View metadata, citation and similar papers at core.ac.uk

brought to you by CORE

provided by Research Papers in Economics

RUHR
ECONOMIC PAPERS

Katja Görlitz

Continuous Training and Wages

An Empirical Analysis Using a Comparison-group Approach

rwi

#197

Imprint

Ruhr Economic Papers

Published by

Ruhr-Universität Bochum (RUB), Department of Economics
Universitätsstr. 150, 44801 Bochum, Germany

Technische Universität Dortmund, Department of Economic and Social Sciences
Vogelpothsweg 87, 44227 Dortmund, Germany

Universität Duisburg-Essen, Department of Economics
Universitätsstr. 12, 45117 Essen, Germany

Rheinisch-Westfälisches Institut für Wirtschaftsforschung (RWI)
Hohenzollernstr. 1-3, 45128 Essen, Germany

Editors

Prof. Dr. Thomas K. Bauer
RUB, Department of Economics, Empirical Economics
Phone: +49 (0) 234/3 22 83 41, e-mail: thomas.bauer@rub.de

Prof. Dr. Wolfgang Leininger
Technische Universität Dortmund, Department of Economic and Social Sciences
Economics – Microeconomics
Phone: +49 (0) 231/7 55-3297, email: W.Leininger@wiso.uni-dortmund.de

Prof. Dr. Volker Clausen
University of Duisburg-Essen, Department of Economics
International Economics
Phone: +49 (0) 201/1 83-3655, e-mail: vclausen@vwl.uni-due.de

Prof. Dr. Christoph M. Schmidt
RWI, Phone: +49 (0) 201/81 49-227, e-mail: christoph.schmidt@rwi-essen.de

Editorial Office

Joachim Schmidt
RWI, Phone: +49 (0) 201/81 49-292, e-mail: joachim.schmidt@rwi-essen.de

Ruhr Economic Papers #197

Responsible Editor: Christoph M. Schmidt

All rights reserved. Bochum, Dortmund, Duisburg, Essen, Germany, 2010

ISSN 1864-4872 (online) – ISBN 978-3-86788-222-4

The working papers published in the Series constitute work in progress circulated to stimulate discussion and critical comments. Views expressed represent exclusively the authors' own opinions and do not necessarily reflect those of the editors.

Ruhr Economic Papers #197

Katja Görlitz

Continuous Training and Wages

An Empirical Analysis Using a Comparison-group Approach

Bibliografische Informationen der Deutschen Nationalbibliothek

Die Deutsche Nationalbibliothek verzeichnet diese Publikation in der deutschen Nationalbibliografie; detaillierte bibliografische Daten sind im Internet über: *http://dnb.d-nb.de* abrufbar.

ISSN 1864-4872 (online)
ISBN 978-3-86788-222-4

Katja Görlitz[1]

Continuous Training and Wages – An Empirical Analysis Using a Comparison-group Approach

Abstract

Using German linked employer-employee data, this paper investigates the impact of on-the-job training on wages. The applied estimation technique was first introduced by Leuven and Oosterbeek (2008). The idea is to compare wages of employees who intended to participate in training but did not do so because of a random event with wages of training participants. The estimated wage returns are statistically insignificant. Furthermore, the decision to participate in training is associated with sizeable selection effects. On average, participants have a wage advantage of more than 4% compared to non-participants.

JEL Classification: J24, J31

Keywords: Continuous training; wage returns; selection effect

July 2010

---

1 RWI. *The author is grateful to Thomas Bauer, Michael Fertig, Bernd Fitzenberger, Sandra Schaffner, Christoph Schmidt, Marcus Tamm and Andrea Weber for helpful comments and suggestions. Financial support from the "Leibniz Gemeinschaft" is gratefully acknowledged. All remaining errors are my own.* *– All correspondence to Katja Görlitz, RWI, Hohenzollernstr. 1–3, 45128 Essen, Germany, e-mail: katja.goerlitz@rwi-essen.de.*

1. Introduction

Estimating wage returns to on-the-job training is important because it reveals e.g. the potential of training to boost labor productivity as hypothesized in the human capital theory (Becker 1962). Even though a variety of empirical studies were already involved in estimating this effect, no final conclusions can be drawn because of ambiguous findings. This ambiguity can be explained by the use of different estimation techniques. Average wage differentials between training participants and non-participants estimated by standard Mincer-type wage equations extended with training measures are quite high (Parent 1999, Loewenstein and Spletzer 1999, Goux and Maurin 2000, Muehler et al. 2007). In some studies, they are even higher than wage returns to schooling (Schøne 2004). As training courses are often of short duration, these estimates appear to be too high to represent the causal effect of training. It is rather likely that these wage differentials encompass differences in unobserved characteristics between participants and non-participants.

One way to reduce selection bias is to apply individual fixed effects models that control for time-invariant omitted variables. Because the application of individual fixed effects produces much lower and more credible estimates (Lynch 1992, Pischke 2001, Schøne 2004, Frazis and Loewenstein 2005), controlling for unobserved heterogeneity matters when estimating training returns. Although these estimates account for time-invariant omitted variable bias, they are still biased when training participants and non-participants differ in wage growth rates. Evidence in favor of this hypothesis is provided in Pischke (2001) and Frazis and Loewenstein (2005). An alternative approach is to use IV or selection models that both require an instrument for training participation (Lynch 1992, Parent 1999, Arulampalam and Booth 2001, Kuckulenz and Maier 2006). The difficulties with using these models are that exclusion restrictions are hard to find and that different instruments can lead to different estimates in the case of heterogeneous treatment effects (Ichino and Winter-Ebmer 1999).

