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Labour market effects of work-related continuous education in Switzerland – evidence from administrative data

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ABSTRACT

This paper presents the first longitudinal estimates of the effect of work-related training on labour market outcomes in Switzerland. Using a novel dataset that links official census data on adult education to longitudinal register data on labour market outcomes, we apply a regression-adjusted matched difference-in-differences approach with entropy balancing to account for selection bias and sorting. We find that training participation increases yearly earnings and reduces the risk of unemployment two and three years after the treatment. The effects are heterogeneous as to age, education, and income position, whereby people in the lowest income tercile benefit most from income increases, while the dampening effect on unemployment is more pronounced for those in the highest income tercile.

1. Introduction

Adult education has become a crucial factor for aging economies to maintain and improve workers' skills and knowledge and to prevent human capital depreciation. Thus, participation in lifelong learning activities has become widespread in many OECD countries. On average, 40 percent of the 25 to 64 year olds participate in non-formal education activities (OECD, 2017). While there is an ongoing interest in and a relatively large literature on the effects of continuing education on labour market outcomes, the evidence is far from being complete. For example, in a recent survey of the literature Midtsundstad (2019) concludes that there is only scarce evidence on the effect of continuing education on employment and that it is highly questionable whether the results from the literature can be generalised to countries with different educational systems, different average levels of education, different labour markets (regulations) and welfare states.

In this paper, we address some of these limitations by studying the labour market effects of continuing education and training (CET) in Switzerland. We are not only interested in earnings effects, but also whether CET affects the risk to become unemployed. Switzerland is particularly interesting because Switzerland had the highest share (58 percent) of 25 to 64-year-olds who participated in job-related non-formal education and training among all European countries participating in the Adult Education Survey (AES) in 2016.[1] For comparison, the average across all European countries was only 35.3 percent. Moreover, the Swiss labour market can be characterised as liberal and

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[1] Cf. Eurostat: Adult Education Survey, 2016: https://ec.europa.eu/eurostat/databrowser/view/trng_aes_121/default/table?lang=en

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adult education is – in contrast to most other countries with high participation rates – privately organised by firms and workers.[2] These conditions resemble more those in Anglo-Saxon countries than those in continental Europe (with less competitive labour markets) or in Northern Europe (where CET is largely state-funded). Because the current literature focuses mainly on training effects in more regulated labour markets and with publicly provided or organised adult education (Midtsundstad, 2019), Switzerland provides an interesting setting to show whether the effects found in the literature so far can be generalised to other countries where labour markets are more competitive and CET is largely privately funded as well (such as Anglo-Saxon countries).[3]

This study is possible because we were able to combine three different administrative data sets. The information on training participation comes from the microcensus on education and training of the Swiss Federal Statistical Office (SFSO) in the year 2016. The survey defined continuous education and training (CET) as all learning activities with a work-related purpose that took place in non-formal courses within the 12 months prior to the survey. According to these data, 67 percent participated in work-related non-formal training, with on average 2.6 (median: 2) training courses.[4] The duration of training was 48 h on average (median: 24 h, minimum: 1 hour; maximum: 96 h) and most participants (78 percent) had their training financed by the employer. This census data is matched to longitudinal administrative data on income and labour market participation from the social insurance statistics and to the administrative data of the unemployment insurance for the years 2014 to 2018.

With this data, we can show that work-related training yields positive labour market outcomes in Switzerland. Our results show that participation in training increases yearly earnings by 3.5 percent compared to non-participants, which is comparable to similar studies in the literature (see Section 2). Moreover, we document that training reduces the risk of becoming unemployed by 1.8 percentage points. In further analysis, we also provide suggestive evidence that training participation reduces the length of an unemployment spell by up to 0.17 months after becoming unemployed in our three-year observation period.

These results are obtained by comparing labour market outcomes before and after the participation in training and between participants and non-participants. Because a simple comparison would lead to biased results due to self-selection into the treatment, we use a regression-adjusted matched difference-in-differences framework (Heckman et al., 1997, 1998; Smith & Todd, 2005a, 2005b; Todd, 2007) to establish identification. This approach allows us to control for selection into the treatment on time-invariant unobserved heterogeneity. To further facilitate the common trend assumption of identical trends in treatment and comparison group in the absence of the treatment, we account for selection on observables in both levels *and* trends for a larger set of predetermined outcomes and covariates. We use entropy balancing to construct matching weights (Hainmueller, 2012). The approach calibrates unit weights in the comparison group such that covariates of the reweighted comparison group satisfy prespecified balancing conditions. In our application, we demand that the comparison group matches the treatment group in terms of income and unemployment two years prior to the treatment, as well as full-time employment, education, occupation, industry, gender, age, marital status, children, citizenship status, and region of residence. Compared to the conventional propensity score matching, the approach has several advantages: First, entropy balancing allows to match not only on average covariates, but also to match the variance of the covariates. This is meaningful because training participants are a more homogenous selection of the population than the comparison group. Second, the non-parametric nature of entropy balancing requires far fewer modelling assumptions than propensity score matching. Third, we do not have to check balancing after matching (as in propensity score matching) because entropy balancing achieves balanced matches by construction.

Our paper further contributes to the literature by documenting a striking pattern in the returns to adult education along the income distribution. The results show that training has large effects on earnings for workers in the lowest tercile and almost no return for workers in the highest tercile. By contrast, the training effects on the risk of getting unemployed materializes only for workers in the highest tercile but are close to zero for workers in the lowest tercile. While this pattern can be rationalized by limited possibilities to increase earnings for workers who already receive high incomes, the result also cautions against concluding that training may not have labour market effects when focusing on the effects of earnings alone. In fact, the result calls for studying several labour market outcomes when assessing the effects of training.

The paper proceeds as follows. Section 2 discusses the related literature. Section 3 introduces the data sources and explains the construction of the dataset and all variables, provides details of the analytical sample, and show descriptive statistics. Section 4 describes the empirical setup and the implementation of the estimator. Section 5 presents the results. Section 6 discusses effect heterogeneity. Section 7 concludes.

