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The pecuniary and non-pecuniary returns to voucher-financed training

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Abstract. This paper analyzes the returns to training that was co-financed by the German voucher program *Bildungsprämie*. The estimation strategy compares outcomes of participants in voucher training with voucher recipients who intended to participate in training, but did not do so because of a random event like course cancellation by the provider of training. We find no impact of voucher training on wages, employment, job tasks and on subjective outcomes (in particular, the risk of job loss and job satisfaction). However, there is evidence that training participants report to better match the skill requirements of their job.

JEL classification: I22, I26, J24, M53

Keywords: Training, vouchers, returns to training, program evaluation

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1. Introduction

Recently, several European countries have introduced training vouchers at the federal or regional level that subsidize the costs of adult education with the aim of stimulating employees' training participation, for instance, Austria, Belgium, Germany, Italy and Switzerland (see e.g. OECD 2004). While there is a large literature analyzing the effects of training programs for the unemployed (see e.g. Card et al. 2010), little is known about the effectiveness of training vouchers for employed individuals. Schwerdt et al. (2012) find no impact of voucher training on earnings and employment analyzing a randomized field experiment in Switzerland. Hidalgo et al. (2014), whose analysis relies on data from a Dutch field experiment, also find no effects of voucher training on earnings and job mobility. In contrast, Singer and Toomet (2013) who apply a dynamic matching approach show that a training voucher for older workers introduced in Germany improves the employment stability for the elderly.

This paper investigates the short-run returns to training that was co-financed by a newly introduced large scale voucher program in Germany.[1] The analysis relies on data that was collected with the specific aim of program evaluation. Our first contribution is to provide further evidence on the effects of subsidized training on earnings and several employment and mobility indicators. This is not only important for evaluating the effectiveness of this specific voucher or voucher programs overall. It also expands our knowledge on the returns to on-the-job training in general. In the literature, the estimated wage returns to training vary dramatically. While some papers find extremely large returns to training that even exceed the returns to schooling (see e.g. Bartel 1995, Loewenstein and Spletzer 1999, Frazis and Loewenstein 2005), others find small returns (Brunello et al. 2012) or even zero returns to training (Kuruscu 2006, Leuven and Oosterbeek 2008, Görlitz 2011). The much smaller literature concerned with estimating the causal effects of training on employment find positive effects (see e.g. Picchio and van Ours 2011, Parent 1999). However, there are just too few studies to draw an overall conclusion.

Second, this paper also provides evidence on the effects of training on job tasks. This provides a more comprehensive picture of the pecuniary and non-pecuniary returns to training. To our

[1] Because the voucher program was intended to increase training participation of individuals with no required involvement of their employers, this paper is closest related to Schwerdt et al. (2012) and Hidalgo et al. (2014). Singer and Toomet (2013) investigate the effects of a training program that is directed to either individuals or employers and that requires employers to co-finance training by paying wages during training participation.

knowledge, we are the first to analyze how training affects job tasks. Job tasks could be affected because training might affect external mobility (as was shown in the literature mentioned above) or internal mobility such as promotions or upgrades (Melero 2010, Krueger and Rouse 1998). These are likely to come along with changes in workers' tasks. But even in the absence of mobility, training might influence the tasks workers are expected to perform within a given position in a firm.

Third, we investigate the effects of training on the self-assessed risk of job loss, job satisfaction and the extent to which employees' perceive their skills to match with the skill requirements of their job. The previous literature on the returns to training has widely neglected these outcomes.[2] There are at least two reasons why subjective outcomes might be affected by training: First, training could modify perceptions without really changing individuals' work productivity. This might be the case, for example, if the fact that individuals did participate in training already leads to a more optimistic assessment of the risk of job loss, one's job satisfaction or the skill match quality. Second, the human capital accumulated through training might indeed improve work productivity which employees realize right away affecting their subjective outcomes in the short-run. In this case, training should actually reduce the objective risk of job loss and/or improve the skill match.[3]

When estimating the returns to training, the identification strategy needs to take the selection into training into account. The empirical strategy used in this paper addresses this selectivity issue by comparing the outcomes of participants and a control group of non-participants who have the same characteristics and motivation to participate in training. In particular, the control group of non-participants is composed of those non-participants who intended to participate in training (as they applied for and received a voucher) but had to cancel their training plans due to a random event such as cancelation of the training course by the provider, an illness or a family-related reason. This approach was developed by Leuven and Oosterbeek (2008). It is similar to using no-shows who applied for the program (Bell et al. 1995), but it is refined since not showing up could be systematically related to unobserved heterogeneity. This problem is circumvented by restricting the control group only to individuals with training intentions who had to cancel training plans due to a random event.