Another approach to estimate training returns that neither relies on panel data nor on exclusion restrictions was suggested by Leuven and Oosterbeek (2008), henceforth LO. This approach defines a group of non-participants that is assumed to have similar characteristics as the group of participants. Individuals who intended to participate in one training course but did not do so because of a random event are considered an appropriate comparison group to training participants. Cancelling a course because of family circumstances or illness are examples of random events. Using Dutch data, the authors find insignificant results for participating in one course. This approach was not yet replicated often in the literature because it requires specific questions in the questionnaire that are usually not available.

Using an innovative linked-employer-employee training data set, this paper applies the comparison-group approach of LO to identify the causal effect of continuous training on wages in Germany. The approach is extended in some respects. First, besides providing estimates of the impact of participating in *one course*, a possibility to analyze the impact of participating in *a second* and *a third course* is presented. This is important as training participants often attend more than one course. In addition, the number of courses seems to matter for wage growth in Germany (Büchel and Pannenberg 2004).

Second, while the previous literature finds that employees self-select themselves into training, there is no information on the size of this selection effect. This will be obtained by comparing wages of the group of non-participants that cancelled training plans with wages of the group of non-participants that had no training intentions. Since both groups did not participate in training, a resulting wage difference has to be ascribed to initial differences in unobservables. Third, employer characteristics will be controlled, in particular, by applying firm fixed effects. Despite the fact that the majority of studies account for at least some firm attributes such as size or industry, only few have access to more detailed information to account for a larger set of firm attributes. However, it was shown that controlling for a broader set of firm attributes can change the results substantially (Goux and Maurin 2000).

The paper is organized as follows. The next two sections present the linked employer-employee data in detail and the empirical strategy. Section 4 provides the regression results and section 5 concludes the study.

2. Data

The analysis is based on the German linked employer-employee data set "WeLL" that was particularly designed to analyze further training activities of individuals. The first wave of the data is used covering 6,404 employees who were employed in December 31, 2006 in one of

149 establishments that were selected for the survey.[1] The following selection criteria applied. Establishments with more than 100 and fewer than 2000 employees that operate in the manufacturing or service sector were considered. This sampling frame of interviewing many workers out of a limited number of firms has the advantage that firm fixed effects can be applied, i.e. the firm-specific environment can be kept constant when analyzing training processes. The employees were interviewed via telephone from October 2007 to January 2008. With respect to training, individuals were asked whether they have participated in training during the last two years, i.e. from January 2006 to the time of the interview. Even though the data also contains questions on less formal learning activities (such as attending conferences or instructions by colleague), this study focuses on "class-room" training like courses, seminars or lectures.[2]

One feature of the data is that wages can be merged from administrative sources of the social security system if respondents gave their permission which applied to more than 90%. Fewer than 10% who did not agree on merging were asked about their current wages in the questionnaire. Due to this procedure, there are only few respondents with missing information on gross monthly wages. Using data from administrative sources has the advantage that wages are unaffected by measurement error. The disadvantage is that wages taken from the social security system are right censored in Germany because they are only reported up to the contribution limit. This is why methods accounting for censored data have to be applied in the empirical strategy.

For the analysis, some data restrictions were made. First, individuals who are unemployed or nonemployed during the time of the interview are deleted which is the case for 3% of observations in the sample. Second, observations with missing information on wages are also excluded which applied to 2% of the initial sample. Furthermore, observations with monthly wages of less than 500 Euros and with more than 20,000 Euros are excluded as well. While the latter restriction eliminates very few outliers, the former reduces the sample by less than 2% which was necessary to delete marginally employed who might exhibit different training patterns and processes. Last, only observations with no item nonresponse on core variables are incorporated which decreased the sample size by another 2%. The final sample is

[1] In the following, no distinction between firms and establishments will be made; both are referred to as firms.
[2] See Bender et al. (2009) for further information on the data set.

composed of 5829 persons. A definition of variables and sample means is presented in the Appendix Table A-1.

3. Empirical Method

The idea of the approach of LO is to narrow down the comparison group of non-participants into a sub-group that is similar to the group of training participants. This is assumed to be the case for persons who intended to participate in training but cancelled due to some random event (also referred to as would-be participants in the following).[3] The identifying assumption is that the characteristics of would-be participants and training participants are identical. The treatment effect on the treated is obtained by comparing wages of these two groups conditional on control variables.

In the WeLL data, the question to identify would-be participants is: "Did you intend to participate in training courses, seminars or lectures in the last two years without realizing this plan?" It is crucial that the reasons for non-participation are random because otherwise selection bias could contaminate the results. Employees cancelling a course because of high training costs are probably not comparable to training participants. Therefore, respondents were asked for the reasons of cancelling their plans. When reporting that the course was cancelled by the organizer or when reporting family or health reasons or reasons related to the job (high work load), this is regarded as random in the following.

Of course, one can argue that these reasons are not random. In particular, it could be shown that health has an impact on wages in Germany (Jäckle and Himmler 2010). This is why the results of this paper are only unbiased, if health reasons refer to transient rather than to permanent and serious health shocks. This seems to be the case as sensitivity checks excluding health as exogenous event show that the main results remain unchanged. Although the most convincing exogenous reason, cancelled by the organizer, was reported frequently, it was not reported frequently enough to define the comparison group only on the basis of this

[3] The approach is similar to using no-shows or early program dropouts as comparison group in the literature of program evaluation (see e.g. Bell et al. 1995). The difference is that LO distinguish the reasons for non-participation. They show that only those persons who cancelled because of an exogeneous reason are an appropriate comparison group to obtain unbiased estimates.

question.[4] Having said this, a test for balancing the characteristics of would-be participants and training participants shows that observables are similar. This indicates that the random events chosen for this paper are appropriate.[5]

While LO focus on the impact of participating in one training course, a way to analyze the wage impact of the second and third course is suggested and implemented in this paper. This is done by defining separate treatment and comparison groups for participating in one course, for the second and for the third course. The reason why this can be done is that the question on cancelling courses is not only posed to non-participants in the WeLL data but also to participants. An additional extension of the model of LO is to determine the "selection effect of the training decision" which measures differences in unobservable characteristics between training participants and non-participants. These unobservable characteristics could represent, for example, the return to innate ability or motivation.

