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 jobseekers in Germany. Using rich administrative data, I investigate how individual labour market outcomes of program participants are affected by the peer “quality” in the course, 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. I find strong evidence that the peer composition matters. Greater average exposure to highly-employable peers has a moderate positive impact on job stability after program participation. Peer effects on earnings are large and differ by program type. They are positive, and long-lasting in classic vocational training and negative but of short duration in retraining. Jobseekers with an individual employability below the median benefit comparatively more across all programs. Overall, the results suggest that peer effects depend on specific program features.

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

JEL Codes: J64, J68

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1 Introduction

There has been an active interest in the evaluation of active labour market policies (ALMP) over the last decades. The earlier literature has primarily focused on evaluating how these programs affect the short and long-term labour market outcomes of jobseekers. 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 of participants to program types. So far however, little is known about the role of the course composition and social spillovers within such programs.

ALMP combines two contexts where the importance of peer effects has been documented: the educational and the job search context. In education, the literature has found that peer ability is a significant determinant of student achievement and labour market outcomes (Sacerdote, 2011; Paloyo, 2020). In the context of job search, social networks have been shown to 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). It is thus likely that peer effects also matter in the context of ALMP. In many of these programs, jobseekers interact on a daily basis with other jobseekers and might be affected by the “quality” of their peers. On the one hand, jobseekers might benefit from existing networks or skills of peers with good employment prospects and find jobs more easily. On the other hand, they might loose in self-esteem or have difficulties to find jobs because they compete with relatively more employable peers. Which of these mechanisms operate and which peer effects emerge is likely to depend on the characteristics of the participants and the program itself. The direction of peer effects and whether any tracking of jobseekers is beneficial is thus not clear ex ante and calls for an empirical analysis.

There are several reasons why it is important to analyse peer effects in ALMP. First, if such externalities exist, we learn how the group composition affects the effectiveness of such programs in the status quo. Second, it gives us an idea of how programs could be redesigned to achieve a better labour market integration of jobseekers. Third, public spending on active labour market policies remains high in many industrialized countries (ranging from 0.2% to 2% of GDP in EU countries (European Commission, 2022) and the efficient use of these resources is thus of great interest.

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

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1For an overview, see e.g. Heckman et al (1999), Martin and Grubb (2001), Card et al. (2010, 2018), Crépon and van den Berg (2016).
by the employment prospects of their peers. For this, I construct a sophisticated measure of peer employability that summarises a large number of individual and labour market characteristics. First, I estimate the effect of an increase in the average peer employability on employment outcomes and second, allow for non-linearities in the peer effects. To identify a causal effect, I exploit idiosyncratic changes in the group composition within courses offered by the same training providers over time. To rule out self-selection with respect to course timing, I exploit the limited validity of vouchers which jobseekers can use to enrol in specific courses and only compare courses in a four month interval. I use 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 to 2012. Peer groups are identified by linking individuals who attend the same training program and have some overlap in time.

I document the following set of results. First, in classic vocational training, a greater average exposure to highly-employable peers has a positive impact on individual labour market outcomes after program participation. While I find no effect on search duration, a one standard deviation increase in the average peer employability increases employment by 13 to 16 days up to five years after program start. This moderate increase in job stability goes hand in hand with a strong increase in individual earnings of 4 to 7%. For retraining programs, I find only a small effect on employment and a negative effect on earnings immediately after program participation that fades in the long run. Second, program participants with an individual employability below the median experience significantly higher benefits from more employable peer groups compared to participants with an employability above the median. Third, I find some evidence for non-linearities in peer effects which differ across program types. In long vocational training and retraining the peer effects are driven by participants at the top of the employability distribution. In short training it is individuals at the middle and top of the distribution that drive the effects. In sum, I find strong evidence that the peer composition matters and that it does so differentially across program types. I argue that in programs with different characteristics different mechanisms are at work that drive peer effects. While peer-to-peer learning and peer networks could be drivers of the positive peer effects in classic vocational training programs, such information spillovers are less likely in retraining. There, increased competition and shifts in self-perception could be explanations for negative earnings effects shortly after program participation.

This study contributes to different strands of literature. The first is the literature evaluating public sponsored training programs. Overall, these programs have been shown to have little or even negative effects on employment in the short run and positive and more substantial effects in the long run. See the recent meta-analyses by Card et al.
Research looking at effect heterogeneity suggests that program effects vary significantly with the characteristics of participants (e.g., Heckman et al., 1997; Bitler et al., 2006; Bergemann and Van den Berg, 2008; Behaghel et al., 2014; Cockx et al., 2019). 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; Card et al., 2018; Knaus et al., 2022). Kruppe and Lang (2018) find differences in the effectiveness of retraining depending on the target occupation and the gender of participants.

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; Cockx et al., 2019). Evidence clearly suggests 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., Behmcke 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 materialise. 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 considering compositional aspects. It examines whether there are potential benefits from regrouping participants.

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. Van den Berg et al. (2019) evaluate a job search assistance program for young unemployed workers in France. 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. Caria et al. (2022) show that job-search assistance programs can have negative spillover effects on non-participants. A randomised intervention in Ethiopia reduced information exchange and support between treated jobseekers and their job-search partners. 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 considering 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 non-linearities. See Sacerdote (2011, 2014), Epple and Romano (2011) or Paloyo (2020) for a more recent overview. Several studies suggest that low achieving students benefit relatively more from the presence of other high ability students (e.g. Cooley Fruehwirth, 2013; Mendolia et al. 2018). At the university level, studies find mostly small peer effects on performance from classmates and room-mates (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 4 introduces the data and reports some descriptive statistics. I define the peer variables of interest, discuss my identification and estimation strategy in Section 5. The results are presented in Section 6. Section 7 concludes.

2 Institutional Background

Further vocational training 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 a successful labour market integration.

Since 2003, the assignment of unemployed individuals to courses has been regulated by a voucher system. Once an 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. Notice, that it is valid for a period up to three months from the date of issuance. Training providers are independently organised 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 to not 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 an individual decides to take up a course, participation is mandatory.

2See e.g. Kruppe (2009) or Doerr et al. (2017) for a detailed description of the institutional details.

3Notice, that participants might obtain a new voucher if the old one expires. Once they redeem the
For my analysis, I aggregate different public sponsored training programs into groups according to their homogeneity with respect to educational contents and organisation. I distinguish between short and long classic vocational training and retraining. All considered types of trainings require full-time participation and combine classroom training with practical elements such as on-the-job training. They are typically provided in small groups. Participants thus have the possibility to interact on a daily basis during the training and in the breaks. All programs are offered for a wide range of fields. 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 3.7 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 generally last up to one year. They focus on maintaining, updating, adjusting, and extending occupational skills. Such 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 or degree courses (retraining) have the longest duration of 2 to 3 years and provide a highly standardised training for a new vocational degree according to the German system of vocational education. They focus on jobseekers who have never completed any vocational training or have not worked in the occupation they are trained for a minimum of four years. The human capital enhancement that is provided by these courses is thus substantial.

3 Which peer effects do we expect?

Jobseekers in vocational training courses interact on a daily basis and are exposed to their peer group during several hours in the classroom and possibly during the breaks. On the one hand, the setting is thus similar to an educational setting at schools where individuals are provided with a set of skills in a group context. On the other hand, participants in further vocational training courses are unemployed and simultaneously looking for jobs, which means that peers are also potential competitors. In the following, I discuss different possible mechanisms through which the employability of peers could affect individual behaviour and labour market outcomes in this context.

A first possible mechanism is peer-to-peer learning. Knowledge spillovers have been discussed as mechanisms behind peer effects in education (Booij et al., 2017; Kimbrough et al., 2022) and at the workplace (De Grip and Sauermann, 2012; Frakes and Wasserman, 2021). Also in vocational training programs, participants might learn from each other and benefit from the skills and knowledge of more employable peers. In a software voucher, non-attendance can lead to benefit sanctions (Doerr et al., 2017).

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training for example, employable peers might share existing know-how on how to perform certain tasks more efficiently. In addition, there could also be knowledge transfers about effective job search strategies such as advice on how to write applications or where to apply. This peer-to-peer learning could improve the skill set and productivity of jobseekers and thereby positively affect their employment opportunities. If information about job search strategies is shared, it is possible that program participants find jobs faster. Skill spillovers could increase job stability and earnings.

A second and closely related mechanism are referrals and job search networks. Employable peers may have better networks and be able to share valuable information with their peers of how to and where to look for jobs effectively. The importance of informal job search networks has been well documented in the literature. It has been shown that individuals benefit from their friends, family or the neighbourhood when looking for jobs and find more stable employment with higher wages (see e.g. Cingano and Rosolia 2012; Pellizzari 2016; Brown et al. 2016; Dustmann et al. 2016). Such effects could also materialize in the context of training programs.

A third potential mechanism is social conformity. Individuals like to act in conformity with their peers and social norms. If they deviate from these norms, they experience losses in utility (see e.g. Bernheim 1994; Akerlof and Kranton 2000 for a theoretical foundation of this argument). 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). A recent contribution by Fu et al. (2019) posits that social comparisons affect job search and that jobseekers orient themselves at their peers’ reservation wages. Also in training programs jobseekers might respond to the job search behaviour of their peers. For example, they might feel pressured to look for jobs faster once highly employable peers exit the program and start working. This might reduce their unemployment duration but possibly also lead to lower entry wages if workers are less selective with regard to their wage.

A fourth mechanism that has been primarily discussed in the education literature (Hoxby and Weingarth 2005; Cristian Pop-Eleches et al. 2013; Antecol et al. 2016) but also in the context of vocational training (Van den Berg et al. 2019) are shifts in self-confidence. Jobseekers might compare themselves to their peers and gain in confidence if they are exposed to peers with relatively poor prospects. In contrast, they might loose in self-esteem and feel discouraged when exposed to a more employable peer group. In the latter case, jobseekers might become demotivated and exert lower search efforts which could

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5 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).
prolong the job search duration and negatively impact the quality of jobs after program participation.

A fifth mechanism that would predict negative peer effects is competition. If jobseekers are grouped with highly employable peers, they might face higher competition on the job market. Competing for the same jobs might negatively affect their employment and earnings, by forcing them to search for longer or possibly taking up lower paid jobs.

Finally, course instructors might endogenously react to the average group quality and adapt their teaching style or the course content. These so called teacher effects have been shown to matter in the school context (Duflo et al., 2011; Lavy et al., 2012; Hoekstra et al., 2018). For highly employable groups, such responses might increase the skills jobseekers acquire during training and their productivity. They might be more attractive to employers and find better paid jobs.

The empirical analysis will inform us on which mechanisms are most likely to drive peer effects in vocational training, if they exist. While it is possible that only one of the presented mechanisms is relevant, several mechanisms could be at work simultaneously. If peer-to-peer learning, job search networks or teacher responses dominate, we would expect to find positive peer effects on employment and earnings after program participation. If competition or shifts in self-confidence prevail, we would expect the opposite. In case social conformity is a main driver and jobseekers are pushed into a fast job entry, we might find reductions in both job search duration and on earnings of the first job.

4 Data and Sample Selection

The analysis is based on administrative data provided by the Institute for Employment Research (IAB). 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 between 1998 and 2012 which is used to compute individual and peer employability (see Section 5.1 and Appendix A.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 characteristics, labour market histories with daily accuracy and the region of residence). As the entire population of registered program participants is covered, I am able to identify peer groups, namely individuals who attended the same course together.
First, I impose sample restrictions at the course level. I focus my analysis on courses for which a peer effects analysis is sensible given the group size and the organization of the training. 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. I further restrict the sample to courses where the number of participants lies between 5 and 30. For reasons related to my identification strategy (see Section 5.2), I only consider training providers which offer courses once per month but multiple times in the observation period. Once the peer variables are constructed, I confine the estimation sample to individuals doing their first vocational training program within my observation window. This yields a final sample of 47279 program participants.

Table 1 summarises selected course and individual characteristics as well as outcome variables by program type. The majority of individuals is in short vocational training programs with a total of 2805 courses, followed by long vocational training with 1009 courses. Retraining programs comprise a sample of 994 courses. Panel A reports the course characteristics. The average number of courses a provider offers over time ranges from 5 to 7. It stands out that providers generally specialise on few occupations. In particular, for longer training programs which require more infrastructure and knowledge, the number of programs a provider offers in the same occupation over time almost coincides with the overall number of programs offered over time. On average there are about 11 to 12 individuals in one course. Appendix Figure A.8 shows the distribution of individuals over different course sizes. Most of the individuals start at the earliest entry date. They spend the majority of course time in class and only few hours in on-the-job training. Total course costs depend on the duration of the programs, but are comparatively high for short training. Generally, it is likely that program participants do not know each other when entering the program. Nevertheless, it cannot be fully ruled out. To assess the magnitude of this probability, I 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 my sample. I will test the sensitivity of my results to the exclusion of these courses in a robustness check.

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6 For the construction of the relevant peer variables (see Section 5.1) 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.