[2] One exception is job satisfaction that was used e.g. in D'Addio et al. (2007) and Burgard and Görlitz (2014) who estimate correlations between training and job satisfaction, but fail to provide causal effects.

[3] A possible discrepancy with objective measures for short-run effects might be due to employers needing more time to observe changes in productivity than employees.

The results suggest that training that was co-financed by the voucher has no effect on wages, employment and job tasks in the short-run. The subjective outcomes job satisfaction and the self-assessed risk of job loss are also found to be unaffected by training. In contrast, there is some evidence that training participants are more likely to perceive their skills to better match with their jobs' skill requirements. Unfortunately, we are not able to further investigate whether this is due a change in individuals' perceptions with no further effects on actual work performance or whether it actually reflects human capital accumulation increasing work performance. However, it can explain why individuals do participate in training, even if the short-run wage and employment returns to voucher training are in fact zero as our findings and some of the above-mentioned literature suggests.

The paper is organized as follows. The next section describes the voucher program in detail. Section 3 presents the data and the empirical strategy. Section 4 provides the regression results for earnings and employment (4.1), job tasks (4.2), the self-assessed subjective outcomes (4.3) and the sensitivity analysis (4.4). The final section concludes the study.

2. The training voucher program

The training voucher program *Bildungsprämie* was introduced in Germany in December 2008. The aim of the program was to increase employees' training participation, to motivate them to finance lifelong learning activities (partly) on their own (and not to solely depend on their employers) and to improve individuals' employment prospects by means of training. Our analysis focuses on individuals who participated in the program in 2010. In 2010, the voucher reduced the direct training costs by 50% up to a maximum subsidy of 500 Euro per training course.[4] Direct costs cover fees for participation in training courses that were charged by the providers. The voucher could be used for training at the vast majority of the German training providers.

Eligibility was pegged to several criteria. First, the voucher was available only for low-income individuals who were either employed, on maternity or parental leave or a job-returnee. The income thresholds referred to (joint) taxable income and were 25,600 Euro per year for singles and 51,200 Euro for married couples. Almost two thirds of all employees in Germany (approx. 25 million) meet these income criteria. The unemployed were not eligible for the *Bildungsprämie* because other active labor market programs were available for them.

[4] As the voucher value and the eligibility criteria were changed occasionally since the introduction of the program, the following descriptions refer to the year 2010.

Second, the voucher program only subsidized work-related training that was not provided by the employer of the voucher recipient. Furthermore, training should not have started before the voucher was issued. Third, the direct training costs that remained after deducting the voucher had to be borne by the applicants themselves, i.e. the voucher could not be combined with other public subsidies. Finally, for each applicant the number of vouchers was restricted to one per year.

To obtain a voucher, individuals had to visit one of the 500 counselling offices that were widely spread all over Germany. The counselling served the purpose of verifying the eligibility criteria, recording the content of training on the voucher and issuing the voucher. When booking a course at a training provider, the voucher reduced the training fees for individuals immediately. Training providers were reimbursed by a governmental agency after submitting the voucher to the agency. In 2010, an overall of 63,000 training vouchers were issued (see RWI et al. 2012).

3. Data and empirical strategy

Data

The data was collected with the specific purpose to evaluate the voucher program (Görlitz and Tamm 2013). It covers voucher recipients who received their training voucher in 2010. Voucher recipients were interviewed by telephone. The first wave of interviews took place with around 5,050 individuals in 2010.[5] The second wave of interviews took place 12 months (± 2 months) after the first interview. For cost-efficiency reasons, the intended number of realized interviews was reduced to 2,210 in the second interview and the sample was stratified according to the status of voucher redemption at the time of the first interview. Specifically, recipients who had not redeemed their voucher at the time of the first interview were more likely to be surveyed in the second wave, while only a smaller number of recipients were sampled again who already had finished the voucher-financed training in the first wave. To adjust the results for this specific sample stratification, all descriptive and regression analyses in this paper use weighting factors.

In the first interview, information was collected on the redemption of the voucher, socio-demographic characteristics, the employment history of the previous two years as well as the current employment status and (if employed) the characteristics of the current job. Amongst

[5] Interviews were scheduled as short as possible after individuals had received the voucher. On average interviews took place around 6 weeks after voucher receipt.

others, job characteristics include job tasks that can be used to differentiate between routine manual, nonroutine manual, routine cognitive, nonroutine analytical and nonroutine interactive tasks as suggested by Spitz-Oener (2006).[6] Job characteristics also contain subjective measures like the perceived risk of job loss, the satisfaction with one's job and a question on whether employees' skills match with the requirements of their job.[7] In addition, information on previous participation in training and the number of training courses is also available in the first wave with a reference period of two years.

