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Working Paper

Life-cycle health effects of compulsory schooling

Ruhr Economic Papers, No. 1006

Provided in Cooperation with:

RWI – Leibniz-Institut für Wirtschaftsforschung, Essen

Suggested Citation: Schmitz, Hendrik; Tawiah, Beatrice Baaba (2023) : Life-cycle health effects of compulsory schooling, Ruhr Economic Papers, No. 1006, ISBN 978-3-96973-172-7, RWI - Leibniz-Institut für Wirtschaftsforschung, Essen, <https://doi.org/10.4419/96973172>

This Version is available at:

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**Life-Cycle Health Effects
of Compulsory Schooling**

Imprint

Ruhr Economic Papers

Published by

RWI – Leibniz-Institut für Wirtschaftsforschung
Hohenzollernstr. 1-3, 45128 Essen, Germany

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Ruhr Economic Papers #1006

Responsible Editor: Ansgar Wübker

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ISSN 1864-4872 (online) – ISBN 978-3-96973-172-7

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Bibliografische Informationen der Deutschen Nationalbibliothek

The Deutsche Nationalbibliothek lists this publication in the Deutsche Nationalbibliografie;
detailed bibliographic data are available on the Internet at <http://dnb.dnb.de>

RWI is funded by the Federal Government and the federal state of North Rhine-Westphalia.

<http://dx.doi.org/10.4419/96973172>

ISSN 1864-4872 (online)

ISBN 978-3-96973-172-7

Hendrik Schmitz and Beatrice Baaba Tawiah¹

Life-Cycle Health Effects of Compulsory Schooling

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 in Germany, using compulsory schooling reforms as a source of exogenous variation. Our results suggest a positive correlation of health and education which increases over the life-cycle. We do not, however, find any positive local average treatment effects of an additional year of schooling on health or health care utilization for individuals up to age 79. An exception is obesity, where positive effects of schooling start to be visible around age 60 and become very large in age group 75-79. The results in age group 75-79 need to be interpreted with caution, however, due to small sample size and possible problems of attrition.

JEL-Code: I10, I12, I21

Keywords: Education; health; life-cycle effects; compulsory schooling

March 2023

¹ Hendrik Schmitz, Paderborn University, RWI Essen and Leibniz Science Campus Ruhr; Beatrice Baaba Tawiah, Paderborn University. – We are grateful to Daniel Kamhöfer, Valentin Schiele and Matthias Westphal for very helpful comments. Detailed data acknowledgements at the end of the text. – All correspondence to: Hendrik Schmitz, Paderborn University, Warburger Strasse 100, 33098 Paderborn, Germany; Email: hendrik.schmitz@uni-paderborn.de

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 22 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 which is blurred, however, 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 main 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) and the German National Educational Panel Study (NEPS). While our data set clearly has disadvantages compared to administrative data in some aspects, its main advantage is that it covers a 36 year-period and allows to follow the individuals born around the reform periods over many decades and estimate both short-run and long-run effects of education on health within the same framework and data set. This allows us to learn about the point in the life-cycle when potential health effects of education set in. Estimating these health effects on 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 of education	Instrument	Studied age group	Results
Adams (2002)	USA	Secondary school	Quarter of birth	51 to 61	Positive effects
Arendt (2005)	Denmark	Middle school	CSR	25 to 64	No effects
Lleras-Muney (2005)	USA				
Oreopoulos (2006)	UK	Secondary school	CSR	32 to 64	Positive effects
Albouy and Lequien (2009)	France	Secondary school	CSR	48 to 80	No effects
Silles (2009)	UK	Secondary school	CSR	25 to 60	Positive effects
Kemptner et al. (2011)	Germany	Secondary school	CSR	16 to 65	No effects - women Positive effects - men
Braakmann (2011)	UK	Secondary school	Being born February-	28 to 45	No effects
Lager and Torssander (2012)	Sweden	Various types	CSR	15 to 64	No effects
Clark and Royer (2013)	UK	Secondary school	CSR	12 to 74	No effects
Jürges et al. (2013)	UK	Secondary school	CSR	32-53 + 44-77	No effects
Gathmann et al. (2015)	Europe	Secondary school	CSR	50+	No effects - women Positive effects - men
Palme and Simeonova (2015)	Sweden	Secondary school	CSR	28 to 66	Negative effects
Buckles et al. (2016)	USA	College	Vietnam War draft	28 to 65	Positive effects
Brunello et al. (2016)	Europe	Secondary school	CSR	50+	Positive effects
Meghir et al. (2018)	Sweden	Secondary school	CSR	16 to 75	No effects
Davies et al. (2018)	UK	Secondary school	CSR	37 to 74	Positive effects
Kamhöfer et al. (2019)	Germany	College	Expansions in college availability	39 to 68	No effects - mental health Positive effects - physical health
Dahmann and Schnitzlein (2019)	Germany	Secondary school	CSR	50 to 85	No effects
Janke et al. (2020)	UK	Secondary school	CSR	42 to 60	No effects (except for diabetes)
Fischer et al. (2021)	Sweden	Secondary school	CSR	18 to 81	Positive effects
Begerow and Jürges (2022)	Germany	Secondary school	CSR	50 to 79	No effects

Notes: Own research of studies without the claim of completeness. CSR stands for compulsory schooling reforms. 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

¹Bhuller 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 suggest a positive correlation of health and education which increases over the life-cycle. For example, one more year of schooling goes along with 0.5 more diagnosed conditions for individuals aged 50-54 and 1.5 more diagnoses for individuals aged 75-79. It also goes along with a 0.5 percentage point higher likelihood to report being in poor health for individuals aged 40-44 but 1.5 percentage point higher likelihood for individuals aged 75-79. Thus, we can replicate a common pattern found in the literature. Yet, when looking at the causal relationship, we hardly find any effects for health and health care utilization at all. An exception is obesity, where positive effects of schooling start to be visible around age 60 and become very large in age group 75-79. An ex-post simulated power analysis as suggested by [Black et al. \(2022\)](#) and an analysis of selective panel attrition indicate that attrition and power do not play an important role in our sample until the age of 74. In contrast, the subgroup of 75-79 years old individuals (the oldest in our sample) suffers from small sample size and potential attrition problems. Yet, the point estimates for hospital stays, poor health and diagnoses also point at zero effects in this group. Yet, due to the mentioned problems, this – and the large estimated effect on obesity – should be interpreted with caution.

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, carry out a power analysis 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 three sources. 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 (Goebel et al., 2019). For our analysis we use SOEP version 37 containing yearly information from 1984 to 2020 (SOEP, 2022). In order to increase the number of observations, we augment our baseline sample with observations from the Survey of Health Ageing, and Retirement (SHARE) and the German National Educational Panel Study (NEPS): Starting Cohort Adults data (NEPS Network., 2022). 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 4-8 but not wave 3 (SHARELIFE) which considers different topics that are not of interest here (Börsch-Supan, 2020a,b,c,d,e,f,g, 2021; Börsch-Supan et al., 2013). 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). We consider all 12 waves of the NEPS from 2007 to 2020.

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 94 for a person from Hamburg, born in 1926, observed in 2020. 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 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.

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

Federal State	Pivotal birth cohort	Reform year	Youngest age in 1984	Oldest age in 2020
Schleswig Holstein	April 1932	April 1947	47	93
Hamburg	April 1931	April 1946	48	94
Lower Saxony	April 1947	April 1962	32	78
Bremen	April 1944	April 1959	35	81
North Rhine-Westphalia	April 1951	April 1966	28	74
Hesse	April 1951	April 1966	28	74
Rhineland Palatinate	April 1952	April 1967	27	73
Baden-Württemberg	April 1952	April 1967	27	73
Bavaria	August 1954	August 1969	25	71
Saarland	April 1943	April 1958	36	82

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.