In the WeLL data, nine groups of training participants are classified according to employees' training attendance and intentions (see Table 1). There are 5,829 employees in the sample, of whom 1,809 are non-participants with no training intentions (31%). There are 150 non-participants having training intentions (3%) and 1,603 persons having participated in exactly one course (28%). To evaluate the impact of the first training course, wages of these two groups are compared. For the effect of the second course, wages of 185 persons who attended one course and cancelled a second one (3%) are compared with wages of 881 persons who participated in exactly two courses (15%). 170 persons with two courses and intentions for a third one (3%) are compared with 377 persons attending three courses (6%). The remaining two groups (i.e. 467 persons with more than three courses, 187 cancelling due to non-random reasons) are not used for identification of the effect.

[4] Cancelled by the organizer and job-related reasons are reported most often (both in the same magnitude). A fewer number of persons indicated family or health reasons.

[5] When using all reasons including non-random reasons to define the comparison group, the balancing test shows that they are also similar to the treatment group in terms of observable characteristics. However, the comparison group defined on the basis of random events is even more similar and will, therefore, be in the focus of this paper.

Table 1: Training attendance of employees within the last 2 years

Training Group	Obs.
No training participation (tr_1)	1,809
No training participation, but intended to participate in one course (tr_2)	150
Training participation in only one course (tr_3)	1,603
Training participation in only one course and intended to participate in a second course (tr_4)	185
Training participation in exactly two courses (tr_5)	881
Training participation in two courses and intended to participate in a third course (tr_6)	170
Training participation in exactly three courses (tr_7)	377
Training participation in more than three courses or intended to do fourth course (tr_8)	467
Employees cancelling training plans due to non-random reasons (regardless of actual participation) (tr_9)	187
Total	5,829

The empirical strategy is implemented as follows:

$$\ln(wage)_{ij} = \alpha_0 + \sum_{k}^{K} tr_{ijk}\beta_k + X_{ij}'\gamma + \alpha_j + \varepsilon_{ij} \quad with\ k = 2...9$$

where ln(wage) represents log gross monthly wage of individual *i* employed in establishment *j*. The dummy variables tr_k represent the nine training groups that were already described in Table 1 (tr_1 serves as the base group). The reason for considering all training groups in a joint regression rather than running separate regressions is to increase the size of the sample. The variables of interest are tr_3-tr_2 for participating in one course, tr_5-tr_4 for the second course and tr_7-tr_6 for the third course. Whether these deviations differ from zero on a statistically significant level is tested by an F-test. The selection effect of the training decision is revealed by the coefficient on tr_2. The vector *X* contains control variables including socio-demographic, occupational and job characteristics. A complete list of characteristics is provided in the Appendix Table A-1.

The establishment-specific time-invariant effect is captured by α_j which is necessary to avoid biased results, for instance, if firms that sponsor training also pay higher wages. This could be the case, for example, for firms with a higher degree of technology use and better technological equipment or with more complex working tasks. It is also plausible that firm effects correlate with the probability to cancel training intentions, especially in the case of job-related reasons. A prerequisite when applying firm fixed effects is that for every firm more than one employee is observed; otherwise these observations are not used for identification. This is very unlikely to hold for movers who leave their firm and start a job in a new firm. As movers might differ in terms of training and wage patterns, excluding them from the sample might induce a sample selection bias. To tackle this problem, an identical firm fixed effect is generated for the 105 movers in the data, i.e. they are treated as if all of them switched to the same firm.[6]

Because of the fact that wages are right-censored in the data, implementing OLS would produce biased estimates. To address the censoring issue, two approaches are applied. First, wages are imputed as suggested in Gartner (2005). These imputed wages enter the OLS regression framework as dependent variable. Second, a Tobit model using the social security contribution limit of wages as right-censoring point is estimated.[7] Unlike Probit models, introducing fixed effects as slope coefficients along with covariates does not lead to biased coefficients in the Tobit model (Greene 2004). Instead the fixed effects Tobit model has inflated marginal effects and downward biased standard errors that both improve when the number of observations per fixed effect increases. In the WeLL data, the average number of employees per firm fixed effect is 39 which I consider as sufficiently large to apply the Tobit fixed effects model. Using two approaches to account for censoring has the advantage of checking the robustness of the results.

The crucial assumption for identification is that characteristics between treatment and comparison group are similar. This can only be tested empirically for differences in observable characteristics while there is no test for differences in unobservables. Besides the variables that enter the regression as covariates, log wages previous to the reference period of the training questions can also be compared for persons who permitted merging administrative data. If there are differences in unobservable characteristics that cannot be included in the balancing, they should be reflected by past wage differentials. In addition, this also tests directly whether there are differences in past wage growth rates between treatment and comparison groups as suggested by Pischke (2001). Therefore, no differences in past wages are seen as an important prerequisite to obtain unbiased estimates. The balancing tests between treatment and comparison groups are presented in Table 2.

