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Does Learning Beget Learning Throughout Adulthood? Evidence from Employees' Training Participation

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ABSTRACT

Does Learning Beget Learning Throughout Adulthood? Evidence from Employees' Training Participation*

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 a reform of the schooling system and 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

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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.

A large number of empirical studies have shown that schooling and training participation later in life display a strong complementary relationship (e.g. Lynch 1992, Lynch and Black 1998, Blundell et al. 1999, Pischke 2001, Fouarge et al. 2013, Görlitz and Tamm 2016). However, the mechanisms driving the training gap between individuals with high and low levels of schooling are not yet well understood because the previous literature generally does not identify causal relationships. We contribute to this literature by using information on exogenous variation in length of schooling due to a reform of the schooling system and on the buildup of universities to identify the causal impact of initial investments in schooling and vocational education on training during adulthood.

Our paper is also 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 happiness, health, marriage, parenting or the intergenerational transfer of education have also been analyzed (Oreopoulos 2007, Oreopoulos and Salvanes 2011, Maurin and McNally 2008). The

impact of schooling on lifelong learning, however, received only few 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.

The paper is organized as follows. Section 2 describes the data and presents descriptive evidence on the correlation between initial investments in schooling/vocational 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 attending school from various cohorts. This enables us to use changes in schooling regulations and in the educational structure 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. 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. Furthermore, the NEPS data includes information on the region of birth which is important for applying instrumental variable methods 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 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 for analysis by around 40 percent.[1]

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 instruments resort to information on region of birth. Since our primary focus is on work-related training, we restrict our sample to employed individuals for most of the analysis. Furthermore, we only consider individuals born between 1944 and 1979, who are aged 30 to 67 at the time of interview.[2] Our final sample includes around 8,600 individuals.

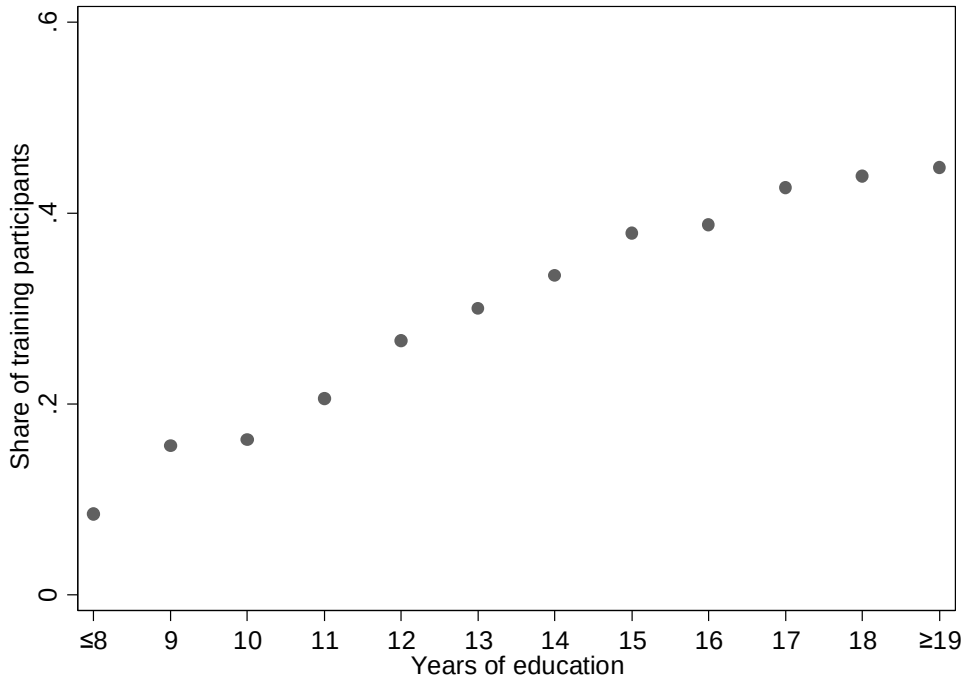
Figure 1 shows differences in participation rates in work-related training by years of education. A clear positive association between education and training emerges which appears to be almost linear. While around 45% of employees with more than 18 years of education participated in work-related training 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.

