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More Education Does Make You Happier—Unless You Are Unemployed

Alexander Bertermann, Daniel A. Kamhöfer, Hannah Schildberg-Hörisch*

September 2023

Abstract

This paper investigates the causal effect of education on life satisfaction, exploring effect heterogeneity along employment status. We use exogenous variation in compulsory schooling requirements and the build-up of new, academically more demanding schools, shifting educational attainment along the entire distribution of schooling. Leveraging plant closures and longitudinal information, we also address the endogeneity of employment status. We find a positive effect of education on life satisfaction for employed individuals, but a negative one for those without a job. We propose an aspiration-augmented utility function as a unifying explanation for the asymmetric effect of education on life satisfaction.

JEL-codes: I26, I31, C26

Keywords: Education; Life satisfaction; Employment status; Compulsory schooling reforms; School openings; Instrumental variable estimation

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I can't get no satisfaction.

Mick Jagger, shortly after dropping out of the London School of Economics in 1963

1 Introduction

Do higher levels of education increase life satisfaction? While there is broad consensus that education improves many important life outcomes such as earnings and health (Heckman et al., 2018), empirical evidence on its causal effect on overall life satisfaction is surprisingly scarce and ambiguous. A better understanding of the link between education and life satisfaction is, however, of great importance. As many individuals (Fleurbaey and Schwandt, 2015) and governments (Stiglitz et al., 2009) aim at maximizing well-being, a lack of education to increase life satisfaction could question the rationale behind both individual investments into education and public subsidies for it.

This paper goes beyond previous work on the link between education and life satisfaction in several respects. We do not only add to the scarce, causal previous evidence, but extend it in important ways. First, we use new sources of exogenous variation in education that allow us to shed light on the effect of education on life satisfaction for individuals along the whole schooling distribution. By contrast, previous work focuses on individuals with low levels of schooling by exploiting arguably exogenous changes in minimum schooling requirements, see Oreopoulos (2007) for evidence from the UK and Ireland, Banks and Mazzonna (2012) for the UK, Dahmann and Schnitzlein (2019) and Elsas (2021) for Germany, de New et al. (2021) for Australia, and Dursun and Cesur (2016) for Turkey.[1] For comparison purposes, we start as well by exploiting changes in the minimum schooling requirements in West Germany (see, e.g., Margaryan et al., 2021). The federal states increased the number of mandatory years of schooling from 8 to 9 years for students in basic schools (Hauptschulen), the then predominant secondary school track, between 1949 and 1969. West Germany also provides an ideal setting for studying the causal effect of years of schooling on life satisfaction for individuals with

[1]See Appendix A.1 for a more comprehensive discussion of previous work on the causal effect of education on life satisfaction and Table A1 for correlational studies.

more than the mandatory years of schooling. After World War II, West Germans experienced an unprecedented number of openings of intermediate schools (Realschulen) and high schools (Gymnasien) that offer 10 and 13 years of schooling, respectively. This massive increase changed the opportunity costs of attending higher school tracks substantially, e.g., by decreasing the distance to the nearest, more academically demanding school (see e.g., Jürges et al., 2011, and Mazumder et al., 2023, for the build-up of new schools as source of exogenous variation in education). As the federal states are in charge of education policy, this expansion in schooling opportunities exhibits both regional and temporal variation that is arguably independent of other determinants of the affected individuals' life satisfaction decades later. Therefore, we use the two types of school openings as a further, novel source of exogenous variation to estimate the causal effect of schooling on life satisfaction. Leveraging the three different sources of exogenous variation in years of schooling in an instrumental variable approach, we find a solid null effect of years on schooling on life satisfaction that equally applies to individuals in the lower, middle, and upper part of the schooling distribution. Importantly, our results suggest that previous evidence on the link between education and life satisfaction is not an artefact of focusing on people with minimum years of education, but applies more generally.

As our second main contribution, we continue by digging deeper into this null result. Although we are not the first to document the absence of an effect of education on life satisfaction (see, e.g., Banks and Mazzonna, 2012; Boyce, 2010; Dahmann and Schnitzlein, 2019; de New et al., 2021), this finding seems surprising at first sight. On average, people with higher levels of education have higher incomes and better labor market outcomes (Gunderson and Oreopolous, 2020) which should result in higher life satisfaction. Focusing on individuals with a job, we indeed document positive, causal effects of years of schooling on satisfaction with pay and work that also translate into higher overall life satisfaction. By contrast, more years of schooling hurt the life satisfaction of non-employed individuals, in a very similar manner for unemployed individuals and those out of the labor force for other reasons. The difference in effect sizes for employed and non-employed individuals is highly significant and the implied magnitudes are substantial. For example, for employed individuals, the effect of one additional year of schooling is similar in magnitude to changing from being single to partnered (see the review by

Frijters et al., 2020). The opposite-signed effect size for non-employed individuals is even more than twice as large. Hence, it is not the case that education does not impact life satisfaction. Instead, we demonstrate that education does play an influential role for overall satisfaction with life—however, in a way that strongly differs for employed and non-employed individuals.[2]

We employ two different strategies to investigate whether selection into employment by education drives these results. First, we follow a large literature that leverages plant closures to isolate arguably exogenous variation in unemployment (see, e.g., Browning and Heinesen, 2012, Del Bono et al., 2012, and Huttunen and Kellokumpu, 2016). Second, we focus on within-individual variation in employment, estimating two separate individual fixed effects models for individuals with lower and higher levels of instrumented education. Both sets of results confirm our main finding. They thereby indicate that the endogeneity of the employment status is unlikely to be responsible for the opposite-signed effects of years of schooling on life satisfaction for employed and non-employed individuals.

In a final step of our analysis, we demonstrate that adding aspirations on top of realized outcomes to people's utility function provides a unifying explanation for the asymmetric effect of education on life satisfaction by employment status. In particular, unmet aspirations represent a possible mechanism why higher education may hurt the life satisfaction of non-employed individuals. We empirically apply this model, focusing on the income domain. A mediation analysis reveals that income and (un)met income aspirations alone can jointly explain more than 55 percent of the baseline estimates for the effect of years of schooling on life satisfaction, both for employed and non-employed individuals. While both income and income aspirations matter for employed and non-employed individuals, the increase in life satisfaction in years of schooling for employed individuals seems to materialize largely through higher income; the reduced life satisfaction of better educated, non-employed individuals through unmet income aspirations.

Our findings have important implications. By uncovering the so far overlooked asymmetric effect of education on the life satisfaction of employed and non-employed individuals, we offer possible explanations for the mixed previous results on the link between education and

[2]Given that only a few studies exploit arguably exogenous variation in education, a meta-analysis on the role of employment status for the effect of education on life satisfaction is not feasible.

life satisfaction: sample compositions that differ in the share of employed and non-employed individuals or differences in norms and institutions that may affect how strongly non-employed individuals' life satisfaction suffers from not being active in the labor market.

Moreover, our finding that higher educational attainment reduces the life satisfaction of non-employed individuals presents an additional layer of concern regarding decreasing labor force participation rates. While labor force participation rates are increasing in many European countries, fueled by demographic change, the United States and Canada witnessed a steady decline in labor force participation of individuals with intermediate and higher education in the last two decades, see Appendix Figure A2. Our results suggest that, if this trend continues, the toll of non-employment on population-wide life satisfaction may even increase in the future.

In addition, our results enhance our understanding of the consequences of unemployment. While the overall decline in life satisfaction due to unemployment is a well-established finding (see, e.g., the meta-analyses of Paul and Moser, 2009, and McKee-Ryan et al., 2005), we go beyond previous work by documenting a sizable education gradient in the associated pain from experiencing unemployment. Policy traditionally focuses on helping individuals with lower education to cope with unemployment, based on the assumption that individuals with higher education are better at adapting to unemployment (CEA, 2016). This assumption is certainly correct for coping with the financial consequences of unemployment and the prospects of finding a new job. However, our results highlight that better educated people face higher psychological costs of unemployment, partly due to a larger gap of aspirations and realized outcomes. This underlines that they as well are an important target group for public support. Given the effectiveness of cognitive behavioral therapy to treat depressive disorders in adults (see the meta-analyses by Cuijpers et al., 2013, 2023) and the large potential this has for economic outcomes (Chisholm et al., 2016; de Quidt and Haushofer, 2019; Knapp and Wong, 2020), traditional active labor market policies might be complemented with psychological counseling.

Finally, the fact that aspirations raise with education implies that higher levels of education only translate into higher life satisfaction for individuals who are successful on the labor market. This finding should be communicated broadly as it has important implications for the choice which level and kind of qualification to obtain. For example, Müller (2021) shows that a nonnegligible share of German students only plans to attend college due to expectations to do so by their college-educated parents. Often these are students with lower grades. Less motivated and able students who attend college mainly to conform parental aspirations will be less likely to excel in their chosen educational track, worsening their prospects for both labor market outcomes and life satisfaction.

The remainder of the paper is structured as follows. In Section 2, we describe our estimation sample and briefly explain the German education system. This sets the stage for Section 3 that outlines the empirical framework and discusses the three instruments that we use to obtain exogenous variation in years of schooling. Section 4 presents results, before Section 5 discusses potential mechanisms behind our findings. Section 6 concludes.

2 Data and institutional setting

Our analysis combines data from several different sources. We use measures of subjective well-being, years of schooling, employment status, and further socio-economic and demographic control variables from the German Socio-Economic Panel (SOEP). The SOEP provides population-representative longitudinal data for about 30,000 individuals in 19,000 households who are surveyed annually (Goebel et al., 2019). To address people's self-selection into years of schooling by an instrumental variables approach, we match the SOEP data with (i) information on compulsory schooling legislation in the various German states from Pischke and von Wachter (2005) and (ii) data on the regional number of intermediate and high schools, taken from various issues of the German Statistical Yearbook (Statistisches Bundesamt, 1990).

2.1 Estimation sample

Sample composition. Our estimation sample is composed as follows: First, we focus on West German non-city states (*Flächenländer*) because our instruments do not apply to East Germany and cross-border commuting to schools outside of the state may jeopardize a clear instrument assignment for the city states (Hamburg, Bremen, West Berlin). Second, we zero in on prime working age individuals with completed years of schooling, but before (early) retirement, i.e., those aged 25–60.[3] We additionally drop individuals who retired in the previous year or left their job due to age restrictions. Third, to ensure instrument availability we focus on the birth cohorts 1940–1980. Fourth, as we use cross-sectional sample weights to make our final sample representative of the German population, only observations with a non-zero sample weight enter the estimation sample. Finally, we drop observations with missing values in variables that are key to our analysis (life satisfaction, years of schooling, and employment status).

Because of item non-response, temporary dropout, sample attrition, refreshment samples, and the age restriction for our sample, the number of observations varies across survey waves. To increase the number of observations and to capture more variation, we combine respondents from different waves. We start with all respondents with complete information in the year 2000 (about 11,000 respondents, 40 percent of our final estimation sample). An advantage of using information from 2000 is that enough respondents are affected by the instruments we employ, while, at the same time, we measure the long-term effects of education on life satisfaction. For some respondents, we have complete information only in another wave. In an iterative process, we therefore add respondents with complete information in later and earlier waves to our estimation sample. When we do not have complete information in the 2000 interview, we first check whether information on a respondent is complete in one of the following waves (wave by wave, starting in 2001 and moving up to 2012 when respondents that are affected by our instruments are outside the considered age range). We thereby also capture a refreshment sample, added to the SOEP in 2010, that increases the number of observations by about 4,500 (about 16 percent of the estimation sample). In a final step, we employ the iterative process backwards, moving from the 1999 to the 1985 wave[4] to add respondents that were part of the SOEP before 2000, but had dropped out by or were too old in 2000. Our final estimation sample consists of 27,714 respondents, each observed in one wave of the SOEP.

Figure A3 shows the resulting number of observations per wave, Figure A4 the distributions of age at interview and year of birth. 86 percent of the respondents in our estimation sample are observed in 2000 or a more recent wave as the forward-moving, iterative process adds a

[3]In Germany, it is common to retire early. We thus exclude people older than 60 years from our analysis because being out of the labor force in that age range will not be perceived in the same way as at younger ages.

[4]We discard the first wave of the SOEP in 1984 because it uses a different definition of employment status.

large number of respondents to the estimation sample that have just become 25 years old. As a robustness check, we will demonstrate that our main results remain very similar when we construct the sample by starting with a different baseline year than 2000.

Key variables. Our main outcome variable is *overall satisfaction with life*, a summary measure of individual well-being. Following much of the literature, life satisfaction is measured as response to the question "How satisfied are you with your life, all things considered?", using a Likert-scale that ranges from 0 (= completely dissatisfied) to 10 (= completely satisfied). This question is included in every wave of the SOEP and exhibits little item non-response (on average, less than 0.01 percent per survey wave). This type of single-item measure exhibits psychometric properties very similar to multiple-item scales (Cheung and Lucas, 2014). Oswald and Wu (2010) demonstrate that the regional average of subjective life satisfaction correlates with objective measures of quality of life. The self-reported measure is also related to friends' assessment of one's life satisfaction (Layard, 2010). Kahneman and Krueger (2006) and Diener et al. (2013) provide further evidence on the validity of life satisfaction measures and discuss issues related to their measurement.