The second interview concentrated on updating information on voucher redemption, socio-demographics, the current employment status and current job characteristics. Training information and the employment history between the two interviews were recorded as well as information on whether the employer was changed. It also contained questions necessary for defining the treatment and the control groups. The treatment group includes individuals who used the voucher to participate in training, but whose training course had not already started before the first interview. This is because for individuals who had already started training, the first-wave information might already be affected by training participation and can, therefore, not be used as controls or in a difference-in-differences regression. We also exclude individuals from the treatment group who used the voucher for training that leads to a formal educational degree. This is necessary for reasons of comparison with the previous voucher literature that mainly looks at short training spells (Schwerdt et al. 2012, Hidalgo et al. 2014). Furthermore, obtaining a formal educational degree by means of training requires participation in a large number of training courses that are organized in modules. This is why it is unlikely that our identification strategy would be able to separately identify the effect of the voucher financed course from the effect of the other courses necessary to obtain the degree, especially since few individuals of the no-shows aimed at such a formal degree.

The control group includes individuals who obtained a voucher, but did not manage to redeem it until the time of the second interview because of a random event. These events include: a cancelation of the training course by the provider, a change of timing or location of the course by the provider, an illness, a family-related reason or not having enough time. Individuals with item nonresponse on the reasons or those with endogenous reasons (e.g. training was

[6] See Appendix A for a description on how the task categories were defined.

[7] In particular, the corresponding survey questions are: „How likely is it that you will lose your job in the next two years? 'very unlikely', 'rather unlikely', 'rather likely', 'very likely'" (coded from 1 to 4); „How satisfied are you with your current job?" (0 'not at all satisfied' to 10 'absolutely satisfied') and „How well do your skills match with the skill requirements in the job?" (0 'not at all' to 10 'absolutely').

considered unnecessary or because of fear of not being able to meet the requirements) are not considered for the analysis.

These restrictions reduce the number of individuals in our analysis sample from 2,210 to 1,116 individuals for whom information is available for the two panel waves.[8] 938 of them belong to the treatment group and 178 to the control group. As many as 106 individuals mention that the reason for non-participation was that the training was cancelled by the provider and 40 individuals report that the conditions of the course such as timing or location were changed unfavorably.

Tables 1 and 2 show that the treatment and the control group have similar characteristics in the first interview, i.e. at the time before the voucher was used for training participation. Table 1 compares the averages of 17 socio-demographic and work-related characteristics between the groups, revealing only two of them to be significantly different from each other. The treatment group is 1.7 years older on average and somewhat less likely to be married. Table 2 presents a comparison of pre-treatment variables, whose second-wave information is used as outcome variable in the regression. None of these 13 variables differs between treatment and control group at a statistically significant level of 10 percent or less. Overall, this indicates that the treatment and the control group are balanced which is evidence that the identifying assumption is likely to hold. Nevertheless, we generally control for differences in (pre-treatment) characteristics in the regression analyses.

[8] Out of the 2,210 individuals, 653 are dropped because training had already started before the first interview, 297 because they pursued a formal degree, 47 because the reason for not using the voucher was clearly endogenous, 74 because they did not use the voucher but did the training anyway (e.g. because the course was fully employer-financed) and 23 because of missing information on core socio-demographic variables (compare upper part of Table 1).

Table 1: Average characteristics of treatment and control group in the pre-treatment period

	Treatment group	Control Group	Difference	t-stat
Socio-demographics and education of all individuals				
Female	0.786	0.741	0.045	1.24
Age	38.166	39.823	-1.658 **	-2.14
Migrant	0.215	0.233	-0.018	-0.53
Married	0.499	0.571	-0.072 *	-1.72
Children	0.425	0.481	-0.056	-1.32
University and non-academic tertiary	0.335	0.317	0.018	0.45
Vocational education	0.640	0.652	-0.012	-0.29
Compulsory education	0.024	0.030	-0.006	-0.42
East Germany	0.224	0.222	0.002	0.05
Self-Employed	0.208	0.193	0.015	0.45
Observations	938	178		
Work- and firm-related characteristics of employees				
Part-time contract	0.388	0.440	-0.052	-1.17
Temporary contract	0.154	0.138	0.017	0.53
Tenure (in years)	5.649	6.385	-0.736	-1.18
Firm size <10	0.480	0.491	-0.011	-0.25
Firm size 10-49	0.246	0.237	0.009	0.24
Firm size 50-99	0.071	0.086	-0.014	-0.57
Firm size ≥ 100	0.203	0.186	0.017	0.48
Observations	849	161		

