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Daniel A. Kamhöfer and Hendrik Schmitz[1]

Analyzing Zero Returns to Education in Germany – Heterogeneous Effects and Skill Formation

Abstract

We analyze the effect of education on wages using German Socio-Economic Panel data and regional variation in mandatory years of schooling and the supply of schools. This allows us to estimate more than one local average treatment effect and heterogeneous effects for different groups of compliers. Our results are in line with previous studies that do not find an effect of compulsory schooling on wages in Germany. We go beyond these studies and test a potential reason for it, namely that basic skills are learned earlier in Germany and additional years of schooling are not effective anymore. This is done by also estimating the effect of education on cognitive skills. The results suggest that education after the eighth year does not seem to have a causal effect on cognitive skills in Germany. This is consistent with the explanation for zero effects of schooling on earnings.

JEL Classification: I21, J24, C26

Keywords: Returns to education; skills; IV estimation

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1 Introduction

One of the most discussed topics in applied econometrics are returns to education. While a positive relationship between education and earnings is confirmed worldwide, the evidence on causal effects of education on wages is not unambiguous. For the UK, the US, and Canada (see for instance Angrist and Krueger, 1991; Oreopoulos, 2006), causal returns to schooling are found to be in the range of 10-15% per year. There is no clear pattern in Continental Europe, however. For some countries, e.g. Norway (Aakvik et al., 2010) and Sweden (Meghir and Palme, 2005) there is evidence for positive effects, but for others, like France (Grenet, 2013) and the Netherlands (Oosterbeek and Webbink, 2007) the earnings returns to additional education seem to be zero. Likewise, in Germany, the studies by Pischke (2007) and Pischke and von Wachter (2008) find zero returns to (additional compulsory) schooling.

One way to explain the mixed evidence is that these studies use different instruments to account for the endogeneity of education. Hence, different local average treatment effects (LATEs, see Imbens and Angrist, 1994) represent effects for different groups of compliers. This might explain the differing results of Ichino and Winter-Ebmer (1999, 2004) and Becker and Siebern-Thomas (2007) who do find a positive effect of schooling in Germany using fathers involved in World War II, having a degree higher than high school, and the type of agglomeration, respectively, as instruments.

However, most international studies use compulsory schooling reforms after World War II, mostly targeting at similar groups of students, namely the comparably low skilled ones in the basic tracks. The zero returns to compulsory schooling in Germany (Pischke and von Wachter, 2008) can, thus, not only be explained on methodological grounds (i.e., by different instruments and, thus, different compliers) but probably also by institutional differences, e.g., between Germany and UK/US. Pischke and von Wachter (2008) can rule out wage rigidities and the prominent role of apprenticeships in Germany as explanations for the zero returns. They hypothesize (but get only indirect evidence) that the extra year of schooling did not enhance labor market relevant skills which are formed earlier in the school life in Germany than in the US.

We contribute to the literature on returns to schooling in two important dimensions. First, we re-analyze the compulsory schooling reform in Germany but also widen the set of instruments by two more variables that capture the institutional environment – the supply of schools – and that do not affect the basic track students (like the reform did) but intermediate and academic track students. Hence, we compute local average treatment effects for different groups of compliers and can get a broader picture of returns to schooling. This does not completely solve the critique of Deaton (2010) and

Heckman (2010) that the LATE is a too narrow parameter to allow meaningful interpretations but it is a step towards increased external validity. Second, we directly test the conjecture of Pischke and von Wachter (2008) that lack of skill formation could be a reason for zero returns in Germany by estimating the causal effect of education on cognitive abilities. Apart from being an explanation for the wage-returns-to-schooling discussion, this is a contribution by itself as there are only very few studies on that in the literature – to the best of our knowledge none so far focusing on Germany, the largest country in the European Union.

Our results reinforce the Pischke and von Wachter (2008) findings. We do not find any economically significant causal effect of schooling on wages in Germany, neither for basic nor for higher tracks. The reason might be a lack of skills learned in the higher grades in Germany as we also do not find significant effects of education on cognitive skills for any group. The remainder of the paper is as follows. Section 2 gives a brief introduction into the German educational set-up. Section 3 summarizes the relevant literature on education and wages as well as cognitive skills. Section 4 explains the data and variables. Section 5 discusses the instruments while Section 6 presents the results. Section 7 concludes.

2 Education in Germany

Germany is a federal republic and its 16 federal states are in charge of education policy. Therefore, there is not one common educational system but rather 16 separate – and in some ways competing – ones. Because of data restrictions described in Section 4 and the stark different education in East Germany before 1990, the scope of the analysis are West-German non-city states (see Table 1 for the states). For the years under review (birth cohorts 1940-1970), enrollment into elementary school is at the child's age of six in all states. After grade four, students visit a secondary school of one out of three possible tracks. Which track a student is assigned to, basically depends on the performance in elementary school (Dustmann, 2004).

