Life-cycle Health Effects of Compulsory Schooling

Johannes Hollenbach
RWI,
Paderborn University

Hendrik Schmitz
RWI,
Paderborn University

Beatrice Baaba Tawiah
MPI-SHARE

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Abstract

We study the effect of education on health (hospital stays, number of diagnosed conditions, self-rated poor health, and obesity) 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 79). However, we do not find causal effects of an additional year of schooling on health or health care utilization, neither earlier, nor later in life. One reason for this may be that while the compulsory schooling reforms succeeded in raising the educational attainment of those that were targeted — individuals at the lowest education margin — they did not result in 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, 45131 Essen, Germany, Tel.: +49 201 8149261, E-mail: johannes.hollenbach@rwi-essen.de.

Hendrik Schmitz: Paderborn University, Warburger Strasse 100, 33098 Paderborn, Germany, Tel.: +49 5251 603213, E-mail: hendrik.schmitz@uni-paderborn.de.

Beatrice Baaba Tawiah: Munich Research Institute for the Economics of Aging and SHARE analyses, Leopoldstraße 139, 80804 München, Germany, E-mail: b.tawiah@mea-share.eu.

1 Introduction

Estimating the effects of education on socio-economic outcomes has been an important part of applied microeconometrics in the past three decades. While most of the literature has focused on labor market outcomes, effects on health have been studied as well. In Table 1 we list 24 studies that estimate health effects of education and use methods of instrumental variable estimation for identification. More than half of these studies do not find statistically significant effects overall or in relevant subgroups. All these mentioned studies have in common that they aggregate effects over age groups, often over several decades. Yet, this may miss relevant patterns. Kaestner et al. (2020) extend the classic Grossman (1972) model of demand for health and conclude that “it is unlikely that the relationship between education and health will be 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 health stock” (Kaestner et al., 2020). 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 sets in late in life and that the aggregate average is blurred by a zero effect for younger individuals.

It is well known that the socio-economic status-health gradient increases over the life-cycle (e.g., Case and Deaton, 2005, Galama and van Kippersluis, 2019). This descriptive pattern has also been shown more specifically for the education-health gradient. As an example, Kaestner et al. (2020) find no differences in mortality by education until the age of 60, but afterwards hazard rates diverge by education. In contrast, they find an education-morbidity gradient only for the age group 45-60 but explain this with possible selective mortality. Bijwaard et al. (2015) find an increasing difference in mortality between those with primary education and those with more than primary education mostly after age 60. They find that the differences are mainly due to selection effects (based on cognitive abilities) at early ages, while the role of education increases after age 60. Leopold and Leopold (2018) find differences in self-rated health between higher-educated and lower-educated individuals over ages 30 to 80, which increase from age 50 (for men). Ross and Mirowsky (2010) find a physical impairment gap between the well-educated and 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 health effects of education by trying to find out whether these effects vary over the life-cycle, thereby going beyond the descriptive analyses. In our study, exogenous variation comes from compulsory schooling reforms in West Germany. Reforms were introduced on federal state level for birth cohorts between 1931 and 1954, depending on the state. Our baseline data set is the German Socio-Economic Panel study

(SOEP), a representative survey running from 1984 until today. We pool these data with the Survey of Health Ageing, and Retirement (SHARE), the German National Educational Panel Study (NEPS) and the German Microcensus. Our compiled dataset covers a 38-year period and allows us to follow individuals born around the pivotal reform-cohorts over many decades and estimate both short-run and long-run effects of education on health within the same framework and dataset. This allows us to identify when possible health returns to education start materializing or growing. 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	Country	Type	Instrument	Age group	Results
Adams (2002)	USA	Secondary	QOB	51 to 61	Positive effects
Arendt (2005)	Denmark	Middle school	CSR	25 to 64	No effects
Lleras-Muney (2005)	USA	Secondary	CSR	35 to 73	Reduction in mortality
Oreopoulos (2006)	UK	Secondary	CSR	32 to 64	Positive effects
Albouy and Lequien (2009)	France	Secondary	CSR	48 to 80	No effects
Silles (2009)	UK	Secondary	CSR	25 to 60	Positive effects
Braakmann (2011)	UK	Secondary	February birth	28 to 45	No effects
Kemptner et al. (2011)	Germany	Secondary	CSR	16 to 65	Women: no effects Men: positive effects
van Kippersluis et al. (2011)	Netherlands	Secondary	CSR	80 to 88	Reduction in mortality
Lager and Torssander (2012)	Sweden	Various	CSR	15 to 64	No effects
Clark and Royer (2013)	UK	Secondary	CSR	12 to 74	No effects
Jürges et al. (2013)	UK	Secondary	CSR	32-53 + 44-77	No effects
Gathmann et al. (2015)	Europe	Secondary	CSR	50+	Women: no effects Men: positive effects
Palme and Simeonova (2015)	Sweden	Secondary	CSR	28 to 66	Negative effects
Brunello et al. (2016)	Europe	Secondary	CSR	50+	Positive effects
Buckles et al. (2016)	USA	College	War draft	28 to 65	Positive effects
Davies et al. (2018)	UK	Secondary	CSR	37 to 74	Positive effects
Meghir et al. (2018)	Sweden	Secondary	CSR	16 to 75	No effects
Kamhöfer et al. (2019)	Germany	College	College expansion	39 to 68	Mental health: no effects Physical health positive e.
Dahmann and Schnitzlein (2019)	Germany	Secondary	CSR	50 to 85	No effects
Janke et al. (2020)	UK	Secondary	CSR	42 to 60	No effects (except diabetes)
Fischer et al. (2021)	Sweden	Secondary	CSR	18 to 81	Positive effects
Begerow and Jürges (2022)	Germany	Secondary	CSR	50 to 79	No effects
Malamud et al. (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. (2016) 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 (2013) and Gehrsitz and Williams Jr (2022).[1] Clark and Royer (2013) find that two changes in British compulsory schooling laws did not affect mortality as a whole, but also not when focussing on 5-year age groups between 20-24 and 65-69. Gehrsitz and Williams Jr (2022) study effects of a reform in Scotland and report results by age for 30-55 years old individuals. They do not find effects on self-reported health but a reduction in hospitalizations for selected conditions. This mainly holds for

[1] Bhuller et al. (2017) and Delaney and Devereux (2019) study life-cycle effects of education on earnings.

men and starts after age 40. In contrast to Clark and Royer (2013), we study life-cycle effects on morbidity and health care utilization and also go beyond age 69 (and age 55 as in Gehrsitz and Williams Jr, 2022).

Our results show a positive correlation of health and education across all age groups. 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, and a lower likelihood of rating one's health as poor or being obese. The associations between education and number of illnesses and poor self-rated health appear to peak between the ages of 55 and 65. For obesity, the health gaps increase steadily before decreasing for those aged 75 to 79. However, when examining the causal relationship, we find hardly any education effects on health and health care use. One exception is poor self-rated health among the oldest age group where we find a sizeable reduction in age-group 7579 which, however, is not statistically significant.

