

Life-cycle Health Effects of Compulsory Schooling *

Johannes Hollenbach

RWI &
Paderborn University

Hendrik Schmitz

RWI &
Paderborn University

Beatrice Baaba Tawiah

MPI-SHARE

February 2025

Abstract

We study the effect of education on health (hospital stays, number of diagnosed conditions, poor or bad self-rated health, and overweight) over the life cycle, using German compulsory schooling reforms as a source of exogenous variation. Our results show clear correlations between educational attainment and better health across all age groups (30 to 74). With the exception of a small beneficial effect on overweight, we do not find causal relationships between additional schooling and health or health care utilization, neither earlier nor later in life. One reason for this may be that the studied compulsory schooling reforms succeeded in raising the educational attainment of the target group — individuals at the lowest educational margin — but did not lead to healthier employment opportunities.

Keywords: Education, Health, Life-cycle effects, Compulsory schooling

JEL Classification: *I10, I12, I21*

Johannes Hollenbach: RWI - Leibniz Institute for Economic Research, Hohenzollernstr. 1-3, 45128 Essen, Germany (johannes.hollenbach@rwi-essen.de); Hendrik Schmitz: Paderborn University, Warburger Str. 100, 33098 Paderborn, Germany (hendrik.schmitz@uni-paderborn.de); Beatrice Baaba Tawiah: Munich Research Institute for the Economics of Aging and SHARE analyses, Leopoldstraße 139, 80804 München, Germany (b.tawiah@mea-share.eu).

*We are grateful to Daniel Kamhöfer, Valentin Schiele and Matthias Westphal for very helpful comments.

1 Introduction

Estimating the effects of education on socioeconomic outcomes has been an important part of applied microeconometrics over the past three decades. While most of the literature has focused on labor market outcomes, health effects have also been investigated. In Table 1 we list 24 studies that estimate health effects of education and use instrumental variable estimation methods for identification. More than half of these studies find no statistically significant effects overall or in relevant subgroups. All of these studies have in common that they aggregate effects across age groups, often over several decades. However, this may miss relevant patterns. Kaestner et al. [1] extend the classic Grossman [2] model of demand for health and conclude that “it is unlikely that the relationship between education and health is constant over the life cycle and that education is likely to have little effect on health at younger ages when there is little depreciation of the stock of health” [1]. Thus, an estimated small and insignificant effect averaged over younger and older individuals does not necessarily imply that health is not causally affected by education. It may well be that the effect occurs late in life and that the aggregate average is obscured by a zero effect for younger individuals.

It is well known that the socioeconomic status-health gradient increases over the life cycle [3, 4]. This descriptive pattern has also been shown more specifically for the education-health gradient. For example, Kaestner et al. [1] find no differences in mortality by education until the age of 60, after which hazard rates diverge by education. In contrast, they find an education-morbidity gradient only for the 45-60 age group, but explain this with possible selective mortality. Bijwaard et al. [5] find an increasing difference in mortality between those with primary education and those with more than primary education, mostly after the age of 60. They find that the differences are mainly due to selection effects (based on cognitive ability) at early ages, while the role of education increases after age 60. Leopold and Leopold [6] find differences in self-rated health between higher and lower educated individuals from ages 30 to 80, which increase from age 50 (for men). Ross and Mirowsky [7] find a gap in physical impairment between the well-educated and the poorly educated over the life cycle, which is more pronounced for women. These studies provide a descriptive picture of the education-health gradient over the life cycle, but do not claim causality.

We contribute to the literature on the health effects of education by examining whether these effects vary over the life cycle, going beyond descriptive analyses. In our study, the exogenous variation is derived from compulsory schooling reforms in West Germany. These reforms were introduced for birth cohorts between 1931 and 1954 and vary across federal states. Our basic data set is the German Socio-Economic Panel (SOEP), an ongoing representative survey that has been conducted since 1984. We pool these data with German participants of the European Survey of Health, Ageing, and Retirement (SHARE), the

German National Educational Panel Study (NEPS), and the German Microcensus. Our compiled dataset covers a period of 38 years and allows us to follow individuals born around the pivotal reform cohorts over many decades and to estimate both short- and long-term effects of education on health within the same framework and dataset. This enables us to identify when potential health returns to education begin to materialize or grow. Estimating life-cycle health effects using outcomes below the level of mortality would not be possible with any available administrative data set in Germany.

Table 1: Effect of education on health – previous economic literature

Authors	Year published	Country	Type	Instrument	Age group	Results
Adams [8]	2002	USA	Secondary	QOB	51 to 61	Positive effects
Arendt [9]	2005	Denmark	Middle school	CSR	25 to 64	No effects
Lleras-Muney [10]	2005	USA	Secondary	CSR	35 to 73	Reduction in mortality
Oreopoulos [11]	2006	UK	Secondary	CSR	32 to 64	Positive effects
Albouy and Lequien [12]	2009	France	Secondary	CSR	48 to 80	No effects
Silles [13]	2009	UK	Secondary	CSR	25 to 60	Positive effects
Braakmann [14]	2011	UK	Secondary	February birth	28 to 45	No effects
Kemptner et al. [15]	2011	Germany	Secondary	CSR	16 to 65	Women: no effects Men: positive effects
van Kippersluis et al. [16]	2011	Netherlands	Secondary	CSR	80 to 88	Reduction in mortality
Lager and Torssander [17]	2012	Sweden	Various	CSR	15 to 64	No effects
Clark and Royer [18]	2013	UK	Secondary	CSR	12 to 74	No effects
Jürges et al. [19]	2013	UK	Secondary	CSR	32 to 53, 44 to 77	No effects
Gathmann et al. [20]	2015	Europe	Secondary	CSR	50+	Women: no effects Men: positive effects
Palme and Simeonova [21]	2015	Sweden	Secondary	CSR	28 to 66	Negative effects
Brunello et al. [22]	2016	Europe	Secondary	CSR	50+	Positive effects
Buckles et al. [23]	2016	USA	College	War draft	28 to 65	Positive effects
Davies et al. [24]	2018	UK	Secondary	CSR	37 to 74	Positive effects
Meghir et al. [25]	2018	Sweden	Secondary	CSR	16 to 75	No effects
Kamhöfer et al. [26]	2019	Germany	College	College expansion	39 to 68	Mental health: no effects Physical health positive e.
Dahmann and Schnitzlein [27]	2019	Germany	Secondary	CSR	50 to 85	No effects
Janke et al. [28]	2020	UK	Secondary	CSR	42 to 60	No effects (except diabetes)
Fischer et al. [29]	2021	Sweden	Secondary	CSR	18 to 81	Positive effects
Begerow and Jürges [30]	2022	Germany	Secondary	CSR	50 to 79	No effects
Malamud et al. [31]	2023	Romania	Secondary	CSR	42 to 71	No effects

Notes: Own research of studies without the claim of completeness. CSR = compulsory schooling reform, QOB = quarter of birth. The age ranges are not always clearly specified in the papers and sometimes deducted by ourselves using information provided on used birth cohorts as well as calendar years when the outcomes are measured. “No effects” usually means no significant effects and abstracts from economic effect sizes which might be non-zero. Brunello et al. [22] use various European countries.

The only two studies we are aware of that also explicitly look at health effects of education over the life cycle are Clark and Royer [18] and Gehrsitz and Williams Jr [32].¹ Clark and Royer [18] find that two changes in British compulsory schooling laws did not affect mortality overall, but also not when focusing on 5-year age groups between 20-24 and 65-69. Gehrsitz and Williams Jr [32] study the effects of a reform in Scotland and report results by age for 30-55 year olds. They find no effect on self-reported health, but a reduction in hospital admissions for selected conditions. This is mainly true for men and starts

¹Bhuller et al. [33] and Delaney and Devereux [34] look at life-cycle effects of education on earnings.

after the age of 40. In contrast to Clark and Royer [18], we investigate life-cycle effects on morbidity and health care utilization and also go beyond age 69 (and age 55 as in Gehrsitz and Williams Jr [32]).

Our results show a clear positive correlation between health and education at all ages. An additional year of education is associated with a lower likelihood of having been hospitalized in the past year, a lower number of diagnosed illnesses, a lower likelihood of rating one's health as poor or fair, and being overweight. The associations between education and number of illnesses and poor self-rated health appear to peak between the ages of 50 and 65. The education-health gap remains relatively constant over time for overweight. However, when we examine the causal relationship, we find that there are hardly any effects of education on health and the utilization of health care. One exception is overweight, where we find small beneficial effects of an additional year of education for the younger age groups in our sample (45-60). This effect may increase with age, as we observe a larger coefficient for the oldest age group, but it is not statistically significant.

One may argue that it comes at no surprise that the effects are modest at best. Previous literature has shown zero to very small effects of the German compulsory schooling reform on outcomes such as health, cognition, or income. In their review, Galama et al. [35] conclude that compulsory schooling reforms seem to affect health only if either there are effects on student's peer groups, or if there are monetary returns to the reform. Both are not the case for the German schooling reform even though the debate whether there have been monetary returns or not is still ongoing [36]. Moreover, even though for a different measure of education, namely university education, Kamhöfer et al. [26] show how individuals with the lowest desire to take education do not seem benefit from education in terms of income or cognition. The effects we identify here are also for the individuals at the lowest margin of willingness to take education. Yet, we argue that this previous empirical evidence only addresses aggregate age effects and, ultimately, it is still an empirical question whether zero or moderate aggregate effects may mask larger health effects at higher age groups.

A limitation of our study is possible low statistical power due to smaller sample sizes and many parameters that are estimated. In the working paper version we perform an exemplary ex-post power analysis using the method of [37] and find that minimum detectable effect sizes are reasonably small [38]. Nevertheless, small sample size in the different age groups stays a limitation.

This paper is structured as follows. In Section 2 we present the institutional framework, data, and descriptive statistics. In Section 3 we show and discuss the main results: instrumental variables estimations for different age groups. We also provide robustness checks and inspect panel attrition. In Section 4 we study a possible reason for the zero effects. We conclude in Section 5.

2 Institutional framework and Data

2.1 Institutional framework and sample selection

In Germany, children enter primary school at the age of six. After four years in primary school they attend one of the three secondary school tracks. Secondary schools in Germany can, generally, be differentiated into basic (*Hauptschule*), intermediate (*Realschule*) and high schools (*Gymnasium*). The basic track (up to 8th or 9th grade) prepares students for apprenticeship, the intermediate track (up to 10th grade) qualifies students for apprenticeship or training in white collar jobs, and the high school certificate (up to 12th or 13th) gives access to academic education in colleges or universities. Before the German educational reform, which occurred from 1946 to 1969 in West Germany, basic track schools covered grades five to eight. The reform increased the number of compulsory schooling years from eight years to nine years. Decisions and policies regarding the educational system in Germany are made at the federal state level, hence the reform was implemented in different years by the various states [39]. Some states introduced a compulsory ninth grade earlier, while the majority of the states only introduced an additional year of schooling due to the Hamburg Accord (*Hamburger Abkommen*) in 1964 [40]. See Table 2 for the reform years. The reform was introduced due to a shortage in labor market opportunities and apprenticeships for school leavers, and to also increase the school leaving age (see Pischke and Von Wachter [41] for details).

Data

We pool data from four sources in order to maximize sample size. The largest one and, thus, our main data source is the German Socio-Economic Panel (SOEP) which is a wide-ranging representative longitudinal study of households in Germany. SOEP, established in 1984, contains yearly information on around 30,000 respondents in nearly 15,000 households. For our analysis we use SOEP version 39 containing yearly information from 1984 to 2022 [42]. We augment our baseline sample with observations from the Survey of Health Ageing, and Retirement (SHARE), the German National Educational Panel Study (NEPS) and the German Microcensus. SHARE is a representative micro dataset which provides health and socio-economic information of people age 50 and older from 28 European countries and Israel. We consider waves 1, 2 and 4-9 [43, 44, 45, 46, 47, 48, 49, 50, 51]. Wave 3 (SHARELIFE) samples different individuals. NEPS is a longitudinal dataset that provides information on the acquisition of education in Germany, and educational processes and trajectories across the entire life span [52, 53]. We consider all 13 waves of the NEPS from 2007 to 2021. The German Microcensus is a comprehensive, annual household survey conducted by the Federal Statistical Office of Germany. It provides detailed and representative data on the social, economic, and demographic structure of the population.

Our final sample includes information from four waves of the Microcensus from 1999, 2003, 2009, and 2017 as these include measures of obesity.

Table 2: Reform years, corresponding first birth cohorts and ages

Federal State	Pivotal birth cohort	Reform year	Youngest age in 1984	Oldest age in 2022
Schleswig Holstein	April 1932	April 1947	47	95
Hamburg	April 1931	April 1946	48	96
Lower Saxony	April 1947	April 1962	32	80
Bremen	April 1944	April 1959	35	83
North Rhine-Westphalia	April 1951	April 1966	28	76
Hesse	April 1951	April 1966	28	76
Rhineland Palatinate	April 1952	April 1967	27	75
Baden-Württemberg	April 1952	April 1967	27	75
Bavaria	August 1954	August 1969	25	72
Saarland	April 1943	April 1958	36	84

Source: Reform years according to Begerow and Jürges [30]. Youngest age in 1984 calculated as follows: 1984 - pivotal cohort - 5. Oldest age in 2020 calculated as follows: 2020 - pivotal cohort + 5.