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). For the NEPS data this is documented in Figure 2. While between ages 30 and 55 the participation rate hovers above 30 percent it shows 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.

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

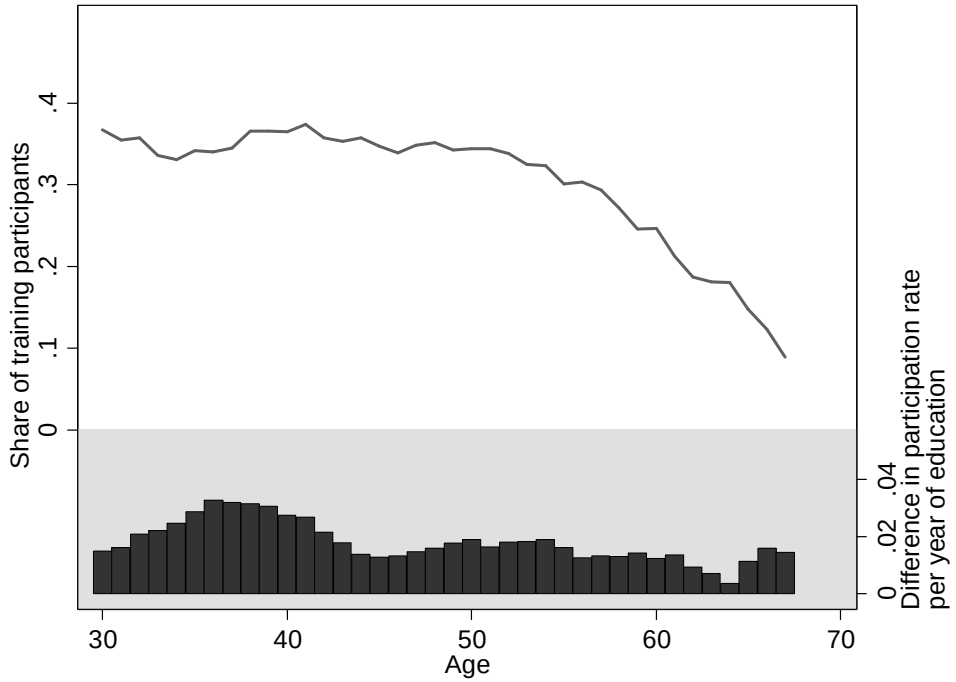
[2] We exclude individuals from younger cohorts, i.e. those born between 1980 and 1986 because few of them already entered the labor market. More importantly, we find that labor market participation in younger cohorts is selectively affected by level of education. Specifically, below age 30 those with 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.

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.

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 percentage difference in participation rate per year of education. (Difference in) participation rates displayed are smoothed using 5-cohorts moving average.

3. Identification Strategy

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

$$train_i = \alpha + \beta\, edu_i + X_i\, \gamma + \varepsilon_i \quad (1)$$

$$edu_i = \theta + \delta\, r_i + X_i\, \lambda + v_i \quad (2)$$

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, state 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. 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 instruments r_i and the same variables X_i that are included in equation (1). We use two types of instruments. The first instrument indicates whether an individual was affected by a schooling reform, the second instrument indicates the availability of universities in the individual's region of birth. Details about the instruments are presented below.

The schooling reform

The first instrument indicates whether an individual was affected by a schooling reform that increased the years of compulsory schooling from 8 to 9 years. This instrument has been used in Pischke and von Wachter (2008) to analyze the impact of education on wages and in several other studies on outcomes such as health, fertility, and political attitudes among others (e.g. Siedler 2010, Kemptner et al. 2011, Cygan-Rehm and Maeder 2013, Piopiunik 2014).