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 (Tawiah, 2022). 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 (Kamhöfer and

Schmitz, 2016). 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, 2008, 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 (SOEP, 2024). 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 (SHARE-ERIC, 2024a,b,c,d,e,f,g,h; Börsch-Supan et al., 2013). 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 (Blossfeld et al., 2011; NEPS Network., 2022). 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: Begerow and Jürges (2022) for the reform years. 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 79 (for age-group 75-79) years to make sure that effects for certain age groups are not completely driven by individuals from single federal states. Nevertheless, effects for the age group 75-79, our oldest age group in the sample, will only be identified from individuals in Schleswig-Holstein, Hamburg, Lower Saxony, Saarland and Bremen. We do not consider this a problem of internal validity and, moreover, do not see a clear reason to assume that the effects in these federal states should differ from effects in this age group in the other states. Yet, there may be some concern regarding certain events during the early childhood years of those in this age group, such as malnutrition resulting from the food crisis in Germany from 1944 to 1948 which was severe in 1945, affecting the educational achievement, occupational status and income of individuals born in the winter of 1945/46, that may have long-term effects on health (Jürges, 2013). Such events may drive cohort/federal state effects which may influence the results instead of education. Individuals in age group 75-79 had already been born by 1945, implying that a majority of them were not affected by the food crisis in-utero. None of the individuals from Hamburg are affected and only 5% of observation in this age group were born in 1945. We, therefore, do not expect the food crisis to have a great impact on our results for the oldest age group but, obviously, cannot rule that out.

The data has information on age, gender, the state in which an individual attended school, years of education and the type of school-leaving degree. We use the school-leaving 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 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,

[2]The 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 (2022) 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.

depressive psychosis, dementia, joint disorder (also osteoarthritis, rheumatism), chronic back complaints and other illnesses. We count the number of diagnoses. *Poor health* is based on the 5-point scale of self-rated health and equals one if individuals choose the worst category. *Obesity* is a binary variable that indicates a body-mass index larger than 30 (based on self-stated body weight and height).

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 group. Note that diagnoses and hospital visits are not included in the NEPS and Microcensus data. Microcensus is only used for obesity and dominates the sample size there.

Table 3: Number of observations

	Hospitalization last year		Poor health		Diagnoses		Obese	
Age group	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)
75-79	1375	(81 / 19 / 0 / 0)	2086	(54 / 12 / 34 / 0)	626	(59 / 41 / 0 / 0)	2478	(25 / 10 / 7 / 58)

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 (145,325 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 18% are obese, while 4% state that they are in poor health. The descriptives for treatment, instrument and other information are, as an example, taken from the sample for poor self-rated health. The average age is 59 years and the sample is almost gender balanced. The average years of schooling is about 10.1 years.

Table 4: Descriptive statistics

	Mean	SD	Min.	Max.	Observations	Survey years
Outcome variables						
Hospitalization	0.13	0.33	0	1	91 149	1984-2022
Poor self-rated health	0.04	0.19	0	1	99 910	1992-2022
Number of diagnoses	1.71	1.60	0	12	23 056	2004-2022
Obese	0.18	0.39	0	1	145 328	1999-2022
Treatment and instrument						
Years of schooling	10.10	1.74	8	13	99 910	
Reform	0.56	0.50	0	1	99 910	
Other information						
Age	59.42	9.13	30	79	99 910	
Female	0.51	0.50	0	1	99 910	

Notes: This table presents summary statistics. The outcome variables are defined as follows: Hospitalization is an indicator variable for whether a person was admitted to a hospital for at least one night in the previous year. Poor self-rated health is a binary indicator of checking the lowest of five possible categories in 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. Obese is a binary indicator of having a BMI > 30. The statistics for age, female, years of schooling, and reform are based on the estimation sample for poor health.

2.3 OLS estimations

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

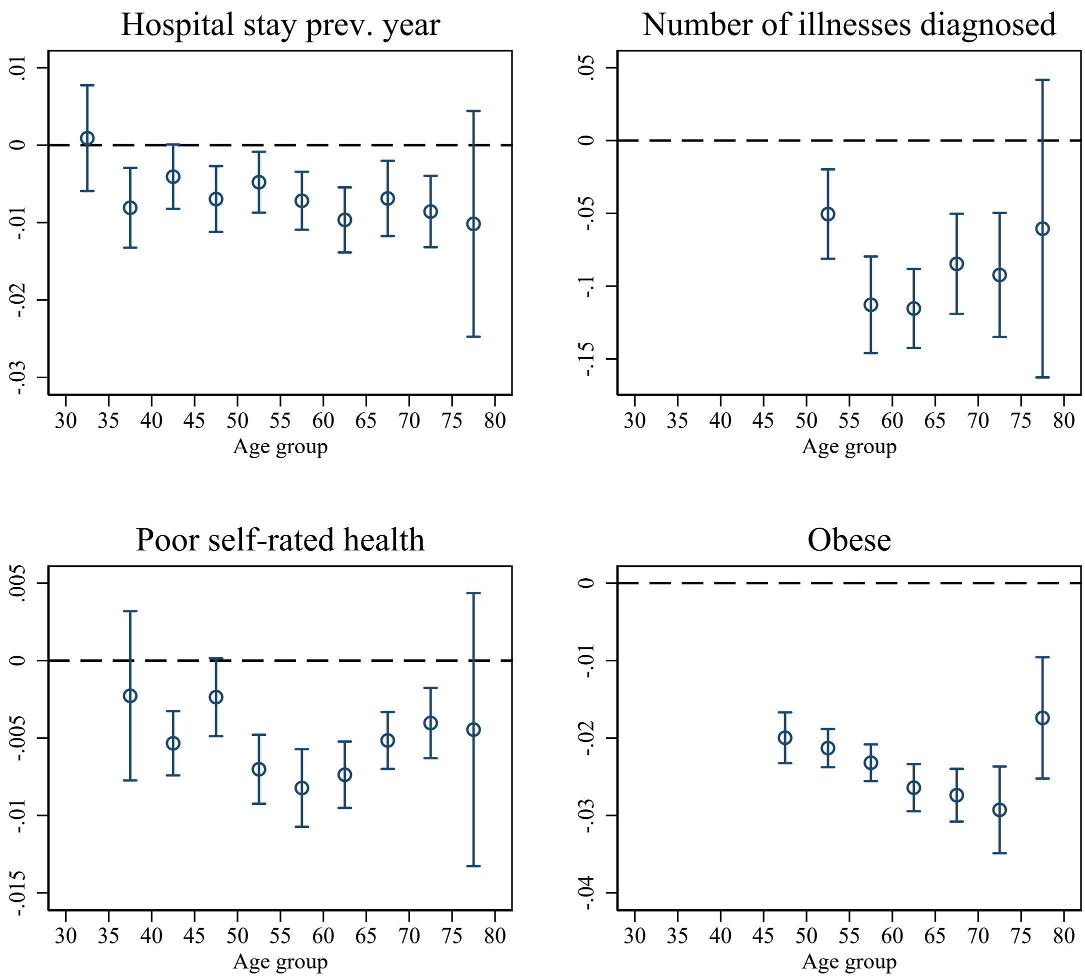
$$H_{ist} = \sum_{g} \beta_g Yed_{is} \times agegroup_{it} + \beta X_{ist} + \varepsilon_{ist} \qquad (1)$$

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,. . ., 75-79.[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 state$\times$year of birth levels in all specifications. The coefficients of interest, β_g, are reported in Figure 1.

