Peer Effects in Active Labour Market Policies

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Abstract

This paper studies peer effects in the context of public sponsored vocational training for job-seekers in Germany. Using rich administrative data, I investigate how individual labour market outcomes of program participants are affected by the peer "quality" in the program focusing on the employability of the peers. To identify a causal effect, I exploit quasi-random variation in the peer group composition within courses offered by the same training providers over time. Results show that an increase in the average peer group employability has positive and considerable effects on individual employment and earnings. For participants in short and long vocational training programs, the effects materialize after the average planned program duration, and persist in the medium and long run. I do not find persistent effects for individuals in retraining. Furthermore, the results suggest that peer employability has non-linear effects which differ across program types.

Keywords: peer effects, active labour market policy, vocational training

JEL Codes: J64, J68

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1 Introduction

There has been a large interest in the evaluation of active labour market policies (ALMP) over the last decades.[1] The earlier literature has primarily focused on the short and long-term effects of job search assistance and training programs on individual labour market outcomes of unemployed workers. In recent years and mainly enabled by the availability of large administrative data, growing attention has been paid to identifying heterogeneity in treatment effects and the best rules for allocation. So far however, little is known about the role of the course composition and social spillovers within such programs. Program participants might benefit from being surrounded by highly employable peers through various channels such as information transmission, motivational effects or because they experience some pressure to conform with peer behaviour. Likewise, a certain degree of homogeneity in the group could be desirable as homogeneous groups could facilitate targeted training and ease collaboration.

The significance of the social context for individual decision making and behaviour has been highlighted by findings in many areas of economic and sociological research.[2] In other educational contexts, peer behaviour and ability have been found to be significant determinants of individual school grades, health and labour market outcomes (Sacerdote, 2011; Paloyo, 2020). The literature on job search finds that social networks facilitate information transmission, reduce search frictions and improve the match quality between firms and workers (Ioannides and Loury, 2004). Moreover, there exists evidence for social multiplier effects in labour supply which emerge from social norms set by peers (Kondo and Shoji, 2019; Schneider, 2019). Understanding the role of the peer group composition in labour market programs is thus of great interest for policy, as it will inform about the efficiency of the allocation process of job-seekers to programs.

[1] For an overview, see e.g. Heckman et al. (1999); Martin and Grubb (2001); Card et al. (2010, 2018), Crépon and van den Berg (2016).

[2] See Jackson et al. (2017) for a comprehensive review of the literature on the economic impact of social networks.

This paper studies peer effects in the context of public sponsored training in Germany. I investigate how individual labour market outcomes of program participants are affected by the the employment prospect of their peers. In order to describe the the peer "quality" in a group, I construct a sophisticated measure of peer employability that summarizes a large number of individual and labour market characteristics. In my analysis, I first consider the average exposure to highly employable peers and finally allow for of non-linearities in the peer effects. I have access to rich administrative data on the universe of job-seeking individuals participating in public sponsored training programs and their labour market outcomes for several years before and after their participation. The analysis focuses on classic vocational training and retraining programs in the years 2007-2012. Peer groups are identified by linking individuals who attend the same training program and have some overlap in time.

The identification and estimation of peer effects pose a number of methodological challenges due to the endogeneity in peer group formation and simultaneity in behaviour.[3] To overcome them, I focus on the reduced-form specification of the linear-in-means model of social interactions widely used in the literature and follow an approach similar to Hoxby (2000) and Elsner and Isphording (2017). I rely on the limited validity of training vouchers to exclude the possibility of an endogenous peer group formation and exploit idiosyncratic changes in the group composition within courses offered by the same training providers over time.

Results show that a greater exposure to highly employable peers has a positive impact on individual labour market outcomes after program participation. I find a heterogeneous pattern of peer effects across short, long vocational training and retraining programs which points at differences in the relative importance of peers and the educational content in these programs. First, for all program types, I find that an increase in the average peer employability significantly increases individual employment around one year after program start. The effects on employment are most persistent and robust for short training programs. They materialize later and fade out earlier for long training. Second, program participants who are surrounded by more employable peers experience substantial in-

[3] See e.g. Manski (1993) and Blume et al. (2011) for an overview.

creases in their earnings right after program participation and cumulatively up to 5 years after program start. This holds in particular for individuals in short training programs. Finally, I find evidence for non-linearity in the employability peer effects which differs across program types.

This study contributes to different strands of literature. The first is the literature on program evaluation. Overall, public sponsored training programs have been shown to have little or even negative effects in the short run and positive and more substantial effects in the long run. See the recent meta-analyses by Card et al. (2010, 2018). Research looking at effect heterogeneity suggests that program effects vary significantly with the characteristics of participants (Heckman et al., 1997; Bitler et al., 2006; Bergemann and Van den Berg, 2008; Behaghel et al., 2014). Several studies posit for example that participants who have relatively bad labour market prospects benefit more from programs than those with a better outlook (Wunsch and Lechner, 2008; Knaus et al., 2020 and Card et al., 2018).

Building upon this heterogeneity in effects, a number of contributions have investigated treatment choices and developed best rules for allocation in ALMP (Eberts et al., 2002, Lechner and Smith, 2007, Frölich, 2008, Staghøj et al., 2010). Evidence clearly suggests potential benefits from introducing statistical treatment rules to assist caseworkers in assigning unemployed workers to program types. Nevertheless, the implementation of a functioning targeting system is challenging (see e.g. Behncke et al., 2009 or Colpitts and Smith, 2002). 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, positive employment effects of voluntary assignment materialize. While this literature informs about the efficient allocation of participants to programs based on individual characteristics, it does not look at how individuals interact with each other. My study contributes to this literature by examining compositional aspects. Moreover, it lays the groundwork to study feasible regrouping policies.

So far, there exists only scarce experimental evidence on peer effects in the context of

ALMP. Lafortune et al. (2018) analyse how the group composition affects the efficacy of a training program for low-skilled Chilean women. They find no evidence that group homogeneity with respect to employment prospects is beneficial for program participants' labour market outcomes. Programs participants rather benefit from a diverse peer group, in particular those with a more limited labour market attachement. Van den Berg et al. (2016) evaluate a job search assistance program in France where young unemployed workers were randomly assigned to either a search club or to a standard counselling program. Their results suggest that irrespective of the participant's own labour market prospects, being in a group with a lower mean group employability has a positive impact on the program's success. They find no indication that the degree of group homogeneity matters for the post-program labour market outcomes. My paper is the first to analyse peer effects in large scale training programs by using rich administrative data. It adds to the findings of the existing experimental studies by looking at a number of different program types and a wider population of participants. Moreover, I am able to observe individual labour market outcomes several years after program start and provide evidence on short-, medium and long-run effects.

I also contribute to the extensive literature on peer effects in education. Findings of this literature suggest that being exposed to a higher degree of peer ability positively affects individual learning outcomes while there appear to be important nonlinearities. See Sacerdote (2011, 2014), Epple and Romano (2011) or Paloyo (2020) for a more recent overview. Several studies suggest that highly achieving individuals benefit from the presence of other high ability students (e.g. Lavy and Schlosser, 2011). At the university level, studies find mostly small peer effects on performance from classmates and roommates (e.g. Stinebrickner and Stinebrickner, 2006; Arcidiacono et al., 2012; Brodaty and Gurgand, 2016; Booij et al., 2017) but large effects on social behaviour like drinking, cheating and fraternity membership (e.g. Sacerdote, 2001 and Gaviria and Raphael, 2001). My findings suggest that peer effects also play a role in educational programs for adults.

The paper proceeds as follows. Section 2 gives an overview of the institutional background, Section 3 introduces the data and reports some descriptive statistics. I define the peer variables of interest, discuss my identification and estimation strategy in Section 4. The results are presented in Section 5.

2 Institutional Background

Further vocational training (Förderung der beruflichen Weiterbildung, FbW) has traditionally been one of the most important instruments in German ALMP. The programs have the objective to update and increase the human capital of participants, to adjust their skills to technological changes, to provide professional degrees and facilitate career prospects.

Since 2003 the assignment of unemployed individuals to courses has been regulated by a voucher system. The features of this allocation system will be key for my identification strategy which I discuss in Section 4.2. Once a individual registers as unemployed, a caseworker reviews their labour 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 educational target, its geographical validity, and the maximum course fee to be reimbursed. It is valid for a period up to three months from the date of issuance. Within this period, the unemployed individual may choose between courses that fit the content stated on the voucher and are offered by certified providers. Caseworkers are instructed to not issue any course-specific recommendations. 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.

For my analysis, I aggregate different public sponsored training programs into groups according to their homogeneity with respect to educational contents and organisation. All considered types of trainings require full-time participation but differ considerably in duration and content. I distinguish between classic vocational training and retraining. Classic vocational programs cover a wide range of fields and may combine classroom training with practical elements such as on-the-job training. I differentiate between short programs with a maximum planned duration of 6 months (short training) and long programs with a planned duration of over 6 months (long training). Short-term programs have an average planned duration of about 3.7 months and offer minor improvements of skills. Longer vocational programs typically last between several months and one year and focus on maintaining, updating, adjusting, and extending occupational skills. Typical courses involve e.g. 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 (retraining) have the longest duration of 2-3 years and provide training for a new vocational degree according to the German system of vocational education.

3 Data and Sample Selection

The analysis is based on administrative data provided by the Institute for Employment Research (IAB) in Nuremberg. The data covers the universe of individuals participating in public sponsored labour market programs between 2007 and 2012 (Database of Program Participants (MTH)) and is enlarged by the Database of Registered Job-Seekers (ASU and XASU) and the Integrated Employment Biographies (IEB). All these sources are linked by a unique individual identifier. Moreover, I use a representative sample of individuals entering unemployment since 1998 which is used to measure individual and peer employability (see Section 4.1). 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 program participants (demographic information, labour market histories with daily accuracy and regional identifiers). As the entire population of registered program participants is covered, I am able to identify peer groups, meaning individuals who attended the same course together.

First, I restrict my analysis on the course level.[4] I focus on courses where individuals start within the same month, overlap for at least one day and exclude self-learning programs and special programs.[5] I further restrict the sample to courses where the number of

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

[5]Special programs usually target a particular sub-population of the labour market. The program *WeGebAU* for example, aims at providing low-qualified and older individuals with further education.

participants lies between 5 and 30. For reasons related to my identification strategy (see Section 4.2), I only consider training providers which offer courses once per month but multiple times over the time interval considered. Once the peer variables are constructed, I confine the estimation sample to individuals doing their first program. This yields a final sample of 47279 program participants.

Table 1 summarizes selected individual, course characteristics and outcome variables by program type. Panel A reports selected course characteristics. The majority of individuals is in short vocational training programs with a total of 2811 courses, followed by long vocational training with 1013 courses. Retraining programs comprise a sample of 1007 courses. The average number of courses per provider ranges from 5 to 7. On average there are about 11-12 individuals in one course. Figure A.2 in the Appendix shows the distribution of individuals over different course sizes. Most of the individuals start at the earliest entry date and spend most of the total course time in class. Total course costs depend on the duration of the programs, but are comparatively high for short training. All program types offer courses for a range of target occupations with different skill levels. Compared to retraining courses short and long training programs aim at integrating participants in more skill intensive jobs.

