# Does learning trigger learning throughout adulthood? Evidence from employees' training participation

Anica Kramer[a] and Marcus Tamm[b,*]

[a] RWI, and Ruhr-University Bochum

RWI, Hohenzollernstr. 1-3, 45128 Essen, Germany, anica.kramer@rwi-essen.de

[b] RWI, and IZA

RWI, Invalidenstr. 112, 10115 Berlin, Germany, tamm@rwi-essen.de

**Abstract.** Individuals with more years of education generally acquire more training later on in life. Such a relationship may be due to skills learned in early periods increasing returns to educational investments in later periods. This paper addresses the question whether the complementarity between education and training is causal. The identification is based on exogenous variation in years of education due to the buildup of universities. Results confirm that education has a significant impact on training participation during working life.

**JEL classification:** I21, I24, I26, J24.

**Keywords:** training, lifelong learning, returns to schooling

[*] This paper uses data from the National Educational Panel Study (NEPS): Starting Cohort Adults, doi:10.5157/NEPS:SC6:5.1.0. From 2008 to 2013, NEPS data was collected as part of the Framework Program for the Promotion of Empirical Educational Research funded by the German Federal Ministry of Education and Research (BMBF). As of 2014, NEPS is carried out by the Leibniz Institute for Educational Trajectories (LIfBi) at the University of Bamberg in cooperation with a nationwide network. The authors acknowledge financial support from the Deutsche Forschungsgemeinschaft (TA 829/2-1 and TA 829/2-2). The authors thank Paula Schneider and Karim Diebold for excellent research assistance and acknowledge comments from two anonymous referees and participants at IWAEE, SPP 1646 Workshop, SMYE, GradIAB Workshop, RGS Doctoral Conference in Economics and RWI Research Network Conference. All correspondence to Marcus Tamm (tamm@rwi-essen.de).

# 1. Introduction

A growing literature documents that educational investments at one point in life may have sizeable impact on (educational) outcomes at later points in life. Evidence often comes from early intervention programs for disadvantaged children. For this group improvements in terms of outcomes such as IQ, test scores, high school graduation and income have been shown to arise during childhood, adolescence as well as early adulthood (e.g. Karoly et al. 1998, Currie 2001, Cunha et al. 2006). These findings inspired the hypothesis that skills beget skills. As is formalized in Cunha and Heckman (2007), a causal relationship that skills beget skills may arise from two main mechanisms. First, skills acquired in early periods in life persist in later periods and may be self-reinforcing (self-productivity). Second, returns to investments might differ by level of previous human capital (dynamic complementarity).

The latter mechanism implies that decisions to invest in human capital might depend on the previous level of human capital. Such a behavioral response might lead to complementarity of educational investments if those with higher levels of human capital early on in life experience more investments later on in life. This paper analyzes whether such complementarity in educational investments persists throughout adulthood, i.e. after individuals finished their initial educational choices and entered the labor market. Our measure of educational investment after labor market entry focuses on participation in work-related training. Note that initial educational choices and training investments later on do not necessarily have to be complements. For example, if equity is important when deciding about investments, e.g. from the side of the government or the employer, training might be used to compensate for lower initial levels of human capital. Additionally, note that a complementary relationship between educational investments at early ages and training during adulthood might also reflect that early investments require follow-up investments in order to be effective.

By identifying causal effects of educational investments, our paper is related to the literature on returns to schooling. This literature traditionally focuses on wages (e.g. Angrist and Krueger 1991, Harmon and Walker 1995, Pischke 2007, Pischke and von Wachter 2008). In recent years, non-monetary outcomes like political attitudes, happiness, health, marriage, parenting or the intergenerational transfer of education have also been analyzed (Oreopoulos 2007, Oreopoulos and Salvanes 2011, Maurin and McNally 2008, Piopiunik 2014, Siedler 2010). The impact of schooling on lifelong learning, however, received no attention. This is surprising because this research question is of high political interest. For policy makers who aim to increase individuals' participation in lifelong learning (this is proposed as policy aim, e.g., in the Lisbon Strategy for European countries), it is important to know whether education affects training or whether it represents a spurious correlation.

On the other hand, there is a large number of non-causal empirical studies that analyze the determinants of training, which also focus on schooling (e.g. Lynch 1992, Lynch and Black 1998, Blundell et al. 1996, Leuven and Oosterbeek 1999, Pischke 2001, Fouarge et al. 2013, Görlitz and Tamm 2016). These studies generally estimate reduced form models of the training probability using different individual, job and firm characteristics as covariates. Virtually all of these studies show that schooling and training participation later in life display a strong complementary relationship. Also across countries there is a clear positive correlation between average levels of education and average rates of training participation (Bassanini et al. 2007). We contribute to this literature by using information on exogenous variation in the buildup of universities at the county level to identify the causal impact of initial investments in schooling and vocational education on training during adulthood. To the best of our knowledge, this is the first paper that establishes such a causal relationship.

