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Abstract

Despite extensive research on nonmarket returns to education, direct and spillover effects on mental health are widely unstudied. This study is the first to analyze heterogeneous intergenerational effects of children's education on parents' mental health. Given ambiguous theoretical implications, I explore potential mechanisms empirically. Using Survey on Health, Ageing and Retirement in Europe (SHARE) data, I estimate IV regressions, exploiting country-level variation in compulsory schooling reforms. Increasing children's education reduces parents' long-term probability of developing depression. Fathers and more educated sons drive this beneficial effect. Since mental illness is frequently undiagnosed, the findings may help improve elderly-specific health care provision.

Keywords: compulsory schooling reforms, depression, old age, instrumental variable regression, intergenerational spillover

JEL classification: I12, I26, J14, J24, C36

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1 Introduction

Due to considerable population aging, understanding the specific health care needs of the elderly poses a major challenge for policy makers worldwide. Mental health problems are among the most important health issues of older adults as measured by disability and economic cost. For instance, they are often undiagnosed among the elderly (Wang et al. 2007), correlated with impaired physical health (Scott et al. 2016), and adversely affect labor force participation (Banerjee et al. 2017). In Europe, the total annual cost of depression, one of the most prevalent mental illnesses, alone amounts to more than 100 billion euros (Sobocki et al. 2006). In the US, the costs are even higher, as documented by Greenberg et al. (2015), who further find that recent increases in the prevalence of depression are mainly driven by individuals aged 50 years and older. Reflecting the relevance of this trend, numerous policies and initiatives aimed at improving mental health have recently been introduced. One of these is the "European Pact for Mental Health and Well-Being", which specifically acknowledges mental health of the elderly (World Health Organization 2015).

Although extensive research has identified various potential risk and protective factors for mental health, surprisingly little is known about the effect of education on mental health. In contrast, numerous studies have been devoted to analyzing its effect on *physical* health. In this literature, the relationship between education and health is ambiguous from a theoretical perspective and empirical evidence is inconclusive. Grossman (2015) and Galama et al. (2018) provide comprehensive reviews of the literature.

This study analyzes the long-term effect of children's education on their parents' mental health. In order to establish causality, I use compulsory schooling reforms as an instrumental variable (IV) for children's years of schooling, which is a widely accepted approach in the literature (see e.g. Galama et al. 2018). Using data from the Survey on Health, Ageing and Retirement in Europe (SHARE) from eleven European countries, I employ a clinical measure for depression, which has been validated across countries. Increasing children's education by one year reduces the long-term probability of their parents developing depression by 5.2 percentage points, on average. There is substantial heterogeneity in the effect by gender of both parents and children. Increasing children's years of schooling is more beneficial for fathers' than for mothers' mental health (8.2 vs. 4.0 percentage points). Also, improvements in parental mental health are driven by more educated sons. The results are strongly robust to various sensitivity analyses. Moreover, the results also highlight the importance of the previously widely neglected direction of intergenerational returns to education from children one generation *up* to their parents (henceforth referred to as "upward intergenerational spillover effects").

This paper makes important contributions to several branches of literature in economics, particularly to the literature on the effects of education on health and the literature on intergenerational returns to education. Evidence on health returns to education with a particular focus on own mental health is limited and, again, inconclusive. Some studies find protective effects of education for mental health (Crespo et al. 2014; Mazzonna 2014), while others even find adverse effects on mental health measures (Dursun and Cesur 2016; Avendano et al. 2017).[1] I contribute to this literature by providing evidence of causal upward intergenerational spillover effects of education on mental health for various European countries, which have not previously been analyzed. Additionally, there is also some concern about the causal nature of health returns to education from previous studies (see e.g. Stephens and Yang 2014). In this paper, I credibly identify causal effects of education on health.

Recent studies have extended the scope of the analysis of the health-education gradient by also considering spillover effects, mostly from parents to children. Studies investigating intergenerational health returns to parental education have mainly focused on the physical health outcomes of infants (Currie and Moretti 2003; Lindeboom et al. 2009; Chou et al. 2010; McCrary and Royer 2011; Carneiro et al. 2013; Grépin and Bharadwaj 2015) and adolescents (Kemptner and Marcus 2013; Lundborg et al. 2014; Huebener 2018). First evidence suggests that parental education also decreases the mental health of adolescent children (Graeber and Schnitzlein 2019).

Although there are several reasons why also the health of parents might be either positively or negatively affected by their children's education, this upward direction of intergenerational spillover effects is widely unstudied. First, more educated children might have more resources to support their parents' health and well-being in old age, while depending less on time and financial investments from them throughout adulthood.[2] As a consequence, parents might invest more in their own well-being. In line with this long-term investment mechanism, evidence suggests that parental transfers are negatively correlated with children's income (McGarry 2016; Haider and McGarry 2018). Second, parents of more educated children might be more content with their children's market and nonmarket achievements, such as potentially higher socioeconomic status and socially desirable family characteristics.[3] Due to the increased likelihood of parental expectations for their children being fulfilled or exceeded, parents might be less worried about their children and be more optimistic overall. Third, more educated children might use the time available more efficiently to support and be with their parents as education increases their nonmarket productivity (Grossman 1972). Fourth, as children stay in school longer, it is likely that they expand their network with community members and might thereby find local employment more easily. McHenry (2013) shows that this may reduce the regional mobility of individuals, particularly at the lower end of the education distribution. Parents and children might therefore live closer to one another, which reduces social isolation and depression among parents (Mosca and Barrett 2016; Ivlevs et al. 2019). Fifth, parents' human capital might increase due to direct information spillovers from their children's health knowledge. Even if children's health knowledge is unaffected by compulsory

[1] In addition, Kamhöfer et al. (2018) find no mental health returns specifically to college education.

[2] Previous studies find labor market returns to some schooling reforms (see e.g. Angrist and Krueger 1991; Black et al. 2005) but not to others (see e.g. Pischke and von Wachter 2008; Grenet 2013). Brunello et al. (2016a) show that at least some of the reforms studied in this paper significantly increase lifetime earnings.

