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

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

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Abstract

We analyze the effect of education on wages using German Socio-Economic Panel (SOEP) 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. While evidence in the literature suggests a positive effect in most countries, this finding does not hold for some Continental European countries. Our results are in line with previous studies that do not find an effect of compulsory schooling on wages in Germany. One potential reason might be that basic skills are learned earlier in Germany and additional years of schooling are not effective anymore. We test this by also estimating the effect of education on cognitive and non-cognitive skills. The results suggest that education after the eighth year does not seem to have a causal effect on cognitive and non-cognitive skills in Germany. This is consistent with the explanation for zero effects of schooling on earnings.

Keywords: Returns to education, Skills, IV estimation

JEL Classification: *I21, J24, C26*

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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 homogeneous. For the UK, the US, and Canada (see for instance [Angrist and Krueger, 1991](#); [Oreopoulos, 2006](#)), causal returns to schooling are established 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 ([Björklund and Moffitt, 1987](#)) 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 prominent studies by [Pischke \(2007\)](#) and [Pischke and von Wachter \(2008\)](#) find zero returns to (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 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 re-

turns 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 and non-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 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 and non-cognitive skills for all groups. The remainder of the paper is as follows. Section [2](#) gives a brief introduction into the German educational system, Section [3](#) explains the data and variables. Section [4](#) discusses the instruments while Section [5](#) presents the results. Section [6](#) 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. For reasons described in Section [3](#), the scope of the analysis are West-German non-city states (see Table [1](#) for the states). For the years under review (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 possible three tracks. Which track a student is assigned to, depends basically on the performance in elementary school.

The tracks are distinguished by the years of schooling, the academic content of the curriculum, and the leaving certificate. Basic track schools (*Hauptschulen*) cover grades 5 to 8 before the compulsory reform in Germany and include 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.

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-

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: [Pischke and von Wachter \(2005, 2008\)](#) and own calculations.

time employee afterwards. On academic schools (*Gymnasien*) students receive a degree qualifying for university entrance (*Abitur*) after grade 13. Afterwards, most students decide to have university studies (in our sample 77%, see Table A1 in the Appendix). For further information see [KMK \(2010\)](#). Beside basic and academic secondary schools, a third track became popular after WWII. 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 (88% do so, see Table A1).¹

3 Data and Variables

Starting in 1984, the 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 first test, respondents have to name as many animals as possible in 90 sec. 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](#)). The second test examines the matching of digits and symbols. Respondents are supposed to match a number (e.g. 3) to a certain symbol (e.g. plus sign) following a given matching key. Again the test score is the number of successful matches in 90 sec. The

¹Additional 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 assume comprehensive schools to be similar to intermediate schools.

ability measured using this test is perceptual speed or “fluid intelligence”. 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 SOEP test scores are comparable to those of larger intelligence tests used in psychology. In order to simplify the interpretation of the test scores, we normalize them to mean 0 and standard deviation 1. For a documentation see [Schupp et al. \(2008\)](#), for general information on the SOEP data see [Wagner et al. \(2007\)](#).

The wave of 2005 includes the “Big Five” personality traits as a measure of non-cognitive skills. The Big Five consist of neuroticism (tendency to be worried, anxious), extraversion (outgoing, sociable), openness (intellectual curiosity), agreeableness (altruism, trust), and conscientiousness (strong-willed, well-organized). Each item is measured with three questions. The respondents were asked to self-assess whether they agree with a statement on a scale from 1 to 7. The trait’s score is the average value of the three questions. For further information on the variables of personality traits in SOEP data, see [Budria and Ferrer-i Carbonell \(2012\)](#). We merge these to the 2006 information on education and wages.²

As a measure of earnings we use the log of hourly gross wage in 2006. It is calculated by dividing the monthly 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.³

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 high share of all students in the small states. Furthermore, we restrict our sample to respondents born in 1940 or later because of the school supply information is only available after 1950. This amounts to a further loss of 94 observations. Additionally, 2,470 observations born after 1970 were dropped. After dropping individuals with missing values in covariates the final sample has information on nearly 5,300 people. For the ultra-short intelligence tests, a computer assisted personal interview (CAPI) was needed and only one third of all SOEP respondents

²Similar results appear if we use the 2005 wave for the entire non-cognitive skills analysis.

³If the information on the state of last school attendance 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 [Becker and Siebern-Thomas \(2007\)](#).

were asked to participate in the tests. Therefore, the cognitive test sample includes only 2,401 observations. The non-cognitive skills sample includes 7,123 observations. This number higher more since each interviewed was asked the non-cognitive skill questions and we do not need to restrict our sample to individuals in the labor force.