7 On average individuals overlap for 80 percent of the total course duration in short training, 75 percent in long training and 60 percent in retraining.

8 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. 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 characterised by partially overlapping peer groups. Identifying peer effects in these types of courses requires a different identification strategy.

9 A mass lay-off in a particular region might indeed 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.
Panel B of Table 1 shows a selection of individual characteristics. While individuals are relatively comparable across program types in terms of their recent labour market history, there exist some differences in terms of demographic characteristics, education and training. Compared to long and short training programs, participants in retraining programs are on average three years younger, more female, less likely to have a high-school degree or vocational training and worked in lower-paying jobs. These differences can be explained by the nature and target group of the respective programs.

The key outcomes of my analysis are individual employment and earnings after program start. They are described in Panel C of Table 1. I consider different employment outcomes which inform about how fast a program participant found a job and how stable their employment was in the years following program participation. Concretely, look at job search duration and at total employment up to 60 months after program start, both measured in days. Since I observe the labour market status of program participants with daily precision, I also study the effects of peer quality on employment more dynamically by considering individual employment probabilities in each month after program start. 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 to 70 percent after around 40 months. The employment rate takes different paths depending on the program type. Retraining programs which have

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

| Panel A - Course characteristics | Short training mean | sd | Long training mean | sd | Retraining mean | sd |
|----------------------------------|---------------------|----|-------------------|----|-----------------|----|
| Number of courses per provider   | 7.22                | 4.79 | 5.57              | 3.88 | 4.95           | 4.40 |
| Number of courses of same occ. per provider | 4.97 | 3.49 | 4.44              | 3.48 | 3.59           | 2.35 |
| Course size                      | 12.23               | 5.15 | 11.69             | 5.18 | 10.70          | 4.95 |
| Fraction of ind. starting at earliest entry date | 0.93 | 0.13 | 0.94              | 0.11 | 0.92           | 0.13 |
| Average planned duration in months | 3.67 | 1.63 | 9.07              | 3.11 | 22.67         | 7.70 |
| Weekly hours                     | 38.69               | 3.14 | 38.37             | 3.00 | 38.27          | 3.00 |
| Total hours in practice (Betrieb) | 12.03 | 88.68 | 23.28             | 163.29 | 18.25 | 250.93 |
| Total hours in class             | 427.50              | 239.18 | 956.46            | 417.39 | 2009.46        | 885.99 |
| Total cost (in 1000 euro)        | 2.89                | 2.19 | 6.33              | 3.60 | 10.31          | 5.00 |
| Target occupation w/ skilled tasks | 0.64 | 0.48 | 0.69              | 0.46 | 0.92           | 0.27 |
| Target occupation w/ complex tasks | 0.09 | 0.28 | 0.12              | 0.33 | 0.01           | 0.09 |
| Apprenticeship certification exam or similar | 0.02 | 0.15 | 0.07              | 0.26 | 0.66           | 0.47 |
| Courses where individuals worked in the same firm as 20% of peers | 0.05 | 0.22 | 0.04              | 0.20 | 0.05           | 0.23 |

Panel B - Individual characteristics

| Age in years | 37.71 | 10.88 | 37.81 | 9.80 | 34.61 | 8.16 |
| Female       | 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 |
| 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 program start | 10.25 | 20.72 | 12.24 | 22.64 | 11.96 | 22.05 |
| Months employed (last 2 years)   | 13.69 | 8.46  | 12.75 | 8.58 | 12.81 | 8.53 |
| Total earnings (last 2 years, 1000 euro) | 22.59 | 22.47 | 20.50 | 20.93 | 18.03 | 17.30 |
| No of programs in last 2 years   | 0.83  | 1.05  | 0.89  | 1.06 | 0.95  | 1.07 |

Panel C - Outcomes

| Search duration for first job (in days) | 716.71 | 939.322 | 824.88 | 934.297 | 966.60 | 817.757 |
| Total employment in month 60 (in days) | 1106.32 | 592.298 | 1033.61 | 569.072 | 909.19 | 501.941 |
| Log total earnings in month 60        | 9.71  | 3.175  | 9.65  | 3.191 | 9.68  | 2.837 |
| Log daily earnings in first job       | 2.87  | 1.507  | 2.81  | 1.526 | 2.74  | 1.475 |
| Entering same firm as one of the peers enters | 0.01  | 0.104  | 0.01  | 0.112 | 0.01  | 0.110 |
| Entering same firm as one of the peers exited | 0.01  | 0.109  | 0.01  | 0.092 | 0.01  | 0.089 |
| Observations                         | 28199 | 9598    | 8641   |
| Number of courses                    | 2805  | 1009    | 994    |
| Number of providers                  | 635   | 281     | 307    |

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. Abbreviations: Occ. occupation, ind. individuals.
longest planned duration are characterised by the slowest increase but ultimately reach the highest level of employment. Because jobseekers in these programs are working toward a vocational degree, there is a particularly high incentive to stay in the program until completion. This can be observed by the yearly kinks.

With regard to earnings, I consider daily earnings in the first job and total earnings up to 60 months after program start. Both earnings outcomes are measured in logs. Since the data contains no information about working hours, daily earnings and total earnings can represent earnings from part-time or full-time jobs\(^{11}\) Probably partially explained by different lock-in effects of the programs\(^ {12}\), individuals in retraining programs accumulate less earnings than individuals in short or long training programs. They also have comparatively lower daily earnings in their first job. Finally, in order to shed some light on the nature of peer effects, I look at some firm outcomes. I investigate whether individuals enter the same firm as any of their peers after the program or if they start working at a firm where any of their peers worked at before starting their program.

5 Empirical Strategy

5.1 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 the leave-one-out sample average of individual \(i\)'s peers employability in group \(g\):

\[
\bar{X}_{(−i)g} = \frac{1}{n_g} \sum_{j \neq i} X_{jg}
\]

Since I do not observe a measure of employability directly in the data, I summarise individual background characteristics which are likely to contain information on a jobseeker’s employability in a single score. I follow an approach similar to Van den Berg et al. (2019)\(^ {13}\) and define employability as the probability of finding a long-term contract within one year of entering unemployment. Long-term contracts are defined as contracts

\(^{11}\)Note that for individuals who are not employed until the end of our observation period, their total employment and log total earnings are set to 0. The same applies to individuals not having a first job by the end of the observation period: Log earnings are set to 0 and the search duration is censored at the end of our observation period.

\(^{12}\)The literature evaluating the overall effectiveness of such programs, documents such lock-in effects. See e.g. McCall et al. (2016).

\(^{13}\)Other studies also use imputed measures of ability to study peer effects, see e.g. Burke and Sass (2013) or Thiemann (2022).
that last for more than 6 months. For the estimation of the model, I rely on a population of comparable jobseekers that do not participate in any program. The reason for doing so is that the employment status without program participation is unobserved for actual participants.

The procedure is the following: First, I draw a random sample of individuals who enter unemployment in the same years as the program participants but do not participate in any program (1.5 million observations). Second, I apply nearest neighbour matching based on the propensity score to adjust for observable differences between the two groups and draw a matched sample of non-participants (the matching procedure is described in detail in Appendix A.1.1). Third, I estimate a logit model based on this sample of non-participants where 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 regression output is displayed in Appendix Table A.2. Fourth, I use the estimated coefficients to predict employability scores for the sample of participants (out-of sample). Finally, I construct the leave-one-out peer averages using the predicted employability.

The distributions of the predicted individual and average employability are shown in Appendix Figures A.2 and A.3 separately by program type. The individual employability ranges from 0.08 to 0.97 while the average employability is naturally more compact, ranging from 0.2 to 0.9. Overall, the distributions are very similar and largely overlapping across program types indicating that participants are quite comparable across program types in terms of their employability. The fact that participants in retraining have lower initial education (see Section 4) does not directly translate into different employment opportunities.

The employability measure allows me to consider a variety of information without having to estimate a high-dimensional model. It entails a number of advantages compared

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14 Note that in around 27 percent of cases individuals do not participate in any program after entering unemployment.

15 Weights are applied to account for the number of times a non-participant has been chosen as a match.

16 In the sample of non-participants I reach an accuracy of prediction of 68 percent. The predicted employability for participants and non-participants is shown in Appendix Figure A.6.

17 I estimate the employability score jointly for all program types. In a sensitivity analysis, I tested whether the scores are affected by potential heterogeneity in predictors across program types. For this, I performed the above mentioned procedure separately by program type. Appendix Figures A.4 and A.5 show that the employability measures that are jointly and separately predicted are very similar. The difference is largest for individuals in long training. The results are robust to using the employability estimated in subsamples. They are available upon request.

18 There exists some evidence that access to further vocational training in Germany is somewhat restricted to individuals with high employment potential (Kruppe 2009). Individuals that are e.g. long-term unemployed, have disabilities or no education have less chances to obtain a voucher in the first place. This supports my finding of a rather homogeneous employability distribution across program types.
to an analysis which considers several 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.\textsuperscript{19} 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 jobseeker. An assessment of the individual employability could thus be rather easily implemented in practice.\textsuperscript{20} In order to address concerns related to a potential measurement error in the employability variable\textsuperscript{21} and to understand possible channels of peer employability, I will conduct a sensitivity analysis where I also consider several readily available peer characteristics separately.\textsuperscript{22} The strongest predictors of employability can be identified from the regression output (Appendix Table A.2) as training, earnings in the past job, occupation, type of employment, recent and long-term labour market attachment as well as old age and disability. Moreover, time of entry into unemployment and region of residence are of importance. Thus, employability depends to a large extent on individual characteristics, which are malleable, as well as on contextual job search factors. This insight is important for the interpretation of any peer effects that I find. In contrast to gender for example, employability can be understood as a peer characteristic that can be shaped.

5.2 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\textsuperscript{23} arises because of the simultaneity in peer behaviour meaning that an individual affects his peer’s outcomes and the peers affect the individual’s outcome. In the context of ALMP, program participants might for example affect each other through their job search behaviour. 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 these effects, but estimate a joint effect.\textsuperscript{24} This joint effect is captured by the average peer employability which might

\textsuperscript{19}It has been pointed out by Graham (2011) that it is not straightforward to study the effects of multiple peer attributes simultaneously and that a ceteris paribus interpretation of such effects is difficult.

\textsuperscript{20}Comparable scores based on predictive algorithms have already been used by public employment services for profiling jobseekers. See Körtner et al. (2019) 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.

\textsuperscript{21}Angrist (2014) points out that peer effects estimates are sensitive to measurement error. In Section 5.4 I argue that measurement error in this setting would to merely cause a small attenuation bias.

\textsuperscript{22}The results of this model are shown in Appendix A.3.1 and suggest that individuals benefit from peers with successful labour market histories. This is in line with the findings from the main analysis and suggests that our measure of peer employability is a proxy for the labour market attachment of peers.

\textsuperscript{23}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 for example matter for the effectiveness of a program, it might not be relevant whether it is peer employ-
proxy peer behaviour but is determined before program start.

The selection problem arises because of common unobserved shocks at the group level on the one hand and endogenous peer group formation on the other hand. In my 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 they might self-select into specific providers or course depending on the characteristics of the course (i.e. timing, content or location).

My 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. Hoxby, 2000; Ammermueller and Pischke, 2009; Bifulco et al., 2011; Lavy et al., 2012; Elsner and Isphording, 2017; Carrell et al., 2018).

Similarly, I control for provider choice and exploit the variation in peer employability between comparable courses offered by the same training providers over time. Moreover, I restrict my attention to providers which offer courses exactly once per month and only compare courses that are four months apart. This restriction is based on the idea that jobseekers can select into specific course months only up to three months after they obtain their voucher.

The intuition behind the identification strategy is illustrated in Figure 2: 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, I 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 I label month groups: January-May-September, February-June-October, March-July-November and April-August-December. They are listed as rows in Figure 2. Conditional on the

ability per se or the unobservable characteristics or behaviours correlated with employability.

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 (Bramoulle et al., 2009; De Giorgi et al., 2010) or by exploiting varying group sizes (Lee, 2007; Boucher et al., 2014).

Even within this 3 month period jobseekers might be constrained in their choice by limited availabilities and capacities of courses.
Notes: The figure depicts feasible comparison groups ($\lambda_{pc}$) 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 4-month divisions of the calendar year $\delta_t$ which serve as seasonality controls.

choice of provider $p$ and the month group $c$, the entry into a specific course is driven by her voucher issuance date which is unlikely to be manipulated. Participants can sort across provider-specific month groups $\lambda_{pc}$ but not within. I can thus overcome systematic self-selection into groups based on preferences for location and time by comparing only courses at the same provider that are four months apart. Since jobseekers starting courses towards the beginning, middle or end of the year might differ in their characteristics, I further control for aggregate time trends. They are captured by seasonality dummies that correspond to 4-month divisions of the respective calendar year (January to April, May to August and September to October) as shown by the columns $\delta_t$ in Figure 2.

