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Title:
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Date:
2019-11

Citation:
Dahmann, S. C. & Schnitzlein, D. D. (2019). No evidence for a protective effect of education on mental health. Social Science & Medicine, 241, pp.112584-112584. https://doi.org/10.1016/j.socscimed.2019.112584.

Persistent Link:
https://hdl.handle.net/11343/283712

No evidence for a protective effect of education on mental health

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JEL codes: I10, I21, I26

Keywords: Mental Health, Returns to Education, Compulsory Schooling Reform

Acknowledgments

Declarations of interest: none. Dahmann and Schnitzlein acknowledge funding from the Federal Ministry of Education and Research under the title „Nicht-monetäre Erträge von Bildung in den Bereichen Gesundheit, nicht-kognitive Fähigkeiten sowie gesellschaftliche und politische Partizipation" (FKZ: NIMOERT2/Kassenzeichen: 8103036999784/#30857). This paper uses data from the Socio-Economic Panel (SOEP) for years 1984-2016; version 33.1, SOEP, 2018, doi:10.5684/soep.v33.1. We thank Falk Voit for excellent research assistance and Britta Augsburg, Colin Cameron, Aileen Edele, Daniel Kemptner, C. Katharina Spieß, Kristof de Witte, two anonymous reviewers, and seminar and conference participants at DIW Berlin, the University of Hamburg, ZEW Mannheim, the Berlin Network of Labor Market Research, Maastricht University, the University of Sydney, the University of Melbourne, DZHW Hannover, the Microeconomics Workshop Lüneburg, the Australian/New Zealand Labour Econometric Workshop 2017, the Workshop on the Economics of Health and Wellbeing at Monash University 2018, ESPE 2017, VfS 2017, EALE 2017, SES 2018, RES 2018, and the 2017 meeting of the standing committee for economics of education of the VfS for helpful comments and discussions.

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Abstract

This paper analyzes whether education has a protective effect on mental health. To estimate causal effects, we employ an instrumental variable (IV) technique that exploits a reform extending compulsory schooling by one year implemented between 1949 and 1969 in West Germany. We complement analyses on the Mental Component Summary (MCS) score as a generic measure of overall mental health with an MCS-based indicator for risk of developing symptoms of mental health disorder and a continuous measure of subjective well-being. Results support existing evidence of a positive relationship between completed years of secondary schooling and mental health in standard OLS estimations. In contrast, the IV estimations reveal no such causal protective effect and negative effects cannot be ruled out.

Keywords

Mental Health, Returns to Education, Compulsory Schooling Reform

1 Introduction

Mental health problems are increasing substantially over time and are prevalent across all countries (see Frank and McGuire (2000) for an overview). Diagnostic trends and reduced stigma around mental disorders may account for some of the observed increase in prevalence; however, the absolute numbers are alarming. In 2001, 450 million people worldwide suffered from mental or neurological disorders or from psychosocial problems (World Health Organization, 2001), while in 2015, the number of individuals suffering from depression alone exceeded 300 million (World Health Organization, 2017). The Global Burden of Disease Study 2010 shows that mental and substance use disorders rank first among all leading causes in explaining years lived with disability and fifth in explaining disability-adjusted life-years (Whiteford et al., 2013). In Germany, the incidence of inability to work due to mental and behavioral disorders has more than doubled from 1,088 days per 1,000 insured, non-retired individuals in 2004 to 2,395 in 2014, with a substantially larger incidence among women than men (BKK Dachverband, 2015).

Mental health problems constitute a severe burden, not only for affected individuals and their families, but for societies more broadly. For individuals, mental health problems decrease quality of life by causing severe disabilities and increase mortality. Every year one million people commit suicide, with between ten and twenty million attempting it (World Health Organization, 2001), and in 90% of suicides the person had a psychiatric diagnosis at the time of death (Bertalote & Fleischmann, 2002). Family members are indirectly affected, facing, for example, economic difficulties, emotional problems and stress in adjusting, disruption of household routines, or restrictions on social activities (World Health Organization, 2001). The increasing prevalence of mental health problems is costly to society in terms of direct spending on treatment and economic losses due to reduced productivity (Frank & McGuire, 2000). In 2008, mental disorders were the third largest medical expense category in Germany–at EUR 28.7 billion (RKI/Destatis, 2015)–posing a large burden on the health care system. Additionally, social security systems and employers are affected by long periods of work inability or reduced productivity. Bubonya et al. (2017) find that poor mental health is significantly associated with both higher absence rates and lower productivity while at work. In Germany, the average period of work absence due to mental disorders was 39.1 days in 2014; three times longer than that due to other illnesses (BKK Dachverband, 2015). Consequently, the 2014 national economic loss due to mental health problems was estimated at a loss of production of EUR 8.3 billion and a loss of net product of EUR 13.1 billion (BMAS/BAuA, 2016).

Despite the increasing prevalence and the severe burden of mental health problems on individuals, families, and societies, there is limited evidence on the causal determinants of mental health conditions. Understanding these pathways is crucial for policy makers in order to adequately address or prevent mental disorders. Education may be one such important determinant, in which case schooling is a potential avenue through which mental health could be targeted. Yet, most of the economic literature is limited to the impact of education on physical health, largely establishing protective effects. Mental health is mostly disregarded in this context, likely because it is usually more difficult to measure.

In this paper, we investigate whether the protective effect of education on physical health extends to the mental dimension. In addition to the mere absence of diseases, the World Health Organization (2014) defines mental health as a state of well-being in which individuals can cope with daily life, realize their potential, and contribute to society. To acknowledge this multidimensionality, we analyze the effect of years of schooling on: (i) a generic measure of mental health—the Mental Component Summary (MCS) score—; (ii) an indicator for being at risk of developing symptoms of a clinically relevant mental health disorder; and (iii) life-satisfaction as a measure of mental well-being. To identify causal effects, we employ an instrumental variable (IV) technique using exogenous variation from a reform that extended compulsory schooling by one year, which was implemented in West German federal states between 1949 and 1969. Our sample is drawn from rich individual data on adults aged 50 to 85 from the German Socio-Economic Panel (SOEP) study.

Potential mechanisms. Since the seminal work of Grossman (1972a, 1972b), theoretical models have presented education as predictor of an individual's demand for health; for overviews of the theoretical literature and relevant empirical studies see Grossman (2000, 2006) and Lochner (2011). These models treat health capital as one dimension of human capital stock. While there are different views of whether education raises the efficiency or alters the composition of inputs, theoretical models agree in predicting a positive correlation between both the demand for and stock of health capital with an individual's level of education, even in the absence of a causal effect. The theoretical contributions focus on the physical health aspect. In the following sections, we analyze if these predictions also carry over to mental health.

Education may positively impact health and mental health through several channels. Better resources, which have been found to account for up to 30% of the relationship between education and health (Cutler & Lleras-Muney, 2010), information (see Kenkel, 1991), and behavior and traits (Currie & Moretti, 2003; Goldman & Smith, 2002)—all accounting for presumably positive impacts of education on health. Other paths are more ambiguous, including the potential negative consequences of education, particularly for mental health. Education provides access to prestigious occupations and positions, where work conditions and the reduced risk of unemployment can be beneficial (Marcus, 2013; Marmot et al., 1991; Schaller & Huff Stevens, 2015; Schiele & Schmitz, 2016), but work pace and work-related stress may be harmful (Damaske et al., 2016; Eibich, 2015; Marmot et al., 1991). Social support—a crucial resource when handling psychological challenges—may be stronger due to assortative mating and differential divorce rates (Jalovaara, 2003), but potentially weaker due to higher mobility rates among the better educated (Malamud & Wozniak, 2012). Education paired with job scarcity can result in overeducation, found to be associated with reporting more depressive symptoms (Bracke et al., 2013).

Previous literature. In the empirical economic literature, the relationship between education and *physical* health is widely studied. While recent literature points to limits in improving population health through mandated increases in schooling for physical functioning and biomarkers (c.f., Courtin et al., 2019a; Courtin et al., 2019b), others have established a set of causal protective effects: schooling, measured in years or degrees obtained, improves health outcomes, such as long-term illness (c.f., Kemptner et al., 2011) and mortality (c.f., Buckles et al., 2016; Lleras-Muney, 2005), as well as health behaviors. Hamad et al. (2018) provide a recent review on this field.