Panel B of Table 1 shows a selection of individual characteristics. Participants in short and long vocational training are on average 38 years old and a few years younger in retraining programs. 40 to 50 percent are female and about 10 percent of participants do not have a German nationality. With respect to education, 12 to 18 percent of participants have a high-school degree (Abitur) and 54 to 64 percent have completed some vocational training. Individuals in retraining programs are on average less educated, worked in lower paying jobs, and have been unemployed longer in the years prior to program participation. Differences between participants in short and long training are less pronounced. Overall, the descriptive evidence illustrates differences in organization, educational content and the composition of participants across program types. Given these differences, we might

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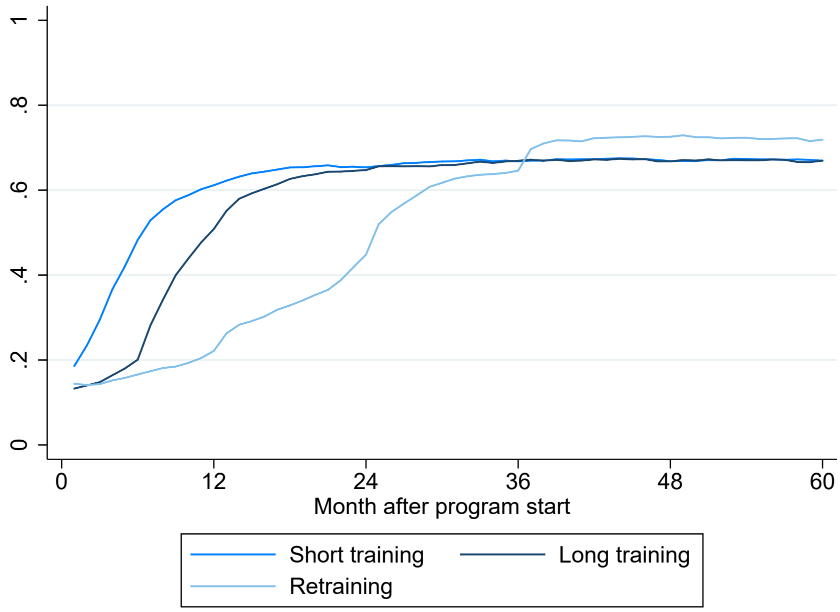
Other special programs are for example *Perspektive 50plus*, *Gute Arbeit für Alleinerziehende*. This information is recorded at the individual level and I exclude courses if all participants are funded via such special programs. A number of vocational courses offered in Germany allow for continuous entry. I 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.

Figure 1: Average employment rate for different program types



also expect peer effects to vary between short, long and retraining courses.

The key outcomes of my analysis are individual employment and earnings after program start. I observe the labour market status of program participants with daily precision and study short-, medium- and long-run outcomes for up to 5 years after they enter a program. I consider all types of employment (part-time and full-time) that are subject to social security payments.[6] Figure 1 shows the average employment by program type, up to 60 months after program start. Average employment is below 20 percent directly after the program starts and increases to about 65-70 percent after around 40 months. The employment rate takes different paths depending on the program type. Retraining programs which have the longest planned duration are characterized by the slowest increase but ultimately reach the highest level of employment. The average unemployment[7] rate, shown in Appendix Figure A.1, mirrors this picture.

Further labour market outcomes are described in Panel C of Table 1. I look at cumulative employment 60 months after program start, the corresponding cumulative earnings (in

[6]This does not include small-scale employment - so-called Mini-jobs - according to §8 SGB IV and §7 SGB V.

[7]Individuals are considered as unemployed if they are registered as a jobseeker at the Federal Employment Office, receive unemployment benefits (ALG I), unemployment assistance (ALG II) or subsistence allowance, or are participating in further labour market programs.

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	2811		1013		1007	
Number of providers	635		282		310	
Number of courses per provider	7.25	4.81	5.58	3.88	4.95	4.40
Course size	12.23	5.15	11.68	5.18	10.70	4.94
Fraction of ind. starting at earliest date	0.93	0.13	0.94	0.11	0.92	0.13
Average planned duration in months	3.68	1.63	9.07	3.11	22.66	7.70
Total hours in practice (Betrieb)	12.01	88.60	23.24	163.15	18.21	250.62
Total hours in class	427.66	239.36	966.15	417.95	2009.12	885.65
Total course costs (1000 EUR)	2.89	2.19	6.34	3.58	10.31	4.99
Target occupation w/ complex tasks	0.06	0.25	0.10	0.30	0.01	0.10
Target occupation w/ highly complex tasks	0.09	0.29	0.09	0.28	0.05	0.21
Panel B - Individual characteristics						
Age in years	37.71	10.87	37.80	9.79	34.62	8.16
Females	0.43	0.50	0.40	0.49	0.50	0.50
Non-German	0.10	0.29	0.11	0.32	0.11	0.31
Dummy for child under 15 yrs	0.18	0.39	0.22	0.41	0.29	0.45
High-school degree (Abitur)	0.17	0.38	0.19	0.39	0.12	0.33
Vocational training	0.64	0.48	0.60	0.49	0.54	0.50
Academic degree	0.08	0.27	0.09	0.29	0.03	0.16
Months unemployed at prg. start	10.24	20.70	12.27	22.67	11.96	22.06
Months employed (last 2 years)	13.69	8.48	12.71	8.59	12.81	8.52
Cum. earnings (last 2 years, 1000 EUR)	22.62	22.53	20.47	20.92	18.07	17.34
Number of programs in last 2 years	0.83	1.05	0.89	1.06	0.95	1.08
Panel C - Outcomes						
Cum. employment in month 60 (in days)	1106.75	592.32	1032.58	569.37	909.09	501.86
Log cum. earnings in month 60	9.72	3.17	9.64	3.19	9.68	2.84
Search duration for first job (in days)	478.74	766.994	571.57	738.360	743.02	639.509
Log daily wage in first job	3.15	1.32	3.11	1.34	3.03	1.28
Observations	28251		9614		8663	

Notes: All amounts in EUR are inflation adjusted.

logs) and some features of the first job (search duration and wage). Probably partially explained by different lock-in effects of the programs, individuals in retraining programs accumulate less employment and earnings than individuals in short or long training programs. They also earn comparatively less on their first job.

4 Empirical Strategy

4.1 Measuring Employability

The objective of this study is to quantify the impact of peer quality on individual post-program labour market outcomes. I focus on the average employment prospects of individual $i's$ peers before program start, referred to as (ex-ante) employability in what follows. I first define employability on the individual level and then construct a measure for peer employability as leave-one-out average. It is the sample average of individual $i's$ peers employability in group g:

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

Inspired by a procedure in Van den Berg et al. (2016)[8], I summarize individual background characteristics which should contain valid information on his or her employability in a single score. For this, I define employability as the probability of finding a long-term contract[9] within one year of entering unemployment. As the employment status without program participation is unknown for actual participants, I use a control population of comparable non-participants. This population consists of a random sample of individuals who enter unemployment in the same period as the program participants but do not participate in any program within the first year. I regress their employability on a large set of variables including demographic characteristics, information on health, education, skills, past labour market outcomes as well as information on the local labour market situation (measured at the time individuals enter unemployment). The estimated coefficients are then used to predict employability values for the sample of program participants. Ap-

[8] Also other studies use imputed measures of ability to study peer effects, see e.g. Burke and Sass (2013) or Thiemann (2017).

[9] Long-term contracts are defined as contracts that last for more than 6 months.

pendix Table 6 gives an overview of the variables considered. I use two different models for this prediction exercise: A logit model and a regularization method, the logit lasso. The latter performs classification tasks with binary outcomes and adds a penalty term[10] to the log-likelihood function, shrinking coefficients of less importance to zero. The logit lasso (henceforth referred to as lasso) has the appealing property of increasing the precision by considering important covariates only and dealing with multicollinearity. In the control sample, both models classify individual employability similarly well and achieve an accuracy of prediction of about 70 percent with the lasso performing slightly better. The distribution of the predicted individual and average employability measures are shown in Figures A.3 and A.4 in the Appendix. Both models result in a very similar predicted employability for program participants.

I account for the fact that individuals in the control population might fundamentally differ in characteristics from program participants by estimating logit models where the observations in the control population are reweighted to look like program participants using propensity score matching methods.[11] Appendix, Figure A.5 shows that reweighting shifts the mass of the predicted individual employability towards the center but that the distributions are still overlapping to a large extent.

This summary measure allows me to consider a variety of information without having to estimate a high-dimensional model. It entails a number of advantages compared to an analysis which considers a set of peer characteristics at the same time. First, it achieves a dimension reduction which allows for a more flexible analysis and an easier interpretation of peer effects.[12] Second, it is data-driven and does not rely on any prior knowledge of the strongest predictors of employability. The approach requires no further information than what the caseworker can observe in his assessment of the job-seeker. In order to address concerns related to a potential measurement error in the employability variable[13] and to

[10]I use the theoretically derived penality term, see e.g. Belloni et al. (2016).

[11]I estimate the conditional probability of being in the sample of program participants including the same set of variables which is also used in the employability model. These so-called propensity scores serve as a basis to construct weights using inverse probability weighting (IPW) and nearest neighbour matching with no replacement.

[12]It has been pointed out by Graham (2011) that is not straight forward to study the effects of multiple peer attributes simultaneously and that a ceteris paribus interpretation of such effects is difficult.

[13]Angrist (2014) points out that peer effects estimates are sensitive to measurement error. Feld and Zölitz (2017) show that in case of a random allocation of groups, measurement error attenuates the effects.

understand possible channels of peer employability, I will conduct a sensitivity analysis where I also consider a set of et of readily available peer characteristics separately.

4.2 Identification of Peer Effects

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 behaviour meaning that the effort one individual puts into a program depends on the effort of her peers and the other way around. 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 behaviour. I do not attempt to separate the effects, but estimate a joint effect[14]. Simultaneity in individual and peer employability is avoided by using employability measures that are determined before program start.

The selection problem arises because of common unobserved shocks at the group level on the one and endogenous peer group formation on the other hand. Two main approaches have been used to tackle these challenges in the educational context, which is close to my set-up. While a first set of studies relies on random assignment of individuals to groups (e.g. Sacerdote, 2001; Duflo et al., 2011; Carrell et al., 2013; Booij et al., 2017),[15] a second set of studies exploits quasi-random variation in the peer group composition controlling for selection into peer groups by including fixed effects at the school or grade level (e.g. Hoxby, 2000; Ammermueller and Pischke, 2009; Bifulco et al., 2011; Lavy et al., 2012; Elsner and Isphording, 2017; Carrell et al., 2018).[16]

I follow an approach closely related to this second strand of literature and identify peer effects using the variation in peer employability between courses offered by the same

[14]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 education for example matter for the effectiveness of a program, it might not be relevant whether it is peer education per se or the unobservable characteristics or behaviours correlated with education.

[15]Note that identification issues related to correlated unobservables and simultaneity in behaviour are pertinent also in case of random group allocation.

[16]Other approaches deal with identification issues 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).

training providers over time. Moreover, I only compare courses taking place in specific sets of calendar months and restrict my attention to providers where courses take place only once per month.[17] The idea is the following: Individuals can choose between a range of courses that take place at various providers and start at different dates up to three months after their voucher was issued.[18] Assuming now that providers offer similar courses multiple times throughout the observation period, job-seekers may sort into specific courses only within the period of validity of the voucher. By controlling for the provider choice and comparing only courses that are four months apart, I can overcome systematic self-selection into groups and control for shocks common to given months. An example is illustrated in Figure 2: A job-seeker 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 job-seeker cannot be grouped with program participants who obtained their vouchers in December (of the previous year) nor with participants who obtain their vouchers in 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 of this reasoning, I can compare all courses starting in the months of April, August and December.

Depending on the start dates of the courses, four groups naturally arise which I label as cohorts: January-May-September, February-June-October, March-July-November and April-August-December. Within each of these cohorts and conditional on the choice of provider, the entry into a specific course is beyond the influence of the participant herself but driven by her voucher issuance date which is unlikely to be manipulated.

[17]In fact, also shorter intervals (e.g. 2 weeks) could be chosen. The feasibility of the respective approach depends on the cyclicality of the time intervals (a closed circle of cohort-intervals is needed) and data availability (providers need to offer courses consecutively in those intervals).

[18]Note that individuals have a very limited choice with respect to the course content. It needs to match an educational target which is defined during the profiling process with the caseworker and depends on the qualifications of the job-seeker but also on the current labour market situation.

Figure 2: Example of comparable courses at a given provider

Notes: The figure depicts feasible comparison groups for a course staring in April, selected by individual i who receives a voucher in February of a specific year (depicted in red). The months in which i's peers possibly obtained their vouchers are depicted in grey. Courses in April can be compared to courses starting in August or December.

4.3 The Empirical Model

To implement the identification strategy I estimate the following linear-in-means model[19] separately for different types of training programs:

$$y_{ipct} = \alpha + \gamma X_{ipct} + \theta \bar{X}_{(-i)pct} + \pi W_{pct} + \lambda_{pc} + \zeta_t + \epsilon_{ipct}, \tag{2}$$

where the outcome of an individual i at a training provider p in cohort c and time t is a linear function of the individual's own observable characteristics X_{ipct}, being her own employability and unemployment duration at program start[20] and the leave-one-out mean employability of individuals in the same course $\bar{X}_{(-i)pct}$. The coefficient of interest 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. In studying effect heterogeneity, I also consider more flexible

[19]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 labour 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.

