training. It is likely that some of these courses cannot clearly be attributed to a single target occupation. We exclude these courses because we need the target occupation for identification.

[12]This does not exclude that individuals attended other types of programs in their current unemployment spell.

the "Manufacturing" occupation group as well as "Business and administration" (see Appendix Table A.4). Overall, the occupation group mix is more or less comparable across program types. In contrast, competence levels vary across training programs. Retraining programs almost exclusively (94%) focus on medium ranked competence level courses. Short and long training programs, instead, more often include courses with low or (more often) high competence level.

On average, there are about 9 to 10 individuals in one course, and they spend the majority of course time in class and only few hours in on-the-job training.[13] Most of the individuals start at the earliest entry date (at least 92 percent). A small share ends the course early, likely because they find a job before the course ends. Thus, peer groups are relatively stable throughout the course.[14]

Typically, participants do not know each other when entering the program, as only 1 or 2% of them were working at the same firm before becoming unemployed.[15] We flag all courses in which program participants previously worked with more than 20 percent of their peers in the same firm. This affects around 5 percent of courses in our sample. Section 6.2 shows that results are not sensitive to excluding these courses.

Panel B of Table 1 shows a selection of individual characteristics. Individuals in short training accumulated more months in employment and earnings in the past two years, and they start the program relatively early in the unemployment spell (p-values for the differences with other programs <0.01). On the other hand, participants in retraining

[13]Appendix Figure A.6 shows the distribution of individuals over different course sizes.

[14]In Appendix A.5.3, we provide evidence that compositional changes are unlikely to drive our estimates. Table A.13 show similar results for courses with a low or high share of dropouts.

[15]A mass lay-off in a particular region might drive workers that know each other into the same courses. Except for that, it is arguably unlikely that friends or co-workers get unemployed at the same time and select into the same programs.

programs are on average three years younger, more likely to be female, less likely to have a high-school degree, vocational training or an academic degree compared to participants in short training (p-values <0.01). These differences can be explained by the nature and target of the programs.

The key outcomes of our analyses are individual employment and earnings (Panel C of Table 1). We consider job search duration after program start and total employment up to 60 months after program start – a proxy for employment stability.[16] We also study the dynamic effect of peer quality on employment by considering employment probabilities in each month after program start. Appendix Figure A.7 shows the employment rate by program type up to 60 months after program start. Average employment is below 20 percent right after the program starts and increases to about 65 to 70 percent after around 40 months. The employment rate increases faster for shorter programs, partially explained by shorter lock-in effects.[17]

With regard to earnings, we consider daily earnings in the first job and total cumulative earnings up to 60 months after program start. In Table 1, both earning outcomes are measured in logs and may correspond to part-time or full-time jobs. Earnings measures are based on the subset of employed (92 or 93 percent).[18] In the following analyses, we further present robustness checks for earnings based on the full sample (i.e. including zeros) with earnings measured in levels and in logs, using both linear and Poisson models.

[16] We consider all types of employment. This includes regular, part-time and so-called marginal employment. The term marginal employment refers to small-scale employment, so-called mini jobs - according to §8 SGB IV and §7 SGB V. The job search duration is censored at the end of the observation period.

[17] The literature evaluating the overall effectiveness of such programs documents such lock-in effects. See e.g., McCall et al. (2016).

[18] For total employment, this includes individuals who are employed at least once in the first 60 months after program start. For earnings in the first job, it includes individuals who found a job until the end of the observation period.

Table 1: Summary Statistics for Selected Individual and Course Characteristics and Outcomes across Program Types

	Short training		Long training		Retraining	
	mean	sd	mean	sd	mean	sd
Panel A - Course characteristics						
Number of courses per provider	6.22	4.06	5.06	3.38	4.67	3.92
Number of courses per provider, competence level and occupation group	4.90	3.13	4.64	3.26	3.60	1.92
Competence level: low-skilled	0.08	0.27	0.04	0.19	0.02	0.14
Competence level: skilled	0.75	0.43	0.76	0.43	0.94	0.23
Competence level: high-skilled	0.17	0.38	0.21	0.40	0.04	0.19
Course size	9.95	4.41	9.77	4.53	9.06	4.20
Average planned duration in months	3.63	1.65	9.24	3.31	22.43	7.50
Weekly hours	38.55	3.58	38.37	3.05	38.24	3.07
Total hours in practice	2.78	43.35	11.69	123.59	26.88	307.88
Total hours in class	433.66	249.04	992.69	413.03	2034.88	848.10
Apprenticeship certification exam or similar	0.02	0.15	0.08	0.26	0.65	0.48
Share starting at earliest entry date	0.93	0.12	0.94	0.12	0.92	0.14
Share ending course early	0.08	0.13	0.11	0.14	0.17	0.20
Share of peers working at the same firm in last job	0.02	0.09	0.01	0.06	0.01	0.07
Panel B - Individual characteristics						
Age in years	37.93	10.73	37.87	9.80	34.58	8.18
Female	0.43	0.50	0.40	0.49	0.47	0.50
Non-German	0.10	0.30	0.11	0.31	0.11	0.31
High-school degree (Abitur)	0.17	0.38	0.19	0.39	0.12	0.32
Vocational training	0.64	0.48	0.61	0.49	0.53	0.50
Academic degree	0.08	0.27	0.09	0.29	0.03	0.16
Months unemployed at program start	9.89	20.41	11.79	22.35	11.87	22.17
Months employed (last 2 years)	13.95	8.43	13.02	8.50	12.90	8.51
Total earnings (last 2 years, 1000 euro)	23.47	22.74	21.21	21.00	18.47	17.60
Program in same unemployment spell	0.22	0.41	0.25	0.44	0.29	0.45
Panel C - Outcomes						
Search duration for first job (in days)	738.57	951.45	833.38	934.80	994.14	837.45
Total employment by month 60 (in days)	1127.71	585.26	1071.05	555.11	911.24	506.65
Positive earnings by month 60	0.92	0.27	0.93	0.26	0.93	0.25
Log daily earnings in first job (if > 0)*	3.34	1.07	3.30	1.09	3.13	1.12
Log total earnings by month 60 (if > 0)*	10.64	1.28	10.62	1.29	10.39	1.20
Observations	33160		11513		11461	
Number of courses	3376		1193		1287	
Number of providers	802		340		396	

Notes: Summary statistics (mean and standard deviation) calculated at the individual level. All amounts in euro are inflation adjusted (in prices of 2010). Individual characteristics are measured at program start. Variables marked with * are based on a smaller sample of people with positive earnings.

4 Empirical Strategy

4.1 Measuring Employability

Our objective is to quantify the impact of peer quality on individual labor market outcomes. We focus on the average employment prospects of individual $i's$ peers at program start, referred to as employability in what follows. We first define employability at the individual level and then construct a measure for peer employability as the leave-one-out sample average of individual i's peers' employability in group g of size n:

$$\bar{X}_{(-i)g} = \frac{1}{n_g} \sum_{j \neq i}^{n_g} X_{jg}$$

For further analyses, we also consider the ordinal rank of the focal worker among her peers, R_{ig}. The percentile rank is constructed following Bertoni and Nisticò (2023) and Murphy and Weinhardt (2020): $R_{ig} = \frac{r_{ig}-1}{n_g-1} \in [0, 1]$, where r_{ig} is the ordinal rank (based on employability) of individual i in group g and n_g is the group size. R_{ig} is thus zero for the lowest-ranked (best) jobseeker and one for the highest ranked (worst) in the group.

We define ex-ante (i.e. net or peer and program effects) employability as the probability of finding a job that lasts for more than 6 months within one year of program start. Because this counterfactual measure is not available for program participants, we construct employability with the following steps.[19] First, we draw a random sample of jobseekers that do not participate in any program (1.5 million observations).[20] Second, we apply nearest neighbour matching based on the propensity score to adjust for observable differences between the two groups at the moment of entry into unemployment (see Appendix

[19]The approach has similarities with Van den Berg et al. (2019).

[20]It is a random sample of individuals entering unemployment between 1998 and 2012. Note that in around 27 percent of cases individuals do not participate in any program after entering unemployment.

A.1.1 for details). The matching deals with non-random selection into training. Third, we estimate an employability model using the matched sample of non-participants and different algorithms: A logit model, a lasso logit model with penalization based on cross-validation, and a lasso logit model with theory-driven penalization (Tibshirani, 1996). We regress individual employability on a large set of variables including demographic characteristics, information on health, education, past labor market outcomes as well as information on the local labor market situation.[21] All models perform similarly well, with the cross-validated lasso slightly outperforming the other models.[22] We thus adopt cross-validation. Fourth, we use the estimated coefficients to predict employability for the sample of participants at program start.[23] Finally, we construct the peer measures using the predicted employability. We discuss in Section 6.1 the robustness of our results to constructing employability using covariates at unemployment entry (and not at program start) and to the exclusion from the employability model of variables related to area, time, and local labor market conditions (which might not be important in generating peer effects).

The distributions of the predicted individual and average peer employability are shown in Appendix Figures A.3 and A.4 separately by program type. The individual employability ranges from 0.01 to 0.96, while average peer employability ranges from 0.16 to 0.88 (see also Table 2). On average, jobseekers in short training have the highest predicted individual employability, followed by long training and retraining courses.

[21] The regression output is displayed in Appendix Table A.3. While the matching is done separately by program type, to allow for heterogeneous selection into training, we estimate the employability score jointly for all program types. The idea is that employability should be independent of which programs jobseekers participate in.

[22] Appendix Table A.2 shows that the cross-validated lasso has an accuracy of prediction of 67.35 percent in the holdout sample. The results are robust to the estimation of the employability model using a logit model. They are available on request.

[23] For the sample of program participants, we measure the input variables at program start, wherever possible. Information on education, training and health is elicited at entry into unemployment.

How can the employability score be interpreted? The measure correlates both with past and future success on the labor market. The prediction models identify age, gender, education, training, past labor market outcomes as important drivers of employability. While the selected predictors of the lasso model should be interpreted with caution, they indicate that what drives employability is correlated with past labor market performance. Furthermore, the results in Section 5 reveal that the predicted employability is strongly correlated with actual labor market success after program participation. The employability score thus seems to be a valid proxy for the labor market success of jobseekers and their peers. This is confirmed by a sensitivity analysis presented in Appendix Table A.9, where we include different peer characteristics that are highly correlated with peer employability directly in the empirical model. We find positive peer effects on employment and earnings from being exposed to peers with successful labor market histories.

Several studies use predicted measures of ability to study peer effects, see e.g., Burke and Sass (2013), Van den Berg et al. (2019) or Thiemann (2022).[24] In our context, using a predicted employability measure entails a number of advantages compared to an analysis based on other, readily available peer characteristics. First, it is data-driven and does not rely on any prior knowledge of the strongest predictors of employability. Second, using a single predicted score reduces dimensionality and allows for an easier interpretation of peer effects. As pointed out by Graham (2011), it is not straightforward to study the effects of multiple peer attributes simultaneously since a ceteris paribus interpretation of such effects is difficult.

[24]Comparable employability measures based on predictive algorithms have also been used by public employment services e.g. for profiling jobseekers. See Körtner and Bonoli (2023) for an overview. So far, profiling has mostly aimed at identifying individuals at risk of long-term unemployment and has been shown to be effective in reducing the duration of unemployment. Also targeting of jobseekers to the best intervention can be based on such predicted scores.

4.2 Identification of Peer Effects and Empirical Model

Two main methodological challenges complicate the empirical analysis of peer effects: the reflection and the selection problem (see e.g., Moffitt, 2001). The reflection problem (Manski, 1993) arises because of the simultaneity in peer behavior, meaning that an individual affects his peer's outcomes and the peers affect the individual's outcome. In the context of labor market training, program participants might for example affect each other through their job search behavior. This complicates the separate identification of exogenous peer effects, i.e., the influence of average peer characteristics, and endogenous peer effects, i.e., the influence of peer behavior. We do not attempt to separate these effects, but estimate a joint effect.[25] This joint effect is captured by the average peer employability, which might proxy peer behavior but is determined before program start.

The selection problem arises because of common unobserved shocks at the group level and endogenous peer group formation. In our setting, peer groups might be endogenously formed if individuals select into specific courses based on unobserved preferences or abilities which are correlated among those belonging to the same peer group. In fact, jobseekers do generally not know who they will be grouped with, but there might be endogenous sorting into specific providers or courses depending on the characteristics of the course (i.e., timing, content or location).

Our identification strategy builds on a strand of literature in the educational context that exploits idiosyncratic variation in the peer group composition controlling for selection into peer groups by including fixed effects at the school or grade level (e.g., Bifulco et al., 2011;

[25] Estimating the effects jointly is still of great interest, since policy makers who decide on how to optimally allocate individuals to a specific program would focus their attention on predetermined characteristics of unemployed that are actually observable. If peer characteristics (like the ex-ante employability) matter for the effectiveness of a program, it might not be relevant whether it is driven by the exogenous or the endogenous component of peer effects.

Lavy et al., 2012; Elsner and Isphording, 2017; Carrell et al., 2018).[26] Based on this idea, we control for provider choice, and exploit the variation in peer employability between comparable courses offered by the same training providers over time. Specifically, we only compare courses within the same provider, competence level, and occupation group. This addresses several concerns with regard to selection that is time constant. First, it controls for potential sorting with regard to location, since training providers are location-specific. Second, it controls for sorting based on skills that are captured by the competence level or that are occupation group-specific.

There might also be dynamic sorting into peer groups due to labor market conditions that vary with the business cycle. The composition of unemployed is likely to be better during recessions than during booms (Mueller, 2017). Business cycles might vary systematically across occupation groups and local labor markets. To address such sorting, we flexibly control for aggregate time trends with month x year x occupation group fixed effects. In a robustness check we further include month x year x region fixed effects to allow for business cycle effects that differ both across occupation groups and regions.

The time trends also take care of potential endogenous selection into peer groups (based on time preferences) that varies over the calendar year and possibly across occupation groups. Individuals might choose their peer group based on expectations on the peer composition. This could happen if there is an option value in waiting until redeeming the voucher, e.g. due to seasonality in the quality of jobseekers that enter the pool of unemployed throughout the calendar year. In fact, due to many regular contracts ending at the end of the year, jobseekers becoming unemployed at the end of a calendar year

[26] Other approaches identify peer effects by relying on random group assignment (e.g., Sacerdote, 2001; Duflo et al., 2011; Carrell et al., 2013; Booij et al., 2017), by using the underlying network structure to construct instrumental variables (Bramoullé et al., 2009; De Giorgi et al., 2010) or by exploiting varying group sizes (Lee, 2007; Boucher et al., 2014).

might be systematically different from jobseekers entering unemployment in the rest of the year. Individuals receiving a voucher in November might, for example, prefer to wait until January to be paired with more employable peers. Such seasonal patterns are differenced out. At the same time, this sorting behaviour will be limited by the unpredictable timing of voucher issuance, the limited voucher validity (jobseekers can select courses only up to three months after they obtain their voucher), course availabilities and capacity constraints.[27] To alleviate further concerns regarding self-selection into courses, we restrict our analysis to providers which offer courses exactly once per month. Thus, given their choice of provider and course month, individuals can enroll in only one course.[28]

The Empirical Model This identification strategy gives rise to the following empirical model, which we estimate separately for the three types of training programs:[29]

$$y_{ipcot} = \alpha + \gamma X_{ipcot} + \theta \bar{X}_{(-i)pcot} + \pi W_{ipcot} + \lambda_{pco} + \delta_{ot} + \epsilon_{ipcot}. \quad (1)$$

It regresses the outcome of an individual i in a course at a provider p, with competence level c, occupation group o, in month t on her own own employability X_{ipcot} and the leave-one-out mean employability of individuals in the same course $\bar{X}_{(-i)pcot}$. The model

[27]The issuance date of vouchers can hardly be influenced by jobseekers. Vouchers are issued in meetings with caseworkers, whose appointment time depends on their entry into unemployment and the efficiency of administrative processes.

[28]An alternative would be to use the group of training participants who receive a voucher in the same month as exogenous peer group. With this strategy the peer group would change from the entire course to a smaller fraction of jobseekers who obtain their voucher at the same time. Possibly spillovers are not confined to these groups. While such an analysis might thus be able to uncover interesting heterogeneities it is likely to underestimate the true peer effects. The approach could not be implemented since the date of voucher receipt is not observed in our data.