[3] Concerning nonmarket outcomes, more educated individuals are more likely to be married and have more stable relationships, for instance (Fort et al. 2016; Lundberg et al. 2016).

schooling reforms, as suggested by Altindag et al. (2011), this mechanism might be important for two reasons. Since more educated children move out of their parents' household later (Keane and Wolpin 2001; Martinez-Granado and Ruiz-Castillo 2002), the parents are exposed to their children's human capital for longer in the short-run. In addition, parents and children living together longer might permanently strengthen the parent-child relationship. Parental exposure to health information might thereby increase in the long run, for instance through more intense and more regular contact with their children. Exploring this channel, Chen et al. (2019) find that having a health professional in the family is beneficial for parents' physical health. Regarding these mechanisms, I would expect a positive effect on parental mental health.

Conversely, children enter the labor force later as they remain in school longer. Thus, parents might provide emotional and financial support to their children for longer, instead of investing in their own health. This might translate into a negative, short-run effect on parental mental health. Furthermore, more educated adult children are likely to work more and might live farther away from their parents (Machin et al. 2012; Malamud and Wozniak 2012). Hence, they might have higher opportunity cost and generally less time for their parents. Lastly, parents might rely on their children's health knowledge and support more often, instead of investing in their own human capital (see e.g. Kuziemko 2014). These mechanisms would predict a negative effect on parental mental health. Given the ambiguous theoretical implications, I investigate the effect in question empirically and shed light on which of the mechanisms drive the results. The findings suggest that the effect is likely to operate through a combination of the long-term investment mechanism, decreased parental social isolation, and increased fertility of adult children.

First evidence indicates that increased education of children slightly decreases parental mortality in Sweden (Lundborg and Majlesi 2018) and Tanzania (De Neve and Fink 2018). The only study analyzing the effects of children's education on parents' self-reported health outcomes is by Ma (2019), who exploits regional variation in compulsory schooling reforms across China and finds ambiguous effects on parental health. Ma (2019) exemplarily demonstrates the interest in upward intergenerational health returns to education worldwide. Due to large differences in the institutional context between Europe and China with respect to family, education, and health policies as well as population health, among others, it is unclear whether her findings can be generalized to European and other developed countries such as the US. This paper adds first evidence of causal, long-term spillover effects of children's education on parental mental health in developed countries. The contribution to the scarce literature on *upward* intergenerational effects of education is also new in the sense that this paper is the very first to provide evidence on heterogeneous effects of children's education on parental mental health. The paper furthermore constitutes the first comprehensive cross-country evidence in the broader literature on intergenerational health returns to education since the sample comprises countries from southern, eastern, northern, and central Europe, none of which have yet been analyzed in this literature.[4] Moreover, this study contributes

[4] In contrast, such cross-country analyses are already more common in the literature on returns to own education

to the understanding of factors shaping health in older age which is of high policy relevance given recent trends in population aging. Since mental illness is strongly associated with increased mortality from various causes (Russ et al. 2012; Khan et al. 2013), the findings also suggest that the intergenerational effect of children's education on parental mortality may partially be explained by causal effects on parental mental health.

The paper proceeds as follows. Section 2 describes the compulsory schooling reforms used as part of the identification strategy and outlines the empirical approach. Section 3 introduces the data. I then present the main results together with analyses of mechanisms, treatment effect heterogeneity, and robustness tests in Section 4. Section 5 concludes.

2 Empirical approach

2.1 Schooling reforms and identification strategy

To solve the endogeneity problem of education, I exploit exogenous changes in the years of compulsory schooling across eleven European countries. The schooling reforms were implemented at the national level between 1959 and 1986. Specifically, I use reforms from Austria, Belgium, Czech Republic, Denmark, France, Greece, Italy, the Netherlands, Poland, Portugal, and Spain. Similar reforms introduced at a regional level in countries such as Germany or Sweden are excluded to avoid measurement error. Table 1 provides detailed information about the reform characteristics. All reforms increased compulsory schooling by one to four years, as displayed in the last column of Table 1. The majority of the reforms were implemented in the 1960s and 1970s and increased compulsory schooling by one year.

The identification strategy requires two main assumptions. First, other changes across the countries examined which are correlated with the outcome variable are uncorrelated with the implementation of the schooling reforms. That is, I assume that the reforms affect parental mental health only through children's years of schooling. Changes in the quality of schooling due to the reforms pose a potential threat to this assumption. For instance, increasing compulsory years of schooling may have caused a temporary shortage of teachers. Following Lundborg et al. (2014), I argue that, if the reforms caused such changes in the quality of schooling at all, these were likely to occur only in the short term. The imposed sample restrictions described in Section 3 address this potential issue.

The second identification assumption is that children's exposure to the compulsory schooling reforms is as good as random, conditional on country and year of birth fixed effects as well as country-specific trends. Policy endogeneity might pose a threat to this assumption. For instance, the reforms might reflect parents' demand for their children's education. As part of the sensitivity analysis, I provide empirical evidence for the exogeneity of the schooling reforms.

(see e.g. Mazzonna 2014; Fort et al. 2016).

2.2 Instrumental variable estimation

My preferred estimation approach employs two stage least squares (2SLS) in order to estimate the effect of children's education on parental health:

$$Health^p_{ict} = \beta_1 YrsSchool_{ict} + \boldsymbol{\beta_2 x_{ict}} + \boldsymbol{\lambda_c} + \boldsymbol{\mu_t} + \boldsymbol{Trend_{ct}} + \epsilon_{ict}, \tag{1}$$

$$YrsSchool_{ict} = \gamma_1 CompSchool_{ct} + \boldsymbol{\gamma_2 x_{ict}} + \boldsymbol{\zeta_c} + \boldsymbol{\eta_t} + \boldsymbol{Trend_{ct}} + \xi_{ict}, \tag{2}$$

where $Health^p_{ict}$ denotes the health outcome of parent p of child i born in country c in year t. $YrsSchool_{ict}$ are the child's years of schooling. The instrumental variable $CompSchool_{ct}$ in the first stage (equation 2) refers to the change in the number of compulsory schooling years induced by the reforms and is zero for all unaffected cohorts. $\boldsymbol{x_{ict}}$ is a vector of additional control variables. Specifically, I control for education, age, and gender of parent p, child i's gender, and the interview year. $\boldsymbol{\lambda_c}$ and $\boldsymbol{\zeta_c}$ are vectors of country fixed effects, while $\boldsymbol{\mu_t}$ and $\boldsymbol{\eta_t}$ are vectors of child birth cohort fixed effects. $\boldsymbol{Trend_{ct}}$ are vectors of country-specific quadratic time trends, which account for unobserved factors affecting countries differently over time (Stephens and Yang 2014).[5] ϵ_{ict} and ξ_{ict} are error terms, which I cluster at the country level.[6]