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 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, 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 data.

Table 3: Number of observations

Age group	Hospital		Poor health		Diagnoses		Obese	
	Obs	(% SOEP / %SHARE / % NEPS)	Obs	(% SOEP / %SHARE / % NEPS)	Obs	(% SOEP / %SHARE / % NEPS)	Obs	(% SOEP / %SHARE / % NEPS)
30	4693	(100 / 0 / 0)						
35	6776	(100 / 0 / 0)	1674	(100 / 0 / 0)				
40	7273	(100 / 0 / 0)	5601	(100 / 0 / 0)				
45	10859	(100 / 0 / 0)	10756	(100 / 0 / 0)			2618	(99 / 1 / 0)
50	14206	(95 / 5 / 0)	15290	(87 / 5 / 8)	1427	(48 / 52 / 0)	6244	(85 / 12 / 3)
55	15225	(90 / 10 / 0)	21355	(64 / 7 / 29)	4788	(68 / 32 / 0)	9063	(73 / 17 / 10)
60	14787	(86 / 14 / 0)	23272	(55 / 9 / 36)	7592	(73 / 27 / 0)	9559	(64 / 22 / 14)
65	9491	(85 / 15 / 0)	13695	(59 / 11 / 30)	5054	(71 / 29 / 0)	6359	(65 / 23 / 12)
70	2857	(87 / 13 / 0)	3627	(69 / 10 / 21)	1271	(71 / 29 / 0)	2079	(67 / 17 / 16)
75	502	(73 / 27 / 0)	507	(72 / 27 / 1)	247	(44 / 56 / 0)	331	(57 / 42 / 1)

Notes: Own calculations based on SOEP, SHARE, and NEPS. Age group 30 stands for age group 30-34, age group 35 stands for age group 35-39, and so on. These are the observations in the final selected sample that enter the regressions below.

Table 4: Descriptive statistics

	Mean	SD	Min.	Max.	Observations	Survey years
<i>Outcome variables</i>						
Hospital stay (yes = 1, no = 0)	.124	.33	0	1	86,669	1984 - 2020 ^a
Poor self-rated health (yes = 1, no = 0)	.036	.185	0	1	95,827	1992 - 2020
# Diagnoses	1.616	1.568	0	11	20,418	2009 - 2020 ^b
Obese (yes = 1, no = 0)	.208	.406	0	1	36,382	2002 - 2020
<i>Treatment and instrument</i>						
Years of schooling	10.411	1.814	8	13	86,669	
Reform	.606	.489	0	1	86,669	
<i>Other information</i>						
Age	57.159	8.049	30	79	86,669	
Female	.505	.5	0	1	86,669	

Notes: Own calculations based on SOEP, SHARE, and NEPS. The statistics for age, female, years of schooling and reform are based on the estimation sample for hospital stay. Hospital stay is an indicator variable for whether an individual was admitted at a hospital for at least one night the previous year. Obese is a binary indicator of having a BMI > 30. Poor self-rated health is a binary indicator of checking the lowest of five possible categories in self-rated health. ^aNo data for hospital stay in 1990 and 1993 in the sample. ^bbiennial.

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 self-rated health having the largest sample (95,827 observations from 13,618 individuals). The smallest sample has 20,418 observations from 6,799 individuals. 12.4% of the observations stayed at least one night in the hospital the previous year. The maximum number of diagnoses in the sample is 11 out of the 13 options mentioned above. There is an average of about 1.6 illnesses being diagnosed and 21% are obese, while almost 4% state that they are in poor health. The average age is 57 years and the sample is almost gender balanced. The average years of schooling is about 10.4 years.

2.3 OLS estimations

As another descriptive statistic, we present results of OLS regressions of the following form:

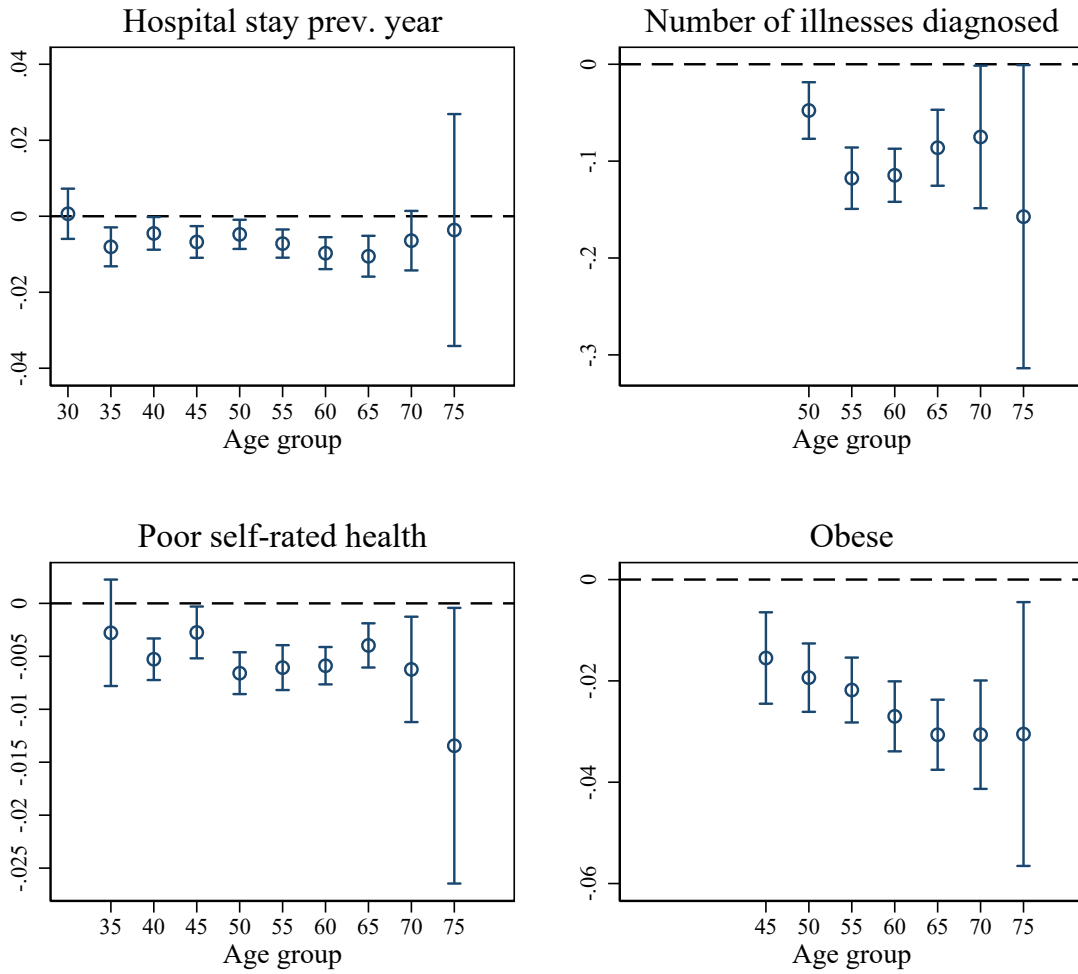
$$H_{ist} = \sum_g \beta_g Yed_{is} \times agegroup_{it} + \beta X_{ist} + \varepsilon_{ist} \quad (1)$$

where H_{ist} is a health outcome of individual i who attended school in state s . Yed_{is} is an individual's number of years of schooling. To flexibly account for the correlation of schooling and health, we define 5-year age brackets, denoted $agegroup_{it}$, as follows: 30-34, 35-39, 40-45, ..., 75-79.² The vector X includes a constant, a full set of age dummies (in years), federal state dummies, female indicator, survey as well as interview year dummies, and state-specific time trends, i.e. interactions of school state dummies with a linear trend in year of birth. We cluster standard errors on state \times year of birth level. The coefficients of interest are the β_g , they are reported in Figure 1.