[29]This model corresponds to individual best response functions derived from a theoretical framework assuming continuous actions, quadratic pay-off functions and strategic complementarities. The assumption of strategic complementarities is likely to hold in the context of labor market training. It implies that if i's peers increase their effort e.g., by coming regularly to class, studying more or applying acquired skills to job search, i will experience an increase in utility if she does the same. Equilibria have been derived for these types of games by Calvo-Armengol et al. (2009) assuming small complementarities and by Bramoullé et al. (2014) using the theory of potential games.

controls for provider x competence level x occupation group fixed effects, λ_{pco}, and month x year x occupation group fixed effects, δ_{ot}. We additionally control for a vector of course-level characteristics, W_{pcot}, which contains the course size, the average planned duration, weekly hours and total hours spent in practice and class.

The coefficient of interest in this linear-in-means model is θ, which represents the impact of a marginal increase in the average peer employability on i's outcome. It can be interpreted as a social effect and is a combination of exogenous and endogenous peer effects.

4.3 Validity Checks

Our empirical strategy requires sufficient residual variation in average peer employability, and this variation should be exogenous, i.e. there is no sorting into peer groups.

Table 2: Variation in Employability Measures

	Short training				Long training				Retraining			
	mean	sd	min	max	mean	sd	min	max	mean	sd	min	max
Panel A - Raw variation												
X_{ipct}	0.61	0.171	0.01	0.96	0.60	0.172	0.07	0.96	0.59	0.172	0.06	0.95
$\bar{X}_{(-i)pct}$	0.61	0.091	0.23	0.88	0.60	0.088	0.25	0.83	0.59	0.091	0.16	0.83
Panel B - Variation net of fixed effects and course characteristics												
X_{ipct}	0.00	0.142	-0.66	0.48	-0.00	0.142	-0.56	0.45	0.00	0.141	-0.60	0.42
$\bar{X}_{(-i)pct}$	0.00	0.048	-0.29	0.28	-0.00	0.041	-0.21	0.21	-0.00	0.045	-0.24	0.23
Observations	33160				11513				11460			

Notes: This table shows summary statistics (mean, standard deviation (sd), minimum (min) and maximum (max)) of the employability variables. X_{ipct} designates the own employablity and $\bar{X}_{(-i)pct}$ the leave-one-out mean peer employability.

We characterize the raw and residual variation in own and peer employability separately by program types in Table 2. The residual variation is obtained by netting out the same fixed effects and controls included in Equation (1). The distribution of both measures is plotted in Figure A.8. The raw standard deviation is around 0.09 for all programs. Netting out fixed effects and controls reduces the variation to about half in all programs.

Endogenous sorting into courses. Our main identifying assumption is that variation in peer employability results from random fluctuations, conditional on fixed effects and controls. To gauge the plausibility of this assumption, we present several checks.

First, we check whether the residual variation in the average peer employability is in line with variation resulting from random fluctuations. For this, we perform a resampling exercise as in Bifulco et al. (2011) and compare the observed residual variation with a simulated one based on a random group allocation (see Appendix A.3.1 for details). Across most simulations, for short programs we cannot reject the null that the distribution of observed peer employability is equal to the one resulting from random group allocation (see Appendix Table A.5 and Figure A.9). Instead, we typically reject the null for long training and retraining. Overall, this exercise supports the exogeneity assumption only for short training program.

Second, we test for a non-zero correlation between own and average peer employability as in Guryan et al. (2009). We regress own employability on the leave-one-out peer employability and control for the same fixed effects and course controls included in Equation (1). To account for a possible mechanical effect of group assignment, we additionally control for the average employability in the pool of jobseekers who are starting a course in the same month. Appendix Table A.6 presents the results. We find no correlation for short vocational training, but a small – albeit significant – negative correlation for the other two course types. Like the resampling check above, this exercise more strongly supports the exogeneity assumption for short training program.

Third, we present another test for potential selection into courses based on own employability, following Chetty et al. (2011) and Balestra et al. (2022). If jobseekers were randomly assigned to courses, course indicators should not predict the ex-ante employability of jobseekers (conditional on the usual set of controls). We regress own employability on the set of fixed effects and course controls included in Equation (1) and retrieve the residuals. Next, we regress the residuals on a full set of course fixed effects and test their joint significance. The test cannot reject the null for any of the three course types (p-values above 0.76, see Appendix Table A.7). The result suggests that there is no systematic selection into courses based on own employability.

Finally, we test whether peer employability is correlated with a wide range of predetermined individual characteristics, conditional on own employability and the usual controls from Equation (1). Appendix Table A.8 presents the results. An F-test for the joint significance of these individual characteristics rejects the null hypothesis across all program types. This raises concerns that our baseline estimates may overestimate (underestimate) peer effects if Equation (1) omits individual characteristics that are correlated in the same (opposite) direction with both peer employability and the outcomes of interest. First, we argue that the results of Table A.8 present evidence of significant correlation only for a minority of variables, and that these correlations are particularly small in magnitude. Second, we later present a robustness test that includes *all* the variables reported in Table A.8 as additional controls (Figure A.11, specification "Individual controls"). For all the outcomes and programs, the main estimates are robust to their inclusion.

Overall, our validity checks suggest that the variation in peer employability that we exploit allows us to identify causal effects for short training programs, but not necessarily for long training and retraining. To some extent, this confirms that the design of our tests allows us to discriminate between data that support or not our assumption.

We can only speculate as to why the exogeneity assumption seems to hold only for short training courses. One possible explanation is that course choice is less consequential for short courses than for long training and retraining, which are more specialized and last longer. As a result, jobseekers may be more strategic in their choice, and non-random composition may be more likely. In particular, the negative correlation between own and peer employability (Table A.6) suggests that our estimates of Equation (1) underestimate peer effects for long training and retraining. For transparency, the next sections also present results for these program types, but we refrain from over-interpreting them.

5 Results

This section presents and discusses the results in three parts. First, we examine the effects of an increase in peer employability on individual labor market outcomes. Second, we investigate whether this effect varies by own employability. Third, we decompose peer effects into knowledge spillover and competition effects. All analyses are conducted separately by program type, with standard errors clustered at the course level.[30]

5.1 Peer Effects on Individual Labor Market Outcomes

5.1.1 Effects on Job Search Duration and Employment

Table 3 presents the estimated effects of an increase in peer employability on employment outcomes. Panel A shows the effects on job search duration, and Panel B the effects on total employment up to five years after program start. Both outcomes are measured in

[30]We do not correct standard errors for the imprecision coming from the estimated employability scores. The reason is that there exists no theoretically recognized procedure for how to do it. Bootstrapping is not feasible for nearest neighbour matching (Abadie and Imbens, 2008). Furthermore, we rely on two different samples for the estimation of the employability and the peer effects model. The results are robust to clustering at the level of providers, as shown in Appendix figure A.12.

days. The table presents the effects of a standard deviation increase in the average peer employability ($\bar{X}_{(-i)pct}$) as well as that of own employability (X_{ipct}).[31] The latter should be interpreted with caution as it may capture spurious correlations and not causal effects.

For short training courses, we find that an increase in the average peer employability has no statistically significant effect on job search duration. In contrast, a standard deviation increase in peer employability moderately increases total employment by around 13 days in the first 5 years after program start. When comparing it to the average days in employment after program start (see Table 1), the estimate implies an increase of about one percent. For long training and retraining programs, for which we are more cautious in our interpretation, we also do not find strong evidence of an effect on search duration, but find a comparable positive effect to short training courses for total employment.

To investigate the dynamics underlying our baseline estimates, Figure 1 presents monthly treatment effects. The figure reports the estimated effect of a one-standard-deviation increase in peer employability on the probability of employment from months 1 to 60 after program start. Empty circles denote estimates that are significant at 5% level. For short training programs, significant effects of roughly 0.5 percentage points materialize immediately after program start and increase up to around 1.5 percentage points. Although the increase is not monotonic, the effect rises sharply from approximately the fifth month onward, i.e. shortly after the completion of short programs (whose average duration is 3.6 months). The comparatively smaller effects observed during program participation are consistent with the notion that reduced job-search effort limits the scope for peer influences on job finding, in line with the lock-in effects documented in the program eval-

[31] We multiply the estimated marginal effects with the residual standard deviation of the respective variable (shown in Panel B of Table 2).

Table 3: Effects of Peer Employability on Employment and Earnings

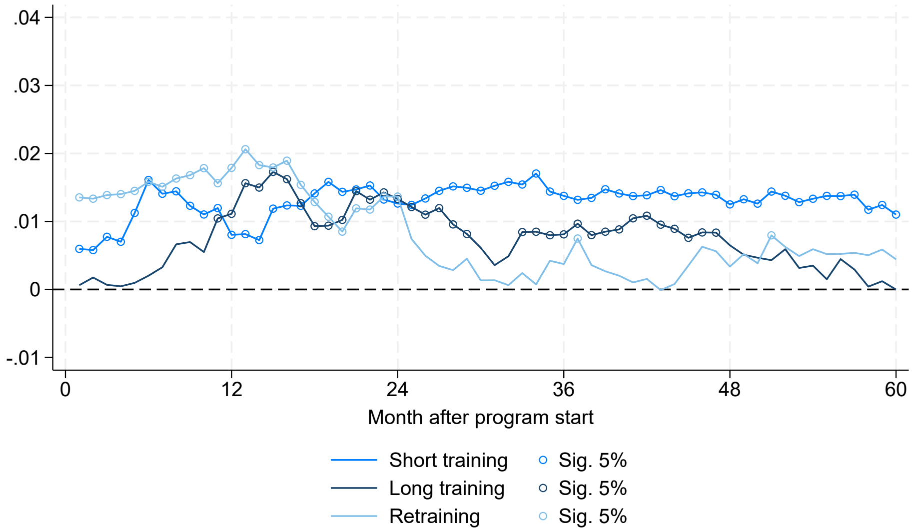
	Short training	Long training	Retraining
	(1)	(2)	(3)
Panel A - Search duration first job (in days)			
$\bar{X}_{(-i)pct}$	2.247	12.085*	8.000
	(5.114)	(6.839)	(6.416)
X_{ipct}	-167.153***	-151.657***	-158.441***
	(5.441)	(8.569)	(7.783)
N	*33160*	*11513*	*11460*
Panel B - Total employment in month 60 (in days)			
$\bar{X}_{(-i)pct}$	13.194***	9.708***	12.903***
	(2.806)	(3.599)	(4.101)
X_{ipct}	181.047***	179.217***	169.346***
	(3.147)	(4.836)	(4.658)
N	*33160*	*11513*	*11460*
Panel C - Log earnings first job (if > 0)			
$\bar{X}_{(-i)pct}$	0.033***	0.000	-0.009
	(0.006)	(0.009)	(0.009)
X_{ipct}	-0.015**	-0.062***	-0.094***
	(0.007)	(0.012)	(0.011)
N	*28479*	*9832*	*9815*
Panel D - Log total earnings in month 60 (if > 0)			
$\bar{X}_{(-i)pct}$	0.049***	0.015	0.012
	(0.007)	(0.010)	(0.009)
X_{ipct}	0.238***	0.257***	0.217***
	(0.008)	(0.013)	(0.013)
N	*30634*	*10675*	*10683*
Provider x competence level x occupation group FE	✓	✓	✓
Month x year x occupation group FE	✓	✓	✓
Additional Controls	✓	✓	✓

Notes: X_{ipct} designates the own employablity and $\bar{X}_{(-i)pct}$ the leave-one-out mean peer employability. All specifications control for course-level controls. Standard errors (in round brackets) are clustered at the course level. Effects are reported in terms of SD increases (see Table 2). Earnings are in prices of 2010 and measured in log(euro). * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

uation literature (McCall et al., 2016). From around the eighteenth month onward, the effect remains fairly stable and persists for at least five years after program start. Comparing these estimates with the employment rates shown in Figure A.7 further indicates that the peer effect stabilizes once aggregate employment rates also level off. The long-run effect of approximately 1.5 percentage points corresponds to about a 2% increase in employment probability.[32]

[32] We obtain similar patterns when using a broader definition of employment that also includes small jobs not subject to social insurance contributions (see Appendix Figure A.10). Although the estimated effects are somewhat smaller, they follow a very similar time profile.

Figure 1: Monthly Effects of Peer Employability on Regular Employment



Notes: The figure presents the estimated effects (in percentage points/100) of a one-standard-deviation increase in mean peer employability on the individual probability to be employed in a regular job in the months 1-60 after program start. Significant effects at the 5 percent level are marked by circles. On top of the mean peer employability, the underlying model includes the individual ex-ante employability, a vector of course-level controls, provider x competence level x occupation group fixed effects and month x year x occupation group fixed effects. Standard errors are clustered at the course level.

In sum, the findings suggest that exposure to a more employable peer group does not lead to a faster integration of participants into the labor market but rather increases employment stability. Previous evaluation studies of the same types of programs in Germany find that program participation increases the employment probability by 10 to 20 percentage points in the long-run (see e.g., McCall et al., 2016, Card et al., 2018). In comparison, a peer effect of 1.5 percentage points seems a realistic and sizable increase.[33]

5.1.2 Effects on Earnings

Table 3 also presents the effects of a more employable peer group on individual (log) daily earnings in the first job and (log) total earnings in the first five years after program start. The sample is restricted to individuals with positive earnings. As shown by the sample

[33]The effect size is modest when compared to Baird et al. (2023) who find particularly large peer effects on employment. They find that a one-standard-deviation increase in the average labor market history of peers increases the probability to be employed in the six quarters after training by 15 percentage points. This corresponds to about half of the job training effect itself and is thus exceptionally high.

size of each regression results, this excludes a minority of participants. Specifically, we consider daily earnings in the first job in the sample of program participants who found a job up to December 2016 (Panel C) and total earnings in the sample of participants that were employed at least once in the first 60 months after program participation (Panel D). Appendix Table A.10 shows that results are similar if we include all jobseekers and (i) use log earnings as outcome but set it to zero when earnings are zero, (ii) use earnings in levels as outcome, (iii) use earnings in levels but estimate a Poisson model.

The results show that a one-standard-deviation increase in the average peer employability increases daily earnings in the first job by 3.3 percent for participants for short training courses (about 93 cents when compared to the average daily earnings of 28.22 euro, see Table 1). In fact, using earnings in levels also gives a point estimate of 0.87 euros (Appendix Table A.10). For participants in long training and retraining, we find zero effects across al the different specifications. Similarly, in the long-run, earnings for short training participants increase by 4.9 percent due to a one-standard-deviation increase in the peer employability, while the effect remains zero for other program types. The Poisson model gives a more conservative (as it does not condition on being employed) but comparable and still sizable estimate of 4.0 percent for short training courses, and using earnings in levels suggests that cumulative earnings increase by about 2,300 euros over the course of five years due a one-standard-deviation increase in the peer employability.[34]

Overall, our findings suggest that exposure to more employable peers enables jobseekers in short training programs to (i) obtain higher-paying initial jobs and (ii) accumulate higher long-run earnings. Notably, the increase in long-run earnings is too large to be

[34] Sizable peer effects on earnings were also found by Baird et al. (2023) for a job training program in the US and by Jarosch et al. (2021) for coworkers in firms.

explained solely by the positive effect of peers on cumulative employment duration, which we estimate to be a modest 13 additional days. We therefore interpret the peer effects on earnings as operating primarily through improvements in job-match quality.

5.2 Does the Effect Depend on One's Own Employability?

Findings from the education literature suggest that students of different ability levels benefit differently from peers' ability (e.g., Burke and Sass, 2013, Carrell et al., 2013, Feld and Zölitz, 2017). Further, the program evaluation literature has shown that vocational training programs are more effective for jobseekers with relatively bad employment prospects (Card et al., 2018). Therefore, we test whether peer effects vary by own employability.

We first divide program participants (within each course type) according to the median predicted employability. Individuals below the sample median are classified as "low employability" participants, while those at or above the median are classified as "high employability" participants. We then estimate a model similar to that in Equation (1), including a dummy variable indicating high employability in place of the continuous measure of own employability (X_{ipct}), as well as an interaction term between this indicator and average peer employability. Table 4 reports the marginal effects of a one-standard-deviation increase in peer employability for both groups.[35]

For short training programs, our estimates suggest that jobseekers with low employability derive slightly larger long-term gains from exposure to a better peer group than participants with high employability. In particular, better peers appear to reduce job-search duration for less employable individuals (although not significantly), while they increase it

[35] "PE low employability" refers to the coefficient estimate for the peer employability term, while "PE high employability" refers to the sum between the former and the coefficient estimate for the interaction term. Standard errors for the latter are computed using the delta method based on the clustered variance–covariance matrix.