In addition to the preferred IV estimation strategy, I also estimate equation (1) using ordinary least squares (OLS) regressions. This naïve approach is applied to compare the arguably causal effects from IV regressions with potentially endogenous correlations of children's education and parental mental health. Moreover, I obtain reduced-form estimates by substituting equation (2) into equation (1) as follows:

$$Health^p_{ict} = \psi_1 CompSchool_{ct} + \boldsymbol{\psi_2 x_{ict}} + \boldsymbol{\nu_c} + \boldsymbol{\theta_t} + \boldsymbol{Trend_{ct}} + \omega_{ict}, \tag{3}$$

which convey an understanding of the overall impact of children's exposure to the compulsory schooling reforms on parental mental health.[7] In all regressions, I weight the observations by the inverse of the parent's number of children as parents may have more than one child and, thus, potentially be affected by the reforms several times.

3 Data

The data I use comprise waves 1, 2, 4, 5, and 6 of the Survey on Health, Ageing and Retirement in Europe (SHARE, version 6.1.1), an ongoing, multi-national panel survey data set.[8] It contains

[5] Using country-specific linear or cubic time trends yields very similar results, as shown in Section 4.

[6] Results from clustering standard errors at the country-child cohort level are generally similar and available upon request. However, these standard errors might be substantially downward-biased in favor of a seemingly strong first stage (Bertrand et al. 2004; Black et al. 2008). In light of this, my preferred specification is more conservative.

[7] The reduced-form results can therefore be interpreted as intention-to-treat (ITT) estimates.

[8] I do not use wave 3 as it is based on a different questionnaire and focuses on the respondents' life histories. DOIs: 10.6103/SHARE.w1.611, 10.6103/SHARE.w2.611, 10.6103/SHARE.w4.611, 10.6103/SHARE.w5.611, 10.6103/SHARE.w6.611.

information on health, labor, and socioeconomic characteristics such as educational attainment for approximately 25,000 individuals aged 50 years and older. As the waves differ slightly with respect to the countries surveyed, pooling the data allows me to include additional schooling reforms in the analysis (see Börsch-Supan et al. (2013) for methodological details).

I use the SHARE data for several reasons. First, the data include additional information at the household and family level. This enables me to observe socioeconomic characteristics of children, including age and education. Following previous studies, I then derive years of schooling from year of birth and educational attainment (see e.g. Pischke and von Wachter 2008; van Kippersluis et al. 2011; Brunello et al. 2013; Clark and Royer 2013; Lundborg et al. 2014; Brunello et al. 2016b; Lundborg and Majlesi 2018). Second, SHARE's large sample size allows me to analyze individuals with children born in a relatively narrow time period prior and subsequent to specific events. Third, unlike administrative data, the SHARE data comprise both broad and rather specific self-reported health measures. This is of particular relevance for this study as mental illness is frequently undiagnosed among the elderly (Wang et al. 2007). All in all, SHARE is one of very few, if not the only data set which allows me to obtain a relatively large sample with detailed information on health and education across generations and countries.

The main mental health outcome I analyze is depression caseness measured using the EURO-D scale (Prince et al. 1999a; Prince et al. 1999b). I focus on depression as it is among the most common mental health conditions in older individuals (see e.g. Substance Abuse and Mental Health Services Administration 2013) and is associated with numerous physical health conditions (Scott et al. 2016) as well as higher mortality from various causes (Russ et al. 2012; Khan et al. 2013). The EURO-D scale is specifically designed to detect depression in the elderly and has been clinically validated across countries. It covers twelve different domains of health and well-being. Specifically, individuals are asked whether they struggle with fatigue, guilt, irritability, and lack of appetite, concentration, enjoyment, or interest, pessimism, sadness or depression, trouble sleeping, suicidality, or tearfulness. Most of these questions refer to either the present or the situation in the last month. Based on the reported number of symptoms, a sum ranging from zero to twelve is calculated. In accordance with Prince et al. (1999b), the indicator variable for depression caseness takes on the value of one if an individual reports four or more symptoms and zero otherwise.[9]

I restrict the sample to parent-child pairs for whom I observe the main variables displayed in Table 2. The second main sample restriction is that I only consider parents born in the country where they are living at the time of the interview. Imposing this sample restriction serves two purposes. First, it reduces measurement bias by excluding children who are likely to have received their education in a different country prior to migrating. Second, it reduces bias due to endogenous

[9] In the robustness section, I also use three as an alternative cut-off point for depression caseness and a variable indicating whether an individual reported being sad or depressed. Coe and Zamarro (2011) and Brunello and Rocco (2018) use similar alternative outcome definitions, for instance.

mobility of families in response to policy changes. For instance, families might selectively move to a different country in order to benefit from or avoid changes in education policies. One advantage of analyzing national changes in education policies compared to regional changes is that bias due to selective mobility based on unobservables is generally less relevant (see e.g. Holmlund et al. 2011). Restricting the sample to nonmigrant parents allows me to even further address this potential problem. Additionally, I apply two minor sample restrictions. I only consider children born between one and ten years before and after the implementation of a compulsory schooling reform, that is, I exclude the pivotal cohort for each reform as some of these children might not actually have been exposed to the reform yet. This sample restriction also allows me to address the potential issue of a shortage of teachers or other short-term changes in the quality of schooling, which might be related to the compulsory schooling reforms (Lundborg et al. 2014). Children who did not complete primary education are also excluded.[10]

Ultimately, the main sample comprises 42,082 observations of parent-child pairs.[11] Table 2 shows the descriptive statistics of the control and outcome variables. Columns one and two report means and standard deviations for the whole sample, respectively. Children have, on average, 11.15 years of schooling, compared to the 9.14 years of schooling of parents. The mean age of children and parents is 46.72 and 73.81 years, respectively. On average, 31.25 percent of parents meet the criteria for depression as measured by the EURO-D scale. The average EURO-D score of parents is 2.71. Columns three to six report means and standard deviations by the children's reform exposure status. Unconditionally, parents whose children were affected by the compulsory schooling reforms are less likely to be depressed (30.19 vs. 33.60 percent). Unconditionally, mean schooling and age are also higher among parents and children in families affected by the reforms.