Notes: The first two columns represent group means. All variables are measured in the first interview in 2010. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Average outcomes of treatment and control group in the pre-treatment period

	Treatment group	Control Group	Difference	t-stat
Training, wages and employment				
Number of trainings in previous 2 years	2.593	2.892	-0.299	-0.81
Observations	938	175		
Gross monthly income	1387.664	1395.419	-7.755	-0.09
Observations	822	159		
Employed (y/n)	0.933	0.924	0.010	0.44
Observations	953	180		
Month in employment in previous 2 years	20.176	20.455	-0.279	-0.49
Observations	941	179		
Month in unemployment in previous 2 years	0.960	0.930	0.030	0.12
Observations	944	180		
Job tasks of employees				
Task index routine manual	0.074	0.090	-0.016	-0.83
Task index nonroutine manual	0.405	0.385	0.020	0.90
Taks index routine cognitive	0.249	0.288	-0.039	-0.96
Task index nonroutine analytic	0.394	0.416	-0.022	-0.80
Task index nonroutine interactive	0.488	0.483	0.005	0.24
Observations	849	161		
Subjective valuation of job loss, satisfaction and skill match				
Risk of job loss	1.867	1.953	-0.086	-1.09
Observations	833	159		
Job satisfaction	7.746	7.653	0.093	0.51
Observations	848	161		
Own skills match with job requirements	8.284	8.215	0.069	0.56
Observations	849	161		

Notes: The first two columns represent group means. All variables are measured in the first interview in 2010. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Empirical strategy

The returns to training are estimated by the approach developed by Leuven and Oosterbeek (2008) and applied e.g. by Görlitz (2011). The model is implemented by comparing the outcomes of the treatment group with the corresponding outcomes of the control group. The groups are defined as described in the former subsection. The survey questionnaire enables us to distinguish two types of outcome variables that need to be analyzed in different regression frameworks. First, some outcomes are only measured in the second wave, but not in the first wave. The second interview asked individuals about changes of characteristics or events that had occurred between the first and the second interview. In particular, this refers to the number of training courses taken, the number of months in employment or unemployment and the occurrence of job changes like leaving one's employer.[9] These variables are analyzed using the following cross-sectional regression model:

$$y_{i,\Delta t_2 t_1} = \alpha_1 + \beta_1 D_i + X_{i,t_1}{}'\gamma_1 + \delta I_{i,\Delta t_2 t_1} + \varepsilon_{i,\Delta t_2 t_1} \tag{1}$$

where $y_{\Delta t_2 t_1}$ is the outcome variable that refers to changes or events between the first interview in t_1 and the second interview in t_2 for individual i. The coefficient α_1 represents the constant and the binary variable D is 1 for the treatment group and 0 for the control group. The vector of control variables X_{t_1} includes socio-demographic factors (e.g. gender, age, migration background, East Germany, married, children), education and the employment status (being employed which is only incorporated when using the number of training courses as outcome, being self-employed), all of them measured in t_1. The variable $I_{\Delta t_2 t_1}$ indicates the time span (in months) between the first and the second interview. ε is the error term.

Second, the majority of outcome variables is contained in both waves and captures conditions at the time of the interview. This is the case for the gross monthly income, the current employment status and several objective and subjective job characteristics. For these outcomes, we apply two different identification strategies. On the one hand, the cross-sectional regression model

$$y_{i,t_2} = \alpha_2 + \beta_2 D_i + X_{i,t_1}{}'\gamma_2 + \nu_{i,t_2} \tag{2}$$

is estimated, where y_{t_2} refers to the outcome variable that is measured in t_2. α_2, D and X_{t_1} are defined similarly as in equation (1). The only difference is that when using job characteristics measured in t_2 as dependent variable, the set of control variables additionally includes job and firm characteristics (e.g. part-time job, temporary employment, tenure in years and firm size) measured in t_1. In this case and when looking at the employment status, of course, we do not control for the binary indicator of being currently employed due to collinearity issues. ν is the error term.