In contrast, studies of the causal effect of education on *mental* health in the economic literature are sparse. The existing studies can mostly be distinguished into three strands and show no clear pattern in the results. The first strand contains studies that investigate long-term effects of schooling on old-age *cognitive abilities* including memory functioning, which relates to dementia. Banks and Mazzonna (2012) find increases in executive functioning among males and in memory functioning among males and females older than 50 when exploiting a compulsory schooling reform in England. Glymour et al. (2008) study the effect of changes in compulsory schooling laws on US residents using data from the Health and Retirement Study. They identify positive effects on memory, but inconclusive results on cognitive functioning. Based on the Survey of Health, Ageing and Retirement in Europe, Schneeweis et al. (2014) and Crespo et al. (2014) exploit variation in compulsory schooling laws across countries and find positive effects of education on memory functioning of individuals aged 50 or older in several European countries. Additionally, Crespo et al. (2014) find that one extra year of schooling decreases the risk of depression by 6.5 percentage points.

This leads to the second strand of the literature, which is concerned with the effect of education on *specific mental disorders*, in particular depression. Employing propensity score matching among individuals born in 1958 in the United Kingdom (UK), Feinstein (2002) finds that as educational levels increase there is a reduction in the likelihood of depression at age 42. Similarly, using the British National Child Development Survey and exploiting individuals' historical information to instrument for education, Chevalier and Feinstein (2007) find a 5 to 7 percentage point decrease in the likelihood of depression for 42-year-olds with a secondary education qualification. However, these effects are non-linear, largest at low to mid-levels of education, and most pronounced for this oldest age group considered. In contrast, other studies find no or even opposite effects of education on depression. Relying on Mendelian randomization, Viinikainen et al. (2018) use genetic information to instrument for education and find no causal decline in depressive symptoms. Avendano et al. (2017) use a rise in the minimum school leaving age in the UK from 15 to 16 years to identify a causal effect of education on mental health conditions in adulthood and find an increase of the prevalence of depression and general mental health conditions. Similarly, Courtin et al. (2019b) find that a 2-year increase in compulsory schooling in France led to higher levels of depressive symptoms in women.

The third strand analyzes more *general measures of mental health* that are: (i) not disorder specific; and (ii) also cover changes in mental health that are below the level of clinical relevance. Johansson et al. (2009) use parental education as an instrument for education among respondents to the Health 2000 population survey in Finland and find that years of education do not impact general mental health, as assessed through a 12-item General Health Questionnaire which covers both the inability to perform usual daily tasks and the experience of newly arising and distressing symptoms. Investigating college education instead, instrumented through college availability, Kamhöfer et al. (2018) find no effects on the MCS score of adult respondents of the German National Educational Panel Study either.

Contributions. We investigate whether the protective effect of education identified on several areas of physical health and some specific mental disorders extends to the mental dimension in general and thereby contribute particularly to the third strand of this literature in several ways.

Firstly, and most importantly, we use the MCS as a general measure of an individual's mental well-being. The MCS is a continuous scale-measure that is not related to any specific disorder and it allows for the identification of even small changes in depressive symptoms among the general population, rather than focusing on the extensive margin of entering depression or other mental disorders. Secondly, this study is the first to identify causal effects of secondary education on mental health in Germany. As the literature suggests there is a positive effect of schooling on physical health in Germany (see e.g. Kemptner et al., 2011), our results for mental health can be compared and contrasted. Lastly, since the effect identified by changes in compulsory schooling regulations necessarily is a local average treatment effect (LATE) at the lower end of the educational distribution, in an additional analysis, we contrast our results with an estimate for the LATE at the upper end of the educational distribution.

Our results support existing evidence on a positive relationship between completed years of secondary schooling and mental health in standard OLS estimations. In contrast, the IV estimations reveal no causal protective effect of mental health and are robust to a variety of sample specifications and methodological alterations. At the upper end of the educational distribution, we again find no evidence for protective causal effects of education on mental health, confirming our main findings.

2 Empirical strategy

To estimate the association between education and mental health, we estimate the following equation:

$$mhealth_{itsb} = \beta_1 + \beta_2 yeduc_{itsb} + \beta_3 X_{it} + \mu_s + \delta_s * b + \nu_b + \eta_t + \varepsilon_{itsb}, \tag{1}$$

where $mhealth_{itsb}$ is the mental health measure of person i, observed in year t, from state s, and born in year b. $yeduc_{itsb}$ are the corresponding years of schooling, X_{it} is a vector of individual characteristics and μ_s, ν_b, η_t are fixed effects accounting for differences between states, years of birth and years of survey respectively. Additionally, we include state specific linear time trends in the year of birth, $\delta_s * b$, but our results are robust to their exclusion or to including them in quadratic or cubic terms. Error terms ε_{itsb} are assumed to be normally distributed with mean zero and are, following the recommendations in the literature to cluster at the most aggregate level (Abadie et al., 2017; Cameron & Miller, 2015), clustered at the state level s. To account for the small number of clusters (10), statistical inference is based on 9 degrees of freedom (Cameron & Miller, 2015).

Our parameter of interest is β_2. However, its ordinary least squares (OLS) estimate, $\hat{\beta}_2$, may be biased due to endogeneity that can arise for several reasons including omitted variables—e.g., parental characteristics and time preferences—and reverse causality. For example, better educated or more involved parents may better support their children's education, but at the same time also care more about their children's health, or individuals with low discount rates value the future more and, therefore, invest in both education and health. Also, individuals with mental health problems may have difficulties in acquiring education since their condition disrupts their learning, unlike their mentally healthy peers. Thus, ultimately, endogeneity leads to specific individuals selecting into higher education based directly on either their mental health condition or factors related to it.

To account for endogeneity and to provide unbiased estimates that can be given a causal interpretation, we employ an instrumental variable (IV) technique by adding a first stage equation to (1) where $yeduc_{itsb}$ is regressed on the instrument z_{itsb} and all exogenous variables from equation (1):

$$yeduc_{itsb} = \beta_4 + \beta_5 z_{itsb} + \beta_6 X_{it} + \rho_s + \kappa_s * b + \tau_b + \lambda_t + \omega_{itsb} \quad (2)$$

We insert the fitted value from the regression of equation (2) for $yeduc_{itsb}$, $\widehat{yeduc}_{itsb}$, in equation (1) and estimate a standard two-stage least squares (2SLS) estimator. The resulting coefficient of interest, $\hat{\beta}_2$, is a local average treatment effect (LATE) for compliers with the instrument. LATE only applies to a subgroup of the analyzed sample and, therefore, differs from the average treatment effect, especially in the case of non-linear or heterogeneous effects. Thus, the choice of the instrumental variable is crucial for the interpretation of the effects. We exploit a reform that expanded compulsory schooling from eight to nine years, likely impacting only individuals at the lower end of the educational distribution who would have otherwise dropped out of school earlier.

To be a valid instrument, the variable must fulfill: (i) *Exogeneity*: the instrumental variable must not be affected by the outcome and it must affect the outcome only through its influence on the endogenous variable; (ii) *Relevance:* variation in the instrumental variable needs to explain sufficient variation in the endogenous variable; and (iii) *Monotonicity*: individuals must either perfectly comply with or not react to the instrument but cannot react *opposite* to it. We present plausible arguments and supportive empirical evidence on the validity of these assumptions in the next section, while we provide F-Statistics for exclusion of the instruments in all first stage regressions, which can be compared to the critical values provided by Stock and Yogo (2005), to highlight the relevance of the instrument.

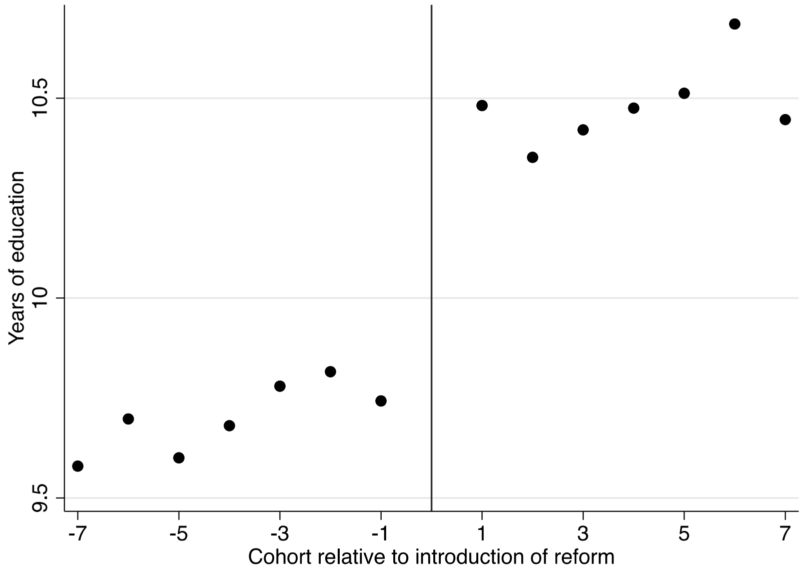
3 The Compulsory Schooling Reform in Germany

Institutional Background. In Germany, educational policy is the responsibility of each federal state. Common across all states is that children enroll in elementary school at the age of six. Usually after four years, they start secondary school, choosing between three different tracks that are provided at separate schools. *Hauptschule* provides the basic track encompassing eight or nine years, including the years at elementary school, after which students obtain a basic school leaving degree. At *Realschule*, students follow an intermediate track and finish after a total of ten years. After leaving school, students from these two tracks usually start an apprenticeship or vocational training. Only graduation from *Gymnasium*, the highest track, leads to the university entrance qualification. In some states, comprehensive schools that provide all three tracks at one school exist in addition. Between 1949 and 1969, compulsory schooling was increased from eight to nine years in the Federal Republic of Germany (West Germany). As this change in law was implemented by the federal states, the year of implementation differs (see online appendix Table A.1 for an overview).