Control variables in the regressions are age, age squared, gender, migrational background, parental education, the number of siblings, and birth cohort as well as state fixed effects.

4 Identification

To solve the endogeneity problem of education in the wage and abilities regressions, the common strategy is to use IV estimation. Since the results depend on the instrument and the external validity of one LATE may be very limited (see e.g. [Heckman, 2010](#)), we use three instruments which capture the effect for all 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.

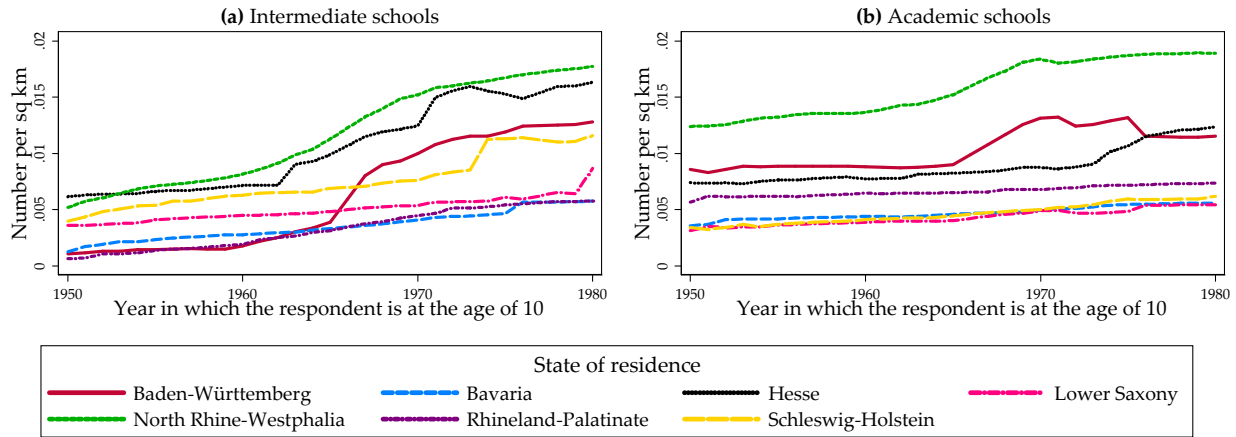
The first instrument is the increase in compulsory years of education. According to Table 1, basic track students in our sample are either exposed to 8 or 9 years of mandatory schooling. The obligatory character of the reform and the large share of compliers (about 76%, see Table 1) guarantee a high relevance regarding the actual years of education. It is plausible to assume the independence of the reform towards wages and cognitive as well as non-cognitive skills. [Petzold \(1981\)](#) argues 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.⁴

While the compliers of the compulsory reform are only basic track students, we use the supply of schools in the two other tracks to instrument education for students of these schools. Supply is measured as the number of schools per sq 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 track 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. This is the case for two reasons: first, the competition about

⁴A 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.

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



Source: Own calculations, data taken from the German Statistical Yearbook ([Statistisches Bundesamt, 1992](#)).

the available places, given a 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. For further explanations regarding the importance of the school supply, see [Card \(1995\)](#) and [Becker and Siebern-Thomas \(2007\)](#).

The supply of schools is taken from several issues of the German Statistical Yearbook ([Statistisches Bundesamt, 1992](#)). Figure 1 shows the number of intermediate (part (a)) the academic schools (part (b)) per sq km per state over time. We clearly see that there is a lot of variation among and across states in school supply. With the education 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.⁵

⁵As [Jürges et al. \(2011\)](#) we do not include state-specific trends as this would discard this variation and would make the instruments useless.

5 Results

5.1 The Effect of Education on Wages

Table 2 reports the coefficients of the regressions of log hourly gross wages and the skill variables on years of education and control variables. Each of the 31 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 the endogeneity problems. Column 2 shows results from 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 instruments.