On the other hand, we exploit methods for panel data by estimating:

[9] Even though questions on the number of training courses and the months in employment and unemployment were posed in both interviews, they differ with respect to the reference period which precludes the application of fixed effects models.

$$y_{it} = \beta_3 D_{i,t} + X_{i,t}{'}\gamma_3 + \delta_t + \alpha_i + \omega_{i,t} \quad (3)$$

where y is the outcome variable measured at time t (with t=1, 2). X is a vector of covariates measured at the same point in time as the dependent variable, that is in t_1 or t_2. D is one for the treatment group in t_2 and zero otherwise, i.e. it identifies a difference-in-differences effect. δ_t measures time effects and α_i is an individual-specific intercept. It captures the impact of all observable and unobservable variables that are time-invariant and allows us to estimate unbiased effects even in the case when training participants differ from the control group in terms of ability, motivation or personality, as long as the impact of these factors is constant across waves. The idiosyncratic error term of the fixed effects model is denoted by ω.

Equations (1) and (2) are estimated using OLS and equation (3) using linear fixed effects models. In the sensitivity analysis, we provide estimates of models for non-linear outcomes, for example, for count data, binary dependent variables and ordered dependent variables.

4. Results

4.1 Training participation, wages and employment

First, we present the results on the impact of voucher-financed training on training participation, wages and employment, i.e. on the outcomes that were already used in the previous voucher literature (see e.g. Schwerdt et al. 2012, Singer and Toomet 2013, Hidalgo et al. 2014). Table 3 (column 1) shows results on training participation according to which the treatment group has participated in almost one more training course (0.94) than the control group. This is similar to the difference in the unconditional mean of the number of training courses between the first and the second interview which is 2.34 for the treatment group and 1.39 for the control group. This difference is significantly different from zero but indistinguishable from one.[10] It corroborates how well-chosen the treatment and the control groups are, because they differ in training participation by one course, but not in terms of average characteristics measured in the first interview (cf. Tables 1 and 2).

Columns (2) and (3) of Table 3 document the effect of training on gross monthly income using the identification strategies described in equations (2) and (3), respectively.

[10] Note that we do not interpret this difference as the long-term effect of the voucher on training because we cannot rule out that the control group only delayed participation and will catch up on the cancelled training in the longer run. Therefore, we are not able to assess the existence and magnitude of the deadweight loss of the voucher program by the empirical method used in this paper.

Specifically, we use gross monthly labor income which is set to zero for unemployed and non-employed individuals. The results show that there is no statistically significant impact on income in either of the two specifications. The point estimates are negative and small in economic terms: They range from –4 Euro in the cross-sectional comparison to –15 Euro in the fixed effects specification. Compared to the average monthly income of 1,388 Euro in the pre-reform period (see Table 2), this relates to a size of –0.03 percent and –1 percent, respectively.

One concern when estimating wage equations is that there is non-random item nonresponse. In fact, around 13 percent of the survey respondents did not report information on their monthly earnings. To reduce item nonresponse in the data, respondents who refused to report earnings information were additionally asked to indicate whether their earnings fall into one of the following seven income brackets: less than 500, 500-999, 1,000-1,499, 1,500-1,999, 2,000-2,499, 2,500-4,999, 5,000 Euro or more. As a robustness check, we imputed earnings by replacing the missing information with the mean of the individual's income bracket which reduces item nonresponse from 13 to only 5 percent. Using imputed earnings as dependent variable confirms the main conclusion that training has no significant effect on wages.

The majority of papers do not analyze labor income that is set to zero for the unemployed or the non-employed as dependent variable as we do. Instead they restrict the wage regressions to employed individuals only to analyze training effects on log hourly wages. We do not follow this strategy in our main specification, because the employment status could potentially be influenced by training. Furthermore, such a regression leads to a dramatic decrease of the sample size (in our case by almost 40 percent), because of missing information on earnings and the working hours and because of the fact that not all individuals are regularly employed in both panel waves. To compare our results with the majority of the literature, we nevertheless analyze the effect of training on the log of gross hourly wages. Again, the effects are found to be statistically insignificant.

Table 3: Treatment effects on training participation and wages

	Number of training courses between 1st and 2nd interview	Gross monthly income in 2nd interview	Gross monthly income
	(1)	(2)	(3)
Treatment effect	0.9408 ***	-3.9999	-15.4686
	(0.1715)	(86.1142)	(53.9793)
Socio-demographics	Yes	Yes	Yes
Education	Yes	Yes	Yes
Employment status	Yes	Yes	Yes
Time between interviews	Yes	No	No
Individual fixed effects	No	No	Yes
Observations	1,114	968	1,944
R^2	0.0349	0.2397	0.1443

Notes: The dependent variables are indicated in the first row. Column (1) is estimated according to equation (1), column (2) according to equation (2) and column (3) according to equation (3). The control variables include socio-demographics (gender, age, migration background, East Germany, married, children), education, the employment status (being employed and being self-employed) and the time between the interviews (in months). The standard errors are shown in parentheses. Standard errors are robust (equations (1) and (2)) and account for clustering at the individual level (equation (3)). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4. Treatment effects on employment and job changes