4 Results

In this section, I initially present the results from the OLS regressions before turning to the first stage, reduced-form, and IV results. Subsequently, I compare the results to related studies, investigate potential mechanisms as well as treatment effect heterogeneity, and perform a variety of sensitivity analyses.

4.1 OLS results

Table 3 shows the results from the OLS regressions to illustrate the basic, endogenous relationship between children's education and parental mental health. The results in column (1) are based on a simple specification without any fixed effects, trends, or control variables and suggest that

[10]As not all countries clearly differentiated between primary and secondary education during the relevant time period, I simply exclude all observations of children with four years of schooling or less.

[11]Following Carneiro et al. (2013), I include multiple observations of parent-child pairs in the sample to increase the estimates' precision. Clustering standard errors at the country level allows me to address arbitrary dependence between such observations. The results are strongly robust to using only the first observation of each parent-child pair instead, as shown in Table 10.

parents of children with one additional year of schooling are 2.1 percentage points less likely to be depressed. When I add country and child birth cohort fixed effects in column (2), the estimate becomes smaller in magnitude but remains highly statistically significant. The estimates do not change when I also add country-specific quadratic trends and additional control variables to the model in columns (3) and (4), respectively. Specifically, I additionally control for parental age and gender, child's gender, and the interview year in column (4).

Column (5) presents the results for the preferred specification, which also considers parental education as a control variable. Parents of children with one additional year of schooling are 0.9 percentage points less likely to be depressed. The results in column (5) are very similar to columns (1) to (4) with respect to the direction and statistical significance, but are slightly smaller in magnitude. For all specifications, parents with more educated children have better mental health.

Moreover, as displayed in Panels B to E, there is heterogeneity in the estimates by gender of the parents and children. Panels B and C show that fathers and mothers of children with one additional year of schooling are less likely to be depressed (0.6 vs. 1.0 percentage points, respectively). The results in Panels D and E show that one additional year of schooling of sons and daughters is associated with less parental depression (0.8 vs. 1.0 percentage points, respectively).

4.2 First stage results

Table 4 shows the results for the first stage regressions of children's years of schooling on the change in compulsory years of schooling induced by the reforms. The columns are arranged as in Table 3. Across all columns, the results suggest increased children's education due to the compulsory schooling reforms. While the estimates for the simple specification in column (1) are very noisy, they become highly statistically significant as soon as I add country and child birth cohort fixed effects in column (2). Adding country-specific quadratic time trends and additional control variables in columns (3) and (4), respectively, does not change the results.

The results for the preferred specification in column (5) show that an additional year of compulsory schooling increases children's actual years of schooling by 0.155 years, on average. The first stage estimate is smaller for male than for female children, as shown in Panels B and C, respectively (0.121 vs. 0.188 years). This pattern emerges across all columns and may be explained by higher levels of education of males during the time period examined, irrespective of compulsory years of schooling. However, the F-statistic is greater than the commonly used cutoff value of ten for all children (Panel A), male children (Panel B), and female children (Panel C), indicating that the instrumental variable is sufficiently strongly correlated with children's years of schooling (Staiger and Stock 1997).

In addition to the first stage estimates, Figure 1 displays the relationship between the compulsory schooling reforms and children's education graphically.[12] For the cohorts of children born one

[12]Since the average increase in the number of compulsory schooling years induced by the reforms is more than one year (see Table 1), the change in children's years of schooling displayed in Figure 1 does not directly correspond to

to ten years before and after the reforms were implemented, there is a slight upward trend in years of schooling. However, the unconditional mean years of schooling of children born in all displayed years after the reforms is considerably higher than in the years prior to the reforms. Additionally, Figure 2 shows slight gender differences in the upward jump in children's education induced by the compulsory schooling reforms. Similar to the results in Table 4, the descriptive graphs highlight that the increase in sons' education (Panel A) is moderately smaller than the increase in daughters' education (Panel B). Furthermore, Figure 2 illustrates that the general upward trend in education documented in Figure 1 is mainly driven by the generally increasing education of males. All in all, the descriptive results from Figures 1 and 2 support the empirical evidence presented in Table 4 that the reforms increase children's education.

4.3 Reduced-form results

Table 5 presents reduced-form results for the long-term spillover effect of children's exposure to the compulsory schooling reforms on parental mental health. The table is organized in the same way as Table 3.

The results across all columns suggest small benefits of the reforms for parental mental health. Parents whose children were obliged to complete more years of schooling are, on average, 0.8 percentage points less likely to be depressed (column 5). Furthermore, I find that fathers benefit from their children's exposure to the reforms more than mothers (Panels B and C). Moreover, the intention-to-treat effect on parental mental health is entirely driven by sons' education (Panels D and E). The gender-specific results displayed in Panels B to E are very similar across all columns.

In addition to the reduced-form estimates, Figure 3 provides a graphical illustration of the unconditional relationship between children's reform exposure and parental mental health. The results exhibit a pattern of discontinuously decreased depression after the reforms, which matches the empirical results in Table 5. Furthermore, there is no trend in parental health over time before the schooling reforms. In fact, the level of parental health remains virtually unchanged throughout all years prior to the implementation of the compulsory schooling reforms, which provides some descriptive evidence that the estimates do not simply pick up naturally deteriorating health of aging parents.

4.4 IV results

Table 6 shows the IV results of the causal effect of children's education on parental mental health. The table is organized in the same way as Tables 3 and 5. That is, I begin with the simple specification without fixed effects, trends, or control variables in column (1). In columns (2) to (4), I sequentially add country and child birth cohort fixed effects, country-specific quadratic trends,

the effect of one additional year of compulsory schooling as shown in Table 4. When regressing children's years of schooling on a simple indicator for reform exposure which neglects the change in the number of compulsory schooling years, the estimate is 0.452 and, hence, corresponds well to Figure 1.

and additional control variables to the model. The set of control variables in column (4) comprises parental age and gender, child's gender, and the interview year. Finally, I also adjust for parental education in the preferred specification in column (5).

The results in column (1) suggest that one additional year of children's education decreases the long-term probability of their parents developing depression by 7.5 percentage points. While the estimate in column (2) of Panel A is smaller and too imprecise to be statistically significant, the results in columns (3) and (4) are, again, statistically significantly estimated and suggest that one additional year of children's schooling decreases parental depression by approximately 5 percentage points.