The first-stage results are presented in the first lines of Table 2 for the wage sample. Unsurprisingly, they do not differ significantly in the other regressions as the first stage is the same for each outcome variable. Students affected by the reform attend school on average 0.96 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) were affected. Regarding the school supply as instruments, we scale the coefficients to a usual increase in the supply by 0.005 schools per sq km (see Figure 1) in order to simplify the interpretation. If one lives, e.g., in state of Hesse instead of Rhineland-Palatinate, the time spent in school is 0.5 years (intermediate schools instrument) respectively 0.87 years (academic schools) longer. The estimated increase is below the actual one for compliers, but here less people are affected by the change in the instrument. All instruments are highly significant. Accordingly, the instrument *F*-statistics at the bottom of Table 2 are all well 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% higher wages. This is the same magnitude of the OLS coefficient as 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 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. The Appendix provides results of several robustness checks where, (1), we use years of schooling instead of ed-

Table 2: Estimation results (full sample)

Dependent variable	OLS	IV		
		Basic	Inter.	Acad.
<u>First stage results</u>				
Years of education (wage regression)	--	0.961*** (0.152)	0.496*** (0.107)	0.874*** (0.200)
<u>Second stage results</u>				
<i>Measure of earnings:</i>				
Log hourly gross wage	0.058*** (0.002)	−0.002 (0.021)	0.005 (0.031)	−0.009 (0.032)
<i>Measure of cognitive skills:</i>				
Crystallized intelligence test score	0.079*** (0.008)	0.068 (0.063)	0.160 (0.183)	−0.011 (0.122)
<i>Measures of non-cognitive skills:</i>				
Neuroticism	−0.046*** (0.004)	−0.130*** (0.049)	−0.052 (0.069)	0.109 (0.095)
Extraversion	0.009* (0.005)	0.026 (0.046)	0.142* (0.080)	0.109 (0.089)
Openness	0.059*** (0.005)	0.040 (0.044)	0.134* (0.076)	−0.001 (0.090)
Agreeableness	0.014*** (0.005)	−0.105 (0.061)	−0.041* (0.078)	−0.083 (0.094)
Conscientiousness	−0.014*** (0.005)	0.014 (0.052)	0.024 (0.078)	−0.013 (0.080)
First-stage <i>F</i> -statistic (wage regression)	--	39.94	21.57	19.17

Source: Own calculations based on SOEP data. Numbers of observations: Wage regressions: 5,283; Cognitive skills regressions: 2,401; Non-cognitive skills regressions: 7,123. Control variables: age, age squared, female, migrational background, dummy indicating that both parents have had at least intermediate schooling, number of siblings, as well as state and birth cohort fixed effects. State of schooling \times year aged 10-clustered standard errors in parentheses. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

ucation, (2), add more control variables that were left out in the preferred specification due to potential “bad control” problems,⁶ (3), also control for the average number of students per school by track, and, (4), add interaction terms for gender and the cohort and state fixed effects. In no specification do we find a significant effect of education

⁶The added controls are dummy variables for university degree, vocational training, at least good self-assessed health status, and obesity (Body Mass Index > 30). Furthermore, a ISCO-based measure for the skill level demanded by the respondent’s job was added.

on wages for either instrument. All in all, the robustness checks underline the baseline finding of zero returns.

5.2 Explanations: Effects of Education on Cognitive and Non-cognitive Skills

[Pischke and von Wachter \(2008\)](#) conjecture that the potential reason for zero returns to (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 and non-cognitive abilities as presented above are used to test this hypothesis. The same identification problems as with wages appear to be relevant in this case (see [Heckman and Vytlačil, 2001](#)), hence, we again prefer IV results with the same instruments over benchmark OLS results in Table 2.

For cognitive abilities the same picture as with wages emerges. Individuals with more years of schooling have higher cognitive abilities – about 7% of a standard deviation more with one more year of schooling in the OLS regressions. However, once controlling for endogeneity, the coefficients approach zero and become insignificant. At first glance, this result may seem puzzling because previous studies find a causal effect of education on cognitive abilities. [Banks and Mazzonna \(2012\)](#) as well as [Schneeweis et al. \(2012\)](#) use changes in compulsory schooling and find positive and significant effects for the UK and Continental Europe, respectively. However, the analysis in this paper is different from their procedure in two ways. First, [Banks and Mazzonna \(2012\)](#) and [Schneeweis et al. \(2012\)](#) use data sets (ELSA and SHARE, respectively) which focus on a different population, namely people aged 50 or older. Consequently, respondents in these analyses are older than those here (compare Table A1). Another difference is that we measure cognitive skills using perceptual speed test scores. The previous studies find significant effects only for the ability to recall a list of words. Because recall is not included in the SOEP data, we are not able to take it into account. Finally, and back to the original explanation of [Pischke and von Wachter \(2008\)](#), [Banks and Mazzonna \(2012\)](#) use data from UK with a different institutional setting than Germany.