Jobseekers have a very limited choice with respect to course content. It needs to match the educational target which is defined during the profiling process with the caseworker and depends on their qualifications but also on the current labour market situation. Nevertheless, it will be important to compare courses of a similar content which I proxy with the skill level and target occupation of a specific course. Note that in my setting conditioning on the provider choice implicitly controls for course content since providers are usually specialised on specific target occupations and thus offer comparable courses over time (see Section 4).
5.3 The Empirical Model

I estimate the following linear-in-means model separately for the three types of training programs:

\[ y_{ipct} = \alpha + \gamma X_{ipct} + \theta \bar{X}_{(i)pct} + \pi W_{pct} + \lambda pc + \delta t + \epsilon_{ipct}, \]  

(2)

where the outcome of an individual \( i \) at a training provider \( p \) in month group \( c \) and time \( t \) (four month interval) is a linear function of the individual’s own observable characteristics \( X_{ipct} \), being her own employability and unemployment duration at program start, and the leave-one-out mean employability of individuals in the same course \( \bar{X}_{(i)pct} \). The coefficient of interest is \( \theta \), 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.

To implement the identification approach illustrated in Figure 2, I include provider-by-month group, \( \lambda pc \) and seasonal fixed effects, \( \delta t \) in the model. Provider-month group fixed effects control for all observable and unobservable mean differences across provider-month group combinations that are constant over time. Seasonal fixed effects control for correlated effects which change over time but are the same across providers and month groups. I additionally control for a vector of course-level characteristics, \( W_{pct} \) which contains the course size, the average planned duration, weekly hours and total hours spent in practice and class. \( W_{pct} \) also includes a set of fixed effects for the target occupation and the skill level associated with a course. These will take care of differential sorting into courses of specific skills that is constant across providers.

Identification relies on the assumption that the variation in peer employability across courses across courses in a given month group offered by the same provider and after removing seasonality and occupation specific effects 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. \( E[\epsilon_{ipct} | X_{ipct}, \bar{X}_{(i)pct}, W_{pct}, \lambda pc, \delta t] = 0 \). I further assume that there are no spillover effects across courses such that the stable unit treatment value assumption (SUTVA) holds.

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26 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.

27 I control for 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.
5.4 Validity Checks

The empirical strategy will produce unbiased estimates of peer effects if a) the residual variation in peer employability is exogenous and b) peer employability is measured without measurement error.

I investigate the plausibility of an exogenous variation in peer employability with a number of checks. First, I characterise the raw and residual variation in the average peer employability. Figure 3 plots the raw distribution of the variable of interest in the form of a solid line centred at zero. The residual variation after controlling for fixed effects and course controls is depicted in the form of a dashed line. The raw average peer employability ranges has a standard deviation of 0.08. The distributions by program type are summarised in Table 2. They are comparable, with short training courses having a slightly lower raw variation compared to long training and retraining courses. Netting out fixed effects and course controls reduces the variation to about half in all program types with the reduction being strongest for long training. Overall, the remaining variation should be sufficient in order to estimate the effects of interest. Second, I test whether the residual variation in peer employability is in line with a variation resulting from random fluctuations. For this I perform a resampling exercise as in [Bifulco et al., 2011] and compare the observed residual variation with a simulated variation based on a random group allocation. I find that the variations are closely matching. Third, I check for systematic correlation between own and peer employability (as in Guryan et al., 2009). I document the absence of such a correlation which provides additional supportive evidence that peer assignment is random. Fourth, I test whether individuals with higher employability sort into certain course months or course types (as measured by the target occupations). Such systematic sorting could cause a selection bias, if not controlled for. I find no evidence for sorting with regard to course timing and some sorting with regard to occupations. This sorting will be taken care of by controlling for provider choice and target occupations as described in Section 5.3. All results, as well as further details on the identifying assumptions as well as the above mentioned tests, are documented in Appendix A.2. Overall, I find strong support for the assumption of the residual variation being idiosyncratic.

Measurement error in my treatment variable could come from two sources. First, the predicted individual employability could suffer from a prediction error. Such an error could for example occur if not all relevant predictors of employability are included in the model. Due to the richness of the data, this error should be small. Furthermore, it is likely to be orthogonal to the true employability variable and the error term, i.e. classical. Second, I might not observe all peers in my data which could lead to a mismeasurement of peer variables. I discuss this possibility in detail in Appendix A.2.4. Overall, I argue for the fraction of missing peers to be small and for the missing data to be distributed independently of group assignment and $\epsilon_{ipct}$, conditional on fixed effects and course controls.
Notes: The figure plots the raw distribution of the average peer employability (solid line) and the distribution of the average average peer employability net of provider-month group fixed effects, seasonality fixed effects and course controls. It is centred at the mean of 0.66 (dashed line). SD refers to the standard deviation. Under these conditions measurement error would merely cause a small attenuation bias and my results would represent a lower bound of the true peer effects (see Ammermueller and Pischke 2009, Feld and Zölitz 2017, Sojourner 2013).

6 Results

This section presents and discusses the results of the analysis in four parts. First, I examine the effects of an increase in the average predicted peer employability on individual labour market outcomes after program start. Second, I investigate whether there is effect heterogeneity with respect to own employability. Third, I test for non-linearities in the peer effects and estimate a model including the shares of individuals in different quintiles of the employability distribution. Fourth, I assess whether the degree of homogeneity within a group matters by including the group’s standard deviation of employability. All of the analyses are run separately by program type. Standard errors are clustered at the course level.
Table 2: Variation in Own and Peer Employability

|                      | Short training |            | Long training |            | Retraining |            |
|----------------------|----------------|------------|---------------|------------|------------|------------|
|                      | mean           | sd         | min           | max        | mean       | sd         | min           | max        | mean       | sd         | min           | max        |
| Panel A - Raw variation |                |            |               |            |            |            |               |            |            |            |               |            |
| Individual employability | 0.66           | 0.161      | 0.08          | 0.97       | 0.65       | 0.163      | 0.08          | 0.97       | 0.66       | 0.161      | 0.12          | 0.97       |
| Mean peer employability   | 0.66           | 0.081      | 0.20          | 0.90       | 0.65       | 0.081      | 0.30          | 0.86       | 0.66       | 0.083      | 0.26          | 0.88       |
| SD peer employability | 0.14           | 0.046      | 0.02          | 0.30       | 0.15       | 0.045      | 0.02          | 0.31       | 0.14       | 0.045      | 0.02          | 0.33       |
| Panel B - Variation net of fixed effects and course characteristics |                |            |               |            |            |            |               |            |            |            |               |            |
| Individual employability | -0.00          | 0.147      | -0.60         | 0.48       | -0.00      | 0.150      | -0.59         | 0.46       | 0.00       | 0.146      | -0.58         | 0.39       |
| Mean peer employability   | 0.00           | 0.049      | -0.31         | 0.25       | -0.00      | 0.046      | -0.27         | 0.28       | 0.00       | 0.047      | -0.30         | 0.19       |
| SD peer employability | -0.00           | 0.031      | -0.13         | 0.17       | 0.00       | 0.030      | -0.10         | 0.15       | 0.00       | 0.032      | -0.11         | 0.16       |
| Observations | 28199         | 9598        | 8641          |            |            |            |               |            |            |            |               |            |

Notes: This table shows summary statistics (mean, standard deviation (sd), minimum (min) and maximum (max)) of the employability variables by program type. Panel A displays the figures for the original variables (raw variation). Panel B refers to the residual variation in these variables (net of provider-month group fixed effects, seasonality fixed effects and course controls).

6.1 The Effects of Peer Employability on Individual Labour Market Outcomes

6.1.1 Effects on Job Search Duration and Employment

The effects of an increase in the average predicted peer employability on individual employment after program participation are presented in Table 3 separately by program type. Panel A shows the effects on job search duration, Panel B the effects on total employment up to five years after program start. Both outcomes are measured in days. The table presents the effects of a standard deviation increase in peer employability as well as the effect of a standard deviation increase in own employability.28

While an increase in own employability significantly reduces the time individuals look for a job after program start by around three months in all program types, 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 10 to 16 days up until 5 years after program start. The effect is strongest for participants in short programs and smallest for participants in retraining. In comparison, a standard deviation increase in jobseekers’ own employability increases employment by 112 to 126 days.

In order to investigate the dynamics behind the employment effects, I also estimate monthly effects of an increase in the average predicted peer employability on individual employment up to 60 months after program start. Figure 4 shows the results by program type. It depicts the effect estimates (in percentage points) of a one standard

28To get a meaningful effect size, I calculate the effect of a standard deviation increase multiplying the marginal effect with the residual standard deviation of the respective variable. See Panel B of Table 2.
Table 3: Effects of Peer Employability on Employment

|                      | Short training (1) | Long training (2) | Retraining (3) |
|----------------------|--------------------|-------------------|----------------|
| **Panel A - Search duration first job (in days)** |                    |                   |                |
| Mean peer employability | -7.133             | 7.970             | -1.055         |
|                      | (5.461)            | (8.063)           | (7.736)        |
| Own employability    | -97.471***         | -81.633***        | -86.889***     |
|                      | (5.788)            | (9.539)           | (8.898)        |
| **Panel B - Total employment in month 60**         |                    |                   |                |
| Mean peer employability | 16.355***         | 12.884***         | 10.311**       |
|                      | (3.303)            | (4.835)           | (4.812)        |
| Own employability    | 126.024***         | 120.226***        | 112.741***     |
|                      | (3.566)            | (5.431)           | (5.263)        |
| Provider-Cohort FEs  | ✓                  | ✓                 | ✓              |
| Seasonal FEs         | ✓                  | ✓                 | ✓              |
| Additional Controls  | ✓                  | ✓                 | ✓              |
| Observations         | 28199              | 9598              | 8641           |

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 course level. Effects are reported in terms of SD increases (see Table 2). * p < 0.05, ** p < 0.01, *** p < 0.001
deviation increase 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. Peer effects materialise for all program types after the average planned program duration, which could be expected given that the program participants reduce their search efforts during the time of the program and that I find no effect on job search duration.\(^{29}\) At that time, one standard deviation increase in the group’s average employability increases the individual employment probability by around 1 percentage point.\(^{30}\) The effects are particularly stable for short training, persisting up to 60 months after program start. For long training, the effects are slightly higher in the months 10 to 30, ranging between 1 and 2 percentage points. They fade out around three years after program start. For retraining, I find significant effects only in single months around one, two and four years after program start. It should be kept in mind, that the sample of participants in long training and retraining is much smaller compared to the sample of participants in short training which reduces the power to identify any effect there.

Overall, these findings suggest that exposure to a more employable peer group does not necessarily lead to a faster integration of participants into the labour market but rather increases employment stability. The effect size is moderate and amounts to 9-12 percent of the individual effect, representing a reasonable size. 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 one percentage point seems like a moderate and realistic increase. However, a direct comparison of the effect sizes is difficult, since the peer effects analysed here have to be interpreted conditional on participation.

### 6.1.2 Effects on Earnings

Table 4 presents the effects of a more employable peer group on individual daily earnings in the first job (Panel A) and total earnings in the first five years after program participation (Panel C). Again, the effects correspond to a standard deviation increase in peer and own employability and are displayed separately by program type. Since around 10 percent of program participants are never employed within the relevant observation window, I also estimate the effects on earnings for the sample of participants in employment. Specifically, I consider daily earnings in the first job in the sample of program participants who found a job up to December 2016 (Panel B) and total earnings in the sample of participants that

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\(^{29}\) Several evaluation studies have found evidence for substantial lock-in effects of training programs in Germany. See e.g. Lechner et al. [2011](#) [Biewen et al., 2014](#).

\(^{30}\) I find slightly higher effects and similar patterns when investigating the effect on regular employment, i.e. excluding individuals in marginal employment that is not subject to social security contributions. For participants in short and long training, the effects range from 1.5-2 percentage points. See Appendix Figure [A.10](#).
were employed at least once in the first 60 months after program participation (Panel D).

Figure 4: Monthly Effects of Peer Employability 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 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, occupation and provider-by-cohort and seasonal fixed effects. Standard errors are clustered at the course level.

I find that a one standard deviation increase in the average peer employability increases daily earnings in the first job by 3.6 percent for participants in short training. The effect is robust to excluding individuals who did not find a job until the end of the observation window. For participants in long training, the effect of a more employable peer group is zero in the full sample but positive and around 2 percent in the restricted sample. This suggests that participants in long training who are able to find jobs, also experienced an increase in earnings when being exposed to a more employable peer group. For participants in retraining, I find a negative effect on daily earnings of 2.4 to 2.9 percent.

Also in the long run, earnings are affected by an increase in the average employability of peers. I find an effect of 7 percent on total earnings in the first 5 years after program participation for participants in short training which corresponds to about 1100 euro. The
effect for participants in long training amounts to 4.5 percent but is not precisely estimated in the full sample. The effects on total earnings are slightly smaller but statistically significant when conditioning on employment. I do not find any effect for individuals in retraining.