[6] Thanks to Petra Todd for this suggestion.
[7] Since the limits differ by West and East Germany, the lower of the two (i.e. the East German limit) is used as upper limit for the whole sample. By doing so, 20% of all observations are treated as censored.

When comparing characteristics of participants in one course with their comparison group (column 1), there are no statistically significant differences with the exception of age. Column 2 documents results for treatment and comparison group of attending a second course. Age and children differ significantly and, most importantly, there are significant differences in log wages in 2005. This suggests that treatment and comparison groups are not as equal as necessary for identifying unbiased estimates. In column 3, age is significantly smaller and the percentage of white collar workers is significantly larger in the comparison group. None of the other characteristics differ on a statistically significant level. In conclusion, while treatment and comparison group for the first and third course match well, there is weaker evidence for the second course.

Table 2: Balancing between treatment and comparison groups

	Employees with one course (tr_3) versus those willing to attend one course (tr_2) (1)				Employees with two courses (tr_5) versus those with one course willing to attend another (tr_4) (2)				Employees with three courses (tr_7) versus those with two courses willing to attend another (tr_6) (3)			
	tr_3	tr_2	$\Delta_{tr3-tr2}$	t-value	tr_5	tr_4	$\Delta_{tr5-tr4}$	t-value	tr_7	tr_6	$\Delta_{tr7-tr6}$	t-value
Male	0.66	0.67	-0.01	-0.27	0.65	0.67	-0.02	-0.61	0.58	0.62	-0.04	-0.94
German	0.96	0.96	0.00	-0.18	0.96	0.94	0.03	1.36	0.97	0.95	0.02	0.82
Age	45.47	43.61	1.85	2.47 **	44.65	43.42	1.24	1.77 *	44.10	42.61	1.49	1.79 *
Married	0.75	0.74	0.01	0.18	0.74	0.74	0.00	0.08	0.67	0.67	0.00	-0.11
Child	0.39	0.41	-0.03	-0.60	0.37	0.48	-0.11	-2.67 ***	0.37	0.39	-0.03	-0.56
Years of schooling	13.02	12.74	0.28	1.43	13.40	13.67	-0.27	-1.27	13.89	14.11	-0.22	-0.92
Tenure	217.51	202.52	14.99	1.39	204.81	194.40	10.41	1.02	194.77	179.79	14.98	1.25
White collar employee	0.66	0.62	0.04	0.89	0.76	0.82	-0.05	-1.64	0.84	0.89	-0.05	-1.68 *
Full time job	0.86	0.84	0.02	0.69	0.85	0.88	-0.03	-0.94	0.86	0.85	0.01	0.37
Temporary contract	0.05	0.04	0.01	0.40	0.03	0.05	-0.01	-0.86	0.05	0.09	-0.04	-1.54
Observations		1753				1066				547		
Log monthly wage 2005	7.92	7.93	-0.01	-0.31	7.96	8.03	-0.07	-1.86 *	8.02	8.03	-0.01	-0.20
Observations		1583				973				501		
Log monthly wage 2004	7.91	7.92	-0.01	-0.31	7.96	8.01	-0.05	-1.32	8.01	8.02	-0.01	-0.22
Observations		1557				949				486		
Log monthly wage 2003	7.90	7.89	0.01	0.20	7.95	7.96	-0.02	-0.31	7.99	8.02	-0.03	-0.67
Observations		1523				922				464		

Notes: The t-test for independent samples is used. Significance level: *** 1%, ** 5%, * 10%.

For reason of comparison, differences in average characteristics between the comparison groups and the group of non-participants (tr_1) are displayed in Table 3. These differences are much more pronounced than differences between treatment and comparison group. Most importantly, the groups differ in previous wages as well as in years of education. The educational background is of particular importance because it is closely linked to unobservable characteristics such as ability. Non-participants who had no training intentions are neither comparable to non-participants with training intentions nor to training participants.

Table 3: Comparison of average characteristics between non-participants and comparison group

	Non-participants (tr_1) versus those willing to attend one course (tr_2) (1)				Non-participants (tr_1) versus those with one course willing to attend another course (tr_4) (2)				Non-participants (tr_1) versus those with two courses willing to attend another course (tr_6) (3)			
	tr_2	tr_1	$\Delta_{tr2-tr1}$	t-value	tr_4	tr_1	$\Delta_{tr4-tr1}$	t-value	tr_6	tr_1	$\Delta_{tr6-tr1}$	t-value
Male	0.67	0.65	0.02	0.55	0.67	0.65	0.02	0.52	0.62	0.65	-0.03	-0.71
German	0.96	0.91	0.05	2.73 ***	0.94	0.91	0.02	1.16	0.95	0.91	0.04	2.29 **
Age	43.61	46.58	-2.97	-3.97 ***	43.42	46.58	-3.16	-4.77 ***	42.61	46.58	-3.97	-5.63 ***
Married	0.74	0.73	0.01	0.20	0.74	0.73	0.01	0.24	0.67	0.73	-0.06	-1.64
Child	0.41	0.32	0.09	2.23 **	0.48	0.32	0.16	4.19 ***	0.39	0.32	0.07	1.89 **
Years of schooling	12.74	12.11	0.63	3.26 ***	13.67	12.11	1.56	7.66 ***	14.11	12.11	2.00	9.38 ***
Tenure	202.52	223.10	-20.58	-1.91 *	194.40	223.10	-28.70	-2.93 ***	179.79	223.10	-43.30	-4.18 ***
White collar employee	0.62	0.43	0.19	4.66 ***	0.82	0.43	0.39	12.63 ***	0.89	0.43	0.47	17.72 ***
Full time job	0.84	0.86	-0.02	-0.54	0.88	0.86	0.02	0.73	0.85	0.86	-0.01	-0.34
Temporary contract	0.04	0.06	-0.02	-1.13	0.05	0.06	-0.01	-0.62	0.09	0.06	0.03	1.29
Observations		1959				1994				1979		
Log monthly wage 2005	7.93	7.80	0.13	3.73 ***	8.03	7.80	0.23	6.31 ***	8.03	7.80	0.23	6.22 ***
Observations		1725				1756				1752		
Log monthly wage 2004	7.92	7.79	0.13	3.58 ***	8.01	7.79	0.22	5.87 ***	8.02	7.79	0.23	5.61 ***
Observations		1704				1736				1726		
Log monthly wage 2003	7.89	7.79	0.10	2.57 **	7.96	7.79	0.17	3.42 ***	8.02	7.79	0.23	5.79 ***
Observations		1657				1687				1676		