The results show a positive relationship between education and health over the life-cycle. With small exceptions, an additional year of schooling is related to better health throughout the life-cycle. Broadly, one more year of education is related to a 10% lower number in the measure of negative health, when we compare the coefficients with the sample means in Table 4. That is, for example, the coefficient for hospital visits is around -0.01 while the sample mean is around 0.1. For all measures but hospital stay, the health-gap in education widens over the life-cycle, where the estimated coefficients for age group 75-79 are two to three times larger than those for the youngest age groups.

²Age groups start with 35-39 for poor health, with 45-49 for obesity, and with 50-54 for diagnoses because they are only covered later in the data.

Figure 1: OLS results



Notes: Own calculations based on SOEP, SHARE, and NEPS. Point estimates of the coefficients β_g based on Eq. (1) with 95 % confidence intervals. Standard errors clustered at state \times year of birth. Controls: age fixed effects, state fixed effects, survey fixed effects, survey year fixed effects, female and state-specific linear trends. Age 30 in the figure stands for age group 30-34, age 35 stands for age group 35-39, and so on.

3 Instrumental variables estimations by age group

3.1 Empirical Strategy

We run the two-stage-least-squares (2SLS) equivalent to regression equation (1) where we instrument $Yed_{is} \times agegroup_{it}$ with $Reform_{is} \times agegroup_{it}$ for the age groups used above in Section 2.3. $Reform_{is}$ is an indicator variable for whether an individual was affected by the reform or not. If certain assumptions hold, the estimated coefficients of the instrumented $Yed_{is} \times agegroup_{it}$ identify the local average treatment effect of education on health for the different age groups, that is, the effects for those individuals who increase years of

schooling solely because they are forced to do so due to the reform. Given that there are no never-takers of the reform, this group of compliers is composed of individuals at the lowest margin of willingness to take education.

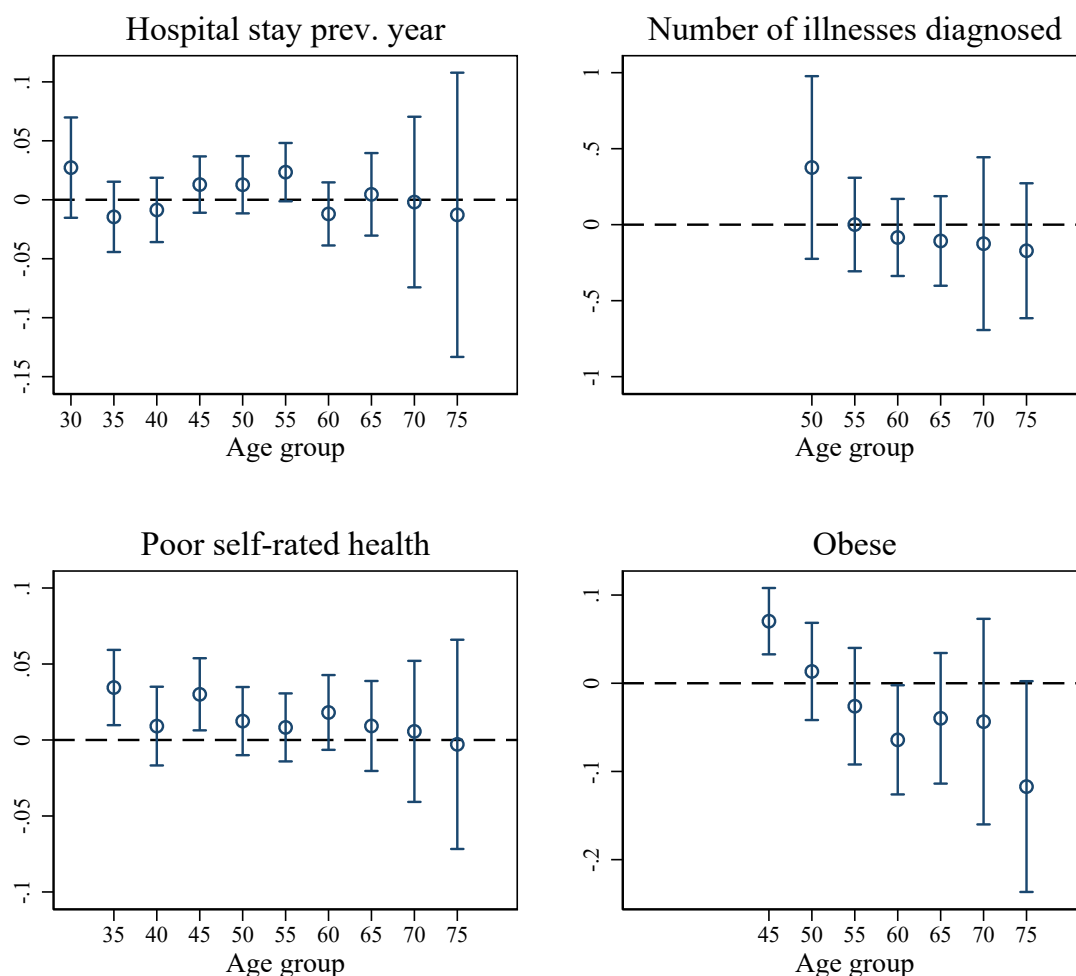
To interpret the coefficients causally, the assumptions for instrumental variables need to be fulfilled. First, the instrument needs to be exogenous. This is fulfilled if all other changes that occur across states prior to reform are uncorrelated with the law change itself and the outcomes given controls. The inclusion of state-specific time trends helps to deal with any factors that affected states over time. Secondly, the exclusion restriction needs to hold. Given how large the reform was, this is not completely obvious. While it is conceivable that the reform may have had effects on health through other channels than education, there is no evidence for this so far and this assumption is standard in the literature.

Third, the instrument needs to be correlated with the endogenous variable, that is, the reform should have a significant effect on the number of years of schooling. While not shown in a separate table, the reform increased the average years of schooling by around 0.5 in our data with an F-statistic on the excluded instrument of around 35. These results are in line with the many other studies that evaluate these reforms in Germany (e.g. [Kemptner et al., 2011](#), or [Kamhöfer and Schmitz, 2016](#)). Finally, when allowing for potential effect heterogeneity, the monotonicity assumption needs to hold in order to be able to interpret the results for a well-defined subgroup, namely the compliers. This assumption means that no individual would reduce the years of schooling due to the reform. Again, we follow the vast previous literature and assume that individuals with high educational attainment do not attain less schooling due to an increase in mandatory years in the basic track.

3.2 Estimation results

The main results using instrumental variables estimations are shown in Figure 2. For the three outcomes hospital stay, poor self-rated health and diagnoses, the estimated age-group-specific effects fluctuate around zero throughout the life-cycle. While being a bit noisy, they clearly provide evidence that the correlation between health and education from Figure 1, which even increases over the life-cycle, is unlikely to be due to a causal effect of education. While precision is an obvious issue given our sample size, the coefficients are all close to zero. An exception is obesity, where the effect of schooling is positive in the age group 45-49 with a five percentage point increase in obesity due to more schooling. Over the life-cycle this turns negative, resulting in a 4 to 8 percentage point lower likelihood to be obese for individuals in the range 60-74 (significant in the age group 60-64 only), and a more than ten percentage point lower likelihood to be obese due to an additional year of schooling in the age group 75-79 (only significant at the 10 percent level).