Validity as Instrument. The change in law forced students to stay one year longer in school and thus is an ideal instrument for explaining years of schooling. The assumption of exogeneity requires that, conditional on all covariates included in the estimation of equations (1) and (2), the implementation of the reform is unrelated to any determinants of mental health. These include any unobserved state-characteristics related to the law change that may affect mental health. We therefore include linear state-specific cohort trends which capture, for example, trends in other policies, including the educational expansion, and can also account for macroeconomic factors like economic growth that may increase public resources for education but also improve health through higher living standards. Furthermore, health policy is centrally determined at the national level in Germany, whereas education policy is the responsibility of each federal state. Hence, residents in all states are subject to the same health system. Any changes therein are, thus, unrelated to the extension of compulsory schooling at different times and are controlled for by cohort and survey-year fixed effects.

Figure 1 shows the average years of schooling in the cohorts prior and post reform and essentially is a graphical representation of our first stage (based on the data and sample described in the next section). The sharp increase of more than 0.5 years with the implementation of the reform illustrates its effectiveness in raising completed years of schooling and reinforces its relevance as an instrument.

Figure 1 Average number of years of schooling by birth cohort



Source: Own illustration, SOEPv33.1.

With regard to monotonicity, it should be safe to assume that the educational choice of individuals preferring the highest track over both the basic *and* the intermediate track, should be unaffected if the length of the lower track increases but does not exceed the length of the other. However, non-linear preferences of students following the intermediate track may be problematic in case their ideal preference lies between eight and ten years. Therefore, we estimate the impact of the reform on attaining at least an intermediate school leaving degree (see online appendix Table A.2), which is statistically insignificant but positive, suggesting that, if at all, attainment of at least an intermediate school leaving degree increases by 4 percentage points in response to the reform. Similarly, we estimate the effect of the reform on dropout probability, to ensure that, although students now spend more years at school, the rate of those completing their education does not fall. The estimated coefficient is close to zero and insignificant, suggesting that changes in dropout rates are not a threat to our identification.

We thus are confident that the change in the compulsory schooling legislation in (West) Germany is well suited to be used as an instrumental variable. In addition, this reform has been used to estimate the causal effects of education on, e.g., wages (Pischke & von Wachter, 2008), physical health (Kemptner et al., 2011), and political preferences (Siedler, 2010), where it has been shown to be a valid instrument.

4 Data

The analysis draws on data from the German Socio-Economic Panel (SOEP) study, a representative household panel surveyed annually with information on around 30,000 individuals in almost 15,000 households (Goebel et al., 2018). In addition to rich information on individuals' background, since 2002 the SOEP includes a detailed measure of respondents' mental health in every second wave.

We restrict our sample to West German observations from 2002 to 2016 that have valid information on mental health and all control variables. To construct a sample that is best suited for our IV approach, we include only individuals born within a window of seven years before and seven years after the cutoff birth year for which the longer compulsory schooling was binding for the first time. To identify these individuals and assign them to the treatment group (affected by the reform) or the control group (not affected), we further need information on the federal state in which their schooling took place. Given that cross-state mobility is generally, and in particular among these older cohorts, low, following Pischke and von Wachter (2008) we use the federal state in which the respondent was observed when first entering the SOEP as a proxy. With a subsample of 2,473 individuals, for whom we also observe their federal state of birth, we validate that 75% of them still live in the same state at survey entry, confirming relatively low rates of cross-state mobility, in line with Pischke (2007).

We include all available observations per person to maximize the utilization of available information and cluster standard errors at the state level, in which individuals are fully nested. Finally, for a homogenous sample, we restrict the sample to observations when individuals are aged between 50 and 85, dropping 16.4% (8.7%) of observations (persons). In total, we observe 20,290 person-year observations from 5,321 persons, with on average 3.8 person-years per individual.

Measurement of Mental Health. As the main outcome, we use the Mental Component Summary (MCS) score, which is a continuous-scale measure of general mental health that can be implemented in large-scale surveys due to its brevity (Ware et al., 1996b). It is found to be reliable and valid (Ware et al., 1996b) and a psychometrically sound measure that is able to detect mental health disorders (e.g., Gill et al., 2007; Salyers et al., 2000). It does not refer to any disorder-specific diagnoses and, therefore, serves as a generic measure of an individual's mental constitution. In the SOEP, it is operationalized through the SF12, a battery of 12 items relating to both physical and mental health (Andersen et al., 2007). For example, questions ask how often in the past four weeks respondents felt down and gloomy or felt that mental health or emotional problems impaired their work or social life. A detailed list of all questions and answer categories is provided in online appendix Table A.3. The SF12 questionnaire collects information on respondents' mental health in an indirect way, which is advantageous as direct measures, such as a self-reported diagnosis of mental illnesses, could be subject to underreporting due to stigmatization (Bharadwaj et al., 2017). Another advantage is that this type of measurement allows us to measure subclinical changes in mental health.

Based on this questionnaire, two scores are computed using factor analysis, one for physical (Physical Component Summary, PCS) and one for mental health (MCS). The scores load on distinct items: PCS loads especially on the dimensions of physical fitness, general health, bodily pain, and role physical, whereas MCS loads primarily on mental health, role emotional, social functioning, and vitality. The original scales are anchored at mean 50 and standard deviation 10 in the 2004 general SOEP population (Nübling et al., 2006). We standardize the scores to have mean zero and standard deviation one within our sample, with higher values indicating better health, which allows the interpretation of changes in terms of standard deviations.

In addition to the MCS score, we calculate an indicator (0/1) if an individual has an MCS score lower than or equal to 45.6, which has high predictive power for developing clinically relevant symptoms of mental disorder (Vilagut et al., 2013). According to this indicator, about one quarter of our sample is at risk of developing these symptoms (see online appendix Table A.4)—to put this number in perspective, Wittchen et al. (2010) report a lifetime incidence of diagnosed depression of about 19% in the German population.

As a third indicator, we use information on life-satisfaction available in the SOEP as a measure of mental well-being. This measure, besides being an alternative, is robust to selective reporting issues of respondents because it is not asked at the same time as the SF12 health questions. Individuals are asked "How satisfied are you with your life, all things considered?" and answer on a scale from 0 (completely dissatisfied) to 10 (completely satisfied), which we again standardize to mean zero and standard deviation one within the sample. While life-satisfaction captures subjective perceptions of own well-being, it may of course also reflect aspects of life other than mental health, including economic well-being or physical health, and should therefore be interpreted with caution.

Other variables. Our primary explanatory variable of interest is education, which we measure by the years of schooling ($yeduc$) computed from an individual's obtained secondary schooling degree. Thus, individuals who have repeated a grade are not assigned an additional year. Analogous to Pischke and von Wachter (2008) and Kemptner et al. (2011), we assign individuals with no or only a basic degree (*Hauptschulabschluss*) 8 years of schooling if they were not affected by the reform and 9 if they were affected. We count an intermediate (*Realschulabschluss*) or other type of degree with 10 years, while completion of vocational secondary education (*Fachhochschulreife*) is accounted for with 12 years, and only graduation from the highest track (*Abitur*) is considered with 13 years. In our sample, 48% of all pre-reform observations are individuals with a basic school degree only.

Further, we control for some individual characteristics including demographic information based on gender and nationality, socio-economic background measured through parental education—accounting also for selective migration related to parent's education—, and whether the individual lives in an urban or rural area. Age is controlled for through the year of birth and survey year dummies, and we control for age squared to account for potential non-linear age effects. We choose this parsimonious set of controls to avoid including bad controls that may themselves be influenced by education and serve as potential mechanisms. Therefore, all variables included are predetermined before obtaining education, except for area type referring to current place of residence—however, results are robust to its exclusion. Online appendix Table A.4 provides an overview of our estimation sample.