We restrict the sample to individuals born five years before and after the pivotal cohorts – that is, the first birth cohorts that were affected by the reform. Table 2 reports the reform years and shows how the age range of individuals we can identify effects for differ by federal states. For instance, for the outcome variables available from 1984 to 2020 in the SOEP (later for the other data sets), the youngest possible age is 25 for a person from Bavaria, born in 1959, observed in 1984. The oldest possible age is 96 for a person from Hamburg, born in 1926, observed in 2022. In our analysis below, we will form 5-year age groups to estimate effects. We restrict the sample to individuals between 30 (starting with age group 30-34) and 74 (for age-group 70-74) years to make sure that effects for certain age groups at the higher and lower ends are not completely driven by individuals from single federal states.

The data include information on age, gender, the state in which an individual attended school, years of schooling, and the type of schooling completed. We use educational degree to infer years of schooling as our explanatory variable of interest.²

2.2 Outcome variables and descriptive statistics

The health outcomes we consider are hospital stay in the previous year, number of illnesses diagnosed, poor self-rated health and obesity. More specifically, *Hospital stay* is an indicator variable based on the question whether a person was admitted at a hospital for at least

²The state an individual attended school is only available in the SOEP. We use the current state individuals live in as a proxy in the other data sets. See the Appendix of Begerow and Jürges [30] for an analysis that shows how in only 5% of the cases – in these data sets – this leads to a false assignment of the reform indicator which is used as an instrumental variable.

one night the previous year. The number of illnesses diagnosed (called *diagnoses* from now on) is constructed from a question asking if an individual has ever been diagnosed by a doctor of one or more illnesses from a list of illnesses. The 13 illnesses asked are sleep disturbance, diabetes, asthma, heart disease, cancer, stroke, migraine, high blood pressure, depressive psychosis, dementia, joint disorder (also osteoarthritis, rheumatism), chronic back complaints and other illnesses. We count the number of diagnoses. *Poor or bad health* is based on the 5-point scale of current self-rated health status, that ranges from one (very good) to five (bad). Our outcome is an indicator variable equal one if an individual's self-assessed health falls within the two worst categories. In the appendix, we also present results for choosing the lowest category (*bad health*). The final outcome is *overweight*, an indicator variable equal to one if an individual's body-mass index is at least 25 (based on self-stated body weight and height). In the appendix, we also present results for being *obese*, i.e., having a body-mass index of at least 30.

Table 3 reports numbers of observations in the final sample by outcome variable and age group. Next to the number of observations, we show from which data set the observations come. Clearly, SOEP has the most observations. Yet, as SHARE samples older individuals, it helps to increase numbers of observations particularly for the oldest age groups. Note that diagnoses and hospital visits are not included in the NEPS and Microcensus data. Microcensus is only used for weight-related outcomes and dominates the sample size there.

Table 3: Number of observations

Age group	Hospitalization last year		Poor or bad health		Diagnoses		Overweight	
	Obs.	(% SOEP / %SHARE / % NEPS/ %MZ)	Obs.	(% SOEP / %SHARE / % NEPS/ %MZ)	Obs.	(% SOEP / %SHARE / % NEPS/ %MZ)	Obs.	(% SOEP / %SHARE / % NEPS/ %MZ)
30-34	4316	(100 / 0 / 0 / 0)						
35-39	6557	(100 / 0 / 0 / 0)	1503	(100 / 0 / 0 / 0)				
40-44	7203	(100 / 0 / 0 / 0)	5140	(100 / 0 / 0 / 0)				
45-49	10357	(100 / 0 / 0 / 0)	10062	(100 / 0 / 0 / 0)			16005	(14 / 0 / 0 / 86)
50-54	13656	(95 / 5 / 0 / 0)	13611	(94 / 5 / 1 / 0)	1299	(47 / 53 / 0 / 0)	27186	(18 / 3 / 0 / 79)
55-59	14813	(90 / 10 / 0 / 0)	16003	(84 / 9 / 8 / 0)	4311	(67 / 33 / 0 / 0)	29875	(21 / 5 / 1 / 74)
60-64	15014	(86 / 14 / 0 / 0)	19432	(67 / 11 / 22 / 0)	7410	(72 / 28 / 0 / 0)	33022	(19 / 6 / 2 / 73)
65-69	12090	(84 / 16 / 0 / 0)	20395	(50 / 10 / 40 / 0)	6498	(70 / 30 / 0 / 0)	26262	(20 / 7 / 7 / 66)
70-74	5768	(83 / 17 / 0 / 0)	11678	(41 / 8 / 51 / 0)	2912	(66 / 34 / 0 / 0)	10500	(24 / 9 / 14 / 52)

Notes: This table presents the number of observations by age group and the composition of our sample for all four health outcomes.

Table 4 reports descriptive statistics of all outcome variables. Some outcome variables are not available in all waves, hence, the sample size varies for the different outcomes with obesity having the largest sample (142,850 observations). The smallest sample has 23,056 observations. 13% of the observations stayed at least one night in the hospital the previous year. The maximum number of diagnoses in the sample is 12 out of the 13 options mentioned above. There is an average of about 1.7 illnesses being diagnosed and 59% are overweight, while 18% state that they are in poor or bad health. The descriptives

for treatment, instrument and other information are, as an example, taken from the hospitalization sample as it includes all ages from 30 to 74. On average, individuals in this sample have 10 years of schooling. They are slightly more likely to be born in or after school reform years. The average age is 54.4 years and the sample is almost gender-balanced.

Table 4: Descriptive statistics

	Mean	SD	Min.	Max.	Observations	Survey years
<i>Outcome variables</i>						
Hospitalization	0.13	0.33	0	1	89 774	1984-2022
Poor or bad health	0.18	0.39	0	1	97 824	1992-2022
Number of diagnoses	1.71	1.60	0	12	22 430	2004-2022
Overweight	0.59	0.49	0	1	142 850	1999-2022
<i>Treatment and instrument</i>						
Years of schooling	9.97	1.69	8	13	89 774	
Reform	0.54	0.50	0	1	89 774	
<i>Other information</i>						
Age	54.36	10.82	30	74	89 774	
Female	0.51	0.50	0	1	89 774	

Notes: This table presents summary statistics. The outcome variables are defined as follows: Hospitalization is an indicator variable for whether a person was hospitalized for at least one night in the past year. Poor or bad self-rated health is a binary indicator of checking one of the two lowest of five possible categories of self-rated health. Number of diagnoses is a count variable of the total number of diagnosed (chronic) conditions and diseases from a list of 13 possible options. Overweight is a binary indicator of having a BMI ≥ 25 . The statistics for age, female, years of schooling, and reform are based on the estimation sample for hospitalizations.

2.3 OLS estimations

We estimate the association between education and health outcomes using an OLS regressions of the following form:

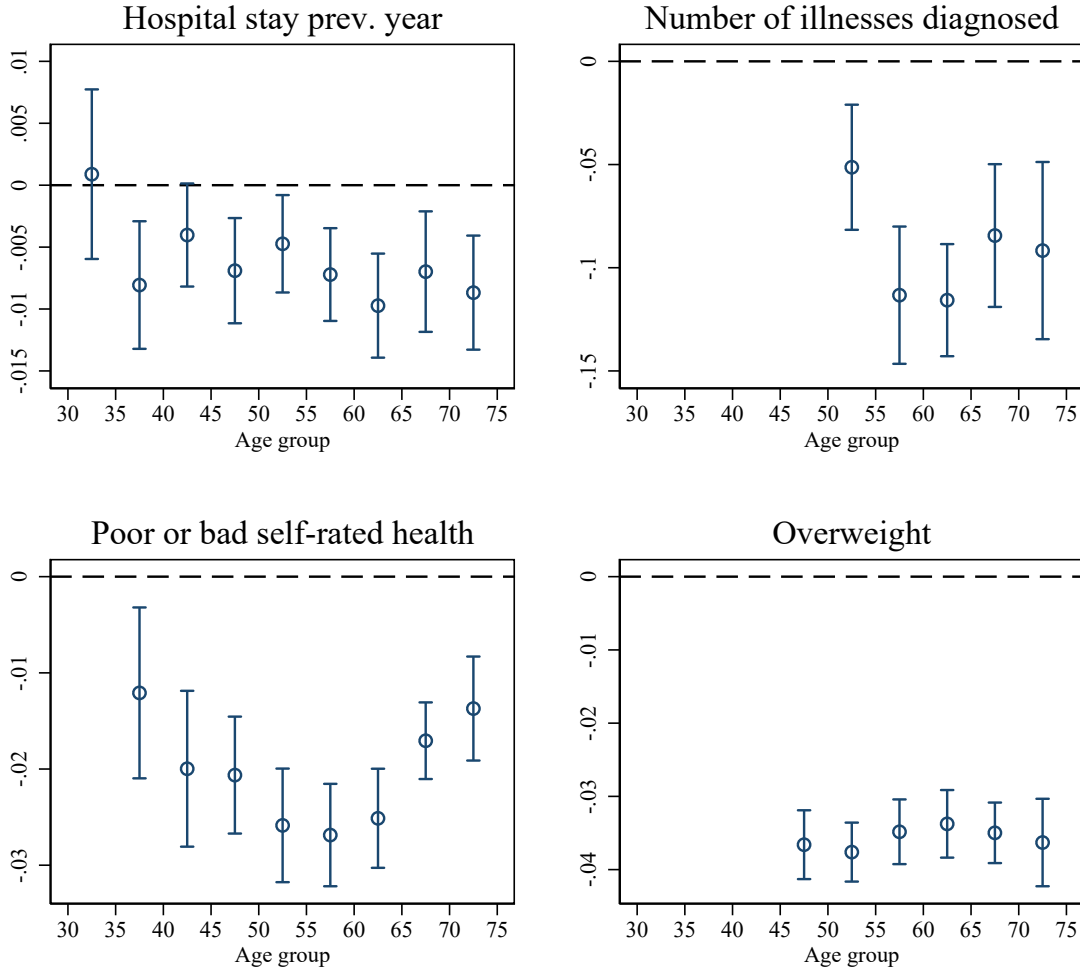
$$H_{ist} = \sum_g \beta_g Yed_{is} \times agegroup_{it} + \beta X_{ist} + \varepsilon_{ist} \quad (1)$$

where H_{ist} is a health outcome of individual i who attended school in state s . Yed_{is} is the number of years of schooling of an individual. To flexibly account for the correlation between schooling and health, we define 5-year age groups, denoted $agegroup_{it}$, as follows 30-34, 35-39, 40-45, ..., 70-74.³ The vector X includes age dummies (in years) to flexibly control for age at the time of the interview, federal state dummies, a female indicator, survey and interview year dummies, and federal state-specific time trends, i.e., interactions of school state dummies with a linear trend in year of birth. We cluster standard errors at the

³The age groups begin at 35-39 for poor health, 45-49 for obesity, and 50-54 for diagnoses because data for younger age groups is unavailable.

state \times year of birth levels in all specifications. The coefficients of interest, β_g , are reported in Figure 1.

Figure 1: OLS results



Notes: This figure shows estimates of the association between years of schooling and four health outcomes across up to nine age groups. The outcome variables are defined as follows: Hospitalization is an indicator variable for whether a person was hospitalized for at least one night in the past year. Poor or bad self-rated health is a binary indicator of checking one of the two lowest of five possible categories of self-rated health. Number of diagnoses is a count variable of the total number of diagnosed (chronic) conditions and diseases from a list of 13 possible options. Overweight is a binary indicator of having a BMI ≥ 25 . Own calculations based on SOEP, SHARE, Microcensus, and NEPS. We visualize point estimates of the coefficients β_g based on Eq. (1) with 95 % confidence intervals. Coefficients plotted between 30 and 35 represent age group 30-34, and so on. Some age groups are missing due to data constraints. Standard errors clustered at state \times year of birth level.

The results show a favorable relationship between education and health over the life cycle. An additional year of schooling is correlated with a lower probability of having been hospitalized in the previous year, a lower number of diagnosed (chronic) diseases, a lower probability of rating one's own health as poor or bad, and a lower probability of being overweight at all ages. Although the coefficients are small in absolute terms, they are sizable when compared to the sample means in Table 4. For example, the coefficient for hospitalization hovers around -0.01, while the sample mean is 0.13, implying a 8%

lower probability of hospitalization per year of education compared to the mean. For the probability of being hospitalized and being overweight, the health gap in education is relatively constant over the life cycle (the exception being the hospitalization coefficient for the youngest age group which is zero). For the number of diagnosed illnesses, the largest gradients exist between the ages of 55 and 65. For judging one's own health to be poor or bad, educational differences are increasing up until age 60 before decreasing again for the oldest age groups.

3 Instrumental variables estimations by age group

3.1 Empirical Strategy

We estimate the effect of education on health using two-stage least squares (2SLS) in a fuzzy regression discontinuity design framework. Specifically, we run the following regressions where $Yed_{is} \times agegroup_{it}$ is instrumented by $Reform_{is} \times agegroup_{it}$:

$$H_{ist} = \sum_g \beta_g Yed_{is} \times agegroup_{it} + \gamma_1 \tilde{bc}_{is} + \gamma_2 \tilde{bc}_{is} \times Reform_{is} + \beta X_{ist} + \varepsilon_{ist} \quad (2)$$

$Reform_{is}$ is an indicator variable for whether an individual was affected by a compulsory schooling reform in school state s or not, i.e., whether they were born in or after the pivotal cohort (see Table 2 above). \tilde{bc}_{is} is the normalized birth cohort (the distance between the year of birth and the relevant pivotal cohort) that is, the running variable. Control variables in X_{ist} are a full set of fixed effects for age, federal state of birth fixed, survey year, data source, and gender. We provide estimates of the relevant first stages in Figure A1 in the Appendix to show that the compulsory schooling reform increased educational attainment across all cohorts and subsamples (identified by data availability for our different outcome variables).