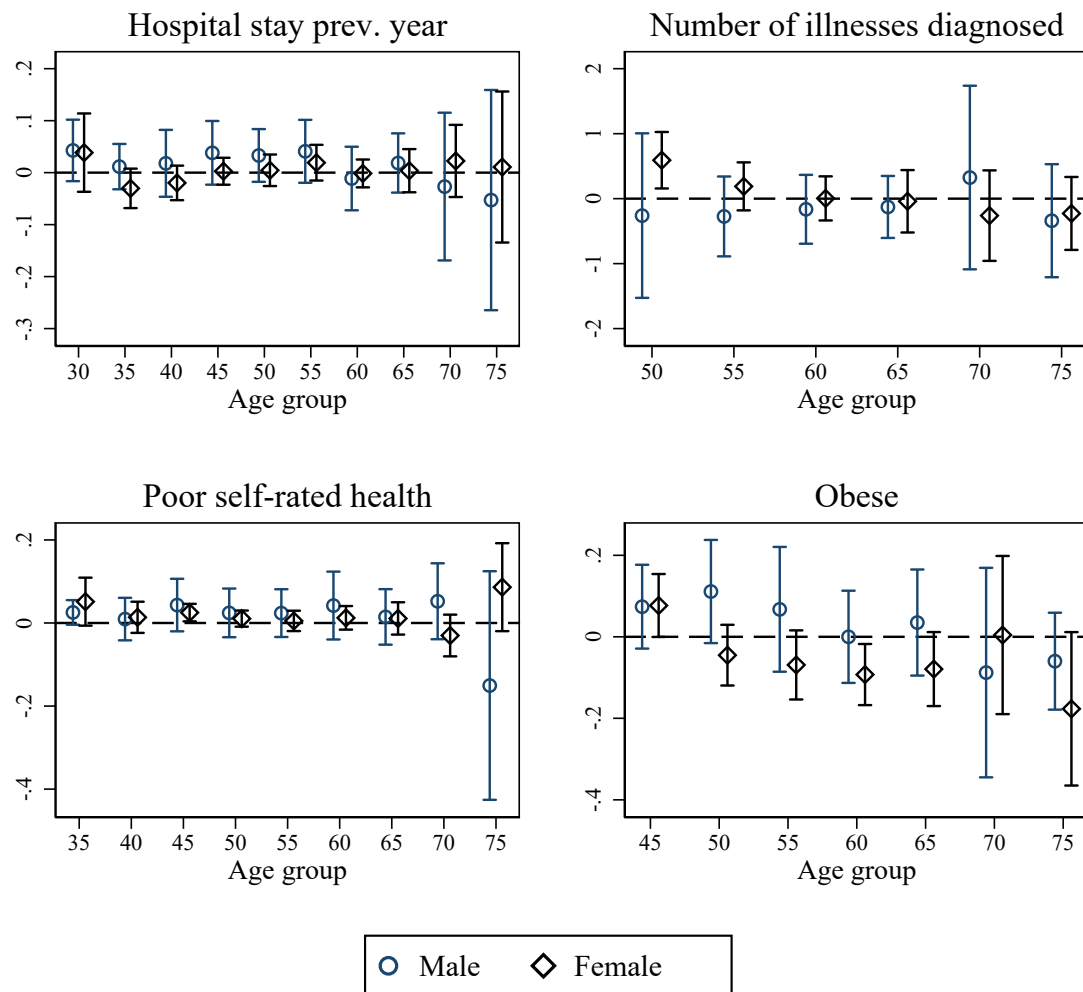
Figure 2: Instrumental variables estimations: Baseline results



Notes: Own calculations based on SOEP, SHARE, and NEPS. Point estimates of the coefficients β_g based on 2SLS versions of Eq. (1), 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. Controls: age fixed effects, state fixed effects, survey fixed effects, survey year fixed effects, female, and state-specific linear trends. Age 30 in the figure stands for age group 30-34, age 35 stands for age group 35-39, and so on.

In Figure 3 we repeat the analysis separately by gender. We do not find a structural difference in results but note that the negative effect on obesity seems to be driven by women. In summary, the main finding of the paper is the following: individuals with more schooling are in better health and the health gap by education increases over time. However, there is hardly any local average treatment effect of additional schooling for individuals at low education margins. Up to the age of 79, we do not observe an improvement in health due to education. In that sense, the results are in line with those of [Clark and Royer \(2013\)](#), although they use different data and outcome variables. They are in contrast, however to some of the findings by [Gehrsitz and Williams Jr \(2022\)](#). An exception

Figure 3: Heterogeneous effects by gender



Notes: Own calculations based on SOEP, SHARE, and NEPS. Point estimates of the coefficients β_g based on 2SLS versions of Eq. (1), 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. Controls: age fixed effects, state fixed effects, survey fixed effects, survey year fixed effects, and state-specific linear trends. Age 30 in the figure stands for age group 30-34, age 35 stands for age group 35-39, and so on.

is obesity which seems to decrease over the life-cycle due to education. However, this relationship is estimated with low precision only.

Robustness checks

We conduct different robustness checks and report their results in the Appendix. In Figure A1 we repeat the baseline estimation of Figure 2 but do not additionally account for state-specific trends. In Figure A2 we account for short school years (SSY). In 1966 – 1967, there was the introduction of two short school years in all states in West Germany except Bavaria. The start of the school year moved from spring to fall but it was already in fall

for Bavaria, see [Pischke \(2007\)](#) for details. This was achieved in most states through two SSY with 24 weeks instead of the regular 37 weeks of instruction each. The introduction of the SSY occurred simultaneously with the compulsory schooling reform in some states. Therefore, the SSY is a possible confounding factor indicating our results may be biased with its omission. For the estimation, we include an indicator variable for SSY in the 2SLS regressions.

In [Figure A3](#) we make a different sample selection. Instead of five years around the pivotal cohort in each state we use all birth cohorts from 1930 to 1960. Finally, in [Figure A4](#), we only use SOEP as a data source. The results in the robustness checks are fairly similar to those in the baseline specification.

3.3 A simulated ex-post power analysis

Findings close to zero for a relevant share of the population, together with a comparably small sample size in the age groups and larger standard errors raise the question of statistical power. For instance, how likely is it that the true effect is economically large but that we fail to identify it given our sample? That is, how likely are we to make a type-II-error?³ In order to receive estimates of statistical power and minimum detectable effect sizes (MDE) in our data and application, we follow the simulation-based approach suggested by [Black et al. \(2022\)](#). In the spirit of their approach, we search for the minimum effect size that has 80% power at the 5% significance level, meaning that – if this was indeed the true effect size – in 80% of all cases we would reject the hypothesis of no effect at the 5% significance level.