2. Literature

The existing literature on the returns of adult education covers a wide range of learning activities. First, studies differ in their definition of adult education: some use a broad definition encompassing almost all forms of adult learning (Blanden et al., 2012; Büchel & Pannenberg, 2004; Dieckhoff, 2007; Ehlert, 2017; Görlitz & Tamm, 2016; Hidalgo et al., 2014; Muehler et al., 2007; Novella et al., 2018; Schwerdt et al., 2012). Other studies restrict continuous or adult education to work-related training, defined as training activities or courses for the purpose of advancing work and career prospects. These training activities take place within the firm or outside and are either worker-financed or financed – fully or only partially – by the employer (Gerfin, 2004; Ruhose et al., 2019). And finally, there is on-the-job training, which is initiated, organized, and financed entirely by the employer (Görlitz, 2011; Goux & Maurin, 2000; Leuven & Oosterbeek, 2008). Second, adult education can involve formal qualifications at secondary or tertiary level, often aimed at individuals with work experience but no formal degrees. These programs, especially prevalent in Scandinavia, are usually time-intensive and often come with financial support for living expenses (Böckerman et al., 2019; Dorsett et al., 2016; Kauhanen, A., 2021; Stenberg et al., 2012; Stenberg & Westerlund, 2015; Stevens et al., 2019). And third, many studies focus on training programs for unemployed individuals, often within the framework of active labour market policies (Bernhard & Kruppe, 2012; Card et al., 2018; Crépon et al., 2012; Doerr et al., 2017; Gerfin & Lechner, 2002; Hujer et al., 1999; Lechner & Wunsch, 2009).

A key empirical challenge for all of these studies is self-selection into training. Many studies use panel models with individual fixed effects or individual time trends to control for unobservable heterogeneity (Blanden et al., 2012; Büchel & Pannenberg, 2004; Ehlert, 2017; Goux & Maurin, 2000; Lechner, 1999; Pischke, 2001). In addition, studies have used panel models with individual-specific linear time trends to control for individual trends in labour market outcomes (Büchel & Pannenberg, 2004). Finally, only few studies use worker-firm matched data (Goux &

[2] According to the OECD employment outlook 2019, Switzerland rages among the countries with low regulatory protection (OECD, 2020); and in the annual report of the Fraser Institute on the economic freedom of the world, it ranges within the first quartile, taking the fourth place (Gwartney, 2020). Moreover, adult education in Switzerland is mainly privately organized, expenses are generally borne by employers or participants (SCCRE Swiss Coordination Centre for Research in Education, 2018).

[3] This study also adds to only two older studies that have looked into the effects of CET on labor market outcomes in Switzerland (Gerfin, 2004; Schwerdt et al., 2012). While the first relied on an IV approach to estimate causal effects, the second studied the effects in the context of an RCT with vouchers for CET.

[4] Training courses at the workplace or outside, organised and financed by the firm or by the individuals themselves.

Maurin, 2000).

Earlier but also more recent studies tried to provide evidence on the effect of adult education based on observational data in combination with econometric estimation techniques to construct a suitable comparison group for training participants. This part of the literature has studied extensively the combination of difference-in-differences estimators with propensity score matching (see, e.g., Dehejia & Wahba, 2002; Heckman et al., 1997, 1998; Smith & Todd, 2005a, 2005b; Todd, 2007). Muehler et al. (2007) and Novella et al. (2018) provide some examples for applications of this method. More recently, Ruhose et al. (2019, 2020) used entropy balancing (Hainmueller, 2012), which is a non-parametric matching technique for the construction of the comparison group, when evaluating monetary and non-monetary returns to work-related training in Germany. Comparing propensity score matching with entropy balancing, they show that entropy balancing produces very similar matches without the need of defining a matching function and without the need of comparing balancing after matching because entropy balancing achieves balancing on specified covariates by construction.

Arguably, a more credible source of identifying variation comes from (quasi-)experiments. For example, studies have used randomized control trails to study the effectiveness of specific training programmes (de Grip & Sauermann, 2012; LaLonde, 1986; Prada, Rucci, & Urzua, 2019). Other experiments exploit the variation of a random allocation of training vouchers, implemented on a wider scale (Görlitz & Tamm, 2016; Schwerdt et al., 2012). However, since voucher take-up is not random, these studies often identify intention-to-treat (ITT) rather than average treatment effects (ATT), and their generalizability is limited. Furthermore, the use of experiments is limited to certain interventions and treatment groups and can therefore not answer every socially relevant question.

Finally, some studies construct control groups from individuals who planned to participate in training but were prevented by random events (Gerfin, 2004; Görlitz, 2011; Leuven & Oosterbeek, 2008). These studies usually find no significant earnings effects, raising concerns about positive self-selection. However, one issue with this approach is that the number of people not participating due to random events is usually rather small, increasing standard errors and statistical uncertainty. This is particularly a problem when the intervention, such as participating in training, is heterogeneous.

The results from the non-experimental (and some experimental) studies suggest that training participation raises earnings between 3 and 12 percent (LaLonde, 1986; Muehler et al., 2007; Novella et al., 2018; Pischke, 2001; Ruhose et al., 2019; Vignoles et al., 2004), though effects vary by gender, age, type of training, or sector (Blanden et al., 2012; Büchel & Pannenberg, 2004; Ehlert, 2017). As already outline above, most experimental studies using arguably exogenous events in non-participation and randomly allocated training vouchers conclude that there are no causal effects from the participation in training (Görlitz, 2011; Görlitz & Tamm, 2016; Leuven & Oosterbeek, 2008), although some of these studies only cover short term effects. Furthermore, while the experimental literature can provide credible evidence on the causal returns to adult education, the effects are often limited to the very specific circumstances of the experiment (e.g., the uptake of a voucher) and therefore often lack external validity. Thus, to receive insights into the relationship between training participation and economic outcomes for a broader adult population, we still must rely on quasi-experimental techniques with observational data.

While earnings effects of training participation are extensively studied, there is much less evidence on the relationship between training participation and unemployment (Midtsundstad, 2019). If at all, employment effects are often studied in the context of active labour market evaluation programs. Most of this work finds no effects and even sometimes negative effects in the short run (Bernhard & Kruppe, 2012; Gerfin & Lechner, 2002; Görlitz, 2011; Görlitz & Tamm, 2016; Hujer et al., 1999; Lechner & Wunsch, 2009).

3. Data

This section provides the information on how the different administrative data records have been merged and what data the analytical sample contains to study the relationship between training participation and labour market outcomes such as earnings and unemployment in Switzerland.

3.1. Data sources

The main data source for adult education activities in Switzerland is the official Swiss Micro-census on Education and Training (MET) from 2016.[5] The MET provides information on the educational activities of the Swiss population, restricted to the permanent resident population between 15 and 74 years of age. The sample includes information from over 10,000 individuals. The data cover socio-demographic characteristics, current educational and training activities, and the reasons for participating in education and learning programmes. The MET was conducted between April and December 2016, and it covers training from April 2015 until December 2016 (see Fig. 1). This also means that we do not have any information about training activities before April 2015 for all individuals. However, we know from other studies that training often begets further training, meaning that participation in one training activity increases the likelihood of participating in another (see, e.g., Ruhose et al., 2019). In Section 4, we discuss how we deal with this data limitation in the empirical strategy.