In a second step, we test several mechanisms that might drive the training gap between individuals with high and low levels of schooling. The previous literature analyzing such mechanisms has looked at aspects related to the job, to the firm, to sociodemographic factors and to preferences and personality traits of employees. For example, Fouarge et al. (2013) test whether the difference in training participation is due to lower returns to training or due to differences in personality traits. They find that wage returns to training are very similar but that differences in preferences (e.g. in future orientation) and in personality traits (e.g. in locus of control and in some of the Big Five factors) make low-educated workers less willing to participate in training. Looking at several countries, Leuven and Oosterbeek (1999) show that the training difference between high- and low-educated employees is not due to differences in firms' willingness to invest in training. Furthermore, Görlitz and Tamm (2016) show that in Germany the difference in training participation between employees with and without college degree is around 15 percentage points. This correlation is not reduced if sociodemographic factors (gender, age, migration background, marriage status, children), aspects related to the job contract (working time, temporary contract, tenure) or firm specific factors are additionally controlled for. In contrast, they find that about half of the difference is due to differences in the job tasks of individuals. Employees with college degree are more likely to perform nonroutine tasks and performing nonroutine tasks is associated with a higher probability of training. Our results partly confirm the previous findings by revealing that the selection into different occupations is an underlying driver of the established causal relationship while selection into specific firms is not.

The paper is organized as follows. Section 2 describes the data and presents descriptive evidence on the correlation between initial investments in education and training participation during adulthood. Section 3 discusses the identification strategy. The main empirical findings and several robustness checks are presented in section 4. Section 5 offers conclusions.

## 2. The Data

The analysis is based on the adult sample of the National Educational Panel Study (NEPS-SC6). NEPS is a panel study on educational, occupational, and family formation processes. It also covers detailed life course information from birth trough to adult life (Blossfeld et al. 2011). For several reasons, the adult sample of the NEPS data is particularly suited for our analysis. It comprises more than 17,000 individuals born between 1944 and 1986 and, thus, covers individuals acquiring education from various cohorts. This enables us to use changes in the supply of educational institutions for identification. With respect to lifelong learning activities, NEPS includes questions that are specific to the employment status of individuals. In the analysis, we use information on participation in work-related training such as seminars or training courses during the previous 12 months, while being employed. This is a measure of training participation that has been used in various studies (e.g. Bassanini et al. 2007). It does not include any re-trainings for the unemployed (as they are part of active labor market policies) or any informal training such as self-learning from books. Most of the training we look at is financed by firms but some individuals also participate in self-financed training. The NEPS data is very detailed about participation in the educational system. All school episodes, vocational training and college attendance are covered, including start and end date for each episode. From this we generate information on years of education by summing up the duration of each educational spell.[1] Furthermore, the NEPS data includes information on the region of birth which is crucial for applying the instrumental variable method used in this paper for identifying the causal effects of education (see section 3).

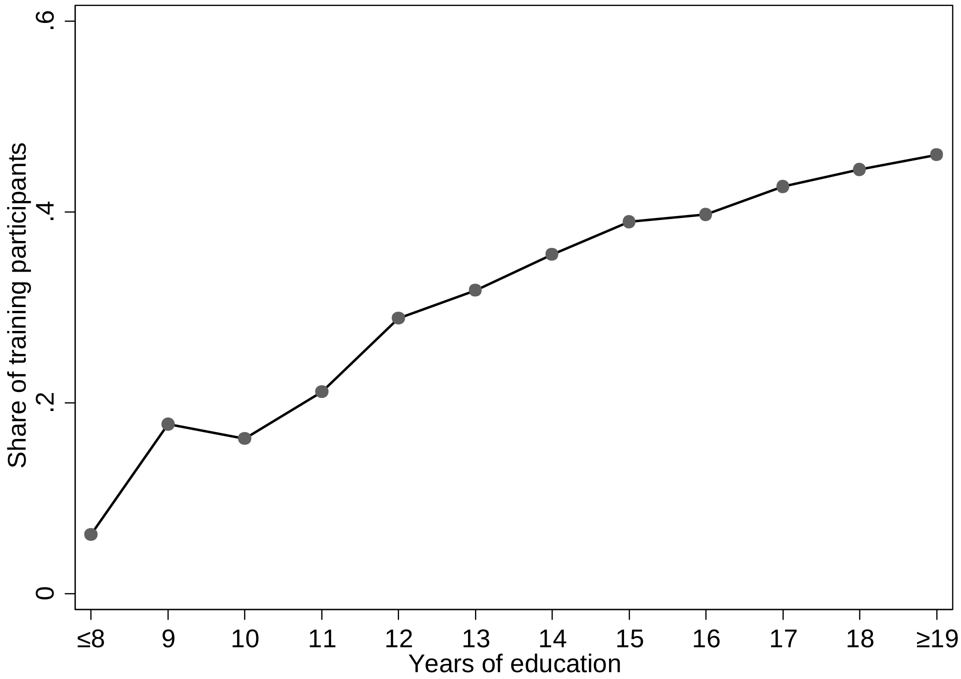
For the analysis, we use cross-sectional information on training participation that comes from two waves of data, namely waves 2009/2010 and 2011/2012. Wave 2009/2010 is the first NEPS wave covering information on training participation. For all individuals interviewed in wave 2009/2010, we use training participation measured during this wave as dependent variable. To increase the sample size, NEPS included a refreshment sample in wave 2011/2012. We also use information on training participation measured during wave 2011/2012 for all interviewees who have not already taken part in wave 2009/2010. This increases the sample size by around 40 percent.[2]

Figure 1 shows differences in participation rates in work-related training by years of education. A clear positive association between education and training emerges which appears to be almost linear. While around 45% of employees with more than 18 years of education participated in work-related training during the previous 12 months, the share is 30% for employees with 13 years of education and as low as 8% for employees with up to 8 years of education. On average each year of education is associated with a 1.8 percentage points higher probability of training participation.