Notes: The t-test for independent samples is used. Significance level: *** 1%, ** 5%, * 10%.

4. Results

The regression results without controlling for firm fixed effects are displayed in Table 4 (for full regression results including all control covariates see Table A-2 in the Appendix). The first column presents OLS results for imputed wages and the second column presents Tobit fixed effects results. In both models, an implausible pattern emerges as would-be participants have larger wages than actual participants (even though the differences are not statistically significant). When accounting for firm fixed effects (Table 5, see Table A-3 for full regression results), however, the pattern is reversed. This indicates that accounting for firm fixed effects is important.

In both column 1 and column 2 of Table 5, the effect for participating in one course is 0.5% (tr_3-tr_2). The impact of the second course is much higher with 2.2% (tr_5-tr_4) and it ranges from -1.8% to -2.4% for the third course. Even if none of these differences are statistically significant, a discussion of the size of the effects is nevertheless warranted to assess the credibility of the results. In the WeLL data, the average hours spent per training course is approximately 38. While a return of 0.5% could be interpreted as plausible for participating in one course, the effect for the second course is probably too high to represent the causal training effect.[8] This conclusion is in accordance with the results of the balancing test showing that the identification assumption for the second course does not hold anyway.

For the third course, the point estimate has a negative sign which is possible from a theoretical point of view if workers contribute to firms' training costs by accepting lower wages during the training period.[9] However, an approximate wage reduction of 2% is huge. In addition, the estimate is not very robust when using different estimation models (as is also shown in the sensitivity analysis below). Therefore, I do not believe that the effect of the third course represents a causal effect.

Table 4: Regression results without applying firm fixed effects, dependent variable: log monthly wage

Regressors	(1)			(2)		
	Marg. Eff.		Stand. Err.	Marg. Eff.		Stand. Err.
No training participation, tr_1	Base category			Base category		
No training participation, but intended, tr_2	0.079	***	0.025	0.073	***	0.025
Training participation in only one course, tr_3	0.050	***	0.013	0.044	***	0.014
Training participation in one course, but intended to do another, tr_4	0.116	***	0.027	0.102	***	0.030
Training participation in exactly two courses, tr_5	0.095	***	0.017	0.086	***	0.019
Training participation in two courses, but intended to do another, tr_6	0.167	***	0.027	0.149	***	0.028
Training participation in exactly three courses, tr_7	0.128	***	0.021	0.117	***	0.022
Training participation in more than three (intended) courses, tr_8	0.166	***	0.024	0.155	***	0.027
Training intention cancelled due to non-random reason, tr_9	0.087	***	0.025	0.072	***	0.026
Individual charact.	Yes			Yes		
Firm fixed effects	No			No		
F-test for tr_2=tr_3, (p-value)	1.21, (0.27)			1.43, (0.23)		
F-test for tr_4=tr_5, (p-value)	0.58, (0.45)			0.38, (0.54)		
F-test for tr_6=tr_7, (p-value)	2.70, (0.10)			1.93, (0.17)		
Observations	5,829			5,829		
R-squared	0.55			---		
Pseudo R-squared	---			0.52		
Log pseudolikelihood	---			-2056.78		

Notes: OLS regression results using imputed wages are shown in column 1. Column 2 contains uncoditional marginal effects of a Tobit regression (1143 right-censored observations). Standard errors are clustered at the establishment level. The control variables include male, German, age and age squared, married, child, an interaction term of male and child, years of schooling, tenure and tenure squared, white collar employee, full time job and temporary contract. Full estimation results are documented in Table A-2 in the Appendix. Significance level: *** 1%, ** 5%, * 10%.

[8] The return for a year of schooling ranges from 5% to 12% in Germany (Ichino and Winter-Ebmer 1999).
[9] In the WeLL data, the vast majority of courses is at least co-financed by employers. In many European countries, employers bear a substantial share of training costs (Bassanini et al. 2007).