[20]I control of the individual unemployment duration at program start since this information cannot be captured in the employability measure but might matter for the selection into specific courses.

versions of $\bar{X}_{(-i)pct}$ using a cubic polynomial.

Finally, I include some course level characteristics[21], W_{pct}, provider-cohort, λ_{pc}, and seasonal fixed effects, ζ_t. Together they take care of correlated unobservables and endogenous group formation. Provider-cohort fixed effects control for all observable and unobservable mean differences across provider-cohort combinations that are constant over time. Seasonal fixed effects which correspond to 4-month divisions of the calendar year[22], control for correlated effects which change over time but are the same across providers and cohorts. Identification relies on the assumption that the variation in peer employability across courses within a given cohort at a given provider and after removing seasonality is uncorrelated with unobservable determinants of individual post-program outcomes. In other words, I assume that this residual variation results from random fluctuations and is not driven by endogenous sorting into specific courses, i.e. $E[\epsilon_{ipct} \mid X_{ipct}, \bar{X}_{(-i)pct}, W_{pct}, \lambda_{pc}, \zeta_t] = 0$.

4.4 Threats to Validity

There are two potential threats to my identification strategy. First, I need sufficient residual variation in the main peer variables and this variation needs to be exogenous. Second, there might be individuals in the peer group which are not observed in the data.

For an illustration of the first issue, consider the error term $\epsilon_{ipct} = \rho_{pct} + \eta_{ipct}$ in equation (2) where ρ_{pct} is a course-specific and η_{ipct} a zero-mean random component. For the identifying assumption to hold ρ_{pct} needs to be uncorrelated with all other regressors. That is, there can be no correlated unobservables that vary within cohorts and providers. This could for example be a different skill composition of the unemployed at different months caused by seasonal labour market fluctuation to which providers respond differently. Furthermore, there should be no endogenous sorting into peer groups such that job-seekers strategically manipulate the moment of voucher issuance. I argue that this is highly unlikely given the institutional framework considered. The voucher issuance time highly depends on the availability of caseworkers and the speed of bureaucratic procedures, which

[21] This includes course size, the average planned course duration, weekly hours of course, total hours spent in practice and class, total course costs and target occupation.

[22] The time unit t is thus 4 months.

are very difficult to predict for job-seekers.

Table 2 shows the distribution of the peer variables of interest by program type. The raw variation in the leave-one-out peer employability is reported in Panel A. The averages of the mean predicted employability variables range from 0.57 to 0.60. There is slightly less variation in retraining compared to the other program types. As could be expected from Figure A.4, the observed variation is slightly smaller for the lasso measure. Panel B examines the variation that is left after regressing the peer variables on provider-cohort fixed effects, seasonal fixed effects and course-specific variables which comprise course size, the average planned duration, weekly hours, number of hours in practice and class, total course costs and the target occupation. The residual variation is is still substantial, but reduced to about half for the predicted employability variables. It is comparable across program types while being slightly smaller in longer programs. Overall, it should be sufficient to estimate the effects of interest under this fixed effects strategy.

To provide evidence that the residual variation in my imputed employability variables results from random fluctuation, I ran a series of simulations (similar to Bifulco et al., 2011). In each simulation I randomly reallocate program participants to groups of the same size within the same provider-cohort and use this simulated distribution to compute the variation of the predicted peer employability measures. Across 500 simulations and after removing fixed effects and course controls the average residual standard deviation in the simulated peer employability measures ranges from 0.05 to 0.06 (Panel C). Whereas the simulated residual variation is very close to to observed one for retraining program it is slightly smaller for long training programs. Thus, there might be some excess variation in these types of programs. I plan to implement further tests to check whether peers somehow systematically sort into specific groups. Since the simulated residual variation is closer to the observed one for the lasso employability measure, I will base my main results on this measure.

The second issue which needs to be discussed is the treatment of unobserved peers. The data covers all job-seekers who are participating in publicly sponsored training measures and register at the local employment agencies. I 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. 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.[23] 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).

Table 2: Variation in peer employability

	Short training				Long training				Retraining			
	mean	sd	min	max	mean	sd	min	max	mean	sd	min	max
Panel A - Raw variation												
Mean employability (Lasso)	0.59	0.115	0.12	0.91	0.56	0.114	0.16	0.80	0.57	0.105	0.14	0.84
Mean employability (Logit)	0.60	0.119	0.11	0.91	0.57	0.120	0.14	0.82	0.59	0.110	0.13	0.86
Panel B - Variation net of fixed effects and course characteristics												
Mean employability (Logit)	0	0.065	-0.32	0.34	0	0.062	-0.35	0.28	0	0.060	-0.34	0.25
Mean employability (Lasso)	0	0.062	-0.31	0.30	0	0.058	-0.31	0.28	0	0.058	-0.32	0.25
Panel C - Variation in simulated peer groups net of fixed effects and course characteristics												
Residual variation (Logit)		0.052				0.054				0.058		
Residual variation (Lasso)		0.050				0.052				0.055		

Notes: The residual variation is net of provider-cohort FE, seasonality FE and course controls. The simulated residual variation averages over 500 simulations randomly reassigning participants within a provider-cohort to groups of the same size.

5 Results

This section presents and discusses the results of the analysis in four parts. First, I examine the effects of an average increase in the predicted peer employability on individual employment and other labour market outcomes after program start. Second, I investigate

[23]There are four groups that could self-select into vocational training programs without being registered. First, there could be individuals who are integrated in the labour 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 labour market will not be of great interest for those who already have a job. As for the non-employed individuals, I might not observe women returning to the labour 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.

whether there is effect heterogeneity with respect to own employability and gender. Third, I allow for non-linearities in the peer effects, consider a more flexible version of the main model using a cubic polynomial and test for effects related to the degree of homogeneity within a group by including the group's standard deviation of employability. Finally, I conduct a sensitivity analysis and estimate a linear-in-means model including a set of predetermined average peer characteristics proxying employability. All of the analyses are run separately by program type. Standard errors are clustered at the cohort-provider level.

My baseline results are based on the employability measure predicted by the lasso model. Results based on the logit model are very similar and can be found in the Appendix. The results are also robust to the employability measure that reweights the control population using inverse probability weighting.

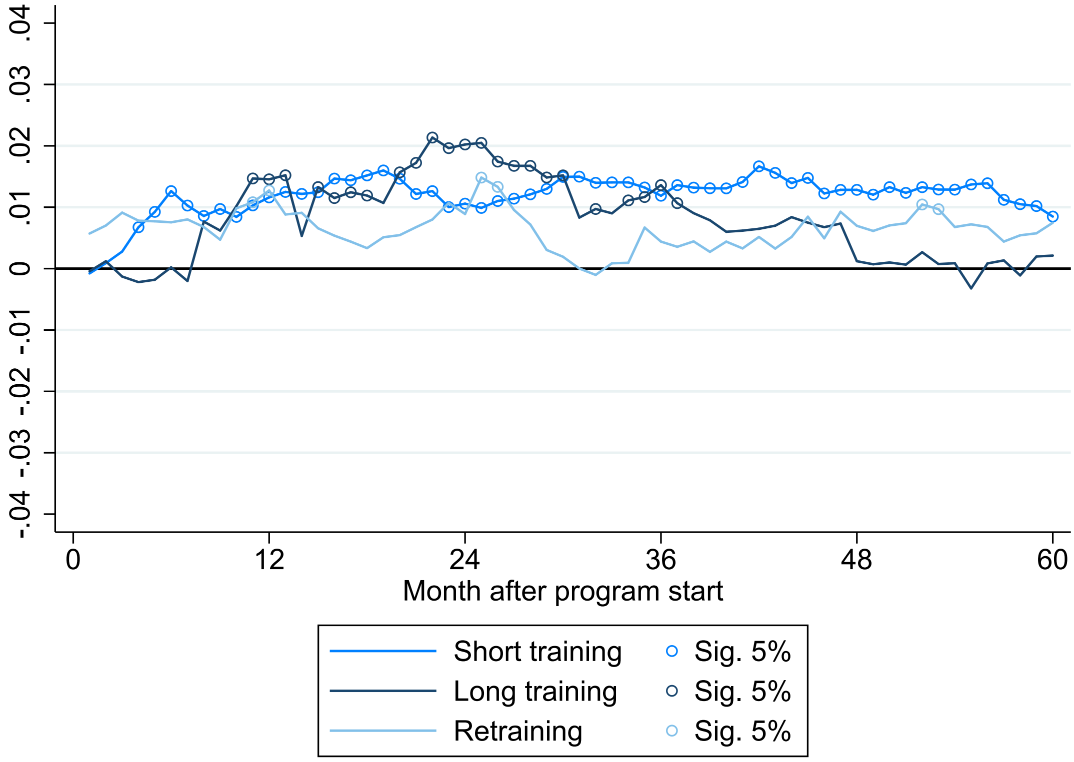
5.1 The Effects of Peer Employability on Individual Labour Market Outcomes

5.1.1 Effects on Employment

I first estimate monthly effects of an increase in the average predicted peer employability on individual employment (and unemployment) up to 60 months after program start. Figure 3 shows the results by program type based on equation (2). It depicts the effect estimates (in percentage points) of a one standard deviation increase[24] in the predicted mean peer employability for the months 1-60 after program start. Empty circles indicate that the effect is significant at the 5 percent level. I find significant effects of a greater exposure to highly employable peers on the individual employment probability for all program types. One year after program start, an increase of one standard deviation in the average group employability increases the individual employment probability by up to 1.5 percentage points. The effects are particularly persistent for short training. They increase in the first year after program start and stay at a rather constant level of 1 to

[24] To get a realistic effect size, I calculate the effect of a standard deviation increase multiplying the marginal effect with the residual standard deviation of the respective peer variable. Whenever the effect of a standard deviation is considered, it is the residual standard deviation net of fixed effects and course controls and can be found in Panel B of Table 4.1.

Figure 3: Monthly effects of peer employability (lasso) on individual employment



Notes: The figure depicts the estimated effects (in percentage points) of a one standard deviation increase in the predicted mean peer employability (lasso model) on the individual employment probability 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 (course size, average planned duration, weekly hours, number of hours in practice and class, total course costs, target occupation) and provider-by-cohort and seasonal fixed effects. Standard errors are clustered at the provider-by-cohort level.

1.2 percentage points up to 60 months after program start. For long training, the effects materialize some months later, reach a peak of about 2.2 percentage points in month 22 and eventually fade out. The effects turn mostly insignificant 3 years after program start. I find significant effects for retraining only in single months around one, two and four years after program start but no consistent pattern.

As for individual unemployment (See Appendix Figure A.6), I find largely reversed time trends. Overall, the results suggest that there are persistent medium and long-run negative effects of the exposure to employable peers on registered unemployment for short and long training programs. The effects are again mostly not statistically significant for individuals in retraining.

Table 3: Effects of peer employability (lasso) on other LM outcomes

	Short training	Long training	Retraining
	(1)	(2)	(3)
Panel A - Cum. employment in month 60			
Mean peer employability	343.24***	246.38*	198.15
	(59.71)	(102.88)	(110.85)
	[21.2]	[14.29]	[11.49]
Panel B - Log cum. earnings 60 months after prg. start			
Mean peer employability	1.60***	1.15*	0.15
	(0.32)	(0.58)	(0.56)
	[0.10]	[0.07]	[0.01]
Panel C - Log earnings first job			
Mean peer employability	0.77***	0.40	-0.04
	(0.15)	(0.24)	(0.29)
	[0.04]	[0.02]	[-0.002]
Panel D - Search duration first job (in days)			
Mean peer employability	-227.30**	-232.46	-60.56
	(78.98)	(147.17)	(143.14)
	[-14.09]	[-13.48]	[-3.51]
Provider-Cohort FEs	✓	✓	✓
Seasonal FEs	✓	✓	✓
Additional Controls	✓	✓	✓
Observations	28251	9614	8663

Notes: All specifications control for course-level controls, individual employability and unemployment duration at program start. Standard errors (in round brackets) are clustered at the provider-cohort level. Effects in terms of SD increases (6.2; 5.8; 5.8 p.p.) are displayed in square brackets. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

5.1.2 Effects on other Labour Market Outcomes

To further investigate how the individual labour market performance might be affected by the quality of peers in the program, I look at other short and long-term outcomes, such as cumulative employment and cumulative earnings (measured in logs) up to 60 months after program start and at aspects of the first job. Table 3 presents the results of this analysis again separately by program type.[25]

[25] Appendix Table A.2 reports the results for the same outcomes conditional on positive labour market participation by month 60 or by December 2016 (when looking at earnings in the first job). The effects are slightly smaller for these sub-samples. Overall, the trends are the same which is not surprising given that by month 60 I observe positive earnings for over 92 percent of the program participants. Interestingly, I also find positive effects of a higher peer group employability on cumulative employment for individuals in retraining.