Earnings data were matched for all respondents in MET. The earnings data was provided by the Central Compensation Office (CCO). The CCO is the federal institution that implements the central pillars of the social security system (old-age pensions, disability insurance and compensation for loss of earnings). Their register data comprise the total yearly gross income from paid employment (excluding income from self-employment for all insured people that are subject to social security contributions. We use the information for the years 2014 to 2018 (see Fig. 1). Since the earnings data cover in principle all individuals surveyed in the MET, we were able to match earnings information to almost all of them (99.1 percent).

An important limitation of this data source is that it only provides us with information on yearly income. Thus, we do not observe the hours worked, which prevents us from decomposing the effect of training participation into changes in hourly compensations and changes along the labour supply margin. The only information on the labour supply margin that we have is the information whether the individual is full-time employed (i.e., working >37.8 h per week) or part-time employed. This information comes from the MET and is available for 2016 only.

The third source of information are the register data on unemployment. This data is collected by the national unemployment insurance and provided to us by the State Secretariat for Economic Affairs (SECO). The register data contain information on the unemployment status of the entire population and lists the monthly unemployment spells, which we aggregated into yearly unemployment information that we could merge to the MET.[6]

3.2. Variables

In the context of this study, we define continuous education and training (CET) as all learning activities with a work-related purpose that

[5] The MET is carried out in a five-year interval. Data collection is done by computer-assisted telephone interview.

[6] In total, we could match information on unemployment spells from the SECO data to 385 individuals included in the MET sample. This represents a total of 1,855 observations, or in other words, we observe for 4.3 percent of the sample at least one unemployment spell in the two years following the treatment.

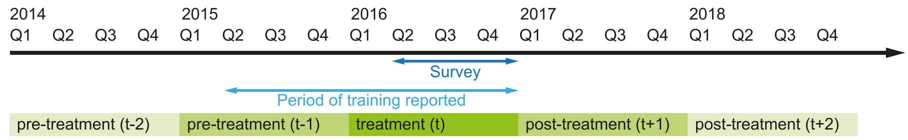

Fig. 1. Timing of surveys.
Notes: The figure shows the timing of the Swiss Micro-census on Education and Training (MET) from 2016 and the available information on earnings and unemployment two years before and two years after the training spells.

takes place in non-formal education.[7] This training can take place within the firm or outside and can be organized individually. The treatment variable takes the value of 1 if the respondent has participated in such a work-related training within the past 12 months, and 0 if the respondent has not participated in CET during that period. Our treatment variable is a rather broad indicator that tells us whether the respondent has participated in training.. It distinguishes roughly between categories such as courses, conferences or private tuition and we know the total number of hours that the person has spent on training in the past year. Furthermore, we know whether the training was financed by the employer or the participant itself. This is important information because the employer's support gives an indication of selection decisions by the employer and their assessment of the productivity of the employees (Leuven et al., 2005). However, the survey does not contain further information about how the employees have been selected into the training.

Our main outcome variables are earnings and the unemployment status of the individual. To assess the effect of CET on earnings, we mainly use the log yearly earnings in 2017 and 2018, i.e., one and two years after the CET participation. For unemployment, we use the information in the official register for people currently not employed, seeking a job and able to immediately start a new job. We distinguish between the risk of becoming unemployed (dummy variable taking the value of 1 if the respondent becomes newly unemployed in the observed year, and 0 if not) and the duration of the employment spell (measured by the number of months per year).

To construct our comparison group, we use a set of conditioning variables that are known to affect the participation in training as well as the labour market outcomes (see Table 1). They cover outcomes before treatment (such as earnings and unemployment experience), demographic characteristics (such as gender, age, marital status, children, citizenship status, and region of residence), education (five categories), and occupation (six categories), as well as employment information on the type of industry (9 categories). Furthermore, we control for the compensation fund, which provides some firm-specific information.[8] The next section provides more detail about how we use them to construct a comparison group.

3.3. Analytical sample and descriptive statistics

To construct the analytical sample(s) for our analysis, we start with 10,056 individuals and 44,505 person-year observations in the matched sample. Restricting the sample to people aged between 20 and 60 years old, who are not self-employed, and for whom we observe income and unemployment information in the pre- and posttretment period, reduces the sample by about half to 5414 unique individuals and 26,611 person-year observations (see Appendix Table A.2 for an overview of the sample construction). Within the sample, we count 17,873 person-year observations (3650 persons) for the group of training participants and 6903 person-year observations (1397 persons) for the group of non-participants.[9] The remaining 367 individuals (1835 person-year observations) report to have participated only in non-work-related training, which we drop from the main analysis.[10] The samples reported in the regression tables may vary due to missing observations for the dependent variables (income, unemployment status, and unemployment duration), or due to missing values for covariates, which we do not impute.

Table 1 reports the descriptive statistics separately for training participants and non-participants. On average, 72 percent of the sample report participation in work-related non-formal training within the 12 months before the survey. Course participation is distributed as follows: The average number of training courses is 2.6 (median: 2). On average, individuals participated in training courses for 48 h (median: 24 h). The large majority of participants (78 percent) gets their training financed by their employer.

The table also reveals that training participants are – not surprisingly given the large share of employers financing – a positively selected group in general, which aligns well with the findings in the related literature. For example, we find a statistically highly significant earnings difference between training participants and the comparison group of around 25,800 Swiss Francs already in 2015 before the treatment. We also find that training participants are less likely to be unemployed than non-participants before the treatment. This aligns well with the observation that university graduates are much more likely to participate in work-related training (88 percent) than workers with vocational education at the secondary level (66 percent).

4. Empirical strategy

Given the positive selection into training activities, documented in our data, conventional OLS estimates would be upward biased and overestimate the effects of CET (Ashenfelter, 1978; Ashenfelter & Card, 1985; LaLonde, 1986). Therefore, as described in Section 2, several approaches exist that try to construct a comparison group that allows comparing like with like. Because we do not observe any experimentally induced variation in participation in CET and in the light of the limitations of experimental approaches (see Section 2), we rely on a matching difference-in-differences approach, which of all non-experimental estimators is shown to work best (Heckman et al., 1997, 1998; Smith & Todd, 2005b; Todd, 2007). We describe below how we use this approach to address the most pressing issues of omitted variables bias, which often stem from higher income levels among

[7] The precise wording of the question we are using for the construction of the training indicator is: "Have you attended a training course (or a conference, seminar) in the past 12 months? Did you attend this course (or the conference, seminar) for professional reasons?"

[8] Compensation or pension funds in Switzerland are organized regionally and are often industry- or even firm-specific. Individuals are affiliated to a pension fund through their employer.

[9] The share of active people in CET in this paper is higher than in the statistics mentioned earlier because in our analytical sample, we restrict ourselves to people in gainful employment in the year surveyed (2015) and not the total of the adult population.