Notice that the effect of own employability on earnings in the short and long-run is much larger in the full sample compared to the sample conditioning on employment. This can be explained by own employability picking up the effect of finding a job. Generally own employability does not have a positive effect on earnings in the first job after conditioning on employment. It is only significant for participants in retraining and negative. This suggests that some of the determinants of employability are negatively associated with daily earnings.

In sum, the results point out clear differences between program types. I find large earnings effects in classic vocational training programs suggesting that participants in these programs are able to find better-paid jobs after interacting with more employable peers. Also in the long run, positive earnings effects materialize which cannot be explained by more days in employment. In retraining, an exposure to a better peer group negatively affects participants’ daily earnings in the first job but has no effect on total earnings in the long run. The negative effect on earnings directly after program start is thus not persistent and jobseekers’ earnings recover. Overall, peer effects are not very sensitive to restricting the sample to employed individuals.

6.1.3 Interpretation and Mechanisms

Since participants do not fundamentally differ in their employability across program types, differences in effects are likely to originate from the features of the programs and the mechanisms at work. In the following, I will interpret the results and discuss how the mechanisms presented in Section 3 could apply to the different types of programs.

I find comparable effects for short and long vocational training programs. As indicated by the name, these programs differ primarily in their duration. They are comparable in the types of jobseekers that they attract and in that they aim at upgrading and extending specific occupational skills. The fact that peer effects are similar in these two programs is therefore not surprising. In both programs exposure to a more employable peer group increases individual job stability and earnings but has no effect on job search duration. Several of the proposed mechanisms are inconsistent with the documented results. First, competition and shifts in self-perception can be ruled out as dominant channels since they would predict negative peer effects on job stability and earnings. Second, social conformity is unlikely to be a major driver since program participants do not reduce their job search duration when exposed to a more employable peer group. Third, if course
Table 4: Effects of Peer Employability on Earnings

|                      | Short training | Long training | Retraining |
|----------------------|----------------|---------------|------------|
|                      | (1)            | (2)           | (3)        |
| Panel A - Log earnings first job |                |               |            |
| Mean peer employability | 0.031***       | -0.008        | -0.028**   |
|                       | (0.008)        | (0.015)       | (0.014)    |
| Own employability     | 0.087***       | 0.037**       | 0.043***   |
|                       | (0.009)        | (0.016)       | (0.016)    |
| N                    | 28199          | 9598          | 8641       |
| Panel B - Log earnings first job (if > 0) |                |               |            |
| Mean peer employability | 0.036***       | 0.021*        | -0.024**   |
|                       | (0.007)        | (0.012)       | (0.011)    |
| Own employability     | 0.008          | -0.016        | -0.038***  |
|                       | (0.007)        | (0.013)       | (0.013)    |
| N                    | 24391          | 8228          | 7527       |
| Panel C - Log total earnings in month 60 |                |               |            |
| Mean peer employability | 0.067***       | 0.045         | -0.005     |
|                       | (0.017)        | (0.029)       | (0.026)    |
| Own employability     | 0.460***       | 0.426***      | 0.407***   |
|                       | (0.021)        | (0.033)       | (0.035)    |
| N                    | 28199          | 9598          | 8641       |
| Panel D - Log total earnings in month 60 (if > 0) |                |               |            |
| Mean peer employability | 0.050***       | 0.031**       | 0.010      |
|                       | (0.008)        | (0.013)       | (0.013)    |
| Own employability     | 0.191***       | 0.211***      | 0.179***   |
|                       | (0.009)        | (0.016)       | (0.016)    |
| N                    | 25862          | 8796          | 8063       |

Provider-Cohort FEs ✓ ✓ ✓
Seasonal FEs ✓ ✓ ✓
Additional Controls ✓ ✓ ✓

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 course level. Effects are reported in terms of SD increases (see Table 2). Earnings are in prices of 2010 and measured in log(euro). * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
instructors were responding endogenously to the peer group composition, I would expect such behaviour to occur equally across training programs or to a greater extent in longer programs. In longer courses there is potentially more room and time for adaptations of the teaching style and course content. Since the results point to larger positive peer effects in shorter programs, teacher effects are unlikely to be the driver. Two remaining plausible channels behind peer effects in classic vocational training programs are thus peer-to-peer learning and peer networks. Jobseekers in short and long vocational training have a comparatively high labour market attachment and a relevant set of skills and networks that might spill over to other course participants. As pointed out in Section 3 such spillovers might help program participants to find well-paid first jobs and allow for more successful careers also in the longer run. Formally testing whether peer-to-peer learning or networks are driving the effects is difficult with the administrative data at hand. I indirectly test for the relevance of peer networks by evaluating whether referrals to the latest employer are more likely in highly employable peer groups. Specifically, I estimate whether the individual probability of taking up a job at the firm where any of the peers worked in their last job depends on the average peer employability. As shown in Appendix A.4 I do not find any evidence for referrals to a past employer for any of the program types. This suggests that an exchange of information about previous employers is not the main driver behind the documented peer effects. Nonetheless, jobseekers could still benefit from more general information about job search and the network of peers that goes beyond the latest employer.

Peer effects in retraining differ from classic vocational training programs particularly with respect to earnings. Being exposed to a more employable peer group in retraining has no effect on job search duration, a more moderate effect on job stability, a negative effect on earnings in the short run and no effect on earnings in the longer run. As in the case of classic vocational training, also in retraining peer-to-peer learning and the networks of peers might positively affect the skill set of workers and their access to good jobs, but they are expected to do so to a lesser extent. In retraining, every participant is trained in a completely new occupation and the likelihood that existing skills or networks of peers play a role is smaller compared to classic vocational training programs. Moreover, retraining programs are comparatively long, and the ex-ante employability of participants may no longer matter much at the end of the program. By then, participants in these programs might have a completely new set of skills. This might explain why I find lower peer effects on employment and no long-term earnings effects for these types of programs. Three mechanisms could explain the negative effects on earnings in the first job: social conformity, shifts in self-perception and competition. All of them are expected to affect labour market outcomes rather in the short run and predict negative effects on entry wages. Yet, they differ in their predictions of the impact of more employable peers on job search duration. While shifts in self-confidence and higher competition would be
expected to increase job search duration, social conformity would predict the opposite. I find no effect on job search duration but a negative effect on earnings in the first job which suggests that more than one of these mechanisms might apply. There are several reasons, why negative effects on earnings are more likely to occur in retraining compared to classic vocational training programs. First, the programs are longer which gives participants more time to get acquainted and increases the probability that they care about the characteristics and behaviour of their peers. Second, participants in retraining are more likely to enter in a direct competition once they exit the program compared to participants in classic vocational training programs. One the one hand, they are more likely to exit the program at the same time after obtaining their vocational degree (see e.g. the jumps in the employment rate in Figure 1). On the other hand, everyone enters the labour market with the same skills for the newly learned profession and might apply for a similar range of jobs. In fact, I show in Appendix A.4 that jobseekers in retraining are more likely to start working at the same employer as their peers when they are exposed to a more employable peer group. This further suggests that participants in retraining courses are somewhat coordinating their job search process.

6.2 Robustness to Knowing one’s Peers

In a robustness check, I aim to rule out that the effects I estimate are endogenously driven by program participants that knew each other from their previous employer. This could bias the peer effects if these jobseekers select into the same courses based on similar preferences or characteristics. I thus exclude all courses from the sample where more than 20 percent worked at the same firm before entering the program. The results are reported in Appendix Table A.5. They are very similar to the results of the main analysis with the effects on employment being slightly attenuated. In contrast to the main analysis, I now find a significant reduction of the search duration for the first job for participants in short training.

6.3 Does the effect depend on the own employability?

Both empirical and theoretical findings in the education literature suggest that students of different ability levels benefit differently from peer ability (e.g. Burke and Sass 2013, Carrell et al. 2013, Feld and Zölitz 2017). Further, the program evaluation literature has shown vocational training programs to be more effective for jobseekers with relatively bad employment prospects (Card et al. 2018). Therefore, I test whether individuals heterogeneously respond to the average employability in their group depending on their own employability.31

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31I also test whether there is any heterogeneity with respect to gender but do not find any evidence for that. See Appendix Table A.6.
I estimate the effects of peer employability on post-program employment and earnings based on the main specification, separately by type of participant. To categorize individuals’ own employability, I define program participants with a predicted employability below the sample median as participants with a low employability and participants with an employability greater or equal to the median as participants with a high employability. The results are displayed in Table 5. Participants with a low employability benefit clearly more from being exposed to a better peer group compared to participants with a high employability in short and long vocational training. The effects on total employment up two years after program start (Panel A) and up to five years after program start (Panel B) are both significant for low types but not significant or significantly lower for high types. The same pattern arises for the effect on earnings up to five years after program start (Panel C). For retraining, participants of low employability benefit more in terms of employment and loose out less in terms of earnings. Nevertheless, the differences in effects are estimated too imprecisely in order to draw strong conclusions.

In sum, these results suggest that participants who are less employable ex-ante draw the highest benefits from being surrounded by more employable peers. They might be able to particularly benefit from the knowledge, skills or networks of better peers and thereby increase their own chances on the labour market.

6.4 Looking Beyond the Average

So far the analysis has focused on how the average outcome of a randomly chosen program participant would change in expectation if the average peer employability was marginally increased. As pointed out by e.g. Graham et al. (2010) this does not measure the effect of an implementable policy for a fixed population of participants. In order to understand how we could optimally assign jobseekers to programs, we need to understand possible non-linearities in effects.

I estimate a more flexible version of model (2) that includes the fraction of peers in the top and bottom quintiles of the employability distribution instead of the average peer employability. The fraction of peers in the middle quintiles serves as the reference category. Everything else remains the same. Table 6 displays the results for the two long-term employment and earnings outcomes in separate panels. The first and second row of each panel report the effects of a 10 percentage point increase in the fraction of peers in the top and bottom quintile respectively accompanied by a reduction in the fraction of peers in the middle quintile. The third row reports the effect of the same increase in the

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32A 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.

33Non-linearities have been found to be important in the literature of peer effects in education (Hoxby and Weingarth 2005).

34Appendix Figure A.7 shows the employability values associated with the five quintiles.
Table 5: Heterogeneity in Effects of Peer Employability Depending on Own Employability

|                      | Short training | Long training | Retraining |
|----------------------|----------------|---------------|------------|
|                      | (1)            | (2)           | (3)        |
| **Panel A - Total employment in month 24** |                |               |            |
| PE low employability | 6.517***       | 8.028***      | 2.198      |
|                      | (1.610)        | (2.499)       | (2.692)    |
| PE high employability| 2.810          | 1.792         | 3.118      |
|                      | (1.746)        | (2.792)       | (3.172)    |
| P-value difference   | 0.045          | 0.032         | 0.769      |
| **Panel B - Total employment in month 60** |                |               |            |
| PE low employability | 16.436***      | 14.920**      | 9.851*     |
|                      | (3.827)        | (5.933)       | (5.946)    |
| PE high employability| 9.366**        | -3.034        | 3.592      |
|                      | (4.101)        | (6.472)       | (5.945)    |
| P-value difference   | 0.107          | 0.014         | 0.356      |
| **Panel C - Log total earnings in month 60** |                |               |            |
| PE low employability | 0.071***       | 0.072**       | 0.007      |
|                      | (0.020)        | (0.034)       | (0.035)    |
| PE high employability| 0.036*         | -0.040        | -0.052*    |
|                      | (0.020)        | (0.035)       | (0.031)    |
| P-value difference   | 0.121          | 0.005         | 0.142      |

Provider-Cohort FEs ✓ ✓ ✓
Seasonal FEs ✓ ✓ ✓
Additional Controls ✓ ✓ ✓
Observations 28199 9598 8641

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 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
fraction of peers in the top quintile accompanied by a reduction in the fraction of peers in the bottom quintile (difference in coefficients).

I find clear evidence for non-linearities in the effects for participants in short training. Increasing the fraction of peers in the middle or top quintiles (and reducing the fraction of peers in the bottom quintile respectively) results in an effect of a similar size. Cumulative employment increases by 16 and 22 days respectively, the effects not being significantly different from each other. Replacing peers in the middle of the distribution by peers in the top increases the effect on cumulative employment only by 6 days (see Panel A). The effect pattern is similar for cumulative earnings (Panel B). An increase of the fraction of peers in the middle or top quintiles increases earnings by a similar amount.

The nature of peer effects is different for participants in long training and retraining. Here, the average participant benefits similarly from an increase in the fraction of peers in the top quintile, independently of whether the fraction of peers in the middle or bottom quintiles are reduced in turn. Highly employable peers are thus the driver behind the positive effect. The effects on cumulative earnings are not statistically different from zero for these two types of programs but point to a similar relationship.

I ran the same type of analysis dividing the employability distribution into three equal parts (cutting at the 33th and 66th percentile). The results (see Appendix Table A.7) reveal the same effect patterns, indicating that a higher share of peers in the middle of the employability distribution has a positive effect on employment and earnings in short training, while the positive effects for long training and retraining are mostly driven by peers in the top group. Furthermore, the results suggest that participants at the extremes of the employability distribution generate larger peer effects. I find higher effects on employment and earnings from increasing the fraction of peers in the top quintile versus the top 33 percent.