The tracks are distinguished by the years of schooling, the academic content of the curriculum, and the leaving certificate. Basic track schools (*Hauptschulen*) covered grades 5 to 8 before the compulsory reform in Germany and included a ninth grade afterwards. As Table 1 shows, all included states initially had eight years of compulsory schooling and added one year over time. There is some variation in the timing of the compulsory schooling reform.

Table 1: Introduction of the compulsory reform by states

	Year of introduction of a mandatory ninth grade	First birth cohort affected by the reform	Share of students affected by the reform
Schleswig-Holstein	1956	1941	76.0%
Lower Saxony	1962	1947	80.5%
North Rhine-Westphalia	1967	1953	74.6%
Hesse	1967	1953	71.4%
Rhineland-Palatinate	1967	1953	78.3%
Baden-Württemberg	1967	1953	72.6%
Bavaria	1969	1955	78.5%
On average			76.0%

Source: columns 1 and 2 are taken from Pischke and von Wachter (2005), column 3 based on own calculations of data provided by the German Statistical Yearbook (German Federal Statistical Office, 1992).

After leaving the basic school, students typically start an apprenticeship. This is part-time training-on-the-job and part-time schooling in the field of work. The duration is usually three years and one enters the firm (or another firm in the sector) as a full-time employee afterwards. In academic schools (*Gymnasien*) students receive a degree qualifying for university entrance (*Abitur*) after grade 13. Afterwards, many students decide to have university studies (in our sample 78%, see Table A1 in the Appendix). For further information see KMK (2010). Beside basic and academic secondary schools, a third track became popular after World War II. In intermediate schools (*Realschulen*), students reach the leaving degree after grade 10. Even if the degree is different from the basic track degree, students usually enter vocational training afterwards (nearly 89% do so, see Table A1).[1]

3 Previous Studies on Education, Earnings, and Cognitive Skills

3.1 Education and Earnings

This section concentrates on literature which employs IV estimation to ensure causal inference. As the IV method provides a local average treatment effect, the effect is dif-

[1] Additional to *Realschulen* some states offer a comprehensive school track (*Gesamtschule*). Since comprehensive schools play only a minor role and most students leave after grade 10, we count comprehensive schools as intermediate schools, too. Even when leaving comprehensive schools out, the results remain unchanged.

ferent from the average treatment effect (ATE) in the likely case of heterogeneous returns to education, as pointed out by Card (1995a, 2001). In anglo-saxon countries, an additional year of schooling – usually instrumented by a change in compulsory schooling – is associated with a positive effect exceeding the OLS one (see Card, 1999). The compliers to the compulsory schooling instrument are those people who visit school longer due to increased compulsory education. In the US, Angrist and Krueger (1991) find an earnings effect of 7.5% for this group. Using college proximity to instrument education, Card (1995b) finds returns above 10% for more educated compliers. For one additional year of compulsory schooling in UK, Harmon and Walker (1995) estimate a 15% increase in wages but neglect birth cohort fixed effects. In a more recent re-analysis Devereux and Hart (2010) find only a 3% increase. For the US, UK, and Canada, Oreopoulos (2006) finds 14.2%, 15.8%, and 9.6% per year of additional schooling, respectively. Regarding Scandinavian countries, Meghir and Palme (2005) and Aakvik et al. (2010) find a positive causal effect of compulsory schooling on earnings for Sweden and Norway. However, for the group of workers with the lowest level of skills, it seems to disappear. Evidence for Central European countries even indicates no effect at all. Oosterbeek and Webbink (2007) and Grenet (2013) find zero returns for the Netherland and France, respectively.

For the largest European economy, Germany, Pischke and von Wachter (2008) find no effect of the German compulsory schooling reform depicted in Section 2. Using data of the Qualification and Career Survey and the Micro Census, the estimated IV coefficients are close to zero and robust to changes in the specifications. Pischke and von Wachter (2008) find that neither wage rigidities nor an apprenticeship degree after leaving the basic track can explain the results. A reason for zero returns suggest by the authors but not tested is the local perspective of the estimator. Using only compulsory schooling to instrument education, they are not able to dismiss that heterogeneity drives the results. Pischke and von Wachter (2008) see the major reason for zero returns in the role of skills. If there would be no gain in (wage relevant) skills due to the ninth grade in Germany but in e.g. the UK/US, this could also explain the ambiguous pattern. However, Pischke and von Wachter (2008) do not directly investigate skill formation as their data do not provide information on abilities. In the same vein as Pischke and von Wachter (2008), Pischke (2007) analyzes the effect of short school years. For this reform the de jure change in the length of schooling (up to two thirds of a year) is smaller then for the compulsory schooling reform. Regarding educational returns, short school years cause an increase in the probability of grade repetition but have no effect on earnings.