My findings suggest that a standard deviation increase in peer employability cumulatively increases employment by around 14 to 21 days for participants of short and long vocational training (Panel A). The effect is strongest for short programs which could be expected from the results seen in Figure 3. Moreover, increasing peer employability by one standard deviation leads to a positive effect on cumulative earnings of around 10 percent for participants in short training and 7 percent for participants in long training (Panel B). This corresponds to an increase of around 1600 and 1000 EUR respectively. For short programs, I also find that individuals are faster in finding a job after program participation (by around 14 days for a standard deviation increase) (Panel D) and earn significantly more in this job than if they were exposed to a lower peer employability (Panel C). These positive short-term effects might explain why I find such persistent employment effects for participants in short training programs even several years after the training. During the program, individuals might benefit from the exposure to highly employable peers, achieve a better reintegration in the labour market which in turn might have long-lasting positive effects on their employment and earnings. I do not find statistically significant effects on the quality of the first job and earnings for individuals in long training or retraining. Nevertheless, the direction of the effects is the same.

5.2 Heterogeneity Analysis

Next, I examine whether individuals heterogeneously respond to the average employability in their group along two dimensions: own employability and gender. For this, I estimate the effects of peer employability on post program employment up to 60 months after program start using the main specification introduce above, separately by type of participant (see Appendix Figures A.8 and A.9). To categorize individuals' own employability, I define program participants with a predicted employability below the sample median as participants with low employability and participants with an employability greater or equal to the median as as participants with high employability. I do not find heterogeneity in effects for participants in short training. For long training, it is mostly individuals with low employability that benefit from a higher exposure to more employable peers. This trend seems to be partially reversed for retraining but the effects are not statistically significant different from zero. The heterogeneity analysis with respect to gender does not reveal significant differences in effects for males and females either. If anything, males seem to benefit slightly more from better peer groups than females. The largest heterogeneity exists for individuals in retraining programs, where females even experience negative effects. Nevertheless, these differences are not statistically significant since confidence intervals are large for this small sample.

5.3 Looking Beyond the Average

So far the analysis has focused on how the average outcome of a randomly choosen program participant would change in expectation if the average peer employability was marginally increased. As pointed out e.g. by Graham et al. (2010) this does not measure the effect of an implementable policy for a fixed population of participants.[26] In order to get an idea of how to achieve a peer composition that mitigates potentially negative spillovers, we need to look beyond average peer effects. In the following I abandon the linearity assumption and estimate two versions of the main model (2) that are more flexible. First, I include the (leave-one-out) standard deviation of peer employability on top of the average peer employability. Second, I estimate a model including a cubic polynomial of $\bar{X}_{(-i)pct}$. All control variables and fixed effects remain unchanged.

The first model variation, including the standard deviation of employability, tests whether a larger dispersion of peer ability is beneficial when holding the average peer employability constant. The results of this augmented model are shown in Table 4 for the outcomes cumulative employment and cumulative log earnings in month 60 after program start. Including this variable does not change the coefficients of the average peer employability by much. Moreover, the standard deviation terms are insignificant in all specifications with one exception. Participants in long training experience slight increases in cumulative earnings from having a greater dispersion of employability in the course. Overall, these findings suggest that the group homogeneity is of less importance for employment outcomes of program participants compared to the average quality of the group.

Nevertheless, these results do not rule out the possibility of other non-linear peer effects.

[26] A marginal increase in the proportion of highly employable individuals across all groups would be infeasible since a higher share of high-type individuals in some of the courses needs to be offset by a lower share in other courses.

Table 4: Group Heterogeneity

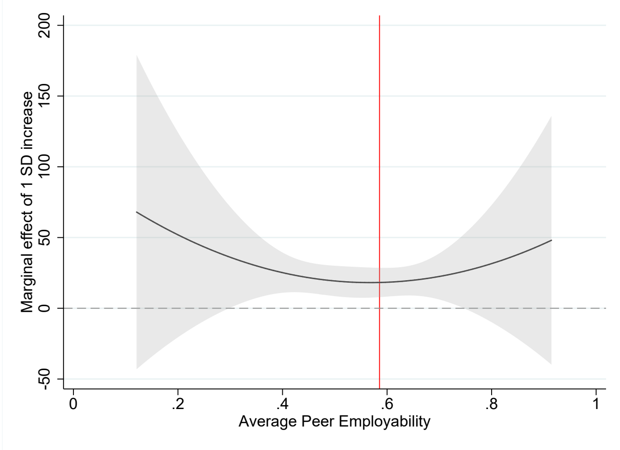
	Short training	Long training	Retraining
	(1)	(2)	(3)
Panel A - Cum. employment in month 60			
Mean peer employability	337.542***	274.974*	120.464
	(66.154)	(110.054)	(116.757)
SD peer employability	-22.743	120.962	-307.643
	(115.239)	(203.304)	(208.222)
Panel B - Log cum. earnings in month 60			
Mean peer employability	1.564***	1.621*	0.140
	(0.367)	(0.637)	(0.665)
SD peer employability	-0.143	1.987*	-0.032
	(0.626)	(0.981)	(1.076)
Provider-cohort FEs	✓	✓	✓
Seasonal FEs	✓	✓	✓
Additional controls	✓	✓	✓
Observations	28250	9614	8663

Notes: All specifications control for course-level controls, individual employability and unemployment duration at program start. Standard errors (in round brackets) are clustered at the provider-cohort level.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

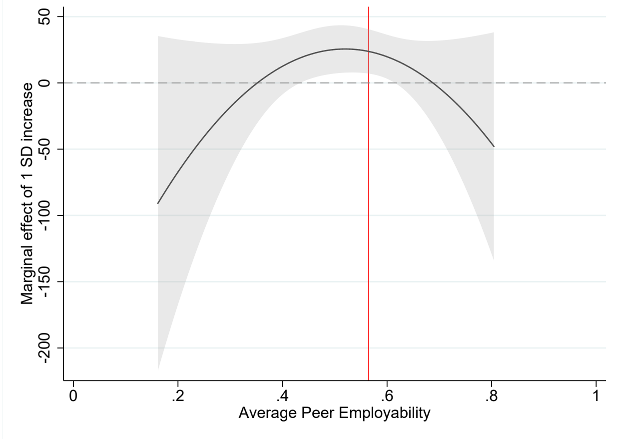
As a next step, I estimate a variation of the original specification which includes a cubic polynomial of the average peer employability. Figure 4 depicts the marginal effects and the corresponding 95% confidence intervals of a one standard deviation increase in peer employability on cumulative employment 60 months after program start over the support of the peer employability distribution, separately by program type. The red line corresponds to the observed average of the mean peer employability. As can be seen from the size of the confidence intervals, the effects are only precisely estimated in the closer neighbourhood of this average. For an average group employability ranging from 0.4 to 0.7, I find positive marginal effects on cumulative employment for all program types. Nevertheless, the shapes of the curves are quite different for the respective program types. Whereas I find rather decreasing and slightly convex effects from an increase in the average group employability on individual employment for individuals in short training programs, the marginal effects are concave for individuals in long training and retraining. The marginal effects on log cumulative earnings are depicted in the Appendix Figure A.10 and follow very similar patterns.

This has different implications for potential regrouping policies. In short training pro-

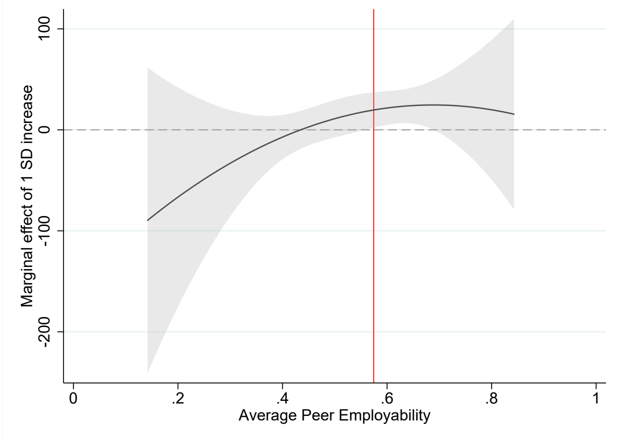
Figure 4: Marginal effects of a one standard deviation increase on cumulative employment 60 months after program start (in days)



(a) Short Training



(b) Long Training



(c) Retraining

Notes: The figures depict marginal effects of a SD increase in the average peer employability on cum. days in employment 60 months after program start, estimated by a cubic polynomial. 95% confidence intervals are depicted in grey. The red line indicates the average peer employability level in the sample. The underlying model controls for the individual ex-ante employability, unemployment at program start, a vector of course-level controls (course size, average planned duration, weekly hours, number of hours in practice and class, total course costs, target occupation) and provider-by-cohort and seasonal FE. Standard errors are clustered at the provider-by-cohort level.

grams, participants in the lower end of the peer employability distribution experience slightly higher gains from being exposed to more employable peers than participants at the upper end. A policy that would foster an allocation where participants of different employability levels are evenly distributed across groups would increase average employment in these programs. Deriving implications for sensible regrouping policies for long training and retraining programs is less straight forward, given the shape of the marginal effect curves. Moreover, due to the imprecise nature of the marginal effects in parts of the peer employability distribution, it should be noted that the results are only indicative of an optimal allocation but not sufficient to derive an optimal reallocation policy.

5.4 Potential Drivers of Peer Effects in Employability

In the following, I investigate which predetermined peer characteristics might drive the effects of peer employability on individual employment and earnings. For this, instead of the predicted peer employability measure, I include a set of readily available individual and peer characteristics in model (2) which should contain information on the employability of the program participants. Specifically, I include average peer age, the fraction of female and non-German peers, the fraction of peers with a high-school degree (Abitur), the fraction of peers with a vocational or university degree, the fraction of peers with a complex or highly complex past occupation, the average unemployment duration directly precedent to the course, the average cumulative time in employment or out of the labour force in the past 2 and 10 years respectively, the average cumulative earnings in the past 2 and 10 years respectively, as well as the average number of labour market programs attended in the past 2 years. In addition, I control for provider-cohort and seasonal fixed effects as well as individual and course-level characteristics[27].

Table 5 reports the estimated coefficients of this multivariate peer effects model separately by program type (short training (ST), long training (LT) and retraining (RT)). I estimate the effects on cumulative employment after 60 months (columns 1-3) and log cumula-

[27]This includes course size, the average planned course duration, weekly hours of course, total hours spent in practice and class, total course costs and target occupation. All characteristics included at the group level are also included at the individual level. Here, the variables are measured at program start. Information on education and vocational training degrees is measured at the point where the job-seekers register with the unemployment agency

tive earnings measured 60 months after program start (columns 4-6). With respect to post-program employment, individuals benefit from a lower average peer unemployment duration at program start and higher average peer employment before program start if they are in short training programs. For participants in long training, I find that an increase in average past peer employment (ten years before program start) and the average number of past labour market programs attended by peers positively affects individual cumulative employment. While the latter result might seem counter-intuitive at first, program participants might indeed benefit from peers who participated in labour market programs before. Through this experience they might have updated their skills and employability[28]. For individuals in retraining, only peer age and education seems to matter. Being exposed to an older peer group is associated with a higher cumulative employment, a higher fraction of peers with a high school degree as opposed to less educated peers has a significant negative effect on cumulative employment. With respect to earnings outcomes, the peer variables considered are mostly insignificant. For short training, I find that peer age is negatively associated with earnings. For longer vocational training, peer education and peer employment in the 10 years previous to program start seem to play a role.