[1] We also take care of parallel spells to avoid double counting.

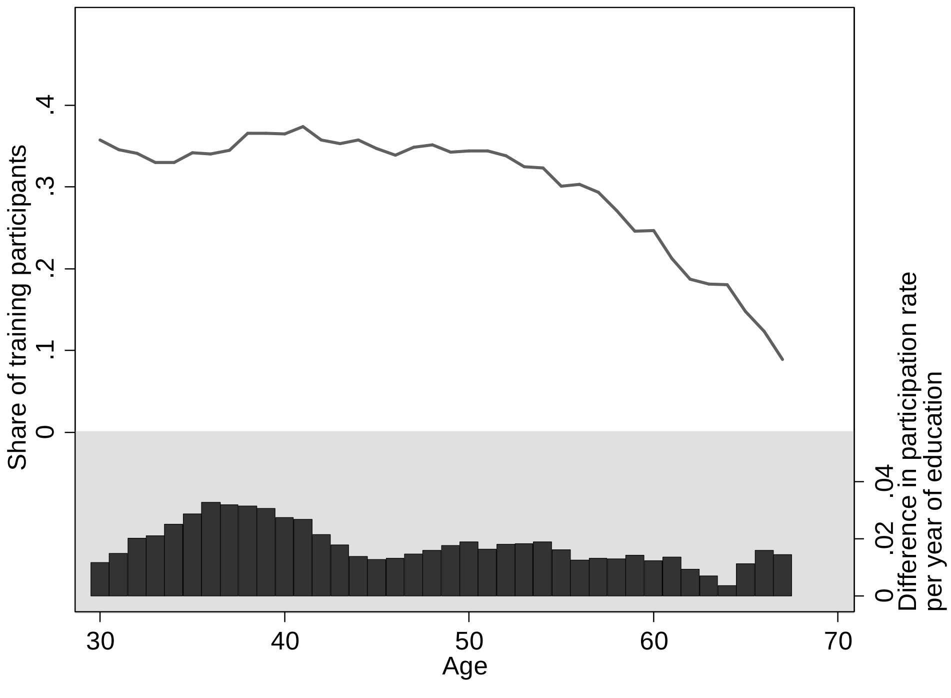
[2] None of the results depend on including individuals from wave 2011/2012 (see section 4).

Figure 1 – Share of individuals participating in work-related training by years of education




*Note: NEPS data. Sample restricted to employed individuals born in West Germany, cohorts 1956 to 1979.*

Figure 2 – Share of training participants and naïve training return by age




*Note: NEPS data. Sample restricted to employed individuals born in West Germany. The straight line in the upper part of the figure indicates the share of individuals having participated in work-related training during the previous 12 months. The bars in the lower part of the figure indicate the percentage difference in participation rates per year of education. (Difference in) participation rates displayed are smoothed using 5-cohorts moving average.*

Training participation rates are not constant across the lifecycle. Generally, they decline for employees close to retirement age (e.g. Booth 1991, Bassanini et al. 2007). This pattern can also be shown with the NEPS data and is documented in Figure 2. Training participation rates reach their peak between the ages 30 and 40 and show a considerable decline after age 55. The lower part of Figure 2 also shows naïve estimates of the training return to education. Overall, the correlation between education and training participation is comparably stable over most of the lifecycle, hovering slightly below 2 percentage points higher participation rate per year of education. Only at around age 35 the association is slightly stronger and it becomes weaker directly preceding retirement age. At the maximum each year of education is associated with a 3.2 and at the minimum with a 0.4 percentage points higher training rate.

Since our primary focus is on work-related training, we restrict our sample to employed individuals for most of the analysis, including self-employed individuals. Furthermore, given the strong age-pattern in training participation and in order to avoid problems due to education potentially affecting retirement, we only consider individuals born between 1956 and 1979, who are aged 30 to 55 at the time of interview.[3] Furthermore, we restrict our sample to individuals who were born in West Germany (excluding Berlin), due to various differences in the schooling and university system between East and West Germany before reunification. The restriction also implies that foreign born individuals are excluded. This is necessary because our instrument resorts to information on region of birth. Our final sample includes around 6,600 individuals. Table A1 in the appendix presents descriptive statistics. On average individuals have 15.5 years of education and 35% participated in work-related training during the previous 12 months.