The effect for the preferred specification in column (5) is very similar: Increasing children's years of schooling by one year decreases their parents' long-term probability of depression by 5.2 percentage points, on average. This effect corresponds to approximately ten percent of a standard deviation (see Table 2). Given that the effect estimated by IV is larger than the association from OLS regressions, it appears likely that the general population differs from the population affected by the compulsory schooling reforms (that is, the compliers). For instance, the reforms are likely to rather affect children at the lower end of the education distribution. Following Imbens and Angrist (1994), I therefore interpret the IV results as local average treatment effects (LATE). There are several additional reasons why the OLS results might be downward biased. First, previous studies document diminishing marginal rates of physical and mental health returns to education (Auld and Sidhu 2005; Grignon 2008; Bracke et al. 2013). In line with these findings, Figures A1 and A2 in the Appendix highlight a generally negative, highly non-linear relationship between depression caseness in old age and own as well as children's education, respectively. Again, this descriptive evidence underlines the local property of the IV results as it suggests different mental health returns across the education distribution. Furthermore, if the schooling reforms increase health knowledge at the child or family level, either more educated children or the parents themselves are likely to be more aware of both formally diagnosed and undiagnosed health issues. Consequently, these parents might be more likely to report symptoms relevant for mental health than the parents of less educated children. Such non-random misreporting would imply a downward bias of the OLS results. In support of this, the medical literature further highlights various ways in which education at the individual and family level may predict awareness, acceptance, diagnosis, and treatment of mental health. For instance, mental health stigma and treatment discontinuation are more common among the less educated (Sirey et al. 2001; Griffiths et al. 2008; Barry et al. 2012). Moreover, family support is positively correlated with response and adherence to treatment (Trivedi et al. 2005). Hence, more educated children might indeed encourage and help their parents to acknowledge potential mental health issues, get diagnosed, and receive appropriate treatment. This might be likely to affect the parents' reporting behavior towards mental health, again implying a downward bias of the OLS results.

There is considerable heterogeneity in the treatment effect by gender of the parents and the children. Panel B shows that fathers' probability of being depressed decreases by 8.2 percentage points due to one additional year of children's schooling. In comparison, the effect on mothers' mental health displayed in Panel C is less than half of that (4.0 percentage points). The pattern of stronger beneficial effects of children's education on the mental health of fathers compared to mothers is consistent across all columns. Interestingly, the gender differences are even larger between male and female children in Panels D and E, that is, sons and daughters, respectively. The results in Panel D show that one additional year of sons' schooling decreases the probability of parental depression by 8.5 percentage points. In contrast, I find no evidence that daughters' education affects their parents' mental health (Panel E). Again, both effects from Panels D and E are very similar across all columns with respect to size, direction, and statistical significance.

4.5 Comparison with related studies

To the best of my knowledge, there are no previous studies analyzing the causal spillover effects of education on parental mental health outcomes in Europe or other developed countries. The study most closely related to mine is recent work by Ma (2019), who analyzes the upward intergenerational spillover effect of children's education on parental health and mental well-being in China.[13] Additional studies most comparable to mine are from the broader literature on the long-term effect of education on own depression (Crespo et al. 2014; Mazzonna 2014; Avendano et al. 2017) and on the upward intergenerational effect of education on physical health (De Neve and Fink 2018; Lundborg and Majlesi 2018).

Ma (2019) uses CHARLS data from China and an IV strategy to analyze health outcomes, including the CES-D mental health scale. Her findings of increased parental physical health and cognition due to additional children's schooling are in line with the results from this study. With respect to the direction of the effect on parental mental health, her results, although very imprecisely estimated, are again generally similar to mine. It is unclear whether these results are directly comparable, irrespective of substantive differences in the institutional context concerning family, education, and health policies as well as population health, for instance. The mental health outcome she reports is the raw CES-D scale. However, the literature suggests employing cut-off points and transforming the scale into an indicator variable if the interest is in measuring depression instead of mere symptoms (see e.g. Radloff 1977; Turvey et al. 1999).[14] Since Ma (2019) does not analyze treatment effect heterogeneity, the present study is also the first to provide evidence on heterogeneous effects of children's education on parental mental health.

[13] The only other study on any kind of causal spillover effects of education on mental health that I am aware of is by Graeber and Schnitzlein (2019) who investigate how parental education impacts the mental health of the next generation. This downward direction of spillover effects is commonly studied in the literature on intergenerational returns to education, which stands in stark contrast to the opposite, upward direction I explore in this study.

[14] For instance, any symptom on its own such as lack of appetite does not necessarily indicate depression. Depression is rather defined by the prevalence of several symptoms. In consequence, the change from having zero to one symptom of depression has a different implication than the change from having three to four symptoms. Altogether, it is less clear to what extent the raw CES-D scale can actually be interpreted as a measure of depression.

Regarding the studies additionally comparable to mine, Crespo et al. (2014) and Mazzonna (2014) use SHARE data, an IV design, and the same mental health outcome as in this study. They both find that one additional year of own schooling decreases the long-term probability of depression for elderly Europeans. Specifically, Crespo et al. (2014) find that increasing education reduces the probability of depression by 6.5 percentage points. While their effect is statistically significantly and robustly estimated, the decreased probability of depression of approximately 3 percentage points found by Mazzonna (2014) appears less robust to model specifications. These direct effects are of very similar magnitude to the intergenerational spillover effect presented in my main analysis. Of the two studies, only Mazzonna (2014) investigates gender differences in the mental health returns to own education. The gender-specific intergenerational effects which I find are slightly larger in size and more precisely estimated, yet generally very similar to the gender-specific direct effects from Mazzonna (2014).[15] He finds that the effects on mental health are mainly driven by males: One additional year of own schooling decreases the probability of depression by approximately 6 to 7 percentage points for elderly males. In what is arguably a different setting, Avendano et al. (2017) analyze long-term mental health returns to own education using a regression discontinuity design and a compulsory schooling reform in the UK. In contrast to Crespo et al. (2014), Mazzonna (2014), and this study, they find that one additional year of schooling increases the probability of depression or anxiety by 3.2 percentage points. They explain this adverse effect of the British reform on mental health through higher emotional stress and unfulfilled expectations as the specific reform under examination may not have yielded any labor market returns, for instance.[16] Similar to Crespo et al. (2014), they also do not investigate treatment effect heterogeneity by gender. These seemingly contradictory findings for continental Europe and the UK are not per se surprising since previous studies have outlined institutional features specific to the UK which arguably drive differences in the returns to education between both regions (see e.g. Fort et al. 2016).