Figure A5 presents a histogram of responses. The distribution is left-skewed. Although the modal response is 8, rather few respondents report a life satisfaction of 9 or 10, demonstrating the absence of major ceiling effects. In our analysis, we will use standardized life satisfaction with mean 0 and standard deviation 1 to ease the interpretation of results in our preferred specification. In addition to the item about general life satisfaction, the SOEP data include similar items asking about the respondents' satisfaction with certain aspects of life, e.g., satisfaction with pay and work. We use these items to gain a better understanding of the main effects.

The main variable of interest is *years of schooling*, calculated using respondents' highest German school-leaving qualification. It ranges from 8 years for those with a basic school degree before the compulsory schooling reform to 13 years for those with a university entrance degree.

Summary statistics. Table A2 provides summary statistics for the main estimation sample. Mean age is 40.5 years and 52 percent of respondents are female. On average, respondents have 10.4 years of schooling and 12.2 years of education (that is, schooling plus post-secondary education). While close to 70 percent got vocational training after leaving secondary school, 21 percent attended university. Labor force participation is about 72 percent, slightly more than 7 percent of respondents are unemployed, and 21 percent are out of the labor force. The most common reasons for job termination are own resignation (34 percent), followed by dismissals (19 percent), leave of absence (16 percent), expiration of a temporary contract (11 percent), mutual agreement (10 percent), and plant closure (4 percent).

2.2 Institutional setting

Although the federal states are in charge of school policy in Germany, secondary schooling mainly consisted of three distinct tracks in all states in the time under review. After four years in elementary school (Grundschule), children around the age of 10 are sorted in one of three tracks. The school tracks have different curricula and their students obtain distinct school-leaving certificates. Basic schools (Hauptschule) award degrees after a total of 8 or 9 years of schooling, while intermediate schools (Realschule) are more academically demanding and require a total of 10 years of schooling. After a total of 13 years, students who successfully attended high schools (Gymnasium) receive the most prestigious school-leaving degree in the secondary schooling system that grants access to university education (Abitur). A fourth type of school, comprehensive schools (Gesamtschule), emerged in the late 1960s but only started to increase in importance in the 1980s. In principle, comprehensive schools allow graduating with any of the three secondary school-leaving degrees. In practice, most students acquire an intermediate school degree (Lundgreen, 2008). For this reason and because of the minor numerical relevance of comprehensive schools in the time under review, we treat them as intermediate schools in the empirical analysis; see Kamhöfer and Schmitz (2016) for a similar approach.

The tracking of students into the different types of secondary schools is formally based on their previous performance and, in some states, additionally reflects their parents' preferences. However, the availability and physical distance of the desired school type mattered as well. Especially in rural areas, many villages had a basic school, but not a high school. In these villages, families faced the trade-off between sending their child to the nearby basic school or to find some commuting arrangement to the next high school.

To address endogeneity in people's years of schooling, we will make use of three instruments, that we describe in the following.

3 Estimation strategy

3.1 OLS and 2SLS estimation

To assess the effect of years of schooling on life satisfaction, we begin by estimating the following equation

$$LS_{i,stl} = \alpha_0 + \alpha_1 Educ_{i,stl} + \alpha_2 X_{i,stl} + \omega_s + \tau_t + \iota_l + \nu_{i,stl} \ , \tag{1}$$

by Ordinary Least Squares (OLS) regression. $LS_{i,stl}$ denotes the outcome variable, life satisfaction of individual i in state s, born in year t, and surveyed in year l. The variable of interest, years of schooling, is denoted by $Educ_{i,stl}$. ω_s, τ_t, and ι_l are state, birth cohort, and survey year fixed effects, respectively, while $X_{i,stl}$ is a vector of additional control variables (in the main specification, only an indicator for gender). In our preferred specification, we do not control for income, health, marital status, and further predictors of life satisfaction because these variables are likely endogenous with respect to education. Controlling for them would thus not yield our parameter of interest—the total effect of education on life satisfaction that allows education to operate through channels such as income (instead of controlling for them). $\nu_{i,stl}$ is the error term. We apply sampling weights in all estimations in order to ensure that the sample is population-representative.

The coefficient α_1 gives the association between an additional year of schooling and life satisfaction. It is unlikely to reflect the causal effect of schooling, as individuals are likely to select themselves into post-mandatory schooling based on idiosyncratic preferences and unobserved attributes such as cognitive skills, openness to new experiences, eagerness to learn new things, and self-esteem. If such factors are also correlated with life satisfaction, α_1 will suffer from omitted variable bias. To address the resulting endogeneity concern, we employ an instrumental variables (IV) approach, using the two stage least squares (2SLS) estimator.

The second-stage equation of the 2SLS approach for the effect of education on life satisfaction is

$$LS_{i,stl} = \beta_0 + \beta_1 \widehat{Educ}_{i,stl} + \beta_2 X_{i,stl} + \omega_s + \tau_t + \iota_l + \epsilon_{i,stl} \tag{2}$$

where $\widehat{Educ}_{i,stl}$ denotes the fitted values for years of schooling based on the first-stage equation. The other variables are as in equation (1), the error term is $\epsilon_{i,stl}$.

The first-stage equation reads

$$Educ_{i,stl} = \delta_0 + \delta_1 Z_{st} + \delta_2 X_{i,stl} + \omega_s + \tau_t + \iota_l + \xi_{i,stl} \ , \tag{3}$$

where Z_{st} denotes the instrument(s). The idea is that variation in Z (conditional on X) is orthogonal to unobserved characteristics that could confound the relationship between education and life satisfaction in equation (1) such that variation in education caused by Z can be used to identify the causal effect of education on life satisfaction. Following Angrist and Pischke (2008, ch. 4.4.1), we distinguish four identifying assumptions to assess the instruments' validity: the independence assumption, the exclusion restriction, relevance, and monotonicity. We present the instruments and discuss the implication of these assumptions in the following.

3.2 Instruments

3.2.1 Compulsory schooling reforms

In the aftermath of World War II, many industrialized countries increased the number of compulsory years of schooling (Harmon, 2017). In West Germany, the basic school degree was initially awarded after the eighth grade. Between 1956 and 1969[5], nearly all of the German states required a mandatory ninth grade to obtain a degree. While students are not required to graduate from basic schools with a degree, it has become illegal and practically infeasible to drop out of school before nine years of attendance after the compulsory schooling reform. Given the states' sovereignty regarding education policy, the introduction of the compulsory schooling reform took place in different years across states. For each state, Table 1 displays the year in which the ninth grade was introduced, the first birth cohort affected, and the share of students attending basic schools. Taken together, whether an individual was affected by the compulsory schooling reform depends on the visited school track, year of birth, and state of

[5] Already in 1949 in Hamburg, one of the three city states that we do not use in our main analysis.

residence during secondary schooling.[6]

Table 1: Introduction of the mandatory ninth grade by state

State	(1) First affected graduation year	(2) First affected birth cohort	(3) Share of students in basic schools (in percent)
Baden-Wuerttemberg	1967	1953	77.3
Bavaria	1969	1955	81.1
Bremen	1958	1943	73.4
Hamburg	1949	1934	74.2
Hesse	1967	1953	72.4
Lower Saxony	1962	1947	78.0
North Rhine-Westphalia	1967	1953	76.9
Rhineland-Palatinate	1967	1953	82.4
Saarland	1964	1949	83.1
Schleswig-Holstein	1956	1941	71.4

Notes: Information in columns (1) and (2) is taken from Pischke and von Wachter (2005). Column (3) stems from Cobb-Clark et al. (2022a) and relies on information from the 1967 German Statistical Yearbook (Statistisches Bundesamt, 1990).

Various empirical approaches can be used to exploit the variation induced by the series of reforms. All methods have in common that they compare the outcomes from birth cohorts before and after the reform. The staggered introduction across states additionally allows for a cross-sectional comparison of the same birth cohort in different states. We exploit this cross-sectional and temporal variation by using an indicator variable for whether an individual had to obtain 8 or 9 years of compulsory schooling as an instrument for the individual years of schooling.

Students who attended intermediate or high schools irrespective of the reform were not affected by the increase in compulsory years of schooling because they obtained 10 or more years of schooling anyhow (always-takers in the terminology of Angrist et al., 1996). The compulsory schooling reform effectively shifted the years of schooling for students in basic schools, more than 70 percent of students at the time of the reform (see column (3) in Table 1).[7] The mandatory character of the reform means that never-takers are practically ruled out

[6] When available, we use information on the state of graduation from secondary school. If the state of graduation is missing, we use current state of residence as a proxy. As migration across states is relatively low (Jürges et al., 2011), instrument mis-assignment is not a major concern (Pischke and von Wachter, 2005).

[7] In some states, the increase in compulsory schooling coincided with the shift of the school year's start from spring to end of summer such that the first cohort affected by the reform had only one-third of a year more schooling (Pischke, 2007). As a result, the effect of schooling is slightly underestimated when using the

(Angrist and Pischke, 2008, ch. 4.4.2). Students who lived near a state border may have decided to go to a basic school in the neighboring state where the compulsory schooling reform was not yet implemented. However, the number of students for which this was a realistic option is very small such that this scenario can be neglected.

In terms of identifying assumptions, the compulsory schooling reform's independence and the exclusion restriction require that the increase in mandatory years of schooling is independent of unobserved determinants of life satisfaction and that it only affects life satisfaction through educational attainment (direct effect) or channels that are themselves the result of education (indirect effect, e.g., through income). Although it is not possible to test these assumptions, the institutional setting makes them plausible and they are regularly maintained in the previous literature. Even if the timing of the compulsory schooling reform was correlated with policies that might have influenced life satisfaction at the time, this is not threatening instrument validity per se. The schooling reform affected students whose life satisfaction we analyze as adults, i.e., decades later. As long as possibly correlated policies had only temporary impacts on education or life satisfaction, they do not require further investigation. Nevertheless, we control for state expenditures other than on schooling as a robustness check and demonstrate that our results remain very similar. The instrument's relevance assumption that the compulsory schooling reform increases years of schooling seems almost tautological in our setting. Moreover, the first stage estimation allows to investigate instrument relevance. Finally, it is highly unlikely that students attend a basic school instead of an academically more demanding school track because of the compulsory schooling reform. Thus, defiers do not exist and the monotonicity assumption is fulfilled.

The reforms have been used to analyze the effects of education on several important life outcomes.[8]

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compulsory schooling reform as an instrument.

[8]Pischke and von Wachter (2008) were the first to exploit the reform to estimate the effect of education on earnings. The zero effect they document is confirmed by Kamhöfer and Schmitz (2016) but challenged by Cygan-Rehm (2022). Other studies use the reform to estimate the effect of education on health (Begerow and Jürges, 2022; Kemptner et al., 2011), fertility (Cygan-Rehm and Maeder, 2013), political participation (Siedler, 2010), attitudes towards immigration (Margaryan et al., 2021), crystallized intelligence (Kamhöfer and Schmitz, 2016), self-control (Cobb-Clark et al., 2022a), as well as patience and risk preferences (Tawiah, 2022). Although their focus is on mental health, Dahmann and Schnitzlein (2019) also consider life satisfaction as an outcome of education. Using a different data source and specification but the same reforms, Elsas (2021) also analyzes the effect of education on life satisfaction, see Appendix A.1.

3.2.2 Expansion of academically more demanding school tracks

The further two instruments we will use refer to the supply of intermediate and high schools. Overall, their number increased rapidly in the 1960s and 1970s. For example, the state of North Rhine-Westphalia had 278 intermediate schools in 1960 but 605 in 1980 (Statistisches Bundesamt, 1990). During the same time period, the number of high schools increased from 465 to 645. By contrast, the number of basic schools decreased by more than 30 percent. As the federal states are in charge of education policy, the intensity and speed of the build-up of schools varies substantially across states. Figure 1 illustrates the number of intermediate and high schools per 100 square kilometers (for comparability across states) over time and in the various states. The changing relevance of these school types is also reflected in their number of students. While 78 percent of students were enrolled in a basic school in 1950, this number fell to 34 percent in 1990. During this period, the share of students in intermediate schools (including *Gesamtschule*) increased from 7 to 35 percent and the one in high schools from 15 to 31 percent (BMBW, 1992).

The number of intermediate and high schools are promising instruments for years of schooling as each additional school improves access to post-compulsory education. When an intermediate or high school opened in an area where there was none before, the opportunity costs of attending this specific school track decreased. While the reasons for the openings of additional academically more demanding schools were manifold, the education expansion after World War II was generally seen as a means to economic and technological progress (Hadjar and Becker, 2006). Moreover, education was considered as a prerequisite for a functioning democracy and, therefore, an important factor in its own right (Dahrendorf, 1966). Electoral cycles and other political and bureaucratic factors also affected the specific timing of the build-up of new schools (Mayer et al., 2009). These variations from the overall trend (our regression models control for year fixed effects) provide arguably exogenous variation in educational opportunities across states. We therefore use the number of intermediate and high schools, respectively, in a state per 100 square kilometers at an individual's age of 9 as instruments for the years of schooling.[9]

[9]In the context of health outcomes, Jürges et al. (2011) use high school supply in Germany as an instrument for education, while Kamhöfer and Schmitz (2016) instrument education with the availability of intermediate and high schools to estimate the effect of education on earnings and cognitive abilities.