5 Empirical results

5.1 Education and mental health

We first present results from standard OLS estimations for our health indicators. Table 1 presents coefficients from separate regressions using our three indicators for mental health: MCS score (column 1), an indicator for MCS scores below or equal to 45.6 (column 2), and subjective well-being (LS, column 3), as dependent variables. To validate our empirical strategy and for comparison with the existing literature, we use a measure of physical health (PCS, column 4) as dependent variable. Our sample reproduces the well-known positive and highly significant relationship between education and physical health. The OLS estimates also show a positive and highly significant relationship between education and mental health measured by both the MCS score and subjective well-being, and a negative correlation with the indicator for being at risk of developing symptoms of a mental health disorder (MCS≤45.6). However, these correlations might not represent a causal relationship, since important determinants of mental health might be unobserved in the above models.

Table 1 OLS Estimations – Effect of Education on Health Indicators

	Outcome variables: aggregate health indicators			
	MCS (1)	MCS≤45.6 (2)	LS (3)	PCS (4)
yeduc	0.031* (0.014) [0.006,0.056]	-0.014** (0.005) [-0.024,-0.004]	0.070*** (0.006) [0.058,0.082]	0.088*** (0.005) [0.078,0.098]
R^2	0.035	0.022	0.04	0.07
Obs.	20290	20290	20290	20290
Pers	5321	5321	5321	5321

Notes: SOEPv33.1 waves 2002 to 2016. OLS regressions. Further, a maximum set of state dummies, year of birth dummies, survey year dummies, linear birth year state trends, and a constant are included. Individual characteristics controlled for include age squared, female, non-German nationality, rural area, and high parental education. Standard errors, reported in parentheses, are clustered at state level; critical values are adjusted to account for the small number of clusters. 90% confidence intervals are reported in brackets. *p<0.1, **p<0.05, ***p<0.01.

To provide causal estimates, we use the exogenous variation in education induced by the compulsory schooling reform using a classical 2SLS framework (see Table 2). The lower panel of Table 2 presents the first stage coefficient of our instrumental variable (a reform indicator). The reform increases the years of schooling of individuals by about 0.59 years. The coefficient is less than 1 as the sample also includes individuals who would have proceeded with their education irrespective of the reform. The magnitude is in line with first-stage results of existing studies finding coefficients of the reform on years of schooling between 0.39 and 0.54 using ALLBUS and ForsaBus data (Siedler, 2010) and between 0.58 and 0.7 based on the German Microcensus (Kemptner et al., 2011).

The upper panel of Table 2 shows the results of the second stage estimations. We present results for men and women together, as separate unreported regressions show no differences. For physical health, we find a weakly significant, positive coefficient in the 2SLS estimation (column 4). Thus, our sample reproduces a causal protective effect of education on physical health—in line with the existing findings of e.g., Kemptner et al. (2011). Turning to the focus of our study, the indicators of mental health, contrary to the OLS estimates, our point estimates show negative effects of an additional year of education. An additional year of education induced by compulsory schooling causes a decrease of the MCS score of 0.192 (19.2%) of a standard deviation which is about 1.9 points on the MCS scale. However, the estimated effect is not significant and far from being relevant either in economic or medical terms. Ware et al. (1996a) argue that changes in the MCS score smaller than 7.9 points should not be considered to have clinical relevance. Our point estimate is clearly below that threshold. Additionally, Table 2 includes the 90% confidence interval for the estimated effect ranging from -0.436 to 0.053 of a standard deviation in MCS, or in absolute values, from -4.3 to 0.53 MCS points. Thus, also the bounds of a very conservative interval estimate are far from showing a clinically or economically relevant effect. Column 2 of Table 2 shows the effect on the probability of having an MCS score below the critical value of 45.6, which is positive indicating a 4.7 percentage point higher likelihood of developing symptoms of a mental health disorder. Thus, the effect is in line with the negative coefficient in column 1, however, again the estimate is not statistically significant. The same is true for the coefficient on subjective well-being in column 3. We find a negative point estimate, however, not statistically significant, and the magnitude corresponds to a decrease of only 0.04 points on the original 11-point scale. Although all these estimates are not statistically significant, it is safe to conclude that there is no evidence for a significant and/or clinically or economically relevant protective effect of education on mental health.

Table 2 IV Estimations – Effect of Education on Health Indicators

	Outcome variables: aggregate health indicators			
	MCS (1)	MCS≤45.6 (2)	LS (3)	PCS (4)
Second stage results				
yeduc	-0.192	0.047	-0.021	0.199*
	(0.133)	(0.045)	(0.083)	(0.099)
	[-0.436,0.053]	[-0.035,0.128]	[-0.173,0.131]	[0.017,0.380]
R^2	-0.127	-0.046	-0.003	-0.002
Obs.	20290	20290	20290	20290
Pers	5321	5321	5321	5321
First stage results				
Reform	0.589***	0.589***	0.589***	0.589***
	(0.158)	(0.158)	(0.158)	(0.158)
F-stat.	13.908	13.908	13.908	13.908

Notes: SOEPv33.1 waves 2002 to 2016. 2SLS regressions. Further, a maximum set of state dummies, year of birth dummies, survey year dummies, linear birth year state trends, and a constant are included. Individual characteristics controlled for include age squared, female, non-German nationality, rural area, and high parental education. P-values for test of exogeneity of yeduc: 0.047 (1), 0.068 (2), 0.171 (3), 0.413 (4). Standard errors, reported in parentheses, are clustered at state level; critical values are adjusted to account for the small number of clusters. 90% confidence intervals are reported in brackets. F-stat. denotes the Kleibergen-Paap Wald rk F statistic for weak identification in case of clustered standard errors. *p<0.1, **p<0.05, ***p<0.01

Thus, based on the aggregate measures available in the SOEP, we can reproduce the positive protective effect of education on physical health known from the existing literature. In contrast, we find no evidence for a protective effect of education on an aggregate measure of mental health. Reduced-form effects confirm this conclusion with an even more precisely estimated (negative) effect on MCS (see online appendix Table A.5). We find also no evidence for a protective effect of education on the intra-individual decline in mental health: 2, 4, 6, 8, 10, and 12-year changes in MCS remain unaffected (see online appendix Table A.6).

As the MCS score is an aggregate index, it is possible, that the estimations mask heterogeneity among the four subscales of the MCS score (a pure mental health dimension, vitality, social functioning, and role emotional). We disentangle this in Table 3 by estimating a separate regression for each subscale, with the results in line with our previous findings: We find negative, however insignificant, point estimates, supporting our finding, that there is no evidence for a protective effect of education on individuals' mental health. This finding is not a result of competing effects among the different subscales but represents a uniform impact of compulsory education on individuals' mental health.

Thus, we can generally rule out a protective, but not a negative effect, of education on mental health, which is in line with findings in the literature on general mental health but contradicts existing disorder-specific results.

Table 3 IV Estimations – Effect of Education on Health Indicators

	Outcome variables: subscales of MCS			
	Mental Health (1)	Vitality (2)	Social Functioning (3)	Role Emotional (4)
Second stage results				
yeduc	-0.044	-0.023	-0.194	-0.104
	(0.106)	(0.115)	(0.156)	(0.131)
	[-0.239,0.151]	[-0.234,0.187]	[-0.480,0.092]	[-0.345,0.136]
R^2	-0.008	-0.004	-0.136	-0.047
Obs.	20290	20290	20290	20290
Pers	5321	5321	5321	5321
First stage results				
Reform	0.589***	0.589***	0.589***	0.589***
	(0.158)	(0.158)	(0.158)	(0.158)
F-stat.	13.908	13.908	13.908	13.908

Notes: SOEPv33.1 waves 2002 to 2016. 2SLS regressions. Further, a maximum set of state dummies, year of birth dummies, survey year dummies, linear birth year state trends, and a constant are included. Individual characteristics controlled for include age squared, female, non-German nationality, rural area, and high parental education. Standard errors, reported in parentheses, are clustered at state level; critical values are adjusted to account for the small number of clusters. 90% confidence intervals are reported in brackets. F-stat. denotes the Kleibergen-Paap Wald rk F statistic for weak identification in case of clustered standard errors. *p<0.1, **p<0.05, ***p<0.01.

5.2 Robustness

In this section, we address the robustness of our findings with respect to possible threats to our identification strategy as well as to the choice of instrument, with the additional results presented in the online appendix.