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, and a lower probability of being obese at all ages. Although the coefficients are small in absolute terms, they are sizable when

[3]The 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.

Figure 1: OLS results



Notes: This figure shows estimates of the association between years of schooling and four health outcomes across up to ten age groups. 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. Some age groups are missing due to data constraints. Standard errors clustered at state$\times$year of birth. Circles between 30 and 35 stand for age group 30-34, and so on.

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 obese, the health gap in education widens over the life cycle (the exception being the obesity coefficient for the 75-79 age group). For the number of diagnosed illnesses and the probability of judging one's own health to be poor, the largest gradients exist between the ages of 55 and 65.

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 $Reform_{is} \times agegroup_{it}$:

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

$Reform_{is}$ 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). $\widetilde{bc}_{is}$ 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 (Pischke, 2007). 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 A4 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. (2011), who show that the estimates are generally robust to these adjustments.

3.2 Estimation results

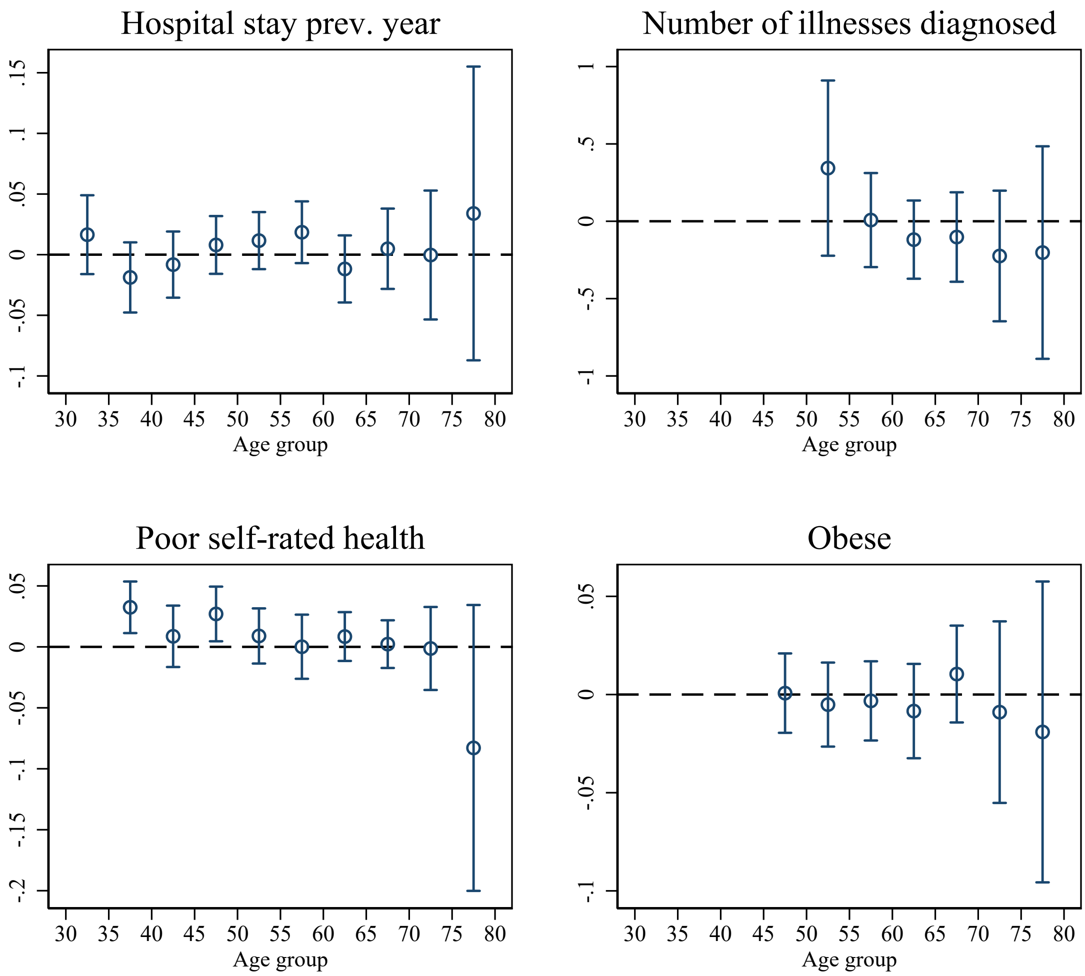
Figure 2 shows our main results from the second stage of the 2SLS estimations. For all outcomes, the effect of an additional year of schooling fluctuates around zero over the life cycle. This pattern provides evidence that the favorable correlation between education and health shown in Figure 1 — which increases over the life-cycle for some outcomes — is unlikely to represent a causal effect of education. While precision is an issue (especially for the oldest cohorts in our sample), the remaining coefficients are close to zero and not statistically significant. The only large effect we find, albeit imprecisely estimated, is a reduction in the likelihood of poor self-rated health among 75-79 year olds. We prefer not to put too much weight on this effect which may well be an outlier, driven in part by observations from SHARE. Given absence of any effects until age 74 it seems highly unlikely that a dramatic effect would set in at age 75. In our robustness check in Table A7, which uses only observations from the German Socioeconomic Panel, we find effect sizes that are only half as large in this age group. Figure 3 shows heterogeneous effects by gender. We do not find a structural difference in results between men and women.

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 over time. However, there is no evidence of a causal local average treatment effect of additional schooling for individuals at low educational margins. We do not observe any improvement in health due to the additional year of schooling from the compulsory schooling reforms we study over the life cycle up to age 79. These results are consistent with Clark and Royer (2013), 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 (2022), 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.