Table 4: Heterogeneity in Effects of Peer Employability Depending on Own Employability

	Short training	Long training	Retraining
	(1)	(2)	(3)
Panel A - Search duration first job (in days)			
PE low employability	-8.792	12.150	6.382
	(5.899)	(8.229)	(7.762)
PE high employability	27.922***	48.674***	38.124***
	(6.128)	(9.014)	(7.877)
P-value difference	0.00	0.00	0.00
N	*33160*	*11513*	*11460*
Panel B - Total Employment (in days)			
PE low employability	19.150***	4.187	3.443
	(3.467)	(4.802)	(4.718)
PE high employability	-4.971	-20.225***	-3.535
	(3.543)	(5.026)	(5.331)
P-value difference	0.00	0.00	0.19
N	*33160*	*11513*	*11460*
Panel C - Earnings in First Job (if > 0)			
PE low employability	0.014**	-0.003	-0.019*
	(0.007)	(0.010)	(0.011)
PE high employability	0.058***	0.021*	0.023*
	(0.007)	(0.012)	(0.012)
P-value difference	0.00	0.04	0.00
N	*28479*	*9832*	*9815*
Panel D - Total Earnings (if > 0)			
PE low employability	0.048***	0.006	-0.013
	(0.008)	(0.012)	(0.012)
PE high employability	0.038***	-0.022*	0.010
	(0.008)	(0.012)	(0.011)
P-value difference	0.25	0.03	0.09
N	*30634*	*10675*	*10683*
Provider x competence level x occupation group FE	✓	✓	✓
Month x year x occupation group FE	✓	✓	✓
Additional Controls	✓	✓	✓

Notes: All specifications control for course-level controls and individual employability. Standard errors (in round brackets) are clustered at the course level. Peer effects (PE) are reported in terms of SD increases (see Table 2). Earnings are in prices of 2010 and measured in log(euro). * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

for highly employable individuals (Panel A). Relatedly, a one-standard-deviation increase in peer employability increases total employment by about 19 days for less employable individuals, but has no effect for more employable individuals (Panel B). The positive effect on search duration for highly employable individuals appears to translate into substantial gains in first-job earnings of almost 6% for a one-standard-deviation increase in peer employability (Panel C), suggesting that these people spend more time looking for a job but eventually get a better one. The effect is also positive and significant for less employable individuals, but about four times smaller. Estimates from a Poisson model confirm that the effect on first-job earnings is larger for highly employable individuals (3.1% vs. 1.6%, see Appendix Table A.11). Overall, peer effects on search duration, total employment, and (re-)entry earnings differ significantly by individual employability.

Turning to total earnings, which provide an overall assessment by capturing effects across all the previous outcomes, the results suggest that in the long run a one-standard-deviation increase in peer employability increases earnings by 3.8% for highly employable individuals and by 4.8% for less employable individuals (Panel D). In this case, the difference is not statistically significant. Similarly, the Poisson model also gives smaller estimates for highly employable individuals (3.0% vs. 4.3%, see Table A.11), and the difference between the two groups is statistically significant with this specification.

The results on earnings for participants in long training and retraining programs are much smaller in magnitude and often imprecise or not statistically significant, consistent with the zero estimates from the baseline model presented in Table 3.

5.3 Mechanisms

Different mechanisms could explain peer effects in the context of labor market training – with possibly counteracting effects. Positive peer effects could arise from knowledge spillovers. Participants might learn from each other and benefit from the skills, knowledge and networks of more employable peers. These spillovers could improve the skill set and productivity of jobseekers and thereby positively affect their employment opportunities.[36]

Negative effects could result from an increased competition in job search. Jobseekers might loose out against highly employable peers when competing for jobs on the labor market.[37] Further, the importance of the within-group competition is likely to depend on the state of the labor market and the external competition for jobs. If the ratio of jobs to the number of unemployed in a labor market is low and the pool of unemployed is well qualified, then the outside competition might play a larger role.[38]

In the following, we disentangle peer effects into a knowledge spillover and competition effect. For this, we build on the literature on rank effects in education (see e.g. Delaney and Devereux, 2022) and a strategy proposed by Cornelissen et al. (2025). We identify knowledge spillovers from the average level of peer employability and competition from the

[36] Knowledge spillovers have been discussed as main mechanism behind peer effects in education (Booij et al., 2017; Kimbrough et al., 2022) and at the workplace (Jarosch et al., 2021; Frakes and Wasserman, 2021; Cornelissen et al., 2025). Such spillovers could also come in the form of referrals or knowledge transfers about effective job search strategies. Networks of friends, family or the neighbourhood have been shown to lead to more stable employment with higher wages (see e.g., Cingano and Rosolia, 2012; Pellizzari, 2010; Brown et al., 2016; Dustmann et al., 2016).

[37] Competition effects between peers have been analyzed at the workplace, e.g. by Cornelissen et al. (2025); Johnsen et al. (2024) and in job search, e.g. by Lazear et al. (2018).

[38] The literature on macroeconomic effects of labor market programs suggests that jobseekers inside and outside these programs compete for the same jobs (Ferracci et al., 2014; Gautier et al., 2018). Crépon et al. (2013) for example found larger displacement effects of labor market programs in weak labor markets.

ordinal rank of a jobseeker within the peer group. We estimate the following specification:

$$y_{ipcot} = \alpha + \sum_{j=2}^{30} \gamma_j X_{ipcot} + \theta \bar{X}_{(-i)pcot} + \psi R_{ipcot} + \pi W_{ipcot} + \lambda_{pco} + \delta_{ot} + \epsilon_{ipcot}, \qquad (2)$$

which, compared to Equation (1), additionally includes R_{ipcot}, the percentile rank of jobseeker i amongst the peers in her course (constructed as explained in Section 4.1). To isolate the effect of rank for a given level of employability, we control for own employability using percentile dummies. We choose a flexible specification with 30 percentile dummies to control for own employability. The rank effect (ψ) is thus identified from differences in higher moments of the employability distribution across groups (for example, two groups with same mean employability but different variance imply different rank for some people with the same employability – see also Murphy and Weinhardt 2020).[39] In order to interpret the coefficient on the average peer employability as pure knowledge spillover effect, we need to assume that rank perfectly controls for the within group competition and that that there are no other mechanisms that drive peer effects. In reality, rank will only capture specific aspects of how within-group competition affects labor market outcomes.[40] Furthermore, θ might pick up other spillovers that do not constitute knowledge spillovers, if there are any.[41] Nevertheless, this excercise will inform us qualitatively about the relative importance of these two most prominent mechanisms.

As mechanisms are likely to depend on the state of the labor market, we additionally

[39]Identifying rank effects is econometrically challenging since for a specific individual i, rank is the percentile of the employability distribution that corresponds to their own employability. Researchers thus usually rely on parametric assumptions to condition on rank and peer quality whose validity depend on the choice of correct functional form.

[40]The education literature argues that rank effects operate primarily through self-confidence and less through competition. The competition channel seems more relevant in the job search context. However, shifts in self-confidence were also discussed as mechanism behind peer effect (Van den Berg et al., 2019).

[41]Other possible mechanisms could be peer pressure (Mas and Moretti, 2009; Fu et al., 2019) or endogenous teacher behavior (Brodaty and Gurgand, 2016). However, they are unlikely to be the main drivers behind the effects. We further discuss them in Appendix A.5.3.

perform a heterogeneity analysis. We split courses depending on the tightness of the labor market at the moment of program start.[42] Courses are considered as tight if the ratio of vacancies to unemployed for their occupation is higher than the median tightness across all courses starting in a given month. They are classified as non-tight if the ratio is equal to or below the median. We then estimate a version of Equation (2) where we include an indicator for whether a course is in a tight market, as well as an interaction term of this indicator with rank and average peer employability, respectively. On top of the previous concerns regarding identification for long training and retraining courses, we also note that this exercise is by construction better suited to capture heterogeneity in short programs. This is because we measure the state of the labor market at program start, which is more relevant for people who spend only few months in training.

The results of the analysis are shown in Tables 5 and 6. Column (1) measures an "overall" peer effect analogous to our baseline specification, but control for own employability using percentile dummies instead of a linear specification. In column (2), we decompose the peer effect into spillover ($\bar{X}_{(-i)pcot}$) and competition (R_{ipcot}) channels. Column (3) presents estimates for the spillover and competition effects separately for non-tight markets (first two coefficients) and tight markets (second two coefficients). P-values from tests for the differences in effects between tight and non-tight markets are shown below.

Column (1) of Panel A, Table 5 confirms that average peer employability has no effect on search duration. Similarly, column (2) presents non-significant effects for peer employability and rank, although the sign of both estimated coefficients is consistent counteracting

[42] We measure labor market tightness as the ratio of vacancies registered at employment agencies to the number of unemployed in a specific occupation (2-digit level, KldB 2010) at the moment of program start. The average tightness-level is about 11 percent, i.e. relatively low. Notice though, that the vacancy data does not cover the universe of jobs. Tightness is measured at program start, since the moment of exit is endogenous.

effects for the two variables. Column (1) of Panel B also confirms that average peer employability has a moderate but positive effect on total employment of around 13 days (as in Table 3). Consistent with counteracting spillover and competition effects, the estimate increases to 15 days once we condition on rank, and the estimate for rank is also positive although small and not significant. Overall, however, competition among peers does not seem to play a big role in determining how quickly jobseekers find a job or for their employment stability.

On the other hand, results for earnings give a different picture. First, column (1) in Panels A and B of Table 6 confirms that peer employability has a positive effect on both short-run and long-run earnings. Second, comparing with column (2) shows that these estimates increase from 2.9% to 4.4% for short-run earnings and from 4.5% to 6.6% for long-run earnings once we condition on rank, consistent with counteracting spillover and competition effects. These results also show that "overall" peer effect estimates would greatly under-estimate spillover effects by around one third. Furthermore, rank also has a positive, significant, and sizable effect on earnings. Estimates from a Poisson model lead to the same conclusions (Table A.12). Together with the results in Tables 3, 4, and 5, these results confirm that peers effect – both spillover and competition – mainly affect earnings in the context of short term training programs.

We now turn to the discussion of the results by labor market tightness, which is measured at program start. Across the different outcomes, as we would have expected, competition effects are quantitatively more important in non-tight labor markets compared to tight labor markets. Intuitively, since the ratio of vacancies to unemployed is comparatively low in non-tight markets, this results in a fiercer competition for jobs in these markets. Consistently, this seems to increase the within group competition. In particular, column

(3) of Panel A, Table 5 shows that a one-standard-deviation increase in ordinal rank leads to a 10-day reduction in job search in non-tight markets and a 3-day reduction in tight markets (although the difference between the two is not significant). We find a similar pattern for total employment in Panel B, although estimates are again imprecise. Results for short-term earnings (column (3) of Panel A, Table 6) also show that competition effects are about 20% larger in non-tight market (although non significantly different). More strikingly, competition effects on long-term earnings are twice as large in non-tight market (column (3) of Panel B, Table 6). The Poisson model (Table A.12) also give estimates that are twice as large in non-tight markets for both short-run and long-run earnings, with the effects being significantly different for both outcomes.

Knowledge spillover effects are also larger in non-tight markets, in particular for earnings. They could be explained by a larger motivation and efforts of highly employable jobseekers in these programs from which other course participants might benefit. This result is also consistent with the literature finding smaller lock-in effects and larger employment effects of training programs when unemployment is high (Lechner and Wunsch, 2009).

Overall, the findings suggest that within group competition attenuates part of the effects of peer employability that operate through knowledge spillovers. Competition has the largest impact on earnings outcomes, suggesting that it primarily affects job-match quality or who obtains the better paid jobs. We further document large differences in competition effects on earnings across tight and non-tight labor markets.

Table 5: Spillover and Competition Effects on Employment

	Short training			Long training			Retraining		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A - Search duration first job (in days)									
$\bar{X}_{(-i)pct}$	2.248	-3.089	-6.131	11.619*	7.562	4.352	6.691	3.946	6.289
	(5.113)	(6.109)	(7.338)	(6.820)	(8.386)	(10.528)	(6.365)	(7.965)	(8.241)
R_{ipcot}		-7.841	-10.033**		-6.902	-4.358		-4.295	-5.062
		(4.890)	(5.044)		(8.280)	(8.544)		(7.469)	(7.647)
$\bar{X}_{(-i)pct}$ (tight)			1.467			9.043			-1.535
			(8.031)			(9.784)			(12.463)
R_{ipcot} (tight)			-3.802			-11.515			-2.354
			(5.719)			(9.403)			(9.028)
P-value diff: $\bar{X}_{(-i)pct}$			0.411			0.682			0.514
P-value diff: R_{ipcot}			0.138			0.279			0.688
N	33160	33160	33160	11513	11513	11513	11460	11460	11460
Panel B - Total Employment(in days)									
$\bar{X}_{(-i)pct}$	13.131***	15.355***	14.166***	9.381***	5.495	6.911	13.019***	10.219**	14.353***
	(2.790)	(3.357)	(4.144)	(3.617)	(4.486)	(5.705)	(4.073)	(4.898)	(5.319)
R_{ipcot}		3.268	4.666		-6.613	-6.308		-4.380	-1.674
		(2.824)	(2.948)		(4.648)	(4.838)		(4.281)	(4.380)
$\bar{X}_{(-i)pct}$ (tight)			15.569***			5.035			-0.834
			(4.120)			(5.358)			(7.201)
R_{ipcot} (tight)			0.453			-7.245			-11.087**
			(3.230)			(5.263)			(5.130)
P-value diff: $\bar{X}_{(-i)pct}$			0.770			0.775			0.040
P-value diff: R_{ipcot}			0.078			0.805			0.014
N	33160	33160	33160	11513	11513	11513	11460	11460	11460

Notes: $\bar{X}_{(-i)pct}$ designates the leave-one-out mean peer employability and R_{ipcot} the percentile rank. All specifications include 30 percentile dummies to control for own employability, course-level controls, provider x competence level x occupation group FE and month x year x occupation group FE. Standard errors are clustered at the course level. Effects are reported in terms of SD increases. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6: Spillover and Competition Effects on Earnings

	Short training			Long training			Retraining		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A - Earnings in First Job (if > 0)									
$\bar{X}_{(-i)pct}$	0.029***	0.044***	0.054***	-0.004	-0.007	-0.010	-0.011	-0.004	-0.003
	(0.006)	(0.007)	(0.009)	(0.009)	(0.011)	(0.014)	(0.009)	(0.012)	(0.012)
R_{ipcot}		0.023***	0.024***		-0.005	-0.009		0.012	0.009
		(0.006)	(0.006)		(0.011)	(0.011)		(0.011)	(0.011)
$\bar{X}_{(-i)pct}$ (tight)			0.033***			0.001			-0.004
			(0.009)			(0.014)			(0.017)
R_{ipcot} (tight)			0.020***			0.001			0.019
			(0.007)			(0.012)			(0.013)
P-value diff: $\bar{X}_{(-i)pct}$			0.031			0.486			0.977
P-value diff: R_{ipcot}			0.423			0.285			0.261
N	28479	28479	28479	9832	9832	9832	9815	9815	9815
Panel B - Total Earnings (if > 0)									
$\bar{X}_{(-i)pct}$	0.045***	0.066***	0.078***	0.012	0.009	0.002	0.010	0.012	0.020
	(0.007)	(0.008)	(0.011)	(0.010)	(0.012)	(0.015)	(0.009)	(0.012)	(0.013)
R_{ipcot}		0.030***	0.037***		-0.004	-0.004		0.003	0.006
		(0.007)	(0.007)		(0.012)	(0.012)		(0.012)	(0.012)
$\bar{X}_{(-i)pct}$ (tight)			0.049***			0.019			-0.009
			(0.010)			(0.015)			(0.017)
R_{ipcot} (tight)			0.019**			-0.006			-0.004
			(0.008)			(0.013)			(0.015)
P-value diff: $\bar{X}_{(-i)pct}$			0.018			0.335			0.070
P-value diff: R_{ipcot}			0.002			0.839			0.353
N	30634	30634	30634	10675	10675	10675	10683	10683	10683

Notes: $\bar{X}_{(-i)pct}$ designates the leave-one-out mean peer employability and R_{ipcot} the percentile rank. All specifications include 30 percentile dummies to control for own employability, course-level controls, provider x competence level x occupation group FE and month x year x occupation group FE. Standard errors are clustered at the course level. Effects are reported in terms of SD increases. Earnings are in prices of 2010 and measured in log(euro). * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

6 Robustness Checks

This section presents robustness checks concerning the construction of the employability measure and the identification strategy.