For a causal interpretation, the cutoff at the pivotal cohorts must be exogenous to the individuals in our sample. This implies that only compulsory schooling changes at the cutoff, so that individuals immediately to the left and right of the cutoff are comparable. The inclusion of state-specific cohort trends as controls supports the validity of this assumption, as these trends help to control for any factors that may have affected cohorts differently in different states. Another reform that occurred concurrently with compulsory education reforms in some states was a shift in the start of the school year from spring to fall. In 1966-1967, most West German states, with the exception of Bavaria, where the school year had already started in the fall, introduced two short school years to accommodate this shift [54]. These short school years had only 24 weeks of instruction compared to the usual

37 weeks. This had the effect that students formally completed nine years of schooling despite having received little additional instruction compared to earlier cohorts. This could potentially affect the identifying assumption and/or bias the estimates. To address both, we show a robustness check where we include indicator variables for short school years in Figure A5 in the Appendix. The estimates do not differ from those obtained in our main results. This is in line with the literature, in particular Kemptner et al. [15], who show that the estimates are generally robust to these adjustments.

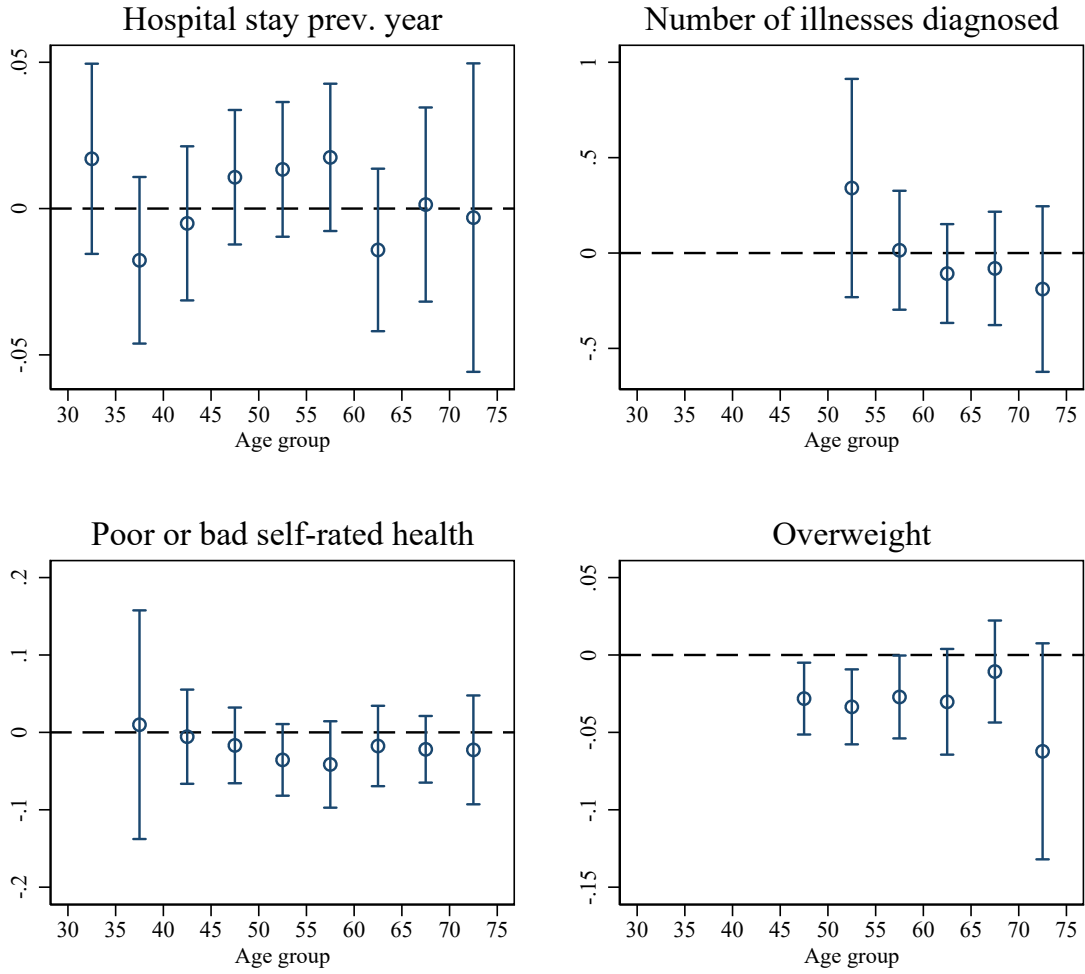
The compulsory schooling reform has been studied in numerous applications. Yet, possible issues with spillover/general equilibrium effects are hardly discussed at all. Schiele [55] studies possible monetary spillover effects of the reform and finds no effects for men but some effects for women. Below we also generate effects separately by men and women but do not find substantial differences.

3.2 Estimation results

Figure 2 shows our main results from the second stage of the 2SLS estimations. For all outcomes except being overweight, the effect of an additional year of schooling fluctuates around zero over the life cycle. This pattern provides evidence that the positive correlation between education and health shown in Figure 1 is unlikely to represent a causal effect of education. The coefficients for hospitalization, number of diagnosed conditions, and poor or bad health are virtually zero. We observe a health gradient for being overweight, where individuals who have additional schooling due to the compulsory education reform in their state are about three percentage points less likely to be overweight between the ages of 45 and 65. The effect in the oldest age group (70-74) is probably larger, about six percentage points, but not statistically significant. Although the number of observations for this age group is smaller than for the others, it is still large (10,500 observations) due to the addition of the Microcensus data. In Figure A2 in the appendix, we report OLS and 2SLS results for two additional sub-outcomes, rating one's health as poor (i.e., the lowest category) and being obese ($BMI \geq 30$). These are essentially more drastic versions of what we examine in our main outcomes, being in poor or bad health and being overweight. Again, there is a health gradient in education in the correlations, which increases until age 60 for bad health and steadily over the life cycle for obesity. However, the causal effect of an additional year of education is practically zero.

Figure 3 shows heterogeneous effects by gender. We find no structural differences between men and women for the first three outcomes. However, the small positive effects of education on being overweight may be driven by women, who consistently have larger negative coefficients (less likely to be obese) than men.

Figure 2: Two-stage least squares results

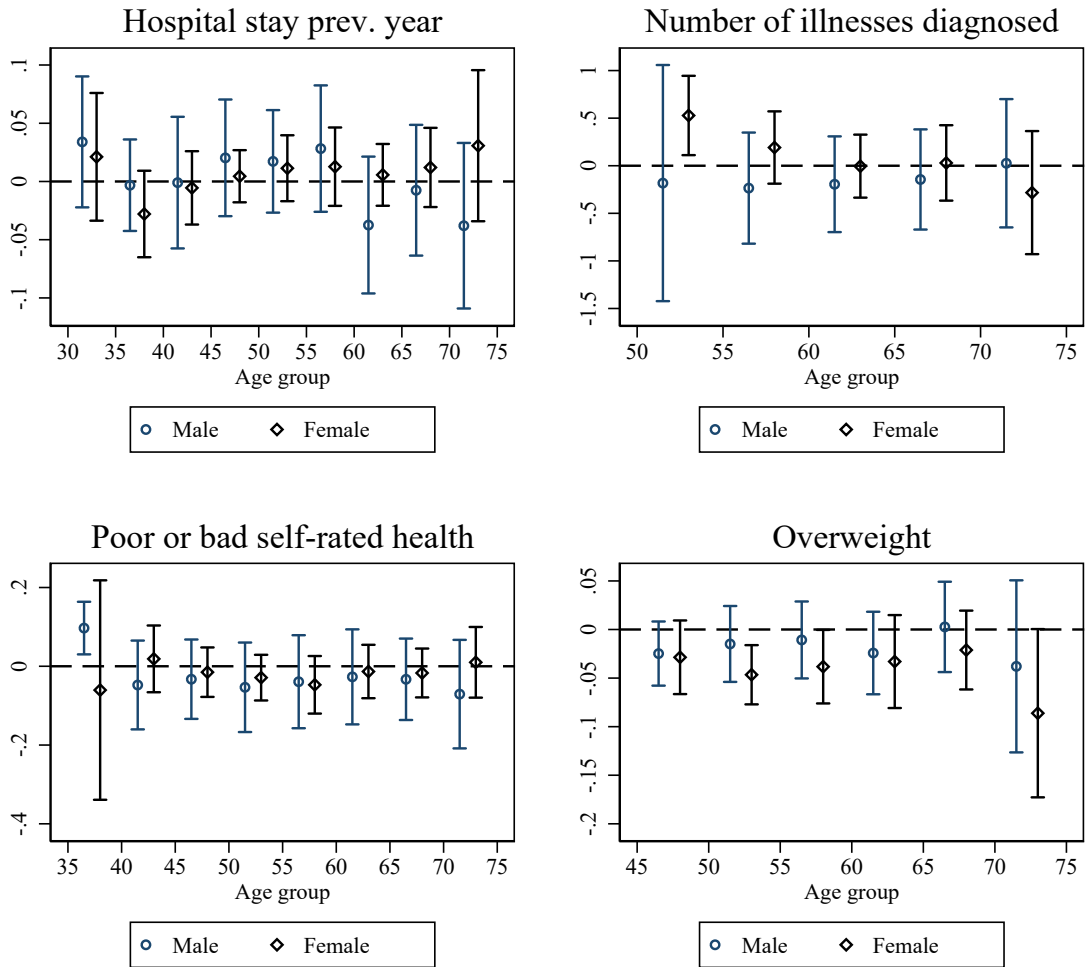


Notes: This figure shows estimates of the effect of years of schooling and four health outcomes across up to nine age groups. The outcome variables are defined as follows: Hospitalization is an indicator variable for whether a person was hospitalized for at least one night in the past year. Poor or bad self-rated health is a binary indicator of checking one of the two lowest of five possible categories of self-rated health. Number of diagnoses is a count variable of the total number of diagnosed (chronic) conditions and diseases from a list of 13 possible options. Overweight is a binary indicator of having a BMI ≥ 25 . Own calculations based on SOEP, SHARE, Microcensus, and NEPS. We visualize point estimates of the coefficients β_g based on 2SLS versions of Eq. (2), where instruments are interactions of reform dummy (pivotal cohort and older) with the age groups. Coefficients plotted between 30 and 35 represent age group 30-34, and so on. Some age groups are missing due to data constraints. Standard errors clustered at state \times year of birth level.

In summary, the main findings of the paper are as follows: individuals with more schooling are in better health, and the health gap by education increases until late midlife for some outcomes. However, there is no evidence of a causal local average treatment effect of additional schooling for individuals with low educational margins. We do not observe broad improvements in health due to the additional year of schooling from the compulsory schooling reforms we study over the life cycle up to age 74. The only exception is overweight, where we see small beneficial effects, mostly for women. However, for obesity, the more drastic outcome, the causal effect is solidly zero across all age groups. Overall, our results are consistent with Clark and Royer [18], who find no effect of education on

mortality over the life cycle using two British compulsory schooling reforms that targeted a similar group as the German reforms: students at highest risk of dropping out of school. Our results contrast with those of Gehrsitz and Williams Jr [32], who find that a British compulsory schooling reform led to economically and statistically significant reductions in hospital admissions for lifestyle-related conditions (such as cardiovascular disease) among men.

Figure 3: Heterogeneous effects by gender



Notes: This figure shows estimates of the effect of years of schooling and four health outcomes across up to nine age groups by gender. The outcome variables are defined as follows: Hospitalization is an indicator variable for whether a person was hospitalized for at least one night in the past year. Poor or bad self-rated health is a binary indicator of checking one of the two lowest of five possible categories of self-rated health. Number of diagnoses is a count variable of the total number of diagnosed (chronic) conditions and diseases from a list of 13 possible options. Overweight is a binary indicator of having a BMI ≥ 25 . Own calculations based on SOEP, SHARE, Microcensus, and NEPS. We visualize separately estimated point estimates of the coefficients β_g based on 2SLS versions of Eq. (2), where instruments are interactions of reform dummy (pivotal cohort and older) with the age groups. Coefficients plotted between 30 and 35 represent age group 30-34, and so on. Some age groups are missing due to data constraints. Standard errors clustered at state \times year of birth level.

Robustness checks

We perform several robustness checks and report their results in the appendix. To corroborate our main results, we first present reduced-form results in Figure A3. It shows estimates of the instrument (being born in or after a pivotal cohort) and its interactions with age groups on our four outcomes. Our main results are also present in the reduced form. The effects have a similar pattern. In Figure A4 we repeat the baseline estimation but do not account for state-specific cohort trends that help lend more credibility our identifying assumption. In Figure A5 we control for short school years by adding an indicator variable for cohorts that were affected (see our point in Section 3.1). In Figures A6 and A7 we provide robustness checks for a different sample selection. Instead of choosing a bandwidth of five years around the pivotal cohort in each state we use four and seven years (Figures A6 and A7, respectively). In Figure A8, we only use observations from the German Socioeconomic Panel — our main data source — to show that pooling different survey datasets does not change our main results. Interestingly, while all other coefficients are similar, the small favorable educational disparity in overweight only seems to be present for younger individuals (45-49) in SOEP. Finally, in Figure A9 we show results using the established state cutoffs from Pischke and Von Wachter [41] to demonstrate that — while the cutoffs from Begerow and Jürges [30] reflect the institutional setting at the time more accurately — our results do not rely on them. Overall, our results are robust to all aforementioned checks. They do not differ qualitatively from our main results.