Students proceed to secondary schooling at the age of 10 on average, but the decision which type of school to attend is made earlier, typically at the age of 9.

In addition to year fixed effects that remove the nationwide time trend in the number of high and intermediate schools, our regression models also control for state fixed effects to, amongst other things, account for a different, potentially endogenous initial number of schools across states. We thus effectively use state deviations from the nationwide trend in the number of schools. The independence assumption (exclusion restriction) requires that time-varying, state-specific factors that are correlated individuals' life satisfaction decades later did not drive (coincide with) variation in the number of intermediate and high schools. These assumptions entail that there are no factors that either cause school openings (independence) or coincide with school openings (exclusion) that are relevant for life satisfaction decades later. This concern is greatly mitigated by the fact that most potential candidates for such policies, like the degree of the social safety net, are not decided on the state level but by the federal government. The only other policy area besides education (and culture) over which states have authority is law enforcement. While we are not aware of any direct links between education policy and law enforcement policy, budgetary aspects may affect both policy areas at the same time (although it is a priori unclear whether a correlation might be positive or negative). However, possibly confounding law enforcement policies are only a matter of concern if they affect citizens' long-term life satisfaction.[10] Although this seems highly unlikely, we will consider states' non-educational expenditures as a control variable in a robustness check. The instruments' relevance can again be tested in the first stage regression. Monotonicity would be violated if individuals do only attend an intermediate or high school if these schools are further away but not if they are close by. In our setting, this is not a likely scenario.

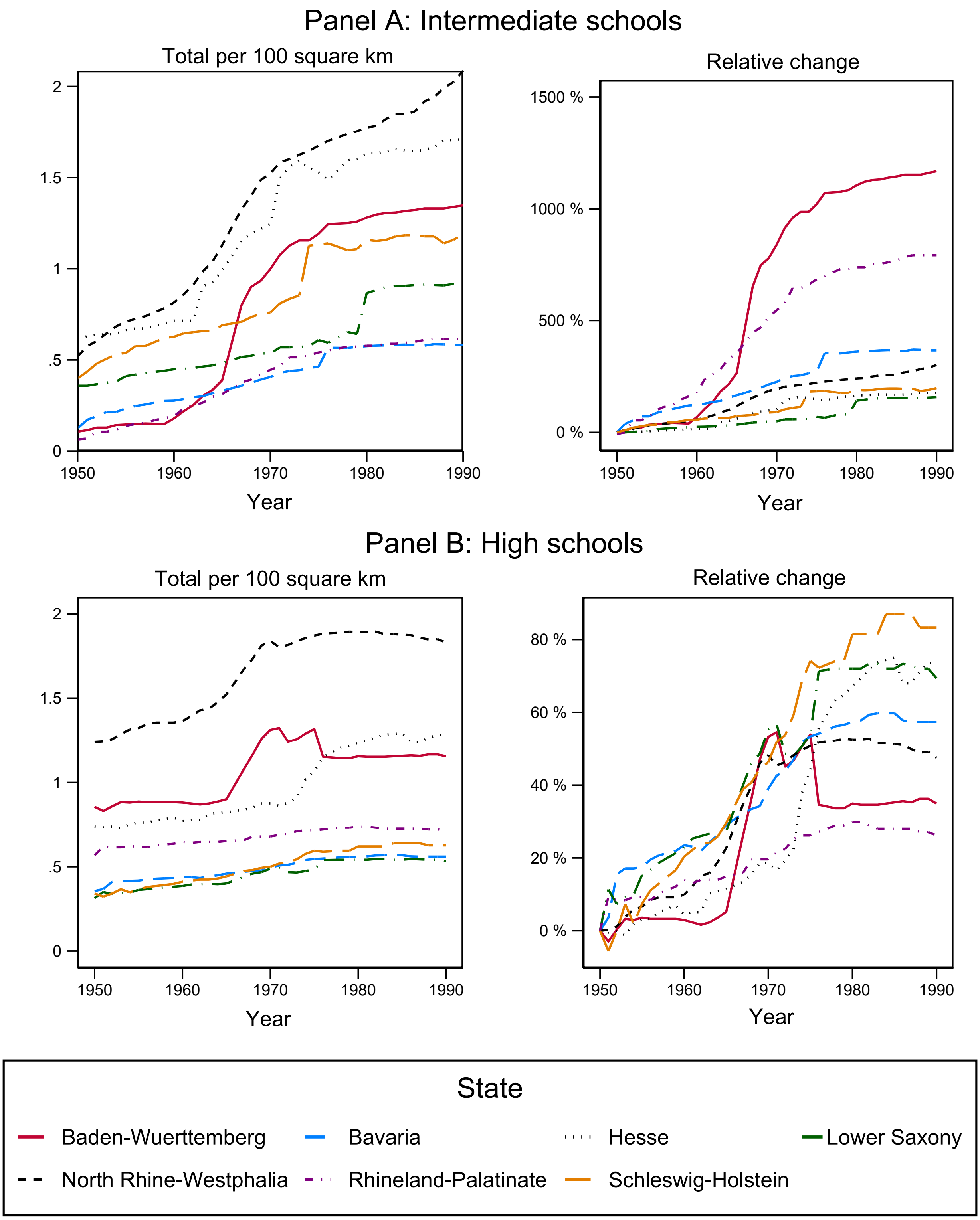
3.2.3 Complying subpopulations

There is a conceptual argument in favor of running a separate 2SLS model for each of the three instruments, yielding three different estimates for $\hat{\beta}_1$ in three second-stage equations: the different instruments, Z_{st}, shift the years of schooling of different individuals. The com-

[10]In particular, school supply at the age of 9 influences the type of secondary track students attend. People enter our sample at the age of 25 the earliest, i.e., at least 16 and on average 32 years later.

pulsory schooling reform affects basic school students. The number of intermediate and high schools varies the access barriers of prospective intermediate or high school students to attend their preferred school track. For each instrument, the affected subpopulation—the compliers—corresponds roughly to one of the three school tracks in Germany (although not each student in each track is necessarily affected by the instrument). The three estimates $\hat{\beta}_1$ will thus yield distinct local average treatment effects (LATE, Imbens and Angrist, 1994), i.e., they state the causal effect for instrument-specific compliers. Importantly, our three instruments enable us to go beyond existing evidence by empirically investigating if the relationship between years of schooling and life satisfaction differs along the distribution of years of schooling. If the effect of years of schooling does differ across instrument-specific compliers, the three 2SLS approaches will yield different LATEs. By contrast, if the three point estimates are statistically indistinguishable, the effect of years of schooling is homogenous (at least among the complying subpopulations). In that case, the instruments can be used in a joint first stage and Z in equation (3) represents a vector of instruments.

Figure 1: Intermediate and high schools per state, 1950–1990



Notes: Data are taken from various issues of the German Statistical Yearbook (Statistisches Bundesamt, 1990). Comprehensive schools are treated as intermediate schools.

4 Results

4.1 The overall effect of years of schooling on life satisfaction

Table 2 displays our estimation results on the relationship between years of schooling and satisfaction with life. We start by showing the descriptive OLS results in column (1). One additional year of schooling is associated with an increase in overall satisfaction with life of 5.1 percent of a standard deviation. Most related, descriptive studies find a positive correlation between education and life satisfaction as well, see Appendix Table A1.

Before discussing the corresponding IV estimates, we consider the first stage of the 2SLS approach. The first-stage results in panel B of Table 2 document that all three instruments for years of schooling are relevant. On average, years of schooling increase by 0.53 years after the compulsory schooling reform. One additional intermediate school per 100 square kilometers increases average years of schooling by 0.37 years, one additional high school by 0.68 years. Given that an increase of one additional school per 100 square kilometers is substantial (see Figure 1)[11], the effects on years of schooling are not overly large when compared to the compulsory schooling reform. These effect sizes still seem plausible as the share of students affected by the school openings is substantially lower than the about 70 percent of students that were affected by the compulsory schooling reform. Finally, the first-stage F-statistics of the instruments are well above the rule-of-thumb value of 10 (see the bottom of Table 2). Thus, all three instruments can be considered as strong by the standard of Staiger and Stock (1997).

We move on by discussing the second stage of the 2SLS estimates in panel A of Table 2, the effect of (instrumented) years of schooling on life satisfaction. In columns (2)–(4), we consider the three instruments (compulsory schooling reforms as well as school openings of intermediate and high schools) separately. In contrast to the descriptive OLS estimate, all IV estimates are small in absolute size and have a negative sign. None of them is significant, indicating no causal, average effect of an additional year of schooling on life satisfaction. The difference between the OLS and IV results underlines the importance of focusing on exogenous variation in education to identify its effect on satisfaction with life.

[11] One additional intermediate school per 100 square kilometers corresponds, for instance, to the average increase in the state of North Rhine-Westphalia between 1950 and 1975.

Table 2: The overall effect of years of schooling on life satisfaction

	(1) OLS	Instrument: (2) Compulsory schooling reforms	(3) Openings of intermediate schools	(4) Openings of high schools	(5) All three instruments jointly
Panel A: Effect on life satisfaction					
Years of schooling	0.051***	−0.018	−0.023	−0.011	−0.021
	(0.016)	(0.019)	(0.027)	(0.016)	(0.021)
Panel B: Effect on years of schooling					
Compulsory schooling reforms		0.529***			0.477***
		(0.086)			(0.082)
Openings of intermediate schools			0.371***		−0.063
			(0.089)		(0.113)
Openings of high schools				0.681***	0.581***
				(0.148)	(0.206)
First-stage F-statistic		37.83	17.37	21.17	21.26
N. of observations	27,714	27,714	27,714	27,714	27,714

Notes: Own calculation based on the estimation sample. This table reports OLS and IV estimates for the effect of years of schooling on life satisfaction. Column (1) reports the OLS estimate. Columns (2)–(4) display IV estimates using the three instruments in separate 2SLS approaches (compulsory schooling in column (2), number of intermediate schools in column (3), and number of high schools in column (4)). Column (5) reports the IV estimate when all three instruments are used in the same 2SLS approach. All specifications control for full sets of state, birth cohort, and survey year fixed effects, and a gender indicator. Panel A states the estimates for the effect of years of schooling on life satisfaction, see equation (1) for the OLS estimate and equation (2) for the IV estimates. Panel B reports the first stage of the IV approaches, i.e., the effect of the instrument(s) on years of schooling, see equation (3). The outcome variable, life satisfaction, is standardized to mean 0 and standard deviation 1. The F-statistics at the bottom of the table refers to the instrument(s) in the first stage. We use sample weights to ensure representativeness. Standard errors are reported in parentheses and allow for a correlation within each state-of-schooling × year-of-birth cell. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Instrumenting years of schooling with the compulsory schooling reform in column (2) of Table 2 yields a point estimate of −1.8 percent of a standard deviation per year of schooling. This is remarkably close to the corresponding estimate in Dahmann and Schnitzlein (2019) of −2.1 percent of a standard deviation that relies on the same data source and reform as we do. Using the original 0–10 Likert-scale for life satisfaction (without standardizing) also yields a very small estimate for the compulsory schooling instrument of 0.021 points (standard error 0.038), see Appendix Table A3. Appendix A.1 provides a more comprehensive discussion of related studies and Figure A1 puts our effect sizes into perspective by illustrating them jointly with those of comparable studies.

Whereas the compliers in previous studies are exclusively located in the lower part of the schooling distribution, using the opening of intermediate and high schools as further instruments adds so far lacking evidence for individuals in the middle and upper part of the schooling distribution. The corresponding results in columns (3) and (4) of Table 2 reveal a new and, at the same time, important insight: the finding that years of schooling do not affect life satisfaction is not restricted to individuals in the lower part of the schooling distribution, but applies to the entire schooling distribution.

In particular, the zero effect for the compulsory schooling instrument in column (2) only refers to individuals who leave school as soon as possible—in Germany, these are basic school students. Students who attend intermediate or academic schools stay longer in school voluntarily. Presumably, they differ in many ways from basic school students such as their cognitive abilities, motivation, and preferences. As a consequence, the effect of an additional school year on life satisfaction may differ for them. Using the number of intermediate and academic schools to instrument years of schooling in columns (3) and (4) of Table 2 allows probing whether this is indeed the case. The corresponding point estimates of -2.3 percent and -1.1 percent of a standard deviation are small and not significant—indicating that the absence of an effect of an additional year of schooling on life satisfaction is not driven by a specific instrument's complying subpopulation but instead holds true for individuals along the entire schooling distribution.[12]

Testing for statistically significant differences between the second-stage estimates for the three different instruments, we fail to reject the hypothesis of no statistically significant differences by wide margins. p-values of pairwise t-tests are 0.87 for column (2) = column (3), 0.83 for column (2) = column (4), and 0.79 for column (3) = column (4). In column (5) of Table 2, we therefore use all three instruments in the same first-stage equation. Unsurprisingly, the size of the years of schooling coefficient on life satisfaction is similar to the coefficient sizes in columns (2)–(4). Given that the differences between the three IV estimates are neither statistically significant nor economically important, we will continue by employing all three instruments in the same first-stage equation to establish exogenous variation in education.