Table 5: Regression results after controlling for firm fixed effects, dependent variable: log monthly wage

Regressors	(1)		(2)	
	Marg. Eff.	Stand. Err.	Marg. Eff.	Stand. Err.
No training participation, tr_1	Base category		Base category	
No training participation, but intended, tr_2	0.046 **	0.021	0.042 **	0.022
Training participation in only one course, tr_3	0.050 ***	0.010	0.047 ***	0.010
Training participation in one course, but intended to do another, tr_4	0.058 ***	0.019	0.056 ***	0.023
Training participation in exactly two courses, tr_5	0.080 ***	0.014	0.078 ***	0.015
Training participation in two courses, but intended to do another, tr_6	0.125 ***	0.021	0.118 ***	0.022
Training participation in exactly three courses, tr_7	0.102 ***	0.015	0.100 ***	0.015
Training participation in more than three (intended) courses, tr_8	0.164 ***	0.019	0.162 ***	0.021
Training intention cancelled due to non-random reason, tr_9	0.069 ***	0.018	0.063 ***	0.019
Individual charact.	Yes		Yes	
Firm fixed effects	Yes		Yes	
F-test for tr_2=tr_3, (p-value)	0.06, (0.81)		0.06, (0.81)	
F-test for tr_4=tr_5, (p-value)	1.01, (0.32)		0.87, (0.35)	
F-test for tr_6=tr_7, (p-value)	1.27, (0.26)		0.73, (0.39)	
Observations	5,829		5,829	
R-squared within	0.57		---	
R-squared between	0.46		---	
Pseudo R-squared	---		0.85	
Log pseudolikelihood	---		-639.54	

Notes: OLS regression results using imputed wages are shown in column 1. Column 2 contains uncoditional marginal effects of a Tobit regression (1143 right-censored observations). Standard errors are clustered at the establishment level. The control variables include male, German, age and age squared, married, child, an interaction term of male and child, years of schooling, tenure and tenure squared, white collar employee, full time job and temporary contract. Full estimation results are documented in Table A-3 in the Appendix. Significance level: *** 1%, ** 5%, * 10%.

The reason for unstable or unreliable effects for the second and third course is not obvious. One problem for those with multiple courses could be rooted in the question on training intentions itself because it cannot be ruled out that cancelled courses were postponed. In this case, some would-be participants are misclassified and should actually be assigned to the treatment group which would be an explanation for the high point estimate of the second course. Recall bias concerning course cancellation might also be a more severe problem when the number of attended courses increases. Thus, when extending the approach of LO to multiple training courses, it might be necessary to improve the questions for participants in multiple courses. For example, it could be emphasized that postponed courses should not be reported. Also, using more detailed questions on training intentions or a shorter time period than two years could reduce recall bias. The latter comes with the disadvantage, though, that there are a fewer number of persons having participated in multiple courses.

Concerning the selection effect of training participation, non-participants with no training intentions have a significantly higher average wage of 4.2% to 4.6% compared to non-participants with no intentions. To interpret the size of this coefficient, naïve OLS estimates are provided in Table A-4 in the Appendix where log wages are regressed on a binary training indicator in addition to socio-demographic and job characteristics. On average, training participants have 7.3% higher wages compared to non-participants. The selection bias accounts for more than 50% of this average difference. The remaining part does not only reflect the average return to the average participant, it also contains the selection effect of choosing the number of courses, i.e. persons participating in one course could again have different unobservables than persons participating in two courses and so on. Even though it would be of interest, the size of this effect cannot be investigated in this paper. Technically, this would be possible by comparing tr_4 with tr_3 for the selection effect of participating in a second course (compared to one course only) and tr_6 with tr_5 for the selection effect of participating in a third course (compared to two courses) if tr_4-tr_7 represent causal effects.

To check the robustness of the results, a variety of sensitivity analysis were undertaken using Tobit regressions. In particular, excluding health reasons as random event, using flexible functional forms for age and tenure, controlling for the interviewer date and for self-reported wages versus administrative wages leaves the main conclusions unchanged. The selection effect ranges between 4.3% and 4.8% and it is always statistically significant. The training effects are statistically insignificant and have a point estimate of 0.1% to 0.6% for the first course, 1.8% to 2.4% for the second course and -1.6% to -2.6% for the third course.

As an additional sensitivity check, wages of treatment and comparison group for the first, second and third course, respectively, are compared in separate regressions. This is equivalent to the empirical implementation of LO.[10] When estimating the selection effect of the training decision by considering tr_1 and tr_2 in one regression, the effect is 5.0% on a statistical significant level. For one course, the point estimate increases to 1.0% but remains statistically insignificant. The estimate for the second course is 2.8% and -0.9% for the third course, both are not significant. This is very similar to the main results.

[10] For this sensitivity check, imputed wages are used because sample sizes are to small for applying Tobit models with fixed effects.

The rather small sample sizes of would-be participants, could cause problems with statistical significance as they might simply be too small to reject the null hypothesis of a zero effect. Running simulations reveal that even when the sample sizes were increased to a conventional size, e.g. by a factor of five, none of the training effects would become statistically significant. Therefore, insignificant results for one, the second and third course are not caused by small sample sizes. However, it should be noted that a small coefficient of 0.5% would even require larger sample sizes than conventionally used.

5. Conclusion

This paper investigates wage returns to training in Germany by using a comparison-group approach that was suggested by LO. One of the main results of the paper is that the returns to participating in one training course is statistically insignificant and has a point estimate of approximately 0.5%. Zero or small returns to training to individuals could arise, if employers who pay most of the costs of training also appropriate most of the monetary returns. This result of insignificant returns to participating in one course was also found in LO.

However, I cannot conclude that firms do not share any of the training returns with their employees based on estimates for the first course only. This would ignore that training participants attend courses frequently and that training returns are a function of the number of courses in Germany. In addition, such a conclusion would also contradict the literature comparing productivity and wage returns to training using firm data (Dearden et al. 2006, Groot 1999, Conti 2005, Ballot et al. 2006, Kuckulenz 2006, Konings and Vanormelingen 2009). In almost every of these papers, positive and significant wage effects of training are found. However, it is also found that firms do keep a larger share of the returns to themselves, in particular, productivity growth exceeds wage growth by 2-5 times.