## 3. Identification Strategy

The differences in training participation by years of education presented in the previous section might either originate from a causal effect of education on training or from spurious correlation that might be due to education and training being both affected by third factors such as ability, motivation to progress or ease and enjoyment of learning. To identify the causal effect of education on training, we use an instrumental variable strategy. Regression estimates are based on two equations:

$$train_i = \alpha + \beta\, edu_i + X_i\, \gamma + \varepsilon_i \tag{1}$$

$$edu_i = \theta + \delta\, uni_i + X_i\, \lambda + v_i \tag{2}$$

[3] We exclude individuals from younger cohorts, i.e. those born between 1980 and 1986 because few of them already entered the labor market. More importantly, we find that labor market participation in younger cohorts is selectively affected by level of education. Specifically, below age 30 those with a lower level of education have higher levels of labor market participation. Because our analysis mainly focuses on employees, including younger cohorts would result in a selected sample.

where $train_i$ is an indicator for training participation that equals 1 if individual $i$ participated in work-related training and 0 otherwise. Training participation is a function of years of education ($edu_i$) and further variables $X_i$ which include cohort fixed effects, region fixed effects, linear state-specific cohort trends, gender, and an indicator for whether training participation is measured in the 2011/2012 wave instead of 2009/2010. The region fixed effects are included to capture differences across regions that are correlated with educational choices and training. The regions refer to labor market regions as defined by Kosfeld and Werner (2012) and comprise areas characterized by close commuter links.[4] Estimating equation (1) without accounting for potential endogeneity of years of education may lead to biased estimates of β. To account for potential endogeneity of education, we use equation (2) and regress education on the instrument $uni_i$ and the same variables $X_i$ that are included in equation (1).

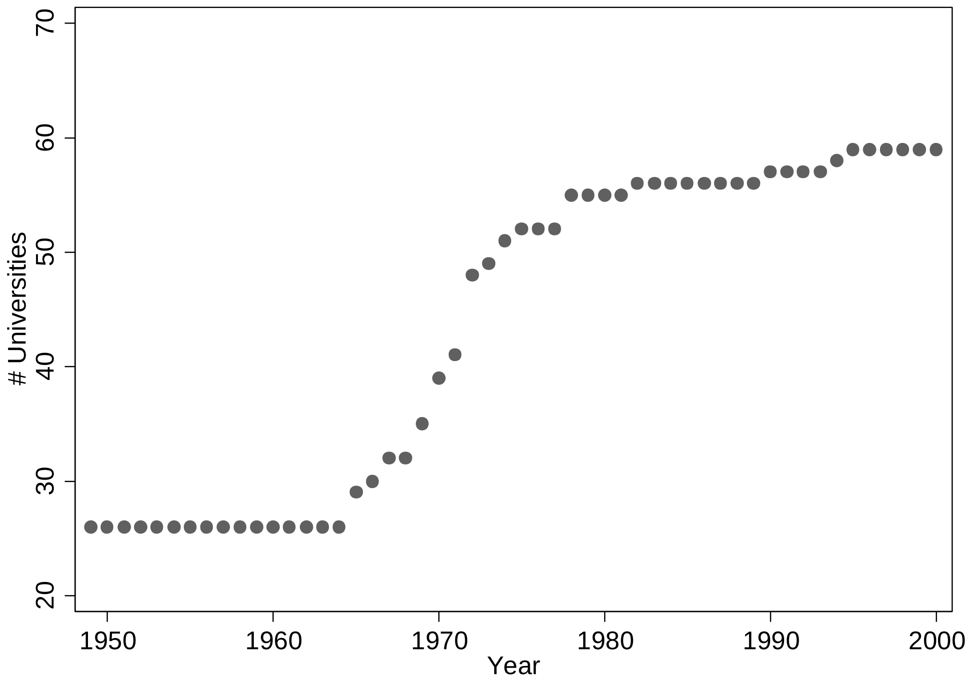
The instrument indicates whether an individual grew up close to a university. We approximate this by using information on whether there was a university in the county of birth at the time the individual turned age 18. The rational of this instrument is that proximity to university is an important determinant of college attendance. Evidence for this is provided in Card (1995) and for Germany in Spiess and Wrohlich (2010). The data on universities comes from the German Rectors' Conference (*Hochschulrektorenkonferenz*) and was updated by checking the webpages of all universities to verify the year of foundation and to investigate the year when teaching started.[5]

Figure 3 displays the number of large universities by year. It shows a strong increase in the number of universities during the educational expansion in the 1960s and 1970s. As documented in Wissenschaftsrat (1960, 1967), the buildup of universities was initially motivated by a large increase in student body after World War II and ensuing capacity constraints at already existing universities. During the 1960s, the aim was broadened and explicitly included the mobilization of additional students to participate in higher education. Some commentators argue that the political motive for the stark expansion of the educational system was also prompted by the Sputnik Crises and the resulting willingness to completely dominate the communist countries also in the area of research and education (cf. Bartz 2006). Figure A1 in the appendix illustrates the regional distribution of universities over time.

[4] Note that the region fixed effects are measured at the level of labor market regions while the instrument is measured at the county level. There are 108 labor market regions in West Germany that are made up of between one and eleven counties. We do so because the number of counties is large compared to the overall sample size. Including county dummies results in quite a number of counties with only one or two interviewees and for the majority of counties there would be no change in university status across interviewees. This leads to insignificant first stage results and weak identification problems.

[5] While for the majority of universities the founding year and the take-up of teaching coincide, some universities started teaching one or two years after official foundation and there are lags in teaching take-up of up to 5 years. In the analysis we only consider universities that already started teaching.

Figure 3 – Buildup of universities in West Germany over time




*Note: The figure indicates the number of large universities by year, excluding colleges for art or music, universities administered by the military, small universities for religious studies, and any university with less than 3000 students in 2012.*

## 4. Empirical Results

Table 1 presents results for our main analysis in column 1 using training participation as dependent variable and years of education as main explanatory variable. OLS estimates are provided in the upper part of the table and IV estimates in the lower part. The OLS estimates confirm the previous descriptive evidence that education and training participation are positively correlated, with each year of education being associated with a 2.0 percentage point higher training probability.