All results presented so far are robust to the inclusion of income, health, and marital status

[12] Changing the level of clustering to the state of schooling instead of the state of schooling × year of birth and employing the wild cluster bootstrap procedure outlined in Roodman et al. (2019) leads to p-values of 0.71, 0.74, 0.88 in the second-stage regressions in columns (2)–(4), respectively—implying again that we cannot reject the hypothesis of a null effect by a sizable margin.

Table 3: The effect of years of schooling on labor market-related domains of life satisfaction

	(1)	(2)
	Outcome variable:	
	Satisfaction with pay	Satisfaction with work
Years of schooling	0.052*	0.083**
	(0.031)	(0.037)
N. of observations	9,615	20,446

Notes: Own calculation based on the estimation sample. This table reports IV estimates for the effect of years of schooling on satisfaction with pay (in column (1)) and satisfaction with work (in column (2)). The specification is the same as in Table 2, i.e., we control for full sets of state, birth-cohort, and survey-year fixed effects, and a gender indicator. Both outcome variables only apply to individuals who work for pay and satisfaction for pay is only elicited for a subset of respondents, which results in a lower number of observations compared to Table 2. To avoid that sample size affects the first-stage estimation, we use the fitted years of schooling from the first stage in column (5) of Table 2. The standard errors are bootstrapped (using 1,000 replications) and allow the error term to correlate within each state-of-schooling × year-of-birth cell. Both outcome variables are standardized to mean 0 and standard deviation 1. We use sample weights to ensure representativeness. Standard errors are reported in parentheses. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

as additional control variables that are influenced by an individual's level of education (see Appendix Table A4).

4.2 The role of employment status

People commonly indicate higher income and better labor market outcomes in general as key motivations for pursuing additional education (see, e.g., Schneider and Franke, 2014, and Fishman, 2015). We therefore expect to observe that respondents with higher educational attainment report higher satisfaction with their income and job. This is indeed what we find. In Table 3 that estimates a 2SLS model using all three instruments jointly one additional year of schooling increases satisfaction with pay by 5.2 percent of a standard deviation and satisfaction with work by 8.3 percent of a standard deviation.

Given the higher satisfaction with labor market outcomes, the null result on the effect of years of schooling on overall satisfaction with life may come as a surprise at first sight. However, work-related benefits of higher educational attainment can only realize for people who are active in the labor market. Therefore, we continue by separately analyzing the effect of years of schooling for individuals who are employed and those who are not. We follow the International Labour Organization and Eurostat by categorizing people into those employed (including self-employed people), unemployed (i.e., looking for work), and outside the labor force (who are not actively looking for work for reasons such as caregiving, disability, or early retirement); see e.g., Eurostat (2023). We hypothesize that overall life satisfaction of employed individuals is increasing in years of schooling as it is well-documented that, among others, higher educated people have higher incomes (Heckman et al., 2018; Gunderson and Oreopolous, 2020).

Results in column (1) of panel B of Table 4 confirm this expectation. One additional year of schooling increases overall life satisfaction for employed individuals by around 6.4 percent of a standard deviation ($p < 0.01$). Unlike the small and non-significant effect sizes in Table 2 that rely on pooling employed and non-employed individuals, 6.4 percent of a standard deviation is a substantial effect. Expressed on the original 0–10, non-standardized Likert-scale for life satisfaction, one additional year of schooling increases life satisfaction by 0.11 points (see Table A5). We can compare this effect size with the associations between life satisfaction and life circumstances in Germany as reported in the review of Frijters et al. (2020). For example, the effect of one more year of schooling on the life satisfaction of employed individuals is comparable to a 20 percent increase in household income or the difference in life satisfaction for being partnered instead of single (both about 0.1 points).

For non-employed individuals, many expected benefits of higher educational attainment, such as a higher income, do not realize. For them, the expected effect of years of schooling on life satisfaction is less clear. Results in column (1) of Table 4 show that one additional year of schooling *decreases* life satisfaction for non-employed individuals by around 14.5 percent of a standard deviation (p-value < 0.1). The effect has thus the opposite sign than for employed individuals and its size is even more substantial. The estimated coefficients for employed and non-employed individuals are highly significantly different (t-test, p-value < 0.01, see Table 4).

Appendix Table A6 reports separate coefficients for the two subgroups of non-employed individuals—unemployed individuals and those outside the labor force. The estimates are similar and tests for equality of coefficients fail to reject the hypothesis of no statistically significant differences. We therefore continue pooling these two groups of individuals in our main analysis.

In sum, years of schooling turn out to be a significant and sizable determinant of people's life satisfaction. A novel insight is that the effect of years of schooling is strongly heterogeneous for employed and non-employed individuals. While the former benefit from higher educational attainment, life satisfaction of the latter suffers. In previous work, these heterogeneous effects of education on life satisfaction have been masked by jointly estimating the effect for employed and non-employed individuals. Before we discuss possible mechanisms underlying this key finding, we continue by presenting various robustness checks and addressing potential endogeneity concerns regarding employment status.

Robustness checks. While we prefer a parsimonious baseline specification, columns (2)–(4) of Table 4 present alternative specifications that alleviate potentially confounding variation in other domains of the society and government. In column (2), we add yearly state expenditures in areas other than education to account for the potential concern that the increasing school supply might be associated with other state expenditures that could influence life satisfaction decades later. For each individual, we match non-education expenditures (also taken from the Statistical Yearbooks) at age 9. To take the rising demand for secondary schools in the analyzed period into account, column (3) adds states' population per year as a further control variable to the baseline specification. Finally, column (4) includes state-specific linear trends. This specification only uses deviations in life satisfaction from the state-specific trajectory (see Stephens and Yang, 2014). None of these alternative specifications changes the interpretation of our results. This increases the confidence in our instruments and underlines the robustness of our main results.

Appendix Table A7 displays estimates as in column (1) of Table 4 but applies each of the three instruments separately. The results underline that the absence of relevant heterogeneity in the effect of years of schooling on life satisfaction along the schooling distribution extends similarly to employed and non-employed individuals. There is only little variation in the magnitude of the estimates for the different instruments.

Appendix Table A8 provides evidence that our results are not driven by the way our estimation sample is constructed. To assemble an alternative estimation sample, we start by including individuals in the 2010 wave in our sample and employ a backward-moving, iterative process. If a respondent has missing values in 2010, we check whether information is complete in 2009, and, if not, in 2008, and so on. We employ this iterative process backward to the 1985 wave. The age and year-of-birth distribution of the resulting, alternative estimation sample is displayed in Appendix Figure A6. As Table A8 shows, we obtain very similar results when using this alternative estimation sample.

Finally, Table A9 adds individuals from the three city states Hamburg, Bremen, and West Berlin to rely on the most comprehensive possible sample (although commuting to schools outside these city states might be an issue). Both the corresponding baseline results and those in the robust checks remain very similar to the results of our main specification.

Addressing endogeneity of the employment status. As far as our instruments are valid, the estimates in panel A of Table 4 reflect the causal effect of years of schooling on life satisfaction. However, we cannot rule out endogeneity concerns regarding employment status in panel B of Table 4. Unemployment may not only cause a drop in life satisfaction, but also result from it. Individuals may quit their job, if they are unhappy with their life. Moreover, unemployment may be correlated with events and decisions related to life satisfaction (such as long-term health shocks) and an individual's personality traits (such as patience, self-control, conscientiousness, and emotional stability, see, e.g., Cobb-Clark et al., 2022b, and Viinikainen and Kokko, 2012). The adverse link between fear of job loss and life satisfaction is well established (see, e.g., Luechinger et al., 2010, and Avdic et al., 2021). In the following, we will address such endogeneity concerns using two different approaches.

First, we focus on job terminations due to plant closures as an exogenous source of unemployment in Table 5. Plant closures are a well-established way to address the endogeneity of unemployment. A non-exhaustive list of studies using plant closures includes Browning and Heinesen (2012), Del Bono et al. (2012), Eliason (2012), Eliason and Storrie (2009), Huttunen and Kellokumpu (2016), Kuhn et al. (2009), Marcus (2014), and Schmitz (2011). Plant closures eliminate many concerns that could confound the results such as voluntary unemployment due to own resignation.[13]

[13]Of course, using plant closures as exogenous variation in employment is not beyond criticism. A possible concern regarding plant closures is that blue-collar workers are more likely to be affected by them, limiting the generalizability of the results (Brand, 2015). On top of that, plant closures often happen with a prior warning so that some individuals may change jobs before they are laid off. Therefore, individuals staying until the plant

Table 5 estimates separate coefficients for individuals who are employed, unemployed, and outside the labor force. Importantly, within the group of unemployed individuals, it differentiates between those unemployed due to a plant closure and those unemployed for other reasons. Reassuringly, the estimated effect of years of schooling on life satisfaction in column (1) is very similar for individuals who got unemployed as a result of a plant closure and individuals who are unemployed for other reasons. Moreover, both coefficients are of similar size as the one for being outside the labor force. The p-value of an F-test for the equality of the fitted years of schooling coefficients for being unemployed due to a plant closure, for other reasons, and being outside the labor force is above 0.99. This result underlines that endogeneity of unemployment is an unlikely driver of our finding that additional education hurts non-employed individuals' life satisfaction.

As a second approach to addressing possible endogeneity concerns regarding employment status, we use the panel structure of our data and estimate an individual fixed effects specification that effectively controls for all time-invariant individual characteristics. For example, personality traits are typically assumed to be stable over time during adulthood. In particular, the specification underlying Table 6 exploits within-individual transitions from non-employment into employment and vice versa to study how employment status affects the life satisfaction of individuals with different levels of education. The table notes provide further details on the sample and exact specification we employ. To overcome the challenge that educational attainment is mostly time-invariant in our adult sample, we perform a median split based on the fitted years of schooling (taken from the specification in column (5) of Table 2). Given our previous, cross-sectional result that each additional year of schooling has a positive effect on the life satisfaction of employed individuals, we expect a larger positive effect of employment on life satisfaction for individuals with above- as opposed to below-median education.

Results in Table 6 are in line with this expectation. The effect of employment on life satisfaction is more than twice as large (about 26 percent versus 12 percent of a standard deviation) for individuals whose instrumented years of schooling are above- rather than below-median schooling. This difference is statistically significant (t-test, p-value < 0.01). Another

closure may not be representative of the whole laid-off staff (Kletzer, 1998).

way of interpreting the results in Table 6 is that moving from employment to non-employment induces a loss of life satisfaction that is larger for individuals with above-median education. Again, these findings reinforce the confidence in our main result that additional education increases the life satisfaction of employed individuals, while it decreases the well-being of non-employed individuals.

Table 4: The role of employment status for the effect of years of schooling on life satisfaction

		Robustness checks: Additionally controlling for		
	(1) Baseline results	(2) state expenditures	(3) state population	(4) state-specific linear trends
Panel A: Second stage, not differentiated by employment status				
Overall effect	−0.021	−0.019	−0.009	0.017
	(0.021)	(0.018)	(0.023)	(0.023)
Panel B: Second stage by employment status				
Employed	0.064***	0.058***	0.061***	0.069***
	(0.022)	(0.019)	(0.023)	(0.021)
Non-employed	−0.145*	−0.141*	−0.143*	−0.139**
	(0.075)	(0.072)	(0.073)	(0.069)
Test for equality of coefficients (p-value)	0.004	0.006	0.005	0.004
First-stage F-statistic	20.89	21.03	21.26	18.42
N. of observations	27,714	27,714	27,714	27,714

Notes: Own calculation based on the estimation sample. This table reports IV estimates for the effect of years of schooling on life satisfaction. Panel A states the overall effect based on a conventional 2SLS approach using all three instruments in a joint first stage. For comparison, column (1) retrieves the estimation from column (5) of Table 2, i.e., controlling for full sets of state, birth-cohort, and survey-year fixed effects, and a gender indicator. Columns (2)–(4) report robustness checks. In column (2), we additionally control for the state's expenditures for other policy areas than education when the individual was 9 years old (this is also the age we use to assign the number of intermediate and academic schools). In column (3), we control for population size in the state of residence. In column (4), we add state-specific linear time trends to the baseline specification. Panel B estimates separate effects of (instrumented) years of schooling on life satisfaction for employed and non-employed individuals. Instead of separate regressions for employed and non-employed individuals, we interact fitted years of schooling in the second stage with employment and non-employment indicators. That is, the first stage is estimated according to equation (3) and additionally controls for employment status. In the second stage, we regress life satisfaction on fitted years of schooling for employed individuals, fitted years of schooling for non-employed individuals, an employment indicator, and baseline controls. The standard errors in panel B are bootstrapped (using 1,000 replications) and allow the error term to correlate within each state-of-schooling × year-of-birth cell. The p-values for a t-test of equality of the years of schooling coefficients for being employed and non-employed are displayed below the estimates. The outcome variable, life satisfaction, is standardized to mean 0 and standard deviation 1. The F-statistics at the bottom of the table refer to the instrument(s) in the first stage. We use sample weights to ensure representativeness. Standard errors are reported in parentheses. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: The effect of years of schooling on life satisfaction by reason of unemployment