Another insight is that the group of non-participants is heterogeneous. They differ in terms of their socio-demographic and job characteristics and in terms of their wages. Would-be participants have a wage advantage of 4% to 5% compared to non-participants with no training intentions. Therefore, average wage differentials of training participants and non-participants after controlling for some covariates (obtained by naïve OLS estimates) reflect to more than 50% differences in unobservable characteristics, e.g. in innate abilities or motivation.

References

Arulampalam, W. and A. Booth (2001), Learning and Earning: Do Multiple Training Events Pay? A Decade of Evidence from a Cohort of Young British Men. *Economica* 68: 379-400.

Ballot, G., F. Fakhfakh and E. Taymaz (2006), Who Benefits from Training and R&D, the Firm or the Workers? *British Journal of Industrial Relations* 44(3): 473-495.

Bassanini, A. P., A. Booth, G. Brunello, M. De Paola, E. Leuven (2007), *Workplace Training in Europe*. Pp. 143-323 in: G. Brunello, P. Garibaldi, E. Wasmer (eds.), Education and Training in Europe. Oxford.

Becker, G. S. (1962), Investment in Human Capital: A Theoretical Analysis. *The Journal of Political Economy* 70(5): 9-49.

Bender, S., M. Fertig, K. Görlitz, M. Huber, and A. Schmucker (2009), WeLL - Unique Linked Employer-Employee Data on Further Training in Germany. *Journal of Applied Social Science Studies* 129 (4): 637-643.

Bell, S., L. Orr, J. Blomquist and G. G. Cain (1995), *Program Applicants as a Comparison Group in Evaluating Training Programs. Theory and a Test.* Upjohn Institute for Employment Research. Kalamazoo.

Büchel, F. and M. Pannenberg (2004), Berufliche Weiterbildung in West- und Ostdeutschland. Teilnehmer, Struktur und individueller Ertrag. *Zeitschrift für ArbeitsmarktForschung* 2/2004: 73-126.

Conti, G. (2005), Training, productivity and wages in Italy. *Labour Economics* 12: 557–576.

Dearden, L., H. Reed and J. V. Reenen (2006), The Impact of Training on Productivity and Wages: Evidence from British Panel Data. *Oxford Bulletin of Economics and Statistics* 68(4): 397-421.

Frazis, H. and M. A. Loewenstein (2005), Reexaming the Returns to Training. Functional Form, Magnitude, and Interpretation. *The Journal of Human Resources* 40(2): 453-476.

Gartner, H. (2005), The imputation of wages above the contribution limit with the German IAB employment sample. FDZ Methodenreport 02/2005. Nürnberg.

Goux, D. and E. Maurin (2000), Returns to firm-provided training: Evidence from French worker-firm matched data. *Labour Economics* 7: 1-19.

Greene, W. (2004), Fixed effects and bias due to the incidental parameters problem in the Tobit model. *Econometric Reviews* 23(2): 125-147.

Groot, W. (1999), Productivity effects of enterprise-related training. *Applied Economics Letters* 6: 369-371.

Ichino, A. and R. Winter-Ebmer (1999), Lower and upper bounds of returns to schooling: An exercise in IV estimation with different instruments. *European Economic Review* 43: 889-901.

Jäckle, R. and O. Himmler (2010), Health and wages - panel data estimates considering selection and endogeneity. *The Journal of Human Resources* 45(2).

Konings, J. and S. Vanormelingen (2009), The Impact of Training on Productivity and Wages: Firm Level Evidence. CEPR Discussion Papers 7473.

Kuckulenz, A. und M. Maier (2006), Heterogeneous Returns to Training. *Jahrbücher für Nationalökonomie und Statistik* 226(1): 24-40.

Kuckulenz, A. (2006), Wage and Productivity Effect of Continuing Training in Germany: A Sectoral Analysis. ZEW Discussion Paper No. 06-025.

Leuven, E. and H. Oosterbeek (2008), An alternative approach to estimate the wage returns to private-sector training. *Journal of Applied Econometrics* 23: 423-434.

Loewenstein, M. and J. Spletzer (1999), General and Specific Training: Evidence and Implications. *The Journal of Human Resources* 34(4): 710-733.

Lynch, L. M. (1992), Private-Sector Training and the Earnings of Young Workers. *The American Economic Review* 82(1): 299-312.

Muehler, G., M. Beckmann and B. Schauenberg (2007), The Returns to Continuous Training in Germany: New Evidence from Propensity Score Matching Estimators. *Review of Managerial Science* 1(3): 209-235.

Parent, D. (1999), Wages and mobility: the impact of employer-provided training. *Journal of Labor Economics* 17: 298-317.

Pischke, J.-S. (2001), Continuous training in Germany. *Journal of Population Economics* 14: 523-548.

Schøne, P. (2004), Why is the Return to Training so High? *Labour* 18(3): 363-378.

Appendix

Table A-1: Variable description and summary statistics

Variable	Description	Mean	Std. Dev.
ln(wage)	Logarithm of gross monthly wages	7.95	0.43
Male	Dummy variable: 1 for males, 0 otherwise	0.64	0.48
German	Dummy variable: 1 for born in Germany, 0 otherwise	0.94	0.23
Age	Age in years	45.18	9.34
Age squared	Age in years squared	2128.54	814.73
Married	Dummy variable: 1 for married employees, 0 otherwise	0.73	0.44
Child	Dummy variable: 1 for employees with underaged children, 0 otherwise	0.37	0.48
Male*Child	Interaction term between male and child	0.25	0.43
Years of schooling	Years of schooling	13.00	2.44
Tenure	Tenure in current job (in months)	209.65	136.74
Tenure squared	Tenure in current job (in months) squared	62646.23	67641.28
White collar employee	Dummy variable: 1 for white collar workers, 0 otherwise	0.65	0.48
Full time job	Dummy variable: 1 for employees working full-time, 0 otherwise	0.86	0.35
Temporary contract	Dummy variable: 1 for employees with temporary contract, 0 otherwise	0.05	0.22

Notes: 5,829 observations.