The German secondary school system is traditionally divided into three tracks. After having completed elementary school in four years, students transfer to one of three secondary school types: the basic (*Hauptschule*), the intermediate (*Realschule*) or the academic track (*Gymnasium*). The first two tracks enable students to apply for an apprenticeship and the academic one for college. Each of these options is of different length. It takes 8 to 9 (basic), 10 (intermediate) or 13 (academic) years to graduate from the different schools tracks. In general, education policy is regulated by the federal states. However, several efforts have been made to reach a certain degree of harmonization in this field. Especially in the decades after World War II major reforms have been undertaken to establish comparability of educational attainment over the federal states. The Hamburg Accord in 1964 (*Hamburger Abkommen*) was one important step. Besides other declarations, the act included an

agreement to increase the years of compulsory schooling of the basic track from 8 to 9 years (for more details see Pischke and von Wachter 2005). Politicians aimed to reach several goals with this law. The universal aim was to increase the educational level of this track significantly, aside from the provision of a better preparation for the professional life (Nordrhein-Westfalen 1962, Linke 1962).

The timing of the increase in school duration differs by state. Table 1 summarizes information on the first birth cohort that has been affected and the year of implementation by state. While some states in the northern part of Germany, such as Hamburg, had already increased the number of years in basic school before 1950, most of the other states followed during the 1960's. Bavaria was the last state where the basic track was extended from 8 to 9 years. The described reform allows exploiting variation in the length of schooling across West German states over time. We use the state and date of birth to assign treatment status of individuals. This differs from the proceeding in Pischke and von Wachter (2008) and most of the other studies relying on the schooling reform which due to data availability restrictions have to resort to state of residence at the time of interview.[3]

Table 1 – Timing of the schooling reform by federal state

Federal state	First birth cohort	First year of implementation
Hamburg	April 1931	1946
Schleswig-Holstein	April 1932	1947
Saarland	April 1943	1958
Bremen	April 1944	1959
Lower-Saxony	April 1947	1962
Baden-Wuerttemberg	April 1952	1967
Hesse	April 1952	1967
North Rhine-Westphalia	April 1952	1967
Rhineland-Palatinate	April 1952	1967
Bavaria	July 1954	1969

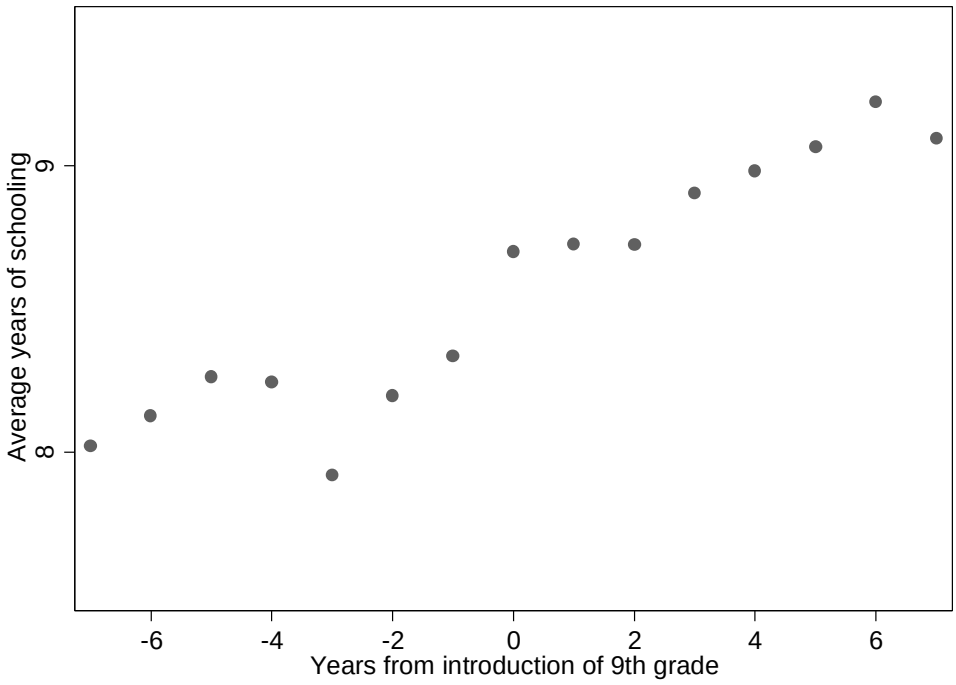
Source: Leschinsky and Roeder (1980).