6.1 Measurement of Peer Employability

Peer effects estimates might be biased if the "true" peer employability to which jobseekers are exposed differs from the one that is observed. Below, we discuss potential sources of error and argue that any resulting bias is likely to be small.

Timing of measurement. Choosing the timing of the construction of the employability score is not obvious. Jobseekers in the training sample and the sample of participants are most comparable at the moment of unemployment entry. At the same time, employability at program start is most relevant for measuring peer effects, and is thus our choice for the baseline estimates. However, since non-participants are only observed at unemployment entry, a potential concern is that the model might perform worse in predicting the employability of jobseekers who start a program late in their unemployment spell. Appendix A.1.2 provides more details on the choice of the timing and possible sources of measurement error. Figure A.5 compares the distribution of peer employability according to the two approaches, and suggests that employability declines over the course of the unemployment spell. However, Figure A.11, specification "Employability estimated at unemployment start" shows that our main results are not sensitive to this choice.

Input variables. A second concern pertains to which variables should be used as input for the cross-validated lasso model. As Appendix Table A.3 shows, we also include variables related to area, time, and local labor market conditions. It is not clear a priori whether peer effects are generated only by personal peer characteristics or also by the labor market conditions in which peers operate. The inclusion of these variables allows for both channels. However, Figure A.5 shows that excluding these variables leaves the distribution of average peer employability mostly unchanged. Consistently, Figure A.11, specification "Employability without variables related to area and time" shows that our results are virtually unchanged if we use this alternative employability measure that excludes variables related to area, time, and local labor market conditions.

Missing peers. Another concern is that we might not observe all peers in our data which could lead to a mismeasurement of peer variables. We discuss this possibility in detail in Appendix A.5.1. We argue that, because it's unlikely that some people participate in the training programs but are not registered at the employment agencies, the fraction of missing peers is likely to be small and its distribution independent of group assignment and the error term, conditional on fixed effects and course controls. Under these conditions, a measurement error would merely cause a small attenuation bias (see Ammermueller and Pischke, 2009, Sojourner, 2013, Feld and Zölitz, 2017).

6.2 Threats to Identification

The tests presented in Section 4.3 suggest that – for short training courses – the variation in peer employability that we exploit to identify peer effects is likely the result of random fluctuations. In this section, we present additional checks to rule out that our results are driven by participants sorting.

Regional business cycles. Business cycles might not only vary at the level of occupational groups but also across regions. To address potential concerns with respect to region-specific sorting over time, we estimate a version of the main model where we additionally control for region-specific time trends (on top of occupation-specific time trends). Specifically, we include month x year x region fixed effects to our baseline model.[43] The results are shown in Figure A.11, specification "Region-specific time controls". The estimates are virtually unchanged for all outcomes.

Provider-specific mechanical sorting. A dynamic self-selection into courses based on month and provider-specific preferences in combination with the three month redemption period of vouchers might cause some mechanical sorting. Consider the pool of jobseekers obtaining a voucher in the same month, e.g. February. If highly employable jobseekers within this pool select into courses starting in a certain month (e.g. April), then the jobseekers attending the other possible months within their voucher validity (e.g. February, March or May) would mechanically be less employable.[44] We address this mechanical effect by differencing out sorting at the provider level that follows the same cyclicality in each quarter.[45] We do so by comparing courses that are exactly 4 months apart at a specific provider. Then, the pool of individuals who select into a specific course month does not overlap with the pool of individuals who select into courses starting exactly 4 and 8 months later. Considering the previous example: The feasible course options of jobseekers who start a course in the month of April do not overlap with those of jobseekers starting a course in August or December. The intuition and execution of the approach is further illustrated in Appendix A.5.2. The results are reported in

[43]We define 9 regions on the level of federal states: 1 "Hamburg and Bremen", 2 "Schleswig-Holstein und Niedersachsen" 3 "Nordrhein-Westfalen" 4 "Rheinland-Pfalz, Hessen and Saarland", 5 "Baden-Wuerttemberg", 6 "Freistaat Bayern", 7 "Berlin", 8 "Brandenburg und Mecklenburg-Vorpommern", 9 "Sachsen-Anhalt and Thüringen".

[44]This mechanical sorting is only problematic if it is provider-specific. If not, it is already controlled for by the occupation group-specific time trends.

[45]The seasonality of endogenous sorting into specific course months is in fact unknown.

Figure A.11, specification "Mechanical sorting". Results are qualitatively similar to the baseline ones but, if anything, larger in magnitude for all outcomes for short programs.

Competence level-specific time trends. Our baseline model controls for occupational group-specific time trends, which is the component of a "target occupation" (see Section 3) across which we expect time trends to vary. An alternative approach is to control for competence level-specific time trend with month x year x competence level fixed effects. Specification "Competence level-specific time controls" in Figure A.11 show that this alternative approach gives almost identical results.

Individual controls. In Section 4.3, we have discussed how some individual characteristics appear to be significantly correlated with peer employability (Table A.3.4). While these correlations appear small in magnitude, specification "Individual controls" in Figure A.11 present results when including all of the additional covariates presented in Table A.3.4 (i.e. not just the significant ones). The results remain significant and qualitatively similar to the main ones, but slightly attenuated for all outcomes.

Previous colleagues. Finally, we rule out our estimates are driven by program participants that knew each other from their previous job. This would be problematic if these jobseekers select into the same courses. We thus exclude all courses from the sample where more than 20 percent of participants worked at the same firm before entering the program. The results are reported in Figure A.11, specification "No previous colleagues", and are comparable to the baseline.

6.3 Robustness Checks for Competition Effects

The identification of competition effects with the rank-augmented model in Equation (2) relies on strong functional form assumptions regarding the relationship between course characteristics and rank (see for example Denning et al., 2023). Following Bertoni and Nisticò (2023), we preform three distinct checks to investigate robustness of our results.

Results are presented in Figure A.13. First, to control more flexibly for the distribution of peer employability and heterogeneous effects, we add controls for all the possible interactions between the standard deviation of peer employability, average peer employability, and own employability (specification "Controlling for SD of peer emplyability and its interactions" in the figure). Second, to model non-linear and heterogeneous peer effects, we add controls for the shares of low (bottom 25%) and high (top 25%) employable individuals in the group, and by interacting these measures by tertiles of own employability ability (specification "Flexibly controlling for the distribution of peer employability"). Finally, we directly hold the distribution of peer employability fixed by including group-specific fixed effects and excluding average peer employability. The three robustness checks confirm that competition has essentially no effect on search or employment duration. Our results on earnings in the first job, however, appear less robust to the presence of heterogeneous spillover effects (i.e. the two specifications that control for interactions between own and peer employability). Still, the estimated effect is virtually unchanged when we estimate competition effect in isolation from spillover effect by including course fixed effects. Most reassuringly, long-term competition effects on earnings remain significant across specifications, with confidence intervals overlapping for all models.

7 Conclusion

This paper investigates how labor market outcomes of participants in publicly sponsored training depend on the peer group composition as measured by the average employability of the peers. Using a number of predetermined characteristics, we construct a measure to proxy for employability, defined as the predicted probability to find a stable employment within one year after program start. To identify a causal effect, we exploit the quasi-random variation in peer employability across courses of the same competence level, in the same occupation group, and offered by the same provider over time.

Our results show that (in the sample of courses where we can credibly identify peer effects) the composition of peers has a significant and positive impact on labor market outcomes. While peer employability does not affect how quickly jobseekers find a job, it has a moderate positive effect on long-term employment. Moreover, we estimate that a one-standard-deviation increase in peer employability leads to about 3% higher earnings in the first job, and later to gains of almost 5% in total earnings over the course of five years since program start. The results further suggest that peer effects are somewhat stronger for less employable people in the long-run, as reflected by larger peer effects on total employment and long-run earnings compared to those for highly employable jobseekers. The latter group benefits more in the short-term from higher (re-)entry earnings.

To understand the mechanisms behind the peer effects, we decompose them into a knowledge spillover and competition component. We follow the literature and identify competition effects using participants' ordinal rank within their groups. While this analysis relies on stronger assumptions and effect sizes should be interpreted cautiously, it informs on the relative importance of both channels. Our results suggest that knowledge spillovers are an important driver behind peer effects, and they are partly attenuated by competition effects – in particular for earnings outcomes. Controlling for rank leads to estimates for the effect of peer employability on earnings that are 50% higher. This implies that, while jobseekers might benefit from the skills and networks of highly employable peers to obtain more stable and better paid jobs, there are negative effects through fiercer competition. Higher-ranked jobseekers tend to secure the better-paid jobs, while lower-ranked individuals are left with less attractive positions – especially in non-tight labor markets.

Our results bear important insights for policy makers. They suggest that group composition could be used to increase the effectiveness of training programs by amplifying knowledge spillovers and dampening competition between participants. Accompanying measures could include greater promotion of knowledge sharing within the group. Furthermore, program effectiveness could be increased by optimal allocation policies. Recent findings in Baird et al. (2023) suggest that, in settings where programs target a specific subset of jobseekers, peer effects are linear-in-means and positive and where the social planner cares about the average outcome of individuals or about those most in need, assigning a higher fraction of highly employable jobseekers to programs is likely to increase program effectiveness. Similar effects could be expected for our setting. However, we cannot rule out the possibility of non-linear effects, which might alter this recommendation. Broadly, policy recommendations regarding regrouping policies should be treated with caution since interventions that manipulate the peer composition have been shown to be confounded by endogenous sorting of individuals (Carrell et al., 2013). Finally, in the case of Germany, a better assessment would be necessary to determine whether the net effects of a targeted allocation are greater than those of a voucher system, given that jobseekers are currently not assigned to courses but are free in their course choice.

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A Appendix

A.1 Construction of the Employability Score

A.1.1 Matching Based on the Propensity Score

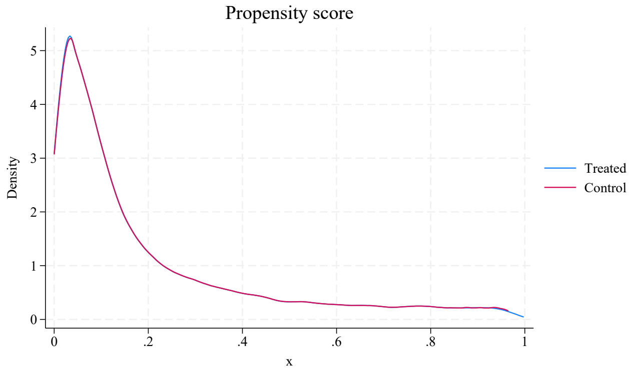
We rely on a sample of jobseekers that do not participate in any program to estimate the employability score. The reason for doing so is that the employment status without program participation is unobserved for actual participants. However, the sample of non-participant is likely to be selective, and we address this concern with a matching approach. For the employability model to result in unbiased predictions, we need to assume that the relation between the observed predictors in the sample of non-participants and the employability outcome would be the same in the sample of program participants. This is more likely to hold the more comparable jobseekers in both samples are.

We select the most comparable sample of jobseekers who do not enter any program during their unemployment spell applying exact nearest neighbour matching (with replacement) based on the propensity score (see e.g., Huber et al., 2013, Abadie and Imbens, 2016). The propensity score is estimated as the conditional probability of being in the sample of program participants based on a set of variables that have been identified as sufficient to eliminate any selection bias in the context of program participation (see Lechner and Wunsch, 2013, Huber et al., 2013, Caliendo et al., 2017). It is estimated separately by program type to account for a possible heterogeneity in selection effects across program types. We include demographic characteristics, information on education, training, characteristics of the last job, the recent labor market history, as well as variables characterizing the local labor market situation in the model. For this step of the analysis, time sensitive variables are measured both for the sample of participants and non-participants at the moment of unemployment entry.

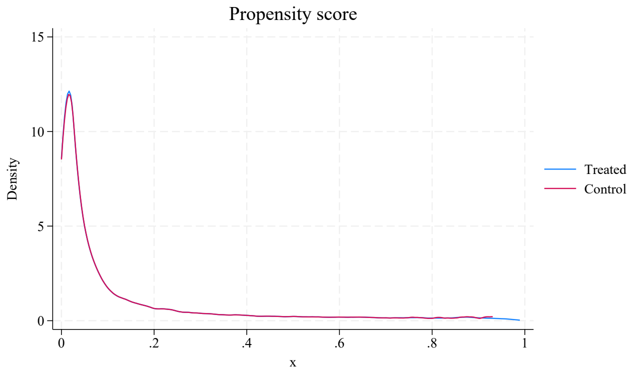
Table A.1 gives an overview of the variables included in the propensity score estimation. It presents averages for the sample of participants (column 1) and non-participants (column 3) as well as the difference in means (column 5) and the standardized bias (column 7). Even though there remain small differences in means for single variables, the standardized bias lies mostly below 25%, which is the reference value given by Rubin (2001). Note that the number of observations in the sample of participants is higher than in the final estimation sample. This is because some sample restrictions were imposed after the employability estimation.

Appendix Figure A.1 shows the distribution of the predicted propensity scores for program participants and matched non-participants by program type. The curves largely overlap for both groups which indicates that the matching worked well. Common support for individuals with very high propensity scores is violated. Nevertheless, this concerns only a small percentage of program participants. Overall, we are thus sufficiently able to balance the sample of program participants and non-participants based on their observable characteristics and therefore assume that they are also balanced in unobservable characteristics.

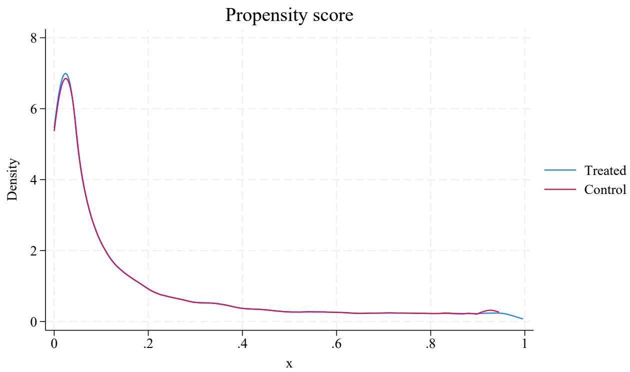
Figure A.1: Predicted Propensity Score (Program Participants and Non-participants)



(a) Short Training



(b) Long Training



(c) Retraining

Notes: This figures display the distribution of the predicted propensity score for program participants (treated) and matched non-participants (control) by program type.