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 A2. 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 A3 we repeat the baseline estimation but do not account for state-specific cohort trends that help lend more credibility our identifying assumption. In Figure A4 we control for short school years by adding an indicator variable for cohorts that were affected. In Figures A5 and A6 we make a different sample selection. Instead of five years around the pivotal cohort in each state we use 4 (Figure A5) and (Figure A6). Finally, in Figure A7, we only use observations from the

Figure 2: Two-stage least squares results – Baseline



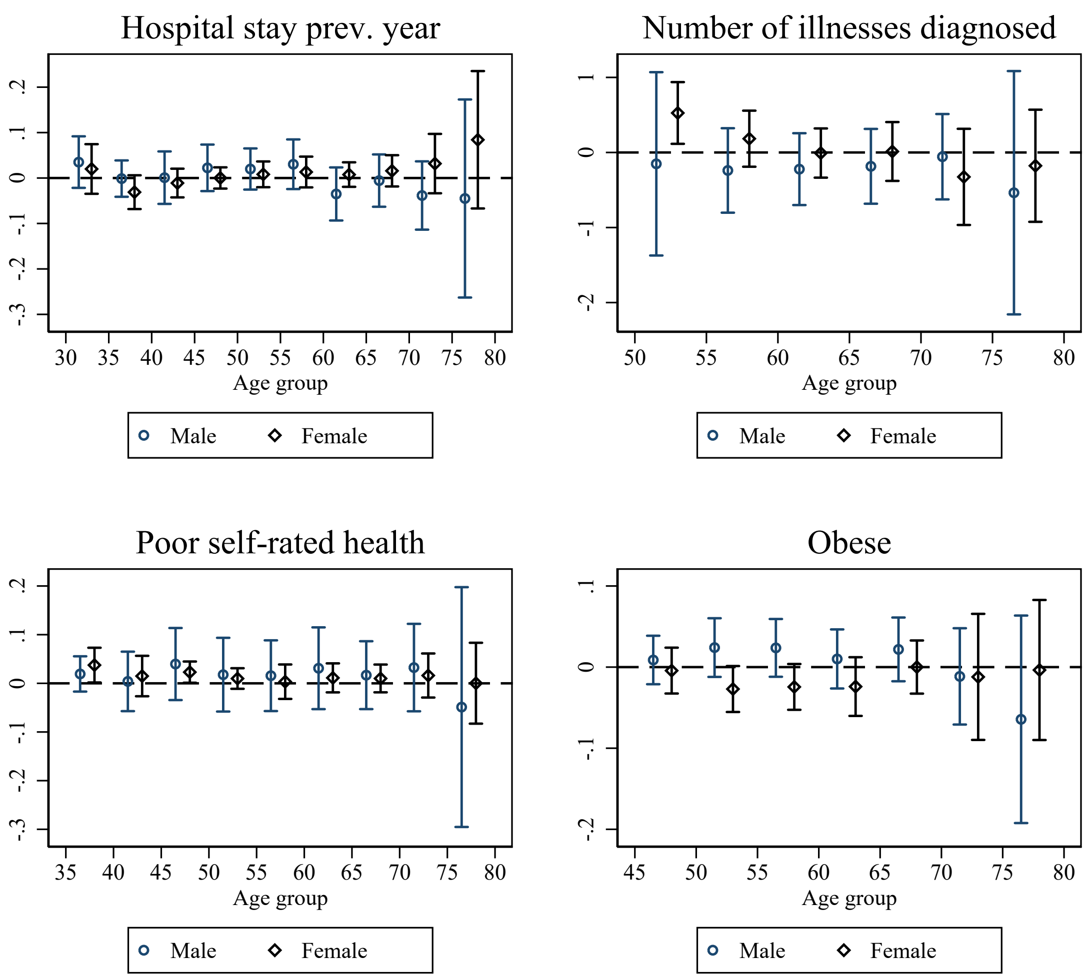
Notes: Own calculations based on SOEP, SHARE, Microcensus, and NEPS. Point estimates of the coefficients β_g based on 2SLS versions of Eq. (2), where instruments are interactions of reform dummy (pivotal cohort and older) with the age groups. 95% confidence intervals. Standard errors clustered at state $\times$ year of birth. Circles between 30 and 35 stand for age group 30-34, and so on.

German Socioeconomic Panel — our main data source — to show that pooling different survey datasets does not change our main results. The results in all robustness checks are very similar to those in our baseline specification.

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 (Banks et al., 2011; Deng et al., 2013; Fichera and Savage, 2015). Attrition hinders a survey from be-

Figure 3: Heterogeneous effects by gender



Notes: Own calculations based on SOEP, SHARE, Microcensus, and NEPS. Point estimates of the coefficients β_g based on 2SLS versions of Eq. (2), separately by gender, where instruments are interactions of reform dummy (pivotal cohort and older) with the age groups. 95% confidence intervals. Standard errors clustered at state $\times$ year of birth. Circles between 30 and 35 stand for age group 30-34, and so on.

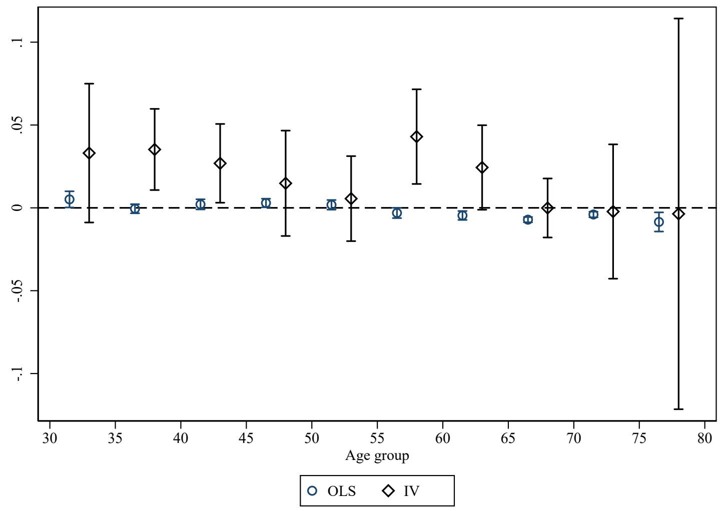
ing representative of the target population and can create barriers to statistical inference (Banks et al., 2011; Deng et al., 2013). Although the surveys we use are constantly updated, selective attrition (e.g. due to mortality) by educational status can be a problem.

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

[4] We 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.

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 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.

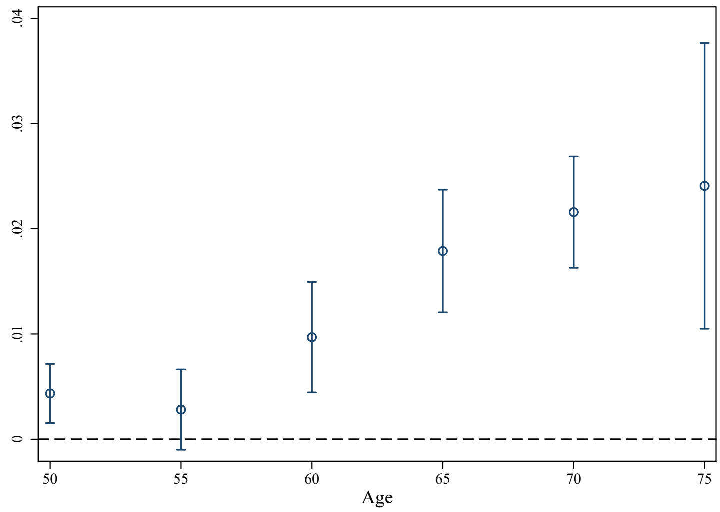
Figure 4: Effect of education on attrition over the life-cycle



Notes: Own calculations based on SOEP, SHARE, Microcensus, and NEPS. 95% confidence intervals. Standard errors clustered at state $\times$ year of birth. Circles between 30 and 35 stand for age group 30-34, and so on.