		Robustness checks: Additionally controlling for		
	(1) Baseline results	(2) state expenditures	(3) state population	(4) state-specific linear trends
Employed	0.065***	0.061***	0.062***	0.066***
	(0.021)	(0.020)	(0.024)	(0.023)
Unemployed due to plant closure	−0.129*	−0.121*	−0.120*	−0.114*
	(0.070)	(0.071)	(0.069)	(0.064)
Unemployed for other reasons	−0.135*	−0.123*	−0.132*	−0.124*
	(0.075)	(0.072)	(0.074)	(0.069)
Outside the labor force	−0.151	−0.164	−0.156	−0.150
	(0.147)	(0.150)	(0.161)	(0.193)
First-stage F-statistic	20.89	21.03	21.26	18.42
N. of observations	27,714	27,714	27,714	27,714

Notes: Own calculation based on a modified estimation sample. This table reports IV estimates for the effect of years of schooling on life satisfaction. The specification and estimation procedure is the same as in panel B of Table 4 and described there. However, employment status enters the model more fine-grained. In the second stage of the 2SLS model that we report here, fitted years of schooling are interacted with indicators for being employed, unemployed due to plant closure, unemployed for another reason, and outside the labor force (in panel B of Table 4 the latter three categories are summarized as non-employed). The estimation sample is slightly modified compared to the one described in the data section. The reason is that only 52 individuals in the initial estimation sample are unemployed due to plant closure at the time of the interview. To increase this number to 371 individuals, we replace, if applicable, the initial interview year with another interview year in which the respondent was unemployed due to plant closure in the latter year but not in the initial one. The p-value of an F-test for the equality of the years of schooling coefficients of the three non-employed categories is < 0.01. The outcome variable, life satisfaction, is standardized to mean 0 and standard deviation 1. The F-statistics at the bottom of the table refer to the instrument(s) in the first stage. We use sample weights to ensure representativeness. Standard errors are reported in parentheses. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6: The effect of employment on life satisfaction by level of schooling using individual fixed effects

	(1) Overall effect	(2) Above-median schooling	(3) Below-median schooling
		Sample split by fitted years of schooling:	
Employed	0.187***	0.253***	0.117***
	(0.005)	(0.008)	(0.008)
N. of observations	251,498	125,749	125,749

Notes: Own calculation based on a sample that follows respondents over time. This table reports the effect of employment status on life satisfaction. For each respondent, we use all waves with complete information and estimate an individual fixed effects specification where life satisfaction in wave t of individual i is regressed on i's employment status in t and, as control variables, full sets of state-of-residence and survey-year fixed effects (year of birth and gender are absorbed by the individual fixed effects). To investigate the role of education, we split the sample along the median of the fitted years of schooling (taken from the specification in column (5) of Table 2) and get separate estimates for individuals with above- and below-median years of schooling. The outcome variable, life satisfaction, is standardized to mean 0 and standard deviation 1. We use sample weights to ensure representativeness. Standard errors are reported in parentheses and are clustered on the individual level. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

5 Discussion

The role of aspirations. What could explain the strongly diverging effect of years of schooling on life satisfaction for employed and non-employed individuals? Aspirations and expectations have proven to be powerful predictors of human behavior and economic outcomes (e.g., Genicot and Ray, 2017) as well as life satisfaction, see, e.g., the large literature on the role of relative income—one example is Stutzer (2004)—or Schwandt (2016) for age patterns in life satisfaction. Only a few previous studies have investigated the role of aspirations for a better understanding of the association between education and life satisfaction. Kristoffersen (2018) focuses on measuring how expectations and aspirations change across education levels. Clark et al. (2015) find that around half of the happiness effect of education that they report is cancelled out by higher aspirations.

We build on and extend this work by suggesting unmet aspirations as a possible mechanism why additional education may hurt the life satisfaction of non-employed individuals, while this is not the case for people with a job. We consider a standard utility function that is augmented with aspirations:

$$U_i = u(Y_i(E_i)) + v(Y_i(E_i) - Y_i^*(E_j)) \text{ with } \; u' > 0, \; v' > 0. \tag{4}$$

Individual i's overall utility U_i depends both on utility $u(\cdot)$ derived from their outcomes Y_i and on utility $v(\cdot)$ from the gap between their realized $Y_i(E_i)$ and aspired outcomes $Y_i^*(E_j)$. We refer to this as the *aspiration gap*.[14] Aspirations are determined by social comparison, i.e., individual i comparing to their peers j. As our analysis focuses on the role of education for life satisfaction, the relevant peer group consists of individuals who have the same years of schooling E, i.e., $E_i = E_j$. Both realized outcomes $Y_i(E_i)$ and aspirations $Y_i^*(E_j)$ are assumed to increase in education as higher educated people have, on average, higher outcomes (e.g., income). This makes the overall effect of education on utility (proxied by life satisfaction, see, e.g., Deaton, 2012) ambiguous.

In the following empirical analysis, we will focus on the role of income and income aspirations

[14] Clark et al. (2015) and Kristoffersen (2018) focus exclusively on the second, aspiration-driven part of the utility function.

to illustrate that the utility function above can explain the opposite-signed effect of years of schooling on employed and non-employed individuals' life satisfaction. Income is not only a very salient outcome of education, but non-employment has an immediate effect on income, which, in turn, is linked to life satisfaction (see, e.g., Lachowska, 2017). Income aspirations (independent of education) are frequently considered as a determinant of life satisfaction (see, e.g., Stutzer, 2004).[15]

Employed individuals derive substantial utility from income $u(\cdot)$. Moreover, their income aspiration gap is likely to be positive. If the peer group they compare to consists of employed and non-employed individuals, the realized income of employed individuals will, on average, exceed the average income of their peers. By contrast, non-employed individuals have a lower income, resulting in lower $u(\cdot)$. On average, their income aspiration gap is negative; their realized income will be smaller than that of their peers, resulting in lower utility from the second part of the utility function $v(\cdot)$. Furthermore, income aspirations rise with education ($\frac{\partial Y^*}{\partial E} > 0$) such that the gap between realized income and income aspirations grows with education, further decreasing $v(\cdot)$ of non-employed individuals with higher years of schooling. Overall, we predict the average life satisfaction of employed individuals to increase in education. This effect is mainly driven by the direct income effect through $u(\cdot)$. For non-employed individuals, the direct income effect on utility (i.e., resulting from possibly received unemployment or maternity benefits) is small such that the negative aspiration gap is likely to dominate the effect of education on life satisfaction through a lower $v(\cdot)$.

Empirical evidence. We continue by empirically quantifying the importance of income and the income aspiration gap for the effect of years of schooling on life satisfaction. As income measure Y_i, we use an individual's gross labor market earnings plus transfers. Since many transfers, such as unemployment and maternity benefits, depend on past earnings, they are also affected by education. The income aspiration gap is measured as the difference between an individual's actual income Y_i and their income aspiration $Y_i^*(E_j)$. Income aspirations are assumed to correspond to the average income of all other individuals with the same number

[15] In principle, Y_i can capture all sorts of outcomes including income, occupational prestige, and health. It could even encompass the level of education and employment status, which we treat as given in the context of our analysis. We focus on income for simplicity, using it as a proxy for overall life circumstances, and for the reasons stated in the text.

of years of schooling, gender, and birth cohort, who live in the same state (on average, 42 individuals in our sample). Average income varies substantially with education. In our sample, individuals with a basic school degree have average, gross monthly earnings of 2,332 euros, those with an intermediate school degree earn 2,711 euros, and those with a high school degree 3,754 euros.

Table 7 displays the regression results for the effect of years of schooling on life satisfaction, estimating separate coefficients for employed and non-employed individuals as in panel B of Table 4. It compares the baseline estimates in column (1) with estimates that additionally control for income and/or the income aspiration gap in columns (2)–(4). This allows us to quantify to which extent individual income and the income aspiration gap drive our baseline estimates. The results in Table 7 are of descriptive nature.

Based on the estimation results in Table 7, Figure 2 illustrates the share of the effect of years of schooling on life satisfaction that the two channels, income and the income aspiration gap, can explain. The left plot refers to employed individuals, the right one to non-employed individuals. Each framed, horizontal bar represents the overall baseline estimate of the effect of years of schooling on life satisfaction in column (1) of Table 4 (repeated in column (1) of Table 7). As discussed before, this effect is positive for employed (6.4 percent of a standard deviation) and negative (-14.5 percent) for non-employed individuals. The filled areas in the bars display the share of this effect that can be attributed to the respective channels (income in the upper bar, the income aspiration gap in the middle bar, and both jointly in the lower bar). The percentages illustrate to which extent the baseline estimates decrease once we account for the possible channels, i.e., to which degree the baseline effect materializes through these channels.

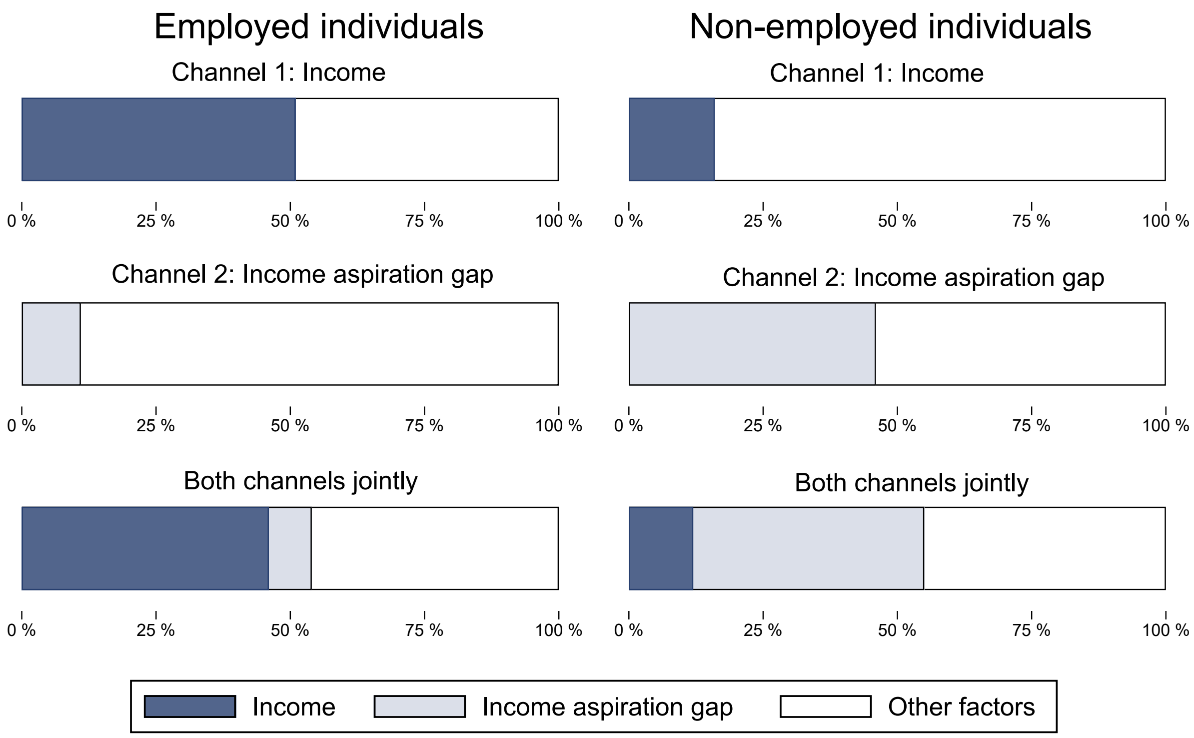
Table 7: The effect of years of schooling on life satisfaction by employment status, controlling for income and income aspiration gaps

		Channels:		
	(1) Baseline results	(2) Channel 1: income	(3) Channel 2: income aspiration gap	(4) Both channels combined
Panel A: Coefficients				
Years of schooling				
– employed	0.064^{***}	0.031^{*}	0.057^{**}	0.029
	(0.022)	(0.017)	(0.029)	(0.018)
– non-employed	-0.145^{*}	-0.122^{*}	-0.078	-0.065
	(0.075)	(0.069)	(0.057)	(0.043)
Income (in 1,000 euros)		0.028^{***}		0.025^{***}
		(0.006)		(0.006)
Income aspiration gap (in 1,000 euros)			0.013^{***}	0.011^{***}
			(0.004)	(0.004)
Panel B: Explained share (in percent) of effect of schooling on life satisfaction				
Employed		51.6	10.9	54.7
Non-employed		22.8	40.0	55.2
N. of observations	27,714	27,714	27,714	27,714