3.3 Attrition

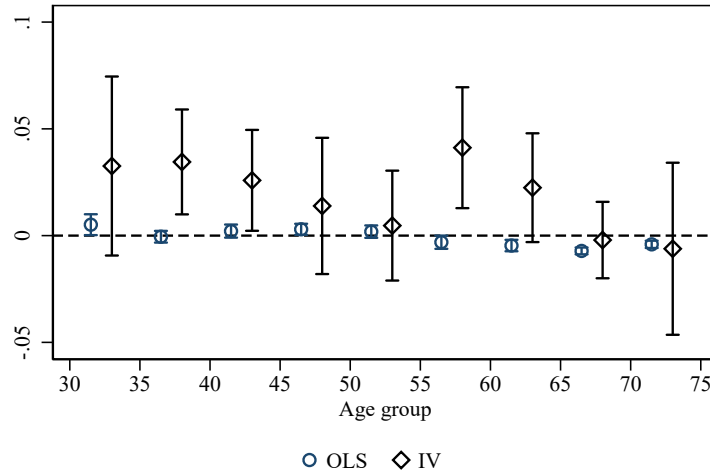
A concern with longitudinal household surveys, especially those focused on older populations and where health is of interest, is potential bias due to attrition [56, 57, 58]. Attrition hinders a survey from being representative of the target population and can create barriers to statistical inference [56, 57]. Although the surveys we use are constantly updated, selective attrition (e.g. due to mortality) by educational status can be a problem.

We use two complementary approaches to test for potential attrition problems. First, we create a binary indicator *attrition* in our working sample. This indicator equals one if an individual does not appear in the next wave of the survey and zero if they either appear in the next wave or if it is the last wave (year 2022) of the survey.⁴ We generate this indicator before selecting the sample based on the pivotal cohort. By this definition, 20 percent of all person-year observations in our sample drop out between two waves. Next, we use *attrition* as the outcome and run OLS and IV regressions as before. Figure 4 shows the results of this exercise. The OLS estimates suggest no relationship between educational attainment and attrition. Our 2SLS estimates point to a small increase in sample attrition

⁴We omit data from the Microcensus for this analysis. Prior to 2005, it was a cross-sectional survey and could not be linked to other waves. Since 2005 it is a rotating panel. However, not all waves include health information. As a result, there are large gaps between successive waves that we use.

due to education for compliers in the youngest age groups as well as for groups 55-60 and 60-65 and a null effect for 50-55 and the oldest age groups. Yet, all differences are vary small.

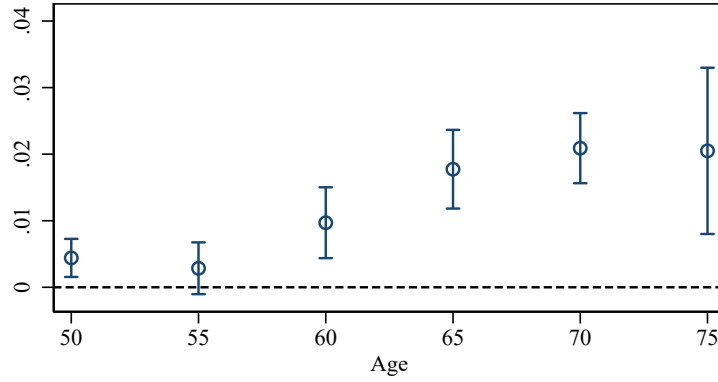
Figure 4: Effect of education on attrition over the life-cycle



Notes: Own calculations based on SOEP, SHARE, Microcensus, and NEPS. 95% confidence intervals. Coefficients plotted between 30 and 35 represent age group 30-34, and so on. Some age groups are missing due to data constraints. Standard errors clustered at state \times year of birth.

As a second approach, we reduce the sample to one observation per individual and create indicator variables if they are still in our original sample at ages 50, 60, 65, 70 o. For example, for a person born in 1930 who drops out of the sample in 2001, all indicator variables except the one for age 75 would be set to one. If a person is still in the sample in 2021 and has not reached one of the age thresholds, the respective indicators are set to missing. 87% of all individuals are still in the survey at age 50 (based on 15,228 individuals). This number drops steadily to 43% who are still in the survey at age 75 (based on 1,592 individuals). We then run separate regressions of all indicators on years of schooling, female, year of birth dummies, survey and state dummies, and state-specific linear cohort trends. The results are shown in Figure 5. We observe a higher probability of remaining in the sample at older ages with more education. The differences increase with age and are statistically significant, but very small in absolute terms. Individuals with one more year of schooling are on average 2.4 percentage points more likely to still be in our sample at age 75. We take the results as indication that selective attrition is unlikely to have a significant effect on our regression results.

Figure 5: Relationship of education on (still) being in the sample at certain ages



Notes: This figure shows estimates of years of schooling on the likelihood of still being in the sample at ages 50-75. Point estimates with 95 % confidence intervals. Own calculations based on data from SOEP, SHARE and NEPS.

4 Potential reasons for zero effects

A possible reason for only very small health effects of the reform (if any), even in the long-run, might be its institutional setting. Pischke and Von Wachter [41] already argued that basic skills of the compliers, necessary for the labor market, might already have been settled after eight years of schooling and that the ninth grade did not further improve them. This is at least consistent with the finding of no returns to cognition of that reform [40]. Another hypothesis could possibly be more important for health effects: the reform might not have affected the types of occupation the compliers worked in afterwards. Apart from health behaviors, job types might be the most important channel how education affects health [59, 60, 61, 62].

To test this, we look at four different classifications of occupations: white-collar vs. blue-collar jobs, physically highly demanding vs. physically less demanding jobs, psychosocially highly demanding vs. psychosocially less demanding jobs and manual vs. non-manual jobs. Occupations are classified as physically (psychosocially) highly demanding if the Overall Physical (Psychosocial) Exposure Index for the occupation derived by Kroll [63] is larger than five and as less demanding if it is less or equal to five, as done by Mazzonna and Peracchi [64]. We group the occupations into manual and non-manual based on the 11 classes of the Erikson and Goldthorpe (EGP) class schema.⁵ EGP classes I, II, III, IVa, IVb and V are classified as non-manual, and classes VI, VII and IVc as manual.⁶ We restrict the sample to those within the working age group i.e. 30 - 65 years and to the SOEP due to data availability.

⁵In the SOEP, the EGP is derived from the ISCO-88 classification as well as the information on self-employment and number of employees/supervisory status [65].

⁶See Table A1 in the Appendix for details.

Table 5 shows results of eight separate regressions (four times OLS and four times 2SLS) where we regress the four outcome variables explained above on years of schooling and the same control variables as before. Here, however, we do not separate results by age groups. The coefficient of years of schooling is reported in the table. In our sample, 67 per cent have white-collar jobs, 53 per cent have physically less demanding jobs, 49 per cent have psychosocially less demanding jobs and 34 per cent are manual workers. OLS estimates show a significant correlation of education and healthier jobs. 2SLS results, however, have coefficients close to zero which are also not statistically significant. It seems that while the compulsory schooling reforms were effective at increasing educational attainment, they did not translate into healthier jobs for those who were exposed to them. This may be part of the explanation why we do not see effects of this reform in the long-run.

Table 5: Effect of education on healthier jobs

	Observations	Sample mean	OLS	2SLS
White collar job	54 348	0.67	0.095*** (0.004)	0.008 (0.041)
Physically less demanding job	54 203	0.53	0.106*** (0.005)	−0.006 (0.054)
Psychosocially less demanding job	53 718	0.49	0.029*** (0.007)	−0.025 (0.065)
Manual work	68 062	0.34	−0.090*** (0.003)	−0.002 (0.044)

Note: This table shows OLS and 2SLS estimates of the causal effect of years of schooling on four outcomes of healthier jobs. Each cell is obtained from a separate regression. Controls include a gender dummy as well as age, state, survey and survey year fixed effects as well as normalized birth cohort and normalized birth cohort \times *Reform*. For the IV estimates, we instrument years of education with a dummy for being born in or after the pivotal cohort of a compulsory schooling reform. Standard errors clustered at the state \times year of birth level in parentheses.

5 Conclusion

We study the relationship of education and health over the life-cycle using compulsory schooling reforms in West Germany as exogenous variation. Our main contribution to the literature is to estimate effects for different age groups starting age 30 and up to age 74, several decades after education took place. This allows to scrutinize a pattern that may have been missed in the previous literature: zero aggregate effects, as often found in the literature, might blur potential health effects that only show up late in life. Stronger effects in older ages can be justified theoretically [1] but may also be expected by descriptive results of an increasing education-health gradient over the life cycle, as found by previous studies, e.g., Case and Deaton [3], Galama and van Kippersluis [4].

While we find a slight increase in the health gap by education over the life cycle, we do not detect causal effects of an additional year of compulsory schooling on health and health care utilization for any age group with the exception of small favorable effects

on overweight for younger age groups in our sample (45-60). Most point estimates are practically zero.

Of course, we only identify local average treatment effects, i.e., effects for individuals who prolonged their education only due to the reform. A possible reason why there are no long-term health effects of this reform might be its institutional setting. While the compulsory schooling reforms succeeded in raising the educational attainment of those they were intended to target — those at the lowest margin of willingness to continue their education — they did not result in healthier employment opportunities for those affected by them. Yet, the most likely channel of how improved education could affect health is through better (and healthier) jobs. This might be different for other changes in the German educational system. For instance, the educational expansion in the 1960s to 1980s with a strong increase in the number of universities and high schools (*Gymnasien*) allowed many individuals to get much more education. Kamhöfer et al. [26] do not only find positive (physical) health effects of this reform for individuals decades later but also that better jobs are a possible mechanism for this effect.

Germany has carried out several reforms of its education system in recent years, also for higher education margins such as university entrance diplomas. While these reforms – most notably the compression of secondary school education from 9 to 8 years, going along with increased instruction times – have been evaluated in terms of short-term health outcomes [66, 67], it cannot be ruled out that larger effects will only show up in some decades. Yet, as these reforms, again, most likely did not have significant effects on individuals' career paths and chosen jobs, the results from this paper at least allow for the prediction that long-run health effects of these reforms might not be substantially larger than the short-term effects.

Acknowledgments

This paper uses data from SHARE Waves 1, 2, 4, 5, 6, 7, 8 and 9 (DOIs: 10.6103/SHARE.w1.900, 10.6103/SHARE.w2.900, 10.6103/SHARE.w4.900, 10.6103/SHARE.w5.900, 10.6103/SHARE.w6.900, 10.6103/SHARE.w7.900, 10.6103/SHARE.w8.100, 10.6103/SHARE.w8.900), see Börsch-Supan et al. [51] for methodological details. The SHARE data collection has been funded by the European Commission, DG RTD through FP5 (QLK6-CT-2001-00360), FP6 (SHARE-I3: RII-CT-2006-062193, COMPARE: CIT5-CT-2005-028857, SHARELIFE: CIT4-CT-2006-028812), FP7 (SHARE-PREP: GA N°211909, SHARE-LEAP: GA N°227822, SHARE M4: GA N°261982, DASISH: GA N°283646) and Horizon 2020 (SHARE-DEV3: GA N°676536, SHARE-COHESION: GA N°870628, SERISS: GA N°654221, SSHOC: GA N°823782) and by DG Employment, Social Affairs & Inclusion through VS 2015/0195, VS 2016/0135, VS 2018/0285, VS 2019/0332, and VS 2020/0313. Additional funding from the

German Ministry of Education and Research, the Max Planck Society for the Advancement of Science, the U.S. National Institute on Aging (U01_AG09740-13S2, P01_AG005842, P01_AG08291, P30_AG12815, R21_AG025169, Y1-AG-4553-01, IAG_BSR06-11, OGHA_04-064, HHSN271201300071C, RAG052527A) and from various national funding sources is gratefully acknowledged (see www.share-project.org).

This paper uses data from the National Educational Panel Study (NEPS; see Blossfeld and Roßbach [68]). The NEPS is carried out by the Leibniz Institute for Educational Trajectories (LIfBi, Germany) in cooperation with a nationwide network.