Overall, these findings inform about which participants drive the peer effects. While it is mostly highly-employable peers at the top of the distribution in long training and retraining, it is peers from the top and the middle of the employability distribution in short training. There only a slight increase in the average peer employability can be very effective in increasing individual labour market prospects. These differences might be driven to some extent by the characteristics of participants in short training programs. Even though they are similar in their employability to participants in longer courses, they have the highest labour market attachment and shortest unemployment durations at program start. If this labour market attachment matters, not only peers at the top but also at the middle of the employability distribution might thus be able to generate positive peer effect through knowledge spillovers and networks.
Table 6: Non-linearity in Effects - Top versus Bottom Quintile

| Panel A - Total employment in month 60 | Short training (1) | Long training (2) | Retraining (3) |
|---------------------------------------|--------------------|-------------------|---------------|
| Fraction of peers in top quintile     | 5.906*             | 16.541***         | 13.443***     |
|                                       | (3.139)            | (5.008)           | (4.936)       |
| Fraction of peers in bottom quintile  | -16.524***         | -0.310            | 1.378         |
|                                       | (3.124)            | (5.124)           | (5.020)       |
| Difference top - bottom quintile      | 22.431***          | 16.851***         | 12.064*       |
|                                       | (3.965)            | (5.864)           | (6.209)       |

| Panel B - Log total earnings in month 60 | | | |
|-----------------------------------------| | | |
| Fraction of peers in top quintile       | 0.006              | 0.041             | -0.022        |
|                                        | (0.016)            | (0.028)           | (0.024)       |
| Fraction of peers in bottom quintile    | -0.078***          | -0.010            | 0.000         |
|                                        | (0.016)            | (0.029)           | (0.028)       |
| Difference top - bottom quintile        | 0.084***           | 0.051             | -0.022        |
|                                        | (0.021)            | (0.035)           | (0.033)       |

Provider-Cohort FEs ✓ ✓ ✓
Seasonal FEs ✓ ✓ ✓
Additional Controls ✓ ✓ ✓
Observations 28199 9598 8641

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 course level. Effects are reported in terms of 10 percentage point increases. Earnings are in prices of 2010 and measured in log(euro). * p < 0.05, ** p < 0.01, *** p < 0.001
Table 7: Group Heterogeneity

|                                | Short training | Long training | Retraining |
|--------------------------------|----------------|---------------|------------|
|                                | (1)            | (2)           | (3)        |
| **Panel A - Total employment in month 60** |                |               |            |
| Mean peer employability        | 16.121***      | 17.025***     | 13.463**   |
|                                | (3.638)        | (5.107)       | (5.362)    |
| SD in peer employability       | -1.800         | 10.663**      | 6.757      |
|                                | (3.403)        | (5.053)       | (5.218)    |
| **Panel B - Log total earnings in month 60** |                |               |            |
| Mean peer employability        | 0.054***       | 0.053*        | -0.012     |
|                                | (0.019)        | (0.031)       | (0.030)    |
| SD in peer employability       | -0.035*        | 0.020         | -0.015     |
|                                | (0.018)        | (0.028)       | (0.030)    |
| Provider-month group FEs       | ✓              | ✓             | ✓          |
| Seasonal FEs                   | ✓              | ✓             | ✓          |
| Additional controls            | ✓              | ✓             | ✓          |
| Observations                   | 28199          | 9598          | 8641       |

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 course level. Effects are reported in terms of standard deviation increases (see Table 2). Earnings are in prices of 2010 and measured in log(euro). * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
6.5 Group Heterogeneity

In another model variation I test whether more heterogeneity in terms of employability is beneficial. Specifically, I include the standard deviation of peer employability on top of the average peer employability. The results are shown in Table 7 for the long-run employment and earnings outcomes. They can be interpreted as the effect of a more dispersed distribution in peer employability that increases the share of participants at the extremes, while lowering the share in the middle of the distribution. The coefficients of the average peer employability do not change substantially. Overall, a higher dispersion in peer employability negatively affects employment and earnings of participants in short training and positively affects participants in long training. Nevertheless, the standard deviation terms are only significant for some of the outcomes. For short training, increasing the standard deviation in peer employability by one standard deviation while holding the average employability constant decreases the cumulative effect on earnings by 3.5 percent. For long training, I find a positive effect of 11 days when increasing the standard deviation in peer employability by one standard deviation, an effect that is again close to the effect of the average peer employability. The effects are insignificant and ambiguous for individuals in retaining.

When considering these results jointly with the results from the previous section, they suggest that peers at the middle of the employability distribution are important in driving peer effects in short training. If we put more weight to the extremes of the distribution in these types of programs, positive effects generated highly-employable peers do not outweigh negative effects generated by a larger share of low-employability peers. The contrary is the case for long-training. There, a higher share of peers at the top and bottom of the distribution coming from a more dispersed distribution is increasing the employment probability of single participants.

7 Conclusion

This paper investigates how labour market outcomes of participants in public sponsored training depend on the peer group composition as measured by the average 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 strong evidence that the peer composition matters and that peer effects differ between classic vocational training and retraining.

Participants in classic vocational training programs (short and long training) experience
positive, economically important and long-lasting effects on employment and earnings after they are exposed to a more employable peer group. Effects on employment are moderate, materialise around the average program duration and lead to a total effect on employment of 13 to 16 days five years after program start. At the same time, individuals in these programs benefit substantially in terms of earnings. These earnings increases are likely to be explained by participants being able to access better paid jobs as a response to the average quality in the group. I find slightly smaller and less robust peer effects on employment after program participation for participants in retraining. Furthermore, having a more employable peer group while in training negatively affects their earnings in the first job but has no effect on earnings in the longer run. There is substantial effect heterogeneity with respect to participants’ own employability, with low employability participants benefiting significantly more from a more employable peer group compared to highly-employable participants. Finally, peers of different employability levels drive the effects. In short training, it is peers of medium and high employability. Participants in long training and retraining are rather affected by peers at the top of the employability distribution.

My findings suggest that peer effects in labour market training and the mechanisms behind it might depend on the content, organization and objective of programs. Individuals in classic vocational training have better labour market attachment compared individuals in retraining and might directly benefit from existing networks of their peers. Furthermore, highly employable workers in these trainings are likely to hold valuable skills which they can share with workers of low employability during a course. In contrast, more employable participants in retraining are less likely to have a comparative advantage in skills and networks compared to their less employable peers. In these programs, everyone learns a new occupation and faces a new job search environment. At the same time, job seekers in a more employable peer group are more likely to face greater competition in their job search in these types of programs. They are likely to apply for the same types of jobs at the same time.

My results bear important insights for policy makers. They suggest that in the current program design jobseekers draw some benefits from being grouped with more employable peers, particularly in classic vocational training programs. I find some negative spillovers on earnings of participants in retraining but they do not persist for long. This is reassuring since the limited empirical evidence that exists on peer effects in the context of ALMP points to negative externalities (e.g. Van den Berg et al., 2019). Moreover, the effects give an idea of how programs could be redesigned to take advantage of the peer effects. This could involve relatively low-cost policy interventions that target a specific group composition through selection or regrouping of participants. What group composition is optimal depends on the programs and objectives being pursued by policy makers. If their
aim was to primarily increase employment opportunities for jobseekers with relatively poor employment prospects in classic vocational training, this could be achieved e.g. by evenly allocating highly-employable jobseekers to courses. In order to maximise benefits for the average jobseeker, the proportion of highly-employable jobseekers per course could be increased by enrolling more of these jobseekers in the program. Redistribution of participants in retraining programs does not seem advisable, since increasing the share of employable peers does not have a lasting effect in these programs. Finally, potential endogenous responses to a changing group composition should be considered when deriving concrete allocation policies. They are difficult to foresee but have shown to be important confounding factors (Carrell et al., 2013). Furthermore, an assessment would be needed whether the net effects of a targeted allocation are higher than those of a voucher system as 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

For our 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 in terms of their characteristics.

I select the most comparable sample of jobseekers who do not enter any program during their unemployment spell applying nearest neighbour matching based on the propensity score (See e.g. Huber et al. [2013], Abadie and Imbens [2016]). For every program participant, I choose 3 most comparable neighbours. The choice is based on the following reasoning: First, allowing for more than one nearest neighbour but limiting the total number of neighbours to a small number addresses the bias-variance trade-off in matching. It keeps the bias small while increasing the efficiency. Second, a higher number of matched individuals quickly increases the amount of computing time involved. Third, the higher the number of neighbours, the more often it happens that a single observation is used many times as a match (since we draw with replacement). I tested the sensitivity of my results to this matching step using only one nearest neighbour or five and do not find any large differences.\(^{35}\)

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]). I include demographic characteristics, information on education, training, characteristics of the last job, the recent labour market history, as well as variables characterizing the local labour market situation in the model. It is the same set of variables that is later used for the estimation of the employability score.

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 standardised bias (column 7). Even though there remain small differences in means for single variables, the standardised bias lies mostly below 25\%, which is the reference value given by Rubin (2001).

Appendix Figure A.1 shows the distribution of the predicted propensity scores for program

\(^{35}\text{Results available upon request.}\)
participants and matched non-participants. 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, I am 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.

For my main analysis, I perform the matching exercise jointly for all program types. To test whether the employability estimation sensitive to heterogeneity in predictors across program types, I run a version of the matching step separately by program types.
Figure A.1: Predicted P-score (Program Participants and Non-participants)

Notes: This figure displays the distribution of the predicted p-score for program participants (PP) and matched non-participants (NP). P-scores for matches are weighted with the inverse of the matching frequency.
| Month of entry into unemployment | PP  | NP  | Diff | pval | SB  |
|---------------------------------|-----|-----|------|------|-----|
| January                         | 0.13| 0.13| 0.46 | 0.61 |     |
| February                        | 0.08| 0.08| 0.02 | 1.7  |     |
| March                           | 0.09| 0.08| 0.01 | 0.12 | 2   |
| April                           | 0.08| 0.08| 0.87 | 0.13 |     |
| May                             | 0.07| 0.07| 0.28 | 0.87 |     |
| June                            | 0.07| 0.07| 0.72 | 0.28 |     |
| July                            | 0.08| 0.08| 0.83 | 0.16 |     |
| August                          | 0.08| 0.08| 0.49 | 0.69 |     |
| September                       | 0.09| 0.09| 0.54 | 0.95 |     |
| October                         | 0.09| 0.09| 0.38 | 1.37 |     |
| November                        | 0.07| 0.07| 0.55 | 0.59 |     |
| December                        | 0.07| 0.07| 0.55 | 0.11 |     |

| Year of entry into unemployment | PP  | NP  | Diff | pval | SB  |
|---------------------------------|-----|-----|------|------|-----|
| 2000                            | 0.01| 0.01| 0.14 | 1.24 |     |
| 2001                            | 0.01| 0.01| 0.2  | 0.79 |     |
| 2002                            | 0.01| 0.01| 0.31 | 0.59 |     |
| 2003                            | 0.01| 0.01| 0.8  | 0.14 |     |
| 2004                            | 0.02| 0.02| 0.89 | 0.08 |     |
| 2005                            | 0.03| 0.03| 0.39 | 0.51 |     |
| 2006                            | 0.06| 0.06| 0.45 | 0.55 |     |
| 2007                            | 0.15| 0.14| 0.01 | 1.96 |     |
| 2008                            | 0.22| 0.22| 0.31 | 1.34 |     |
| 2009                            | 0.26| 0.27| -0.01| 1.86 |     |
| 2010                            | 0.15| 0.14| 0    | 2.18 |     |
| 2011                            | 0.07| 0.07| 0.09 | 1.11 |     |

| Federal state                   | PP  | NP  | Diff | pval | SB  |
|---------------------------------|-----|-----|------|------|-----|
| Schleswig-Holstein              | 0.04| 0.04| 0.89 | 0.25 |     |
| Freie und Hansestadt Hamburg    | 0.04| 0.04| 0.53 | 1.39 |     |
| Niedersachsen                   | 0.09| 0.1 | 0.66 | 0.38 |     |
| Freie Hansestadt Bremen         | 0.02| 0.02| 0.74 | 0.46 |     |
| Nordrhein-Westfalen             | 0.23| 0.23| 0.12 | 1.38 |     |
| Hessen                          | 0.05| 0.05| 0.34 | 0.86 |     |
| Rheinland-Pfalz                 | 0.04| 0.04| 0.99 | 0.01 |     |
| Baden-Wuerttemberg              | 0.11| 0.11| 0.71 | 0.27 |     |
| Bayern                          | 0.16| 0.16| 0.54 | 0.47 |     |
| Saarland                        | 0.02| 0.02| 0.77 | 0.26 |     |
| Berlin                          | 0.02| 0.02| 0.5  | 0.85 |     |
| Brandenburg                     | 0.03| 0.03| 0.12 | 0.95 |     |
| Mecklenburg-Vorpommern          | 0.04| 0.05| 0.59 | 1.16 |     |
| Freistaat Sachsen               | 0.06| 0.06| 0.19 | 1    |     |
| Sachsen-Anhalt                  | 0.02| 0.02| 0.06 | 1.16 |     |
| Freistaat Thüringen             | 0.04| 0.04| 0.2  | 0.9  |     |