[10] We do that to achieve a more homogeneous control group because non-work-related training comprises even a larger and more heterogeneous range of training activities. Furthermore, there are difficulties in delimiting non-formal training activities that are declared being not work-related but which can have an effect on the workers' productivity (e.g., language courses or IT training). We analyse non-work-related training in Appendix Table A.8 and show that they do not have an effect on income or unemployment.

Table 1
Descriptive statistics.

Variable	Label	Full sample	Training participants	Comparison group	
		Average	Average	Difference to (4)	p-value of (5)
(1)	(2)	(3)	(4)	(5)	(6)
Training characteristics					
Participation in work-related training	0=no; 1=yes	0.72	1.00	–	–
Number of training courses	average/median	–	2.6/2.0	–	–
Number of training hours	average/median	–	48/24	–	–
Training financed by employer	0=no; 1=yes	–	0.78	–	–
Labour market characteristics					
Log yearly income (average)	in 2014 Swiss Francs	11.115	11.228	−0.412	0.000
Log yearly income, 2014 [(c)]	in 2014 Swiss Francs	11.061	11.168	−0.389	0.000
Log yearly income, 2015 [(c)]	in 2014 Swiss Francs	11.102	11.214	−0.407	0.000
Log yearly income, 2016	in 2014 Swiss Francs	11.133	11.245	−0.412	0.000
Log yearly income, 2017	in 2014 Swiss Francs	11.145	11.256	−0.409	0.000
Log yearly income, 2018	in 2014 Swiss Francs	11.154	11.270	−0.426	0.000
Unemployed (average)	0=no; 1=yes	0.034	0.020	0.014	0.000
Unemployed, 2014 [(c)]	0=no; 1=yes	0.025	0.020	0.005	0.013
Unemployed, 2015 [(c)]	0=no; 1=yes	0.030	0.022	0.007	0.000
Unemployed, 2016	0=no; 1=yes	0.040	0.030	0.011	0.000
Unemployed, 2017	0=no; 1=yes	0.038	0.026	0.013	0.000
Unemployed, 2018	0=no; 1=yes	0.035	0.025	0.010	0.000
Unemployed spell (nb. months)	months	0.293	0.119	0.174	0.000
Full time employed [(c)]	0=no; 1=yes	0.603	0.620	−0.060	0.000
Demographic characteristics					
Female [(c)]	0=male; 1=female	0.490	0.533	0.061	0.000
Age [(c)]	years	43.011	42.778	0.840	0.000
Married [(c)]	0=no; 1=yes	0.601	0.616	0.021	0.003
Children [(c)]	0=no; 1=yes	0.367	0.373	−0.021	0.002
Swiss citizen [(c)]	0=no; 1=yes	0.802	0.830	−0.102	0.000
Federal state [(c),]	24 categories	13.588	13.550	0.137	0.227
Education					
Compulsory schooling [(c)]	0=no; 1=yes	0.091	0.049	0.153	0.000
Upper secondary: vocational [(c)]	0=no; 1=yes	0.417	0.382	0.127	0.000
Upper secondary: general [(c)]	0=no; 1=yes	0.099	0.095	0.014	0.000
Tertiary education: vocational [(c)]	0=no; 1=yes	0.162	0.194	−0.116	0.000
Tertiary education: university [(c)]	0=no; 1=yes	0.232	0.281	−0.178	0.000
Occupational classification					
Management/judicial authorities [(c)]	0=no; 1=yes	0.132	0.152	−0.072	0.000
Scientists [(c)]	0=no; 1=yes	0.191	0.228	−0.132	0.000
Technicians/professionals [(c)]	0=no; 1=yes	0.252	0.285	−0.121	0.000
Commercial employees [(c)]	0=no; 1=yes	0.085	0.072	0.048	0.000
Sales/services	0=no; 1=yes	0.120	0.101	0.069	0.000
Craftsmen [(c)]	0=no; 1=yes	0.098	0.077	0.077	0.000
Unskilled workers [(c)]	0=no; 1=yes	0.078	0.052	0.092	0.000
Other	0=no; 1=yes	0.044	0.033	0.040	0.000

Notes: The table shows descriptive statistics of the main variables. We use a *t*-test to test for the significance of the difference between the training participants and the comparison group. Variables refer to the year 2015 if not noted otherwise. Descriptive statistics by federal state are shown in Appendix Table A.1.
Data sources: Swiss Micro-census on Education and Training (MET), Central Compensation Office (CCO), and State Secretariat for Economic Affairs (SECO). Authors' own calculations.
[(c)] indicate variables that are used as conditioning variables.

training participants compared to non-participants in the pre-treatment period (e.g., due to positive self-selection of more motivated or capable individuals into training) and from steeper pre-treatment income trajectories (e.g., because individuals with stronger career motivation or upward mobility tendencies are more likely to engage in training) (see, e.g., Pischke, 2001; Ruhose et al., 2019).[11]

In what follows, we focus on the implementation of a regression-adjusted difference-in-differences matching approach to estimate an ATT, i.e., the training-induced change in earnings and unemployment of those individuals who participated in work-related training (treatment group). Eq. (1) describes the estimator. In this setting, n_1 is the number of treated individuals, and group membership is indicated by I_1 (treated) and I_0 (comparison), respectively. The counterfactual comparison group is a weighted average of the change in outcome variables, with weights equal to $w(i,\ j)$. Y_0^{after} and Y_0^{before} refer to potential outcomes from before and after the treatment in the absence of treatment. Y_1^{after} describes the potential outcome after the treatment for the treatment group.

$$\widehat{\beta}_{DiD} = \frac{1}{n_1}\sum_{i\in I_1}\left[\left(Y_{1i}^{after} - Y_{0i}^{before}\right) - \sum_{j\in I_0} w(i,j)\left(Y_{0j}^{after} - Y_{0j}^{before}\right)\right] \tag{1}$$

The literature has often employed propensity score matching to find weights $w(i,\ j)$ for the construction of a comparison group that has on average similar observable characteristics as the treatment group prior to the treatment (Caliendo & Kopeinig, 2008; see, e.g. Dehejia & Wahba, 2002).[12] In this paper, we rely on entropy balancing instead of propensity scores to construct the weights $w(i,j)$ (Hainmueller, 2012). Entropy balancing is a non-parametric reweighting technique that is more

[11] For some training participants, we may also observe that earnings decrease prior to the training (Ashenfelter, 1978).