Table A.1: Average Characteristics of Participants and Non-participants

	(1) PP	(2) NP	(3) Diff	(4) pval	(5) SB
Month of entry into unemployment					
January	0.12	0.13	0.00	0.66	0.31
February	0.09	0.08	0.00	0.10	1.36
March	0.09	0.08	0.01	0.00	2.78
April	0.09	0.09	0.00	0.86	0.13
May	0.07	0.07	0.00	0.12	1.02
June	0.07	0.07	0.00	0.31	0.67
July	0.08	0.08	0.00	0.33	0.77
August	0.08	0.08	0.00	0.18	1.22
September	0.09	0.10	-0.01	0.04	2.40
October	0.09	0.09	0.00	0.32	1.37
November	0.07	0.07	0.00	0.16	0.93
December	0.07	0.07	0.00	0.56	0.44
Year of entry into unemployment					
2000	0.01	0.02	0.00	0.01	1.95
2001	0.01	0.01	0.00	0.15	0.90
2002	0.01	0.01	0.00	0.18	0.80
2003	0.01	0.01	0.00	0.56	0.33
2004	0.02	0.02	0.00	0.25	0.68
2005	0.03	0.03	0.00	0.47	0.43
2006	0.06	0.06	0.00	0.05	1.40
2007	0.15	0.14	0.01	0.00	2.53
2008	0.21	0.22	0.00	0.56	0.51
2009	0.27	0.27	-0.01	0.22	1.21
2010	0.15	0.15	0.00	0.21	1.19
2011	0.07	0.07	0.00	0.15	0.91
Federal state					
Schleswig-Holstein	0.04	0.04	0.00	0.13	1.09
Freie und Hansestadt Hamburg	0.04	0.04	0.00	0.37	0.98
Niedersachsen	0.08	0.09	0.00	0.71	0.30
Freie Hansestadt Bremen	0.02	0.02	0.00	0.42	0.96
Nordrhein-Westfalen	0.24	0.23	0.00	0.40	0.68
Hessen	0.05	0.05	0.00	0.70	0.26
Rheinland-Pfalz	0.04	0.03	0.00	0.24	0.87
Baden-Wuerttemberg	0.10	0.10	0.00	0.71	0.27
Bayern	0.16	0.16	0.00	0.16	1.12
Saarland	0.01	0.01	0.00	0.16	0.92
Berlin	0.02	0.02	0.00	0.43	0.56
Brandenburg	0.03	0.03	0.00	0.31	0.66

Mecklenburg-Vorpommern	0.05	0.05	0.00	0.78	0.45
Freistaat Sachsen	0.07	0.06	0.00	0.08	1.16
Sachsen-Anhalt	0.02	0.02	0.00	0.29	0.69
Freistaat Thüringen	0.04	0.04	0.00	0.96	0.04
Demographic characteristics					
Younger than 25 years	0.14	0.15	-0.01	0.00	3.58
25-29 years	0.17	0.17	0.00	0.77	0.22
30-34 years	0.15	0.15	0.00	0.29	0.88
35-39 years	0.14	0.14	0.00	0.71	0.40
40-44 years	0.15	0.14	0.00	0.25	1.13
45-49 years	0.13	0.12	0.00	0.07	1.40
50-54 years	0.09	0.09	0.00	0.77	0.24
Older than 54 years	0.03	0.04	0.00	0.67	0.29
Female	0.44	0.44	0.00	0.74	0.29
German	0.90	0.90	0.00	0.79	0.21
Single	0.46	0.46	0.00	0.55	0.52
Married	0.37	0.37	0.00	0.42	0.65
Marital status: missing	0.16	0.16	0.00	0.89	0.14
Children < 15	0.23	0.23	0.00	0.87	0.15
Children < 3	0.15	0.16	0.00	0.25	1.21
Single parent	0.07	0.06	0.01	0.03	3.14
Restrictions or disability	0.10	0.10	0.00	0.26	1.02
No schooling degree	0.06	0.06	0.00	0.05	1.36
Schooling degree without Abitur	0.73	0.74	-0.01	0.08	1.37
Schooling degree with Abitur	0.17	0.16	0.00	0.06	1.34
Schooling: missing	0.04	0.04	0.00	0.22	1.14
Without vocational training	0.27	0.27	0.00	0.46	0.75
Vocational training	0.61	0.61	0.00	0.69	0.37
Academic degree	0.07	0.07	0.00	0.88	0.12
Vocational Training: missing	0.05	0.04	0.00	0.18	0.89
Job searched: part time	0.05	0.05	0.00	0.06	1.25
Job searched: full time	0.74	0.73	0.01	0.16	1.40
Job searched: missing	0.21	0.22	-0.01	0.04	2.19
Job returner	0.03	0.02	0.00	0.00	2.21
Characteristics of the last job					
No last job	0.01	0.02	-0.01	0.00	4.83
Last job: unskilled/semiskilled tasks	0.09	0.09	0.00	0.25	0.91
Last job: skilled tasks	0.66	0.66	0.00	0.68	0.34
Last job: complex tasks	0.04	0.04	0.00	0.61	0.32
Last job: highly complex tasks	0.07	0.07	0.00	0.66	0.34
Last job: skills missing	0.14	0.14	0.00	0.14	1.30
Last job: part time	0.67	0.69	-0.01	0.00	3.00
Last job: full time	0.31	0.29	0.02	0.00	4.49

Last job: working time missing	0.01	0.02	-0.01	0.00	4.97
LM history relative to unemployment start					
Regular EMP 3 months before	0.61	0.63	-0.02	0.00	3.65
Marginal EMP 3 months before	0.14	0.12	0.02	0.00	5.62
Part-time 3 months before	0.10	0.11	0.00	0.31	0.67
Program 3 months before	0.09	0.09	-0.01	0.01	3.04
OLF 3 months before	0.02	0.02	0.00	0.16	0.84
Regular EMP 6 months before	0.61	0.63	-0.02	0.00	3.76
Marginal EMP 6 months before	0.13	0.11	0.02	0.00	5.89
Part-time 6 months before	0.10	0.11	0.00	0.18	0.90
Program 6 months before	0.09	0.10	-0.01	0.01	3.73
OLF 6 months before	0.03	0.04	-0.01	0.06	2.85
Regular EMP 9 months before	0.60	0.62	-0.02	0.00	3.30
Marginal EMP 9 months before	0.13	0.11	0.02	0.00	6.29
Part-time 9 months before	0.10	0.10	0.00	0.11	1.06
Program 9 months before	0.09	0.09	0.00	0.35	0.94
OLF 9 months before	0.04	0.05	-0.01	0.07	2.52
Regular EMP 12 months before	0.61	0.62	-0.02	0.00	3.27
Marginal EMP 12 months before	0.12	0.11	0.02	0.00	4.83
Part-time 12 months before	0.10	0.10	0.00	0.21	0.83
Program 12 months before	0.09	0.09	0.00	0.88	0.13
OLF 12 months before	0.04	0.05	-0.01	0.05	2.70
Regular EMP 18 months before	0.57	0.58	-0.01	0.03	1.94
Marginal EMP 18 months before	0.12	0.11	0.01	0.01	2.51
Part-time 18 months before	0.09	0.09	0.00	0.37	0.60
Program 18 months before	0.09	0.09	-0.01	0.13	2.08
OLF 18 months before	0.05	0.06	-0.01	0.03	2.76
Regular EMP 24 months before	0.53	0.54	-0.01	0.15	1.29
Marginal EMP 24 months before4	0.12	0.11	0.01	0.00	2.54
Part-time 24 months before	0.09	0.09	0.00	0.78	0.19
Program 24 months before	0.09	0.09	0.00	0.69	0.38
OLF 24 months before	0.06	0.06	0.00	0.02	1.86
Regular EMP 36 months before	0.48	0.49	-0.01	0.18	1.15
Marginal EMP 36 months before	0.12	0.12	0.00	0.92	0.13
Part-time 36 months before	0.08	0.08	0.00	0.67	0.31
Program 36 before	0.09	0.09	0.00	0.71	0.33
OLF 36 before	0.07	0.07	0.00	0.09	1.36
Total days in EMP 6 months before	132.98	133.17	-0.19	0.78	0.29
Total days in ALO 6 months	38.50	36.07	2.43	0.00	4.04
Total days in program 6 months before	15.26	17.18	-1.93	0.00	4.39
Total days in EMP 12 months before	263.39	263.99	-0.60	0.62	0.50
Total days in ALO 12 months before	76.72	70.67	6.05	0.00	5.66
Total days in program 12 months before	30.60	33.23	-2.63	0.00	3.34

Total days in EMP 24 months before	512.65	513.89	-1.24	0.61	0.55
Total days in ALO 24 months before	154.84	142.11	12.72	0.00	6.91
Total days in program 24 months before	61.81	65.54	-3.73	0.02	2.75
Total days in EMP 60 months before	1177.16	1178.72	-1.57	0.79	0.30
Total days in ALO 60	415.91	386.40	29.51	0.00	7.15
Total days in program 60 months	156.67	162.16	-5.49	0.06	2.09
Total days in EMP 120 months before	2129.20	2128.06	1.14	0.91	0.11
Total days in ALO 120	739.56	683.61	55.95	0.00	7.73
Total days in program 120 months before	273.21	281.18	-7.97	0.13	1.98
Number of ALMP 24 months before	0.61	0.63	-0.02	0.23	1.67
Number of ALMP 60 months before	1.26	1.25	0.01	0.67	0.48
Log. total earnings 12 months before	8.51	8.47	0.04	0.21	1.55
Log. total benefits 12 months before	1.87	1.82	0.05	0.09	1.39
Log. total earnings 24 months before	9.37	9.30	0.06	0.07	2.61
Log. total benefits 24 months before	2.69	2.63	0.06	0.06	1.52
Log. total earnings 48 months before	10.12	10.04	0.08	0.03	3.59
Log. total benefits 48 months before	3.95	3.87	0.08	0.02	1.91
Log. total earnings 60 months before	10.35	10.26	0.09	0.02	4.11
Log. total benefits 60 months before	4.47	4.43	0.04	0.24	1.02
Log. total earnings 120 months before	11.03	10.91	0.12	0.00	5.86
Log. total benefits 120 months before	5.97	5.80	0.17	0.00	4.20
Regional characteristics (at district level)					
Local unemployment rate	8.62	8.54	0.08	0.00	2.38
Population density	792.50	785.65	6.85	0.33	0.77
Employment share primary sector	1.84	1.86	-0.01	0.29	0.77
Employment share secondary sector	26.25	26.37	-0.12	0.06	1.39
Employment share third sector	71.91	71.77	0.14	0.06	1.42
GDP per capita	30.68	30.69	0.00	0.97	0.03
No regional information	0.09	0.10	-0.01	0.07	2.74
Observations	62909	52486			

Notes: This table displays the average characteristics of the sample of program participants (PP) and the average characteristics of the sample of matched non-participants (NP) in columns (1) and (2), respectively. Column (3) displays the difference in means and column (4) the standardized bias (SB) calculated for each characteristic X_k as $SB_k = \frac{|E_{cs}(X_k)-E_p(X_k)|}{\sqrt{(V_{cs}(X_k)+V_p(X_k))/2}} \times 100$.

A.1.2 Estimation of the Employability Model

For the estimation of the employability model we compare the predictive performance of three models. A simple logit model, a lasso logit model where the regularization parameter (lambda) is chosen by cross-validation (10-folds) and a lasso logit based on a theory driven regularization parameter. To test the performance of the model, we split the sample of matched non-participants and keep one fourth as a holdout sample. The rest of the observations are used to train the model.

All models yield a similar predictive performance of around 67-68 percent (ROC area) in the training and holdout sample. The lasso based on cross-validation slightly outperforms the other models. See Table A.2. Compared to the so called rigorous lasso that is based on the theory driven penalization, it selects a larger lambda (0.0001556) and 206 out of 220 covariates. The rigorous lasso, which has the lowest performance selects 62.

The set of variables included is the same as the one for the matching exercise with the addition of interactions between individual characteristics and gender. For our main analysis, we choose to measure employability at program start. This implies that we measure time sensitive variables for program participants at program start, wherever possible. Note that we do not observe all time sensitive variable at program start but some only at unemployment entry. This concerns for example education, vocational training (with the exception of training in the context of ALMP) and the health and disability status. For the sample of non-participants, who we use to train the model with, time sensitive variables are measured at entry into unemployment. Measuring their employability at some hypothetical moment of program start is not possible, since for a part of the sample it would condition on the outcome.

An employability score measured at program start thus suffers from the following potential sources of imprecision: First, we do not have true variation in all predictors with regard to timing. Second, the group who we train the model with is only observed once, at unemployment entry. If actual program participants are unemployed for a long time when entering a program, the model might perform worse in predicting their true employability at program start. This is because the matched training sample of non-participants might not be representative for jobseekers with long unemployment durations. Furthermore, if there is dynamic selection into programs, e.g. ex-ante non-employable individuals enter programs later than highly employable individuals or vice versa, there might be a systematic measurement error in the employability score. To test the sensitivity of the results to the timing of measurement, we perform a robustness check where we measure employability at unemployment entry.

Given the performance of the three models in the holdout sample (ROC area), we choose the cross-validated lasso model for our main specification. As Appendix Figure A.2 shows, the distribution of the employability measure (average peer employability) estimated by logit and cross-validated lasso are virtually the same. Not surprisingly, they also yield the same results (can be obtained by demand). Appendix Table A.3 gives an overview of the estimated coefficients in the preferred prediction model.

Table A.2: Summary of Model Performance

Estimator	**Sample**	**ROC Area**	**Std. Err.**	**95% CI Lower**	**95% CI Upper**
Logit	Training	0.6860	0.0028	0.68054	0.69153
Logit	Hold-out	0.6721	0.0049	0.66242	0.68173
Rigorous Lasso	Training	0.6695	0.0029	0.66389	0.67510
Rigorous Lasso	Hold-out	0.6691	0.0050	0.65942	0.67884
Lasso Logit (CV)	Training	0.6858	0.0028	0.68027	0.69127
Lasso Logit (CV)	Hold-out	0.6735	0.0049	0.66382	0.68310

Table A.3: Predictors of Employability in Training Sample of Non-Participants (crossvalidated lasso logit)

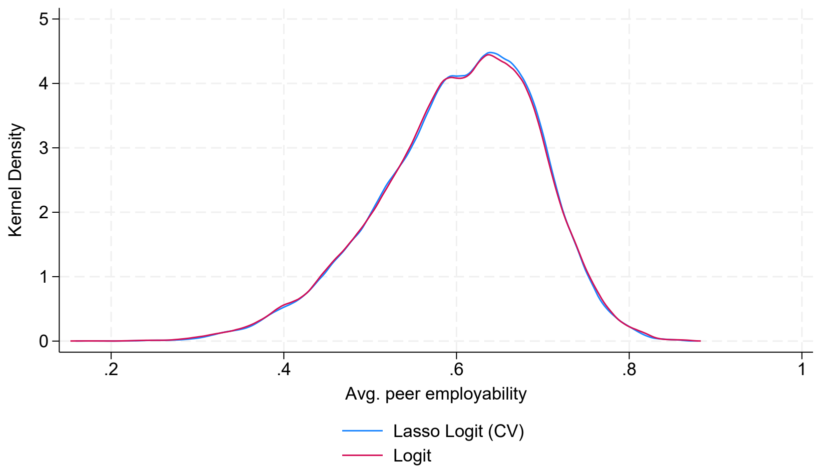
	Coefficient
Demographics	
Younger than 25 years	-0.07
30-34 years	0.01
40-44 years	-0.04
45-49 years	-0.00
50-54 years	-0.11
Older than 54 years	-0.57
Female	0.72
Non-German	-0.03
Married	0.06
Maritial status missing	0.09
Children	0.10
Children $<$ 3	-0.05
Restrictions or disability	-0.35
No schooling degree	-0.08
Schooling degree with Abitur	-0.08
Education missing	-0.31
Without Vocational Training	-0.20
Vocational degree missing	-0.05
Job searched:Part-time	-0.15
Job searched: missing	-0.11
Job returner	-0.13
Welfare benefits	-0.19
Month and year of entry into unemployment	
Feb	-0.06
Mar	-0.20
Apr	-0.25
May	-0.12
Jun	-0.21
Jul	-0.23
Aug	-0.26
Sep	-0.25
Oct	-0.19
Nov	-0.02
Dec	0.07
2006	-0.11
2007	0.06
2008	-0.15
2010	0.27
2011	0.15
Residence - Federal state	
Schleswig-Holstein	-0.07
Freie und Hansestadt Hamburg	0.16
Niedersachsen	0.09
Freie Hansestadt Bremen	0.29
Hessen	0.02
Rheinland-Pfalz	0.08
Freistaat Bayern	0.14
Saarland	0.02
Berlin	0.10
Brandenburg	0.17
Mecklenburg-Vorpommern	0.16
Freistaat Sachsen	0.18
Sachsen-Anhalt	-0.05

Freistaat Thüringen	0.08
Characteristics of the last job	
No last job	-3.03
Unskilled/semiskilled tasks	-0.00
Complex tasks	-0.16
Highly complex tasks	-0.02
Part-time	-0.01
Time missing	0.07
Log daily earnings in last job	0.07
Agriculture, forestry, farming, and gardening	0.45
Construction, architecture, surveying and technical building services	0.17
Natural sciences, geography and informatics	-0.22
Traffic, logistics, safety and security	0.08
Commercial services, trading, sales, the hotel business and tourism	0.11
Business organisation, accounting, law and administration	-0.03
Health care, the social sector, teaching and education	0.04
Philology, literature, humanities, social sciences, media, art	-0.36
Occupation: missing	-0.07
Labor market history	
Regular EMP 3 months before	-0.02
Marginal EMP 3 months before	0.28
Part-time EMP 3 months before	-0.04
In ALMP 3 months before	0.05
OLF 3 months before	-0.23
Regular EMP 6 months before	0.04
Marginal EMP 6 months before	0.04
In ALMP 6 months before	0.07
OLF 6 months before	-0.12
Regular EMP 9 months before	0.01
Marginal EMP 9 months before	0.15
In ALMP 9 months before	-0.07
OLF 9 months before	-0.14
Regular EMP 12 months before	-0.02
Marginal EMP 12 months before	0.32
Part-time EMP 12 months before	-0.10
In ALMP 12 months before	0.05
OLF 12 months before	-0.12
Regular EMP 18 months before	0.18
Marginal EMP 18 months before	0.13
Part-time EMP 18 months before	-0.09
In ALMP 18 months before	0.21
OLF 18 months before	0.01
Regular EMP 24 months before	-0.17
Marginal EMP 24 months before	0.05
Part-time EMP 24 months before	0.18
In ALMP 24 months before	-0.03
OLF 24 months before	0.04
Regular EMP 36 months before	-0.05
Marginal EMP 36 months before	0.11
Part-time EMP 36 months before	-0.11
In ALMP 36 months before	0.11
OLF 36 months before	-0.04
Cum. EMPL 6 months before (days)	0.00
Cum. UE 6 months before (days)	-0.00
Cum. EMPL 12 months before (days)	-0.00
Cum. UE 12 months before (days)	0.00
Cum. ALMP 12 months before (days)	-0.00
Cum. EMPL 24 months before (days)	0.00