| Demographic characteristics     | PP  | NP  | Diff | pval | SB  |
|---------------------------------|-----|-----|------|------|-----|
| Younger than 25 years           | 0.15| 0.17| -0.02| 4.74 |     |
| 25-29 years                     | 0.17| 0.17| 0.46 | 0.88 |     |
| 30-34 years                     | 0.15| 0.14| 0.01 | 1.81 |     |
| 35-39 years                     | 0.14| 0.13| 0.41 | 1.08 |     |
| 40-44 years                     | 0.15| 0.14| 0.06 | 1.54 |     |
| 45-49 years                     | 0.13| 0.12| 0.42 | 0.67 |     |
| 50-54 years                     | 0.09| 0.09| 0.19 | 1.29 |     |
| Characteristics of the last job | 0.01 | 0.03 | -0.01 | 0 | 8.5 |
|---------------------------------|------|------|--------|---|-----|
| No last job                     | 0.09 | 0.09 | 0.73   | 0.32 |  |
| Unskilled/semiskilled tasks     | 0.66 | 0.66 | 0.05   | 0.18 | 1.02 |
| Skilled tasks                   | 0.04 | 0.04 | 0.07   | 0.01 | 1.02 |
| Complex tasks                   | 0.07 | 0.07 | 0.32   | 0.01 | 1.02 |
| Highly complex tasks            | 0.14 | 0.15 | 0.05   | 0.01 | 1.02 |
| Skills missing                  | 0.67 | 0.67 | 0.05   | 0.01 | 1.02 |
| Full-time                       | 0.31 | 0.31 | 0.05   | 0.01 | 1.02 |
| Part-time                       | 0.02 | 0.02 | 0.05   | 0.01 | 1.02 |
| Working time missing            | 0.02 | 0.02 | 0.05   | 0.01 | 1.02 |

**LM history relative to unemployment start**

| Regular EMP 3 months before     | 0.61 | 0.61 | -0.02 | 0 | 4.51 |
| Marginal EMP 3 months before    | 0.14 | 0.15 | -0.02 | 0 | 7.68 |
| Part-time EMP 3 months before   | 0.14 | 0.15 | -0.02 | 0 | 7.68 |
| In ALMP 3 months before         | 0.09 | 0.10 | -0.02 | 0 | 7.68 |
| OLF 3 months before             | 0.09 | 0.10 | -0.02 | 0 | 7.68 |
| Regular EMP 6 months before     | 0.61 | 0.61 | -0.02 | 0 | 4.51 |
| Marginal EMP 6 months before    | 0.14 | 0.15 | -0.02 | 0 | 7.68 |
| Part-time EMP 6 months before   | 0.14 | 0.15 | -0.02 | 0 | 7.68 |
| In ALMP 6 months before         | 0.09 | 0.10 | -0.02 | 0 | 7.68 |
| OLF 6 months before             | 0.09 | 0.10 | -0.02 | 0 | 7.68 |
| Regular EMP 9 months before     | 0.61 | 0.61 | -0.02 | 0 | 4.51 |
| Marginal EMP 9 months before    | 0.14 | 0.15 | -0.02 | 0 | 7.68 |
| Part-time EMP 9 months before   | 0.14 | 0.15 | -0.02 | 0 | 7.68 |
| In ALMP 9 months before         | 0.09 | 0.10 | -0.02 | 0 | 7.68 |
| OLF 9 months before             | 0.09 | 0.10 | -0.02 | 0 | 7.68 |
| Regular EMP 12 months before    | 0.61 | 0.61 | -0.02 | 0 | 4.51 |
| Marginal EMP 12 months before   | 0.14 | 0.15 | -0.02 | 0 | 7.68 |
| Part-time EMP 12 months before  | 0.14 | 0.15 | -0.02 | 0 | 7.68 |
| In ALMP 12 months before        | 0.09 | 0.10 | -0.02 | 0 | 7.68 |
| OLF 12 months before            | 0.09 | 0.10 | -0.02 | 0 | 7.68 |
| Regular EMP 18 months before    | 0.61 | 0.61 | -0.02 | 0 | 4.51 |
| Marginal EMP 18 months before   | 0.14 | 0.15 | -0.02 | 0 | 7.68 |
| Part-time EMP 18 months before  | 0.14 | 0.15 | -0.02 | 0 | 7.68 |
| In ALMP 18 months before        | 0.09 | 0.10 | -0.02 | 0 | 7.68 |
| Variable                                      | PP 18 months before | PP 24 months before | WN 24 months before | PN 24 months before | PP 36 months before | PP 60 months before | PP 120 months before |
|-----------------------------------------------|---------------------|---------------------|---------------------|--------------------|--------------------|--------------------|----------------------|
| OLF                                           | 0.05                | 0.06                | 0                   | 0.06               | 1.41               |                    |                      |
| Regular EMP                                   | 0.52                | 0.54                | -0.02               | 0                  | 3.17               |                    |                      |
| Marginal EMP                                  | 0.12                | 0.11                | 0.02                | 0.2                | 4.91               |                    |                      |
| Part-time EMP                                 | 0.08                | 0.09                | 0                   | 0.35               | 0.74               |                    |                      |
| In ALMP                                        | 0.09                | 0.1                 | -0.01               | 0.19               | 2.6                |                    |                      |
| OLF 24 months before                          | 0.06                | 0.07                | -0.01               | 0.01               | 2.47               |                    |                      |
| Regular EMP 36 months before                  | 0.47                | 0.48                | -0.01               | 0.04               | 2.14               |                    |                      |
| Marginal EMP 36 months before                 | 0.12                | 0.11                | 0.02                | 0.02               | 3.29               |                    |                      |
| Part-time EMP 36 months before                | 0.07                | 0.08                | 0                   | 0.23               | 0.95               |                    |                      |
| In ALMP 36 months before                      | 0.09                | 0.09                | 0                   | 0.68               | 0.45               |                    |                      |
| OLF 36 months before                          | 0.07                | 0.08                | 0                   | 0.06               | 1.67               |                    |                      |
| Total EMP 6 months before                     | 132.24              | 132.12              | 0.11                | 0.88               | 0.17               |                    |                      |
| Total ALO 6 months before                     | 38.91               | 34.81               | 4.1                 | 0                  | 6.84               |                    |                      |
| Total ALMP 6 months before                    | 15.46               | 18.28               | -2.82               | 0                  | 6.29               |                    |                      |
| Total EMP 12 months before                    | 261.51              | 261.72              | -0.21               | 0.88               | 0.17               |                    |                      |
| Total ALO 12 months before                    | 77.86               | 68.49               | 9.37                | 0                  | 8.78               |                    |                      |
| Total ALMP 12 months before                   | 31.12               | 35.98               | -4.87               | 0.01               | 5.96               |                    |                      |
| Total EMP 24 months before                    | 508.95              | 509.53              | -0.58               | 0.84               | 0.26               |                    |                      |
| Total ALO 24 months before                    | 156.71              | 137.98              | 18.73               | 0                  | 10.22              |                    |                      |
| Total ALMP 24 months before                   | 62.58               | 70.4                | -7.81               | 0.02               | 5.55               |                    |                      |
| Total EMP 60 months before                    | 1168.88             | 1166.9              | 1.98                | 0.79               | 0.37               |                    |                      |
| Total ALO 60 months before                    | 417.37              | 379.1               | 38.27               | 0                  | 9.26               |                    |                      |
| Total ALMP 60 months before                   | 158.16              | 168.84              | -10.68              | 0.03               | 3.97               |                    |                      |
| Total EMP 120                                 | 2107.71             | 2099.79             | 7.92                | 0.49               | 0.78               |                    |                      |
| Total ALO 120                                 | 738.81              | 669.49              | 69.32               | 0                  | 9.6                |                    |                      |
| Total ALMP 120                                | 272.27              | 281.37              | -9.1                | 0.14               | 2.26               |                    |                      |
| Number of ALMP 24 months before               | 0.62                | 0.64                | -0.03               | 0.21               | 2.88               |                    |                      |
| Number of ALMP 60 months before               | 1.26                | 1.27                | -0.01               | 0.74               | 0.55               |                    |                      |
| Log. total earnings 12 months before          | 8.46                | 8.42                | 0.05                | 0.22               | 1.76               |                    |                      |
| Log. total benefits 12 months before          | 1.86                | 1.79                | 0.07                | 0.02               | 2.08               |                    |                      |
| Log. total earnings 24 months before          | 9.32                | 9.24                | 0.08                | 0.04               | 3.4                |                    |                      |
| Log. total benefits 24 months before          | 2.67                | 2.58                | 0.08                | 0.02               | 2.21               |                    |                      |
| Log. total earnings 48 months before          | 10.08               | 9.97                | 0.11                | 0.02               | 4.55               |                    |                      |
| Log. total benefits 48 months before          | 3.92                | 3.8                 | 0.12                | 0                  | 3.04               |                    |                      |
| Log. total earnings 60 months before          | 10.31               | 10.19               | 0.12                | 0.01               | 5.16               |                    |                      |
| Log. total benefits 60 months before          | 4.43                | 4.35                | 0.08                | 0.06               | 1.94               |                    |                      |
| Log. total earnings 120 months before         | 10.99               | 10.84               | 0.15                | 0                  | 6.79               |                    |                      |
| Log. total benefits 120 months before         | 5.91                | 5.68                | 0.22                | 0                  | 5.42               |                    |                      |

**Regional characteristics (at district level)**

| Local unemployment rate                      | 8.57                | 8.47                | 0.09                | 0                  | 2.64               |                    |                      |
| Population density                           | 776.83              | 765.49              | 11.34               | 0.27               | 1.29               |                    |                      |
| Employment share primary sector              | 1.86                | 1.91                | -0.05               | 0.02               | 2.58               |                    |                      |
| Employment share secondary sector            | 26.17               | 26.31               | -0.14               | 0.12               | 1.61               |                    |                      |
| Employment share third sector                | 71.97               | 71.78               | 0.19                | 0.05               | 1.97               |                    |                      |
| GDP per capita                               | 30.68               | 30.64               | 0.04                | 0.76               | 0.34               |                    |                      |
| No regional information                      | 0.08                | 0.09                | 0                   | 0.34               | 1.53               |                    |                      |

**Observations**

47279 96809

**Notes:** This table displays the average characteristics of the sample of program participants (PP) and the weighted 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 $SB_k = \frac{|E_{PP}(X_k) - E_{NP}(X_k)|}{\sqrt{(Var_{PP}(X_k) + Var_{NP}(X_k))/2}} \times 100$.  

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A.1.2 The Employability Score and Group Size

Table A.2: Predictors of the Probability to be Employed 12 Months after UE Entry

| Demographics                  | Coefficient (1) | SE (2)   |
|-------------------------------|-----------------|----------|
| Younger 25 years              | -0.055          | (0.067)  |
| 30-34 years                   | -0.031          | (0.062)  |
| 35-39 years                   | -0.014          | (0.060)  |
| 40-44 years                   | -0.040          | (0.059)  |
| 45-49 years                   | -0.082          | (0.061)  |
| 50-54 years                   | -0.139          | (0.073)  |
| Older 54 years                | -0.691***       | (0.091)  |
| Female                        | 0.505           | (0.336)  |
| Non-German                    | 0.035           | (0.053)  |
| Married                       | 0.078*          | (0.034)  |
| Missing                       | 0.087*          | (0.043)  |
| Children < 15                 | 0.176***        | (0.052)  |
| Children < 3                  | -0.099          | (0.062)  |
| Restrictions or disability    | -0.388***       | (0.041)  |
| No educational degree         | -0.156*         | (0.069)  |
| High school degree (Abitur)   | -0.080          | (0.056)  |
| Educational degree missing    | -0.281**        | (0.107)  |
| Without Vocational Training   | -0.170***       | (0.043)  |
| Academic Degree               | 0.101           | (0.091)  |
| Vocational degree missing     | -0.135          | (0.083)  |
| Job searched: Part-time       | -0.046          | (0.058)  |
| Job searched: Missing         | -0.072          | (0.039)  |
| Job returner                  | 0.222           | (0.117)  |
| Welfare benefits              | -0.028          | (0.130)  |