Further evidence for Germany is by Ichino and Winter-Ebmer (1999, 2004). They use data of about 1,800 full-time employed males taken from the German Socio-Economic

Panel. The authors take heterogeneous returns to education int account and allow different marginal rewards and marginal costs. They assume that both factors can be expressed in a binary way, resulting in four groups of persons: low abilities/low financial constraints, high abilities/low financial constraints, low abilities/high financial constraints, and high abilities/high financial constraints. Ichino and Winter-Ebmer (1999) use father's involvement in the World War II to instrument education for the first group (they call them "the stupid rich") and whether the father has an academic secondary school degree as instrument for the last group ("the smart poor"). The estimated effects are 14% and 4.8%, respectively, indicating indeed heterogeneous returns. Because of concerns regarding the exogeneity of instruments the authors interpret the estimates as upper and lower bounds. Ichino and Winter-Ebmer (2004) additionally exploit own involvement in World War II and find similarly results. As already stressed in the introduction, we interpret the different results in the light of the different LATEs. This is particularly the case because Pischke and von Wachter (2008) and Pischke (2007) use institutional variations to instrument the education while Ichino and Winter-Ebmer (1999, 2004) use variations in the social background. For the latter instruments, compliers are not assigned by the secondary school track. Another study is by Becker and Siebern-Thomas (2007). They use the type of the agglomeration during adolescence to instrument education. Their results are in the range of the results of Ichino and Winter-Ebmer (1999, 2004).

Like our paper, Saniter (2012) wants to broaden the understanding of education and wage by explaining the differences between the works of Pischke and von Wachter (2008) as well as Ichino and Winter-Ebmer (1999, 2004) and Becker and Siebern-Thomas (2007). Saniter (2012) does not use IV but a control function approach with conditional heteroskedasticity as proposed by Klein and Vella (2010). Using this approach, the model is not identified through an exclusion restriction but because the impact of the error term of the education equation is assumed to vary across the covariates. By employing the approach for different sub-samples, Saniter (2012) finds that OLS regression over-estimates the effect for basic school students and under-estimates the returns for academic school students. As the author points out, these causal findings are only true within a sample. Using the approach of Klein and Vella (2010) "does not, however, allow for causal inference across the sample as self-selection into higher and lower education is still a problem" (Saniter, 2012, p.23). In this paper, on the contrary, we use the instruments to overcome the self-selection problem.

3.2 Education and Cognitive Skills

So far, the existing literature on the effect of education on skill formation either focuses on males in the late teens or on an aged population (50+). Falch and Massoh (2011) estimate the effect of an additional year of education on the difference between a broad intelligence test score of Swedish military enrollment data at the age of 20 and an intelligence test score of the Malmö Longitudinal Dataset at the age of 10. The effect found by this value added approach is about 0.2 standard deviations (SDs). Carlsson et al. (2012) exploit that the date of military eligibility tests was randomly assigned in Sweden. This leads to an independence of the test date and days of schooling at the age of 18. The estimated effect of 10 additional days of schooling is 1% of a SD for the crystallized intelligence test score and zero for fluid intelligence. Brinch and Galloway (2012) use a change in compulsory schooling in Norway and estimate an effect of 3.7 points increase of the intelligence test score (mean: 100, SD: 15) per year of education. Separate estimations for whites, backs, and hispanics are given by Cascio and Lewis (2006). Instrumenting education with birthdays near the school-entry cutoff, they only find an significant effect about 0.35 SD for non-white persons in the US.

To the best of our knowledge, only four studies provide causal evidence focusing on a population other then young males. These studies aim at a population of 50 years and older. They all use changes in compulsory schooling to instrument education. The first study is from Glymour et al. (2008). They apply spilt-sample IV to the US Health and Retirement Study (HRS) and data from the 1980 US 5% census. Because the group of people affected by the compulsory schooling reform is rather small, the average actual effect in less than 0.05 school years to a de jure increase of of 1 year. The effect of one additional year of education on a memory test score is 0.18 SD. Taken a scale of the mental status as outcome, the coefficient is close to zero. By having a clear cutoff point, Banks and Mazzonna (2012) employ fuzzy regression discontinuity design to data of the English Longitudinal Study on Aging (ELSA). For males with mandatory years of education, they find an effect of education on memory and executive functioning up to 0.5 SD at a 5% significance level, depending on the bandwidth. For females only the effect on memory is significant at the 10% level. For individuals leaving education later then mandatory, the coefficients are somewhat lower and insignificant. For Continental Europe results by Schneeweis et al. (2012) and Mazzonna (2012) based on the Survey of Health, Aging and Retirement in Europe (SHARE). Schneeweis et al. (2012) pool the information on Austria, the Czech Republic, Denmark, France, Germany, and Italy and estimate the effect of additional schooling on memory, crystallized intelligence, numeracy, and orientation in time. Only the effect on memory is significant and ranging between 0.14 and 0.37 SD, depending on the sample. To instrument years of education, Mazzonna (2012) uses information whether a person was the first-born or not additionally to the compulsory school length. Regarding the first-born instrument, he is not able to fully rule out "endogeneity concerns" (Mazzonna, 2012, p.19). Nevertheless, he argues that an endogeneity bias only plays a minor role. Using compulsory schooling, the effect on memory is 0.28 SD for males at the 10% significance level, for females the coefficient is lower and insignificant. When education is instrumented with the birth order, the coefficients decrease about 10 percentage points and significance remains only in the sample restricted to a 10-years bandwidth around the reform.