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 or 75. 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. 95 % confidence intervals.

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 (2008) 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 (Kamhöfer and Schmitz, 2016). 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 (Marmot, 2004; Erikson, 2006; Burgard and Lin, 2013; Darin-Mattsson et al., 2017).

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 (2011) is larger than five and as less demanding if it is less or equal to five, as done by

Mazzonna and Peracchi (2017). 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.

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***	0.008
			(0.004)	(0.041)
Physically less demanding job	54 203	0.53	0.106***	−0.006
			(0.005)	(0.054)
Psychosocially less demanding job	53 718	0.49	0.029***	−0.025
			(0.007)	(0.065)
Manual work	68 062	0.34	−0.090***	−0.002
			(0.003)	(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×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 79, several decades after education took place. This allows to scrutinize a pattern that may

[5]In 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 (SOEP Group, 2022).

[6]See Table A1 in the Appendix for details.

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 (Kaestner et al., 2020) 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, 2005, Galama and van Kippersluis, 2019).

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. Most point estimates are practically zero. One exception is the probability of assessing the own health as poor in the highest age group (75-79). While the estimate is not statistically significant, it shows a sizable reduction for this age group. We believe that this estimate is a statistical outlier, as the sample sizes in the oldest age groups are much smaller compared to the others.

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. (2019) 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 (e.g., Quis, 2018, Marcus et al., 2020), 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.

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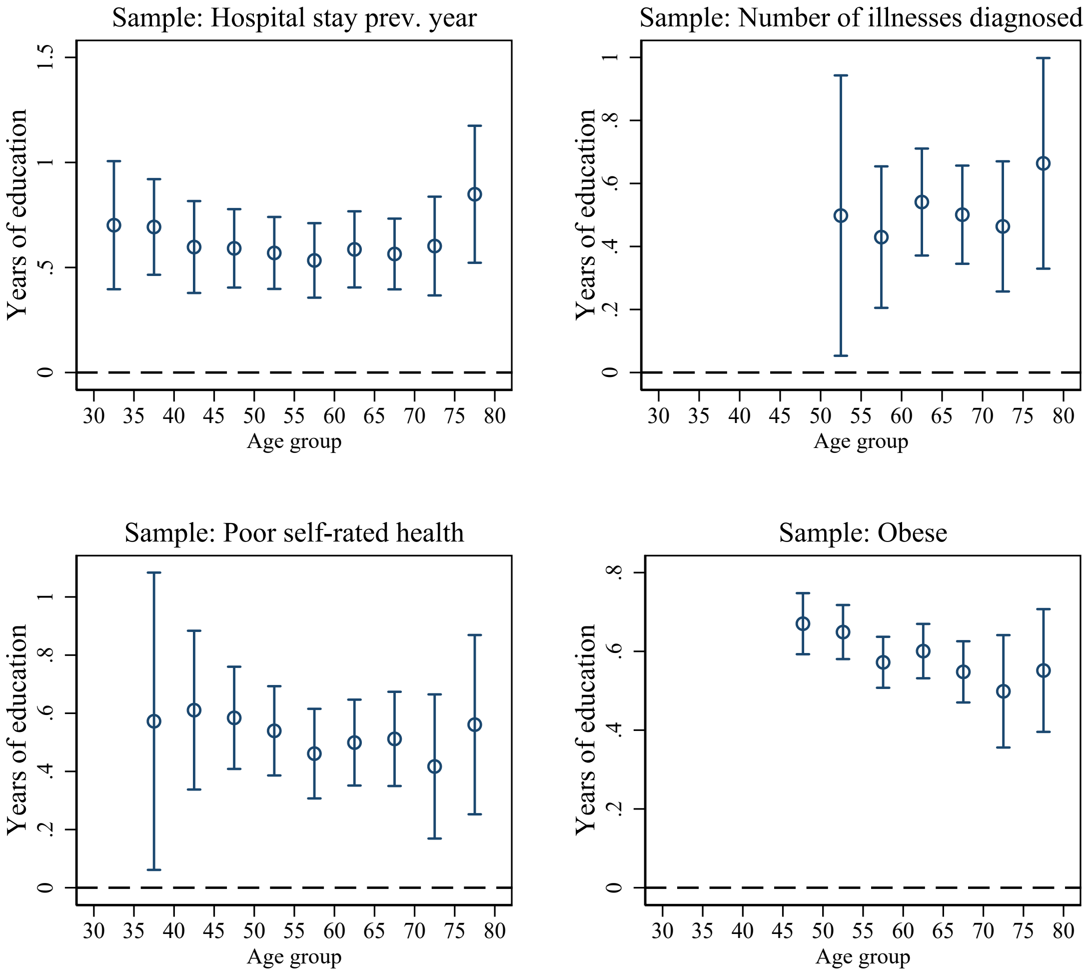
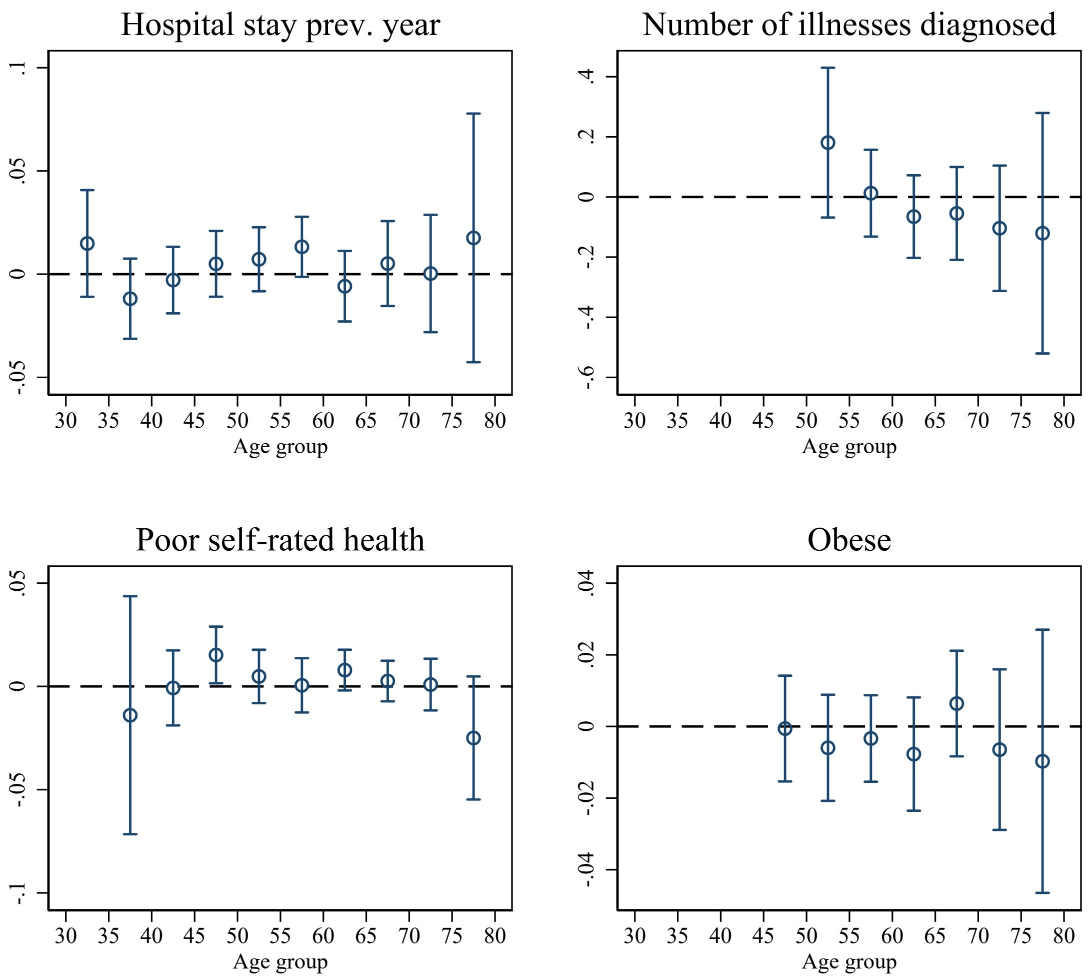
Appendix: Additional tables and figures