Figure A1 in the appendix gives an indication of the importance of the reform for the population under analysis. Among cohorts born during the 1940s and 1950s, the share of students attending the basic track was between 40% and 60%. Thus, even though we are measuring a local average treatment effect (LATE), the target group of low-track students is quite representative for these birth cohorts. The apparent cohort to cohort fluctuations are due to limited sample size in the NEPS data, but the overall picture resembles descriptives in Pischke and von Wachter (2005).

[3] Using the NEPS data, we find that at the time of interview only about 76% of individuals in our analysis sample are still living in the same state they were born in (the share is 79% if we compare the state at time of interview and the state of residence at age 10 when the reform was effective). According to our data, the mobility between states would lead to misclassification of treatment status of around 5% of individuals, if the state of residence at the time of interview is used instead of the state of birth.

Figure 3 provides a first graphical indication that the reform had an impact on years of schooling. It displays the average number of years of schooling of basic track students who finished school shortly before or shortly after the reform came into effect. Figure 3 shows that the last cohorts of pre-reform students attending the basic school track had slightly above 8 years of schooling and that after the reform the number of years of schooling rapidly rose to around 9 years. The finding that the first couple of cohorts that were affected by the reform still had slightly less than 9 years of schooling might be due to short school years that were temporarily implemented parallel to the schooling reform in some of the states (cf. Pischke 2007).

Figure 3 – Average years of schooling before and after the reform (students from basic track)



Note: NEPS data. Sample restricted to individuals born in West Germany who attended the basic school track. Treatment status defined according to state and date of birth.

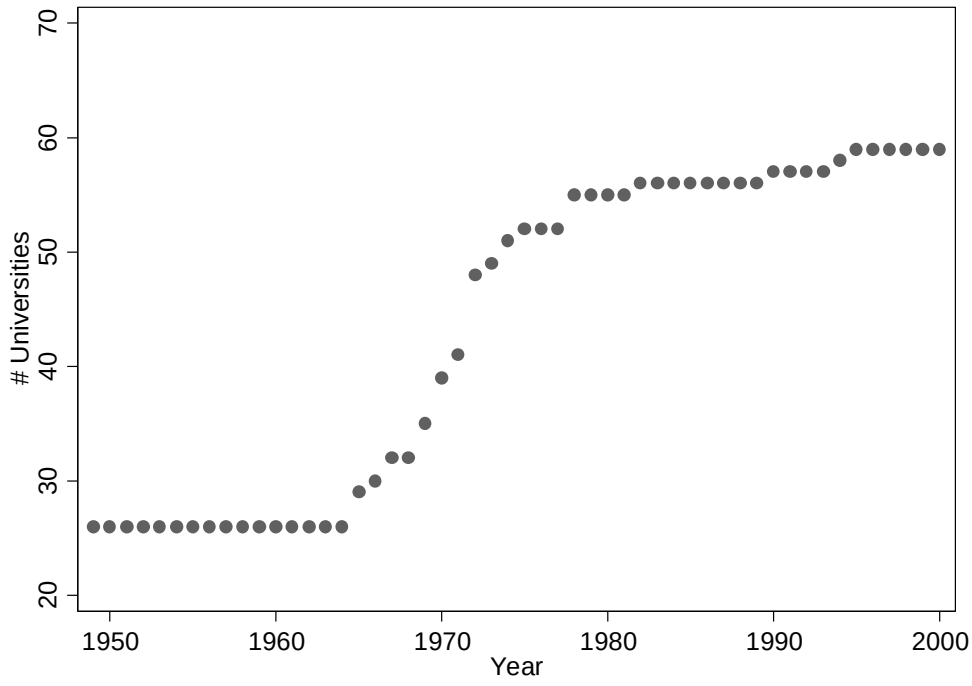
Educational expansion and the buildup of universities

The second 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.[4]

Figure 4 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 A2 in the appendix illustrates the regional distribution of universities over time.