Our analysis of the effect of education on skills extents the previous literature in several ways. First, our sample is neither limited to young males nor individuals aged 50 or older. In this respect, the sample is more comparable to the average population. Second, this is the first analysis with a focus on Germany. Since the evidence on the effect of schooling on wages presented in Section 3.1 is different for Germany, one should probably not draw conclusions for Germany from studies based on another educational system. Third, all studies so far but Mazzonna (2012) use only one instrument and hence estimate just one LATE. Here again, we take heterogeneous returns into account by instrumenting education for all groups of students. By widening the understanding of the direct effect of education on cognitive skills, this paper underlines the findings of Bingley and Martinello (2013) and Mazzonna and Peracchi (2012) who show that education is a strong confounder even when analyzing other determinants of cognitive functioning, e.g., retirement age.

4 Data and Variables

Starting in 1984, the Socio-Economic Panel (SOEP) is the most important German longitudinal household survey containing yearly information on about 22,000 individuals (Wagner et al., 2007). We use the 2006 wave that, apart from information on educational background and wages, also includes cognitive skills measures which are generated by ultra-short intelligence tests. In the test used here, respondents have to name as many animals as possible in 90 seconds. The test score is the number of correct unique answers. The measured ability is word fluency, which is a form of "crystallized intelligence". This type of intelligence includes e.g. the so-called problem solving ability (Lichtenberger and Kaufman, 2009). In a second test "fluid intelligence" is assessed by perceptual speed. We disregard this type of intelligence because it is innate, thus education is supposed to have no impact of the test score. Both ability measures refer to different modules of the well-established Wechsler Adult Intelligence Scale (WAIS), see Lichtenberger and Kaufman (2009). Lang et al. (2007) show that the ultra-short intelligence tests applied in the SOEP are comparable to more extensive ones used in psychology. In order to simplify the interpretation of the crystallized intelligence test score, we use it's log value. For a documentation see Schupp et al. (2008), for general information on the SOEP data see Wagner et al. (2007).

As a measure of earnings we use the log of hourly gross wage in 2006. It is calculated by dividing the monthly gross income by 4.3 weeks per month times the number of hours worked per week. For the years of education, we follow Pischke and von Wachter (2008) and compute it by using the regular length of the track, taking the compulsory reform in the case of basic schools into account. [2]

For the wage regression sample, we start with SOEP information on over 12,000 individuals who participate in the labor market in 2006, so we can calculate the hourly gross wage. Since our instruments are limited to West-Germany, we drop 2,500 respondents from the East. We leave out 630 persons living in the three city states Berlin, Bremen, and Hamburg. This is because, students living in city states but visiting schools in the surrounding state, build a considerable share of all students in the small states. Furthermore, we restrict our sample to respondents born in 1940 or later because the school supply information is only available after 1950. This amounts to a further loss of 94 observations. Additionally, 2,470 respondents born after 1970 were dropped. After dropping individuals with missing values in covariates, the final sample has information on nearly 5,500 people. For the ultra-short intelligence tests, a computer assisted personal interview (CAPI) was needed and only one third of all SOEP respondents were randomly asked to participate in the tests. Therefore, the cognitive test sample includes only 2,500 observations.

Control variables in the regressions are gender and birth cohort as well as state fixed effects. Further variables – which might depend on education and are therefore left out in the preferred specification – are added later on to check the robustness.

5 Identification

Since the results of instrumental variables estimation depend on the instrument and the external validity of one local average treatment effect may be considered limited (see e.g. Heckman, 2010), we use three instruments which capture the effect for all

[2] We are able to assign the federal state using retrospective information on the state of last school attendance. If this information is missing, the state of residence in 2006 is used. Due to a low migration rate across the states in Germany, a misleading assignment is only a minor problem, see e.g. Becker and Siebern-Thomas (2007).

kinds of students in the German educational system. We apply all instruments one by one. Doing so, this paper adds to knowledge on heterogeneity of the effects.