Cum. UE 24 months before (days)	0.00
Cum. EMPL 60 months before (days)	0.00
Cum. UE 60 months before (days)	-0.00
Cum. ALMP 60 months before (days)	-0.00
Cum. EMPL 120 months before (days)	0.00
Cum. UE 120 months before (days)	-0.00
No of ALMP 24 months before	0.02
No of ALMP 60 months before	-0.00
Log cum. earnings 12 months before	0.04
Log cum. benefits 12 months before	-0.02
Log cum. earnings 24 months before	-0.01
Log cum. benefits 24 months before	0.01
Log cum. earnings 48 months before	0.04
Log cum. benefits 48 months before	-0.01
Log cum. earnings 60 months before	-0.00
Log cum. benefits 60 months before	0.02
Log cum. benefits 120 months before	0.00
Interactions with gender	Yes
Regional labor market characteristics	Yes
Observations	39364

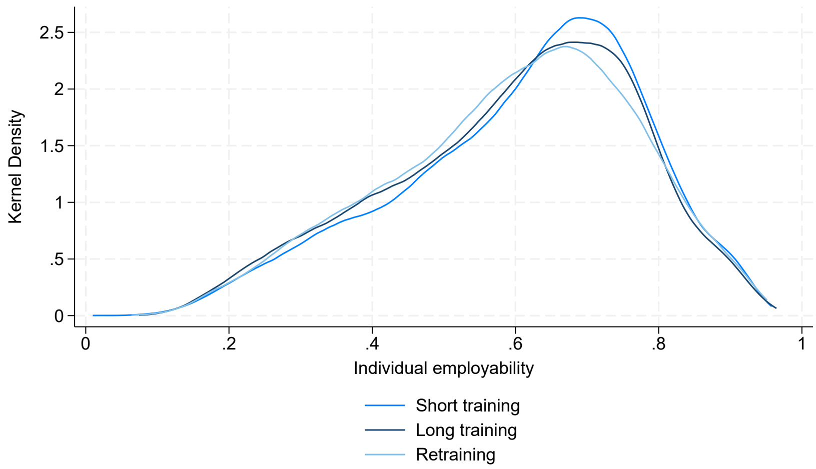
Notes: This table displays the coefficients of a lasso logit regression (lambda selected by cross-validation) of the employability measure on individual characteristics measured at the beginning of the unemployment spell in the training sample of non-participants. Coefficients based on interaction terms and for regional labor market characteristics are omitted in the table.

Figure A.2: Distribution of the Average Peer Employability across Models



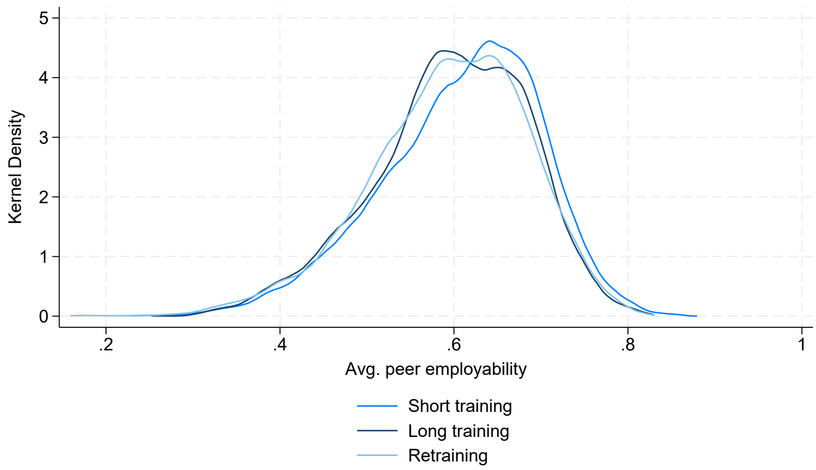
Notes: This figure plots the distribution of the average peer employbility estimated by a cross-validated lasso logit model (main specification) and a simple logit model.

Figure A.3: Distribution of the Individual Predicted Employability by Program Type



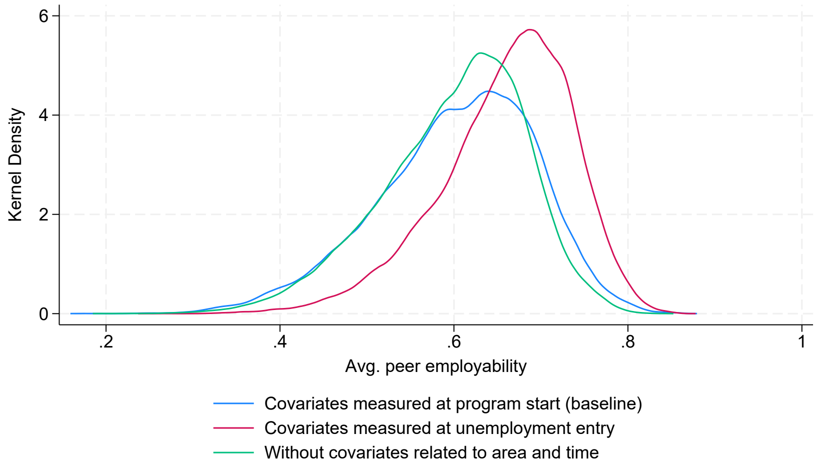
Notes: This figure plots the distribution of the predicted individual employbility separately for participants in short training, long training and retraining. It is based on a cross-validated lasso logit model.

Figure A.4: Distribution of the Average Predicted Employability by Program Type



Notes: This figure plots the distribution of the predicted average peer employbility separately for participants in short training, long training and retraining. It is based on a cross-validated lasso logit model.

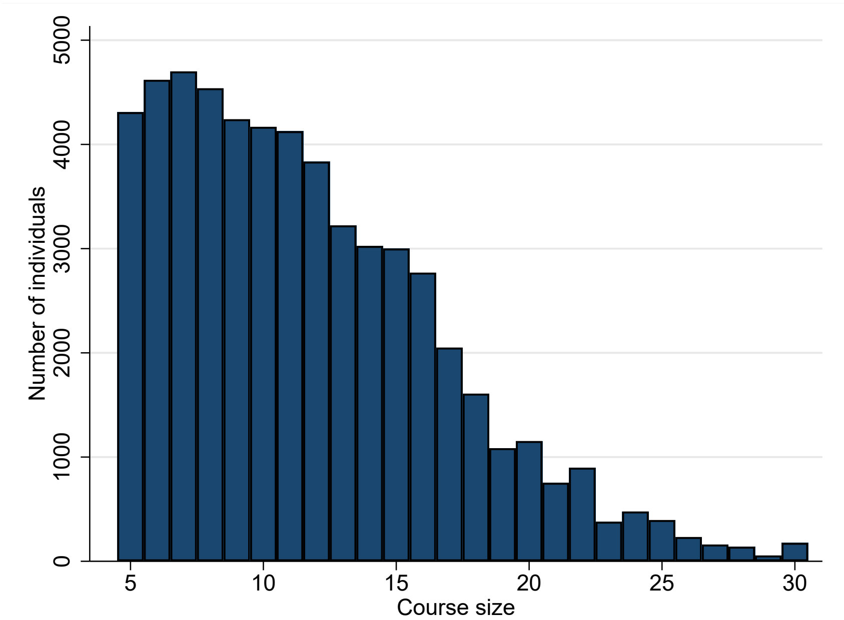
Figure A.5: Alternative Employability Measures



Notes: This figure plots the distribution of the predicted average peer employbility using three different sets of covariates, as described in Section 6.1. Figure A.11 reports the estimates of the effect for the three measures.

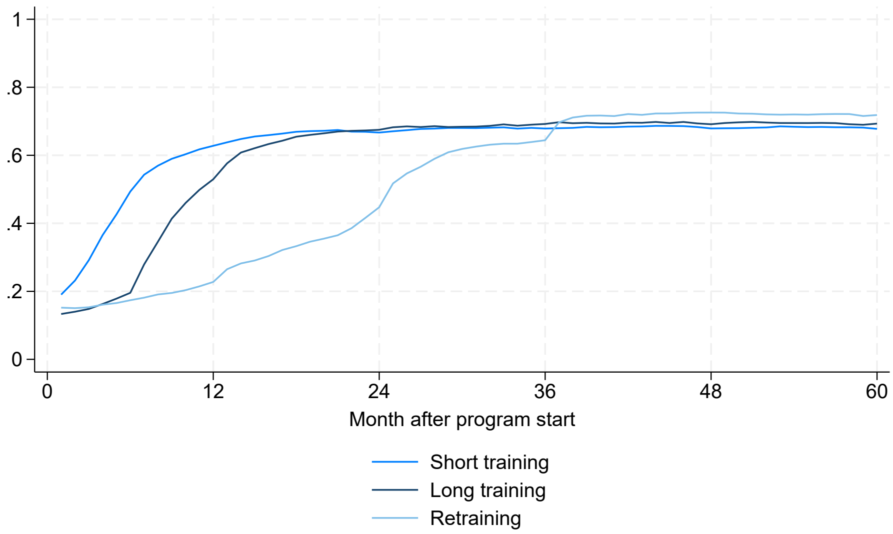
A.2 Descriptive Statistics

Figure A.6: Distribution of Individuals over Different Course Sizes



Notes: This figure plots the distribution program participants in the estimation sample over different course sizes. It displays the total number of participants on the y-axis.

Figure A.7: Average Employment Rate for Different Program Types



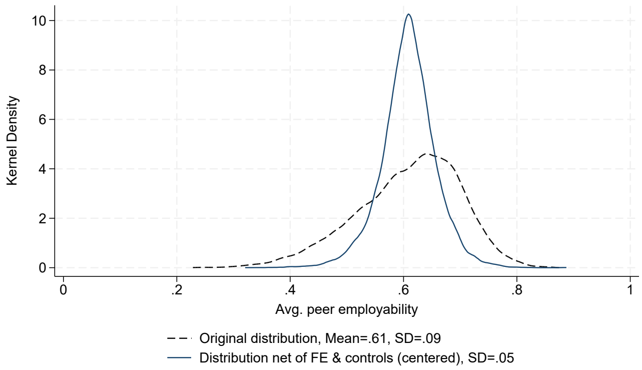
Notes: The figure depicts the average employment probability by month after program start separately for the three program types.

Table A.4: Occupation groups across Programs

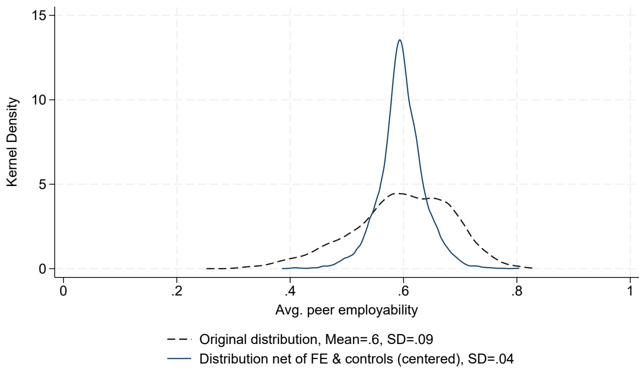
	Short training		Long training		Retraining	
	mean	sd	mean	sd	mean	sd
Production of raw materials and goods, and manufacturing	0.19	0.39	0.20	0.40	0.23	0.42
Construction, architecture, surveying and building services	0.02	0.13	0.03	0.16	0.00	0.07
Natural sciences, geography and informatics	0.04	0.20	0.07	0.25	0.03	0.18
Traffic, logistics, safety and security	0.26	0.44	0.23	0.42	0.16	0.36
Commercial services, trading, sales and tourism	0.05	0.22	0.03	0.17	0.04	0.20
Business organization, accounting, law and administration	0.17	0.38	0.14	0.35	0.18	0.39
Health care, social sector and education	0.24	0.43	0.27	0.44	0.35	0.48
Philology, literature, humanities, & others	0.02	0.15	0.04	0.19	0.00	0.07
Observations	33160		11513		11461	

Notes: Summary statistics (mean and standard deviation) calculated at the individual level.

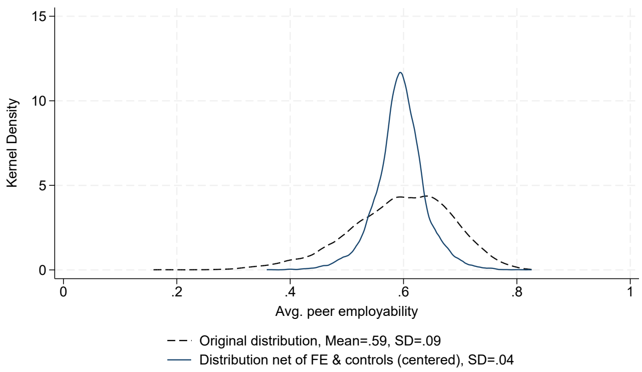
Figure A.8: Raw and Net Variation in Average Peer Employability



(a) Short Training



(b) Long Training



(c) Retraining

Notes: The figure plots the raw distribution of the average peer employability (dashed line) and the distribution of the average peer employability net of provider x competence level x occupation group, month x year x occupation group fixed effects and course controls (solid line) separately by program type. The residual distribution is centered at the mean. SD refers to the standard deviation.

A.3 Identifying Assumptions and Validity Checks

A.3.1 Resampling Test

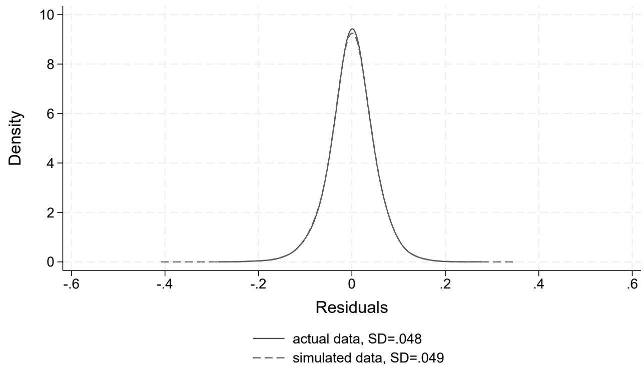
To provide evidence that the residual variation in our employability variables results from random fluctuation, we ran a series of simulations (similar to Bifulco et al., 2011). In each simulation, we implement the following steps. First, we randomly reallocate program participants to groups of the same size within the same provider x competence level x occupation group cell. Second, we estimate the residual variation in average peer employability in the simulated sample. Third, we use a Kolmogorov-Smirnov test to check whether the distribution of the residual variation in peer employability is different in the simulated sample compared to our original sample. Table A.5 reports summary statistics from the 500 Kolmogorov-Smirnov tests. For short training programs, using a confidence level of 5%, we reject the null hypothesis that the two distributions are equal in only 9.6% of cases, which supports the assumption that variation in residual average peer employability results from random fluctuations. However, for long training and retraining, we often reject the null hypothesis. On one hand, this result undermines a causal interpretation of the estimates for these types of programs. On the other hand, it shows that the test allows us to discriminate between data that support or not our assumption, thus strengthening our confidence that we can interpret as causal the results for short training programs. Figure A.9 further plots the distribution of observed and simulated residual average peer employability for the different programs.

Table A.5: Kolmogorov-Smirnov tests using simulated groups

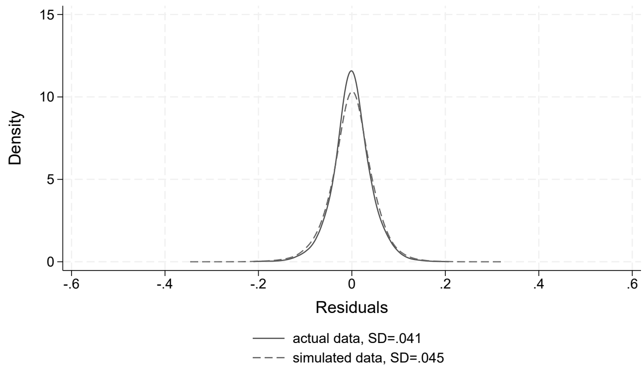
	Share of rejection at 5%	p25 p-value	p50 p-value	p75 p-value
Short training	9.60%	0.12	0.27	0.51
Long training	100.00%	0.00	0.00	0.00
Retraining	48.20%	0.02	0.05	0.13
Number of tests/simulated samples	500			

Notes: We simulate 500 samples by randomly allocating participants to groups of the same size within the same provider-competence level-occupation group cell. After estimating the residual variation in peer employability in each simulated sample, we use a Kolmogorov-Smirnov to check if the simulated distribution is equal to the one in our original sample. The table reports the 25th, 50th and 75th percentiles from the distribution of p-values across the 500 tests, as well as the share of rejections when using a 5% confidence level.