Figure 4 – 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] 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.

4. Empirical Results

Table 2 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 1.8 percentage point higher training probability.

When looking at the IV results, the first stage indicates that the schooling reform increased years of education by 0.6 and 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 and even larger than the OLS estimate. According to the IV estimate each year of education gained early in life raises the training probability by 3.1 percentage points. This confirms that education has a causal impact on training participation later in life.

Robustness of the results

In the following we present several robustness tests. Because the timing of the schooling reform and 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 specification (2) we present results that additionally account for squared state-specific cohort trends and in specification (3) we drop all state-specific cohort trends. Results in Table 2 show that controlling for state-specific trends has only minor effects on the IV estimates. The impact of years of education on training is unchanged by including squared state-specific cohort trends and it drops somewhat to 2.7 when dropping the state-specific cohort trends in specification (3). In specification (3), however, the schooling reform instrument turns marginally significant in the first stage.

Another potential threat to the validity of the instruments are differences between counties that are correlated with educational choices and training. In our main specification, we might not account for these differences as the availability of universities is measured at the county level while regional fixed effects are included at the state level. 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. Table 2 presents two alternative specifications that try to rule out that potential confounders at the regional level that are fixed

over time contaminate the results. First, specification (4) includes dummies for labor market regions. Labor market regions are defined following Kosfeld and Werner (2012) and comprise areas characterized by close commuter links. There are 108 labor market regions in West Germany that are made up of between one and eleven counties. Controlling for labor market region fixed effects does not challenge our findings. If any, the IV estimate gets larger with a point estimate of 4.9. Second, in specification (5) we include an indicator for counties having a large university in the year 2008, i.e. in this specification the instrument *university in county by age 18* is similar to a difference-in-differences term. In specification (5) the F-statistic of excluded instruments in the first stage becomes small and the second stage coefficient for years of education turns insignificant. Yet, the point estimate is 4.2 which is well within the range of estimates of the other specifications.

For instrumental variable estimates with several instruments, Angrist and Pischke (2009) recommend also to present estimates using the single best instrument, which in our case is university in county by age 18, and to drop all other instruments. The respective results are presented in specification (6). They are close to the main results.

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 specification (7), the instrument also accounts for those other types of colleges. We find that this leads to a somewhat smaller first stage coefficient. The second stage result, however, remains statistically significant, confirming the impact of education on training. The point estimate is 4.6 which is even somewhat larger than in the main specification.

As noted in section 2, our data comprises training information on individuals that were interviewed at different points in time. In specification (8), 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 6,064 but the IV estimate is 3.4 which is close to the main specification. Furthermore, in specification (9) 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.

Table 2 – The impact of education on training participation

	Basic specification (1)	Spec. (1) with squared state-specific cohort trends (2)	Spec. (1) without linear state-specific cohort trends (3)	Spec. (1) with labor market region fixed effects (4)	Spec. (1) with dummy for counties with university in year 2008 (5)	Spec. (1) using only university instrument (6)	Spec. (1), university instrument accounting for all types of colleges (7)	Spec. (1), sample restricted to wave 2009/2010 (8)	Spec. (1), sample restricted to individuals with 7 to 28 years of education (9)
OLS estimates									
Years of education	0.0181***	0.0181***	0.0181***	0.0183***	0.0180***	0.0181***	0.0181***	0.0180***	0.0232***
	[12.37]	[12.37]	[12.33]	[12.40]	[12.04]	[12.37]	[12.37]	[10.30]	[15.31]
IV estimates									
Years of education	0.0307**	0.0299*	0.0269*	0.0489**	0.0426	0.0291*	0.0457**	0.0339*	0.0336**
	[1.98]	[1.84]	[1.76]	[2.32]	[1.06]	[1.80]	[2.56]	[1.85]	[1.97]
First stage									
Schooling reform	0.6496	0.4755	0.6951*	0.6055	0.6526		0.6682	0.2486	0.3181
	[1.45]	[0.95]	[1.73]	[1.36]	[1.47]		[1.50]	[0.66]	[1.14]
University	0.8756***	0.8609***	0.8786***	0.7462***	0.6545**	0.8783***	0.6147***	0.9082***	0.8212***
	[7.66]	[7.47]	[7.69]	[5.57]	[2.06]	[7.71]	[5.87]	[6.31]	[8.65]
F-test (excluded instruments)	30.59	28.35	31.31	16.12	3.06	56.92	21.46	19.78	33.09
Observations	8639	8639	8639	8639	8639	8639	8639	6064	8492