The first instrument is the increase in compulsory years of education. According to Table 1, basic track students in our sample are either exposed to 8 or 9 years of mandatory schooling. The obligatory character of the reform and the large share of compliers (about 76%, see Table 1) guarantee a high relevance regarding the actual years of education. It is plausible to assume the independence of the reform with respect to wages and cognitive skills. Petzold (1981) argues that the main reason for the reform was that 14-year-olds did not have the maturity to enter the labor market. Thus, the policy decision to increase compulsory schooling is likely to be independent of income and skills.[3]

While the compliers of the compulsory schooling reform are only basic track students, we use the supply of schools in the two other tracks to get local average treatment effects for students of these schools. We measure the supply as the number of both intermediate and academic schools, respectively, per square km in the state of residence at the student's age of 10. Here, our identifying assumption is that school supply is an exogenous determinant of school choice. Academic school supply was used by Jürges et al. (2011) as an instrument in the context of health and health behaviours.

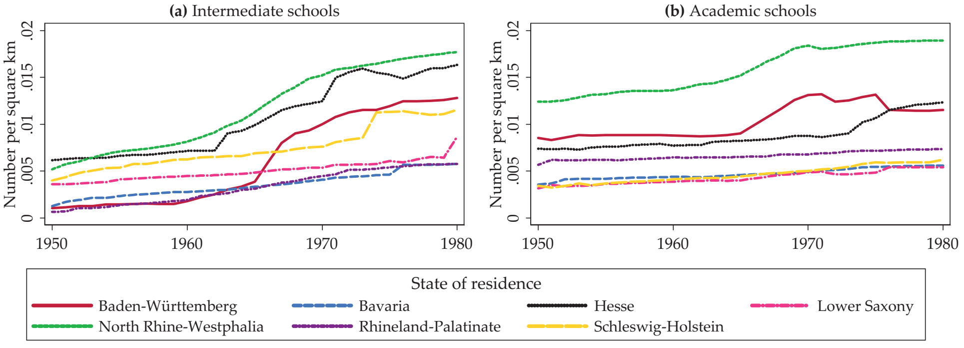
An increased supply of intermediate (respectively academic) schools enables more students to visit such a school, see Freier and Storck (2012). This is the case for two reasons. First, the competition about the available places per school is lower. Second, the availability of intermediate and academic schools is likely to be higher in rural areas. This decreases the costs (e.g. commuting) of visiting a school which offers more years of education. The idea of instrumenting education by the availability of educational institutions goes back to Card (1995b).

Information on the supply of schools is taken from several issues of the German Statistical Yearbook (German Federal Statistical Office, 1992). Figure 1 shows the number of intermediate (part a) the academic schools (part b) per square km per state over time. We see that there is a lot of variation among and across states in school supply. With the educational expansion in the 60s and 70s, all states increased the number of schools but starting points and intensity varied across states. This generates exogenous variation in school supply that can be used to identify causal effects of schooling on wages and skills. Since we use a full set of year of birth and federal state dummies we basically exploit state level deviations from the national trend in increased school supply.[4]

[3]A similar reasoning for the independence of the German change in compulsory schooling can be found in Pischke and von Wachter (2008) for wages and in Schneeweis et al. (2012) for cognitive skills.

[4]As Jürges et al. (2011) we do not include state-specific trends as this would discard this variation and would reduce the explanatory power of the school supply instruments considerably. The second-

Figure 1: The number of intermediate and academic schools per square km



Source: Own calculations, data taken from the German Statistical Yearbook (German Federal Statistical Office, 1992). Since birth cohorts 1940-1970 are used and school supply is measured at the respondent's age of 10, we use information on school supply from 1950-1980.

The identifying assumption regarding school supply is that the variation in the timing of the educational expansion is independent from wage and skill expectations. This assumption would be violated if individuals with lower income expectations (or worse skills) demand more schools to improve their (or their childrens') chances on the labour market relative to individuals from other states. This is unlikely to be the case and, if so, should largely be taken into account by the state fixed effects. More likely reasons for the different timing are electoral cycles and political preferences (see Hadjar and Becker, 2006 and Jürges et al., 2011).

Another concern might be the weighting of the number of schools with the state's area instead of, e.g., the state's cohort size. Here we argue that three reasons challenge the use of schools per students as instrument (see Jürges et al., 2011). First, the cohort size is more volatile and would thus mainly drive the instrument's value instead of the number of schools. Second, the cohort size probably affects earnings directly (see e.g. Freeman, 1979 and Welch, 1979), which threats the validity. Third, the schools per students instrument would no longer take the average distance to school into account.

stage results would, however, not change qualitatively. I.e., the second-stage coefficients are not large and significant after including state-specific trends, thus not leading to different conclusions.