[12] The propensity scores are estimated probabilities to receive the treatment. They are either used to find non-treated units with similar treatment propensities (e.g., as in nearest neighbour matching), but they can also be used directly to weight the units in the comparison group (inverse probability weighting).

effective in reducing covariate imbalance than propensity score matching (Marcus, 2013; Ruhose et al., 2019). The method allows to set a series of balancing conditions for pre-specified covariates. These balancing conditions follow the logic that the mean of a covariate X in the treated sample should be the same as the weighted mean of the same covariate in the comparison sample. Eq. (2) provides a formal representation of this idea.

$$\sum_{i|i\in I_1} X_i = \sum_{j|j\in I_0} w(i,j)X_j \tag{2}$$

The entropy balancing algorithm then searches for a vector of weights for the individuals in the comparison group, which satisfies the balancing constraints for all specified covariates (see, Hainmueller (2012) for further details of the approach). In our application, we require the same mean and variance of the conditioning variables as in the treatment group (see Table 1). While we discuss the covariates, which we use for constructing the control group at the end of this section, we can show that adjusting for the variance is potentially important. For example, the standard deviation on log yearly earnings in 2015 is equal to 0.99 in the comparison group (about 9.3 percent of the comparison group mean) where it is only equal to 0.74 in the treatment group (about 7.0 percent of the comparison group mean). Another advantage of this approach over using propensity score matching to construct a suitable control group emerges from the observation that our pool of potential comparison units is almost as large as the pool of treated units, which is a specific feature of the Swiss adult education sector. Propensity score matching, however, usually requires a larger pool of potential comparison units to find satisfying matches. Entropy balancing ensures a much quicker convergence in the weights that yield a satisfactory control group.

The estimator from Eq. (1) is implemented in two steps: In the first step, we construct the weights $w(i,j)$ using entropy balancing. In the second step, we estimate a difference-in-differences regression with the weights obtained in the first step. The estimator is similar to the traditional difference-in-differences estimator in that it partials out selection on unobservables that is time-invariant. In addition, however, we also partial out all differences in observable characteristics that we have included in the first step of the procedure. To give the estimates a causal interpretation, we have to assume that no unobserved variables exist that simultaneously influence changes in labour market outcomes and the probability of training participation. That is, the labour market outcomes of treated individuals would have followed the same trend that we observe for the matched comparison group in the absence of treatment. Formally, this means:

$$E\left[Y_0^{after} - Y_0^{before}|EB(X), D=1\right] = E\left[Y_0^{after} - Y_0^{before}|EB(X), D=0\right] \tag{3}$$

where $EB(X)$ refers to the weights obtained from entropy balancing.

Conditioning on a rich set of covariates makes it more likely that this condition is fulfilled. Table 1 summarizes all variables that we use for the balancing. Most importantly, we condition on the yearly income and the unemployment experience in 2014 and 2015. This balancing on the outcome variables prior to the treatment controls flexibly for different pre-treatment labour-market trajectories.[13] By doing so, it also mitigates omitted variable bias arising not only from the self-selection of training participants based on their pre-treatment income levels, but also from the fact that they often experience stronger income growth even before participating in the training activity compared to the control group. While this pattern may result from a self-selection of, e.g., more motivated and capable workers being more likely to participate in training, it is also possible that it reflects the effect of prior participation in training. This possibility is especially relevant for our study because we do not observe and therefore cannot balance on training activities prior to Q1–2015. Thus, not conditioning on pre-treatment individual income and unemployment trajectories would likely lead to a treatment effect that captures the effect of all previous trainings and would therefore overstate the treatment effect of the training spell we analyse.[14] In that sense, income and unemployment can be seen as a catch-all measure, capturing all activities and choices (including participation in previous training activities) that have led to the observed income and unemployment trajectories. Nevertheless, it is still possible that our treatment effect estimates capture the impact of earlier training activities if the effects of training are highly non-linear or otherwise too complex to be fully accounted for by conditioning on the two years prior to the observed training. However, conditioning on income and unemployment status in 2015 comes at the cost of potentially controlling away some treatment effects for workers who participated in training during that year. This may even mean that your estimates provide a lower bound to the true effect of training on income and unemployment.

We further condition on full-time employment at the time of the MET-survey to proxy for the intensive labour supply margin because we have no information about the hours worked in the administrative income data. Moreover, we condition on demographic characteristics such as gender, age, marital status, having children, citizenship status, and region of residence. We also condition on education in five categories and occupational groups in six categories. All these information come from the MET data, which are based on the year 2016. Therefore, similar concerns apply for some of these variables as for conditioning on income and unemployment in 2015. For example, it could be that training leads to better paid occupations or that labour supply decisions change after training participation. However, this should usually bias our estimates towards zero because some of the treatment effects are not considered.

In conclusion, our approach addresses important omitted variable biases that arise from time-invariant differences and self-selection based on income and unemployment trajectories prior to treatment between training participants and non-participants. However, whether our estimates approximate those obtained under an experimental ideal—where training participation is effectively random—critically depends on whether we have successfully modelled the self-selection into training. In that sense, our estimates rely on the same selection-on-observables assumption as all non-experimental studies mentioned in Section 2.

5. Results

In Fig. 2(a) and Column (1), Panel A of Table 2, we show significant earnings returns to the participation in work-related CET. While the effect in year 2014 is close to zero due to including this covariate in the entropy balancing stage, we find that participants of work-related training earn 2.8 percent more than the individuals that did not participate in adult education in the year of the treatment (2016). This effect remains stable in 2017 and increases up to 3.9 percent in 2018. Averaged over the post-treatment period (years 2017 and 2018), the effect of work-related training amounts to 3.4 percent (Column (1), Panel B in Table 2), which is in line with the effects found in other countries (Muehler et al., 2007; Novella et al., 2018; Ruhose et al., 2019).[15]

[13] Alternatively, one can use coarsened exact matching (Iacus et al., 2012) to achieve balance on the set of covariates. Appendix Table A.4 shows that the results are very similar when using this approach compared to our main results reported in Table 2. However, covariate imbalance remains after matching (see Appendix Table A.5), which is why we use entropy balancing in our preferred specification.

[14] Using only the income and unemployment information of the year 2014 for constructing the control group, leads to an estimated earnings effect that is equal to 0.058 with standard error 0.015, which is almost twice as large as the effect we report in Table 2, Panel B.

[15] The results are very similar when we estimate the model on a balanced panel, which is a panel in which we observe each individual with all information in all periods (see Appendix Table A.3).