Another way through which education could affect wages is the development of non-cognitive skills. Even if not explicitly addressed by [Pischke and von Wachter \(2008\)](#), [Heckman et al. \(2006, p. 477\)](#) conclude that “non-cognitive ability is as important, if not more important, than cognitive ability”. [Anger and Heineck \(2010\)](#) show the importance of personality traits using SOEP data. Lines 3 to 7 of Table 2 present the effects of education on self-assessed traits. Using the non-cognitive skill measures as dependent

variable, education only affects neuroticism significantly for basic school students. As reported in the Appendix, the coefficients give the effect of an additional year of education on the average agreement with different issues regarding their behavior and thoughts on a 1 to 7 scale. In both cases—OLS and IV estimation—none of the coefficients exceeds an effect of 0.5 points. Indeed, most coefficients have an absolute value lower than 0.1. Thus, the causal effect of education on non-cognitive skills seems to be close to zero, too. Due to the lower variance of instrumented education—and, accordingly, the higher standard error of the point estimators—the IV coefficients are not significant.

In this section, we show that there seems to be no effect of education on wages in Germany. Since we also find no effect of education on cognitive and non-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.

6 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. We estimate three LATEs covering all groups of students in Germany. 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 are able to show that the results are not only driven by the “local” nature of the IV approach.

In a second step we test one hypothesis for zero returns to schooling, namely that basic skill formation – relevant for the labor market at least – takes places before the eighth grade in Germany. This is done by estimating the causal effect of education on cognitive and non-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.

Other studies, especially for the US and the UK, estimate returns about 10% to 15% per additional year of education. The zero returns found here, allow three conclusions. At least in Germany, a part of the fact that more education goes along with higher wages can be explained by signaling function of education ([Arrow, 1973](#); [Spence, 1973](#)). The organization of education plays an important role for educational outcomes. This is because different schools (e.g. academic vs. vocational schools) teach distinct contents

with dissimilar relevance for the labor market. Those institutional differences and the fact that IV estimation (probably using institutional reforms) only gives a LATE, underline that extrapolating results from one setting to another is a hard task.

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Appendix

Table A1: Means of selected variables by track

	Basic	Inter.	Acad.	Total
<i>Income</i>				
Gross hourly wage (in €)	13.96	16.42	20.49	16.41
Gross monthly wage (in €)	2,277	2,746	3,627	2,769
<i>Education</i>				
Years of education	10.25	12.13	16.65	12.46
University degree (in %)	4.73	14.55	76.27	26.52
Apprenticeship (in %)	79.04	89.03	35.79	72.12
<i>Cognitive skills</i>				
Crystallized intelligence test score	23.46	28.18	31.30	26.91
<i>Personality traits^a</i>				
Neuroticism	3.91	3.81	3.69	3.82
Extraversion	4.77	4.93	4.86	4.85
Openness	4.36	4.59	4.76	4.54
Agreeableness	5.43	5.41	5.41	5.42
Conscientiousness	6.16	6.06	5.87	6.06
<i>Socio-demographic characteristics</i>				
Female (in%)	42.68	50.62	45.26	46.11
Age (in years)	48.48	46.43	47.37	47.49
Mother has intermediate school degree (in %)	6.02	16.02	41.26	18.19
Father has intermediate school degree (in %)	7.41	22.50	50.94	23.42
Number of siblings	2.32	1.68	1.48	1.89
Self-assessed health stats at least good (in %)	45.86	56.00	59.48	52.73
Obesity: Body Mass Index > 30 (in %)	22.32	14.45	11.59	16.94
Migratinal background (in %)	28.59	7.65	7.67	16.13
Skills need for job ^b	2.19	2.71	3.48	2.71
Observations ^c	2,137	1,869	1,277	5,283
Share (in %)	40.45	34.38	24.17	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.

^aMeasured on a scale from 1 (do not agree) to 7 (fully agree).

^bMeasured on a scale from 1 (low skills needed) to 4 (high skills needed).

^cBased on wage information.