Overall, individuals seem to benefit from peers with successful labour market histories. For all program types, I find that an increase in average cumulative peer earnings from two years before the program start is associated with an increase in individual post-program employment and earnings while an increase in average peer earnings accumulated 10 years before program start is negatively associated with the same outcome. This points at non-linearities in the effect of peer earnings. Nevertheless, this observation should be treated with caution since the effects are only statistically significant for participants in short training when looking at cumulative employment. Generally, I find insignificant effects for many of the peer variables and and the results are rather inconclusive. For the interpretation of the effects of this model, it should be kept in mind that we look at *ceteris paribus* increases in the exposure to specific average peer characteristics. Potentially, some of these characteristics might only matter in combination and are highly correlated. As

[28]In fact I also find own program participation to be positively associated with better employment outcomes (although not statistically significantly so).

Table 5: Effects of average peer characteristics on individual employment after program start

	Cum. empl. in month 60			Log cum. earnings in month 60		
	ST	LT	RT	ST	LT	RT
	(1)	(2)	(3)	(4)	(5)	(6)
Peer age	-0.781	0.914	6.277*	-0.012*	0.002	0.024
	(1.202)	(2.050)	(2.980)	(0.006)	(0.015)	(0.015)
Female peers	-43.205	13.257	55.100	-0.243	0.024	0.120
	(22.170)	(40.556)	(38.314)	(0.126)	(0.229)	(0.217)
Non-German peers	21.026	77.742	-42.133	0.034	0.056	0.025
	(36.908)	(63.262)	(55.341)	(0.192)	(0.381)	(0.322)
Peers with high-school degree	35.023	70.334	-134.542*	-0.154	0.862*	-0.281
	(37.087)	(63.777)	(64.146)	(0.216)	(0.381)	(0.360)
Peers with voc./ac. degree	-20.298	21.019	163.999	0.072	0.083	0.518
	(53.235)	(80.099)	(139.830)	(0.311)	(0.494)	(0.938)
High-skilled peers	22.792	30.124	57.996	0.056	0.122	0.235
	(28.208)	(43.640)	(43.192)	(0.154)	(0.274)	(0.233)
Months of UE at prg. start	-1.550*	-1.290	-0.073	-0.007	-0.010	-0.007
	(0.738)	(1.025)	(1.040)	(0.004)	(0.006)	(0.006)
Months of EMPL in last 2 yrs	6.398*	-1.796	1.422	0.024	-0.041	-0.040
	(2.737)	(4.544)	(4.676)	(0.014)	(0.027)	(0.025)
Months OLF in last 2 yrs	3.068	3.367	-7.341	0.021	0.023	-0.003
	(3.898)	(6.209)	(5.857)	(0.021)	(0.036)	(0.036)
Months of EMPL in last 10 yrs	0.233	2.791**	0.796	0.002	0.017*	0.003
	(0.722)	(1.065)	(1.108)	(0.004)	(0.007)	(0.006)
Months OLF in last 10 yrs	0.289	-0.196	-0.130	-0.002	0.006	-0.005
	(1.039)	(1.698)	(1.510)	(0.006)	(0.010)	(0.009)
Cum. earn. in last 2 yrs (1000 EUR)	2.218	1.220	3.666	0.012	0.013	0.023
	(1.236)	(2.161)	(2.398)	(0.006)	(0.013)	(0.013)
Cum. earn. in last 10 yrs (1000 EUR)	-0.553*	-0.615	-0.632	-0.002	-0.005	-0.002
	(0.272)	(0.442)	(0.485)	(0.002)	(0.003)	(0.003)
Number of ALMP in last 2 yrs	21.067	40.512*	-10.407	0.104	0.201	-0.032
	(11.834)	(19.937)	(16.975)	(0.063)	(0.115)	(0.102)
Provider-Cohort FEs	✓	✓	✓	✓	✓	✓
Seasonal FEs	✓	✓	✓	✓	✓	✓
Additional controls	✓	✓	✓	✓	✓	✓
Joint sign. of peer variables	0.000	0.031	0.013	0.000	0.074	0.293
Observations	28249	9614	8663	28249	9614	8663

Notes: All regression include provider-cohort, seasonal fixed effects, course-level controls (course size, avg. pl. duration, weekly hours, number of hours in practice and class, total course costs, target occupation) and a vector of individual-level controls. On the individual level I include the same information as on the group level. Abbreviations: Fr. fraction, M. mean, voc. vocational, ac. academic, UE unemployment, EMPL employment, OLF out of the labour force, cum. cumulative, earn. earnings, yrs years, ST short training, LT long training, RT retraining. Cum. employment is measured in days Standard errors are clustered at the provider-cohort level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

a matter of fact they are jointly significant in all specifications with one exception being the subsample of participants in retraining.

5.5 Potential Mechanisms

There might be different mechanisms driving the positive effects of peer employability on individual employment outcomes. While formally testing for these mechanisms is difficult given the administrative data at hand, this section provides a discussion of three possible channels. A first mechanism is information transmission. On the one hand, program participants might benefit from the knowledge of more employable peers, e.g. learn how to effectively write job applications. One the other hand, more employable peers might be better connected and have valuable information networks that they might share. The importance of informal job search networks has been well documented in the literature. When looking for jobs, individuals benefit from their friends, family or the neighbourhood and find more stable employment with higher wages. (See e.g. Ioannides and Loury, 2004; Bayer et al., 2008; Pellizzari, 2010; Brown et al., 2016; Dustmann et al., 2016).

A second potential channel are motivational effects. More employable program participants might be relatively more successful in their job search compared to their less employable peers. The latter might observe the motivation and success of the former and gain motivation with respect to their own job search. This channel is supported by evidence from the literature on effort provision which shows that individuals get motivated by the presence of good peers (Eisenkopf, 2010) and peers with whom they have strong social ties (Bandiera et al., 2010).

Finally, a third potential mechanism explaining peer effects in employability is the willingness to comply with peer behaviour and the social norm to work. It has been shown that individuals like to act in conformity with their peers and social norms. If they deviate from this norm, they experience losses in utility (see e.g. Bernheim, 1994; Akerlof and Kranton, 2000 for a theoretical foundation of this argument). This is particularly true for unemployed individuals whose wellbeing has been shown to be highly dependent of the unemployment status of their reference group (Clark, 2003; Stutzer and Lalive, 2004; Hetschko et al., 2014). Moreover, peer pressure and norm compliance have been identified as important drivers behind peer effects in labour supply at the extensive (Kondo and Shoji, 2019; Schneider, 2019) and intensive margin (Mas and Moretti, 2009; Falk and Ichino, 2006). This mechanism might also apply in the context of training programs. Program participants might exert more effort in their job search if they see more employable peers entering into employment.

6 Conclusion

This paper investigates how labour market outcomes of participants in public sponsored training depend on the exposure to different group compositions with a focus on the employability of the peers. Using a number of predetermined characteristics, I construct a summary measure to proxy for employability, which is defined as the predicted probability to find a stable occupation within one year after entering unemployment. To identify a causal effect, I exploit the quasi-random variation in the average peer employability across courses offered by the same training providers over time. Overall, I find that program participants who are exposed to a more employable peer group experience positive and persistent effects on post-program employment and earnings. I find positive effects on employment for all three types of training programs around one year after program start but the effects are not very robust for individuals in retraining. The effects persist up to five years for participants of short training programs. They fade out earlier for participants in long training, leading to a cumulative effect on employment of 14 to 21 days, five years after program start. At the same time, individuals in these programs experience substantial effects on cumulative earnings. While there is substantial heterogeneity in effects across program types, I find no heterogeneity in effects with respect to own employability and gender.

It is not surprising that peer effects differ in size and shape across program types, given the heterogeneous content, organization and objective of the courses. Short and long vocational training programs as well as retraining programs target different subpopulations which might respond differently to their course environment. Individuals in short training have more labour market attachment compared individuals in longer training and might directly benefit from existing networks of their peers. In contrast, individuals in retraining often face a serious skill mismatch and are trained in completely new areas. Here, more employable peers are less likely to have a comparative advantage in starting a new job compared to their less employable peers since everyone faces a new job search environment. Moreover, the educational content and human capital accumulation might be of a relatively higher importance compared to the peer environment. The persistence in effects, which I find in particular for short training programs might be explained by the fact that participants in these programs experience a more rapid and successful integration into employment after being exposed to more employable peers. These short run effects on the job search duration and earnings might allow for more successful careers also in the longer run.

Finally, I find evidence for some non-linearity in the employability peer effects. Whereas the homogeneity in a group as measured by the standard deviation in peer employability does not seem to matter for most individual post-program employment outcomes, program participants experience different benefits or losses depending on how high the average peer employability is in their course. The shapes of the marginal effects are again heterogeneous across program types. Policy recommendations regarding the regrouping of individuals with respect to their employability should take these differences into account.

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Appendix

Table A.1: Summary statistics for predictors of employability

	Control Population		Prg. Participants	
	mean	sd	mean	sd
Entry in unemployment in				
1997	0.00	0.003	0.00	0.044
1998	0.00	0.007	0.00	0.051
1999	0.00	0.040	0.00	0.058
2000	0.09	0.289	0.01	0.075
2001	0.10	0.297	0.01	0.093
2002	0.11	0.309	0.01	0.110
2003	0.10	0.301	0.01	0.116
2004	0.10	0.295	0.02	0.139
2005	0.09	0.288	0.03	0.166
2006	0.07	0.254	0.06	0.229
2007	0.06	0.242	0.15	0.355
2008	0.06	0.241	0.22	0.411
2009	0.07	0.260	0.26	0.441
2010	0.07	0.263	0.15	0.357
2011	0.07	0.258	0.07	0.255
Jan	0.17	0.379	0.13	0.332
Feb	0.08	0.266	0.08	0.277
Mar	0.06	0.243	0.09	0.280
Apr	0.06	0.238	0.08	0.278
May	0.05	0.217	0.07	0.254
Jun	0.05	0.224	0.07	0.255
Jul	0.07	0.254	0.08	0.277
Aug	0.06	0.232	0.08	0.271
Sep	0.06	0.229	0.09	0.287
Oct	0.07	0.253	0.09	0.283
Nov	0.10	0.296	0.07	0.256
Dec	0.18	0.380	0.07	0.252
Demographic Characteristics				
Age in years	37.95	11.330	36.19	10.273
Age squared	1568.53	887.066	1415.17	771.195
Age cubic	69471.99	55984.232	59064.26	46308.070
Female	0.31	0.463	0.44	0.496
Non-German	0.09	0.289	0.10	0.303
Single	0.44	0.497	0.47	0.499
Married	0.46	0.498	0.37	0.482
Family status miss.	0.10	0.303	0.16	0.368
Single parent	0.03	0.177	0.07	0.254
Single parent miss.	0.00	0.045	0.01	0.075
Child under 15 yrs	0.21	0.410	0.22	0.415

Child under 3 yrs	0.13	0.341	0.15	0.357
Restrictions bad health conditions	0.07	0.254	0.10	0.301
Restrictions bad health conditions miss.	0.01	0.074	0.02	0.146
Disabled	0.02	0.142	0.02	0.153
Disability miss.	0.00	0.026	0.00	0.037
No schooling degree	0.08	0.264	0.07	0.250
Schooling degree without Abitur	0.80	0.399	0.73	0.444
Schooling degree with Abitur	0.11	0.310	0.17	0.372
Schooling degree miss.	0.02	0.123	0.04	0.190
No vocational training	0.22	0.411	0.27	0.443
Vocational training	0.72	0.451	0.61	0.487
Academic degree	0.04	0.204	0.07	0.257
Vocational degree missing	0.03	0.159	0.05	0.214
Place of residence (state)				
Schleswig-Holstein	0.04	0.186	0.04	0.204
Freie und Hansestadt Hamburg	0.02	0.122	0.04	0.189
Niedersachsen	0.09	0.293	0.09	0.292
Freie Hansestadt Bremen	0.01	0.078	0.02	0.130
Nordrhein-Westfalen	0.14	0.352	0.23	0.422
Hessen	0.05	0.219	0.05	0.212
Rheinland-Pfalz	0.04	0.192	0.04	0.191
Baden-Wuerttemberg	0.08	0.270	0.11	0.307
Freistaat Bayern	0.20	0.397	0.16	0.362
Saarland	0.01	0.088	0.02	0.141
Berlin	0.04	0.188	0.02	0.126
Brandenburg	0.05	0.224	0.03	0.166
Mecklenburg-Vorpommern	0.05	0.207	0.04	0.204
Freistaat Sachsen	0.09	0.286	0.06	0.241
Sachsen-Anhalt	0.05	0.223	0.02	0.140
Freistaat Thüringen	0.05	0.227	0.04	0.197
Bundesland miss.	0.00	0.009	0.00	0.014
Job Search				
Searching part-time	0.05	0.216	0.06	0.228
Searching full-time	0.89	0.316	0.74	0.441
Searching status missing	0.06	0.243	0.21	0.406
State before unemployment				
Employment	0.86	0.352	0.65	0.477
Self-Employment	0.01	0.100	0.01	0.094
Other employment	0.03	0.174	0.03	0.170
OLF	0.01	0.117	0.05	0.217
Education/Apprenticeship	0.05	0.215	0.06	0.246
ALMP	0.00	0.020	0.04	0.199
Disability	0.03	0.183	0.03	0.156
Other services/programmes	0.00	0.039	0.01	0.088
Spell splitting	0.00	0.036	0.02	0.131
LM status before inflow missing	0.00	0.053	0.11	0.308