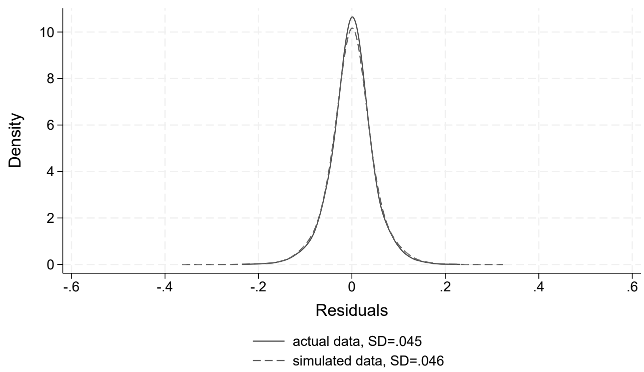
Figure A.9: Observed and Simulated Residual Variation in the Average Peer Employability



(a) Short Training



(b) Long Training



(c) Retraining

Notes: This figure plots the observed (solid line) and simulated (dashed line) distribution of the residual average peer employability by program type. SD refers to the standard deviation.

A.3.2 Systematic Correlation of Own and Peer Employability

Table A.6: Exogeneity Test Controlling for the Average Employability in the Pool

	(1)	(2)	(3)
	Short Training	Long Training	Retraining
Mean peer employability ($\bar{X}_{ipct}$)	-0.001	-0.007***	-0.004***
	(0.001)	(0.001)	(0.001)
Mean peer employability (pool)	-0.133***	-0.131***	-0.129***
	(0.002)	(0.004)	(0.004)
Provider x competence level x occupation group FE	✓	✓	✓
Month x year x occupation group FE	✓	✓	✓
Additional Controls	✓	✓	✓
N	33160	11513	11460

Notes: This table displays the coefficients of a regression of the individual employability on the average employability in the course and in the pool of participants starting in the same month. Standard errors are clustered at the course level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

A.3.3 Systematic Correlation of Own Employability and Course Dummies

Table A.7: Orthogonality of Own Employability and Course Dummies

	Short training	Long training	Retraining
F-statistic	0.856	0.497	0.576
P-value	0.766	1.000	1.000
Observations	33160	11513	11460

Notes: F-tests and corresponding p-values from a regression of the residualized own employability (net of Provider x competence level x occupation group FE, Month x year x occupation group FE, and course level controls) on course fixed effects.

A.3.4 Systematic Correlation of Peer Employability and other Predetermined Characteristics

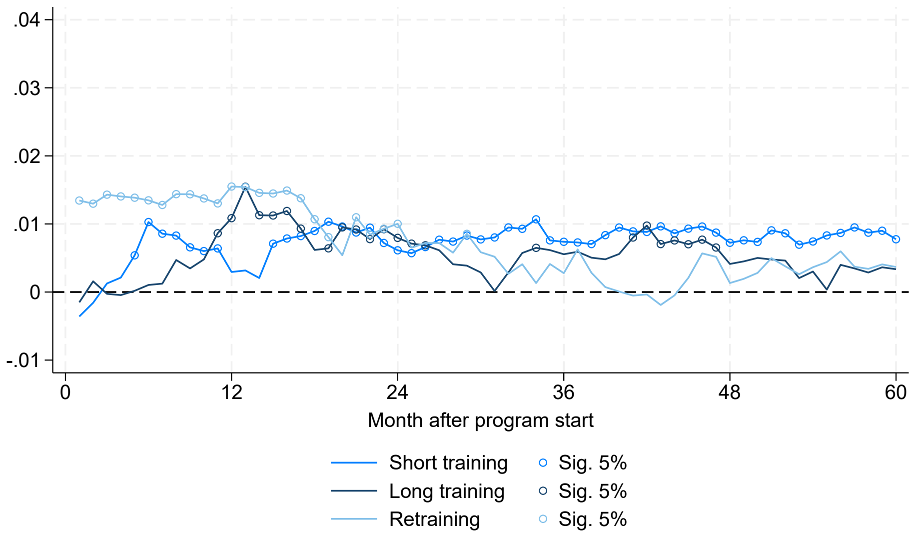
Table A.8: Correlation of Peer Employability with Individual Characteristics

	(1) Short Training	(2) Long Training	(3) Retraining
Age	-0.0001**	-0.0001*	-0.0000
	(0.0000)	(0.0000)	(0.0001)
Female	0.0026***	0.0016	-0.0007
	(0.0008)	(0.0011)	(0.0011)
Not German	-0.0030**	0.0007	-0.0003
	(0.0010)	(0.0012)	(0.0012)
Bad health	-0.0008	-0.0002	0.0017
	(0.0009)	(0.0013)	(0.0013)
Married	0.0007	0.0015	0.0009
	(0.0006)	(0.0008)	(0.0009)
Number of kids	0.0010**	-0.0005	0.0015**
	(0.0004)	(0.0005)	(0.0005)
Searching full-time	-0.0000	0.0013	0.0006
	(0.0006)	(0.0008)	(0.0009)
High education	-0.0006	0.0004	0.0015
	(0.0009)	(0.0010)	(0.0012)
Vocational training	0.0028*	-0.0010	0.0021
	(0.0013)	(0.0015)	(0.0022)
High-skilled	-0.0024***	-0.0012	0.0011
	(0.0006)	(0.0008)	(0.0009)
Unemployment duration at program start	0.0000	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)
Months employed in the last 2 years	0.0003***	0.0000	0.0003**
	(0.0001)	(0.0001)	(0.0001)
Months OLS in the last 2 years	0.0004***	0.0002	0.0002
	(0.0001)	(0.0001)	(0.0001)
Earnings in last the 2 years (1000 EUR)	0.0001***	0.0001*	0.0000
	(0.0000)	(0.0000)	(0.0000)
Months employed in the last 10 years	0.0000	0.0000*	0.0000
	(0.0000)	(0.0000)	(0.0000)
Months OLS in the last 10 years	0.0000	0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0000)
Earnings in the last 10 years (1000 EUR)	0.0000	-0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)
Dummies for labor market status before inflow:			
Employment	0.0426	0.0007	0.0025*
	(0.0297)	(0.0011)	(0.0012)
Self-employment	0.0447	-0.0030	-0.0024
	(0.0298)	(0.0032)	(0.0048)
Other employment	0.0435	-0.0026	0.0023
	(0.0297)	(0.0024)	(0.0026)
Out of labor force	0.0429	0.0010	0.0000
	(0.0297)	(0.0017)	(0.0019)
Education/Apprenticeship	0.0418	0.0009	0.0026
	(0.0297)	(0.0017)	(0.0021)
ALMP	0.0401	0.0007	0.0006
	(0.0297)	(0.0020)	(0.0018)
Disability	0.0435	0.0042	-0.0024
	(0.0297)	(0.0024)	(0.0026)
Other services/programs	0.0457	-0.0069	0.0021
	(0.0298)	(0.0041)	(0.0037)
Spell splitting	0.0426	0.0002	0.0002
	(0.0298)	(0.0027)	(0.0028)
Missing	0.0393	0.0000	0.0000
	(0.0297)	(0.0000)	(0.0000)
P-val: joint F-test	0.0000	0.0035	0.0002
Own employability	✓	✓	✓
Provider x competence level x occupation group FE	✓	✓	✓
Month x year x occupation group FE	✓	✓	✓
Additional Controls	✓	✓	✓
N	33160	11513	11460

Notes: The table presents estimates from a regression of peer employability on a range of individual characteristics, conditional on own employability and the usual controls from Equation (1). Standard errors are clustered at the course level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

A.4 Further Results

Figure A.10: Monthly Effects of Peer Employability on All Employment



Notes: The figure presents the estimated effects (in percentage points/100) of a one-standard-deviation increase in mean peer employability on the individual probability to be employed in any job in the months 1-60 after program start. Significant effects at the 5 percent level are marked by circles. On top of the mean peer employability, the underlying model includes the individual ex-ante employability, a vector of course-level controls, provider x competence level x occupation group fixed effects and month x year x occupation group fixed effects. Standard errors are clustered at the course level.

Table A.9: The Effect of Other Average Peer Characteristics

	Total employment in month 60			**Log total earnings month 60**		
	ST	LT	RT	ST	LT	RT
Peer earnings in last 2 years (1000 euro)	9.804***	0.359	10.073**	0.034***	0.004	0.011
	(2.980)	(3.667)	(4.078)	(0.007)	(0.009)	(0.009)
Peer months of employment in last 2 years	13.412***	1.621	10.702***	0.039***	0.005	-0.001
	(2.737)	(3.645)	(4.120)	(0.006)	(0.009)	(0.009)
Peer months of employment in last 10 years	1.032	6.081	6.608*	0.013**	0.020**	0.003
	(2.813)	(3.748)	(3.781)	(0.006)	(0.009)	(0.008)
Individual-level controls	✓	✓	✓	✓	✓	✓
Provider x competence level x occupation group FE	✓	✓	✓	✓	✓	✓
Month x year x occupation group FE	✓	✓	✓	✓	✓	✓
Course-level controls	✓	✓	✓	✓	✓	✓
Observations	33160	11513	11460	33160	11513	11460

Notes: All specifications control for course-level controls, individual age, gender, nationality, education, vocational degree, unemployment duration at program start as well as the individual labor market attachment. Standard errors are clustered at the course level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A.10: Effects of Peer Employability on Earnings using Alternative Outcomes or Models

	Short training	Long training	Retraining
	(1)	(2)	(3)
Panel A - Earnings in first job (y=log(earnings) with y=0 if earnings=0)			
$\bar{X}_{(-i)pct}$	0.021***	-0.011	-0.023*
	(0.008)	(0.012)	(0.012)
Panel B - Earnings in first job (y=earnings, linear model)			
$\bar{X}_{(-i)pct}$	0.869***	-0.016	-0.216
	(0.166)	(0.221)	(0.213)
Panel C - Earnings in first job (y=earnings, Poisson model)			
$\bar{X}_{(-i)pct}$	0.025***	-0.003	-0.007
	(0.005)	(0.007)	(0.007)
Panel D - Total earnings in month 60 (y=log(earnings) with y=0 if earnings=0)			
$\bar{X}_{(-i)pct}$	0.067***	0.017	-0.002
	(0.015)	(0.021)	(0.020)
Panel E - Total earnings in month 60 (y=earnings, linear model)			
$\bar{X}_{(-i)pct}$	2282.007***	1169.957***	850.207***
	(252.968)	(381.331)	(307.173)
Panel F - Total earnings in month 60 (y=earnings, Poisson model)			
$\bar{X}_{(-i)pct}$	0.040***	0.021***	0.019***
	(0.005)	(0.006)	(0.006)
Provider x competence level x occupation group FE	✓	✓	✓
Month x year x occupation group FE	✓	✓	✓
Additional Controls	✓	✓	✓
Observations	33160	11513	11460

Notes: X_{ipct} designates the own employability and $\bar{X}_{(-i)pct}$ the leave-one-out mean peer employability. Standard errors (in round brackets) are clustered at the course level. Effects are reported in terms of SD increases. Earnings are expressed in terms of prices in 2010. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.11: Heterogeneity in Effects of Peer Employability Depending on Own Employability: Poisson model

	Short training	Long training	Retraining
	(1)	(2)	(3)
Panel A - Earnings in First Job			
PE low employability	0.016**	-0.011	-0.017*
	(0.006)	(0.008)	(0.008)
PE high employability	0.031***	-0.003	0.005
	(0.006)	(0.008)	(0.009)
P-value difference	0.02	0.30	0.02
N	33160	11513	11460
Panel B - Total Earnings			
PE low employability	0.043***	0.018*	0.006
	(0.006)	(0.007)	(0.008)
PE high employability	0.030***	-0.006	0.008
	(0.005)	(0.007)	(0.007)
P-value difference	0.02	0.00	0.82
N	33160	11513	11460
Provider x competence level x occupation group FE	✓	✓	✓
Month x year x occupation group FE	✓	✓	✓
Additional Controls	✓	✓	✓

Notes: All specifications control for course-level controls and individual employability. Standard errors (in round brackets) are clustered at the course level. Peer effects (PE) are reported in terms of SD increases (see Table 2). Earnings are in prices of 2010 and measured euro. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

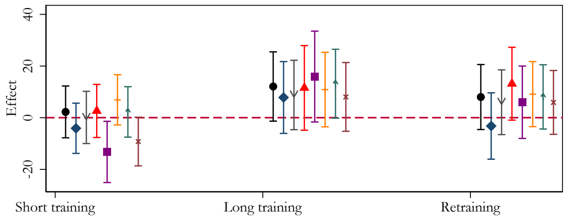
Table A.12: Spillover and Competition Effects on Earnings: Poisson model

	Short training			Long training			Retraining		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A - Earnings in First Job									
$\bar{X}_{(-i)pct}$	0.022***	0.033***	0.041***	-0.005	-0.001	-0.000	-0.009	-0.005	-0.009
	(0.005)	(0.006)	(0.007)	(0.007)	(0.009)	(0.010)	(0.007)	(0.009)	(0.009)
R_{ipcot}		0.016***	0.019***		0.007	0.004		0.005	-0.000
		(0.005)	(0.005)		(0.008)	(0.009)		(0.008)	(0.008)
$\bar{X}_{(-i)pct}$ (tight)			0.022**			0.001			0.006
			(0.007)			(0.010)			(0.013)
R_{ipcot} (tight)			0.010			0.011			0.015
			(0.006)			(0.010)			(0.010)
P-value diff: $\bar{X}_{(-i)pct}$			0.031			0.486			0.977
P-value diff: R_{ipcot}			0.020			0.277			0.023
N	33160	33160	33160	11513	11513	11513	11460	11460	11460
Panel B - Total Earnings									
$\bar{X}_{(-i)pct}$	0.037***	0.048***	0.053***	0.019***	0.017*	0.016	0.017**	0.015*	0.023**
	(0.004)	(0.005)	(0.007)	(0.006)	(0.007)	(0.009)	(0.006)	(0.007)	(0.008)
R_{ipcot}		0.016***	0.020***		-0.004	-0.003		-0.002	0.000
		(0.004)	(0.004)		(0.006)	(0.006)		(0.006)	(0.006)
$\bar{X}_{(-i)pct}$ (tight)			0.041***			0.019*			-0.004
			(0.006)			(0.008)			(0.011)
R_{ipcot} (tight)			0.010*			-0.005			-0.008
			(0.004)			(0.007)			(0.008)
P-value diff: $\bar{X}_{(-i)pct}$			0.110			0.786			0.017
P-value diff: R_{ipcot}			0.007			0.742			0.173
N	33160	33160	33160	11513	11513	11513	11460	11460	11460

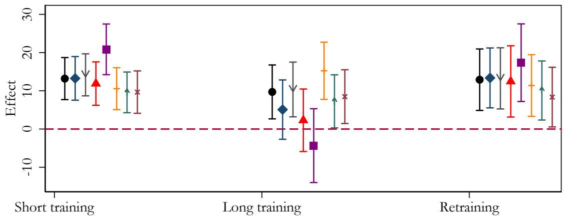
Notes: $\bar{X}_{(-i)pct}$ designates the leave-one-out mean peer employability and R_{ipcot} the percentile rank. All specifications include 30 percentile dummies to control for own employability, course-level controls, provider x competence level x occupation group FE and month x year x occupation group FE. Standard errors are clustered at the course level. Effects are reported in terms of SD increases. Earnings are in prices of 2010 and measured in euro. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

A.5 Robustness Checks

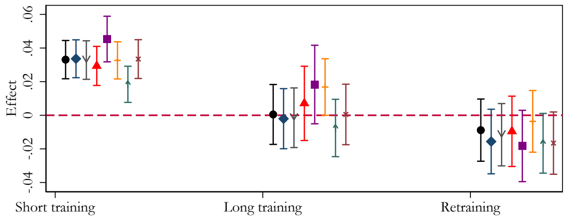
Figure A.11: Robustness Checks: Effect of Peer Employability