Table A2: Robustness checks regarding the wage equation

Specification	OLS	IV		
		Basic	Inter.	Acad.
<u>First stage results</u>				
Only school years	--	0.419*** (0.101)	0.273*** (0.069)	0.521*** (0.126)
Log net hourly wage	--	0.961*** (0.152)	0.496*** (0.107)	0.874*** (0.200)
Socio-economic controls	--	0.863*** (0.089)	0.430*** (0.067)	0.663*** (0.114)
Institutional controls	--	0.850*** (0.162)	0.355*** (0.134)	0.864*** (0.266)
Female interaction terms	--	0.965*** (0.152)	0.506*** (0.104)	0.883*** (0.194)
Limited to the track	--	0.867*** (0.122)	0.296*** (0.113)	0.361 (0.254)
<u>Second stage results</u>				
Only school years	0.087*** (0.003)	-0.021 (0.049)	-0.004 (0.058)	-0.022 (0.055)
Log net hourly wage	0.056*** (0.002)	0.018 (0.022)	0.017 (0.029)	-0.004 (0.030)
Socio-economic controls	0.026*** (0.004)	-0.003 (0.022)	-0.020 (0.033)	-0.046 (0.038)
Institutional controls	0.058*** (0.002)	0.005 (0.025)	0.033 (0.049)	0.018 (0.038)
Female interaction terms	0.058*** (0.002)	-0.008 (0.021)	0.000 (0.030)	-0.018 (0.033)
Limited to the track	--	-0.047 (0.040)	-0.116 (0.099)	0.035 (0.085)
Reduced form	--	-0.009 (0.020)	-0.002 (0.016)	-0.019 (0.027)

Source: Own calculations based on SOEP data. Control variables: age, age squared, female, migrational background, dummy indicating that both parents have had at least intermediate schooling, number of siblings, 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: Only school years: endogenous explanatory variable is limited to primary and secondary education. Observations: 5,058. Log net hourly wage: dependent variable is the net instead of the gross hourly wage in logs. Observations: 5,283. Socio-economic controls: additional control variables: dummies for university degree, vocational training, good or excellent self-assessed health status, and obesity (Body Mass Index > 30). Moreover, an occupation based measure of skills demanded by the job is included. Observations: 4,641. Institutional controls: additional control variables for the average size of the schools per track is included. Observations: 5,283. Female interaction terms: additional interaction terms between female and the state and birth cohort fixed effects are included. Observations: 5,283. Limited to the track: regressions are limited to respondents visiting the track the instrument refers to. Observations: basic track: 2,137; intermediate track: 1,839; academic track: 1,277. Reduced form: instrument directly plugged into the the wage equation instead of instrumented years of education. Observations: 5,283.

Table A3: First stages of skill measures by sample

Kind of skills	Instruments referring to		
	Basic	Inter.	Acad.
<i>All information available:</i>			
Cognitive skills	1.142*** (0.225)	0.281* (0.154)	0.682** (0.265)
Non-cognitive skills	1.133*** (0.133)	0.448*** (0.085)	0.661*** (0.165)
<i>Only information on employed persons:</i>			
Cognitive skills	1.139*** (0.312)	0.397* (0.215)	0.615* (0.354)
Non-cognitive skills	0.961*** (0.152)	0.496*** (0.107)	0.874*** (0.200)

Source: Own calculations based on SOEP data. Numbers of observations: all information available: 2,401 and 7,123 (cognitive skills and non-cognitive skills, respectively); only information on employed persons: 1,398 and 4,543. Control variables: age, age squared, female, migrational background, dummy indicating that both parents have had at least intermediate schooling, number of siblings, as well as state and birth cohort fixed effects. State of schooling \times year aged 10-clustered standard errors in parentheses. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Second stages for employed persons only

Dependent variable	OLS	IV		
		Instruments referring to		
		Basic	Inter.	Acad.
<i>Measure of cognitive skills:</i>				
Crystallized intelligence test score	0.074*** (0.011)	0.037 (0.103)	−0.061 (0.215)	−0.196 (0.276)
<i>Measures of non-cognitive skills:</i>				
Neuroticism	−.0027*** (0.005)	−0.166*** (0.060)	−0.043 (0.073)	0.081 (0.094)
Extraversion	−0.001 (0.006)	0.084 (0.062)	0.170* (0.089)	0.134 (0.088)
Openness	0.049*** (0.006)	0.068 (0.066)	0.084 (0.072)	−0.019 (0.078)
Agreeableness	0.005 (0.006)	−0.123* (0.071)	0.025 (0.092)	−0.002 (0.106)
Conscientiousness	−0.034*** (0.005)	0.093 (0.067)	0.109 (0.082)	0.070 (0.083)

Source: Own calculations based on SOEP data. Numbers of observations: Cognitive skills regressions: 1,398; Non-cognitive skills regressions: 4,543. Control variables: age, age squared, female, migrational background, dummy indicating that both parents have had at least intermediate schooling, number of siblings, as well as state and birth cohort fixed effects. State of schooling \times year aged 10-clustered standard errors in parentheses. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.