Figure A1: First stages



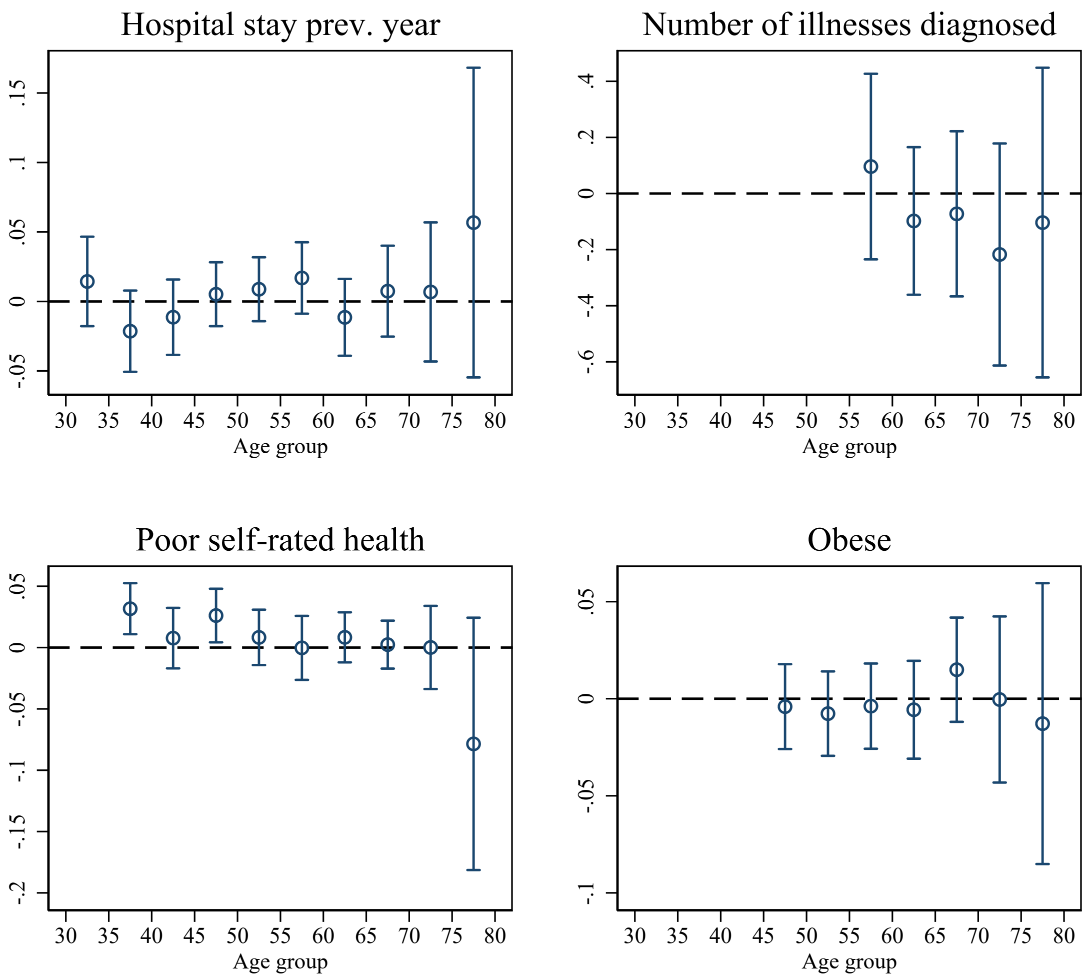
Notes: Own calculations based on SOEP, SHARE, Microcensus, and NEPS. Point estimates of the coefficients of $Reform \times agegroup$ in the first stage regressions of Eq. (2). 95% confidence intervals. Standard errors clustered at state × year of birth. Circles between 30 and 35 stand for age group 30-34, and so on.