As an exemplary procedure we choose the estimation using the outcome variable *obesity* where small to no effects are found for age groups below 75 and significant effects at the 10 percent level are found for age group 75-79. To simplify matters, instead of estimating the two stage least squares regressions resulting in [Figure 2](#), we, in this section, estimate the reduced form-relationship. That is, we estimate the direct effect of being affected by the reform on the indicator of obesity by running this regression:

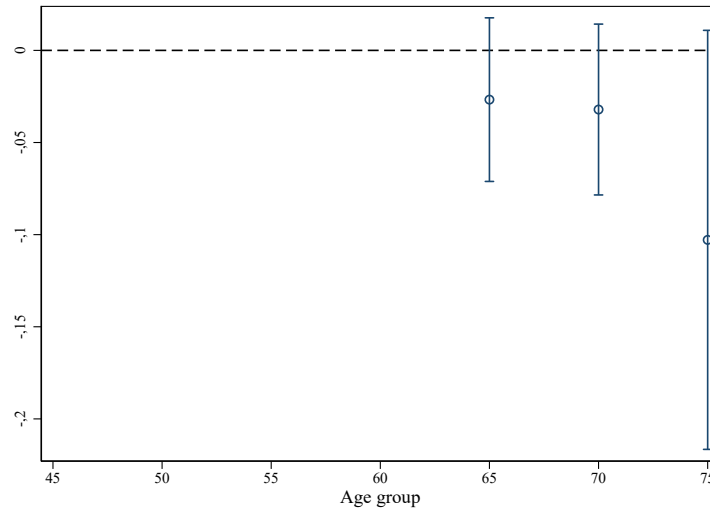
$$obese_{ist} = \sum_g \beta_g reform_{is} \times agegroup_{it} + \beta X_{ist} + \varepsilon_{ist} \quad (2)$$

The reason is that, in the simulated power analysis described below, we randomly assign the treatment (being affected by the reform). By only considering the reduced form, we do not need to make further assumptions on how individuals react to the reform, that is, whether they are compliers or always takers. Provided that the first stage coefficient

³The description of our procedure takes a lot from [Freise et al. \(2022\)](#).

is large, the reduced form results resemble the two stage-least squares results anyway. Finally, we focus on the 8,769 observations in the three age groups 65-69, 70-75, and 75-79. As seen in Figure 4, the reduced form results for this group are in line with the 2SLS results from Figure 2.

Figure 4: Reduced form regression



Notes: Own calculations based on SOEP, SHARE, and NEPS. Point estimates of the coefficients β_g with 95 % confidence intervals. Standard errors clustered at state \times year of birth. Number of observations: 8,769. The regression equation is: $Obese_{ist} = \sum_g \beta_g reform_{is} \times agegroup_{it} + \beta X_{ist} + \varepsilon_{ist}$. Controls: age fixed effects, state fixed effects, survey fixed effects, survey year fixed effects, female, and state-specific linear trends. Age 65 in the figure stands for age group 65-69, age 70 stands for age group 70-75, and so on.

Our procedure to get estimates of the three minimum detectable effect sizes for the three age groups is the following:

1. Use the baseline sample and drop all observations that are affected by the reform, that is, all who are younger or equal to the pivotal cohorts. Thus, only use observations not treated to make sure that possible real treatment effects do not affect our results. This means, from the 8,769 observations between 65 and 79 with information on obesity and who are born ± 5 years around the pivotal cohort, we drop 3,212 and keep 5,557 observations.
2. Refill the sample to get a sample size of 8,769 such that in each state we have the exact same number of observations per age group as before. We do this in two ways. First, we take data from individuals in the state born between -6 to -10 years relative to the pivotal cohort. For the remaining observations, we oversample untreated individuals.
3. Randomly select individuals of the sample created in step 2 and assign them the treatment (*reform*) such that the share of treated individuals per federal state is the same as in the original sample.

4. Assign the treated individuals a uniform and constant effect of X which is added to their measure of obesity.
5. Estimate Eq. (2) and check whether the estimated coefficients of the treatment effects in the three age groups is significant at the 10% level, that is, whether or not we made a type-II-error (fail to reject the null hypothesis of a zero effect although we know that the true effect is $X \neq 0$).
6. Repeat steps 3 to 5 1,000 times and count the share of significant treatment effect estimates.
7. Repeat step 6 80 times where the imposed treatment effect X is gradually increased from -0.002 to -0.160 in steps of 0.002.

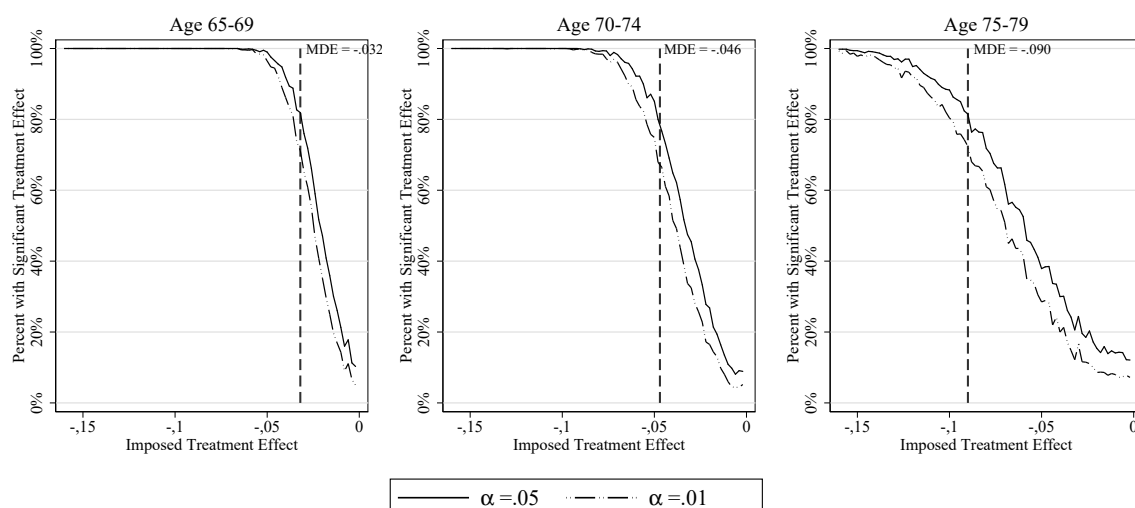
Figure 5 reports the results of this exercise where for each of the 80 imposed treatment effects the share of significant estimates in 1,000 repetitions is shown. The figure reports results for two different significance levels. The minimum detectable effect size is defined to be where the 5% significance curve shows 80% power. This is at -0.034 in age group 65-69. This means: if the true effect was -0.032, our data would allow for an analysis with a power of 80%. The minimum detectable effect size for age group 70-74 is -0.046, while it is -0.090 for age group 75-79. The minimum detectable effect sizes for the three age groups are in the same range as our estimated effects. We interpret these findings as evidence that power-problems do not rule out a useful analysis by age groups given our data and that the estimated effects in Figure 4 seem to be well-powered. Yet, it should be noted the MDE in the age group 75-79 is very large. This works in the example of obesity, where indeed our point estimate is in this range. However, the MDEs in that age group for the three other outcomes are large, too (not shown). Hence, we cannot rule out that actual health effects on education are considerable in this age group. All in all, we conclude that our analysis yields reliable results for individuals up to age 74 but the results need to be interpreted with caution for the group 75-79.

3.4 Attrition

A major concern with longitudinal household surveys, especially those focused on the older population and where the interest is health, is the potential bias from attrition (Banks et al., 2011; Deng et al., 2013; Fichera and Savage, 2015). Attrition hinders a survey from being representative of the target population and, hence, introduces potentially substantial biases to statistical inference (Banks et al., 2011; Deng et al., 2013). Even though the surveys we use are constantly being refreshed, selective attrition (possibly due to mortality) by educational status might be an issue.