*Note: NEPS data. Sample restricted to employees born in West Germany, cohorts 1944 to 1979. All regressions include cohort fixed effects, state fixed effects, linear state-specific cohort trends, gender and an indicator for wave 2011/2012. Specification (2) additionally includes squared state-specific cohort trends. Specification (3) is without linear state-specific cohort trends. Specification (4) additionally includes labor market region fixed-effects. Specification (5) estimates a difference-in-differences model for university buildup. Specification (6) uses only the single best instrument (university in county by age 18). In specification (7) the university instrument accounts for all types of colleges, not only for large universities. Specification (8) drops observations from wave 2011/2012. Specification (9) drops individuals with extremely low or high number of years of education. t-tests in brackets are adjusted for clustering at the state-cohort level. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.*

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 begets 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 slightly 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.

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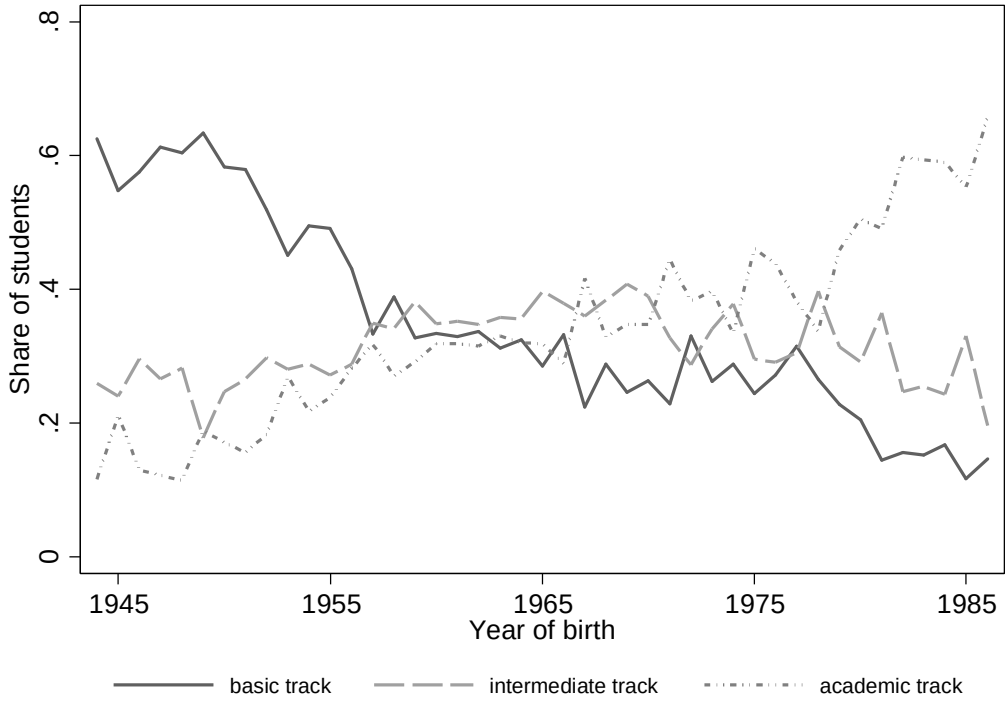
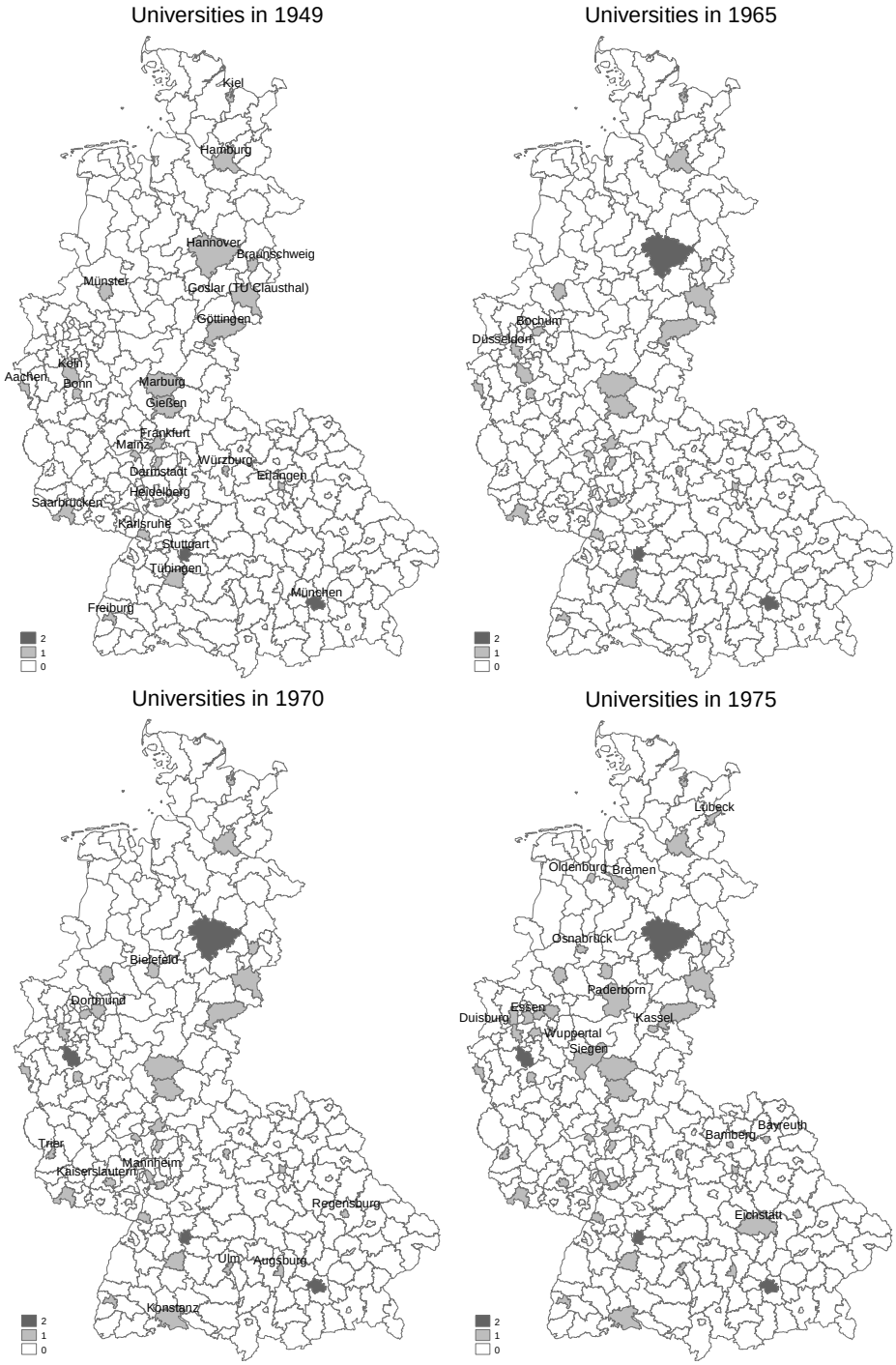
Appendix

Figure A1 – Distribution of students to school tracks by cohort



Note: NEPS data. Sample restricted to individuals born in West Germany. Lines indicate share of students by school track.

Figure A2 – 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.