6 Results

6.1 The Effect of Education on Wages

Table 2 reports the coefficients of the regressions of log hourly gross wages and the log crystallized intelligence test scores on years of education and control variables. Each of the 14 cells is the result of one different regression. For the sake of clarity, we only report the coefficients of the instruments in the first stage regressions and of years of education in the second stage regressions. The first column shows results from simple OLS regressions, thereby neglecting any endogeneity problems. Column 2 shows results of IV regressions with compulsory schooling as an instrument. This instrument refers to the basic track. Column 3 uses the number of intermediate track schools per square km in the state and column 4 the number of academic track schools per state as instrument.

The first-stage results are presented in the first line of Table 2 for the wage sample. Students affected by the compulsory schooling reform attend school on average 0.91 years longer due to the reform. Since the de jure change was one year, the result may seem high, but it should be taken into account that a large share of students (those in basic schools) was affected. In order to interpret the school supply results, we linearly re-scale the instruments as a change in the number of schools per square km by one unit would be unrealistically high, compare Figure 1. The coefficients in Table 2 are, thus, to be interpreted as effects of an increase in the number of schools by 0.005 schools per square km. This is, for example, the difference in the increase in intermediate schools between the states of Baden-Württemberg and Rhineland-Palatinate from 1960 to 1970. In other words, the first-stage coefficients give the effect of the relatively higher increase in the number of intermediate/academic schools on years of education. If one lives, e.g., in the state of Baden-Wüettemberg instead of Rhineland-Palatinate, the person receives on average 0.46 years more schooling because of the higher increase of intermediate schools in Baden-Württemberg. When education is instrumented with the supply of academic schools, the effect is 0.84 years. All three instruments are highly significant. Accordingly, the instrument F-statistics at the bottom of Table 2 are all above the Staiger and Stock (1997) rule of thumb value of 10.

Regarding the structural equation and wages as dependent variable, the OLS coefficient is statistically different from zero. An increase by one year of education goes along with about 6.9% higher wages. The magnitude is in line with the one in Pischke and von Wachter (2008, Table 2) although they use different data sources. Contrary to the OLS case, the IV coefficients of education are not only statistically but also economically insignificant. Using the instruments for all tracks of secondary schooling

Table 2: Estimation results

Dependent variable	OLS	IV		
		Instrument referring to		
		Basic	Inter.	Acad.
First stage results				
Measure of earnings:				
Years of education	– –	0.9079***	0.4595***	0.8361***
		(0.1894)	(0.1230)	(0.2131)
Measures of cognitive skills:				
Years of education	– –	1.0289***	0.2954*	0.8464***
		(0.2695)	(0.1720)	(0.3041)
Second stage results				
Measure of earnings:				
Log hourly gross wage	0.0686***	−0.0004	−0.0002	0.0010
	(0.0020)	(0.0276)	(0.0373)	(0.0360)
Measures of cognitive skills:				
Log crystallized intelligence test score	0.0483***	0.0192	0.0285	−0.0310
	(0.0051)	(0.0571)	(0.1517)	(0.0807)
First-stage F-statistic (wage regression)	– –	22.99	13.94	15.40

Source: Own calculations based on SOEP data. Numbers of observations: Wage regressions: 5,499; Cognitive skills regressions: 2,518. Control variables: female, as well as state and birth cohort fixed effects. State of schooling $\times$ year aged 10-clustered standard errors in parentheses. Coefficients in the first stage refer to the respective instruments. Coefficients in the second stage refer to years of schooling. Coefficients of other control variables not reported here but available upon request. Significance: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

one by one, the effect of additional education is practically zero. For basic and academic students the sign is even negative (but very small and insignificant). Hence, this observation indicates zero returns to education. Table A2 of the Appendix provides results of several robustness checks where we, (1) use net instead of gross wages as outcome variable (2), limit years of education to primary and secondary schooling (no post-secondary education), (3), add more control variables that were left out in the preferred specification due to potential "bad control" problems,[5] (4), also control for the average number of students per school by track, (5), add interaction terms for gender and the cohort and state fixed effects, and, (6) estimate the reduced-form coef-

[5]The added controls are dummy variables for mother's/father's education (at least intermediate school degree), number of siblings, dummy variables for at least good self-assessed health status, obesity (Body Mass Index > 30), migrational background, university degree, completed apprenticeship, and an ISCO scale-based measure of the skill level demanded by the respondent's job.

ficients. In no specification do we find a significant effect of education on wages for either instrument. All in all, the robustness checks underline the baseline finding of zero returns.

6.2 Explanations: Effects of Education on Cognitive Skills

Pischke and von Wachter (2008) conjecture that the potential reason for zero returns to (additional compulsory) schooling is that German students have learned the important skills already before the additional ninth grade. Because SOEP data do not include information on job-specific skills, the more general measures of cognitive abilities, crystallized intelligence, as presented above is used to test this hypothesis. The same identification problems as with wages appear to be relevant in this case (see Heckman and Vytlacil, 2001), hence, we again prefer IV results with the same instruments as before over benchmark OLS results in Table 2.