We apply two complementary approaches to test for potential attrition problems. In the first approach, we use the data from our working sample and generate a binary indicator

Figure 5: Simulated power analysis

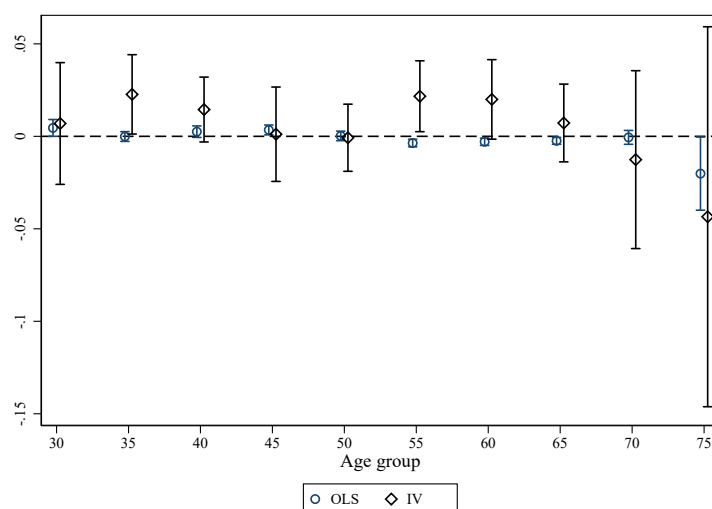


Notes: Own calculations, based on the procedure suggested by Black et al. (2022). Detailed simulation procedure described in the text.

attrition. This indicator equals one if a person does not appear in the next survey wave and zero if she either appears in the next wave or if it is the last wave (year 2020) in the survey. We generate this indicator before we make the sample selection based on the pivotal cohort. According to this definition, 25 percent of all person-year-observations in our sample drop out between two waves. Next, we use *attrition* as an outcome and run an OLS and IV regressions as before. Figure 6 shows the results of this exercise. Until age group 70-74, there is hardly any difference in attrition by education. This is different for age group 75-79 where one more year of education goes along with a marginally significant two percentage point decrease in attrition (OLS, five percent in IV, but not significant). This difference is small, however.

As a second approach, we keep each individual only once in the sample and ask for the likelihood to (still) be in the sample at ages 50-54, 55-59,...75-79. To give an example, consider a person born in 1930 who drops out in 2000. The indicator *before50* takes on the value one for her, while the indicator *before75* takes on the value zero. An individual born in 1960 would have non-missing values for indicators *before50*, *before55*, and *before60*. As she turns 65 after 2020, the remaining indicators are missing for this person. 86% of all individuals are still in the survey at age 50 (based on 15,094 individuals). This number constantly goes down to 47% who are still in the survey at age 80 (based on 427 individuals). We, then, run separate regressions of all indicators on years of schooling, female, birth year dummies, survey and federal state dummies and state-specific linear trends in birth year. Results are shown in Figure 7. We observe a higher likelihood to stay in the sample with more education. The differences are statistically significant but small in economic terms until (and including) age group 70-74. They are somewhat larger, however,

Figure 6: Effect of education on attrition over the life-cycle



Notes: Own calculations based on SOEP, SHARE, and NEPS. Point estimates of the coefficients β_g based on OLS and 2SLS versions of Eq. (1), 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. Controls: age fixed effects, state fixed effects, survey fixed effects, survey year fixed effects, female, and state-specific linear trends. Age 30 in the figure stands for age group 30-34, age 35 stands for age group 35-39, and so on.

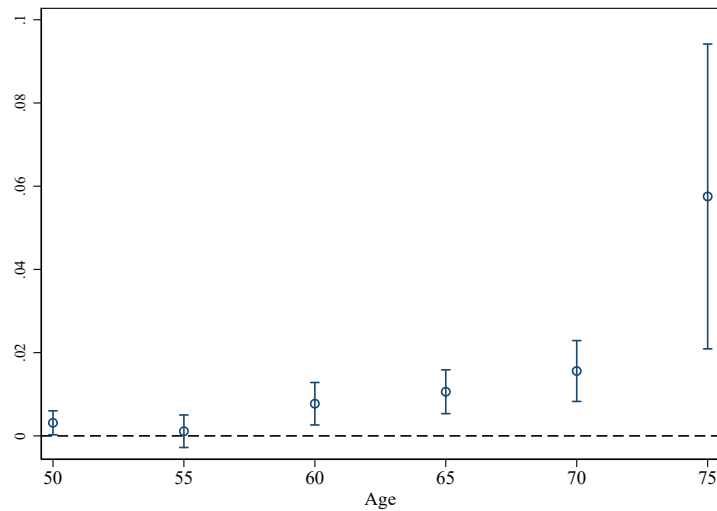
for age group 75-79 where education has a stronger relationship with the likelihood to stay in the sample.

We take these results to draw a similar conclusion as with the power analysis in the previous subsection. While attrition does not seem to play an important role for the results of age groups until 74, the estimated effects for the highest age groups might be subject to selective attrition and, again, should be interpreted cautiously.

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

Figure 7: Effect of education on (still) being in the sample at certain ages



Notes: Own calculations based on SOEP, SHARE, and NEPS. Point estimates of six separate regressions. 50 stands for the coefficient of years of education in a linear regression of *before50* on years of education, female, birth year dummies, survey and federal state dummies and state-specific linear trends in birth year. 95 % confidence intervals. Standard errors clustered at state \times year of birth.

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.⁴ EGP classes I, II, III, IVa, IVb and V are classified as non-manual, and classes VI, VII and IVc as manual.⁵ 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

⁴In 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).

⁵See Table A1 in the Appendix for details.

cent have psychosocially less demanding jobs and 34 per cent are manual workers. OLS estimates show a significant correlation of education and possibly healthier jobs. 2SLS results, however, have coefficients close to zero which are also not statistically significant. It seems that there is no effect of an additional year of compulsory schooling on healthier jobs. This may be part of the explanation why we do not see effects of this reform in the long-run.

Table 5: Potential mechanisms

	Observations	Sample mean	OLS	2SLS
White collar job	53,944	0.67	0.097*** (0.004)	0.013 (0.044)
Physically low jobs	53,816	0.53	0.108*** (0.005)	0.022 (0.058)
Psychosocially low jobs	53,373	0.49	0.030*** (0.007)	-0.017 (0.070)
Manual work	71,049	0.34	-0.090*** (0.003)	-0.007 (0.044)

Note: Own calculations based on SOEP and individuals younger than 66. Point estimates of the coefficient of years of schooling from regressing each outcome on years of schooling, female, age fixed effects, state fixed effects, survey and survey year fixed effects, and state-specific linear trends for OLS, and instrumenting years of schooling with the reform for the 2SLS version. Standard errors clustered at state×year of birth.

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 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 an increase in the health gap by education over the life cycle, we basically do not find causal effects of an additional year of compulsory schooling on health and health care utilization for any age group up to 70-74. We also tend to interpret findings for age group 75-79 as evidence of absence of health effects even though the effects are a bit less clear and we suffer from a small sample here. Yet, most point estimates are basically zero for our oldest age group. Obesity (as a measure of health behavior and not necessarily health status) is the only exception. Here we do find effects starting age 60 and increasing

until 79. Of course, we only identified a local average treatment effect, i.e., effects for individuals at the lowest margin of education willingness. A possible reason why there are no long-term health effects of this reform might be its institutional setting. The additional year of compulsory schooling did not bring individuals on a different career path. 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.

Acknowledgments

This paper uses data from SHARE Waves 1, 2, 4, 5, 6, 7 and 8 (DOIs: 10.6103/SHARE.w1.710, 10.6103/SHARE.w2.710, 10.6103/SHARE.w4.710, 10.6103/SHARE.w5.710, 10.6103/SHARE.w6.710, 10.6103/SHARE.w7.711, 10.6103/SHARE.w8.100, 10.6103/SHARE.w8ca.100), see [Börsch-Supan et al. \(2013\)](#) 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 \(2019\)](#)). The NEPS is carried out by the Leibniz Institute for Educational Trajectories (LIfBi, Germany) in cooperation with a nationwide network.