Month and year of entry into unemployment

| Month       | Coefficient (1) | SE (2)   |
|------------|-----------------|----------|
| Feb        | -0.098          | (0.070)  |
| Mar        | -0.336***       | (0.085)  |
| Apr        | -0.355***       | (0.073)  |
| May        | -0.328***       | (0.083)  |
| Jun        | -0.174*         | (0.082)  |
| Jul        | -0.380***       | (0.070)  |
| Aug        | -0.376***       | (0.086)  |
| Sep        | -0.391***       | (0.085)  |
| Oct        | -0.244**        | (0.084)  |
| Nov        | -0.203*         | (0.082)  |
| Dec        | 0.057           | (0.068)  |
| 2000       | -0.016          | (0.095)  |
| 2001       | -0.208          | (0.110)  |
| 2002       | -0.349***       | (0.092)  |
| 2003       | -0.345***       | (0.087)  |
| 2004       | -0.353***       | (0.101)  |
| 2005       | -0.141          | (0.087)  |
| 2006       | -0.022          | (0.072)  |
| 2007       | 0.141*          | (0.060)  |
| 2008       | -0.196***       | (0.053)  |
| 2010       | 0.227***        | (0.053)  |
| 2011       | 0.148**         | (0.055)  |
### Residence - Federal state

| Region                          | Logit   | Std. Error |
|---------------------------------|---------|------------|
| Schleswig-Holstein              | -0.059  | (0.118)    |
| Freie und Hansestadt Hamburg    | 0.088   | (0.087)    |
| Niedersachsen                   | 0.115   | (0.066)    |
| Freie Hansestadt Bremen         | 0.023   | (0.264)    |
| Hessen                          | 0.230** | (0.085)    |
| Rheinland-Pfalz                 | 0.106   | (0.071)    |
| Baden-Wuerttemberg              | 0.228***| (0.062)    |
| Freistaat Bayern                | 0.177** | (0.059)    |
| Saarland                        | 0.197   | (0.126)    |
| Berlin                          | 0.088   | (0.093)    |
| Brandenburg                     | 0.146   | (0.097)    |
| Mecklenburg-Vorpommern          | 0.073   | (0.126)    |
| Freistaat Sachsen               | 0.196*  | (0.096)    |
| Sachsen-Anhalt                  | 0.045   | (0.096)    |
| Freistaat Thüringen             | 0.248** | (0.083)    |
| Missing                         | -0.276  | (0.772)    |

### Characteristics of the last job

| Feature                                         | Logit   | Std. Error |
|-------------------------------------------------|---------|------------|
| No last job                                     | -0.022  | (0.404)    |
| Unskilled/semiskilled tasks                     | -0.026  | (0.073)    |
| Complex tasks                                   | -0.034  | (0.082)    |
| Highly complex tasks                            | -0.045  | (0.094)    |
| Skill level missing                             | 0.055   | (0.060)    |
| Part-time                                       | 0.025   | (0.067)    |
| Time missing                                    | -0.070  | (0.347)    |
| Log earnings in last job                        | 0.082***| (0.016)    |
| Agriculture, forestry, farming, and gardening    | 0.254** | (0.090)    |
| Production of raw materials and goods,           | -0.044  | (0.046)    |
| Construction, architecture and building services | 0.084   | (0.048)    |
| Natural sciences, geography and informatics     | -0.142  | (0.117)    |
| Commercial services, trading, sales, tourism    | 0.036   | (0.062)    |
| Business organisation, accounting, law and admin.| -0.044  | (0.082)    |
| Health care, the social sector, teaching and education | 0.043 | (0.105)    |
| Philology, literature, humanities, social sciences, media, art | -0.503***| (0.093)    |

### LM history

| Period                           | Logit   | Std. Error |
|----------------------------------|---------|------------|
| Regular EMP 3 months before UE entry | 0.063  | (0.055)    |
| Marginal EMP 3 months before UE entry | 0.367***| (0.074)    |
| Part-time EMP 3 months before UE entry | -0.033 | (0.076)    |
| In ALMP 3 months before UE entry  | -0.042  | (0.160)    |
| OLF 3 months before UE entry     | -0.172**| (0.063)    |
| Regular EMP 6 months before UE entry | 0.056  | (0.051)    |
| Marginal EMP 6 months before UE entry | 0.081  | (0.084)    |
| Part-time EMP 6 months before UE entry | -0.263**| (0.084)    |
| In ALMP 6 months before UE entry  | 0.046   | (0.120)    |
| OLF 6 months before UE entry     | -0.215***| (0.060)    |
| Regular EMP 9 months before UE entry | -0.070 | (0.058)    |
| Marginal EMP 9 months before UE entry | 0.142  | (0.090)    |
| Part-time EMP 9 months before UE entry | 0.134  | (0.081)    |
| In ALMP 9 months before UE entry  | -0.050  | (0.164)    |
| OLF 9 months before UE entry     | -0.096  | (0.062)    |
| Regular EMP 12 months before UE entry      | -0.095*| (0.043)    |
| Marginal EMP 12 months before UE entry    | 0.134   | (0.074)    |
| Part-time EMP 12 months before UE entry   | 0.020   | (0.074)    |
| In ALMP 12 months before UE entry        | 0.218   | (0.122)    |
| OLF 12 months before UE entry            | -0.205**| (0.066)    |
| Regular EMP 18 months before UE entry     | 0.219***| (0.044)    |
| Characteristic                                      | Coefficient | Standard Error |
|---------------------------------------------------|-------------|----------------|
| Marginal EMP 18 months before UE entry            | 0.299***    | (0.067)        |
| Part-time EMP 18 months before UE entry           | -0.079      | (0.065)        |
| In ALMP 18 months before UE entry                 | 0.323**     | (0.109)        |
| OLF 18 months before UE entry                     | -0.064      | (0.054)        |
| Regular EMP 24 months before UE entry             | -0.018      | (0.038)        |
| Marginal EMP 24 months before UE entry            | 0.133*      | (0.056)        |
| Part-time EMP 24 months before UE entry           | -0.135*     | (0.063)        |
| In ALMP 24 months before UE entry                 | -0.146      | (0.086)        |
| OLF 24 months before UE entry                     | 0.022       | (0.055)        |
| Regular EMP 36 months before UE entry             | -0.040      | (0.036)        |
| Marginal EMP 36 months before UE entry            | 0.099*      | (0.049)        |
| Part-time EMP 36 months before UE entry           | 0.026       | (0.054)        |
| In ALMP 36 months before UE entry                 | -0.018      | (0.074)        |
| OLF 36 months before UE entry                     | -0.093      | (0.050)        |
| Total EMPL 6 months before UE entry (days)        | 0.004***    | (0.001)        |
| Total UE 6 months before UE entry (days)          | 0.001       | (0.001)        |
| Total ALMP 6 months before UE entry (days)        | 0.001       | (0.002)        |
| Total EMPL 12 months before UE entry (days)       | -0.000      | (0.001)        |
| Total UE 12 months before UE entry (days)         | 0.000       | (0.001)        |
| Total ALMP 12 months before UE entry (days)       | 0.000       | (0.001)        |
| Total EMPL 24 months before UE entry (days)       | -0.000      | (0.000)        |
| Total UE 24 months before UE entry (days)         | 0.000       | (0.000)        |
| Total ALMP 24 months before UE entry (days)       | -0.001      | (0.001)        |
| Total EMPL 60 months before UE entry (days)       | 0.000**     | (0.000)        |
| Total UE 60 months before UE entry (days)         | 0.000       | (0.000)        |
| Total ALMP 60 months before UE entry (days)       | 0.000       | (0.000)        |
| Total EMPL 120 months before UE entry (days)      | 0.000***    | (0.000)        |
| Total UE 120 months before UE entry (days)        | -0.000      | (0.000)        |
| Total ALMP 120 months before UE entry (days)      | -0.000      | (0.000)        |
| No of ALMP 24 months before UE entry              | 0.030       | (0.036)        |
| No of ALMP 60 months before UE entry              | -0.017      | (0.020)        |
| Log total earnings 12 months before UE entry      | 0.037**     | (0.012)        |
| Log total benefits 12 months before UE entry      | -0.010      | (0.007)        |
| Log total earnings 24 months before UE entry      | -0.028*     | (0.013)        |
| Log total benefits 24 months before UE entry      | -0.004      | (0.007)        |
| Log total earnings 48 months before UE entry      | 0.041       | (0.023)        |
| Log total benefits 48 months before UE entry      | -0.020      | (0.011)        |
| Log total earnings 60 months before UE entry      | 0.032       | (0.036)        |
| Log total benefits 60 months before UE entry      | 0.036**     | (0.011)        |
| Log total earnings 120 months before UE entry     | -0.066      | (0.034)        |
| Log total benefits 120 months before UE entry     | 0.011       | (0.006)        |

Notes: This table displays the coefficients and corresponding standard errors (SE) of a logit regression of the employability measure on individual characteristics measured at the beginning of the unemployment spell in the sample of non-participants. Coefficients based on interaction terms and for regional labour market (LM) characteristics are omitted in the table.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Figure A.2: Distribution of the Individual Predicted Employability by Program Type

Notes: This figure plots the distribution of the predicted individual employability for the sample of program participants in short training, long training and retraining. It is based on a logit model that is jointly estimated for all program types.

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

Notes: This figure plots the distribution of the predicted average employability for the sample of program participants in short training, long training and retraining. It is based on a logit model that is jointly estimated for all program types.
Figure A.4: Distribution of the Predicted Individual Employability (Joint vs. Separate Estimation)

Notes: These figures plot the distribution of the predicted individual employability for the sample of program participants based on a model that is estimated jointly (red) or separately (blue) by program type. The upper left graph shows the distributions for participants in short training (ST), the upper right graph shows the distributions for participants in long training (LT) and the lower left graph shows the distributions for participants in retraining (RT).
Figure A.5: Distribution of the Predicted Average Employability (Joint vs. Separate Estimation)

Notes: These figures plot the distribution of the predicted average employability for the sample of program participants based on a model that is estimated jointly (red) or separately (blue) by program type. The upper left graph shows the distributions for participants in short training (ST), the upper right graph shows the distributions for participants in long training (LT) and the lower left graph shows the distributions for participants in retraining (RT).
Figure A.6: Individual Predicted Employability (Program Participants and Non-participants)

Notes: This figure plots the distribution of the predicted individual employability for the sample of program participants (PP, blue) and the sample of non-participants (NP, red). The distribution is based on a logit model and estimated jointly for all program types. The means and standard deviations (SD) are displayed below the graph.

Figure A.7: Quintiles of the Individual Employability Distribution

Notes: This figure plots the distribution of the predicted individual employability for the sample of program participants estimated jointly by a logit model. The red lines and corresponding labels mark the thresholds of the second to fifth quintiles.
A.2 Discussion of the Identifying Assumption and Validity Checks

For identification, I need sufficient residual variation in the main peer variables and this variation needs to be exogenous.

For an illustration, consider the error term $\epsilon_{ipct} = \rho_{pct} + \eta_{ipct}$ in equation (2) where $\rho_{pct}$ is a course-specific and $\eta_{ipct}$ a zero-mean random component. For the identifying assumption to hold $\rho_{pct}$ needs to be uncorrelated with all other regressors. That is, there can be no correlated unobservables that vary within month groups and providers. Furthermore, there should be no endogenous sorting into peer groups such that jobseekers 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 are very difficult to predict for jobseekers.

A.2.1 Sorting Over Time and Across Occupations

I thus test for sorting i) over time and ii) across occupations by comparing the distribution of individual employability across different course months and target occupations. Figure A.9 plots the employability distribution across different months of unemployment entry (a) and across months of course start (b). With regard to unemployment entry, there is some sorting of individuals with higher employability into months at the end and at the beginning of the calendar year. Nevertheless, there is no such sorting visible when
comparing months of course start. This provides some evidence that the timing of the course start is not directly linked to the timing of unemployment entry. Comparing courses that start in different calendar months will thus not exploit variations in course the composition resulting from higher employable jobseekers sorting into specific course months.

I repeat the same exercise in Figure A.9 (c) and compare the distribution of individual employability across different areas of target occupations (so called occupational areas according to the German classification of occupations, KldB 2010). Note that jobseekers are not as evenly distributed across these occupations as compared to course months. While courses with target occupations in health care, the social sector or teaching comprise 23 percent of individuals, courses targeting agriculture comprise only 0.17 percent. Moreover, we do not have information for the target occupation for around 10 percent of individuals. Overall, the employability distributions overlap to a great extent. Nevertheless, there are some notable differences. In particular, jobseekers for whom the target occupation is missing are on average less employable compared to jobseekers in known occupations. In Section 5.2, I argue that controlling for provider choice will mostly take care of a possible selection into courses targeting certain occupations. Additionally controlling for the target occupation will take care of a possible selection bias with regard to occupations that is constant over time and across providers.

A.2.2 Resampling Test

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-month group 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 0.042 to 0.046 (Table A.3). Whereas the simulated residual variation is very close to the 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.