References

- [1] Robert Kaestner, Cuiping Schiman, and Jason Ward. Education and health over the life cycle. *Economics of Education Review*, 76:101982, 2020. ISSN 0272-7757. doi: <https://doi.org/10.1016/j.econedurev.2020.101982>.
- [2] Michael Grossman. *The Demand for Health: A Theoretical and Empirical Investigation*. National Bureau of Economic Research, 1972.
- [3] Anne Case and Angus S. Deaton. Broken down by work and sex: How our health declines. In *Analyses in the Economics of Aging*, NBER Chapters, pages 185–212. National Bureau of Economic Research, Inc, August 2005.
- [4] Titus J Galama and Hans van Kippersluis. A theory of socio-economic disparities in health over the life cycle. *The Economic Journal*, 129(617):338–374, January 2019. ISSN 0013-0133. doi: [10.1111/eoj.12577](https://doi.org/10.1111/eoj.12577).
- [5] Govert E. Bijwaard, Hans van Kippersluis, and Justus Veenman. Education and health: The role of cognitive ability. *Journal of Health Economics*, 42:29–43, 2015. ISSN 0167-6296. doi: <https://doi.org/10.1016/j.jhealeco.2015.03.003>.
- [6] Liliya Leopold and Thomas Leopold. Education and health across lives and cohorts: A study of cumulative (dis)advantage and its rising importance in Germany. *Journal of Health and Social Behavior*, 59(1):94–112, 2018. doi: [10.1177/0022146517751206](https://doi.org/10.1177/0022146517751206). PMID: 29337605.
- [7] Catherine E. Ross and John Mirowsky. Gender and the health benefits of education. *The Sociological Quarterly*, 51(1):1–19, 2010. doi: [10.1111/j.1533-8525.2009.01164.x](https://doi.org/10.1111/j.1533-8525.2009.01164.x).
- [8] Scott J. Adams. Educational attainment and health: Evidence from a sample of older adults. *Education Economics*, 10(1):97–109, 2002. doi: [10.1080/09645290110110227](https://doi.org/10.1080/09645290110110227).
- [9] Jacob Nielsen Arendt. Does education cause better health? A panel data analysis using school reforms for identification. *Economics of Education Review*, 24(2):149–160, 2005. ISSN 0272-7757. doi: <https://doi.org/10.1016/j.econedurev.2004.04.008>.

- [10] Adriana Lleras-Muney. The relationship between education and adult mortality in the United States. *The Review of Economic Studies*, 72(1):189–221, 01 2005. ISSN 0034-6527. doi: 10.1111/0034-6527.00329.
- [11] Philip Oreopoulos. Estimating average and local average treatment effects of education when compulsory schooling laws really matter. *American Economic Review*, 96(1): 152–175, March 2006. doi: 10.1257/000282806776157641.
- [12] Valerie Albouy and Laurent Lequien. Does compulsory education lower mortality? *Journal of Health Economics*, 28(1):155–168, 2009. ISSN 0167-6296. doi: <https://doi.org/10.1016/j.jhealeco.2008.09.003>.
- [13] Mary A. Silles. The causal effect of education on health: Evidence from the United Kingdom. *Economics of Education Review*, 28(1):122–128, 2009. ISSN 0272-7757. doi: <https://doi.org/10.1016/j.econedurev.2008.02.003>.
- [14] Nils Braakmann. The causal relationship between education, health and health related behaviour: Evidence from a natural experiment in England. *Journal of Health Economics*, 30(4):753–763, 2011. ISSN 0167-6296. doi: <https://doi.org/10.1016/j.jhealeco.2011.05.015>.
- [15] Daniel Kemptner, Hendrik Jürges, and Steffen Reinhold. Changes in compulsory schooling and the causal effect of education on health: Evidence from Germany. *Journal of Health Economics*, 30(2):340–354, 2011. ISSN 0167-6296. doi: <https://doi.org/10.1016/j.jhealeco.2011.01.004>.
- [16] Hans van Kippersluis, Owen O’Donnell, and Eddy van Doorslaer. Long-Run Returns to Education: Does Schooling Lead to an Extended Old Age? *Journal of Human Resources*, 46(4):695–721, October 2011. ISSN 0022-166X, 1548-8004. doi: 10.3368/jhr.46.4.695.
- [17] Anton Carl Jonas Lager and Jenny Torssander. Causal effect of education on mortality in a quasi-experiment on 1.2 million Swedes. *Proceedings of the National Academy of Sciences*, 109(22):8461–8466, 2012. doi: 10.1073/pnas.1105839109.
- [18] Damon Clark and Heather Royer. The effect of education on adult mortality and health: Evidence from Britain. *American Economic Review*, 103(6):2087–2120, October 2013. doi: 10.1257/aer.103.6.2087.
- [19] Hendrik Jürges, Eberhard Kruk, and Steffen Reinhold. The effect of compulsory schooling on health—evidence from biomarkers. *Journal of Population Economics*, 26(2):645–672, April 2013. doi: 10.1007/s00148-012-0409-9.
- [20] Christina Gathmann, Hendrik Jürges, and Steffen Reinhold. Compulsory schooling reforms, education and mortality in twentieth century Europe. *Social Science & Medicine*, 127:74–82, 2015. ISSN 0277-9536. doi: <https://doi.org/10.1016/j.socscimed.2014.01.037>. Special Issue: Educational Attainment and Adult Health: Contextualizing Causality.

- [21] Mårten Palme and Emilia Simeonova. Does women's education affect breast cancer risk and survival? evidence from a population based social experiment in education. *Journal of Health Economics*, 42:115–124, 2015. ISSN 0167-6296. doi: <https://doi.org/10.1016/j.jhealeco.2014.11.001>.
- [22] Giorgio Brunello, Margherita Fort, Nicole Schneeweis, and Rudolf Winter-Ebmer. The causal effect of education on health: What is the role of health behaviors? *Health economics*, 25(3):314–336, 2016. doi: <https://doi.org/10.1002/hec.3141>.
- [23] Kasey Buckles, Andreas Hagemann, Ofer Malamud, Melinda Morrill, and Abigail Wozniak. The effect of college education on mortality. *Journal of Health Economics*, 50: 99–114, 2016. ISSN 0167-6296. doi: <https://doi.org/10.1016/j.jhealeco.2016.08.002>.
- [24] Neil M Davies, Matt Dickson, George Davey Smith, Gerard J Van Den Berg, and Frank Windmeijer. The causal effects of education on health outcomes in the UK Biobank. *Nature human behaviour*, 2(2):117–125, 2018. doi: <https://doi.org/10.1038/s41562-017-0279-y>.
- [25] Costas Meghir, Mårten Palme, and Emilia Simeonova. Education and mortality: Evidence from a social experiment. *American Economic Journal: Applied Economics*, 10 (2):234–56, April 2018. doi: 10.1257/app.20150365.
- [26] Daniel A Kamhöfer, Hendrik Schmitz, and Matthias Westphal. Heterogeneity in marginal non-monetary returns to higher education. *Journal of the European Economic Association*, 17(1):205–244, 02 2019. ISSN 1542-4766. doi: 10.1093/jeea/jvx058.
- [27] Sarah C. Dahmann and Daniel D. Schnitzlein. No evidence for a protective effect of education on mental health. *Social Science & Medicine*, 241:112584, 2019. ISSN 0277-9536. doi: <https://doi.org/10.1016/j.socscimed.2019.112584>.
- [28] Katharina Janke, David W. Johnston, Carol Propper, and Michael A. Shields. The causal effect of education on chronic health conditions in the UK. *Journal of Health Economics*, 70:102252, 2020. ISSN 0167-6296. doi: <https://doi.org/10.1016/j.jhealeco.2019.102252>.
- [29] Martin Fischer, Ulf-G Gerdtham, Gawain Heckley, Martin Karlsson, Gustav Kjellsson, and Therese Nilsson. Education and health: long-run effects of peers, tracking and years. *Economic Policy*, 36(105):3–49, 06 2021. ISSN 0266-4658. doi: 10.1093/epolic/eiaa027.
- [30] Tatjana Begerow and Hendrik Jürges. Does compulsory schooling affect health? Evidence from ambulatory claims data. *The European Journal of Health Economics*, 23 (6):953–968, 2022. doi: <https://doi.org/10.1007/s10198-021-01404-y>.
- [31] Ofer Malamud, Andreea Mitrut, and Cristian Pop-Eleches. The Effect of Education on Mortality and Health: Evidence from a Schooling Expansion in Romania. *Journal of Human Resources*, 58(2):561–592, March 2023. ISSN 0022-166X, 1548-8004. doi: 10.3368/jhr.58.4.1118-9863R2. Publisher: University of Wisconsin Press Section: Articles.

- [32] Markus Gehrsitz and Morgan C. Williams Jr. The Effects of Compulsory Schooling on Health and Hospitalization over the Life-Cycle. *Working Paper*, 2022.
- [33] Manudeep Bhuller, Magne Mogstad, and Kjell G Salvanes. Life-cycle earnings, education premiums, and internal rates of return. *Journal of Labor Economics*, 35(4): 993–1030, October 2017. doi: <https://doi.org/10.1086/692509>.
- [34] Judith M. Delaney and Paul J. Devereux. More education, less volatility? The effect of education on earnings volatility over the life cycle. *Journal of Labor Economics*, 37(1): 101–137, 2019. doi: 10.1086/698897.
- [35] Titus Galama, Adriana Lleras-Muney, and Hans van Kippersluis. The effect of education on health and mortality: A review of experimental and quasi-experimental evidence. *Oxford Research Encyclopedia of Economics and Finance*, 05 2018. doi: 10.1093/acrefore/9780190625979.013.7. URL <https://oxfordre.com/economics/view/10.1093/acrefore/9780190625979.001.0001/acrefore-9780190625979-e-7>.
- [36] Kamila Cygan-Rehm. Are there no wage returns to compulsory schooling in Germany? A reassessment. *Journal of Applied Econometrics*, 37(1):218–223, 2022. doi: <https://doi.org/10.1002/jae.2854>.
- [37] Bernard Black, Alex Hollingsworth, Letícia Nunes, and Kosali Simon. Simulated power analyses for observational studies: An application to the Affordable Care Act Medicaid expansion. *Journal of Public Economics*, 213:104713, 2022. ISSN 0047-2727. doi: <https://doi.org/10.1016/j.jpubeco.2022.104713>.
- [38] Hendrik Schmitz and Beatrice Baaba Tawiah. Life-cycle health effects of compulsory schooling. *Ruhr Economic Papers*, (1006), 2023. doi: 10.4419/96973172. URL <https://ideas.repec.org/p/zbw/rwirep/1006.html>.
- [39] Beatrice Baaba Tawiah. Does education have an impact on patience and risk willingness? *Applied Economics*, 54(58):1–16, June 2022. doi: 10.1080/00036846.2022.2078780.
- [40] Daniel A Kamhöfer and Hendrik Schmitz. Reanalyzing zero returns to education in Germany. *Journal of Applied Econometrics*, 31(5):912–919, 2016. doi: <https://doi.org/10.1002/jae.2461>.
- [41] Jörn-Steffen Pischke and Till Von Wachter. Zero returns to compulsory schooling in Germany: Evidence and interpretation. *The Review of Economics and Statistics*, 90(3): 592–598, 2008. doi: <https://doi.org/10.1162/rest.90.3.592>.
- [42] SOEP. Socio-Economic Panel (SOEP), data for years 1984-2022, SOEP-Core v39, EU Edition, 2024.
- [43] SHARE-ERIC. Survey of Health, Ageing and Retirement in Europe (SHARE) Wave 9. Release version: 9.0.0. SHARE-ERIC. Data set. DOI: 10.6103/SHARE.w1.900. Data set, 2024.