Notes: Own calculation based on the estimation sample. This table reports IV estimates for the effect of years of schooling on life satisfaction by employment status for different specifications. Column (1) of this table repeats the specification from column (1) in panel B of Table 4. Columns (2)–(4) add further variables to this specification. Column (2) additionally controls for income (encompassing gross labor market income as well as transfers). Column (3) controls for the income aspiration gap (measured as the difference between own income and the jackknife average income of individuals in the same years-of-schooling × state × birth-cohort × gender cell). Column (4) includes both income and the income aspiration gap as additional control variables. Panel A reports the coefficients of years of schooling, income, and the income aspiration gap. Columns (2)–(4) of panel B state the share by which the coefficient of years of schooling changes due to the inclusion of income and/or the income aspiration gap compared to the specification in column (1). The shares are calculated as 1 minus the coefficient in columns (2), (3), or (4), respectively, divided by the corresponding baseline coefficient in column (1). Figure 2 depicts these shares graphically. We use sample weights to ensure representativeness. Standard errors are reported in parentheses and allow for a correlation within each state-of-schooling × year-of-birth cell. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7 documents that both income and positive income aspiration gaps (i.e., individual income that exceeds aspirations) are positively associated with life satisfaction. For employed individuals, we find that the positive effect of years of schooling on life satisfaction shrinks by half of its magnitude when income is controlled for (see column (2) of Table 7 and the upper left bar in Figure 2). Put differently, about half of the increase in life satisfaction due to years of schooling can be attributed to higher income. By contrast, the coefficient of years

Figure 2: Share of the effect of years of schooling attributed to income and income aspiration gap by employment status



Notes: Own illustration based on the estimation sample. Each framed, horizontal bar represents the effect of years of schooling (column (1) of panel B in Table 4 and repeated in column (1) of Table 7). This effect is positive for employed individuals in the left plot (6.4 percent of a standard deviation) and negative for non-employed individuals in the right plot (−14.5 percent). The filled area in the upper bar displays the share of this effect that can be attributed to income. That is, the change in the years of schooling coefficient in column (2) of Table 7 (that controls for income), relative to the baseline coefficient in column (1) of Table 7. The filled area in the middle bar shows the share that can be attributed to the income aspiration gap. The filled area in the bottom bar shows both channels' contributions when jointly included in the model. The shares are calculated as 1 minus the coefficient in columns (2), (3), or (4), respectively, divided by the corresponding baseline coefficient in column (1) of Table 7.

of schooling reduces only by about 10 percent when the income aspiration gap is controlled for in column (3) of Table 7. This smaller change is intuitive since, on average, the earnings of employed individuals are relatively similar to their aspirations with an average positive income aspiration gap of 456 euros. However, the income aspiration gap is typically negative and substantially larger for non-employed individuals. The average difference in our sample is −1,956 euros. The estimate in column (3) of Table 7 and the middle bar on the right of Figure 2 show that controlling for the income aspiration gap reduces their estimated coefficient for the effect of years of schooling on life satisfaction by 46 percent. The fact that non-employed individuals fall short of their income aspirations seems to be a major driver of their reduced life satisfaction. Non-employed individuals' income corresponds (at most) to unemployment and maternity benefits and is relatively low. Consequently, income plays a much smaller role in the baseline effect of years of schooling on life satisfaction for non-employed than employed individuals. The upper right bar in Figure 2 illustrates that it accounts for about 15 percent of the baseline effect for non-employed individuals.

Overall, results are in line with our predictions: The increase in life satisfaction with more years of schooling for employed individuals materializes to a sizable degree through income. The reduced life satisfaction of non-employed individuals with higher levels of schooling is to a substantial extent the result of them falling short of their income aspirations.

Jointly, income and the income aspiration gap explain about 55 percent of the baseline estimates for the effect of years of schooling on life satisfaction, both for employed and non-employed individuals (see column (4) of Table 7 and the lower bars of Figure 2). Given the explanatory power of realized income and income aspirations, income turns out to be the single, most important channel for the effect of education on life satisfaction.

6 Conclusion

The impact of education on life satisfaction is a longstanding, but still open question that is highly relevant for individuals and governments alike. The findings of this paper advance our understanding of the link between education and life satisfaction in two important ways. First, we document that the effect of education on life satisfaction does not differ systematically for individuals with different levels of schooling, underlining the general nature and importance of our findings. By contrast, previous studies that exploit quasi-experimental variation in school attainment exclusively focused on individuals with low levels of schooling.

As a second novel insight, we identify an individual's employment status as a key determinant of the direction of the education effect. While higher educated, employed individuals are substantially more satisfied with their life, the life satisfaction of non-employed individuals suffers stronger the better educated they are. This important asymmetry is robust to using exogenous variation in employment status and again applies irrespective of an individual's level of education. In previous work, the heterogeneous effect of education on life satisfaction has been hidden in a jointly estimated effect for employed and non-employed individuals. Our results offer a possible explanation for the lack of consensus on the effect of education on life satisfaction given that studies differ in sample composition and in particular the share of individuals with and without a job.

Our results also contribute to a better understanding of the consequences of unemployment. While it is well-documented that unemployment hurts life satisfaction (see, e.g., the meta-analyses of Paul and Moser, 2009 and McKee-Ryan et al., 2005), we go beyond previous work by documenting a sizable education gradient in the associated pain from experiencing unemployment. While higher educated individuals will typically be better able to cope with the financial consequences of unemployment and have better prospects of finding a new job, our findings underline that their life satisfaction suffers more strongly when they are unemployed. To minimize such psychological costs, public policies supporting unemployed people in their job search efforts should address all unemployed individuals to a similar extent, irrespective of their educational attainment.

We also provide a possible, unifying explanation for the asymmetric effect of education on life satisfaction by employment status. In particular, we find support for the assumption that, on top of realized outcomes, aspirations matter for people's life satisfaction. A mediation analysis reveals that income and met or unmet income aspirations alone can jointly explain more than half of the estimated effect size of years of schooling on life satisfaction, both for employed and non-employed individuals. The increase in life satisfaction in years of schooling for employed individuals seems to be largely due to higher income. Unmet income aspirations reduce the life satisfaction of non-employed individuals, an effect that is stronger the better educated someone is. Since aspirations raise with education, higher levels of education only translate into higher life satisfaction for individuals who are successful on the labor market. This finding underlines the importance of forming realistic expectations regarding own ability and labor market demand before making educational and occupational choices.

Overall, our findings suggest that education is more than a traditional investment good: it changes not only the likelihood of specific labor market outcomes but also how they are evaluated. Even though it is, for instance, widely accepted that education decreases the likelihood of unemployment, we show that it also increases the pain from experiencing it.

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More education does make you happier—unless you are unemployed

—Online Appendix—

Alexander Bertermann

Daniel A. Kamhöfer

Hannah Schildberg-Hörisch

September 15, 2023

A Online Appendix

A.1 Previous literature

While numerous studies report correlations between education and life satisfaction (see Table A1), considerably fewer studies aim at tackling the inherent endogeneity of education. As this study, four published studies use compulsory schooling legislation, either in an instrumental variables estimation or regression discontinuity designs (RDD): Dahmann and Schnitzlein (2019) for Germany, Banks and Mazzonna (2012) for the UK, and Oreopoulos (2007) for the UK and Ireland, and de New et al. (2021) for two Australian states.[16] Dahmann and Schnitzlein (2019) and de New et al. (2021) derive estimates using a 0–10 scale to measure life satisfaction, Banks and Mazzonna (2012) an index from 0 to 57, and Oreopoulos (2007) employs a 1–4 scale. To be able to compare our effect sizes to their estimates, Table A5 repeats the analysis of Table 4 using the non-standardized, 0–10 scale for life satisfaction. Figure A1 illustrates the resulting point estimates in comparison to those studies, re-scaling their estimates to the 0–10 scale (if necessary) by using a linear transformation. As those in Dahmann and Schnitzlein (2019), Banks and Mazzonna (2012), and de New et al. (2021), our estimates are very small and not significant, while the one of Oreopoulos (2007) is significantly positive.

Other studies also exploit compulsory schooling changes, but do not fit into the comparison above. Elsas (2021) exploits the German reform we consider, but does not control for age fixed effects. While the estimates are difficult to compare, they indicate are a rather large and negative effect of schooling on life satisfaction, that is, however, not significantly different from zero. Dursun and Cesur (2016) analyze a Turkish reform that made it compulsory to take three more years of secondary schooling (up to the eight grade) instead of possibly dropping out after five years of elementary schooling. This reform has are large, positive effect on women's life satisfaction, but decreases men's life satisfaction.[17]

Kim (2021) estimates a positive and statistically significant effect of college education on a binary happiness indicator for South Korea, using changes in access quotas for colleges in an IV approach. Jiang (2022) uses a Chinese reform that sharply increased the number of college students in an RDD approach. The effect of college education on a 5-point happiness scale is positive and significant, but is limited to the years directly around the introduction of the reform.

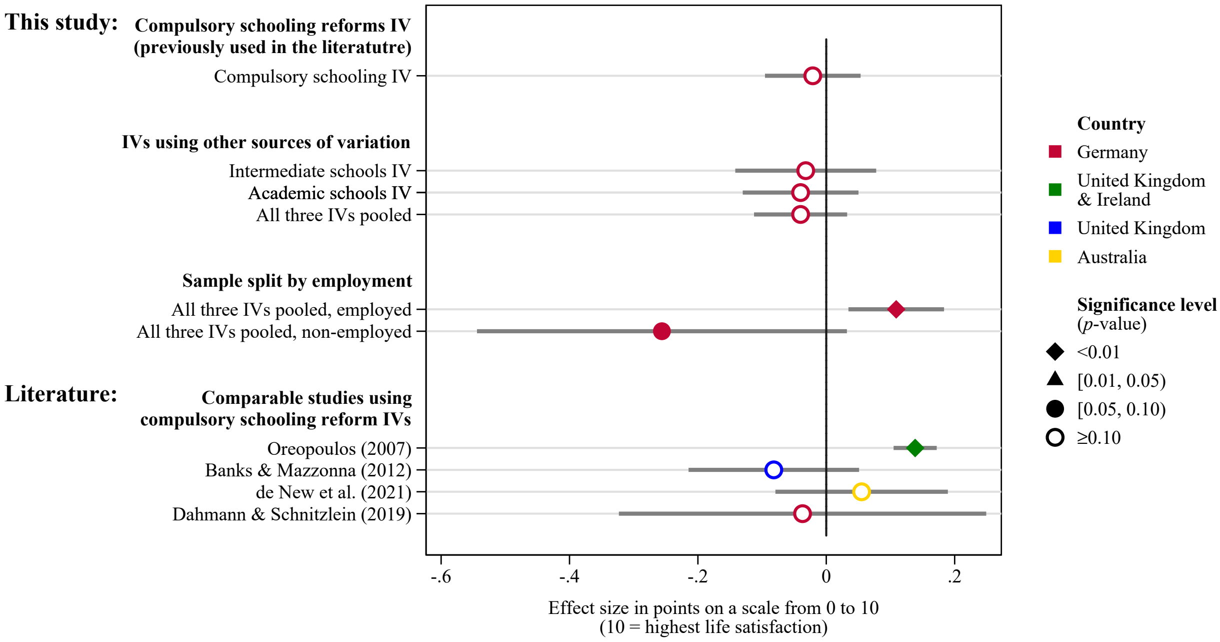
Boyce (2010) and Powdthavee et al. (2015) apply a fixed effects vector decomposition (FEVD) model to panel data on education and life satisfaction. Using German SOEP data, Boyce (2010) finds a non-significant point estimate of -0.3 percent of a standard deviation in life satisfaction per year of education, which is in line with the zero effect of compulsory schooling on life satisfaction in Germany in Dahmann and Schnitzlein (2019) and this study. Using Australian panel data, Powdthavee et al. (2015) report a small, negative effect of education on life satisfaction of -2.8 percent of a standard deviation, that is, however, significantly different from zero.[18]

[16] In their review article, Oreopoulos and Salvanes (2011) report similar results as Oreopoulos (2007).

[17] Tran et al. (2021) instrument women's years of schooling with their husbands' years of schooling to analyze the effect on life satisfaction in Australian survey data. However, in the presence of assortative mating, concerns about the instrument's validity remain.

[18] In the working-paper version, Powdthavee et al. (2013), the authors use the Australian compulsory schooling reform for identification instead of an FEVD model and find a non-significant, positive effect for men and a negative effect for women that is significant at the 10 percent level.