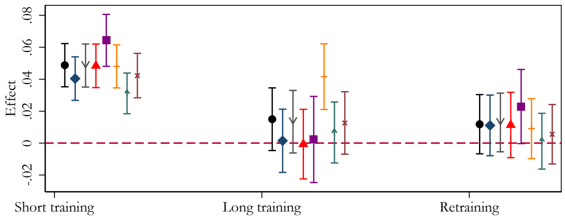
(a) Search Duration First Job (in days)



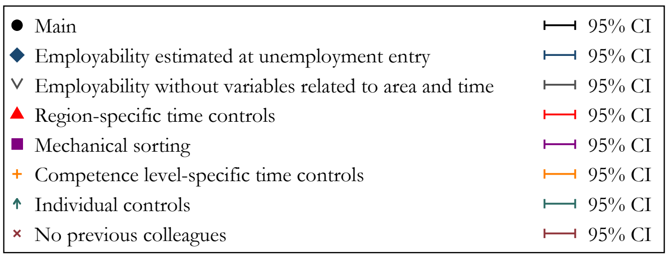
(b) Total Employment in month 60



(c) Log Earnings in First Job

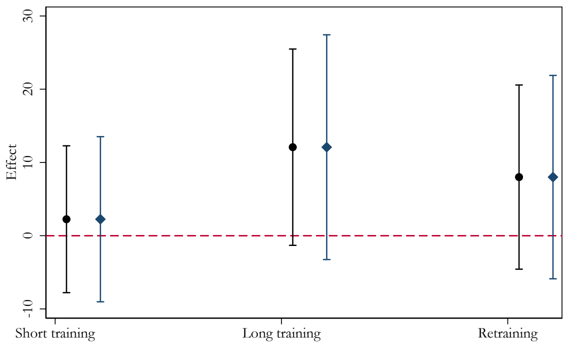


(d) Log Total Earnings in month 60

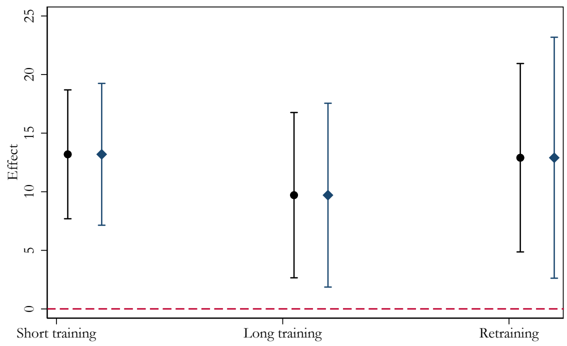


Notes: This figure summarizes the results of the main specification ("Main") and various robustness checks described in Section 6. It shows point estimates and the corresponding 95 percent confidence intervals.

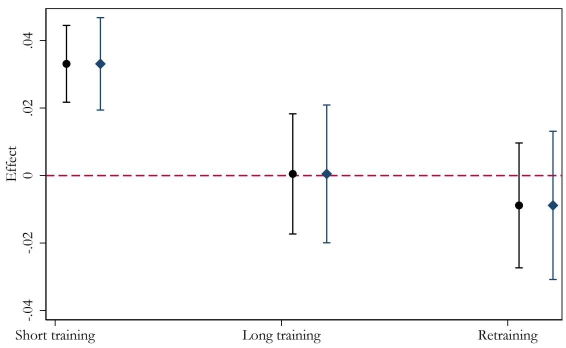
Figure A.12: Robustness Checks: Clustering



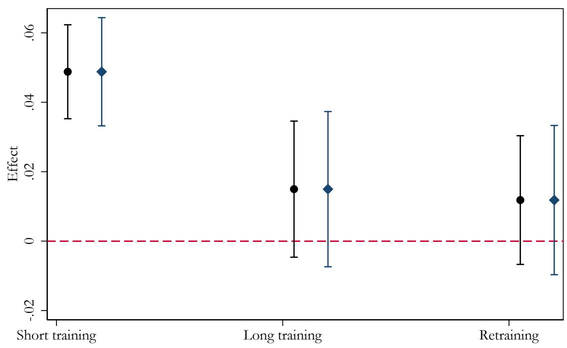
(a) Search Duration First Job (in days)



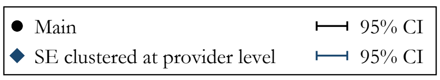
(b) Total Employment in month 60



(c) Log Earnings in First Job

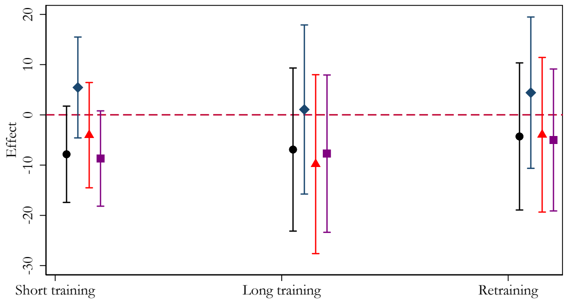


(d) Log Total Earnings in month 60

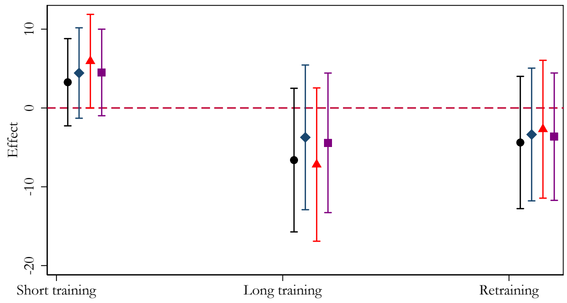


Notes: This figure summarizes the results of the main specification ("Main") which clusters standard errors at the course level and a specification clustering at the level of providers. It shows point estimates and the corresponding 95 percent confidence intervals.

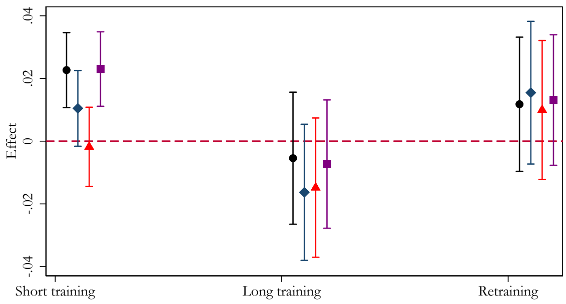
Figure A.13: Robustness Checks: Rank Specification



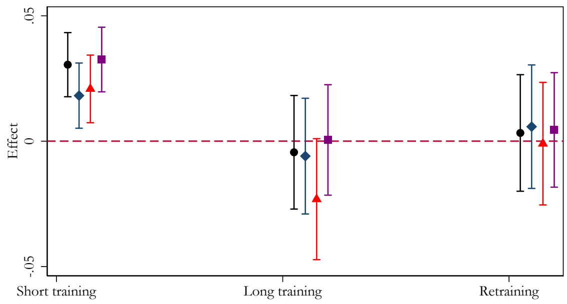
(a) Search Duration First Job (in days)



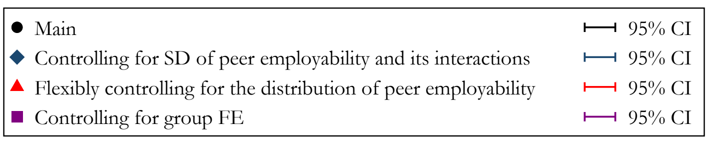
(b) Total Employment in month 60



(c) Log Earnings in First Job



(d) Log Total Earnings in month 60



Notes: This figure summarizes the results of the main rank specification ("Main") and various robustness checks described in Section 6.3. It shows point estimates and the corresponding 95 percent confidence intervals.

A.5.1 Measurement Error through Unobserved Peers

There is a possibility that we do not observe all individuals in a training program. The data covers all jobseekers who are participating in publicly sponsored training measures and register at the local employment agencies. We do not observe individuals participating in a training program on their own or on their employer's initiative and do so without registering the employment agencies. There are different groups that could self-select into vocational training programs without being registered. First, there could be individuals who are integrated in the labor market and are either employed or self-employed. At the same time, the likelihood that those individuals would participate in long, full-time courses is very low. If employed, employers would need to grant them leave of absence or in case of self-employment, they would incur major costs of leaving their business. Moreover courses directed at the integration of unemployed individuals into the labor market will not be of great interest for those who already have a job. As for the non-employed individuals, we might not observe women returning to the labor market, non-employed individuals who are not eligible for unemployment assistance and non-registered recipients of social assistance. Nevertheless, all three groups face substantial incentives to register as unemployed if willing to participate in training measures. By registering they could still get access to funding for the course-related costs, for example. By restricting the analysis to full-time courses and excluding courses that are specifically directed at employed individuals the existence of unobserved participants is highly unlikely. A small fraction of unobserved peers would lead to a attenuation bias that is negligible, assuming that there is no selection into courses that specifically relates to quality. Missing and observed data can come from arbitrarily different distributions (e.g., participants missing in the data may be more skilled than the ones observed) but the distribution needs to be independent of group assignment and ϵ_{ipct} after controlling for fixed effects and course controls (Ammermueller and Pischke, 2009; Sojourner, 2013).

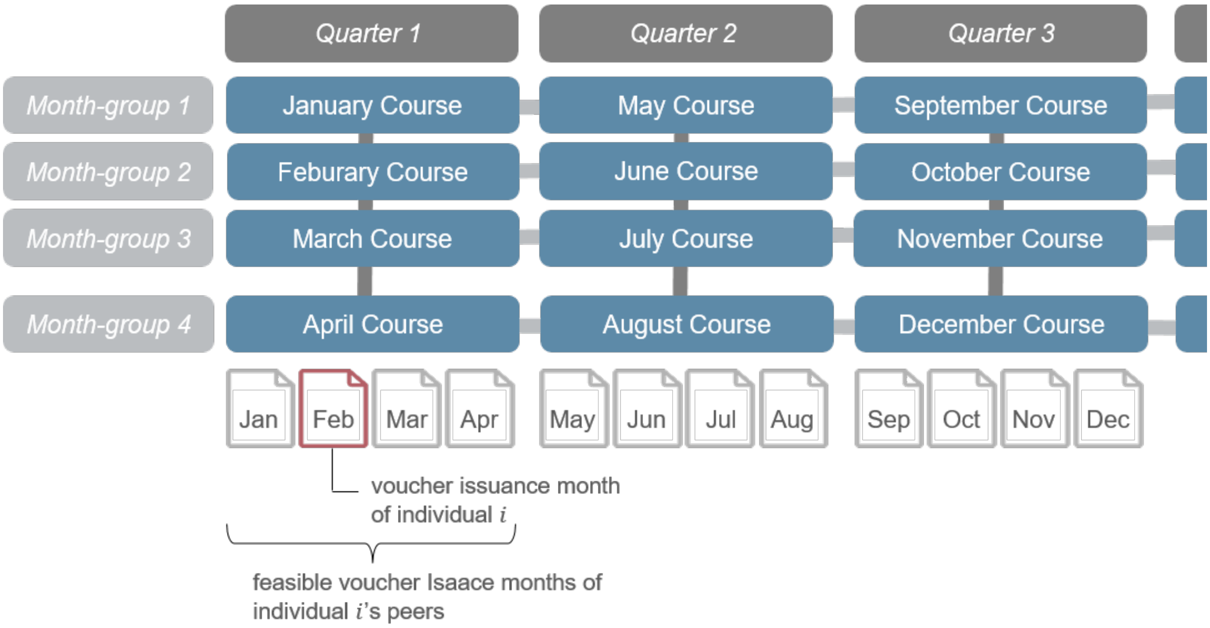
A.5.2 Mechanical Sorting

If individuals select into courses based on preferences that are provider and month-specific, the three-month redemption period of the vouchers might provoke some mechanical sorting which might aggravate the sorting issue. Such sorting is difficult to control for since it is not clear whether it takes place and if there exist specific sorting patterns. What can be controlled for is sorting that follows the same cycle in each quarter.

The intuition behind the identification strategy is illustrated in Figure A.14: A jobseeker obtains a voucher in February and decides to participate in a course starting in April at a particular provider. Her course mates may have obtained their vouchers earlier or later than her, i.e., in the months from January to April. Because of the three month redemption period, the jobseeker cannot be grouped with program participants who obtained their vouchers before January nor with participants who obtain their vouchers starting from May. At the same time, no participant in the April course could select into courses starting before January or later than July. As for the August course, no participant could start a course earlier than May or later than November.

In line with this reasoning, we can compare all courses starting in the months of April, August and December at a particular provider without participants being able to self-select into the respective other courses. Depending on the start dates of the courses, four groups naturally arise which we label *month groups*: January-May-September, February-June-October, March-July-November and April-August-December. They are listed as rows in Figure A.14. Conditional on the choice of provider and the month group, the entry into a specific course is driven by the voucher issuance date and the preferences for specific months in a quarter (which

Figure A.14: Identification Strategy Correcting for Mechanical Sorting



Notes: The figure depicts feasible comparison groups depending on the course start month in rows. These groups are provider-specific and contain courses that are each four months apart. The bottom of the figure illustrates how individuals can select into a specific course month depending on their voucher issuance exemplary for the month April. Columns designate year-specific quarters, i.e. 4-month divisions of the calendar year which serve as seasonality controls.

are assumed to follow the same pattern for every quarter). The voucher issuance is unlikely to be manipulated. Vouchers are issued in meetings with caseworkers, whose appointment time can hardly be influenced by the jobseekers. It depends on their entry into unemployment and the efficiency of administrative processes. Participants can sort across provider-specific month groups but not within.

A.5.3 Compositional Changes and other Mechanisms

There are other possible mechanisms than knowledge spillovers and competition which could play a role in the context of labor market training. Since peer groups are not perfectly stable throughout the course, compositional changes might cause some of the observed effects. We thus perform a heterogeneity analysis where we compare peer effects in courses with a high and a low share of dropouts. Courses with a high share of dropouts are courses where the share of dropouts exceeds the median in the sample. The results are shown in Appendix Table A.13. Results for short training are similar and not statistically different for both types of courses.

Other possible mechanisms discussed in the literature include social pressure and so-called teacher-effects. Social pressure might lead jobseekers to respond to the job search behavior of their peers, e.g. look for jobs faster once they observe highly employable peers find a job (Mas and Moretti, 2009; Fu et al., 2019). Nevertheless, we find little evidence for a shortening of the job search duration in response to a higher peer group quality, which speaks against this mechanism playing a dominant role. Finally, course instructors might endogenously respond to the average group quality and adapt their teaching style or the course content. While such responses cannot be ruled out, they are unlikely to be the main explanation of the results. Trainings are often based on standardized curricula. If jobseekers were systematically selecting into specific courses offered by popular and good teachers, our peer effect estimates might pick up these teacher effects. However, such sorting behavior is limited by the voucher validity and capacity constraints.

Table A.13: Effects by Compositional Change

	(1) Short Training	(2) Long Training	(3) Retraining
Panel A - Search duration			
Low share of dropouts	1.249	12.170	13.653
	(5.655)	(8.175)	(9.074)
High share of dropouts	10.987	14.247*	5.965
	(7.035)	(8.162)	(7.278)
P-value difference	0.197	0.823	0.455
Panel B - Total Employment			
Low share of dropouts	13.357***	12.167***	7.171
	(3.072)	(4.481)	(5.841)
High share of dropouts	10.924***	6.763	15.999***
	(4.045)	(4.454)	(4.761)
P-value difference	0.564	0.307	0.189
Panel C - Earnings in First Job (if > 0)			
Low share of dropouts	0.032***	-0.005	0.016
	(0.007)	(0.011)	(0.014)
High share of dropouts	0.030***	0.005	-0.025**
	(0.008)	(0.011)	(0.011)
P-value difference	0.752	0.419	0.013
Panel D - Total Earnings (if > 0)			
Low share of dropouts	0.050***	0.007	0.014
	(0.008)	(0.012)	(0.014)
High share of dropouts	0.044***	0.024*	0.011
	(0.010)	(0.012)	(0.011)
P-value difference	0.536	0.222	0.847
Provider x competence level x occupation group FE	✓	✓	✓
Month x year x occupation group FE	✓	✓	✓
Additional Controls	✓	✓	✓

Notes: All specifications control for course-level controls and individual employability. The table presents estimates for peer effects for courses with low and high shares of dropouts, which are reported in terms of SD increases. Standard errors (in round brackets) are clustered at the course level. Earnings are in prices of 2010 and measured in log(euro). * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$