Figure A2: Reduced form effects



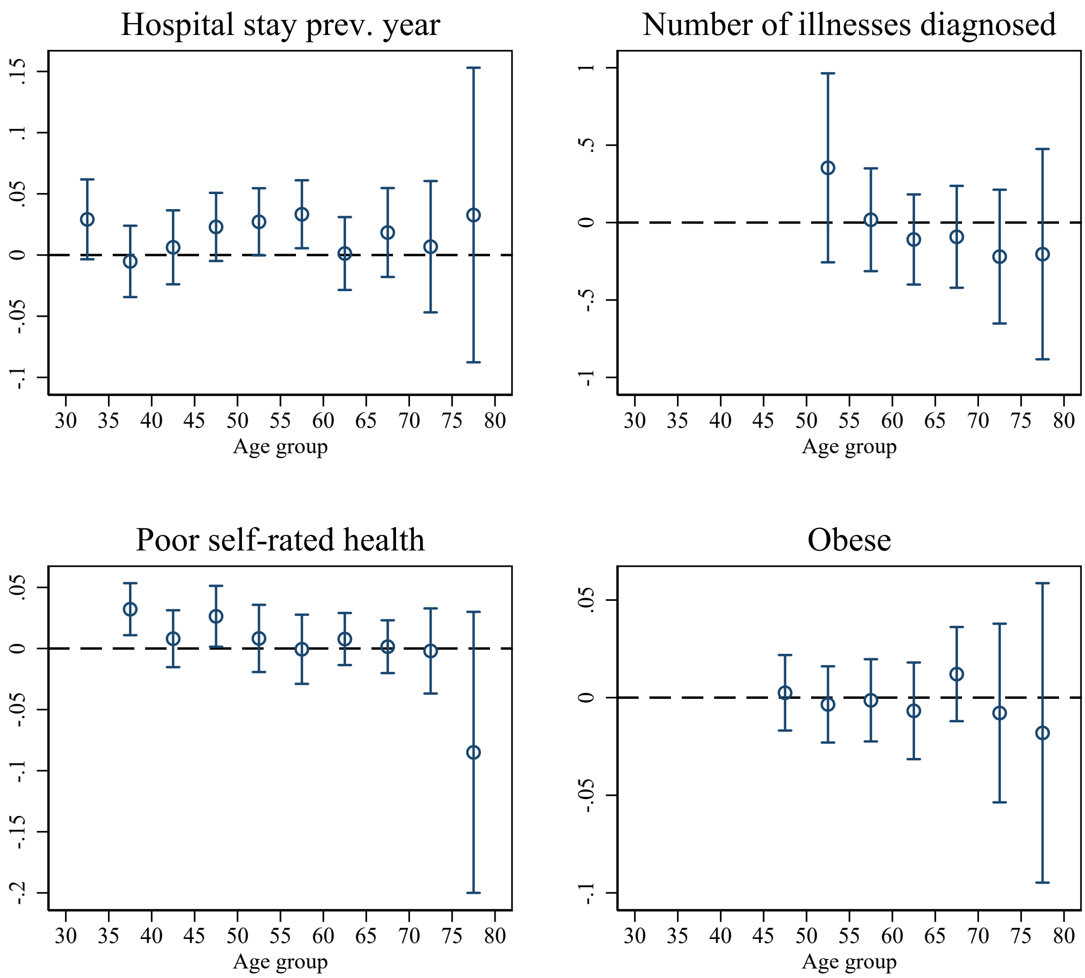
Notes: This figure shows reduced form effects, i.e., the direct effect of compulsory schooling reforms on four health outcomes for up to ten age groups. We visualize coefficients of interactions between the instrument (a dummy for being born in or after the key cohort of a compulsory schooling reform) and the age group. Some age groups are missing due to data limitations.

Figure A3: Robustness – no state×cohort trends



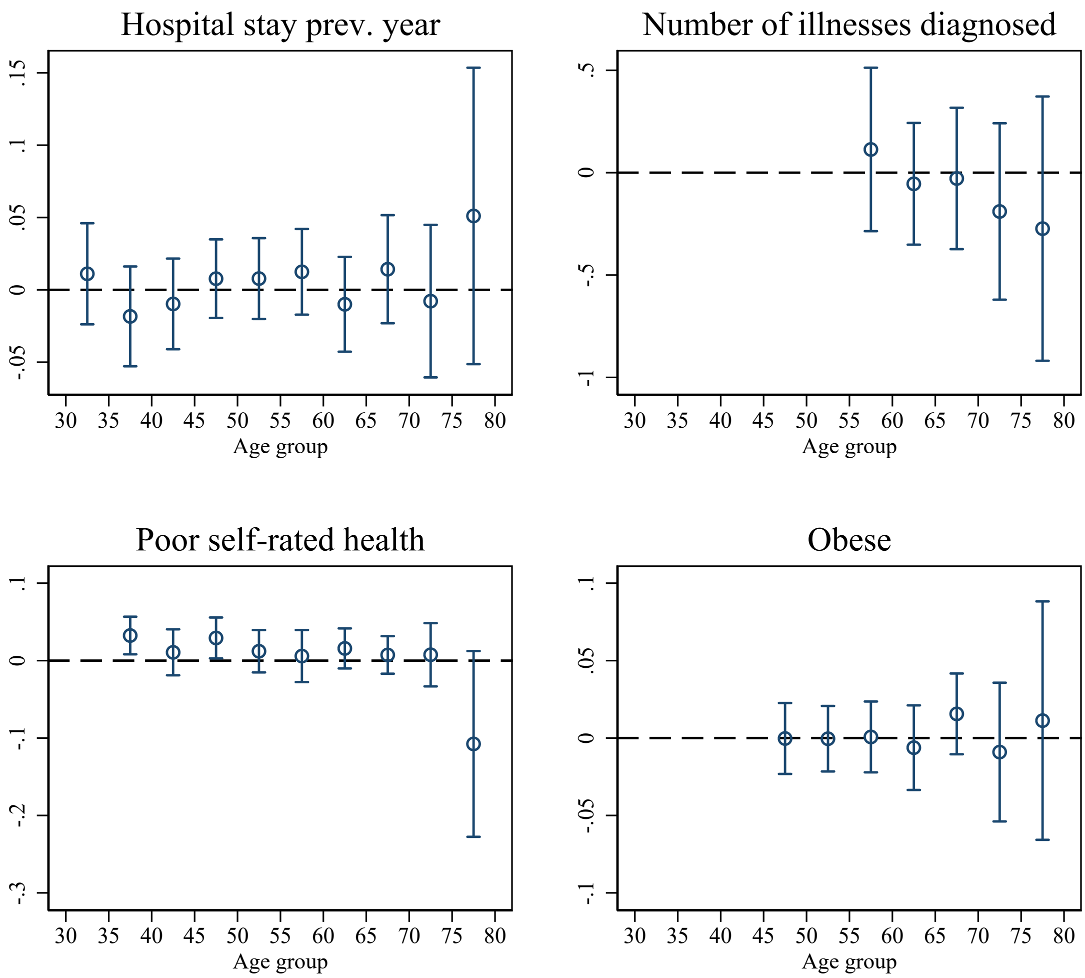
Notes: This figure shows a robustness check of our main results. We estimate the causal effect of years of schooling on four health outcomes across up to ten age groups. Here, we omit state-specific cohort trends that we usually control for. We visualize the 2SLS versions of the point estimates of coefficients 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. Some age groups are missing due to data limitations.