First, we address measurement error in the reform dummy. Historically, the cut-off dates for school entry did not always coincide with the calendar year, but varied by year and federal state (see Görlitz et al., 2019, Table A3), such that we may misclassify the exposure to the reform by focusing only on the year of birth. Therefore, for each state we exclude the two cohorts born around the first year affected by the reform. Results are robust to this exercise (Table A.7).

Second, we address measurement error in the years of schooling: in 1966 and 1967 most federal states introduced two short school years with the length being two-thirds of a calendar year to move the beginning of the academic year from spring to summer (Pischke, 2007), reducing total length of schooling for students at the time. Therefore, we adjust the measure of years of schooling to specifically incorporate the actual length of school years by subtracting one-third or two-thirds, respectively, depending on whether the individual was affected by one or even two short school years, determined by the year of birth, the federal state, and the choice of secondary school track (based on the assignment in Pischke (2007), Table 2). The estimates based on this alternative measure for years of schooling indicate that results, both on the mental and physical health measures, are unaltered (Table A.8).

Third, we address the selection of the time period by varying the sample window we consider around the compulsory schooling reform. Our main results are based on limiting our sample to seven cohorts prior and seven cohorts post the reform in each federal state. Alternatively, we provide estimates for samples bounded by three to 10 cohorts prior and post reform, respectively. While the fewer observations in the smaller sample span of three years before and after the reform naturally decrease the power of our instrument, the original negative coefficient of education on MCS is clearly preserved in each of these samples, although again none are statistically significant. The same holds true for the coefficients on the indicator of being at risk of developing symptoms of a mental health disorder and life-satisfaction as indicator for mental well-being, except for positive, but again not statistically significant, coefficients on life-satisfaction based on the three- to four-year cohort window. Overall, these results suggest that our findings are not driven by the choice of the observation period alongside the introduction of the compulsory schooling reform (Table A.9).

Fourth, we re-estimate our analyses ten times, excluding each federal state one at a time. The unreported results are consistent with our conclusion, suggesting that no single state is driving our results.

Fifth, we specify two alternative definitions of our sample to account for potentially overweighting more frequently observed individuals due to the unbalanced structure of our panel: (i) we weight each observation by the inverse frequency the individual is encountered in the data; and (ii) we restrict the sample to only one observation per person, choosing the one closest to the center of our age restriction (67.5). Again, the results support the finding that there is no evidence for a causal protective effect of education induced by compulsory schooling on mental health (Tables A.10 and A.11).

Finally, to overcome the limitation that our results represent a LATE at the lower end of the educational distribution in Germany, we use individual availability of higher education measured by the spatial distance from the place of birth to the nearest university as a second instrumental variable to estimate a LATE at the upper end of the educational distribution (c.f., Card, 1995; Currie & Moretti, 2003); see the online appendix for details on the estimation strategy and results. We find a point estimate for MCS of -0.028 standard deviations. Although this estimate is more symmetric around zero and should be interpreted with caution, again the effect is neither statistically significant nor relevant in medical terms. Also, for the indicator of being at risk of developing symptoms of a mental health disorder and subjective well-being, the estimates are small and not statistically significant. Thus, our finding of a non-existent protective effect of education on mental health appears not specific to the situation of the very low educated but may also be found at the upper end of the educational distribution, which is in line with Kamhöfer et al. (2018) finding no causal effect of tertiary education on mental health in Germany.

6 Conclusion

The incidence of mental health problems is increasing and globally prevalent. Despite their severe burden on several affected parties, little is known about the causal determinants of mental health conditions. Education may yield important protective effects through various channels; yet, the economic literature largely focuses on the impact of education on physical health, rather than mental health.

To analyze changes even at the intensive margin, we provide first evidence on the causal impact of years of secondary schooling on a generic measure of mental health for Germany. Using rich individual data on adults from the SOEP, we exploit two distinct instrumental variables to estimate causal effects at the lower and the upper tail of the educational distribution. Our results support existing evidence on a positive relationship between completed years of schooling and mental health in standard OLS estimations. In contrast, the IV estimations reveal no such causal protective effect: unlike physical health, mental health is not significantly altered by education, while negative effects cannot be ruled out.

Summarizing, our findings suggest that education is not an appropriate tool for fostering mental health in the long-run. Therefore, consistent with Courtin et al. (2019a) and Courtin et al. (2019b), we conclude that mandating greater years of schooling may not be sufficient in and of itself in promoting greater population health. There is a need for further research on alternative or complementary policies. However, first understanding the reasons underlying the (non)-relationship between education and mental health is critical to unravel the potential for intervention. Our context helps us to rule out some explanations. The aggregate results on the MCS score do not mask effect heterogeneity among the four subscales of the MCS score. Neither do the average zero effects originate from potential non-linearities in the effects, i.e., an additional year of schooling having differential impacts depending on the overall level. The two distinct LATEs we estimate consistently show no evidence for a protective effect on mental health. We cannot rule out different reporting behavior by socio-economic status as an explanation, if the better educated are more comfortable reporting emotional problems; however, our mental health measures incorporate various dimensions and are only surveyed indirectly to reduce this risk.

There are further potential explanations for our results and the effect discrepancy between mental and physical health; we propose three promising avenues for future research. The first is a better understanding of labor market outcomes resulting from completing more years of education. If labor market outcomes improve, increased earnings and resources could explain improved health outcomes more generally. However, they may come at the cost of more demanding work conditions which offset especially any potential mental health benefits. In contrast, if labor market outcomes remained unchanged, the resulting job-education mismatch may again negatively influence mental well-being (Bracke et al., 2013). In both cases, specific policies tied to the labor market may be needed to accompany educational policies for fostering mental health. Additionally, these might have spillover effects, as mental health is impacted also by spousal labor market success (Marcus, 2013). The second avenue is a consideration of qualitative aspects of education: evidence on the benefits of health knowledge on health behaviors (Kenkel, 1991) suggests that returns may be tied to a specific curriculum. While in Germany physical education is an integral part of every student's curriculum, healthy behaviors fostering mental and psychological aspects, such as stress coping mechanisms, are not usually taught at school. This could account for some of the discrepancy in the effects and should be investigated in more detail. Lastly, it may be of particular interest in further investigations to replicate the results with younger cohorts that may be exposed to different levels of stress.

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Online Appendix

A. Additional Tables

Table A.1 Increase of compulsory schooling from 8 to 9 years by federal state

	First year all students were supposed to graduate after a minimum of 9 years	First birth cohort affected by change in compulsory schooling law
Hamburg	1949	1934
Schleswig-Holstein	1956	1941
Bremen	1958	1943
Lower Saxony	1962	1947
Saarland	1964	1949
North Rhine-Westphalia	1967	1953
Hesse	1967	1953
Rhineland-Palatinate	1967	1953
Baden-Wuerttemberg	1967	1953
Bavaria	1969	1955

Source: Pischke and von Wachter (2005)

Table A.2 Effects of the Reform on Track Choice and Success

	Outcome variables	
	Intermediate Degree or higher	Dropout
	(1)	(2)
Reform	0.044	-0.000
	(0.034)	(0.008)
R^2	0.119	0.028
Obs.	20290	20290
Pers.	5321	5321

Notes: SOEPv33.1 waves 2002 to 2016. OLS regressions. Further, a maximum set of state dummies, year of birth dummies, survey year dummies, linear birth year state trends, and a constant are included. Individual characteristics controlled for include age squared, female, non-German nationality, rural area, and high parental education. Standard errors, reported in parentheses, are clustered at state level; critical values are adjusted to account for the small number of clusters. * p<0.1, ** p<0.05, *** p<0.01.

Table A.3 SF12 Questionnaire in SOEP

Question	Answers
How would you describe your current health?	Very good/ Good/ Satisfactory/ Poor/ Bad
When you have to climb several flights of stairs on foot, does your health limit you greatly, somewhat, or not at all?	Greatly/ Somewhat/ Not at all
And what about other demanding everyday activities, such as when you have to lift something heavy or do something requiring physical mobility: Does your health limit you greatly, somewhat, or not at all?	Greatly/ Somewhat/ Not at all
During the last four weeks, how often did you... **... feel down and gloomy?** **... feel calm and relaxed?** **... feel energetic?** ... have severe physical pain? ... feel that due to physical health problems you achieved less than you wanted to at work or in everyday activities? ... feel that due to physical health problems you were limited in some way at work or in everyday activities? **... feel that due to mental health or emotional problems you achieved less than you wanted to at work or in everyday activities?** **... feel that due to mental health or emotional problems you carried out your work or everyday tasks less thoroughly than usual?** **... feel that due to physical or mental health problems you were limited socially, that is, in contact with friends, acquaintances, or relatives?**	Always/ Often/ Sometimes/ Almost never/ Never

Notes: MCS is calculated using factor analysis based on *all* items, including physical and mental health. The same holds true for PCS. Bold items denote mental health dimension.