When looking at the IV results, the first stage indicates that individuals born in a region with a university have almost 0.9 more years of education. Furthermore, the F-test of excluded instruments indicates that there is no weak identification problem (Staiger and Stock 1997). The point estimate of the second stage for years of education is statistically significant. According to the IV estimate each year of education gained early in life raises the training probability by 3.7 percentage points. This confirms that education has a causal impact on training participation later in life. Columns (2) and (3) provide estimates separately by gender. They do not hint at any major differences between men and women.

Table 1 – The impact of education on training participation

| | Basic specification (1) | Male sample: spec. (1) (2) | Female sample: spec. (1) (3) | Spec. (1) with squared state-specific cohort trends (4) | Spec. (1) without linear state-specific cohort trends (5) | Spec. (1), sample restricted to wave 2009/2010 (6) | Spec. (1), sample restricted to individuals with 7 to 28 years of education (7) |
|---|---|---|---|---|---|---|---|
| OLS estimates | | | | | | | |
| Years of education | 0.0203*** | 0.0161*** | 0.0278*** | 0.0203*** | 0.0204*** | 0.0200*** | 0.0232*** |
| | [0.0017] | [0.0022] | [0.0024] | [0.0017] | [0.0017] | [0.0020] | [0.0019] |
| IV estimates | | | | | | | |
| Years of education | 0.0373** | 0.0397 | 0.0358* | 0.0382** | 0.0379** | 0.0454** | 0.0382** |
| | [0.0161] | [0.0276] | [0.0206] | [0.0161] | [0.0159] | [0.0220] | [0.0178] |
| First stage | | | | | | | |
| University (dummy) | 0.8553*** | 0.8468*** | 0.8034*** | 0.8571*** | 0.8604*** | 0.8205*** | 0.7631*** |
| | [0.1517] | [0.2243] | [0.1815] | [0.1515] | [0.1516] | [0.1834] | [0.1327] |
| F-test (excluded instrument) | 31.78 | 14.25 | 19.59 | 32.01 | 32.22 | 20.01 | 33.05 |
| Observations | 6684 | 3346 | 3338 | 6684 | 6684 | 4676 | 6579 |

*Note: NEPS data. Sample restricted to employees born in West Germany, cohorts 1956 to 1979. All regressions include cohort fixed effects, fixed effects at the level of labor market regions, linear state-specific cohort trends, gender and an indicator for wave 2011/2012. Specification (4) additionally includes squared state-specific cohort trends. Specification (5) is without linear state-specific cohort trends. Specification (6) drops observations from wave 2011/2012. Specification (7) drops individuals with extremely low or high number of years of education. Standard errors in brackets are adjusted for clustering at the county level. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*

Note that the point estimate of the IV model is larger than the OLS estimate. This might be explained by the fact that IV identifies local average treatment effects. The variation in the availability of universities is most likely to affect the marginal student who has a less favorable family background, while for students from more favorable families attending university is also possible when they are not living close to a university, because the cost of commuting or of moving out of the family home is less of a barrier. If the effect of education on training is higher for the group of marginal students than for the overall population, the IV estimate will exceed the OLS estimate. Having said this, also note that the confidence interval of the IV estimate overlaps the point estimate of the OLS estimate, i.e. the magnitudes do not differ significantly.

***Robustness of the results***

In the following we present several robustness tests. Because the timing of university buildup across states might correlate with cohort trends in training participation, we include linear state-specific cohort trends in our main specification. This is similar to proceedings in Pischke and von Wachter (2008) and should take up any smooth trends in education and training participation at the state level. In column (4) we present results that additionally account for squared state-specific cohort trends and in column (5) we drop all state-specific cohort trends. Results in Table 1 show that controlling for state-specific trends has only minor effects on the IV estimates.

As noted in section 2, our data comprises training information on individuals that were interviewed at different points in time. In column (6), we restrict the analysis sample to individuals taking part in the 2009/2010 wave of the NEPS and exclude those from the refreshment sample in 2011/2012. The number of observations drops to 4,676 but the IV estimate remains statistically significant. Furthermore, in column (7) we exclude individuals with very low or very high number of years of education from the analysis. We do so because the NEPS data asks about the start and end date of school and vocational spells retrospectively and responses might suffer from reporting error. If we restrict the analysis to individuals with at least 7 and at most 28 years of education this hardly affects results.

Another potential threat to the validity of the findings is that we restrict the analysis to employed individuals and that employment might be endogenous. Table 2 shows results that test whether employment is affected by years of education. The IV results are not significant, indicating that sample selection does not bias the main findings.