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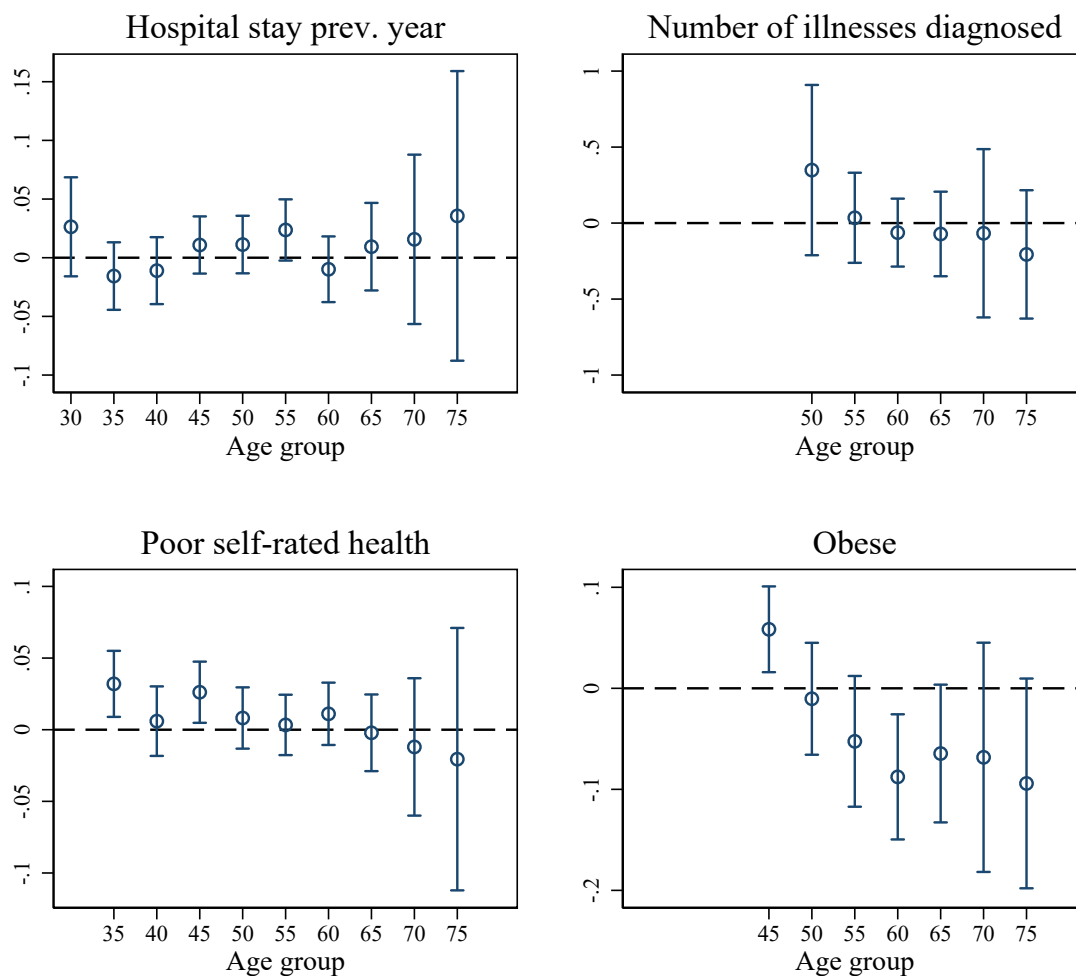
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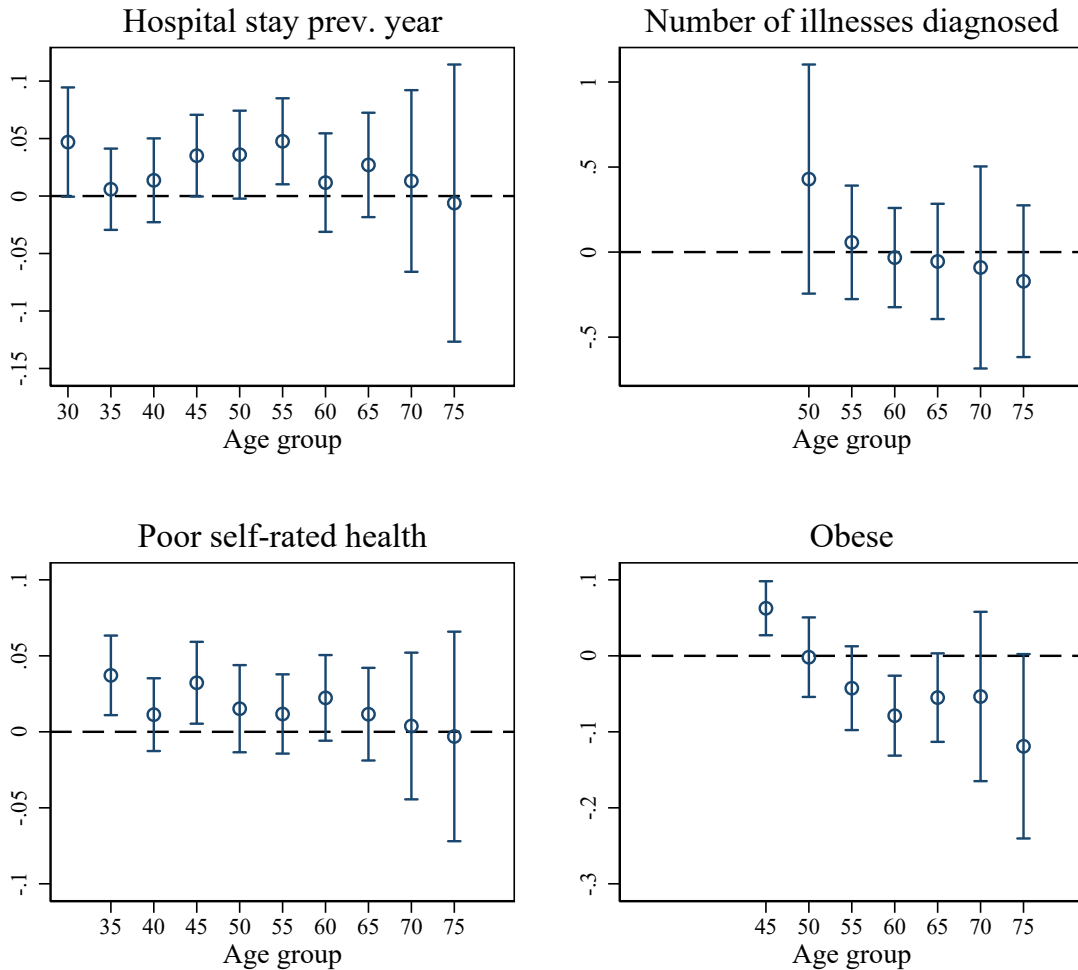
Appendix: Additional tables and figures