With respect to the literature on upward intergenerational effects of education on health, De Neve and Fink (2018) and Lundborg and Majlesi (2018) analyze the effect of children's education on parental mortality employing instrumental variable estimation and data from Tanzania and Sweden, respectively. De Neve and Fink (2018) find that one additional year of schooling decreases the probability of paternal and maternal death by 0.8 and 3.7 percentage points, respectively. The gender pattern in the upward intergenerational mortality effect is opposed to mine. However, they find mortality effects for both parents which is generally similar to the intergenerational paternal and maternal mental health effects found by this study, despite the largely different institutional contexts. In an institutional setting more similar to mine, Lundborg and Majlesi (2018) find no effect of children's education on parental mortality in their overall sample. However, they do

[15] It is not surprising in itself that the intergenerational spillover and direct effect are of similar magnitude as some prior studies also find larger spillover than direct effects of education on health outcomes (see e.g. Huebener 2018; Ma 2019).

[16] In contrast, Clark and Royer (2013), however, find that the same reform had significant effects on wages.

find moderate treatment effect heterogeneity by gender. Their estimates suggest that lengthening children's education by one year increases the likelihood of fathers surviving to ages 75 and 80 by 2.1 and 1.3 percentage points, respectively. In contrast, they find no effects of children's education on mothers' survival. This pattern corresponds with the gender differences that I find in the intergenerational effects on parental mental health. In summary, my findings are generally in line with previous studies on the effects of education on *own* mental health and parental *physical* health.

4.6 Mechanisms

As outlined in detail in the introduction, there are at least eight potential mechanisms through which children's education might affect parental mental health. In Tables 7 and 8, I empirically analyze child and parent outcomes, respectively, to shed light on which of these mechanisms drive the results. Both tables comprise a combination of market and nonmarket outcomes.

Columns (1) and (2) of Table 7 show that children's education has hardly any effect on their own labor market participation as measured by the probability of being full-time employed and unemployed, respectively. These results are generally supported by Pischke and von Wachter (2008) and Stephens and Yang (2014) who find no employment effects of education.[17] In the remaining columns, I explore several nonmarket, direct returns to children's education. Similar to Fort et al. (2016) and Lundberg et al. (2016), I find that increased education raises the probability of being married (column 3). In particular, the marriage effect is large (10.0 percentage points) and strongly supported by Fort et al.'s (2016) gender-specific findings for Europe. One additional year of schooling also increases completed fertility by approximately 0.22 children (column 4). The fertility effect is entirely driven by more educated males (Panel B). In line with McCrary and Royer (2011), I do not find an effect of females' education on their completed fertility (Panel C). While Fort et al. (2016) find that one additional year of schooling raises females' completed fertility by 0.2 to 0.3 children, this effect is very similar to my overall estimate in Panel A. Column (5) shows that there is no overall effect of education on children's probability of living close to their parents. However, daughters are more likely to live close to their parents due to one additional year of schooling (Panel C). This gender pattern is in line with findings from Chan and Ermisch (2015). Column (6) presents evidence that one additional year of schooling increases the children's probability of having regular contact with their parents, irrespective of the child's gender.

Next, in Table 8, I turn to spillover effects on different parent outcomes. The results in column (1) suggest that fathers are more likely to be financially stable as a result of their children's increased education.[18] Furthermore, fathers are more likely to retire early as a result of their children's additional education, while this effect is absent for mothers (Panels B and C of column

[17] One limitation of the data is that I cannot use alternative measures for children's labor market returns to their education such as wages, which are predominantly analyzed in the related literature. I therefore do not want to place too much weight on the small estimates in columns (1) and (2) of Table 7.

[18] I measure parental financial stability using a variable indicating whether parents easily make ends meet.

2, respectively). Hence, fathers of more educated children might be more likely to be able to afford (early) retirement, as opposed to mothers. This gender-specific pattern is generally in line with my main findings and provides a plausible explanation for why I find smaller maternal mental health returns to their children's education. Retirement substantially decreases the stress and physical strain related to employment, for instance. In support of this, previous studies show that retirement is beneficial for mental health (Johnston and Lee 2009; Eibich 2015; Gorry et al. 2018).[19] In the remaining columns of Table 8, I focus on nonmarket parent outcomes. I find no evidence that children's education changes the parents' probability of retiring due to impaired health (column 3). Increasing children's education also does not alter the parents' probability of being married (column 4). Column (5) shows that, overall, increasing children's education does not affect elderly parents' provision of childcare to the offspring born to these more educated children (Panel A). However, mothers are more likely to provide childcare to their more educated children's offspring as a result of the children's additional education (Panel C).

Overall, the results from Tables 7 and 8 suggest that it is rather a combination of different mechanisms that drives the results. First, the findings provide ample support for the long-term investment mechanism, that is, more educated children being less dependent on their parents throughout adulthood. In contrast, my findings do not support the mechanism of parents investing less in their own health due to higher investments in children during adolescence.[20] Children's education might be beneficial for parental mental health and particularly paternal mental health because children's education slightly increases financial outcomes and retirement opportunities for fathers. Second, the gender-specific pattern for the effects of children's education on their fertility generally matches the pattern for the main effects. It is, therefore, an additional, plausible explanation for the treatment effect heterogeneity by children's gender. The implications of the fertility mechanism are in line with Chen and Fang (2018) who find that having fewer children decreases elderly parents' mental health. Moreover, as Brunello and Rocco (2018) show that increased provision of childcare decreases grandparents' mental health, the findings presented in column (5) of Table 8 provide an additional, plausible explanation for why I observe smaller maternal mental health returns to their children's education. Thus, the results suggest that having adult children who themselves have children has a protective effect on elderly parents' mental health, as long as these parents are not increasingly involved in caring for their children's offspring. Third, I find no evidence of changes in parents' geographic distance to children driving the overall results. Fourth, exchange of information between parents and children may partially explain the effects on parental mental health. The findings from Table 7 suggest that, in general, regular contact with children decreases the social isolation of the parents, which, again, improves their

[19] Although this literature is mostly conclusive, some evidence also points towards a negative effect of retirement on mental health (Dave et al. 2008; Heller-Sahlgren 2017).