In the main specification, the instrument measuring the availability of a university at the county level only accounts for large universities and disregards other types of colleges such as universities of applied science, colleges for art or music or teacher training colleges. In Table 3 we present results using alternative instruments that also account for those other types of colleges. In addition to the dummy indicating the availability of a large university, specification (1) includes a dummy indicating whether any other type of college (comprising universities of

Table 2 – The impact of education on employment

| | Basic specification (1) |
|---|---|
| IV estimates | |
| Years of education | -0.0188 |
| | [0.0126] |
| First stage | |
| University (dummy) | 0.8939*** |
| | [0.1446] |
| F-test (excluded instrument) | 38.19 |
| Observations | 7289 |

*Note: NEPS data. Sample restricted to individuals born in West Germany, cohorts 1956 to 1979. The regression includes cohort fixed effects, fixed effects at the level of labor market regions, linear state-specific cohort trends, gender and an indicator for wave 2011/2012. Standard errors in brackets are adjusted for clustering at the county level. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.*

Table 3 – IV results using alternative instruments

| | Instruments: availability of universities and of other colleges (1) | Instruments: availability of universities and number of universities (2) |
|---|---|---|
| IV estimates | | |
| Years of education | 0.0365** | 0.0377** |
| | [0.0158] | [0.0162] |
| First stage | | |
| University (dummy) | 0.7535*** | 0.9931** |
| | [0.1749] | [0.4044] |
| Other type of college (dummy) | 0.1722 | |
| | [0.1518] | |
| Number of universities | | -0.1141 |
| | | [0.2814] |
| F-test (excluded instrument) | 16.5 | 16.39 |
| Observations | 6684 | 6684 |

*Note: NEPS data. Sample restricted to employees born in West Germany, cohorts 1956 to 1979. The regression includes cohort fixed effects, fixed effects at the level of labor market regions, linear state-specific cohort trends, gender and an indicator for wave 2011/2012. Standard errors in brackets are adjusted for clustering at the county level. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.*

applied science, colleges for art or music, teacher training colleges or small universities) was present in the county of birth at age 18. The first stage point estimate of those other types of colleges is positive, but considerably smaller than the effect of universities and not significant. In specification (2) we test whether the number of universities is relevant for years of education. We find that the dummy indicating that there is at least one university in the county of birth is highly significant but that the number of universities is not. Overall, the second stage IV estimates remain stable if we consider the additional instruments.

***Mechanisms***

The results presented up to now show that education has a causal impact on training participation later in life but they do not reveal why this is the case. In this subsection we test several possible mechanisms. Among others, we test whether the impact of education is transmitted by working in different firms, by working in different occupations or positions, by changing fertility patterns or by influencing personality traits. We do so by estimating the basic specification of the IV model presented above and additionally control for each of the potential mechanisms in the regression. Any reduction in the point estimate of the second stage estimate of education on training would suggest that the respective additional controls serve as mechanisms. Table 4 presents the results.

For reasons of comparison, column (1) of Table 4 simply repeats the baseline specification from Table 1. Specification (2) controls for firm size using three categories (less than 10 employees, 10 to 99 employees and 100 or more employees). Specification (3) controls for the sector of the firm using the 2 digit NACE code. Specification (4) controls for four different dummies indicating whether the firm supports training. In detail, the four dummies indicate whether there is an agreement on training between the workers' council and the management, whether the firm organizes and finances training for the employees, whether the firm has a plan to regularly train the employees and whether the firm has a division or an employee who is responsible for training activities. Specification (5) controls for the occupation of the employee using the 3 digit ISCO code. Specification (6) controls for a dummy indicating if the employee has a management position. Specification (7) controls for a dummy indicating if the employee works part-time. Specification (8) controls for two dummy variables indicating if the employee has any child below age 18 and any child below age 6, respectively. Specification (9) controls for personality traits of the individual using the Big Five.

For most of the additional controls we do not find that they have any major effect on the second stage IV estimate of education on training. All point estimates are close to the baseline specification in column (1). Only the estimate in column (5) that controls for occupations is somewhat smaller and not statistically significant any more. This implies that neither selection into specific firms, nor changes in fertility patterns or personality traits are mechanisms why education increases training. To some degree the selection into occupations appears to be a mechanism. But the explanatory power of occupational sorting is not overly large, explaining at most a quarter of the relationship.

Given that occupations strongly differ in the content and composition of job tasks, these findings are in line with those of Görlitz and Tamm (2016) who show that job tasks are driving training differences between college educated employees and those without college, while other factors, including selection into specific firms, are not. Our findings are in contrast to

Table 4 – Mechanisms

| | Basic specification (1) | Controls: Firm size (2) | Controls: Sector (2 digit) (3) | Controls: Firm supports training (4 indicators) (4) | Controls: Occupation (3 digit ISCO) (5) | Controls: Manage-ment position (6) | Controls: Dummy for part-time (7) | Controls: Children (2 indicators) (8) | Controls: Personality traits (Big Five) (9) |
|---|---|---|---|---|---|---|---|---|---|
| IV estimates | | | | | | | | | |
| Years of education | 0.0373** | 0.0401** | 0.0437** | 0.0403** | 0.0302 | 0.0402** | 0.0369** | 0.0386** | 0.0361** |
| | [0.0161] | [0.0163] | [0.0212] | [0.0164] | [0.0237] | [0.0171] | [0.0158] | [0.0161] | [0.0173] |
| Observations | 6684 | 6684 | 6684 | 6684 | 6684 | 6684 | 6684 | 6684 | 6670 |

*Note: NEPS data. Sample restricted to employees born in West Germany, cohorts 1956 to 1979. All regressions include cohort fixed effects, fixed effects at the level of labor market regions, linear state-specific cohort trends, gender and an indicator for wave 2011/2012. Standard errors in brackets are adjusted for clustering at the county level. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.*

those in Fouarge et al. (2013) who show that personality traits are important. One reason for this difference might be that we look at actual training participation while Fouarge et al. (2013) look at the self-assessed willingness to participate in training, another reason might be that Fouarge et al. (2013) use more measures of personality traits while we only resort to the Big Five.