Source: SOEP questionnaire SOEPv33 (TNS Infratest Sozialforschung, 2016).

Table A.4 Descriptive Statistics

	Mean	Std. Dev.	Min.	Max.	No. of obs.
(Non-standardized) aggregate health measures					
MCS	51.644	9.976	3.533	79.432	20290
MCS≤45.6	0.245	0.43	0	1	20290
LS	7.203	1.759	0	10	20290
PCS	47.449	9.757	11.147	75.456	20290
Instrument					
Reform	0.453	0.498	0	1	20290
Control variables					
yeduc	10.052	1.822	8	13	20290
Age	58.985	6.062	50	85	20290
Female	0.512	0.5	0	1	20290
Non-German nationality	0.035	0.183	0	1	20290
Rural area	0.261	0.439	0	1	20290
High parental education	0.132	0.338	0	1	20290
Year of birth	1950.663	5.38	1928	1961	20290

Notes: SOEPv33.1 waves 2002 to 2016. The distribution of the aggregate health measures prior to standardization is shown. Hence, the numbers for MCS, LS, and PCS correspond to the original scale provided in the data.

Table A.5 Reduced Form Results

	Outcome variables: aggregate health indicators			
	MCS (1)	MCS≤45.6 (2)	LS (3)	PCS (4)
Reform	-0.113*	0.027	-0.012	0.117
	(0.061)	(0.021)	(0.047)	(0.079)
	[-0.224,-0.001]	[-0.012,0.067]	[-0.098,0.074]	[-0.028,0.262]
R^2	0.033	0.019	0.026	0.049
Obs.	20290	20290	20290	20290
Pers.	5321	5321	5321	5321

Notes: SOEPv33.1 waves 2002 to 2016. OLS regressions. Further, a maximum set of state dummies, year of birth dummies, survey year dummies, linear birth year state trends, and a constant are included. Individual characteristics controlled for include age squared, female, non-German nationality, rural area, and high parental education. Standard errors, reported in parentheses, are clustered at state level; critical values are adjusted to account for the small number of clusters. 90% confidence intervals are reported in brackets. * p<0.1, ** p<0.05, *** p<0.01.

Table A.6 IV Estimations – Changes in MCS Score Over Time

	Outcome variables: Changes in MCS score over...					
	2 years (1)	4 years (2)	6 years (3)	8 years (4)	10 years (5)	12 years (6)
Second stage results						
yeduc	0.021	-0.018	-0.003	-0.042	0.020	0.125
	(0.028)	(0.050)	(0.075)	(0.131)	(0.139)	(0.300)
R^2	-0.001	-0.000	0.002	-0.005	0.004	-0.029
Obs.	14601	10589	7309	5030	3130	1692
Pers.	4203	3441	2416	2006	1511	1046
First stage results on yeduc						
Reform	0.620***	0.604***	0.584***	0.578***	0.514***	0.392***
	(0.168)	(0.172)	(0.172)	(0.144)	(0.123)	(0.107)
F-stat.	13.589	12.304	11.561	16.027	17.354	13.333

Notes: SOEPv33.1 waves 2002 to 2016. 2SLS regressions. Further, a maximum set of state dummies, year of birth dummies, survey year dummies, linear birth year state trends, and a constant are included. Individual characteristics controlled for include age squared, female, non-German nationality, rural area, and high parental education. Standard errors, reported in parentheses, are clustered at state level; critical values are adjusted to account for the small number of clusters. 90% confidence intervals are reported in brackets. F-stat. denotes the Kleibergen-Paap Wald rk F statistic for weak identification in case of clustered standard errors. * p<0.1, ** p<0.05, *** p<0.01.

Table A.7 IV Estimations – Dropping Pivotal Cohorts

	Outcome variables: aggregate health indicators			
	MCS (1)	MCS≤45.6 (2)	LS (3)	PCS (4)
Second stage results				
yeduc	-0.435	0.146	-0.146	0.332
	(0.320)	(0.110)	(0.164)	(0.221)
	[-1.021,0.151]	[-0.055,0.348]	[-0.446,0.155]	[-0.074,0.737]
R^2	-0.589	-0.371	-0.107	-0.145
Obs.	17233	17233	17233	17233
Pers.	4551	4551	4551	4551
First stage results on yeduc				
Reform	0.667***	0.667***	0.667***	0.667***
	(0.170)	(0.170)	(0.170)	(0.170)
F-stat.	15.479	15.479	15.479	15.479

Notes: SOEPv33.1 waves 2002 to 2016. 2SLS regressions. Further, a maximum set of state dummies, year of birth dummies, survey year dummies, linear birth year state trends, and a constant are included. Individual characteristics controlled for include age squared, female, non-German nationality, rural area, and high parental education. Standard errors, reported in parentheses, are clustered at state level; critical values are adjusted to account for the small number of clusters. 90% confidence intervals are reported in brackets. F-stat. denotes the Kleibergen-Paap Wald rk F statistic for weak identification in case of clustered standard errors. * p<0.1, ** p<0.05, *** p<0.01.

Table A.8 IV Estimations – Accounting for Short School Years

	Outcome variables: aggregate health indicators			
	MCS (1)	MCS≤45.6 (2)	LS (3)	PCS (4)
Second stage results				
yeduc	-0.218	0.053	-0.023	0.226*
	(0.158)	(0.052)	(0.095)	(0.114)
	[-0.508,0.071]	[-0.042,0.148]	[-0.198,0.151]	[0.017,0.436]
R^2	-0.151	-0.054	-0.004	-0.014
Obs.	20290	20290	20290	20290
Pers.	5321	5321	5321	5321
First stage results on yeduc				
Reform	0.517***	0.517***	0.517***	0.517***
	(0.158)	(0.158)	(0.158)	(0.158)
F-stat.	10.675	10.675	10.675	10.675

Notes: SOEPv33.1 waves 2002 to 2016. 2SLS regressions. Further, a maximum set of state dummies, year of birth dummies, survey year dummies, linear birth year state trends, and a constant are included. Individual characteristics controlled for include age squared, female, non-German nationality, rural area, and high parental education. Standard errors, reported in parentheses, are clustered at state level; critical values are adjusted to account for the small number of clusters. 90% confidence intervals are reported in brackets. F-stat. denotes the Kleibergen-Paap Wald rk F statistic for weak identification in case of clustered standard errors. * p<0.1, ** p<0.05, *** p<0.01.

Table A.9 IV Estimations – Different Reform Windows

	Outcome variables: aggregate health indicators						
	MCS (1)	MCS≤45.6 (2)	LS (3)	PCS (4)	Obs.	Pers.	F-stat.
±3 years							
First stage	0.531***	0.531***	0.531***	0.531***	9066	2331	7.091
	(0.199)	(0.199)	(0.199)	(0.199)			
Second stage	-0.111	0.012	0.133	0.200			
	(0.156)	(0.042)	(0.082)	(0.126)			
±4 years							
First stage	0.551***	0.551***	0.551***	0.551***	12274	3139	13.710
	(0.149)	(0.149)	(0.149)	(0.149)			
Second stage	-0.195	0.030	0.009	0.304**			
	(0.123)	(0.032)	(0.083)	(0.120)			
±5 years							
First stage	0.543***	0.543***	0.543***	0.543***	15040	3880	12.741
	(0.152)	(0.152)	(0.152)	(0.152)			
Second stage	-0.253*	0.058	-0.011	0.250*			
	(0.137)	(0.041)	(0.089)	(0.112)			
±6 years							
First stage	0.594***	0.594***	0.594***	0.594***	17750	4599	11.201
	(0.177)	(0.177)	(0.177)	(0.177)			
Second stage	-0.242	0.055	-0.052	0.215*			
	(0.142)	(0.045)	(0.085)	(0.116)			
±8 years							
First stage	0.560***	0.560***	0.560***	0.560***	22586	5990	15.561
	(0.142)	(0.142)	(0.142)	(0.142)			
Second stage	-0.190	0.045	-0.016	0.195*			
	(0.133)	(0.043)	(0.097)	(0.091)			
±9 years							
First stage	0.552***	0.552***	0.552***	0.552***	24950	6704	24.026
	(0.113)	(0.113)	(0.113)	(0.113)			
Second stage	-0.174	0.033	-0.054	0.147			
	(0.119)	(0.040)	(0.073)	(0.083)			
±10 years							
First stage	0.537***	0.537***	0.537***	0.537***	27236	7419	25.550
	(0.106)	(0.106)	(0.106)	(0.106)			
Second stage	-0.171	0.041	-0.055	0.123			
	(0.106)	(0.034)	(0.072)	(0.084)			

Notes: SOEPv33.1 waves 2002 to 2016. 2SLS regressions. Further, a maximum set of state dummies, year of birth dummies, survey year dummies, linear birth year state trends, and a constant are included. Individual characteristics controlled for include age squared, female, non-German nationality, rural area, and high parental education. Standard errors, reported in parentheses, are clustered at state level; critical values are adjusted to account for the small number of clusters. 90% confidence intervals are reported in brackets. F-stat. denotes the Kleibergen-Paap Wald rk F statistic for weak identification in case of clustered standard errors. * p<0.1, ** p<0.05, *** p<0.01.