The same picture as with wages emerges. Individuals with more years of schooling have higher intelligence test score – about 4.8% with one more year of schooling in the OLS regressions. However, once accounting for the endogeneity of school length, the coefficients approach zero and become insignificant. At first glance, this result may seem puzzling because previous studies (not with German data, though) find a causal effect of education on cognitive skills as presented in Section 3.2. But for reasons mentioned above, it is not implausible that the results presented here differ. The sample we use here is broader than the ones previously analyzed. Moreover, positive effects were mostly found for memory tests as intelligence measure. Previous results for word fluency – the measure we use here – are not always strong and significant either. While recalling a word list (the memory test often used in the literature) may be an appropriate measure to capture the long lasting effects of education when people were retired, this does not need to be the case when also younger individuals are taken into account. Finally, and back to the original explanation of Pischke and von Wachter (2008), previous studies use data from countries with a different institutional setting than Germany.

If one compares the first-stage results of the wage and skills regressions in Table 2, the relevance of the supply of intermediate schools decreases for skills. The instrument is only significant at the 10%, probably due the lower sample size in the skills regression. The other instruments remain highly significant. Even if one would lose faith in the IV results for intermediate tracks, there is no reason to believe that the effect should drastically differ from those in the other two tracks.

Like for the wage regression, we carry out a series of robustness checks, see Table A3. The specifications are the same as those for wages as outcome variable. The results are basically in line with the baseline results. In a handful of estimations in the robustness checks, we find somewhat higher coefficients than before (both in positive and negative direction). They are never significant and, if any, do not systematically point into one direction. We conclude that –– following a statement of James Heckman on the effect of training programs for unemployed – it is fair to say that "zero is not a bad number" to describe the effect of schooling on cognitive skills in Germany.[6]

In this section, we show that there seems to be no effect of education on wages in Germany. Since we also find no systematically and significant effect of education on cognitive abilities, a lack of skills learned in school is an explanation for the zero wage returns to additional education that is fully consistent with the evidence in this paper.

7 Conclusions

In this paper, we examine the effect of education on wages and abilities. Contrary to most other articles on education and wages, we instrument endogenous years of education with three instead only one instrument. This allows us to take the heterogeneity of the effects into account. Thereby, we reinforce Pischke and von Wachter's (2008) result of zero returns to compulsory schooling. Moreover, we establish further evidence of zero returns for intermediate and academic students. By doing so, we show that the results do not only seem to be driven by the "local" nature of the instrumental variable approach.

In a second step we test an established hypothesis for zero returns to schooling, namely that basic skill formation – relevant for the labor market at least – takes places before the ninth grade in Germany. This is done by estimating the causal effect of education on cognitive skills. We, again, find no significant effects of education here which is consistent with the mentioned explanation for no effects of schooling on wages. Of course, this does not prove that basic skill formation *does* indeed take place *before* the ninth grade in Germany. It is, however, some evidence that it *might not* take place *after* the eighth grade. Both positive correlations of education with earnings and skills seem to be mainly driven by selection of higher skilled individuals into more years of schooling.

[6]The statement was published on p.23 of the 6 April 1996, edition of *The Economist* and also cited by, e.g., Lechner et al. (2011).

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Appendix

Table A1: Means of selected variables by track

	Basic	Inter.	Acad.	Total
Income				
Gross hourly wage (in €)	14.64	17.65	23.10	17.84
Gross monthly wage (in €)	2,473	3,053	4,268	3,131
Education				
Years of education	10.26	12.17	16.73	12.57
University degree (in %)	5.00	14.84	78.15	27.42
Apprenticeship (in %)	79.00	88.97	34.23	70.99
Cognitive skills				
Raw crystallized intelligence test score	23.51	28.16	30.92	26.92
Socio-demographic characteristics				
Female (in%)	39.50	47.31	40.36	42.41
Age (in years)	48.32	46.54	47.54	47.51
Mother has intermediate school degree (in %)	6.07	15.91	42.08	19.07
Father has intermediate school degree (in %)	7.64	22.14	51.48	24.36
Number of siblings	2.35	1.67	1.47	1.89
Self-assessed health stats at least good (in %)	46.18	55.60	60.61	53.11
Obesity: Body Mass Index > 30 (in %)	22.00	14.52	11.60	16.67
Migratinal background (in %)	29.09	7.39	7.40	16.08
Measure of skills needed for job[a]	2.19	2.73	3.50	2.74
Observations[b]	2,200	1,894	1,405	5,499
Share (in %)	40.01	34.44	25.55	100

Source: Own calculations based on SOEP data. For cognitive skills and personality traits the number of observations varies from the number given at the bottom of the table.

[a]ISCO scale-based measured of the skill level demanded by the respondents job, scale ranges from 1 (low skills needed) to 4 (high skills needed), see International Labour Organization (2012).