## 5. Conclusion

The hypothesis that skills beget skills argues that returns to educational investments might increase with previous level of human capital. Such a mechanism implies that educational investments at one point in life influence the decision on educational investments at later points in life. Also, initial investments might require follow-up investments to be effective. This would lead to complementarity in educational investments, i.e. learning triggers learning. Results in this paper document that such a complementarity in educational investments persists throughout adulthood. Using exogenous variation in years of education, we find that education has a causal impact on participation in work-related training through most of working life. The estimates of instrumental variables regressions even exceed those of naïve OLS estimates. These results imply that any policy designed to increase educational participation of children and adolescents today may also increase lifelong learning activities of the workforce tomorrow.

## Literature


Angrist, J. and A. Krueger (1991), Does Compulsory School Attendance Affect Schooling and Earnings. Quarterly Journal of Economics 106(4), 979-1014.

Bartz, O. (2006), Wissenschaftsrat und Hochschulplanung – Leitbildwandel und Planungsprozesse in der Bundesrepublik Deutschland zwischen 1957 und 1975. Universität zu Köln.

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

Blossfeld, H.-P., H.-G. Roßbach and J. von Maurice (eds.) (2011), Education as a Lifelong Process – The German National Educational Panel Study (NEPS). Zeitschrift für Erziehungswissenschaft, Special Issue 14.

Blundell, R., L. Dearden and C. Meghir (1996), The Determinants and Effects of Work-Related Training in Britain. London: Institute for Fiscal Studies.

Booth, A. (1991), Job-related Formal Training: Who Receives it and What is it Worth? Oxford Bulletin of Economics and Statistics 53, 281-294.

Card, D. (1995), Using Geographic Variation in College Proximity to Estimate the Return to Schooling. In: L.N. Christofides, E.K. Grant and R. Swidinsky (eds), Aspects of Labor Market Behaviour: Essays in Honour of John Vanderkamp. Toronto: University of Toronto Press.

Cunha, F., J. Heckman, L. Lochner and D. Masterov (2006), Interpreting the Evidence on Life Cycle Skill Formation. In: E. Hanushek and F. Welch (eds.), Handbook of Education Economics, vol. 1, Elsevier, 697-812.

Cunha F. and J. Heckman (2007), The Technology of Skill Formation. American Economic Review 97(2), 31-47.

Currie, J. (2001), Early Childhood Education Programs. Journal of Economic Perspectives 15(2), 213-238.

Fouarge, D., T. Schils and A. de Grip (2013), Why do low-educated workers invest less in further training? Applied Economics 45(18), 2587-2601.

Görlitz, K. and M. Tamm (2016), Revisiting the Complementarity between Education and Training – The Role of Job Tasks and Firm Effects. Education Economics 24(3), 261-279.

Harmon, C. and I. Walker (1995), Estimates of the Economic Return to Schooling for the United Kingdom. American Economic Review 85(5), 1278-1286.

Karoly, L., P. Greenwood, S. Everingham, J. Houbé, M. Kilburn, C. Rydell, M. Sanders and J. Chiesa (1998), Investing in our children – What we know and don't know about the costs and benefits of early childhood interventions. Washington, D.C.: RAND.

Kosfeld, R. and A. Werner (2012), Deutsche Arbeitsmarktregionen - Neuabgrenzung nach den Kreisgebietsreformen 2007 - 2011. Raumforschung und Raumordnung, 70(1), 49–64.

Leuven, E. and H. Oosterbeek (1999), The Demand and supply of work-related training: Evidence from four countries. Research in Labor Economics 18, 303-330

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

Lynch, L. and S. Black (1998), Beyond the Incidence of Employer-Provided Training. Industrial and Labor Relations Review 52(1), 64-81.

Maurin, E. and S. McNally (2008), Vive la Révolution! Long-Term Educational Returns of 1968 to the Angry Students. Journal of Labor Economics 26(1), 1-33.

Oreopoulos, P. (2007), Do Dropouts Drop Out Too Soon? Wealth, Health and Happiness from Compulsory Schooling. Journal of Public Economics 91(11-12), 2213-2229.

Oreopoulos, P. and K. Salvanes (2011), Priceless: The Nonpecuniary Benefits of Schooling. Journal of Economic Perspectives 25(1), 159-84.

Piopiunik, M. (2014), Intergenerational Transmission of Education and Mediating Channels: Evidence from a Compulsory Schooling Reform in Germany. Scandinavian Journal of Economics 116(3), 878-907.

Pischke, S. (2001), Continuous Training in Germany. Journal of Population Economics 14(3), 523-548.

Pischke, S. (2007), The Impact of Length of the School Year on Student Performance and Earnings: Evidence from the German Short School Years. Economic Journal 117(523), 1216-1242.

Pischke, S. and T. von Wachter (2008), Zero Returns to Compulsory Schooling in Germany: Evidence and Interpretation. Review of Economics and Statistics 90(3), 592-598.

Spiess, K. and K. Wrohlich (2010), Does distance determine who attends a university in Germany? Economics of Education Review 29(3), 470–479.