	Months in employment between 1st and 2nd interview	Employed (y/n) in 2nd interview	Employed (y/n)	Months in unemployment between 1st and 2nd interview	Unemployed (y/n) in 2nd interview	Unemployed (y/n)	Change of job or employment between 1st and 2nd interview
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment effect	0.0179	0.0041	-0.0063	-0.0460	-0.0182	-0.0064	-0.0061
	(0.1638)	(0.0205)	(0.0273)	(0.0963)	(0.0166)	(0.0195)	(0.0376)
Socio-demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Education	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Employment status	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time between interviews	Yes	No	No	Yes	No	No	Yes
Individual fixed effects	No	No	Yes	No	No	Yes	No
Observations	1,114	1,116	2,234	1,114	1,115	2,233	1,116
R^2	0.0988	0.0180	0.0080	0.0123	0.0116	0.0008	0.0219

Notes: The dependent variables are indicated in the first row. Columns (1), (4) and (7) are estimated according to equation (1), columns (2) and (5) according to equation (2) and columns (3) and (6) according to equation (3). The control variables include socio-demographics (gender, age, migration background, East Germany, married, children), education, employment status (being self-employed) and the time between the interviews (in months). The standard errors are shown in parentheses. Standard errors are robust (equations (1) and (2)) and account for clustering at the individual level (equation (3)). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 displays the effects of the voucher training on employment outcomes. None of the considered employment variables is statistically significantly influenced by training. Furthermore the size of the coefficients is always small. For instance, column (1) reveals that the average employment duration between the first and the second interview is only half a day longer for the treatment group than it is for the control group (0.0179 × 30 days). With respect to the days in unemployment, column (4) shows that the average unemployment duration is one and a half days shorter for the treatment group (–0.0460 × 30 days). Column (7) of Table 3 provides results for a summary measure on any employment change. The variable equals one if there is any change between the first and second interview of the employment status (i.e. from being employed to being un-/non-employed and vice-versa) or of the self-employment status or if the individual reports to having changed his or her employer. Thus, it can be interpreted as a measure of external mobility. The treatment effect is also small for this variable (–0.6 percentage points).

4.2 Job tasks

This subsection presents the effects of voucher-financed training on job tasks in order to obtain a more comprehensive picture of the non-pecuniary returns to training. Even though the voucher training did not change external mobility – as was shown above – job tasks could have changed nevertheless, e.g., if the voucher training affects internal mobility like promotions or organizational changes in the jobs workers perform. Table 5 depicts the results of training on changes in the composition of job tasks. As already mentioned, we follow Spitz-Oener (2006) and differentiate between routine manual, nonroutine manual, routine cognitive, nonroutine analytic and nonroutine interactive tasks (see Appendix A for a more detailed description). Table 5 indicates that there are no significant effects for any of the five task categories, neither when using a cross-sectional comparison based on equation (2) nor when using a fixed effects specification based on equation (3).

Table 5. Treatment effects on job tasks

	Routine manual tasks in 2nd interview	Routine manual tasks	Nonroutine manual tasks in 2nd interview	Nonroutine manual tasks	Routine cognitive tasks in 2nd interview	Routine cognitive tasks	Nonroutine analytic tasks in 2nd interview	Nonroutine analytic tasks	Nonroutine interactive tasks in 2nd interview	Nonroutine interactive tasks
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment effect	-0.0164	-0.0124	0.0144	-0.0042	-0.0158	0.0342	0.0131	0.0436	-0.0033	0.0000
	(0.0187)	(0.0172)	(0.0221)	(0.0203)	(0.0400)	(0.0479)	(0.0288)	(0.0279)	(0.0209)	(0.0221)
Socio-demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Education	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Employment variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual fixed effects	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	956	1,906	956	1,906	956	1,906	956	1,906	956	1,906
R^2	0.0743	0.0042	0.0399	0.0081	0.0435	0.0032	0.0684	0.0158	0.0688	0.0029

Notes: The dependent variables are indicated in the first row. Columns (1), (3), (5), (7) and (9) are estimated according to equation (2) and columns (2), (4), (6), (8) and (10) according to equation (3). The control variables include socio-demographics (gender, age, migration background, East Germany, married, children), education and employment variables (being self-employed, part-time, temporary contract, tenure in years, firm size in categories). The standard errors are shown in parentheses. Standard errors are robust (equation (2)) and account for clustering at the individual level (equation (3)). Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