Table A.10 IV Estimations – Person-level frequency weighting

	Outcome variables: aggregate health indicators			
	MCS (1)	MCS≤45.6 (2)	LS (3)	PCS (4)
Second stage results				
yeduc	-0.205	0.063	0.075	0.222**
	(0.142)	(0.049)	(0.100)	(0.086)
	[-0.465,0.055]	[-0.026,0.152]	[-0.108,0.259]	[0.065,0.380]
R^2	-0.129	-0.072	0.016	-0.014
Obs.	20290	20290	20290	20290
Pers.	5321	5321	5321	5321
First stage results on yeduc				
Reform	0.556***	0.556***	0.556***	0.556***
	(0.153)	(0.153)	(0.153)	(0.153)
F-stat.	13.252	13.252	13.252	13.252

Notes: SOEPv33.1 waves 2002 to 2016. 2SLS regressions. Observations are weighted by the inverse frequency of the individual observed in the sample. Further, a maximum set of state dummies, year of birth dummies, survey year dummies, linear birth year state trends, and a constant are included. Individual characteristics controlled for include age squared, female, non-German nationality, rural area, and high parental education. Standard errors, reported in parentheses, are clustered at state level; critical values are adjusted to account for the small number of clusters. 90% confidence intervals are reported in brackets. F-stat. denotes the Kleibergen-Paap Wald rk F statistic for weak identification in case of clustered standard errors. * p<0.1, ** p<0.05, *** p<0.01.

Table A.11 IV Estimations – One observation per person

	Outcome variables: aggregate health indicators			
	MCS (1)	MCS≤45.6 (2)	LS (3)	PCS (4)
Second stage results				
yeduc	-0.201	0.086	0.120	0.252**
	(0.178)	(0.075)	(0.084)	(0.106)
	[-0.528,0.126]	[-0.052,0.223]	[-0.033,0.273]	[0.058,0.447]
R^2	-0.127	-0.135	0.008	-0.038
Obs.	5321	5321	5321	5321
Pers.	5321	5321	5321	5321
First stage results on yeduc				
Reform	0.555***	0.555***	0.555***	0.555***
	(0.151)	(0.151)	(0.151)	(0.151)
F-stat.	13.441	13.441	13.441	13.441

Notes: SOEPv33.1 waves 2002 to 2016. 2SLS regressions. For each individual the closest observation to age 67.5 is chosen. If there are two, the one at younger age is taken. Further, a maximum set of state dummies, year of birth dummies, survey year dummies, linear birth year state trends, and a constant are included. Individual characteristics controlled for include age squared, female, non-German nationality, rural area, and high parental education. Standard errors, reported in parentheses, are clustered at state level; critical values are adjusted to account for the small number of clusters. 90% confidence intervals are reported in brackets. F-stat. denotes the Kleibergen-Paap Wald rk F statistic for weak identification in case of clustered standard errors. * p<0.1, ** p<0.05, *** p<0.01.

B. Estimating a LATE at the upper end of the distribution

In the main paper, we have presented consistent evidence for non-existing protective effects of an additional year of education induced by compulsory schooling on individuals' mental health outcomes. However, one inherent characteristic of our identification strategy is, that we are only able to identify a LATE for those individuals who were affected by the compulsory schooling reform. Thus, our results represent a LATE at the lower end of the educational distribution in Germany. To overcome this limitation, in this additional analysis we propose a second instrumental variable to estimate a LATE at the upper end of the educational distribution and contrast the results based on this specification with the findings presented in the main paper.

Institutional Background. Our second instrument for years of schooling is the individual availability of higher education measured by the spatial distance to the nearest university or university of applied sciences.[1] This approach is used, for example, by Card (1995) to estimate earnings returns and by Currie and Moretti (2003) to identify health effects on the newborns of the mothers. In our setting, we compute this spatial distance when the individual is 19 years old as students typically complete secondary education and start tertiary education at this age.[2] Since the place of residence at age 19 is not observed in our data, we resort to the geographic coordinates of the place of birth.[3] Hence, variation in the instrument originates from regional variation and differences across time with new universities opening. Figure A.2 illustrates the regional variation in both places of birth and universities, while Figure A.1 displays the growth of universities over time between 1930 and 1980.

To account for non-linearity, we do not use the absolute measure of the distance in kilometers but instead group individuals in ten deciles according to their relative position in the distribution of the distance across the sample.[4]

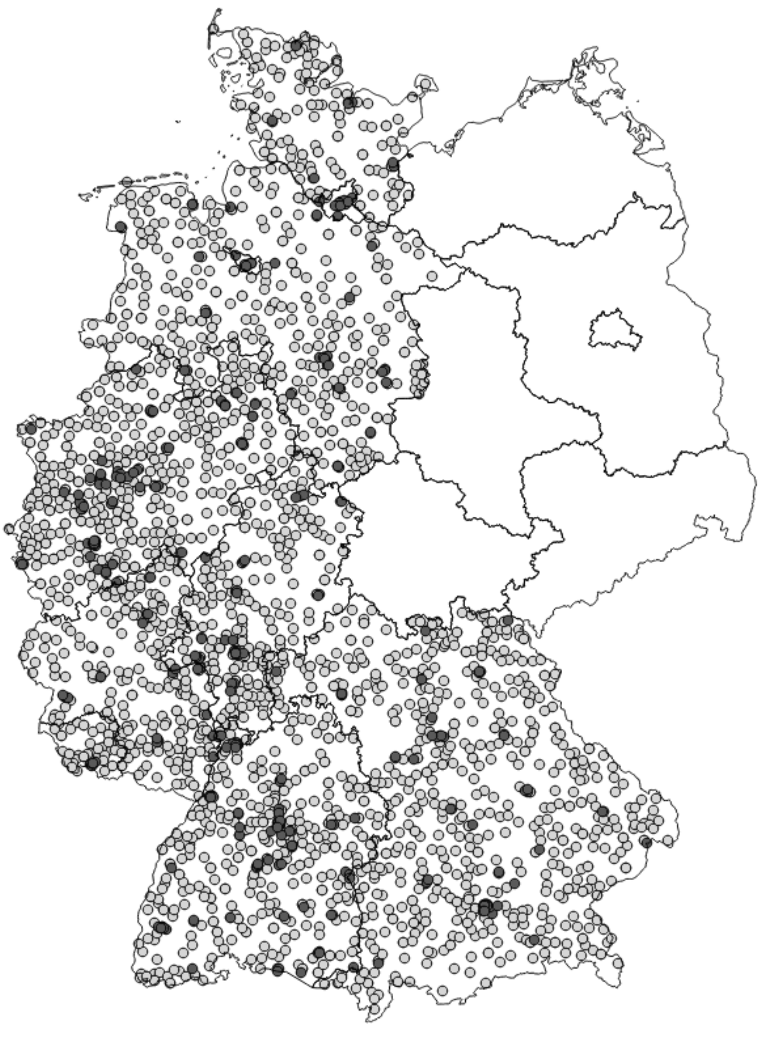
This instrument may induce changes in educational aspirations through different channels, including neighborhood effects and the reduction in transaction costs. Neighborhood effects occur when living close to a university yields a high share of university students in the area, which may stimulate the individual's own educational decision, for example through an information network or through the peer group. Transaction costs are a purely financial argument suggesting that living closer to a university lowers the costs associated with enrolling. For Germany, Spieß and Wrohlich (2010) find that these financial aspects are the main drivers, while neighborhood effects only play a minor role. Importantly, this supports the assumption of monotonicity of this instrument, since rational individuals can be expected to have, not necessarily linear, but monotonic preferences in costs. It is, therefore, unlikely that a closer university offering available education at cheaper costs will induce individuals to attain less education than they would have otherwise.