Figure A1: No trends



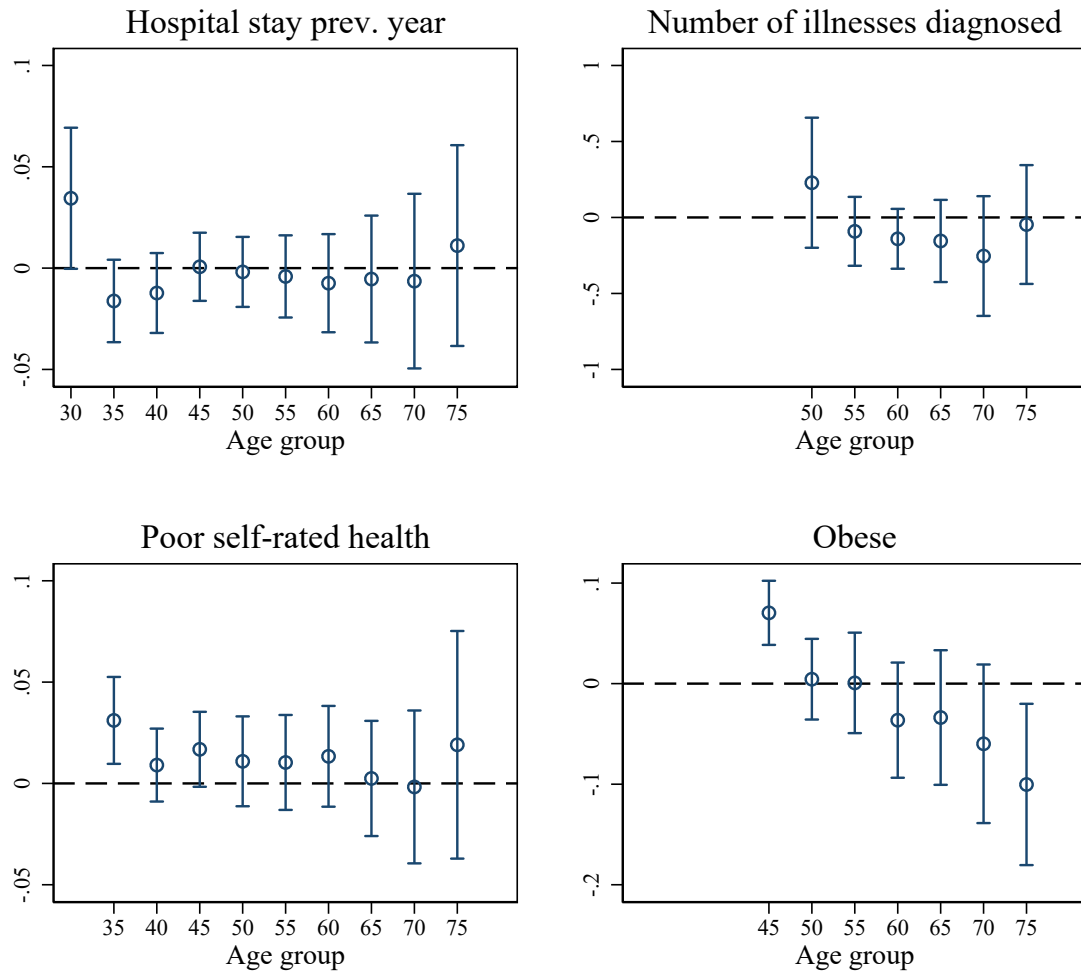
Notes: Own calculations based on SOEP, SHARE, and NEPS. Point estimates of the coefficients β_g based on 2SLS versions of Eq. (1), where instruments are interactions of reform dummy (pivotal cohort and older) with the age groups. 95 % confidence intervals. Standard errors clustered at state \times year of birth. Controls: age fixed effects, state fixed effects, survey fixed effects, survey year fixed effects, and female. Age 30 in the figure stands for age group 30-34, age 35 stands for age group 35-39, and so on.

Figure A2: Including short school years



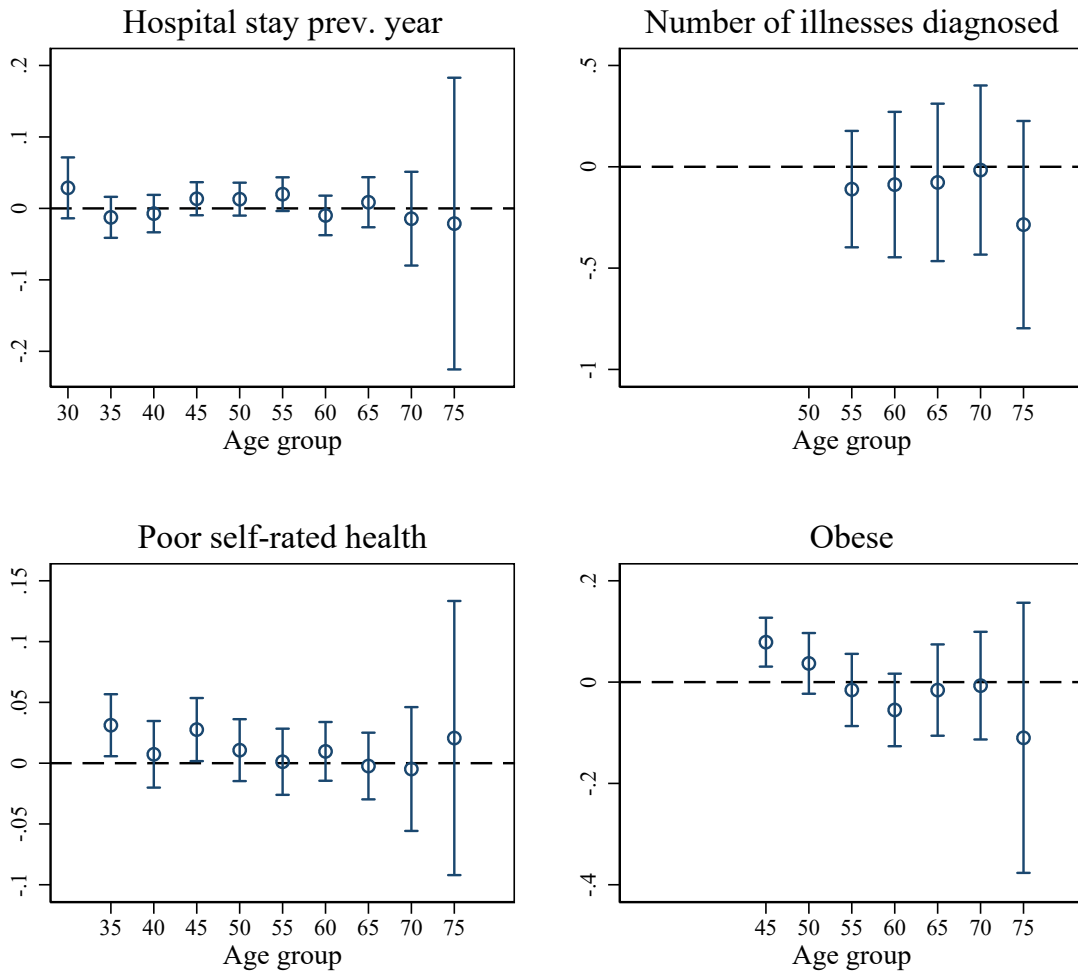
Notes: Own calculations based on SOEP, SHARE, and NEPS. Point estimates of the coefficients β_g based on 2SLS versions of Eq. (1), where instruments are interactions of reform dummy (pivotal cohort and older) with the age groups. 95 % confidence intervals. Standard errors clustered at state \times year of birth. Controls: age fixed effects, state fixed effects, survey fixed effects, survey year fixed effects, female, state-specific linear trends, and an indicator for short school years.. Age 30 in the figure stands for age group 30-34, age 35 stands for age group 35-39, and so on.

Figure A3: Sample 1930 - 1960



Notes: Own calculations based on SOEP, SHARE, and NEPS. Point estimates of the coefficients β_g based on 2SLS versions of Eq. (1), 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. Controls: age fixed effects, state fixed effects, survey year fixed effects, female and state-specific linear trends. Age 30 in the figure stands for age group 30-34, age 35 stands for age group 35-39, and so on.

Figure A4: Only SOEP



Notes: Own calculations based on SOEP. Point estimates of the coefficients β_g based on 2SLS versions of Eq. (1), 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. Controls: age fixed effects, state fixed effects, survey fixed effects, survey year fixed effects, female and state-specific linear trends. Age 30 in the figure stands for age group 30-34, age 35 stands for age group 35-39, and so on.

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. Number of observations: 77,887.