Siedler, T. (2010), Schooling and Citizenship in a Young Democracy: Evidence from Post-War Germany. Scandinavian Journal of Economics 112(2), 315-338.

Staiger, D. and J. Stock (1997), Instrumental variables regression with weak instruments. Econometrica 65(3), 557-586.

Wissenschaftsrat (1960), Empfehlungen des Wissenschaftsrats zum Ausbau der wissenschaftlichen Einrichtungen. Wissenschaftsrat: Tübingen.

Wissenschaftsrat (1967), Empfehlungen des Wissenschaftsrats zum Ausbau der wissenschaftlichen Hochschulen bis 1970. Wissenschaftsrat: Tübingen.

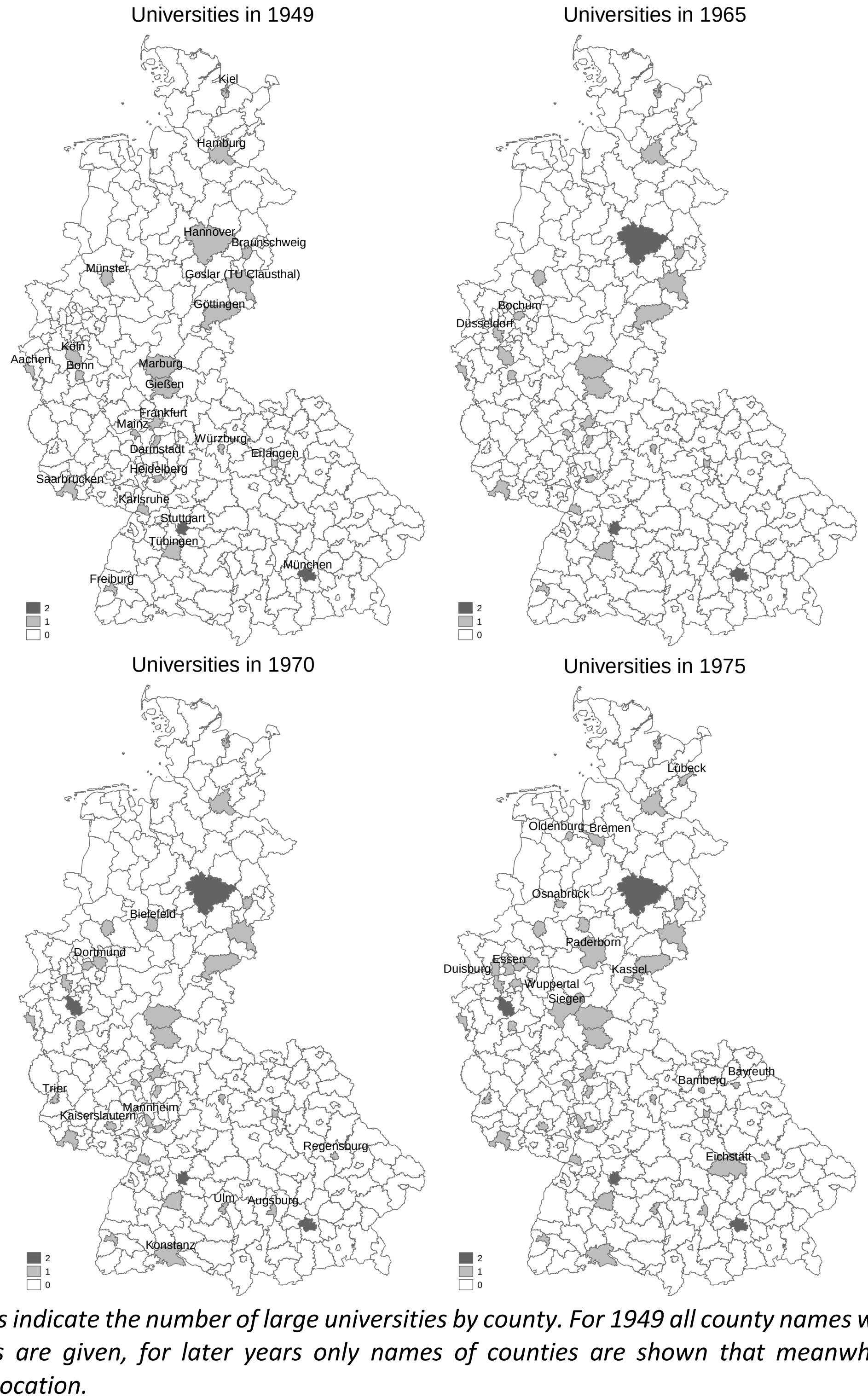
## Appendix

Table A 1 – Summary statistics

| | Mean | Std. dev. |
|---|---|---|
| Training participation | 0.350 | 0.477 |
| Years of education | 15.5 | 4.0 |
| University in county of birth at age 18 | 0.299 | 0.458 |
| Other type of college in county of birth at age 18 | 0.464 | 0.499 |
| Woman | 0.483 | 0.500 |
| Age | 43.7 | 6.4 |
| Wave 2011/2012 | 0.311 | 0.4631 |

*Note: NEPS data. Sample restricted to employees born in West Germany, cohorts 1956 to 1979.*

Figure A1 – Regional distribution of universities in West Germany over time




*Note: Maps indicate the number of large universities by county. For 1949 all county names with existing universities are given, for later years only names of counties are shown that meanwhile became university location.*