Job returner	0.01	0.088	0.03	0.162
Receiving top up welfare benefits	0.01	0.081	0.02	0.122
No previous employment	0.00	0.052	0.01	0.119
Unskilled/semiskilled tasks	0.08	0.266	0.09	0.291
Skilled tasks	0.77	0.418	0.66	0.473
Complex tasks	0.03	0.177	0.04	0.195
Highly complex tasks	0.05	0.220	0.07	0.253
Skill level last job missing	0.07	0.247	0.14	0.344
Full-time	0.87	0.339	0.67	0.469
Part-time	0.13	0.334	0.31	0.463
Working time last job missing	0.00	0.066	0.02	0.128
Military	0.00	0.010	0.00	0.017
Agriculture, forestry, farming, and gardening	0.06	0.233	0.02	0.128
Prod. of raw materials and goods, manufacturing	0.20	0.401	0.23	0.418
Construction, architect., building services etc.	0.25	0.432	0.06	0.242
Natural sciences, geography and informatics	0.01	0.101	0.02	0.139
Traffic, logistics, safety and security	0.16	0.369	0.18	0.385
Comm. services, trading, sales and tourism	0.11	0.316	0.12	0.326
Business organisation, accounting, law and admin.	0.07	0.255	0.12	0.330
Health care, the social sector, teaching and educ.	0.07	0.248	0.09	0.279
Philology, literature, soc. sciences, culture etc.	0.03	0.174	0.03	0.173
Occupation miss.	0.04	0.194	0.13	0.340
Information on previous LM staus/employment				
Log wage in previus occupation	3.88	0.905	3.32	1.224
Log duration between last job and UE start (days)	1.89	1.988	2.17	2.004
Log cum. earnings 12 months before UE entry	9.00	2.455	8.46	2.590
Log cum. benefits 12 months before UE entry	3.95	3.848	1.86	3.281
Log cum. earnings 24 months before UE entry	9.95	1.946	9.32	2.317
Log cum. benefits 24 months before UE entry	5.18	3.911	2.67	3.743
Log cum. earnings 48 months before UE entry	10.79	1.384	10.08	2.086
Log cum. benefits 48 months before UE entry	6.39	3.746	3.92	4.140
Log cum. earnings 60 months before UE entry	11.02	1.265	10.31	2.032
Log cum. benefits 60 months before UE entry	6.74	3.670	4.43	4.208
Log cum. earnings 120 months before UE entry	11.63	1.105	10.99	1.935
Log cum. benefits 120 months before UE entry	7.59	3.446	5.91	4.126
Employed 6 months prior to UE entry	0.81	0.396	0.74	0.439
In ALMP 6 months prior to UE entry	0.07	0.254	0.09	0.286
OLF 6 months prior to UE entry	0.05	0.224	0.03	0.174
Employed 12 months prior to UE entry	0.69	0.462	0.73	0.443
In ALMP 12 months prior to UE entry	0.07	0.251	0.09	0.284
OLF 12 months prior to UE entry	0.06	0.231	0.04	0.204
Employed 24 months prior to UE entry	0.68	0.468	0.68	0.467
In ALMP 24 months prior to UE entry	0.06	0.234	0.09	0.288
OLF 24 months prior to UE entry	0.06	0.239	0.06	0.237
Employed 48 months prior to UE entry	0.63	0.484	0.61	0.488
In ALMP 48 months prior to UE entry	0.05	0.209	0.09	0.289

OLF 48 months prior to UE entry	0.07	0.248	0.08	0.268
Cum. EMPL 12 months before UE entry (days)	273.70	106.465	261.46	124.430
Cum. EMPL 24 months before UE entry (days)	545.61	182.703	508.87	237.049
Cum. EMPL 60 months before UE entry (days)	1304.99	403.672	1168.10	555.080
Cum. EMPL 120 months before UE entry (days)	2367.20	865.224	2106.77	1053.028
Cum. UE 12 months before UE entry (days)	66.81	83.633	77.84	115.365
Cum. UE 24 months before UE entry (days)	140.31	143.980	156.75	205.903
Cum. UE 60 months before UE entry (days)	355.90	318.710	417.48	462.576
Cum. UE 120 months before UE entry (days)	618.17	571.004	737.77	803.391
Cum. ALMP 12 months before UE entry (days)	23.39	73.809	31.11	75.535
Cum. ALMP 24 months before UE entry (days)	44.46	121.682	62.52	129.303
Cum. ALMP 60 months before UE entry (days)	96.39	215.689	158.01	258.082
Cum. ALMP 120 months before UE entry (days)	141.26	283.628	272.19	387.185
Number of ALMP 24 months before UE entry	0.32	0.651	0.62	0.958
Number of ALMP 60 months before UE entry	0.62	1.043	1.31	1.641
Number of ALMP 120 months before UE entry	0.84	1.365	1.92	2.219
Regional Characteristics				
Unemployment rate	9.08	3.816	8.57	3.503
Population density	652.40	850.159	777.14	875.014
Employment share in first sector	2.50	2.020	1.86	1.827
Employment share in second sector	27.82	8.163	26.17	8.986
Employment share in third sector	69.68	9.064	71.97	9.781
GDP per capita	27.57	11.383	30.68	12.306
Regional information missing	0.17	0.377	0.08	0.276
Observations	1500000		47279	

Notes: This table summarizes the variables included in the model for the prediction of the individual ex-ante employability for the control population (representative sample of individuals entering unemployment between 1997 and 2001) and the sample of program participants.

Table A.2: Effects of peer employability (lasso) on other LM outcomes

	Short training	Long training	Retraining
	(1)	(2)	(3)
Panel A - Cum. employment in month 60			
Mean peer employability	311.71***	227.62*	224.14*
	(55.52)	(100.62)	(106.86)
	[19.32]	[13.20]	[13]
N	*25925*	*8812*	*8086*
Panel B - Log cum. earnings 60 months after prg. start (if > 0)			
Mean peer employability	1.03***	0.93***	0.42
	(0.14)	(0.26)	(0.23)
	[0.06]	[0.05]	[0.02]
N	*25911*	*8806*	*8083*
Panel C - Log earnings first job (if > 0)			
Mean peer employability	0.65***	0.58**	0.03
	(0.12)	(0.22)	(0.26)
	[0.04]	[0.03]	[0.001]
N	*26135*	*8930*	*8209*
Panel D - Search duration first job (in days)			
Mean peer employability	-287.63***	-41.89	-124.20
	(82.12)	(130.41)	(128.62)
	[-17.83]	[-2.38]	[7.20]
N	*28251*	*9614*	*8663*

Notes: All specifications control for cohort-provider, seasonal FE, course-level controls, ind. employability and unemployment duration at program start. The sub-samples in Panels A and B condition on positive labour market participation up to month 60, positive labour market participation up December 2016 in Panel C. The search duration in Panel D is censored at the maximum duration at the end of the observation period. Standard errors (in round brackets) are clustered at the provider-cohort level. Effects in terms of SD increases are displayed in square brackets.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure A.1: Average unemployment rate for different program types