[20] While there could be negative short-term health implications of children's education that I cannot observe, I find no evidence of such potential effects persisting into old age.

mental health.[21] This plausible explanation is in line with evidence from Mosca and Barrett (2016) and Ivlevs et al. (2019). However, the benefit of contact with daughters for parental mental health is likely to be offset by other mechanisms. For instance, couples are more likely to live closer to the woman's parents (Chan and Ermisch 2015), which is similar to my findings. As residential proximity is necessary for the provision of childcare, it is likely that the plausible adverse mental health effect of elderly mothers' involvement in childcare for their adult children's offspring mainly affects mothers of more educated daughters.

My findings provide no evidence in favor of the opportunity cost mechanism.

4.7 Effect heterogeneity by social isolation and relative education

In Table A1 in the Appendix, I also conduct a more thorough analysis of two of the mechanisms discussed by shedding light on potential treatment effect heterogeneity. Since social isolation is one of the main predictors of mental illness among the elderly, I initially focus on the role of this factor for the effect of children's education on parental mental health. Specifically, the proxies for social isolation for parents in Panels A and B are not living in cohabitation and living alone, respectively. In each of these two Panels, column (1) presents the results for the more socially isolated subsample (i.e. parents who are not cohabiting and who live alone), whereas column (2) presents the results for the less socially isolated subsamples (i.e. parents who are cohabiting and who do not live alone). The estimates for the more socially isolated parents range from a 5.0 to 9.8 percentage point decrease in parental depression due to one additional year of children's schooling (column 1). Furthermore, all estimates for the less socially isolated parents (column 2) are statistically insignificant and smaller in magnitude compared to column (1) and the main effect.

To provide us with a better understanding of the role of information spillovers, the remaining Panels of Table A1 in the Appendix present further treatment effect heterogeneity by the children's education relative to their parents' (Panel C) and siblings' (Panel D) education. Since children's education may serve as a substitute for parents' education (Kuziemko 2014), the importance of a child's education for parental health and health behavior is likely to depend on the degree to which parents rely on the education of that particular child in order to access information. The results in columns (1) and (2) of Panel C show that the beneficial effect for parental mental health is largely driven by parent-child pairs in which the parent is not more educated than the child (7.9 vs. -10.9 percentage points, respectively, though the former estimate is very imprecise). Panel D presents heterogeneous treatment effects by the education of children relative to their siblings. Increasing the years of schooling by one year for children who are not more educated than their

[21] Additionally, in Section 4.7 I investigate whether information spillovers that are potentially relevant for parental health and health behavior might also partially drive the results.

siblings reduces their parents' probability of depression by 3.8 percentage points (column 2).[22] This estimate is smaller in magnitude than the main effect. Hence, the results from Panels C and D generally suggest that parents benefit less from their children's education if they have alternative means of accessing (health) information such as through their own education or the education of other children, in other words, if they rely less on knowledge spillovers from that particular child.

Overall, the findings from Table A1 in the Appendix underline that social isolation is an important mechanism and also show that upward intergenerational returns to education are particularly pronounced among more socially isolated parents. Moreover, the findings provide some suggestive evidence in favor of the (health) information spillover mechanism.

4.8 Sensitivity analysis

In this section, I first analyze further potential threats to the identification strategy. Second, I thoroughly examine the robustness of the results concerning the estimation approach as well as alternative model specifications.

A potential threat to the identification strategy is selective exposure to the compulsory schooling reforms. While I already address selection into treatment due to endogenous mobility (see Section 3), family characteristics and particularly parental education might still predict reform exposure if the policies reflect parents' demand for their children's education. In the case of such policy endogeneity, children's education might simply be a proxy of parental education, regardless of the compulsory schooling reforms.

In Table 9, I provide empirical evidence that children's exposure to the compulsory schooling reforms is exogenous. The specification in column (1) is a very simple regression of children's reform exposure status on parental education without fixed effects or any other control variables. Parental education explains barely any variation in the reform status, as illustrated by the small adjusted R^2 value of 0.01 (Panel A). As soon as country and child birth cohort fixed effects enter the regression in column (2), the estimate of parental education further drops precisely to zero. Adding country-specific quadratic trends and additional control variables in columns (3) and (4), respectively, does not change the results substantially.[23] I also investigate whether the education of fathers and mothers might differentially influence their children's reform exposure status and present the results in Panels B and C, respectively. The gender-specific results are precisely in line with those in Panel A. Neither paternal nor maternal education predicts children's reform exposure status as soon as I add country and child birth cohort fixed effects. All in all, the results in Table 9 further demonstrate exogeneity of the instrument.

Table 10 provides additional, comprehensive sensitivity analyses regarding potential identifica-

[22] I do not find a statistically significant effect of children's education on parental mental health if the child is more educated than their siblings (column 1). However, I do not want to overemphasize this estimate given that the compulsory schooling reforms were not targeted at this specific, rather highly educated subgroup of children.

[23] Just as in the main analysis, the results are robust to using country-specific linear and quadratic trends. Results are available upon request.

tion, estimation, and model specification issues for the pooled sample (see Table A2 in the Appendix for the corresponding gender-specific results). Panel A of Table 10 displays results from placebo regressions which, again, underline the credibility of the identification strategy. I construct placebo reforms for each country with artificial implementation dates three years prior to the actual reform dates.[24] The estimates for all specifications (columns 1-5) are small in magnitude and statistically insignificant. Moreover, there is no clear pattern in the direction of the estimates across columns. Rather, the placebo estimates appear to fluctuate randomly around zero. Overall, these results also strongly indicate that the identification strategy allows me to identify causal effects.

Next, in Panels B to D of Table 10, I address potential issues related to the estimation approach. Initially, I estimate the main specification using only the first observation of each child-parent pair (Panel B). Subsequently, in Panel C, I assess the robustness of the results to restricting the sample to cohorts of children born one to seven years before and after the reforms were implemented. In Panel D, I analyze whether restricting the sample to cohorts of children born two to ten years prior to and after the reforms were implemented has an impact on the results (i.e. I exclude the pivotal and also the two adjacent cohorts). This specification also addresses potential threats to the identification strategy due to short-term changes in the quality of schooling induced by the reforms, for instance (Lundborg et al. 2014). The estimates across all columns are very similar to the main analysis, despite the smaller sample size in these sensitivity analyses and therefore slightly reduced precision.