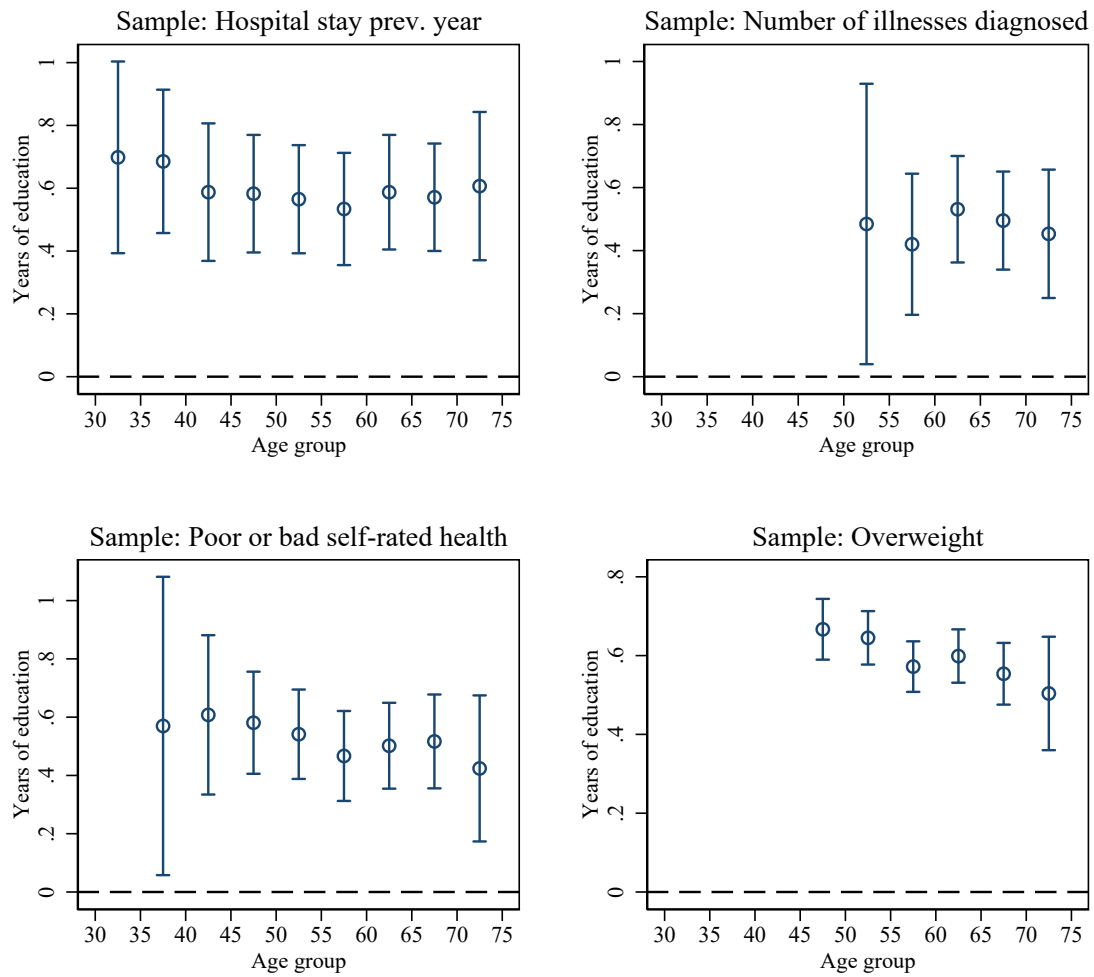
- [44] SHARE-ERIC. Survey of Health, Ageing and Retirement in Europe (SHARE) Wave 9. Release version: 9.0.0. SHARE-ERIC. Data set. DOI: 10.6103/SHARE.w2.900. Data set, 2024.
- [45] SHARE-ERIC. Survey of Health, Ageing and Retirement in Europe (SHARE) Wave 9. Release version: 9.0.0. SHARE-ERIC. Data set. DOI: 10.6103/SHARE.w4.900. Data set, 2024.
- [46] SHARE-ERIC. Survey of Health, Ageing and Retirement in Europe (SHARE) Wave 9. Release version: 9.0.0. SHARE-ERIC. Data set. DOI: 10.6103/SHARE.w5.900. Data set, 2024.
- [47] SHARE-ERIC. Survey of Health, Ageing and Retirement in Europe (SHARE) Wave 9. Release version: 9.0.0. SHARE-ERIC. Data set. DOI: 10.6103/SHARE.w6.900. Data set, 2024.
- [48] SHARE-ERIC. Survey of Health, Ageing and Retirement in Europe (SHARE) Wave 9. Release version: 9.0.0. SHARE-ERIC. Data set. DOI: 10.6103/SHARE.w7.900. Data set, 2024.
- [49] SHARE-ERIC. Survey of Health, Ageing and Retirement in Europe (SHARE) Wave 9. Release version: 9.0.0. SHARE-ERIC. Data set. DOI: 10.6103/SHARE.w8.900. Data set, 2024.
- [50] SHARE-ERIC. Survey of Health, Ageing and Retirement in Europe (SHARE) Wave 9. Release version: 9.0.0. SHARE-ERIC. Data set. DOI: 10.6103/SHARE.w9.900. Data set, 2024.
- [51] Axel Börsch-Supan, Martina Brandt, Christian Hunkler, Thorsten Kneip, Julie Korbmacher, Frederic Malter, Barbara Schaan, Stephanie Stuck, and on behalf of the SHARE Central Coordination Team Zuber, Sabrina. Data Resource Profile: The Survey of Health, Ageing and Retirement in Europe (SHARE). *International Journal of Epidemiology*, 42(4):992–1001, 06 2013. ISSN 0300-5771. doi: 10.1093/ije/dyt088.
- [52] Hans-Peter Blossfeld, Jutta Von Maurice, and Thorsten Schneider. The National Educational Panel Study: need, main features, and research potential. *Zeitschrift für Erziehungswissenschaft*, 14(2):5–17, 2011. doi: <https://doi.org/10.1007/s11618-011-0178-3>.
- [53] NEPS Network. National Educational Panel Study, Scientific Use File of Starting Cohort Adults., 2022.
- [54] Jörn-Steffen Pischke. The impact of length of the school year on student performance and earnings: Evidence from the German short school years. *The Economic Journal*, 117(523):1216–1242, 2007. doi: <https://doi.org/10.1111/j.1468-0297.2007.02080.x>.
- [55] Valentin Schiele. *Labor market spillover effects of a compulsory schooling reform in Germany*. 2022.

- [56] James Banks, Alastair Muriel, and James P Smith. Attrition and health in ageing studies: Evidence from ELSA and HRS. *Longitudinal and life course studies*, 2(2), 2011. doi: 10.14301/llcs.v2i2.115.
- [57] Yiting Deng, D Sunshine Hillygus, Jerome P Reiter, Yajuan Si, and Siyu Zheng. Handling attrition in longitudinal studies: The case for refreshment samples. *Statistical Science*, 28(2):238–256, 2013. doi: 10.1214/13-STS414.
- [58] Eleonora Fichera and David Savage. Income and health in Tanzania. An instrumental variable approach. *World Development*, 66:500–515, 2015. ISSN 0305-750X. doi: <https://doi.org/10.1016/j.worlddev.2014.09.016>.
- [59] Michael Marmot. Status syndrome. *Significance*, 1(4):150–154, 2004. doi: <https://doi.org/10.1111/j.1740-9713.2004.00058.x>.
- [60] Robert Erikson. Social class assignment and mortality in Sweden. *Social Science & Medicine*, 62(9):2151–2160, 2006. ISSN 0277-9536. doi: <https://doi.org/10.1016/j.socscimed.2005.09.009>.
- [61] Sarah A Burgard and Katherine Y Lin. Bad jobs, bad health? How work and working conditions contribute to health disparities. *American Behavioral Scientist*, 57(8):1105–1127, 2013. doi: <https://doi.org/10.1177/0002764213487347>.
- [62] Alexander Darin-Mattsson, Stefan Fors, and Ingemar Kåreholt. Different indicators of socioeconomic status and their relative importance as determinants of health in old age. *International journal for equity in health*, 16(1):1–11, 2017. doi: <https://doi.org/10.1186/s12939-017-0670-3>.
- [63] Lars Eric Kroll. Construction and validation of a general index for job demands in occupations based on ISCO-88 and KldB-92. *methods, data, analyses*, 5(1):28, 2011. doi: <https://doi.org/10.12758/mda.2011.003>.
- [64] Fabrizio Mazzonna and Franco Peracchi. Unhealthy retirement? *Journal of Human Resources*, 52(1):128–151, 2017. doi: 10.3368/jhr.52.1.0914-6627R1.
- [65] SOEP Group. SOEP-Core v37 – PGEN: Person-Related Status and Generated Variables. *SOEP Survey Papers 1186: Series D – Variable Descriptions and Coding.*, 2022.
- [66] Johanna Sophie Quis. Does compressing high school duration affect students’ stress and mental health? Evidence from the National Educational Panel Study. *Journal of Economics and Statistics (Jahrbuecher fuer Nationaloekonomie und Statistik)*, 238(5): 441–476, September 2018.
- [67] Jan Marcus, Simon Reif, Amelie Wuppermann, and Amélie Rouche. Increased instruction time and stress-related health problems among school children. *Journal of Health Economics*, 70(C), 2020. doi: 10.1016/j.jhealeco.2019.1.
- [68] Hans-Peter Blossfeld and Hans-Günther Roßbach, editors. *Education as a Lifelong Process: The National German Educational Panel Study (NEPS)*. Springer VS, edition zfe

(2nd ed.) edition, 2019.

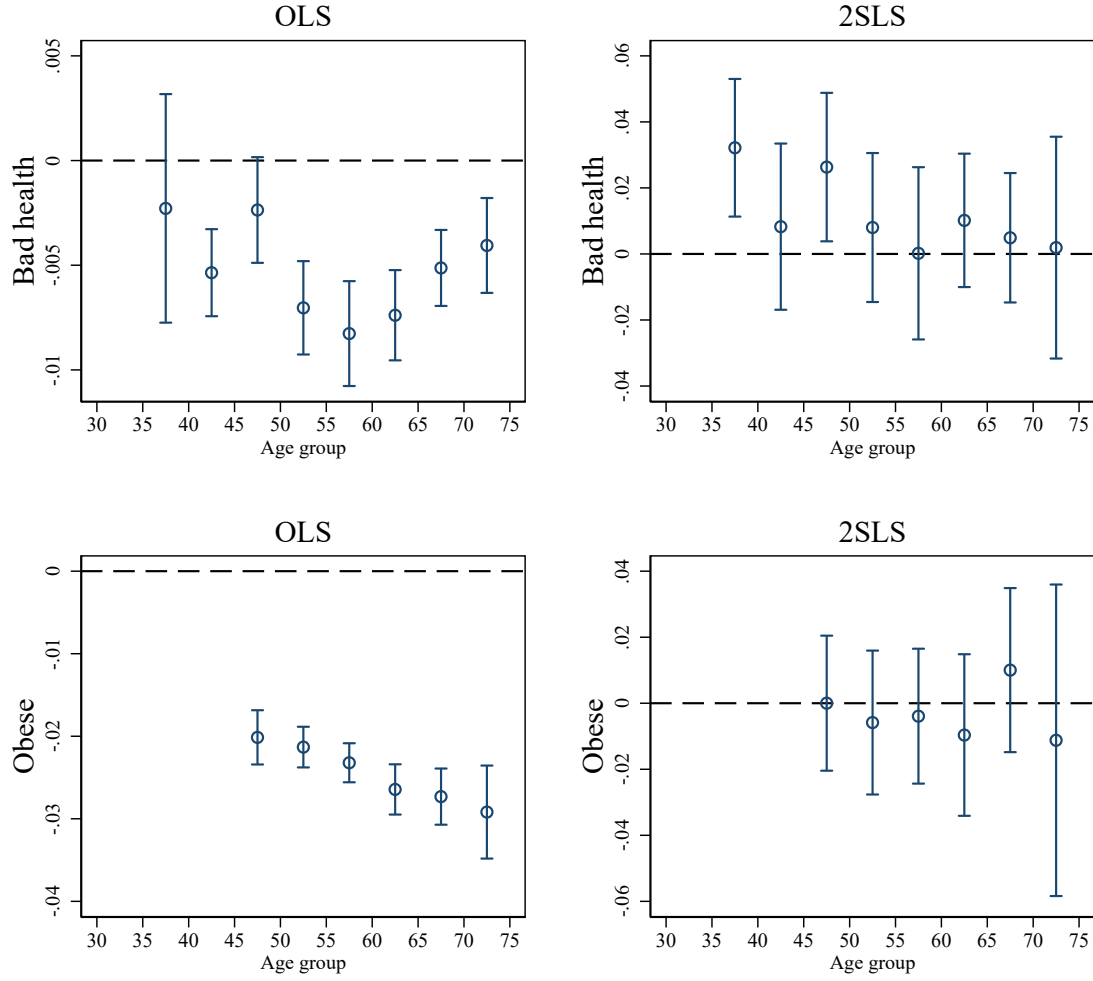
Appendix: Additional tables and figures

Figure A1: First stages



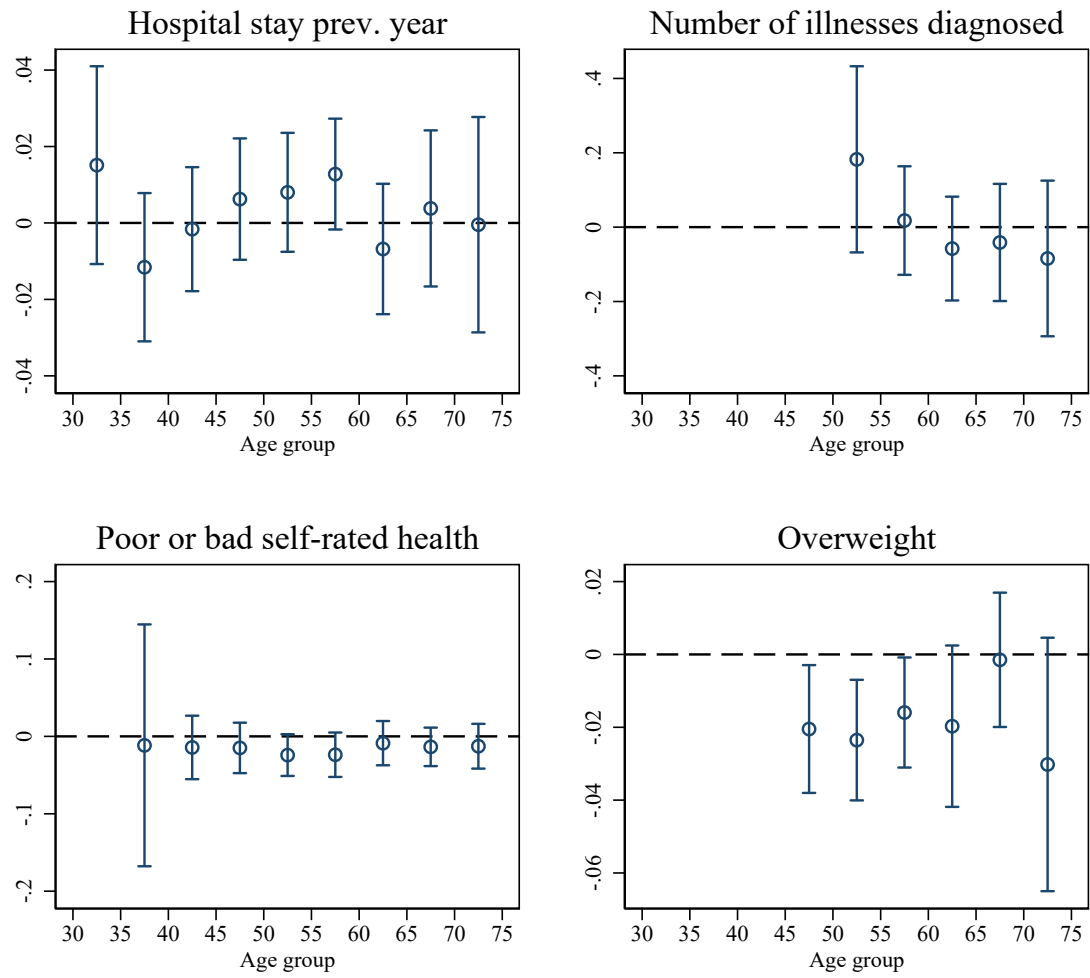
Notes: This figure shows first stage results of our main specification, i.e., point estimates and 95% confidence intervals of the coefficients of $Reform \times agegroup$ in the first stage regressions of Eq. (2). Own calculations based on SOEP, SHARE, Microcensus, and NEPS. Standard errors clustered at state \times year of birth. Coefficients plotted between 30 and 35 represent age group 30-34, and so on.