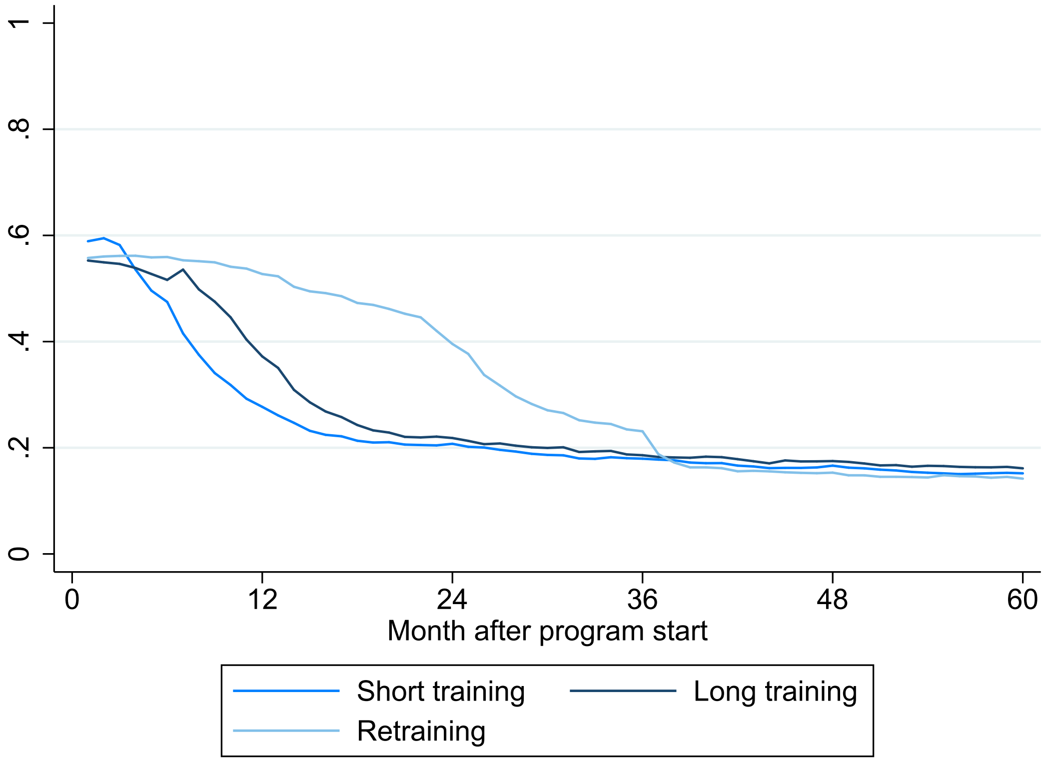


Figure A.2: Distribution of individuals over different course sizes

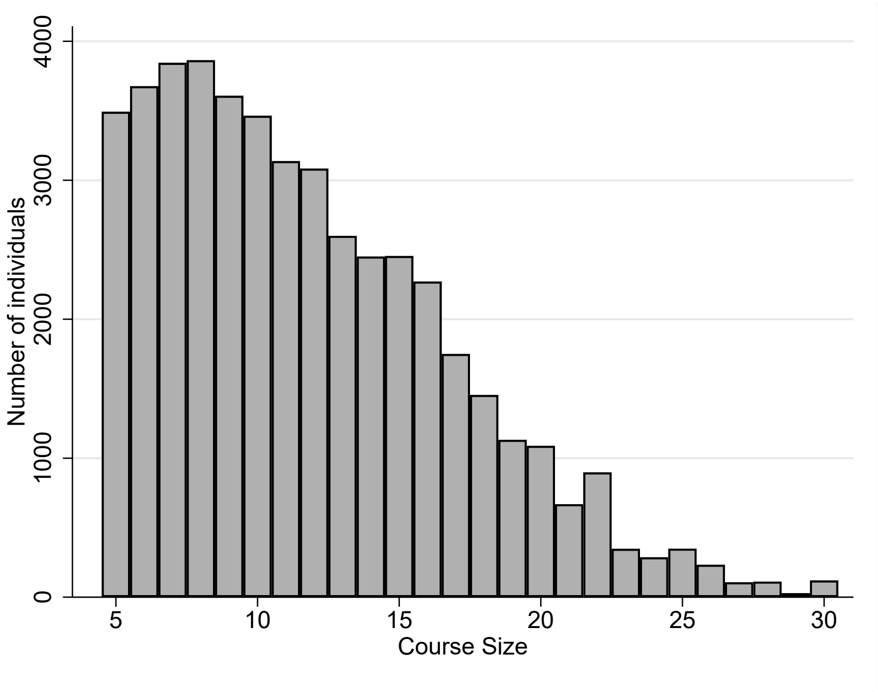


Figure A.3: Individual predicted employability

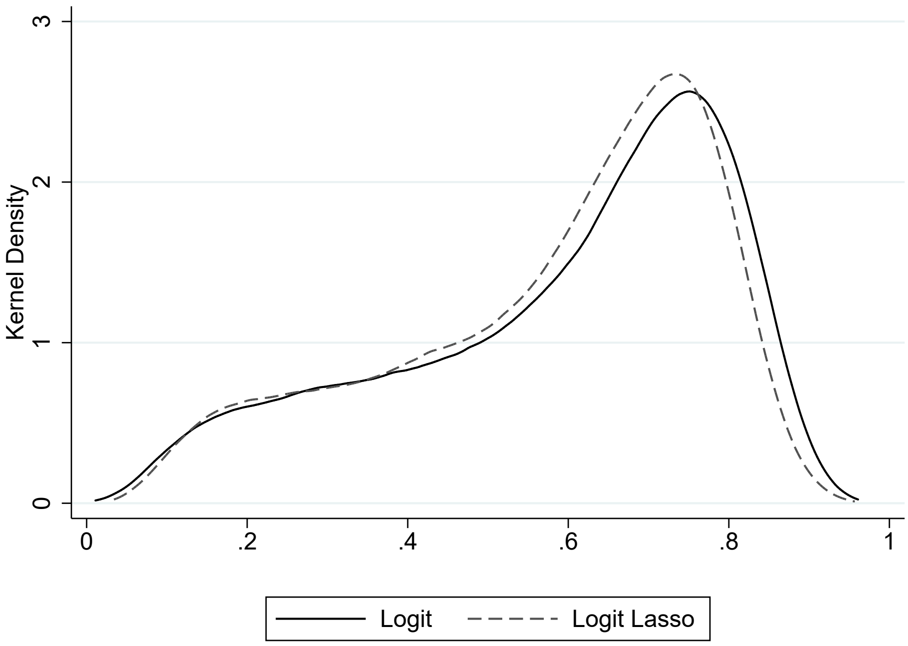


Figure A.4: Distribution of the individual predicted employability

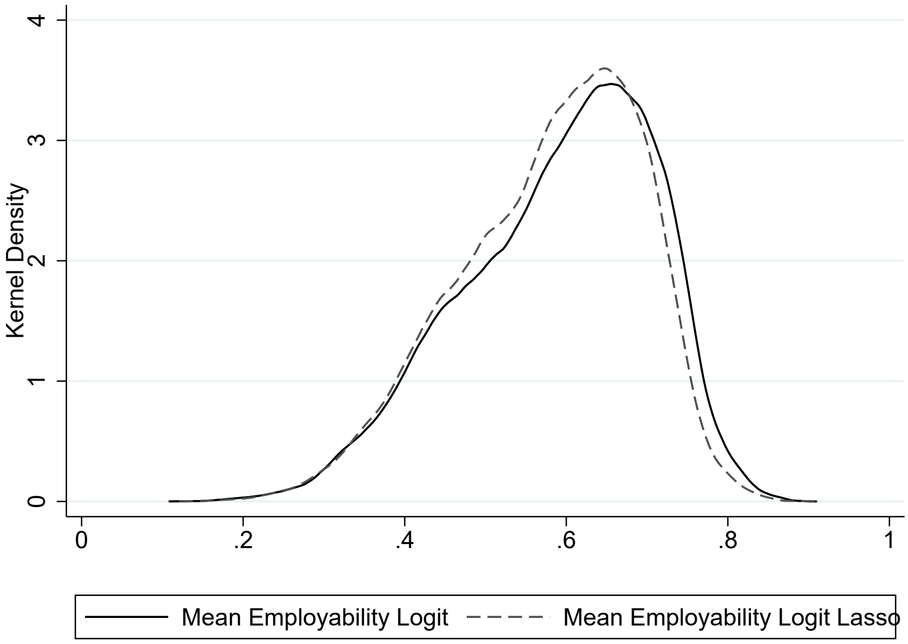


Figure A.5: Distribution of the weighted and unweighted predicted individual employability measures

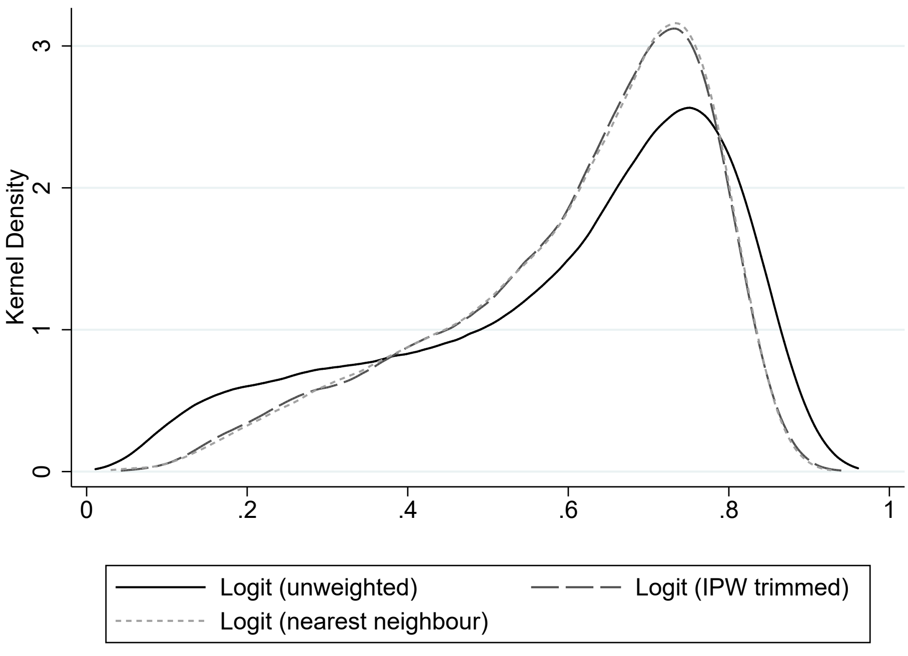
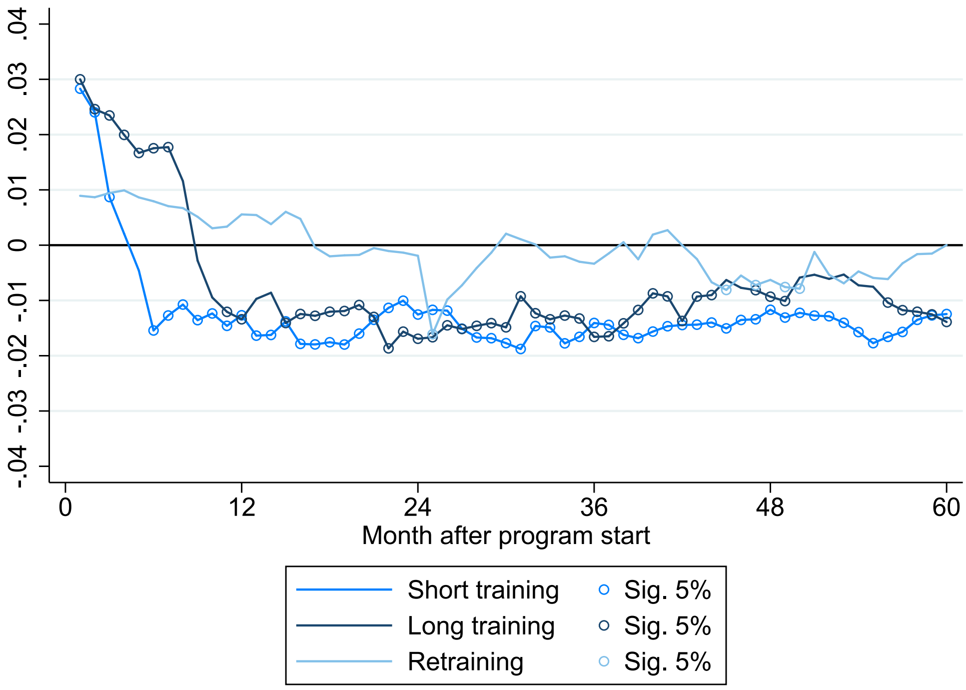
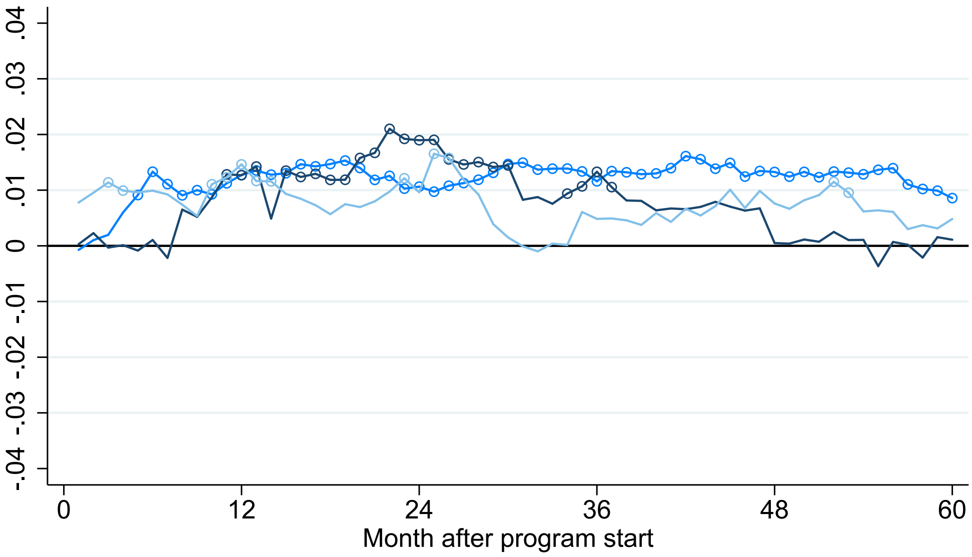


Figure A.6: Monthly effects of peer employability (lasso) on individual unemployment (including program participation)

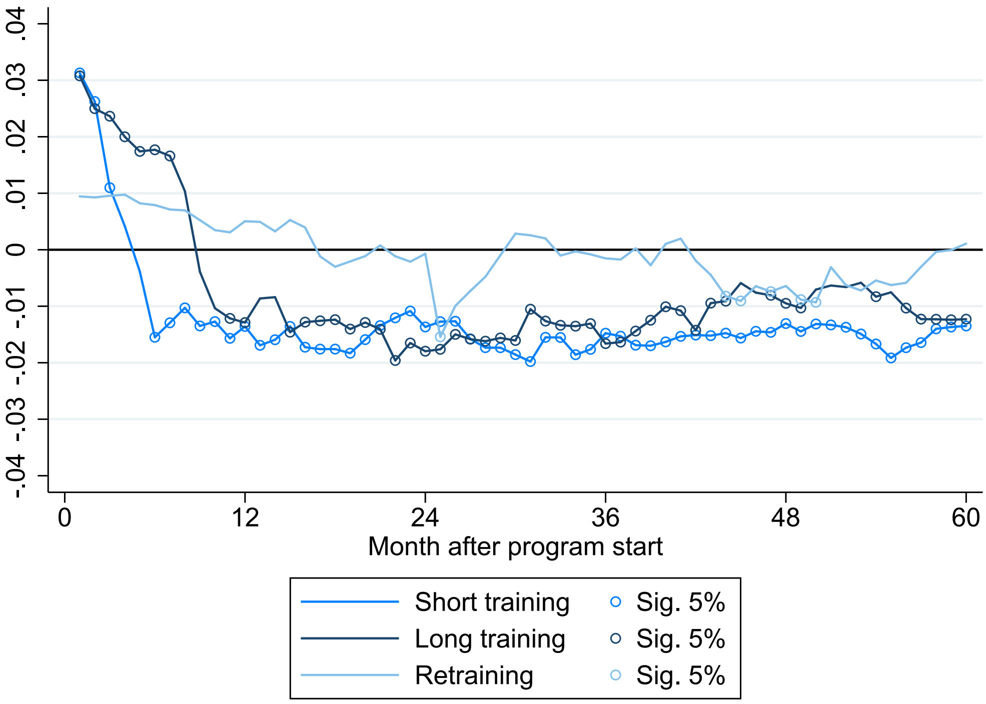


Notes: The figure depicts the estimated effects (in percentage points) of a one standard deviation increase in the predicted mean peer employability (lasso model) on the individual unemployment probability in the months 1-60 after program start. Significant effects at the 5 percent level are marked by circles. On top of the mean peer employability, the underlying model includes the individual ex-ante employability, unemployment at program start, a vector of course-level controls (course size, average planned duration, weekly hours, number of hours in practice and class, total course costs, target occupation) and provider-by-cohort and seasonal fixed effects. Standard errors are clustered at the provider-by-cohort level.