Validity as Instrument. A threat to the exogeneity of this instrument may be endogenous mobility. To circumvent this problem, we rely on the place of birth of the individual. This is arguably exogenous to any

[1] Compared to universities, universities of applied sciences have a more practical profile and usually grant Bachelor and Master degrees but not doctorates. For ease of notation, we refer to both simply as universities.
[2] As a robustness test we compute the distance to the nearest university around age 10, when in most German Federal States the tracking into different school tracks takes place. Our findings are robust to that change. However, explanatory power of the instrument is weaker compared to the distance at age 19. This reinforces our original choice of using the distance at age 19.
[3] We intentionally do not consider any place or change of residence we can observe in the SOEP in year 1984 and later as these are beyond age 19 and, therefore, not exogenous to the educational decisions of the individuals.
[4] This accounts for outliers with unusually high distances of up to 148 kilometers. In a robustness check we also use the absolute measure which does not alter the results.

educational choices or health developments of individuals themselves. Yet, it may be endogenous to parental characteristics. In particular, higher educated parents may continue living close to the university they studied at when starting their own family. In general, families who highly value education may sort into areas where local universities exist. Therefore, it is important to control for parental education, which we do in all specifications.[5]

Figure A.1 Geographical Location of Birth Places and Universities (West Germany)



Own illustration, *Data Sources:*
○ Places of birth: SOEP waves 2012-2014
● Universities (existent in 2016): Stiftung zur Förderung der Hochschulrektorenkonferenz

Spieß and Wrohlich (2010) show that in the German context, a shorter distance to the nearest university at the time of completing secondary schooling significantly raises educational aspirations: having a shorter distance than the median, which corresponds to 12.5 kilometers, yields a 7 percentage points higher likelihood of enrolling in tertiary education within five years after school completion. Yet, enrolling in university requires

[5] Do (2004) discusses this issue in more detail presenting evidence against such sorting. In addition, Spieß and Wrohlich (2010) argue that in the German context moving probabilities are even lower than in the US. Therefore, conditional on parental characteristics, the presence of a university near the place of birth can be assumed to be exogenous.

students to complete the highest secondary school track, thus earning the Abitur, which is the university entrance qualification. Therefore, changes in the desire to follow tertiary education need to be accompanied by an increase in secondary school attainment. Therefore, we expect that the spatial distance to the nearest university affects not just tertiary, but also secondary, educational decisions. Indeed, we find a highly significant coefficient for this relationship.

Figure A.2 Growth of Universities over Time (West Germany)

Own illustration, *Data Source:*
Universities (existent in the respective year): Stiftung zur Förderung der Hochschulrektorenkonferenz

Estimation sample. Again, we restrict our sample to West Germany, as in East Germany at the time of the German Democratic Republic (GDR), the choice to study was limited and it was impossible to attend a university situated in West Germany. The instrument of the distance to the nearest university is, therefore, less meaningful for individuals growing up in the GDR. Further, the educational systems historically differed between both parts of Germany, such that educational degrees are not directly comparable. However, in contrast to the identification strategy based on the reform of the compulsory schooling legislation, more detailed information on the geographic location is necessary to compute exact distances to existing universities. Ideal would be the place of residence at age 19, when the decision of starting tertiary education is usually made. Any later place of residence may be endogenous if individuals move to and stay where they study. However, since we do not observe individuals at the age of 19, we use the place of birth as a proxy. We use the latitude and longitude coordinates of the center of the municipality where the individual was born.[6] To calculate the distance to the nearest university or university of applied sciences, we use data provided by the *Stiftung zur Förderung der Hochschulrektorenkonferenz*, the foundation supporting the German Rectors' Conference, on the location and opening years of all currently existing universities and universities of applied sciences.[7] For each individual, the resulting measure is the minimum of the distances from his or her place of birth to each university which was existent when the individual was 19 years old.

Table A.12 shows descriptive statistics for this *Minimum Distance* sample. The numbers show that this sample is comparable in observable characteristics with our main estimation sample. It is important to note that, by construction, non-German nationality is very low in the *Minimum Distance* sample, as only individuals with birth places in Germany are considered, and that the average age differs as our main sample is intentionally restricted in birth years around the introduction of the compulsory schooling reform.

Table A.12 Descriptive statistics (Minimum Distance Sample)

	Main Sample	Minimum Distance Sample
(Non-standardized) aggregate health measures		
MCS	51.644	52.053
MCS$\leq$45.6	0.245	0.231
LS	7.203	7.250
PCS	47.449	46.526
Instruments		
Reform	0.453	
Minimum Distance (in km)		21.234
Control variables		
yeduc	10.052	9.950
Age	58.985	62.861
Female	0.512	0.509
Non-German nationality	0.035	0.003
Rural area	0.261	0.271
High parental education	0.132	0.124
Year of birth	1950.663	1947.421

Notes: SOEPv33.1 waves 2002 to 2016, different samples. The distribution of the aggregate health measures prior to standardization is shown. Hence, the numbers for MCS, LS, and PCS correspond to the original scale provided in the data.

[6] The municipality is defined via the municipality key (*Gemeindekennzahl*), with 11,306 different municipalities defined for Germany in 2016 (see https://www.riserid.eu/glossarbegriffe/source/default/term/amtlicher-gemeindeschluessel-ags/). This data was obtained retrospectively in the 2012, 2013, and 2014 waves. For data protection reasons, it is not included in the usual SOEP data distribution, but instead is only available on-site at the SOEP research data center.

[7] All distances are obtained using the geographical information system QGIS and measure the airline distance between the center of the municipality of birth and the visiting address of the university.

Results. The results of this additional analysis are shown in Table A.13. Using deciles of the distance to the nearest university as instrument, we find a significant negative coefficient in the first stage, which means education levels decrease, the further away (available only at higher costs) the nearest university was when the individual was 19. The coefficient of -0.05 suggests that moving down, for example, five deciles in the distribution of the distance to the nearest university increases schooling by a quarter of a year. This corresponds, for example, to moving from the 6th decile in which students live, on average, around 19 kilometers away from the closest tertiary education institution to the group with the nearest university at 800 meters. Using the availability of higher education as our instrument, we find a point estimate for MCS of -0.028 standard deviations being very close to zero. The 90% confidence interval ranges from -0.207 to 0.151 or, in absolute terms, from -2.0 to +1.5 MCS points. Although this estimate is somewhat more symmetric around zero, again the effect is neither statistically significant nor relevant in medical terms. Columns 2 and 3 show the estimation results for specifications using the indicator of being at risk of developing symptoms of mental health disorders and the continuous measure of subjective well-being. In both cases, the estimates are small and statistically not significant. In summary, the results from this additional exercise confirm the findings from our main estimation.[8] We interpret our results as evidence that the finding of a non-existent protective effect of education on mental health is not specific to the situation of the very low educated but can also be found at the upper end of the educational distribution. This is in line with findings by Kamhöfer et al. (2018) who find no causal effect of tertiary education on mental health in Germany.

Table A.13 IV Estimations – Effect of Education on Health Indicators at the upper end of the distribution

	Outcome variables: aggregate health indicators			
	MCS (1)	MCS≤45.6 (2)	LS (3)	PCS (4)
Second stage results				
yeduc	-0.028	0.002	0.041	0.119
	(0.098)	(0.034)	(0.094)	(0.086)
	[-0.207,0.151]	[-0.060,0.063]	[-0.132,0.214]	[-0.039,0.278]
R^2	0.005	0.009	0.020	0.038
Obs.	25270	25270	25270	25270
Pers.	5576	5576	5576	5576
First stage results on yeduc				
Min. Distance	-0.050***	-0.050***	-0.050***	-0.050***
	(0.009)	(0.009)	(0.009)	(0.009)
F-stat.	31.360	31.360	31.360	31.360

Notes: SOEPv33.1 waves 2002 to 2016, Minimum Distance Sample. 2SLS regressions. Further, a maximum set of state dummies, year of birth dummies, survey year dummies, linear birth year state trends, and a constant are included. Individual characteristics controlled for include age squared, female, non-German nationality, rural area, and high parental education. Standard errors, reported in parentheses, are clustered at state level; critical values are adjusted to account for the small number of clusters. 90% confidence intervals are reported in brackets. F-stat. denotes the Kleibergen-Paap Wald rk F statistic for weak identification in case of clustered standard errors. * p<0.1, ** p<0.05, *** p<0.01.

[8] In an alternative specification, we include district fixed effects instead of state fixed effect. Our findings are robust to that change. Results are available upon request.

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