Table A-2: Full regression results without applying firm fixed effects, dep. variable: log monthly wage

Regressors	(1)			(2)		
	Marg. Eff.		Stand. Err.	Marg. Eff.		Stand. Err.
No training participation, tr_1	Base category			Base category		
No training participation, but intended, tr_2	0.079	***	0.025	0.073	***	0.025
Training participation in only one course, tr_3	0.050	***	0.013	0.044	***	0.014
Training participation in one course, but intended to do another, tr_4	0.116	***	0.027	0.102	***	0.030
Training participation in exactly two courses, tr_5	0.095	***	0.017	0.086	***	0.019
Training participation in two courses, but intended to do another, tr_6	0.167	***	0.027	0.149	***	0.028
Training participation in exactly three courses, tr_7	0.128	***	0.021	0.117	***	0.022
Training participation in more than three (intended) courses, tr_8	0.166	***	0.024	0.155	***	0.027
Training intention cancelled due to non-random reason, tr_9	0.087	***	0.025	0.072	***	0.026
Male	0.243	***	0.0244	0.219	***	0.025
German	-0.011		0.0196	-0.013		0.021
Age	0.019	***	0.0056	0.017	***	0.006
Age squared	-0.0002	***	0.0001	-0.0002	***	0.000
Married	-0.006		0.0110	-0.008		0.012
Child	-0.053	**	0.0248	-0.050	**	0.025
Male*Child	0.140	***	0.0265	0.126	***	0.028
Years of schooling	0.054	***	0.0040	0.048	***	0.004
Tenure	0.001	***	0.0002	0.001	***	0.000
Tenure square	0.000		0.0000	0.000		0.000
White collar employee	0.212	***	0.0224	0.191	***	0.023
Full time job	0.480	***	0.0276	0.432	***	0.028
Temporary contract	-0.082	***	0.0262	-0.068	***	0.028
Firm fixed effects	No			No		
Observations	5,829			5,829		

Notes: OLS regression results using imputed wages are shown in column 1. Column 2 contains uncoditional marginal effects of a Tobit regression (1143 right- censored observations). Standard errors are clustered at the establishment level. Significance level: *** 1%, ** 5%, * 10%.

Table A-3: Full regression results after controlling for firm fixed effects, dep. variable: log monthly wage

Regressors	(1)			(2)		
	Marg. Eff.		Stand. Err.	Marg. Eff.		Stand. Err.
No training participation, tr_1	Base category			Base category		
No training participation, but intended, tr_2	0.046	**	0.021	0.042	**	0.022
Training participation in only one course, tr_3	0.050	***	0.010	0.047	***	0.010
Training participation in one course, but intended to do another, tr_4	0.058	***	0.019	0.056	***	0.023
Training participation in exactly two courses, tr_5	0.080	***	0.014	0.078	***	0.015
Training participation in two courses, but intended to do another, tr_6	0.125	***	0.021	0.118	***	0.022
Training participation in exactly three courses, tr_7	0.102	***	0.015	0.100	***	0.015
Training participation in more than three (intended) courses, tr_8	0.164	***	0.019	0.162	***	0.021
Training intention cancelled due to non-random reason, tr_9	0.069	***	0.018	0.063	***	0.019
Male	0.156	***	0.0130	0.146	***	0.0135
German	0.066	***	0.0208	0.056	***	0.0181
Age	0.024	***	0.0045	0.023	***	0.0048
Age squared	-0.0002	***	0.0000	-0.0002	***	0.0001
Married	0.004		0.0084	0.002		0.0087
Child	-0.081	***	0.0224	-0.080	***	0.0222
Male*Child	0.124	***	0.0243	0.120	***	0.0248
Years of schooling	0.051	***	0.0028	0.047	***	0.0030
Tenure	0.000		0.0002	0.000	*	0.0002
Tenure square	0.000		0.0000	0.000		0.0000
White collar employee	0.196	***	0.0135	0.192	***	0.0139
Full time job	0.477	***	0.0278	0.452	***	0.0279
Temporary contract	-0.069	***	0.0198	-0.065	***	0.0209
Firm fixed effects	Yes			Yes		
Observations	5,829			5,829		

Notes: OLS regression results using imputed wages are shown in column 1. Column 2 contains uncoditional marginal effects of a Tobit regression (1143 right- censored observations). Standard errors are clustered at the establishment level. Significance level: *** 1%, ** 5%, * 10%.

Table A-4: Regression results not accounting for endogeneity

Regressors	Log monthly wage		
	Coeff.		Stand. Err.
Training participation (yes/no)	0.073	***	0.015
Male	0.217	***	0.026
German	-0.014		0.022
Age	0.017	***	0.006
Age squared	0.000	***	0.000
Married	-0.010		0.012
Child	-0.050	**	0.025
Male*Child	0.127	***	0.028
Years of schooling	0.049	***	0.004
Tenure	0.001	**	0.000
Tenure square	0.000		0.000
White collar employee	0.202	***	0.023
Full time job	0.433	***	0.029
Temporary contract	-0.070	**	0.027
Firm fixed effects	No		
Observations	5,829		
R-squared	0.51		

Notes: Tobit results are shown (1143 right-censored observations). Standard errors are clustered at the establishment level. Significance level: *** 1%, ** 5%, * 10%.