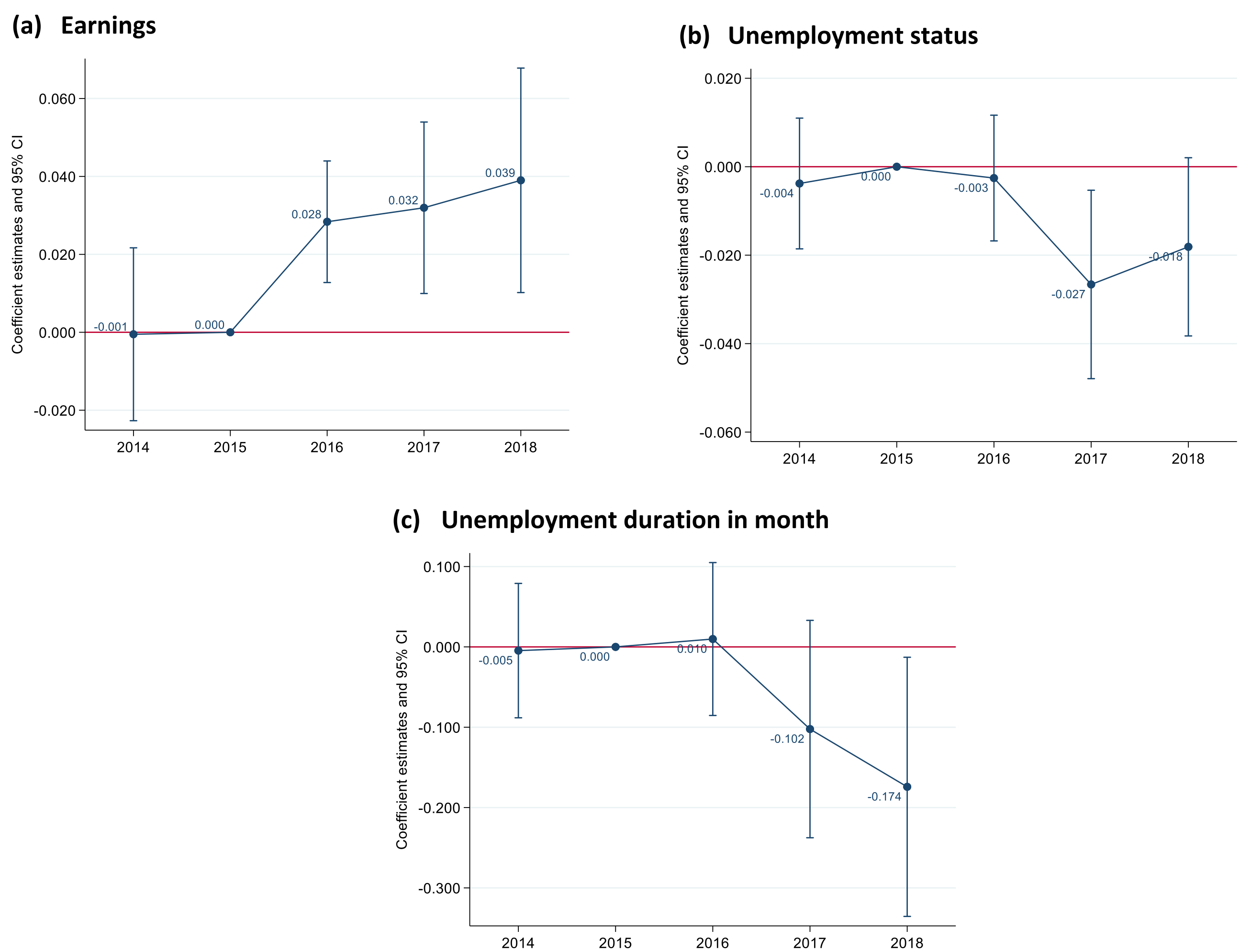

Fig. 2. Training effects on earnings and unemployment.
Notes: The figure shows the results of training participation on log yearly earnings (a), unemployment status (b), and unemployment duration in month (c). Panel A in Table 2 provides the corresponding regression results. Reference period is equal to 2015. Observations in the comparison group are weighted by balancing weights. 95 percent confidence intervals are plotted and obtained from standard errors that are clustered at the individual level.
Data sources: Swiss Micro-census on Education and Training (MET), Central Compensation Office (CCO), and State Secretariat for Economic Affairs (SECO).

For the risk of becoming unemployed, the coefficients of the difference-in-differences model also reveal statistically significant effects of work-related CET within the years after the treatment (see Fig. 2 (b) and Column (2), Panel A of Table 2). There are no significant pre-treatment differences and very small and insignificant differences in 2016 between the treatment and control group in the risk of becoming unemployed. The effect size increases to 2.7 percentage points and 1.8 percentage points in 2017 and 2018, respectively. On average, we observe a decrease in the unemployment probability by 2.1 percentage points after training participation (Column (2), Panel B of Table (2)). Compared to the unemployment rate in the comparison group in 2015, which is equal to3.7 percent, this implies that training participation lowers the average unemployment risk by about half of the average.

In Fig. 2(c) and Column (3) of Table 2, we study the length of a potential unemployment spell after someone has become unemployed during our observation period. We find a significant effect of work-related CET only for the last year. Thus, for the year 2018, we observe that individuals with training have spent 0.17 month less in unemployment after becoming unemployed during the observation period compared to individuals without training. On average, we observe a decrease in the duration of unemployment of 0.14 month (Column (3), Panel B of Table 2). While the direction and magnitude of the effect seems to point into the expected direction, we like to caution against a strong interpretation of this result because standard errors are large, the number of affected individuals is low, and our rather short panel creates a censoring issue at the end of the panel.

6. Effect heterogeneity

The average results conceal heterogeneous effects based on individual characteristics. For each heterogeneity, we construct and use new entropy balancing weights that are based on the treated and comparison group in each subpopulation. In line with the existing literature, we anticipate heterogeneous effects across characteristics that reflect individuals' qualifications and labour market positions, particularly educational attainment. Notably, individuals with lower levels of formal education are expected to benefit disproportionately from participation in work-related education and training, as such interventions may serve as a key mechanism for improving their labour market prospects and reducing skills gaps (Arellano-Bover, 2022; Knaus et al., 2022; Sritharan, 2023). While we do not find significant differences for males and females, Table 3 shows that the earnings effect is slightly more pronounced among younger (20–39 years) than older workers. Regarding heterogeneity by qualification, we find that the average effect on

Table 2
Main results.

	Log yearly earnings (1)	Unemployment risk (2)	Unemployment spell (3)
	Panel A: yearly effects		
Training x 2018	0.039***	−0.018*	−0.174**
	(0.015)	(0.010)	(0.082)
Training x 2017	0.032***	−0.027**	−0.102
	(0.011)	(0.011)	(0.069)
Training x 2016	0.028***	−0.003	0.010
	(0.008)	(0.007)	(0.049)
Training x 2014	−0.001	−0.004	−0.005
	(0.011)	(0.008)	(0.043)
R-squared	0.017	0.006	0.007
Observations	21,764	22,236	22,236
	Panel B: average effects		
Training x post	0.034**	−0.021***	−0.136**
	(0.014)	(0.007)	(0.068)
R-squared	0.012	0.007	0.006
Observations	17,424	17,769	17,769

Notes: The table shows the results of training participation on log yearly earnings and unemployment status. Reference period in Panel A is equal to 2015. Observations in the comparison group are weighted by balancing weights. The treatment year 2016 in Panel B is omitted. Standard errors clustered at the individual level reported in parentheses.
Data sources: Swiss Micro-census on Education and Training (MET), Central Compensation Office (CCO), and State Secretariat for Economic Affairs (SECO). Authors' own calculations.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table 3
Heterogeneity by individual characteristics: log yearly earnings.