4.3 Fear of job loss, job satisfaction and skill requirements

Apart from the returns to training that are measurable by objective outcomes, training could also change how employees value and perceive aspects of their job. Columns (1) and (2) of Table 6 present the results for individuals' perceived risk of job loss where higher values indicate a higher risk. The point estimates have the expected negative sign given that one would assume that training decreases the risk of job loss. However, the estimates are insignificant and the size of the effects is small compared to the standard deviation of the variable which is 0.83 in the treatment group in the first wave. In fact, the size of the effect is –0.05 of a standard deviation (–0.0448/0.83 or –0.0410/0.83). With regard to job satisfaction (which has a higher value for higher satisfaction), columns (3) and (4) of Table 6 similarly show that results are statistically and economically insignificant. The effect size ranges between –0.015 and 0.005 of a standard deviation (which is 2.0 for the treatment group in the pre-treatment period).

Table 6. Treatment effects on subjective outcomes

	Risk of job loss in 2nd interview	Risk of job loss	Job satis-faction in 2nd interview	Job satis-faction	Own skills match job re-quirements in 2nd interview	Own skills match job re-quirements
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment effect	-0.0448	-0.0410	0.0103	-0.0301	0.1814 *	0.1981 *
	(0.0728)	(0.0723)	(0.1605)	(0.1577)	(0.1079)	(0.1178)
Socio-demographics	Yes	Yes	Yes	Yes	Yes	Yes
Education	Yes	Yes	Yes	Yes	Yes	Yes
Employment variables	Yes	Yes	Yes	Yes	Yes	Yes
Individual fixed effects	No	Yes	No	Yes	No	Yes
Observations	949	1,883	956	1,906	956	1,906
R^2	0.0556	0.0010	0.0845	0.0067	0.0386	0.0037

Notes: The dependent variables are indicated in the first row. Columns (1), (3) and (5) are estimated according to equation (2) and columns (2), (4) and (6) according to equation (3). The regressions are estimated by OLS. The control variables include socio-demographics (gender, age, migration background, East Germany, married, children), education and employment variables (being self-employed, part-time, temporary contract, tenure in years, firm size in categories). The standard errors are shown in parentheses. Standard errors are robust (equation (2)) and account for clustering at the individual level (equation (3)). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Finally, columns (5) and (6) of Table 6 contain the effects of training co-financed by the voucher on the match between respondents' skills and their job requirements. A higher value indicates a better match of skills and requirements. In both specifications, there is a positive effect which is weakly statistically significant. It indicates that the training increased the match between skills and requirements. The effect is equivalent to an improvement by almost a sixth of a standard deviation of the variable in the treatment group in the first wave (0.1814/1.25 and 0.1981/1.25).

The findings show that training has the potential to improve the subjective perception of the match between own skills and job requirements. This result is in accordance with individuals' answers to the question about the main objective followed by training participants in the *Bildungsprämie* program. Descriptive statistics on the objectives (with multiple answers possible) show that the most frequently mentioned are: improving one's professional capacity (95 percent), coping with new job requirements (88 percent) and refreshing working skills and knowledge (87 percent). Other objectives such as increased earnings or preparation for a job change are mentioned less often by participants (64 percent and 44 percent, respectively).

4.4 Sensitivity analysis

The sensitivity analysis probes the robustness of our results with respect to several aspects. First, since some of the outcomes are not inherently continuous, more adequate models for binary, censored, count or ordinal outcomes are estimated. The number of training courses is re-estimated by a negative binomial model. The binary outcome variables being employed, being unemployed and having experienced any employment change are re-estimated by a Probit model when using a cross-sectional comparison and by a Logit fixed effects model when exploiting the longitudinal dimension of the data. The months in employment and unemployment are re-estimated by a Tobit model. For the subjective outcomes risk of job loss, job satisfaction and the match between skills and job requirements, the ordered Probit model is used for estimating cross-sectional comparisons and the "blow-up and cluster" (BUC) estimator (Baetschmann et al. 2014) is applied to account for fixed effects. This does not change any of the results. For those outcomes where results are statistically significant in the main analysis, the regression results are contained in Table 7. It can be seen that the results are statistically significant and the size of the coefficients is also similar to the main results.