Figure A2: Additional outcomes –
Bad health & obesity



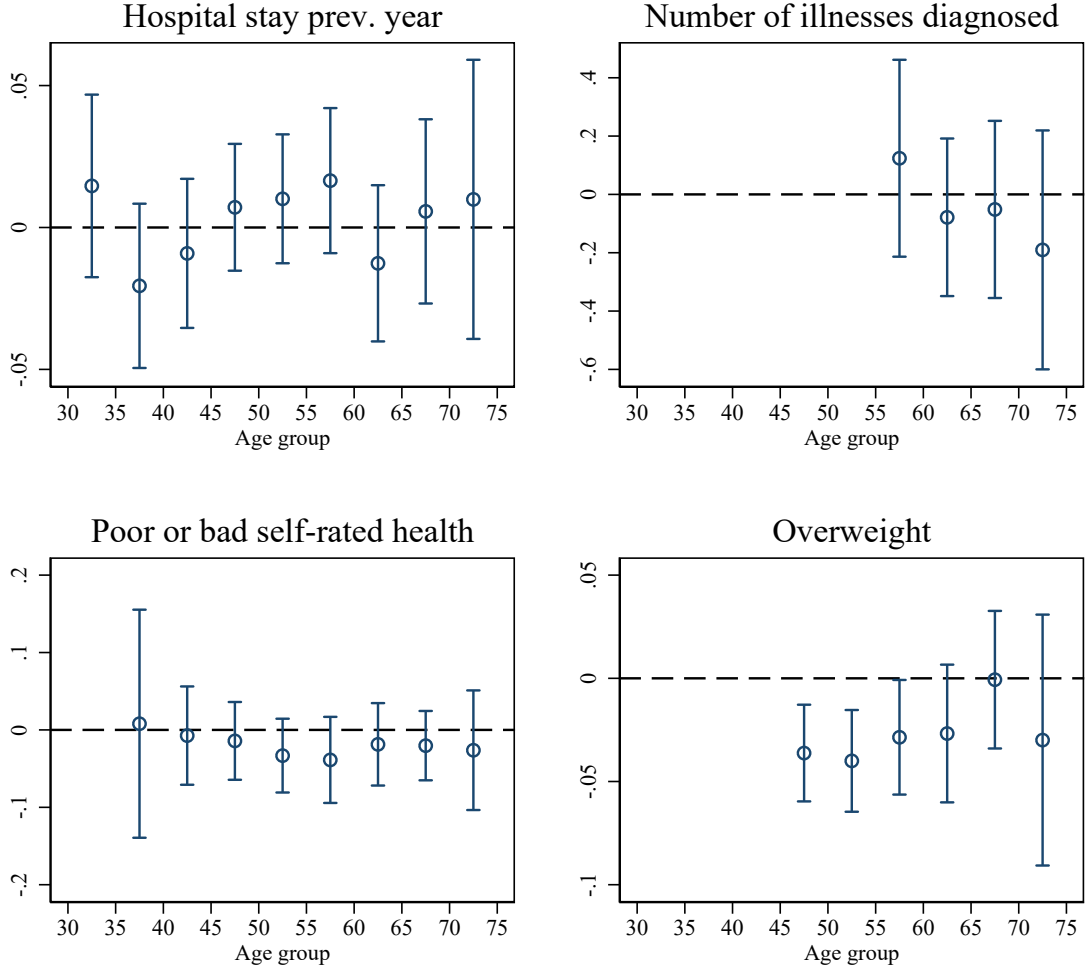
Notes: This figure shows estimates of the effect of years of schooling and four health outcomes across up to nine age groups. The outcome variables are defined as follows: Bad self-rated health is a binary indicator of checking the lowest of five possible categories of self-rated health. Obese is a binary indicator of having a BMI ≥ 30 . Own calculations based on SOEP, SHARE, Microcensus, and NEPS. We visualize point estimates of the coefficients β_g based on 2SLS versions of Eq. (2), where instruments are interactions of reform dummy (pivotal cohort and older) with the age groups. Coefficients plotted between 30 and 35 represent age group 30-34, and so on. Some age groups are missing due to data constraints. Standard errors clustered at state \times year of birth level.

Figure A3: Reduced form effects



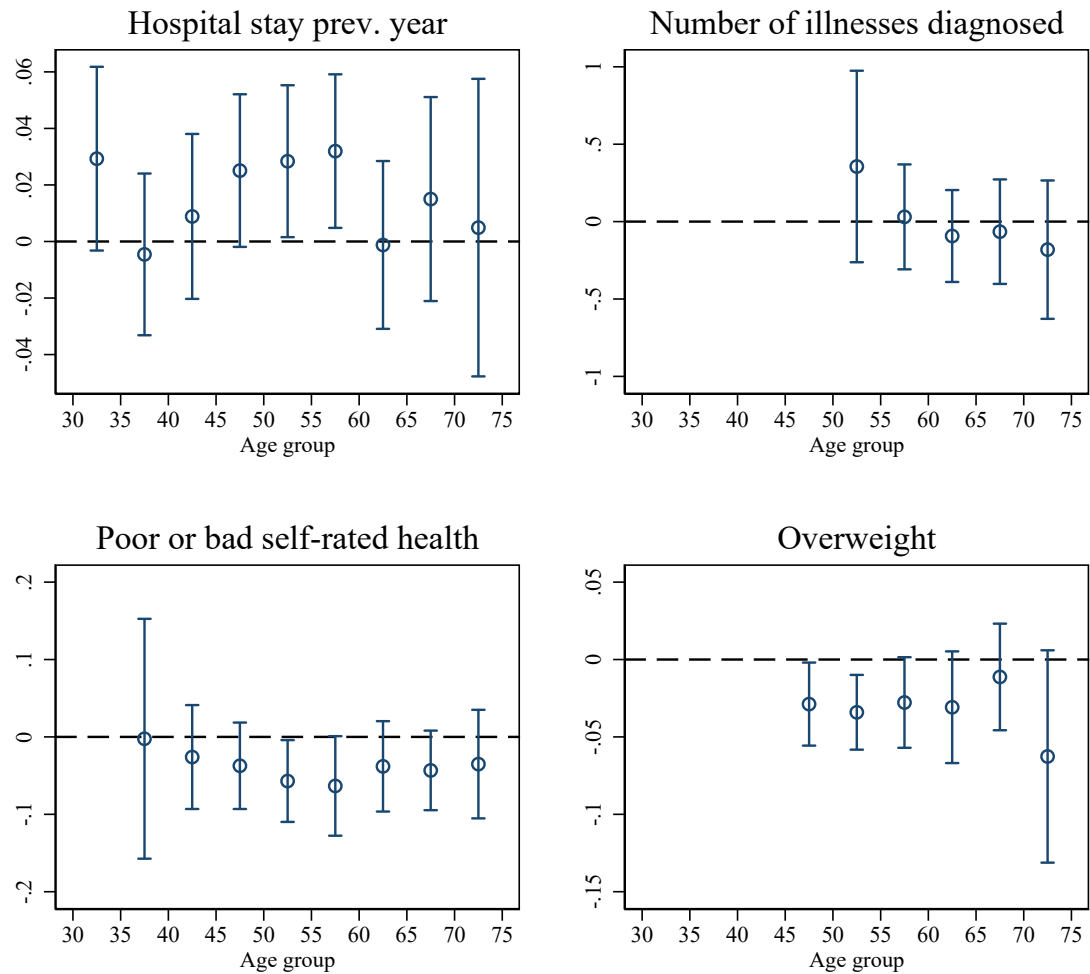
Notes: This figure shows reduced form effects, i.e., the direct effect of compulsory schooling reforms on four health outcomes for up to ten age groups. We visualize coefficients of interactions between the instrument (a dummy for being born in or after the key cohort of a compulsory schooling reform) and the age group. Coefficients plotted between 30 and 35 represent age group 30-34, and so on. Some age groups are missing due to data constraints. Standard errors clustered at state×year of birth level.

Figure A4: Robustness –
No state \times cohort trends



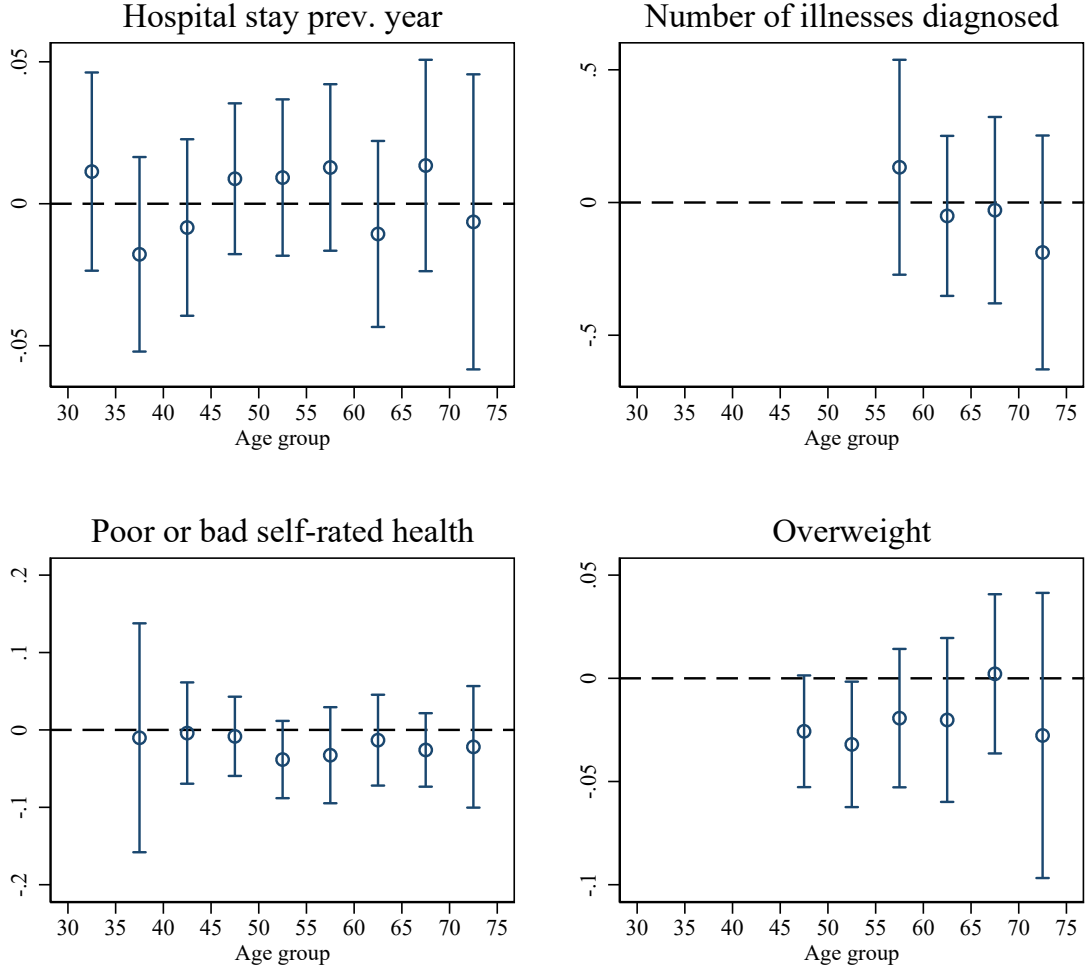
Notes: This figure shows a robustness check of our main results. We estimate the causal effect of years of schooling on four health outcomes across up to nine age groups. Here, we omit state-specific cohort trends that we usually control for. We visualize the 2SLS versions of the point estimates of coefficients β_g (Eq. 2) with 95 % confidence intervals. We instrument the interactions between years of education and age group with interactions between a dummy for being born in or after the pivotal cohort of a compulsory schooling reform and the age group. Coefficients plotted between 30 and 35 represent age group 30-34, and so on. Some age groups are missing due to data constraints. Standard errors clustered at state \times year of birth level.