A.2.3 Systematic Correlation of Own and Peer Employability

I test whether the residual variation in peer employability is random by regressing the individual employability on the average leave-one-out peer employability and control for the variables on which randomization was conditioned (course level characteristics including occupation and skill level fixed effects, provider-course month effects, time fixed effects). The argument in this classical test for random peer assignment is that if selection into
Figure A.9: Sorting in Individual Employability Across Time and Occupation

(a) Month of entry into unemployment

(b) Month of course start

(c) Target occupation

Notes: This figure plots the distribution of the predicted individual employability (kernel density) based on a logit model by month of entry into unemployment (a), month of entry into the program (b) and target occupation (c).
Table A.3: Observed and Resampled Standard Deviation

|               | (1)  | (2)  | (3)  |
|---------------|------|------|------|
|               | ST   | LT   | RT   |
| Observed SD (raw) | 0.081| 0.081| 0.086|
| Observed SD (net)  | 0.049| 0.046| 0.047|
| Simulated SD raw  | 0.075| 0.077| 0.079|
| Simulated SD net  | 0.042| 0.044| 0.046|
| Simulations       | 500  | 500  | 500  |

Notes: This table plots the standard deviation (SD) of the observed peer employability and the average standard deviation of the simulated peer employability measure (over 500 simulations), raw and net of fixed effects and course controls. The figures are displayed separately by program type: short training (ST), long training (LT), retraining (RT).

Peer groups is ignorable and assignment of peers is quasi-random, then this regression should yield a coefficient of zero. There is a mechanical relationship between the individual and the mean peer employability which stems from the fact that each individual’s peers are drawn from a population with a different mean employability. Following Guryan et al. (2009) I control for that mean, by including the average peer employability at the provider level. Table A.4 shows that there is no significant correlation of the average peer employability and the individual’s own employability.

Table A.4: Exogeneity Test Controlling for the Average Employability at Provider Level

|                          | (1)          | (2)           | (3)           |
|--------------------------|--------------|---------------|---------------|
|                          | Short Training | Long Training | Retraining    |
| Mean peer employability  | 0.001        | -0.002        | 0.001         |
|                          | (0.001)      | (0.001)       | (0.001)       |
| Mean peer employability (provider) | -1.128***     | -0.903***     | -0.863***     |
|                          | (0.023)      | (0.023)       | (0.019)       |
| Provider-Cohort FEs      | ✓            | ✓             | ✓             |
| Seasonal FEs             | ✓            | ✓             | ✓             |
| Additional Controls      | ✓            | ✓             | ✓             |
| Observations             | 28199        | 9598          | 8641          |

Notes: This table displays the coefficients of a regression of the individual employability on the average employability in the course and at the provider. Standard errors are clustered at the course level. * p < 0.05, ** p < 0.01, *** p < 0.001
A.2.4 Measurement Error through Unobserved Peers

There is a possibility that I 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. 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. 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 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. 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 $\epsilon_{ipct}$ after controlling for fixed effects and course controls [Ammermueller and Pischke 2009] [Sojourner 2013].
A.3 Further Results

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

Notes: The figure depicts the estimated effects (in percentage points/100) of a one standard deviation increase in the predicted 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, occupation and provider-by-month group and seasonal fixed effects. Standard errors are clustered at the course level.
Table A.5: Effects of Peer Employability (Excluding Courses where Participants Worked at the Same Firm)

|                      | Short training | Long training | Retraining |
|----------------------|----------------|---------------|------------|
|                      | (1)            | (2)           | (3)        |
| **Panel A - Search duration first job (in days)** |                |               |            |
| Mean peer employability | -11.887**      | -1.462        | -3.745     |
|                      | (5.337)        | (7.544)       | (7.747)    |
| Own employability    | -99.164***     | -85.589***    | -90.683*** |
|                      | (5.916)        | (9.675)       | (9.111)    |
| **Panel B - Total employment in month 60** |                |               |            |
| Mean peer employability | 14.973***      | 12.402**      | 7.266      |
|                      | (3.347)        | (4.884)       | (4.674)    |
| Own employability    | 125.608***     | 120.340***    | 110.477*** |
|                      | (3.659)        | (5.532)       | (5.381)    |
| **Panel C - Log earnings first job** |                |               |            |
| Mean peer employability | 0.036***       | 0.010         | -0.029**   |
|                      | (0.008)        | (0.014)       | (0.014)    |
| Own employability    | 0.088***       | 0.047***      | 0.043**    |
|                      | (0.009)        | (0.016)       | (0.017)    |
| **Panel D - Log total earnings 60 months after prg. start** |                |               |            |
| Mean peer employability | 0.067***       | 0.052*        | -0.006     |
|                      | (0.018)        | (0.029)       | (0.028)    |
| Own employability    | 0.461***       | 0.430***      | 0.404***   |
|                      | (0.021)        | (0.034)       | (0.036)    |
| Provider-Cohort FE$\checkmark$ | ✓              | ✓             | ✓          |
| Seasonal FE$\checkmark$ | ✓              | ✓             | ✓          |
| Additional Controls  | ✓              | ✓             | ✓          |
| Observations         | 26810          | 9215          | 8178       |

**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 course level. Effects are reported in terms of standard deviation increases (see Table 2). $^* p < 0.05, ^{**} p < 0.01, ^{***} p < 0.001$
Table A.6: Heterogeneity in Effects of Peer Employability Depending on Gender

|                        | Short training | Long training | Retraining |
|------------------------|----------------|---------------|------------|
|                        | (1)            | (2)           | (3)        |

**Panel A - Total employment in month 24**

| Gender   | Short training | Long training | Retraining |
|----------|----------------|---------------|------------|
| PE males | 5.603***       | 7.528***      | 4.090      |
|          | (1.631)        | (2.470)       | (2.783)    |
| PE females | 6.461***     | 7.614***      | 3.392      |
|           | (1.728)        | (2.821)       | (3.075)    |
| P-value difference | 0.654         | 0.978         | 0.825      |

**Panel B - Total employment in month 60**

| Gender   | Short training | Long training | Retraining |
|----------|----------------|---------------|------------|
| PE males | 15.510***      | 11.737**      | 12.727**   |
|          | (3.896)        | (5.678)       | (5.678)    |
| PE females | 16.526***   | 13.050**      | 7.350      |
|           | (4.135)        | (6.547)       | (5.921)    |
| P-value difference | 0.826         | 0.861         | 0.418      |

**Panel C - Log total earnings in month 60**

| Gender   | Short training | Long training | Retraining |
|----------|----------------|---------------|------------|
| PE males | 0.061***       | 0.013         | 0.016      |
|          | (0.021)        | (0.034)       | (0.034)    |
| PE females | 0.075***   | 0.097**       | -0.029     |
|           | (0.022)        | (0.042)       | (0.032)    |
| P-value difference | 0.592         | 0.082         | 0.273      |

Provider-Cohort FEs: ✓ ✓ ✓
Seasonal FEs: ✓ ✓ ✓
Additional Controls: ✓ ✓ ✓
Observation: 28199 9598 8641

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 course level. Effects are reported in terms of standard deviation increases (see Table 2). * p < 0.05, ** p < 0.01, *** p < 0.001
Table A.7: Non-Linearity in Effects - Upper versus Lower Third

|                           | Short training | Long training | Retraining |
|---------------------------|----------------|---------------|------------|
|                           | (1)            | (2)           | (3)        |

Panel A - Total employment in month 60

|                                                        |                  |              |            |
|--------------------------------------------------------|------------------|--------------|------------|
| Fraction highly-employable peers                       | 6.051*           | 6.262        | 10.622**   |
|                                                        | (3.173)          | (4.585)      | (4.562)    |
| Fraction low employable peers                          | -9.807***        | -6.308       | -3.246     |
|                                                        | (2.946)          | (5.082)      | (4.491)    |
| Difference high-low                                     | 15.857***        | 12.570***    | 13.867***  |
|                                                        | (3.005)          | (4.846)      | (4.757)    |

Panel B - Log total earnings in month 60

|                                                        |                  |              |            |
|--------------------------------------------------------|------------------|--------------|------------|
| Fraction highly-employable peers                       | 0.020            | 0.015        | 0.015      |
|                                                        | (0.017)          | (0.026)      | (0.024)    |
| Fraction low employable peers                          | -0.044***        | -0.029       | 0.003      |
|                                                        | (0.016)          | (0.026)      | (0.025)    |
| Difference high-low                                     | 0.064***         | 0.043        | 0.011      |
|                                                        | (0.016)          | (0.029)      | (0.024)    |

Provider-Cohort FEs                                      | ✓                | ✓             | ✓           |
Seasonal FEs                                             | ✓                | ✓             | ✓           |
Additional Controls                                      | ✓                | ✓             | ✓           |
Observations                                             | 28199            | 9598         | 8641        |

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 course level. Effects are reported in terms of 10 percentage point increases. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
A.3.1 Potential Drivers of Peer Effects in Employability

I investigate potential drivers of the effects in peer employability by including readily available peer characteristics in model (2) which do not rely on a first-step prediction. I test three different models. The first includes the average number of months peers have been employed in the past two years before program start. The second the average number of months peers have been employed in the past 10 years before program start. The third includes the average peer earnings in the two years before program start. All models control for a set of individual level characteristics such as well as the set of fixed effects and course controls used in the main model. The effects of the three peer variables on cumulative employment and earnings are displayed in A.8 as separate rows for the three program types short training (ST), long training (LT) and retraining (RT).

I find that the recent labour market attachment of the peer group is positively associated with individual post-program employment in all three program types. The effect sizes are moderate. Increasing the average number of months in peer employment in the two years before program start by one, increases employment up to five years after program start by up to 9 days. The effect is strongest for individuals in short training and retraining. A one month increase in the average months in peer employment 10 years prior to program start also results in a positive effect on individual employment by one to two days. For the interpretation of the effect sizes, it is important to keep in mind that a one month increase in employment over 10 years represents a smaller increase compared to a one month increase over two years. An increase in the average peer earnings (in the past two years) by 1000 euro results in a longer employment duration of two to three days. We also find some positive effects on cumulative earnings, in particular for individuals in short training.

Overall, the results suggest that individuals seem to benefit from peers with successful labour market histories. This is in line with our findings from the main analysis and suggests that our measure of peer employability is a proxy for the labour market attachment of peers.
Table A.8: The Effects 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) | 2.12*** | 1.70* | 3.74*** | 0.01*** | 0.00 | 0.01** |
|                                | (0.46) | (0.77) | (1.03) | (0.00) | (0.00) | (0.00) |
| Peer months of employment in last 2 years | 9.22*** | 5.20** | 8.14*** | 0.04*** | 0.01 | 0.01 |
|                                | (1.12) | (1.90) | (2.14) | (0.01) | (0.01) | (0.01) |
| Peer months of employment in last 10 years | 0.85** | 1.44*** | 1.39** | 0.00* | 0.00 | 0.00 |
|                                | (0.28) | (0.43) | (0.47) | (0.00) | (0.00) | (0.00) |
| Individual-level controls      | Yes | Yes | Yes | Yes | Yes | Yes |
| Provider-Cohort FEs            | Yes | Yes | Yes | Yes | Yes | Yes |
| Seasonal FEs                   | Yes | Yes | Yes | Yes | Yes | Yes |
| Additional Controls            | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations                   | 28199 | 9598 | 8641 | 28199 | 9598 | 8641 |

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 labour market attachment. Standard errors are clustered at the course level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Table A.9: Effects of Peer Employability on Firm Choice

|                     | Short training | Long training | Retraining |
|---------------------|----------------|---------------|------------|
| **Panel A**         |                |               |            |
| Mean peer employability | 0.001          | -0.001        | 0.003***   |
| (0.001)             | (0.001)        | (0.001)       |            |
| Own employability   | -0.000         | -0.000        | 0.000      |
| (0.001)             | (0.001)        | (0.001)       |            |
| **Panel B**         |                |               |            |
| Mean peer employability | 0.000          | 0.000         | 0.000      |
| (0.001)             | (0.001)        | (0.001)       |            |
| Own employability   | 0.000          | 0.000         | 0.001      |
| (0.001)             | (0.001)        | (0.001)       |            |
| Provider-Cohort FEs | ✓              | ✓             | ✓          |
| Seasonal FEs        | ✓              | ✓             | ✓          |
| Additional Controls | ✓              | ✓             | ✓          |
| Observations        | 28199          | 9598          | 8641       |

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 course level. Effects are reported in terms of standard deviation increases (see Table 2).

∗ p < 0.05, ** p < 0.01, *** p < 0.001

A.4 Mechanisms

I perform two types of checks informing about potential mechanisms behind peer effects in training programs. First, I investigate whether jobseekers in more employable peer groups have a higher probability of starting to work at the same firm after program start. Second, I test whether they have a higher probability of starting to work in a firm where any of their peers worked in their last job (prior to entering unemployment). Both variables are set to zero for individuals not entering a first job up until the end of our observation period. As can be seen from Table 2, both of these events are rare and only happen for one percent of participants. In fact, I do not find a significant effect of an increase in the average peer employability on any of the two outcomes, with the exception for participants in retraining (See Appendix Table A.9). Their probability of starting to work in the same firm as any of their peers is increasing by 0.3 percentage points for a standard deviation increase in the average peer employability.