[b]Based on wage information.

Table A2: Robustness for wage as outcome variable

Specification	OLS	IV		
		Instruments referring to		
		Basic	Inter.	Acad.
First stage results				
Log net hourly wage	– –	0.908***	0.432***	0.836***
		(0.189)	(0.138)	(0.213)
Only school years	– –	0.373***	0.231***	0.490***
		(0.115)	(0.088)	(0.131)
Socio-economic controls	– –	0.902***	0.415***	0.597***
		(0.101)	(0.071)	(0.119)
Institutional controls	– –	0.864***	0.387**	1.039***
		(0.207)	(0.179)	(0.277)
Female specification	– –	0.924***	0.459***	0.858***
		(0.191)	(0.132)	(0.207)
Second stage results				
Log net hourly wage	0.067***	0.008	0.024	0.000
	(0.002)	(0.028)	(0.040)	(0.038)
Only school years	0.102***	−0.031	−0.022	−0.012
	(0.004)	(0.068)	(0.085)	(0.063)
Socio-economic controls	0.029***	0.017	−0.018	−0.043
	(0.004)	(0.026)	(0.039)	(0.053)
Institutional controls	0.069***	0.015	0.035	0.033
	(0.002)	(0.030)	(0.054)	(0.035)
Female specification	0.068***	0.000	0.004	0.005
	(0.002)	(0.027)	(0.041)	(0.035)
Reduced form	– –	0.000	−0.003	0.001
		(0.025)	(0.019)	(0.030)

Source: Own calculations based on SOEP data. Control variables: female, as well as state and birth cohort fixed effects. State of schooling × year aged 10-clustered standard errors in parentheses. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Explanations: Log net hourly wage: dependent variable is the net instead of the gross hourly wage in logs. Observations: 5,499. Only school years: endogenous explanatory variable is limited to primary and secondary education. Observations: 5,268. Socio-economic controls: additional control variables: dummy variables for mother's/father's education (at least intermediate school degree), number of siblings, dummy variables for at least good self-assessed health status, obesity (Body Mass Index > 30), migrational background, university degree, completed apprenticeship, and an ISCO scale-based measure of the skill level demanded by the respondent's job. Observations: 4,666. Institutional controls: additional control variables (starting with the baseline model) for the average size of the schools per track is included. Observations: 5,499. Female interaction terms: additional interaction terms between female and the state and birth cohort fixed effects are included. Observations: 5,499. Reduced form: instrument directly plugged into the the wage equation instead of instrumented years of education. Observations: 5,499.

Table A3: Robustness for crystallized intelligence as outcome variable

Specification	OLS	IV		
		Instruments referring to		
		Basic	Inter.	Acad.
First stage results				
Only school years	– –	0.593***	0.316***	0.702***
		(0.177)	(0.108)	(0.191)
Socio-economic controls	– –	1.125***	0.624***	0.820***
		(0.172)	(0.122)	(0.209)
Institutional controls	– –	1.115***	0.477**	1.161***
		(0.268)	(0.225)	(0.354)
Female specification	– –	0.999***	0.294**	0.769**
		(0.248)	(0.169)	(0.294)
Second stage results				
Only school years	0.069***	0.072	0.013	−0.027
	(0.009)	(0.093)	(0.121)	(0.093)
Socio-economic controls	0.026**	0.021	−0.044	−0.112
	(0.014)	(0.071)	(0.086)	(0.113)
Institutional controls	0.048***	0.000	−0.024	−0.003
	(0.005)	(0.056)	(0.102)	(0.066)
Female specification	0.048***	0.016	0.003	−0.041
	(0.005)	(0.057)	(0.133)	(0.087)
Reduced form	– –	0.020	0.008	−0.025
		(0.062)	(0.039)	(0.065)

Source: Own calculations based on SOEP data. Control variables: female, as well as state and birth cohort fixed effects. State of schooling $\times$ year aged 10-clustered standard errors in parentheses. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Explanations: Sample limited to employed persons: only respondents who are also included in the wage regressions. Observations: 1,412. Only school years: endogenous explanatory variable is limited to primary and secondary education. Observations: 2,406. Socio-economic controls: additional control variables: dummy variables for mother's/father's education (at least intermediate school degree), number of siblings, dummy variables for at least good self-assessed health status, obesity (Body Mass Index > 30), migrational background, university degree, completed apprenticeship, and an ISCO scale-based measure of the skill level demanded by the respondent's job. Observations: 1,499. Institutional controls: additional control variables (starting with the baseline model) for the average size of the schools per track is included. Observations: 2,518. Female interaction terms: additional interaction terms between female and the state and birth cohort fixed effects are included. Observations: 2,518. Reduced form: instrument directly plugged into the the wage equation instead of instrumented years of education. Observations: 2,518.