Table 7. Sensitivity analyses using models for count data and ordered outcomes

	Number of training courses between 1st and 2nd interview	Own skills match job requirements in 2nd interview	Own skills match job requirements
	(1)	(2)	(3)
Treatment effect	0.9444 ***	0.1658 *	0.5455 *
	(0.1689)	(0.0971)	(0.2800)
Socio-demographics	Yes	Yes	Yes
Education	Yes	Yes	Yes
Employment status	Yes	Yes	Yes
Time between interviews	Yes	No	No
Individual fixed effects	No	No	Yes
Observations	1,114	956	1,534

Notes: The dependent variables are indicated in the first row. Column (1) is estimated according to equation (1) by the negative binomial regression model. Column (2) is estimated according to equation (2) using the ordered Probit model. Column (3) is estimated by equation (3) applying the BUC-estimator (Baetschmann et al. 2014). Standard errors are shown in parentheses. Standard errors are robust (equations (1) and (2)) and account for clustering at the individual level (equation (3)). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Second, we check the robustness of the results to using alternative definitions of the control group. While a cancelation of the training course or changes of timing/location by the training provider are very likely exogenous reasons, this is not necessarily true for health-, family- and time-related reasons. Therefore, all regressions are re-estimated using as control group only those individuals who could not realize their training plans because of course cancellations or changes induced by the provider of training. This reduces the control group from 178 to 146 individuals. The results confirm the significantly positive effect on the number of training courses and all insignificant results (not shown). However, the skill match quality is no longer statistically significantly affected by voucher training in either of the two specifications. The size of the effects decreases slightly from 0.18 to 0.17 when estimating equation (2) and from 0.20 to 0.12 when estimating equation (3).

5. Conclusion

This paper estimates a variety of pecuniary and non-pecuniary returns to training that was co-financed by a training voucher. In particular, wages, employment, job tasks, the risk of job loss, job satisfaction and the skill match are considered as outcomes. The empirical strategy follows Leuven and Oosterbeek (2008) and compares the outcomes of training participants with a subgroup of non-participants who intended to participate in training, but had to cancel their training plans due to a random event. Comparing average characteristics of training participants and the subgroup of non-participants reveals that Leuven and Oosterbeeks' approach is able to identify a treatment and a control group that are similar in a variety of characteristics, but differ in their training activities. The results indicate none of the considered outcomes to be significantly affected by training participation. The only exception is a weakly positive effect on the self-assessed match between employees' skills and the skill requirements of the job, even though this effect is not entirely statistically robust.

The insignificant results of voucher training on wages and employment corroborate the results of Schwerdt et al. (2012) and Hidalgo et al. (2014). Compared to the results for training in general, i.e., training that is not necessarily (co-)financed by a voucher, our results are also in line with the more recent literature estimating zero wage returns to training (Kuruscu 2006, Leuven and Oosterbeek 2008, Görlitz 2011). However, they contrast to the positive effects of training on employment found by Picchio and van Ours (2011), Parent (1999) and Singer and Toomet (2013). One reason for the difference in the results could be that these trainings are more often co-financed by one's employer which contrasts to voucher-financed training where firms are not involved to a large extent because finance is provided by the voucher. While a goal of any training with employer-involvement needs to be job stability (otherwise firms would not invest), the opposite could be the case at least for some participants in voucher training whose training objective is to get prepared for leaving their employer by changing jobs. Thus, the short-run effect of voucher training on the employment stability would be heterogeneous pooling together positive and negative effects which could explain an insignificant overall effect.

The weakly positive effect of training on the skill match might result from an actual improvement of employees' stock of human capital. However, since we have no objective measure of the stock of human capital, we can't rule out that the positive effect is simply due to subjective illusion. This notwithstanding, even if the return simply reflects subjective perceptions, it would provide an explanation for employees' training investments. The zero wage returns to training found in many analyses would otherwise raise the question why individuals invest in training after all. Our finding might explain this behavior.

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Appendix A

The tasks indices were defined similar to Spitz-Oener (2006) who distinguishes five categories of working tasks: routine manual, nonroutine manual, routine cognitive, nonroutine analytical and nonroutine interactive tasks. The job tasks are not assigned on the basis of occupations, but capture employee-specific information on their work activities. In the interview, employees were asked for twelve different work activities how often they are performed on a regular working day (answers possible: frequently, occasionally or never). Table A-1 documents the twelve activities and shows how they are assigned to the task categories. The definition of the task index for each category j is based on the number of activities performed frequently:

$$\text{Task}_{ji} = \frac{\text{Number of activities in category j frequently performed by worker i}}{\text{Total number of activities in category j}}.$$

Table A-1. Assignment of work activities to task categories

Task category	Activities
Routine manual	Fabricating and producing goods; Supervising and controlling machines
Nonroutine manual	Repairing and patching; Nursing, serving and healing
Routine cognitive	Measuring, controlling and quality checks
Nonroutine analytic	Developing and researching; Gathering information and investigating
Nonroutine interactive	Informing and advising; Training, teaching and educating; Organizing and planning; Negotiating; Buying, providing and selling

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