Figure A4: Robustness - Controlling for short school years



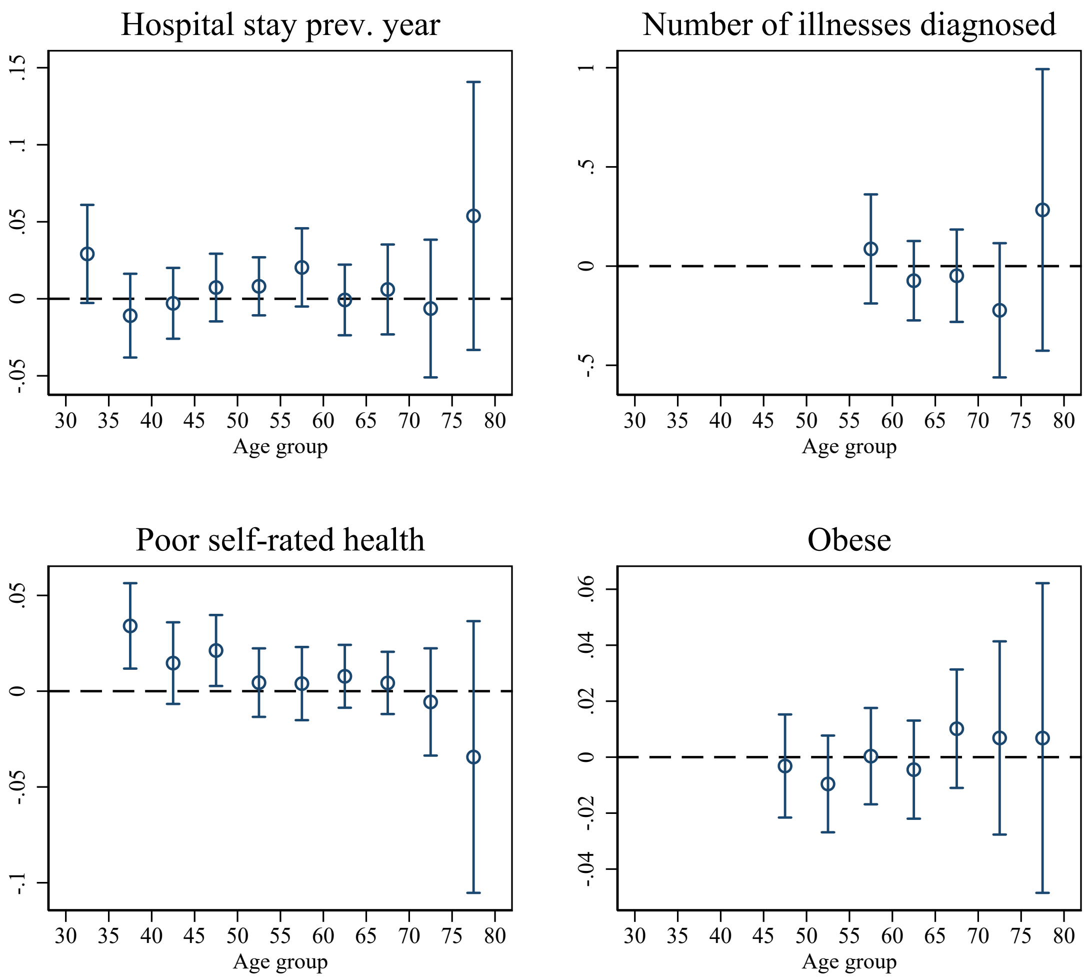
Notes: This figure shows a robustness check of our main results. We estimate the causal effect of years of schooling on four health outcomes across up to ten age groups but add an indicator variable for cohorts who were exposed to short school years. Between 1966 and 1967, the introduction of two short 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. Some age groups are missing due to data limitations.

Figure A5: Robustness – Bandwidth of 4 years



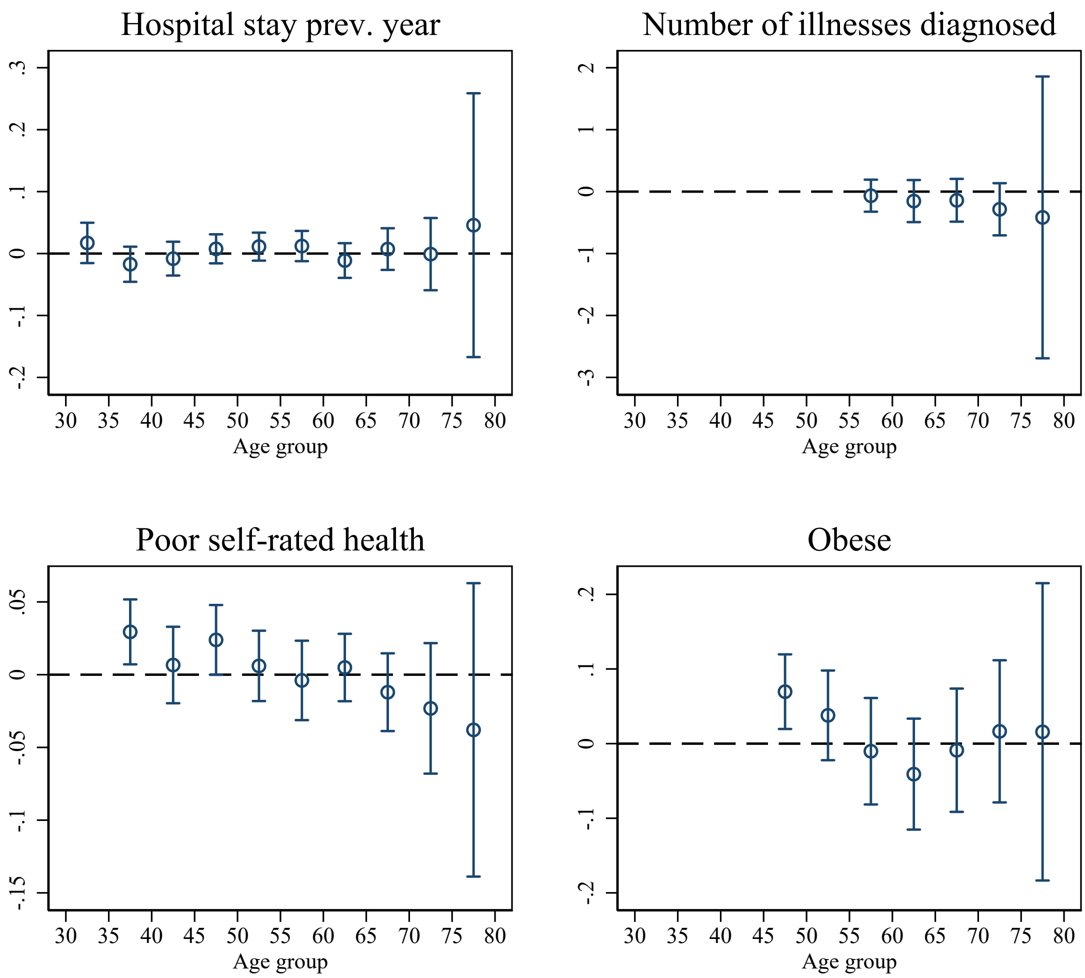
Notes: This figure shows a robustness check of our main results. We estimate the causal effect of years of schooling on four health outcomes across up to ten age groups. Here, instead of selecting our sample to be ± 5 cohorts around each pivotal cohort of the education reform, we use ± 4. We visualize 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. Some age groups are missing due to data limitations.

Figure A6: 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 ten 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. Some age groups are missing due to data limitations.

Figure A7: 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 ten 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. Some age groups are missing due to data limitations.

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 (2022). *Notes:* Own grouping for manual/non-manual and calculation based on SOEP and NEPS.