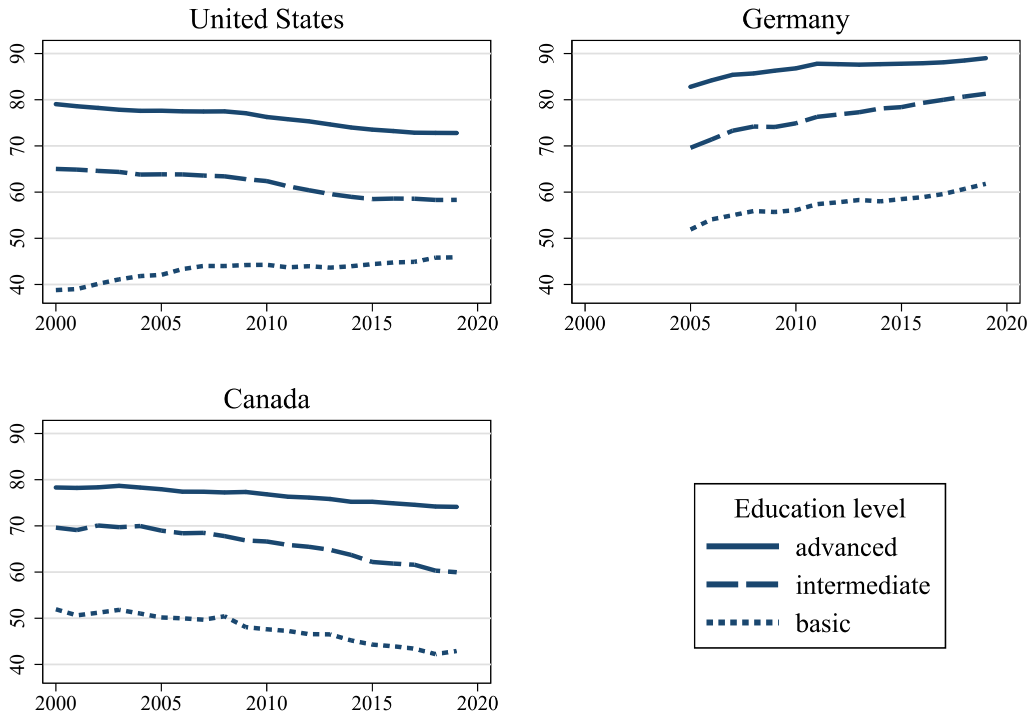
Figure A1: Comparison of the estimates of the effect of education on life satisfaction in this study with estimates in comparable previous studies



Notes: Own illustration. The figure compares the effect sizes of years of schooling on life satisfaction (indicated by the markers) and their 95 percent confidence intervals (the spikes) in this study with the corresponding estimates in comparable previous studies. These previous studies also exploit exogenous variation in school attendance by one year that is caused by compulsory schooling reforms. We re-estimate our effect sizes using the unstandardized, 0–10 scale for life satisfaction because not all previous studies provide the information that is necessary to compare effect sizes based on a standardized life satisfaction scale. The first line ("Compulsory schooling reforms IV") plots the coefficient and confidence interval as in column (2) of Table 2 on the 0–10 scale. This estimate is closest to the ones in the previous literature that are reported in the bottom panel. The three following lines plot the coefficient for the two new instruments as well as the coefficient when using all three instruments in the same first stage as in columns (3)–(5) in Table 2, but on the 0–10 scale. Lines 5 ("All three IVs pooled, employed") and 6 ("All three IVs pooled, non-employed") plot the coefficients from panel B of column (1) in Table 4, again on the 0–10 scale for comparison with the previous estimates.

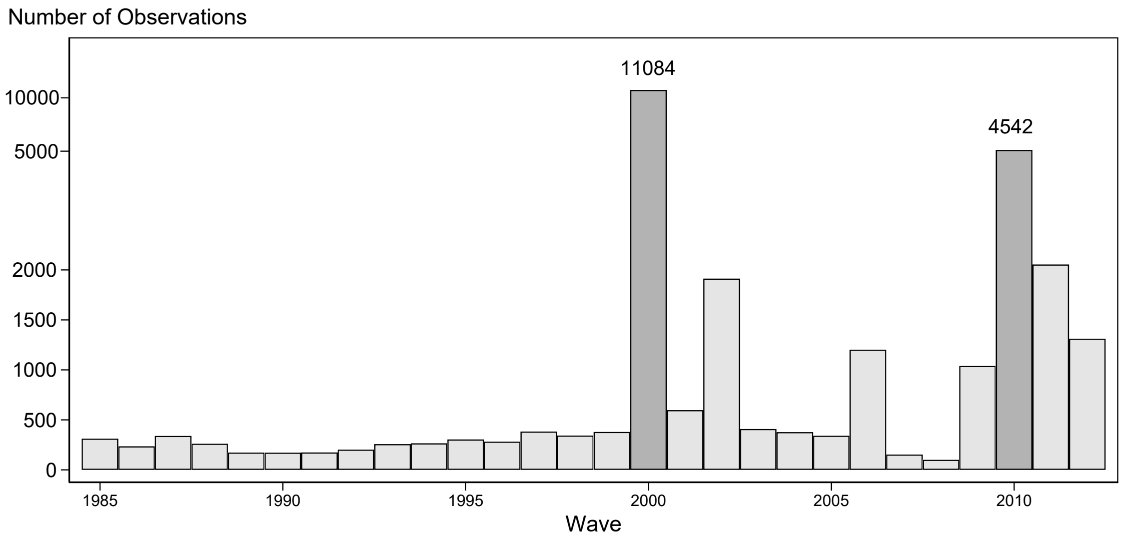
A.2 Additional figures and tables

Figure A2: Labor force participation over time in selected countries



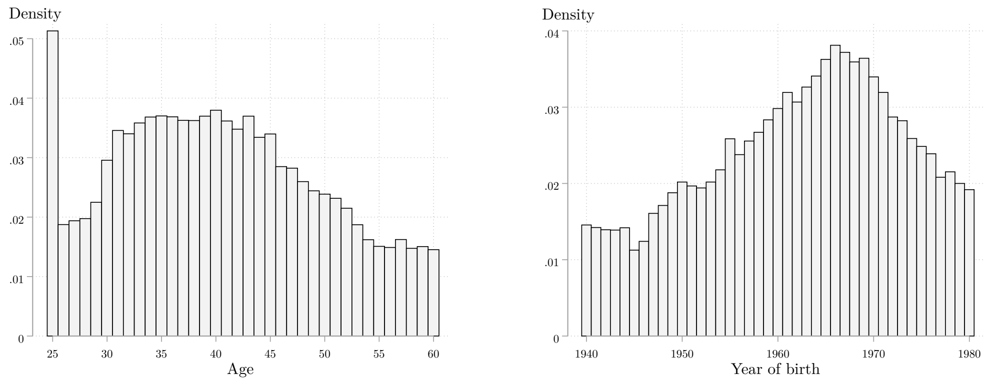
Notes: Own illustration. Data for the United States and Canada are taken from ILO (2023), data for Germany are taken from Eurostat (2023).

Figure A3: Number of observations per wave in the main estimation sample



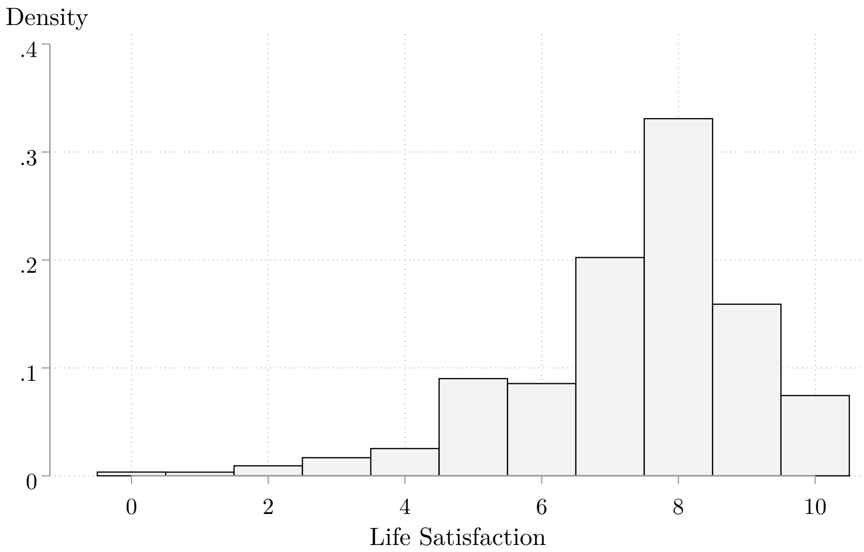
Notes: Own illustration based on the estimation sample. This figure shows the number of observations per wave in the main estimation sample. The vertical scale is adapted for numbers of observations higher than 2000. This allows both displaying the particularly high numbers in the years 2000 (11,084 observations, baseline year of our sampling scheme that also encompasses a refreshment sample) and 2010 (4,542 observations, largely due to a refreshment sample) and the variation in the remaining years. 86 percent of respondents are observed in 2000 or a more recent wave.

Figure A4: Distribution of age and year of birth in the main estimation sample



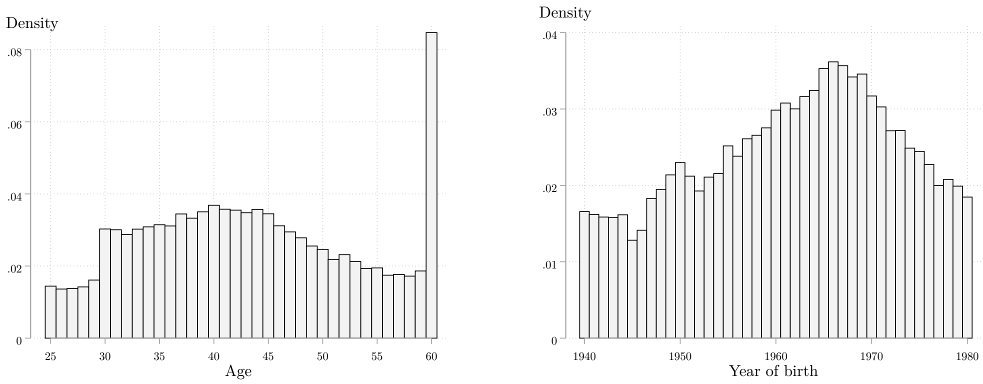
Notes: Own illustration based on the estimation sample. The left plot illustrates the age distribution (in years) in the estimation sample. The forward-moving iterative process for composing the estimation sample, described in the text, adds a large number of respondents that have just become 25 years old (lower age threshold used in the sample composition). The right plot shows the respective distribution of year of birth.

Figure A5: Distribution of life satisfaction



Notes: Own illustration based on the estimation sample. This Figure displays responses to the question "How satisfied are you with your life, all things considered?", using a Likert-scale that ranges from 0 (= completely dissatisfied) to 10 (= completely satisfied). For data analysis, we use standardized life satisfaction (mean 0, standard deviation 1).

Figure A6: Distribution of age and year of birth in the alternative estimation sample



Notes: Own illustration based on the estimation sample. The left plot illustrates the age distribution (in years) in the alternative estimation sample that starts by including individuals in the 2010 wave in our sample and employs a backward-moving iterative process until the year 1985. The right plot shows the respective distribution of year of birth.

Table A1: Previous, correlational studies on the link between education and life satisfaction

Paper	Country	Result
Araki (2022)	24 countries	+
Artés et al. (2014)	28 countries	+
Blanchflower and Oswald (2004)	USA	+
Cuesta and Budría (2014)	Germany	Null
Bukenya et al. (2003)	West Virginia (USA)	+
Stutzer and Lalive (2004)	Switzerland	+/−
Chen (2012)	Japan, Taiwan, Korea and China	+
Clark et al. (2017)	Australia, USA, UK, Indonesia	+
Clark et al. (2015)	Japan	+
Clark and Oswald (1996)	United Kingdom	−
Cuñado and de Gracia (2012)	Spain	+
Degutis and Urbonavicius (2013)	Lithuania	+
Di Tella et al. (2001)	12 European countries	+
Piper (2015)	United Kingdom	−
Easterbrook et al. (2016)	United Kingdom	+
Elgar et al. (2011)	50 countries	+
Frey and Stutzer (2000)	Switzerland	+
Hartog and Oosterbeek (1998)	Netherlands	+/−
Kristoffersen (2018)	Australia	−
Salinas-Jiménez et al. (2011)	11 OECD countries (Germany included)	+
Ngoo et al. (2015)	28 Asian countries	+
Nikolaev (2016)	Australia	+
Nikolaev (2018)	Australia	+
Nikolaev and Rusakov (2016)	Australia	+
Shields et al. (2009)	Australia	−
Subramanian et al. (2005)	USA	+
Tokuda and Inoguchi (2008)	Japan	+
Selim (2008)	Turkey	−/Null
Gerdtham and Johannesson (2001)	Sweden	+
Yakovlev and Leguizamon (2012)	USA	+
Ventura (2021)	Turkey	Null

Notes: The table provides an overview on previous studies on the link between education and life satisfaction, focusing mainly on studies published after 2000. The link between education and subjective well-being life is not the primary research question in all listed studies.

Table A2: Descriptive statistics

	Mean	Standard deviation
Well-being measures, original scales[2]		
Life satisfaction	7.32	1.74
Satisfaction with pay	5.85	2.79
Satisfaction with work	7.23	2.13
Demographics		
Age (in years)	40.47	9.34
Female (in percent)	52.30	49.94
Year of birth	1962.10	10.54
Migration background[2] (in percent)	24.41	42.96
Religious (in percent)	84.12	36.54
Household and family		
Having a partner (in percent)	79.63	40.27
Household with children (in percent)	57.50	49.43
Number of siblings	1.60	1.85
Mother: intermediate schooling or above (in percent)	17.96	38.38
Father: intermediate schooling or above (in percent)	20.87	40.64
Education		
Years of schooling	10.41	1.64
Years of education	12.16	2.78
University degree (in percent)	20.87	40.64
Vocational training (in percent)	69.13	46.19
Labor market		
Labor force participation (in percent)	71.56	45.11
Out of labor force (in percent)	21.32	40.69
Unemployed (in percent)	7.12	25.72
Gross monthly income (in euros)	2526.23	2393.25
Self-employed (in percent)	8.41	27.76
Reason for job termination[3]		
Plant closure (in percent)	4.36	19.05
Own resignation (in percent)	34.14	47.43
Dismissal (in percent)	18.62	38.29
Mutual agreement (in percent)	9.71	29.92
Temporary contract expired (in percent)	10.80	31.27
Leave of absence, sabbatical (in percent)	16.31	37.83
Business closed down, self-employed (in percent)	3.62	18.46
Other (in percent)	2.42	5.46

Notes: Own calculation based on the estimation sample. Column (1) gives the mean value of the variable stated on the left, column (2) its standard deviation. [1]The original scales range from 0 (= completely dissatisfied) to 10 (= completely satisfied). For data analysis, we use standardized life satisfaction (mean 0, standard deviation 1). [2]Includes individuals who migrated to Germany and individuals of migrant origin, but born in Germany. [3]Percentages add up to 100 and are conditional on termination of employment. Note that individuals with job termination due to age restrictions are dropped.