Figure A.7: Monthly effects of peer employability (logit) on individual (a) employment and (b) unemployment (including program participation)



(a)

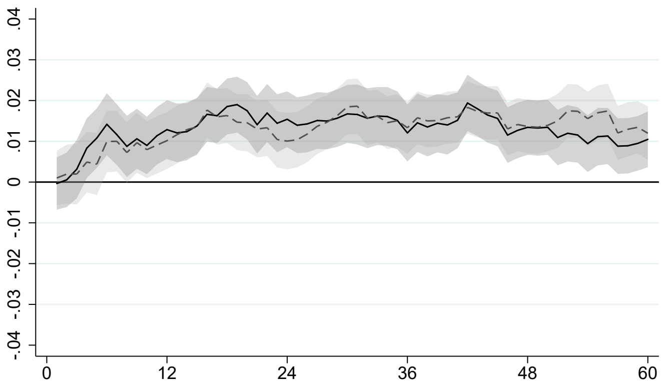


(b)

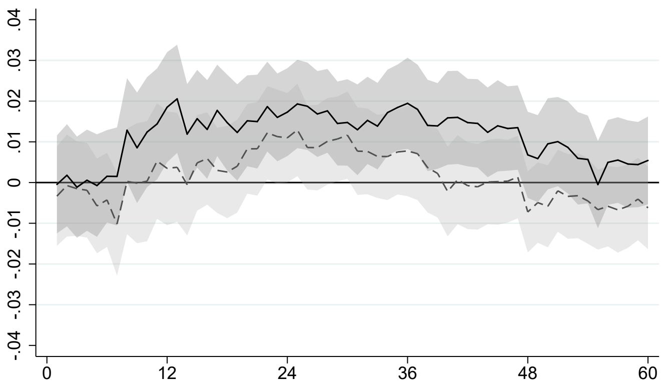
Notes: The figures depict the estimated effects (in precentage points) of of a standard deviation increase in the predicted mean peer employability (logit model) on the individual (a) employment and (b) unemployment probability in the months 1-60 after program start. Significant effects at the 5 percent level are marked by circles. On top of the mean peer employability, the underlying model includes the individual ex-ante employability, unemployment at program start, a vector of course-level controls (course size, average planned duration, weekly hours, number of hours in practice and class, total course costs, target occupation) and provider-by-cohort and seasonal fixed effects. Standard errors are clustered at the provider-by-cohort level.

Figure A.8: Heterogeneous effects of peer employability (lasso) on individual employment with respect to own employability

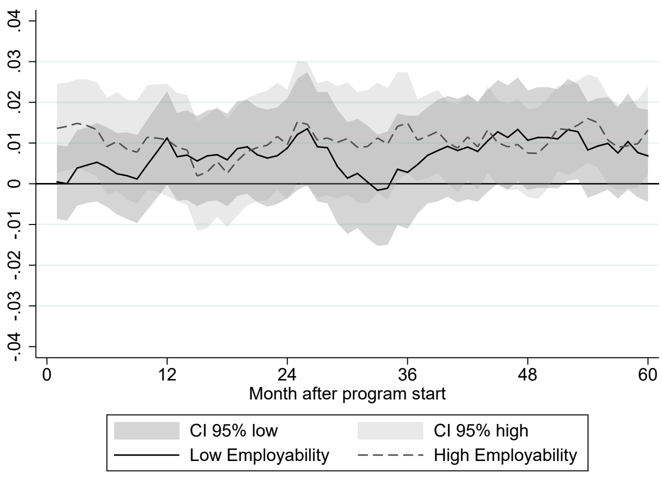
(a) Short Training



(b) Long Training



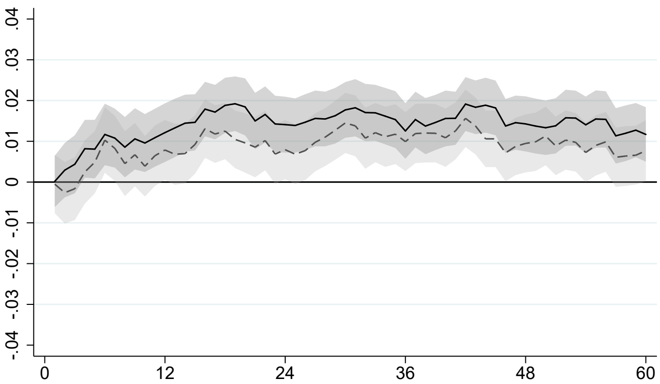
(c) Retraining



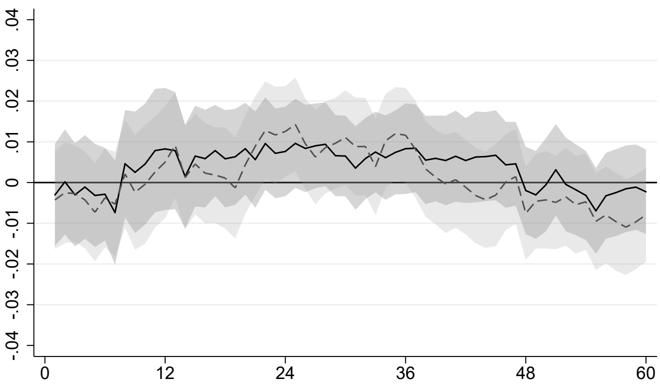
Notes: The figures depict the estimated effects (in percentage points) of a standard deviation increase in the predicted mean peer employability (lasso model) on the individual employment probability in the months 1-60 after program start for participants with an individual employability below and above the median employability. 95% confidence intervals are depicted as shaded areas. On top of the mean peer employability, the underlying model includes the individual ex-ante employability, unemployment at program start, a vector of course-level controls (course size, average planned duration, weekly hours, number of hours in practice and class, total course costs, target occupation) and provider-by-cohort and seasonal fixed effects. Standard errors are clustered at the provider-by-cohort level.

Figure A.9: Heterogeneous effects of peer employability (lasso) on individual employment with respect to gender

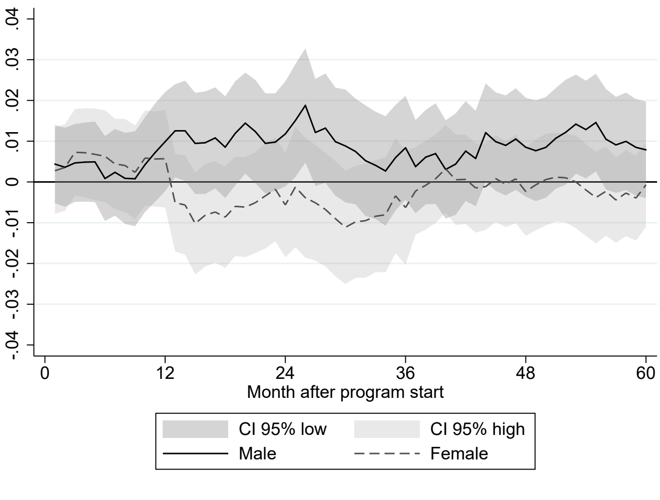
(a) Short Training



(b) Long Training

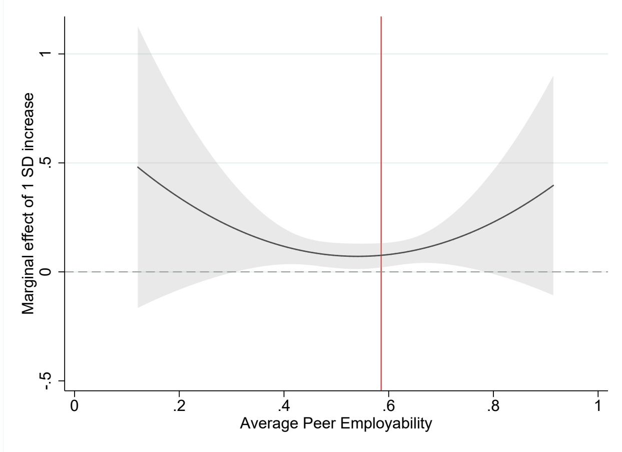


(c) Retraining

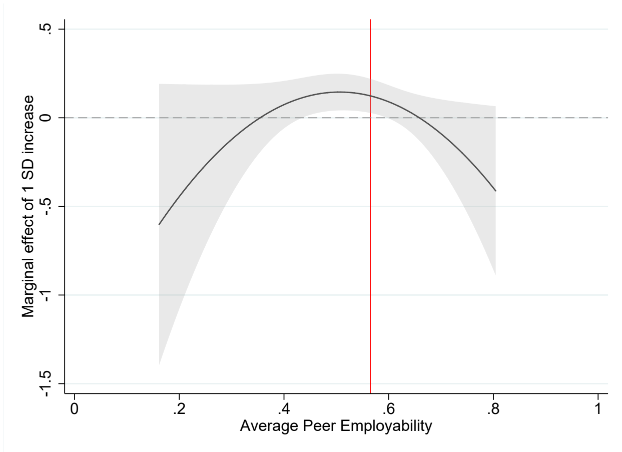


Notes: The figures depict the estimated effects (in percentage points) of a standard deviation increase in the predicted mean peer employability (lasso model) on the individual employment probability in the months 1-60 after program start for males and females. 95% confidence intervals are depicted as shaded areas. On top of the mean peer employability, the underlying model includes the individual ex-ante employability, unemployment at program start, a vector of course-level controls (course size, average planned duration, weekly hours, number of hours in practice and class, total course costs, target occupation) and provider-by-cohort and seasonal fixed effects. Standard errors are clustered at the provider-by-cohort level.

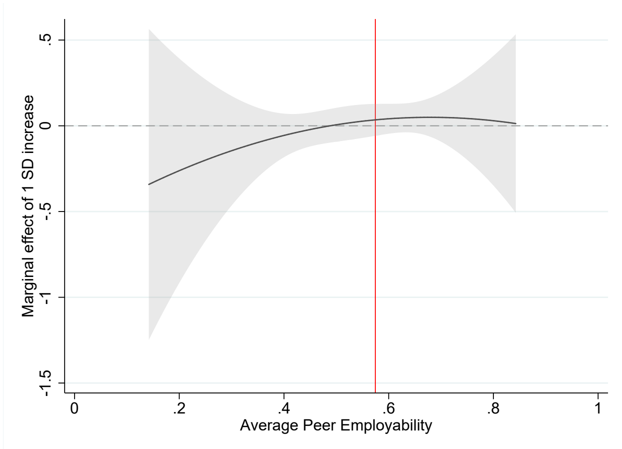
Figure A.10: Marginal effects of a one standard deviation increase on log cumulative earnings 60 months after program start



(a) Short Training



(b) Long Training



(c) Retraining

Notes: The figures depict marginal effects of a SD increase in the average peer employability on log cum. earnings 60 months after program start, estimated by a cubic polynomial. 95% confidence intervals are depicted in grey. The red line indicates the average peer employability level in the sample. The underlying model controls for the individual ex-ante employability, unemployment at program start, a vector of course-level controls (course size, average planned duration, weekly hours, number of hours in practice and class, total course costs, target occupation) and provider-by-cohort and seasonal FE. Standard errors are clustered at the provider-by-cohort level.