Figure A5: Robustness –
Controlling for short school years



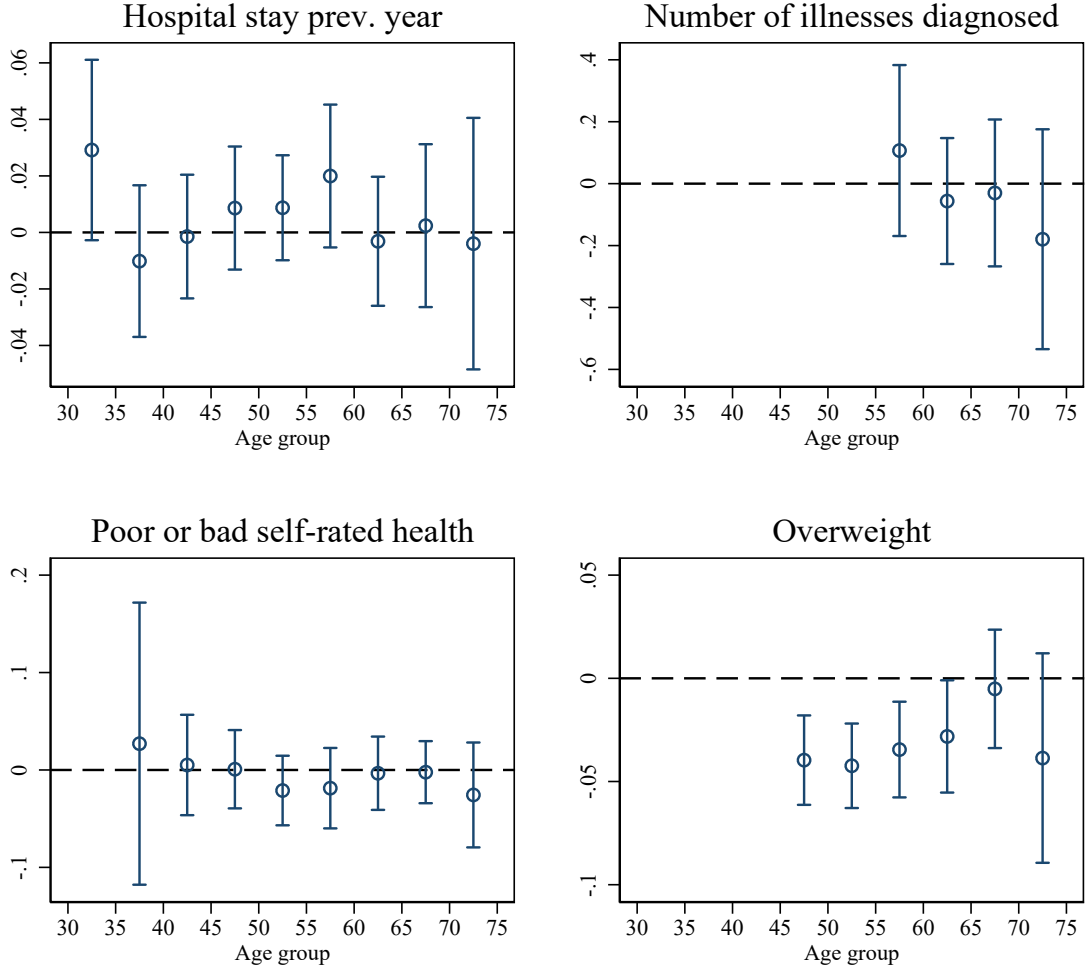
Notes: This figure shows a robustness check of our main results. We estimate the causal effect of years of schooling on four health outcomes across up to nine age groups but add an indicator variable for cohorts who were exposed to short school years. Between 1966 and 1967, the introduction of two short school years in West Germany to adjust the academic calendar coincided with some compulsory schooling reforms and may have overstated formal schooling for some cohorts. We visualize the 2SLS versions of the point estimates of coefficients β_g (Eq.2) with 95 % confidence intervals. We instrument the interactions between years of education and age group with interactions between a dummy for being born in or after the pivotal cohort of a compulsory schooling reform and the age group. Coefficients plotted between 30 and 35 represent age group 30-34, and so on. Some age groups are missing due to data constraints. Standard errors clustered at state \times year of birth level.

Figure A6: Robustness –
Bandwidth of 4 years



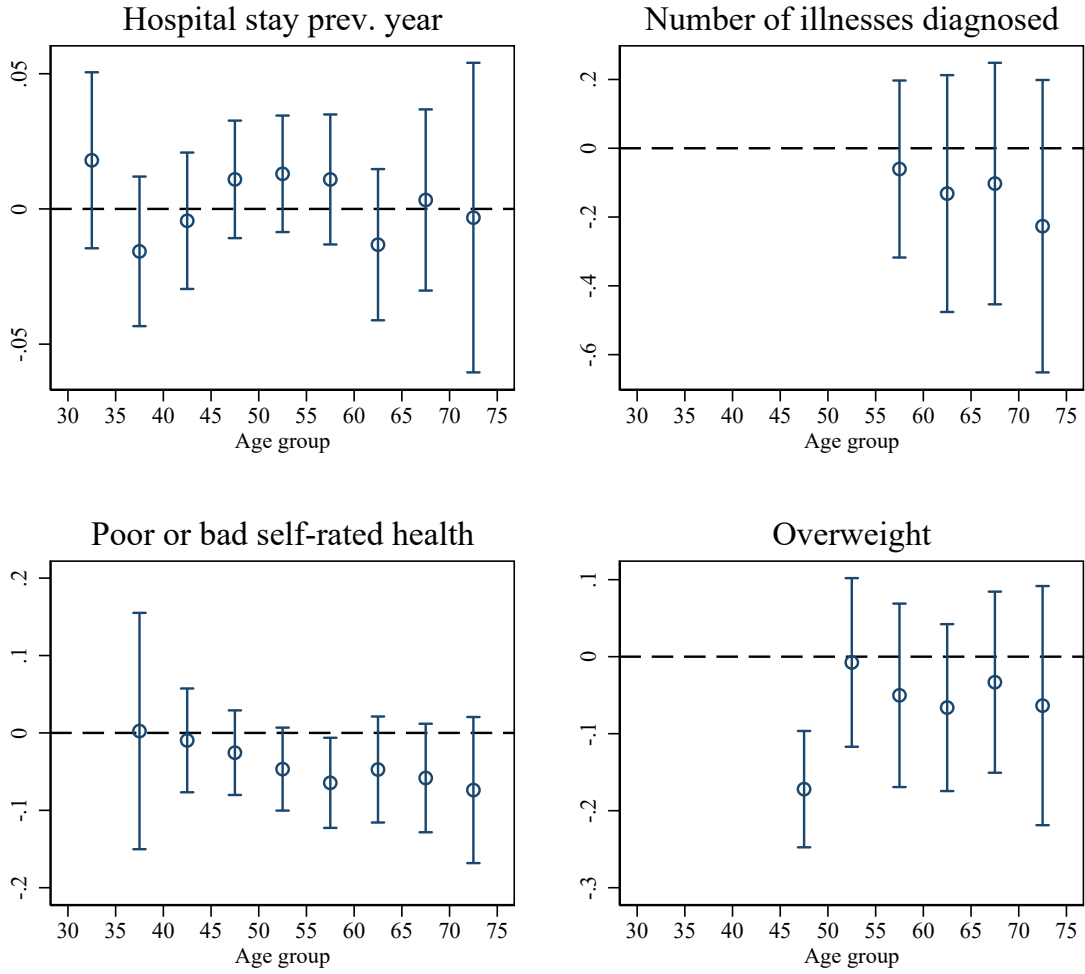
Notes: This figure shows a robustness check of our main results. We estimate the causal effect of years of schooling on four health outcomes across up to nine age groups. Here, instead of selecting our sample to be ± 5 cohorts around each pivotal cohort of the education reform, we use ± 4 . We visualize the 2SLS versions of the point estimates of coefficients β_g (Eq.2) with 95 % confidence intervals. We instrument the interactions between years of education and age group with interactions between a dummy for being born in or after the pivotal cohort of a compulsory schooling reform and the age group. Coefficients plotted between 30 and 35 represent age group 30-34, and so on. Some age groups are missing due to data constraints. Standard errors clustered at state \times year of birth level.

Figure A7: Robustness –
Bandwidth of 7 years



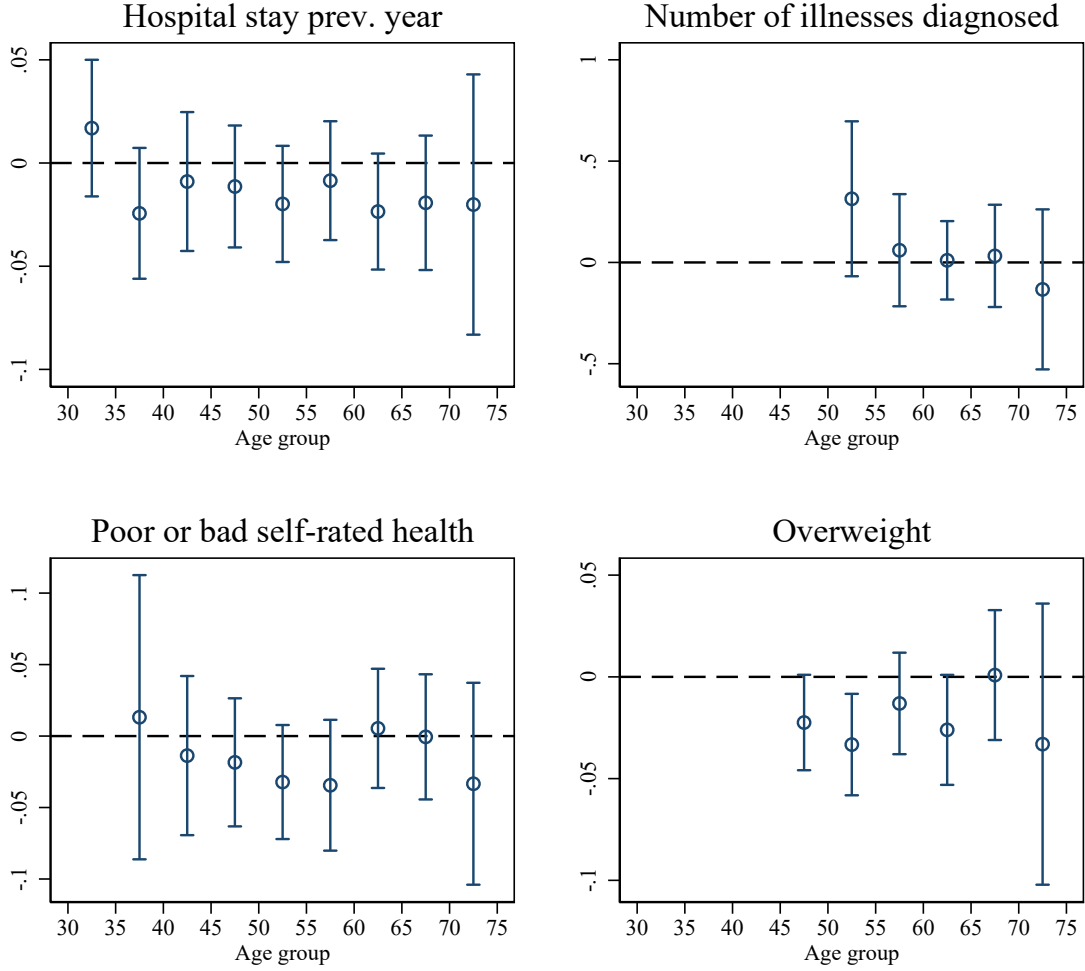
Notes: This figure shows a robustness check of our main results. We estimate the causal effect of years of schooling on four health outcomes across up to nine age groups. Here, instead of selecting our sample to be ± 5 cohorts around each pivotal cohort of the education reform, we use ± 7 . We visualize the 2SLS versions of the point estimates of coefficients β_g (Eq.2) with 95 % confidence intervals. We instrument the interactions between years of education and age group with interactions between a dummy for being born in or after the pivotal cohort of a compulsory schooling reform and the age group. Coefficients plotted between 30 and 35 represent age group 30-34, and so on. Some age groups are missing due to data constraints. Standard errors clustered at state \times year of birth level.

Figure A8: Robustness –
Only data from SOEP



Notes: This figure shows a robustness check of our main results. We estimate the causal effect of years of schooling on four health outcomes across up to nine age groups using only data from the German Socioeconomic Panel (SOEP). We visualize the 2SLS versions of the point estimates of coefficients β_g (Eq.2) with 95 % confidence intervals. We instrument the interactions between years of education and age group with interactions between a dummy for being born in or after the pivotal cohort of a compulsory schooling reform and the age group. Coefficients plotted between 30 and 35 represent age group 30-34, and so on. Some age groups are missing due to data constraints. Standard errors clustered at state \times year of birth level.

Figure A9: Robustness –
Cutoffs from Pischke and Von Wachter [41]



Notes: This figure shows a robustness check of our main results. We estimate the causal effect of years of schooling on four health outcomes across up to nine age groups using the well-established compulsory schooling state cutoffs from Pischke and Von Wachter [41]. We visualize the 2SLS versions of the point estimates of coefficients β_g (Eq.2) with 95 % confidence intervals. We instrument the interactions between years of education and age group with interactions between a dummy for being born in or after the pivotal cohort of a compulsory schooling reform and the age group. Coefficients plotted between 30 and 35 represent age group 30-34, and so on. Some age groups are missing due to data constraints. Standard errors clustered at state \times year of birth level.

Table A1: Job classifications

EGP classification	Manual/Non-manual
(I) Higher Managerial and Professional Workers	Non-manual worker
(II) Lower Managerial and Professional Workers	Non-manual worker
(IIIa) Routine Clerical Work	Non-manual worker
(IIIb) Routine Service and Sales Work	Non-manual worker
(IVa) Small Self-Employed with Employees	Non-manual worker
(IVb) Small Self-Employed without Employees	Non-manual worker
(V) Manual Supervisors	Non-manual worker
(VI) Skilled Manual Workers	Manual worker
(VIIa) Semi- and Unskilled Manual Workers	Manual worker
(VIIb) Agricultural Labour	Manual worker
(IVc) Self-Employed Farmers	Manual worker

Source: SOEP Group [65]. *Notes:* Own grouping for manual/non-manual and calculation based on SOEP and NEPS.