Table A3: Employing each instrument separately: The effect of years of schooling (overall and by employment status) on life satisfaction (original 0–10 Likert-scale)

	(1) Compulsory schooling reforms	(2) Openings of intermediate schools	(3) Openings of high schools
Panel A: Second-stage, not differentiated by employment status			
Overall effect	−0.021	−0.032	−0.040
	(0.038)	(0.056)	(0.046)
Panel B: Second-stage by employment status			
Employed	0.100***	0.102***	0.105***
	(0.032)	(0.033)	(0.035)
Non-employed	−0.260*	−0.238*	−0.231*
	(0.154)	(0.140)	(0.136)
Test for equality of coefficients (p-value)	0.004	0.005	0.005
Panel C: First stage			
Years of schooling	0.539***	0.381***	0.746***
	(0.081)	(0.089)	(0.163)
First-stage F-statistic	44.27	18.32	20.94
N. of observations	27,714	27,714	27,714

Notes: Own calculation based on the estimation sample. This table repeats the analysis conducted for Table A7 but life satisfaction is reported on the original 0–10 Likert-scale instead of being standardized. The column headers denote the respective variables that instrument years of schooling. The outcome variable in the second stage is life satisfaction. The specification is the same as in Table 4, column (1). The coefficient in the first stage refers to the respective instrument. p-values for a test of equality (t-test) of the estimates for employed and non-employed individuals are displayed below the estimates. We use sample weights to ensure representativeness. Standard errors are reported in parentheses and allow for a correlation within each state-of-schooling × year-of-birth cell. They are bootstrapped using 1,000 replications. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A4: The overall effect of years of schooling on life satisfaction with additional controls

		Instrument:			
	(1)	(2)	(3)	(4)	(5)
	OLS	Compulsory schooling reforms	Openings of intermediate schools	Openings of high schools	All three instruments jointly
Panel A: Effect on life satisfaction					
Years of schooling	0.020***	−0.009	−0.022	−0.017	−0.014
	(0.004)	(0.012)	(0.020)	(0.023)	(0.018)
Controls					
Income	0.051***	0.096***	0.109***	0.080***	0.087***
(in 1,000 euros)	(0.004)	(0.020)	(0.033)	(0.023)	(0.016)
Income^2	−0.005***	−0.010***	−0.011***	−0.008***	−0.009***
(in 10 million euros)	(0.001)	(0.003)	(0.004)	(0.003)	(0.003)
Health	0.419***	0.455***	0.463***	0.444***	0.449***
(scale: 1−5)	(0.008)	(0.017)	(0.023)	(0.018)	(0.015)
Marital status	0.155***	0.130***	0.127***	0.135***	0.133***
(1 = married, 0 = else)	(0.010)	(0.015)	(0.017)	(0.015)	(0.015)
Baseline controls	✓	✓	✓	✓	✓
Panel B: Effect on years of schooling					
Compulsory schooling reforms		0.526***			0.439***
		(0.081)			(0.086)
Openings of intermediate schools			0.404***		−0.104
			(0.094)		(0.127)
Openings of high schools				0.921***	0.802***
				(0.165)	(0.222)
First-stage F-statistic	–	41.63	18.44	30.85	22.85
N. of observations	27,714	27,714	27,714	27,714	27,714

Notes: Own calculation based on the estimation sample. This table repeats the analysis conducted for Table 2, additionally controlling for income and income squared (in 1,000 euros), self-reported health ranging from 0 (= bad) to 5 (= very good), and marital status (1 if the respondent has a partner and 0 otherwise). The baseline controls (also included in Table 2) are state, birth-cohort, and survey-year fixed effects, as well as an indicator for being female. The outcome variable, life satisfaction, is standardized to mean 0 and standard deviation 1. We use sample weights to ensure representativeness. Standard errors are reported in parentheses and allow for a correlation within each state-of-schooling × year-of-birth cell. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A5: Original 0–10 Likert-scale for life satisfaction: The role of employment status for the effect of years of schooling on life satisfaction

		Robustness checks: Additionally controlling for		
	(1) Baseline results	(2) state expenditures	(3) state population	(4) state-specific linear trends
Panel A: Second stage, not differentiated by employment status				
Overall effect	−0.040	−0.037	−0.017	0.034
	(0.038)	(0.035)	(0.043)	(0.045)
Panel B: Second stage by employment status				
Employed	0.109***	0.098***	0.103***	0.117***
	(0.038)	(0.032)	(0.039)	(0.036)
Non-employed	−0.256*	−0.249*	−0.252*	−0.246**
	(0.135)	(0.128)	(0.129)	(0.122)
Test for equality of coefficients (p-value)	0.004	0.006	0.005	0.004
First-stage F-statistic	20.89	21.03	21.26	18.42
N. of observations	27,714	27,714	27,714	27,714

Notes: Own calculation based on the estimation sample. This table reports IV estimates for the effect of years of schooling on life satisfaction. It repeats the analysis conducted for Table 4, but life satisfaction is not standardized and instead reported on the original 0–10 Likert-scale. Panel A states the overall effect based on a conventional 2SLS approach using all three instruments in a joint first stage. For comparison, column (1) retrieves the estimation from column (5) of Table 2, i.e., controlling for full sets of state, birth-cohort, and survey-year fixed effects, and a gender indicator. Columns (2)–(4) report robustness checks. In column (2), we additionally control for the state's expenditures for other policy areas than education when the individual was 9 years old (this is also the age we use to assign the number of intermediate and academic schools). In column (3), we control for population size in state of residence. In column (4), we add state-specific linear time trends to the baseline specification. Panel B estimates separate effects of (instrumented) years of schooling on life satisfaction for employed and non-employed individuals. Instead of separate regressions for employed and non-employed individuals, we interact fitted years of schooling in the second stage with employment and non-employment indicators. That is, the first stage is estimated according to equation (3) and additionally controls for employment status. In the second stage, we regress life satisfaction on fitted years of schooling for employed individuals, fitted years of schooling for non-employed individuals, an employment indicator, and baseline controls. The standard errors in panel B are bootstrapped (using 1,000 replications) and allow the error term to correlate within each state-of-schooling × year-of-birth cell. The p-values for a t-test of equality of the years of schooling coefficients for being employed and non-employed are displayed below the estimates. The F-statistics at the bottom of the table refer to the instrument(s) in the first stage. We use sample weights to ensure representativeness. Standard errors are reported in parentheses. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A6: Zooming in on non-employed individuals: The role of employment status for the effect of years of schooling on life satisfaction

		Robustness checks:		
	(1) Baseline results	(2) Including state expenditures besides the area of education	(3) Including state population per year	(4) Including state-specific linear trends
Unemployed	-0.133^{*}	-0.124^{*}	-0.127^{*}	-0.118^{*}
	(0.074)	(0.072)	(0.070)	(0.067)
Outside the labor force	-0.153	-0.161	-0.158	-0.148
	(0.151)	(0.149)	(0.162)	(0.197)
Test for equality of coefficients (p-value)	0.83	0.79	0.81	0.82
N. of observations	8,163	8,163	8,163	8,163

Notes: Own calculation based on the estimation sample. This table repeats the analysis conducted for Table 4, but splits non-employment into unemployment and being out of labor force. Apart from that, the specification is the same as in Table 4. The p-values for a test of equality (t-test) of the estimates for individuals being out of the labor force and unemployed individuals are displayed below the estimates. We use sample weights to ensure representativeness. Standard errors are reported in parentheses and allow for a correlation within each state-of-schooling × year-of-birth cell. They are bootstrapped using 1,000 replications. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A7: Employing each instrument separately: The effect of years of schooling (overall and by employment status) on life satisfaction

	(1) Compulsory schooling reforms	(2) Openings of intermediate schools	(3) Openings of high schools
Panel A: Second-stage, not differentiated by employment status			
Overall effect	−0.012	−0.016	−0.021
	(0.021)	(0.032)	(0.026)
Panel B: Second-stage by employment status			
Employed	0.054***	0.059***	0.061***
	(0.017)	(0.018)	(0.019)
Non-employed	−0.148*	−0.136*	−0.132*
	(0.079)	(0.071)	(0.069)
Test for equality of coefficients (p-value)	0.004	0.005	0.005
Panel C: First stage			
Years of schooling	0.539***	0.381***	0.746***
	(0.081)	(0.089)	(0.163)
First-stage F-statistic	44.27	18.32	20.94
N. of observations	27,714	27,714	27,714

Notes: Own calculation based on the estimation sample. This table repeats the analysis conducted for Table 4, column (1), employing the three instruments separately instead of a joint first-stage regression. The column header denotes the respective variable that instruments years of schooling. The outcome variable in the second stage is life satisfaction (standardized to mean 0 and standard deviation 1). The specification is the same as in Table 4, column (1). The coefficient in the first stage refers to the respective instrument. p-values for a test of equality (t-test) of the estimates for employed and unemployed individuals are displayed below the estimates. We use sample weights to ensure representativeness. Standard errors are reported in parentheses and allow for a correlation within each state-of-schooling × year-of-birth cell. They are bootstrapped using 1,000 replications. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A8: Alternative estimation sample: The role of employment status for the effect of years of schooling on life satisfaction

		Robustness checks:		
	(1)	(2)	(3)	(4)
	Baseline results	Including state expenditures besides the area of education	Including state population per year	Including state-specific linear trends
Panel A: Second-stage, not differentiated by employment status				
Overall effect	-0.038	-0.029	-0.021	-0.008
	(0.055)	(0.038)	(0.035)	(0.020)
Panel B: Second-stage by employment status				
Employed	0.056^{***}	0.057^{***}	0.054^{**}	0.061^{***}
	(0.020)	(0.019)	(0.023)	(0.021)
Non-employed	-0.133^{*}	-0.138^{*}	-0.133^{*}	-0.142^{**}
	(0.071)	(0.074)	(0.074)	(0.077)
Test for equality of coefficients (p-value)	0.005	0.004	0.005	0.003
First-stage F-statistic	20.43	20.83	21.06	17.78
N. of observations	25,926	25,926	25,926	25,926

Notes: Own calculation based on a sample that uses the 2010 wave as starting point. This table repeats the analysis conducted for Table 4, but unlike the baseline estimation sample, the sample used here uses the 2010 wave as starting point and adds additional observations through a backward-moving iterative process. We start by including individuals in the 2010 wave in our sample. If a respondent has missing values in 2010, we check whether the respondent has complete information in 2009, and, if not, in 2008, and so on. We employ this iterative process backward to the 1985 wave. Apart from that, the specification is the same as in Table 4. p-values for a t-test of equality of coefficients for employed and non-employed individuals are displayed below the estimates. We use sample weights to ensure representativeness. Standard errors are reported in parentheses and allow for a correlation within each state-of-schooling × year-of-birth cell. They are bootstrapped using 1,000 replications. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A9: The effect of years of schooling (overall and by employment status) on life satisfaction including city states

		Robustness checks:		
	(1) Baseline results	(2) Including state expenditures besides the area of education	(3) Including state population per year	(4) Including state-specific linear trends
Panel A: Second-stage, not differentiated by employment status				
Overall effects	−0.17	−0.009	−0.015	0.014
	(0.019)	(0.012)	(0.014)	(0.018)
Panel B: Second-stage by employment status				
Employed	0.066***	0.069***	0.059**	0.063***
	(0.023)	(0.019)	(0.018)	(0.020)
Non-employed	−0.147*	−0.141*	−0.153*	−0.133**
	(0.081)	(0.079)	(0.084)	(0.074)
Test for equality of coefficients (p-value)	0.004	0.004	0.004	0.006
First-stage F-statistic	19.23	18.76	20.01	17.97
N. of observations	29,600	29,600	29,600	29,600

Notes: Own calculation based on an extended estimation sample that includes the city states. This table repeats the analysis conducted for Table 4, using a more comprehensive sample. Apart from that, the specification is the same as in Table 4. The sample is constructed in the same way as the baseline estimation sample, but additionally includes respondents from the three city states Hamburg, Bremen, and West Berlin. The p-values for a test of equality (t-test) of the estimates for individuals being out of the labor force and unemployed individuals are displayed below the estimates. We use sample weights to ensure representativeness. Standard errors are reported in parentheses and allow for a correlation within each state-of-schooling $\times$ year-of-birth cell. They are bootstrapped using 1,000 replications. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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