	(1)	(2)	(3)
Gender	**Male**	**Female**	
Training x post	0.032**	0.043*	
	(0.015)	(0.025)	
R-squared	0.013	0.012	
Observations	9121	8303	
Age groups	**20–39**	**50–59**	**50–60**
Training x post	0.041	0.020	0.032
	(0.029)	(0.025)	(0.020)
R-squared	0.057	0.020	0.020
Observations	6231	5351	5806
Education	**Unskilled**	**Vocational**	**General**
Training x post	0.052	0.039***	0.007
	(0.054)	(0.014)	(0.023)
R-squared	0.030	0.008	0.028
Observations	1418	10,180	5826
Income groups	**1st tercile**	**2nd tercile**	**3rd tercile**
Training x post	0.080*	0.023	0.0034
	(0.048)	(0.029)	(0.019)
R-squared	0.096	0.002	0.000
Observations	4589	6110	6725
Finance model	**Self-payer**	**Firm-financed**	
Training x post	0.032	0.032**	
	0.025	(0.013)	
R-squared	0.016	0.011	
Observations	7133	15,135	

Notes: The table shows the results of training participation on log yearly earnings for subgroups specified in the column header. Observations in the comparison group are weighted by balancing weights that are computed for each subgroup separately. The treatment year 2016 is omitted. Income groups: 1st tercile (below CHF 50,600, mean: 28,649), 2nd tercile (CHF 50,600 – 85,990: mean: 68,345), 3rd tercile (over CHF 86,000; mean CHF 137,492). Standard errors clustered at the individual level reported in parentheses.
Data sources: Swiss Micro-census on Education and Training (MET), Central Compensation Office (CCO), and State Secretariat for Economic Affairs (SECO). Authors' own calculations.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

earnings seems to be driven by those with a vocational qualification.[16] For them, the estimated coefficient of 0.039 is close to the average effect of 0.034 (see Column (1), Panel B, Table 2). Training effects are slightly more pronounced for unskilled workers, which are workers without an occupational qualification and close to zero for individuals with a general qualification.[17] A striking pattern emerges when studying effect heterogeneity by income groups. Individuals in the lowest income tercile show the strongest returns to training with a coefficient of 0.081. Effects then decline and are close to zero for the highest income groups. The finding that the positive income effects are limited to people with the lowest incomes can be explained by the fact that these individuals also have the lowest level of human capital and therefore the relative gain in productivity due to limited investment in human capital is greatest (Card et al., 2018; Carneiro et al., 2011; Fialho, Quintini, & Vytlacil, 2019).

The last entry in Table 3 refer to who pays for the training. The reason for making this distinction is that the training motivation may differ between those who pay by themselves and those where the firm pays for the training. A priori, it is unclear which type attracts more motivated workers. For example, while self-funded training may indicate a high level of motivation, it is also possible that employers choose to fund training only for workers who already demonstrate a sufficiently high degree of motivation. Moreover, while self-financed training may enhance outside options and thereby increase participants' earnings, it is plausible that firms must share the returns from firm-financed training with workers to incentivize their participation. Thus, there is no clear prediction as to which type of training is more effective in improving labour market outcomes. The results imply that there are no significant differences between those who pay themselves for the training and those where the training is financed by the firm.

The same picture emerges when we study effect heterogeneity by the intensity of training. Splitting the treatment group by the median total number of training hours across all courses attended by treated workers, we find that training effects are very similar for individuals with above-median total training hours compared to those below the median (Appendix Table A.6).[18] This suggests that even shorter trainings yield significant returns in the labour market. However, the result may also reflect the fact that we are examining outcomes only within two years after the treatment. It is possible that the treatment effects would differ if assessed over a longer period.

Because our baseline indicator of work-related training is a comprehensive measure that includes all learning activities with a work-related purpose that takes place in non-formal training (i.e. courses,

[16] Our category "Vocational" includes vocational education training at the upper secondary level as well as at the tertiary level (Professional Education and Training; PET). Specifications with separate categories for upper-secondary and tertiary level degrees show a statistically significant effect of vocational education at the upper secondary level, but only a small effect of tertiary level professional education. We conclude from this, that the statistically significant effect of the category "vocational" education (not reported here) is basically due to the group of people with professional education.

[17] The category «general qualifications» comprises workers with a general education at the upper secondary qualification (e.g. baccalaureate schools) or at university. Our categorization takes account of the strong differentiation of the Swiss educational system into a vocational sector and a general sector which both cover the upper secondary as well as the tertiary level.

[18] The conclusion does not change when we split the treatment group by terciles or even quartiles of the total training hours distribution (results not shown).

Table 4
Heterogeneity by individual characteristics: unemployment.

	Risk of unemployment			Duration of unemployment (in months)		
	(1)	(2)	(3)	(4)	(5)	(6)
Gender	**Male**	**Female**		**Male**	**Female**	
Training x post	−0.020**	−0.021		−0.237***	−0.108	
	(0.010)	(0.013)		(0.082)	(0.115)	
R-squared	0.004	0.006		0.013	0.003	
Observations	9121	8506		9263	8506	
Age groups	**20–39**	**40–49**	**50–60**	**20–39**	**40–49**	**50–60**
Training x post	−0.007	−0.028*	−0.016*	−0.087	−0.134	−0.220*
	(0.012)	(0.014)	(0.008)	(0.093)	(0.119)	(0.122)
R-squared	0.001	0.009	0.009	0.004	0.006	0.013
Observations	6421	5423	5889	6421	5423	5889
Education	**Unskilled**	**Vocational**	**General**	**Unskilled**	**Vocational**	**General**
Training x post	−0.034*	−0.018***	−0.017	−0.288	−0.108	−0.188
	(0.020)	(0.008)	(0.013)	(0.249)	(0.071)	(0.147)
R-squared	0.014	0.005	0.003	0.023	0.005	0.006
Observations	1451	10,369	5949	1451	10,369	5949
Income groups	**1st tercile**	**2nd tercile**	**3rd tercile**	**1st tercile**	**2nd tercile**	**3rd tercile**
Training x post	−0.011	−0.009	−0.029**	0.083	−0.049	−0.341**
	(0.012)	(0.014)	(0.014)	(0.105)	(0.079)	(0.134)
R-squared	0.002	0.004	0.006	0.001	0.004	0.024
Observations	4720	6233	6816	4720	6233	6816
Finance model	**Self-payer**	**Firm-financed**		**Self-payer**	**Firm-financed**	
Training x post	−0.020**	−0.019**		−0.147*	−0.192***	
	(0.010)	(0.008)		(0.086)	(0.061)	
R-squared	0.005	0.005		0.016	0.010	
Observations	7298	15,425		7298	15,425	

Notes: The table shows the results of training participation on unemployment status (Columns (1) to (3)) and duration of unemployment in month (Columns (4) to (6)) for subgroups specified in the column header. Observations in the comparison group are weighted by balancing weights that are computed for each subgroup separately. The treatment year 2016 is omitted. Income groups: 1st tercile (below CHF 50,600, mean: 28,649), 2nd tercile (CHF 50,600 – 85,990: mean: 68,345), 3rd tercile (over CHF 86,000; mean CHF 137,492). Standard errors clustered at the individual level reported in parentheses.
Data sources: Swiss Micro-census on Education and Training (MET), Central Compensation Office (CCO), and State Secretariat for Economic Affairs (SECO). Authors' own calculations.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

seminars or conferences), we also check how the results change when studying only work-related training that took place in courses. As expected, we observe slightly stronger income effects compared our main results using the more comprehensive measure (Appendix Table A.7). However, the difference is not very large (0.043 versus 0.034) and the effects on unemployment risk are slightly weaker for courses than for the more comprehensive training measure. In addition, the lower number of participants under this definition limits our ability to conduct meaningful heterogeneity analyses.

Finally, we also study the effects of participating in non-work-related courses. That are courses pursued solely for leisure purposes. While we do not necessarily expect that these courses do not have any labour-market effects, we would expect that the returns are substantially smaller compared to the participation in work-related training. While participants in these courses are also very likely to participate in work-related training ($r = 0.36$), we find small non-significant treatment effects (Appendix Table A.8). This finding confirms earlier results suggesting that only training pursued with a vocational purpose can produce labour market effects.

Regarding the effects of training on unemployment, we report effect heterogeneity in Table 4. Notably, many effects in these subpopulations are not precisely estimated, which cautions against strong conclusions. However, there are some clear tendencies visible in the data. While the training effect on the unemployment risk is similar for males and females, the training effect on unemployment spells is much larger for males than for females. We also only find moderate differences by age. If at all, the effect is largest for the group of 40–49 years old, and almost zero for the youngest cohorts. Regarding education, the effect of training on unemployment risk and spell is largest for unskilled workers (even though large standard errors render the coefficients only at the 10 percent level significant). This is in line with the training effects on income, which are also largest for low-skilled workers. Thus, it seems that low-skilled workers profit in two ways from training – by increasing their earnings and lowering their unemployment risk. We also observe heterogeneous effects with regard to the position in the income distribution. While earnings effects appeared to be strongest in the lowest income tercile, we find, conversely, that unemployment effects are strongest in the largest tercile. Thus, training is also useful for high-income workers as they can secure themselves against unemployment. This seems plausible since their strong income position limits the potential for further income increases.

Overall, the results suggest that training primarily increases earnings for younger workers, unskilled workers, and those in the lowest income terciles. This may also help explain the limited treatment effect heterogeneity by type and intensity of training, as even short training courses can plausibly have a meaningful impact on the labour market outcomes of this group—particularly when compared to other low-skilled, low-income individuals who do not participate in *any* form of training. Given, that early labour market disadvantages can have persistent effects on earnings trajectories (Arellano-Bover, 2022), our findings further suggest that work-related training may serve as a corrective mechanism that helps to mitigate these long-term disadvantages, particularly for individuals with lower socioeconomic backgrounds or weaker formal qualifications.

7. Conclusion

A few decades ago, non-formal continuing education and training (CET) was propagated primarily as a means for adults to close gaps in formal education in later working life. However, in the face of accelerating structural change and digitalisation, CET has become a necessity for a broad segment of the workforce, not least formally highly qualified individuals. The latter are particularly vulnerab le to a depreciation of their human capital over time and therefore need to continuously invest in it. Not only when structural change forces them to change their occupational field or sector, but also to maintain their skill level in their traditional occupation.

Against this background, it is astonishing how narrow the empirical literature is that has investigated the economic benefits of CET, especially in comparison to the countless studies on the returns to formal education. Two reasons may be decisive for this. Firstly, the great heterogeneity and constantly changing offers in adult education, relative to formal qualifications. And secondly, the fact that selection into further education, and thus the potential biases in the estimates of the effects, are even more relevant in further education than in formal education pathways.

In this paper, we attempt to make a new contribution to the existing literature by estimating labour market returns (income and reduction of the risk of becoming unemployed) using a novel dataset that combines census data on individual training activity with register data on income and unemployment. This dataset allows us, on the one hand, not to rely on self-reported data on labour market returns and, on the other hand, to consider a longer period of time before and after the training, which allows us to construct a comparable control group to our treatment group. We do this by applying a regression-adjusted matched difference-in-differences approach with entropy balancing to account for selection bias and sorting on trends in income gains.

The empirical data come from Switzerland, which is interesting for at least three reasons. Firstly, Switzerland is one of the countries with the highest average CET participation, at least in a European comparison. Secondly, in contrast to other countries with high participation rates, this CET is mostly privately organised with only few state interventions and thirdly, the labour market is also fairly liberalised and, as far as the strength of labour market regulation is concerned. These conditions correspond more to Anglo-Saxon countries than to continental European countries.

The empirical results document that on average, training participation increases earnings by 3.4 percent and reduces the risk of becoming unemployed by 2.1 percentage points, which is a fairly large relative effect, given that unemployment rates are quite low in Switzerland. Furthermore, we document an interesting effect heterogeneity along the income distribution. While those in the lowest income tercile profit from training through earnings increases, workers at the top of the income distribution mostly profit by decreasing unemployment risks. For vocationally trained workers, we find that training seems to provide a double dividend of increased earnings and reduced unemployment risk.

While our paper shows that the effects of CET on earnings are comparable to those observed in other countries, we also provide evidence that CET reduces the risk of unemployment. In this sense, CET can yield a double dividend for those who benefit from it. However, the pronounced effect heterogeneity also indicates that CET does not work for everyone in every context—an important consideration when deciding to invest time and money in such training.

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CRediT authorship contribution statement

Stefan Denzler: Conceptualization, Methodology, Formal analysis, Writing – original draft, Project administration. **Jens Ruhose:** Conceptualization, Methodology, Formal analysis, Writing – original draft, Supervision. **Stefan C. Wolter:** Conceptualization, Methodology, Writing – original draft, Supervision, Project administration.

Declaration of competing interest

The authors declare no competing interests.

Supplementary materials

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