Panels E to H address a variety of model specification issues. The specifications in Panels E and F include country-specific linear and cubic trends, respectively, instead of quadratic trends. Finally, in Panels G and H, I employ alternative parental mental health outcomes. Specifically, the dependent variable in Panel F indicates whether an individual reported being sad or depressed. In Panel H, I code depression caseness as having a EURO-D score of at least three. The results are strongly robust to all of these different model specifications. All estimates in these sensitivity analyses are very similar to the main analysis with respect to size, direction, and statistical significance and demonstrate strong robustness of the effects.

5 Conclusion

This is the first study to analyze the causal effect of children's education on parental mental health in developed countries. Additionally, this study is the very first to provide evidence on heterogeneous effects of children's education on parental mental health. To overcome the endogeneity of education, I estimate instrumental variable (IV) regressions and exploit exogenous variation in compulsory schooling reforms across eleven European countries. Based on data from the Survey on Health, Ageing and Retirement in Europe (SHARE) and a clinical measure for depression

[24]Results employing placebo reforms four or five years before the actual reforms are very similar and available upon request.

validated across countries, the results from reduced-form and IV regressions highlight important upward intergenerational returns to education for parental mental health. Specifically, I find that one additional year of children's schooling decreases their parents' long-term probability of developing depression by 5.2 percentage points, on average. Treatment effect heterogeneity analyses reveal substantial gender differences in the effect by the gender of both parents and children: Extending children's education decreases the probability of depression by 8.2 percentage points for fathers versus 4.0 percentage points for mothers. Moreover, improvements in parental mental health are driven by more educated sons (8.5 percentage points). In contrast, I cannot reject the null of no effect of daughters' education on parental mental health. The results are strongly robust to numerous sensitivity tests regarding the identification strategy, the estimation approach, and alternative model specifications.

The novel results from this study make important contributions to the literature on effects of education on health and the growing literature on intergenerational returns to education. Concerning the first branch of literature, this study adds to the limited evidence on effects of education on *mental* health. With respect to the second branch of literature, the findings of the study extend the very scarce evidence on *upward* intergenerational returns to education. Another novel aspect of the findings is that no previous study on intergenerational health returns to education analyzes any country included in this paper. My findings suggest that the intergenerational effect of children's education on parental mortality documented in De Neve and Fink (2018) and Lundborg and Majlesi (2018) may partially be explained by causal effects on parental mental health. In line with this, a large body of literature establishes a robust association of impaired mental health, even including mild psychological distress and depression, with substantially higher mortality from a range of different causes (see e.g. Russ et al. 2012; Khan et al. 2013). In addition, my findings add to the understanding of factors which contribute to the health of the elderly. This is becoming increasingly important as populations in most developed countries are aging considerably.

The results yield important implications for designing education policies, on the one hand, and for targeting the supply of elderly health care and social support, on the other. First, the findings emphasize that upward intergenerational spillover effects should not be ignored when attempting to determine the full impact of education policies. Second, the results suggest that elderly individuals whose family members, and particularly their potential informal care givers, have lower levels of education are especially vulnerable to impaired mental health. These findings may be highly relevant for more efficient targeting of health care supply for high-risk elderly individuals with a view to addressing the issue of mental illness being frequently undiagnosed and untreated, particularly among older individuals (Wang et al. 2007).

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Figures

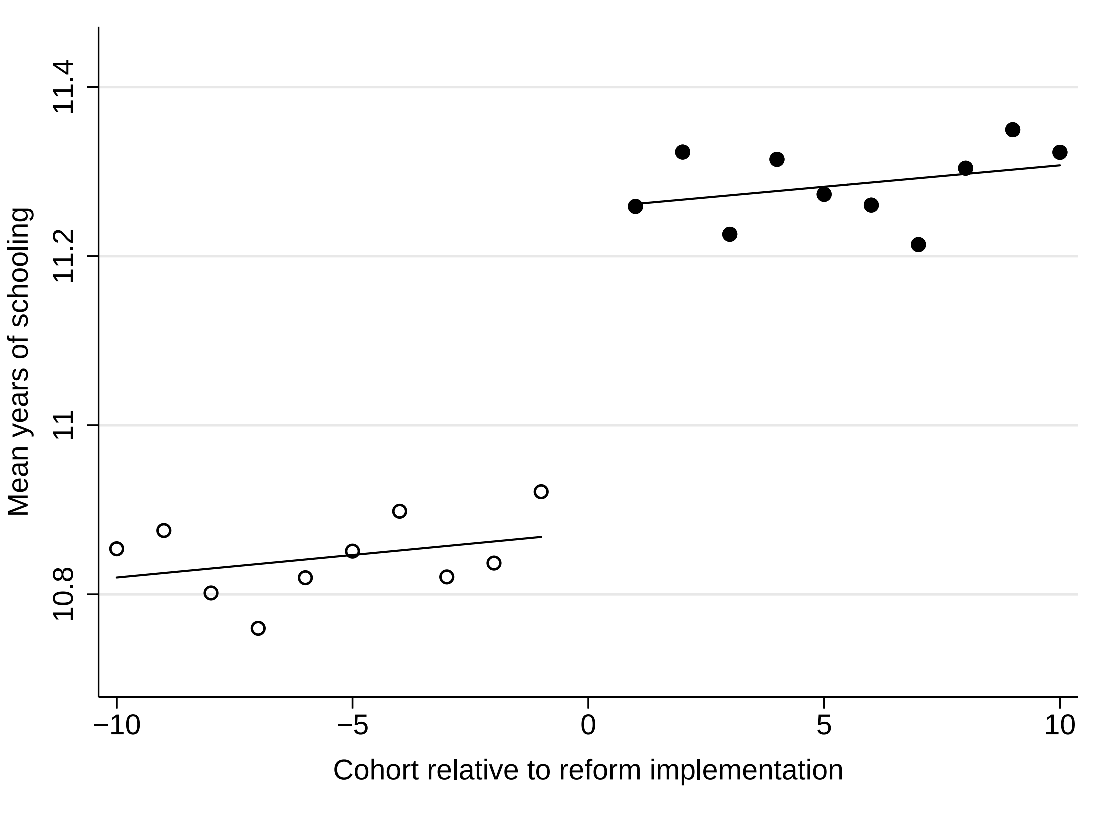

Figure 1: Compulsory schooling reforms and years of schooling.
Note: The figure displays the mean years of schooling of cohorts of children born within ten years before and after the implementation of a compulsory schooling reform.

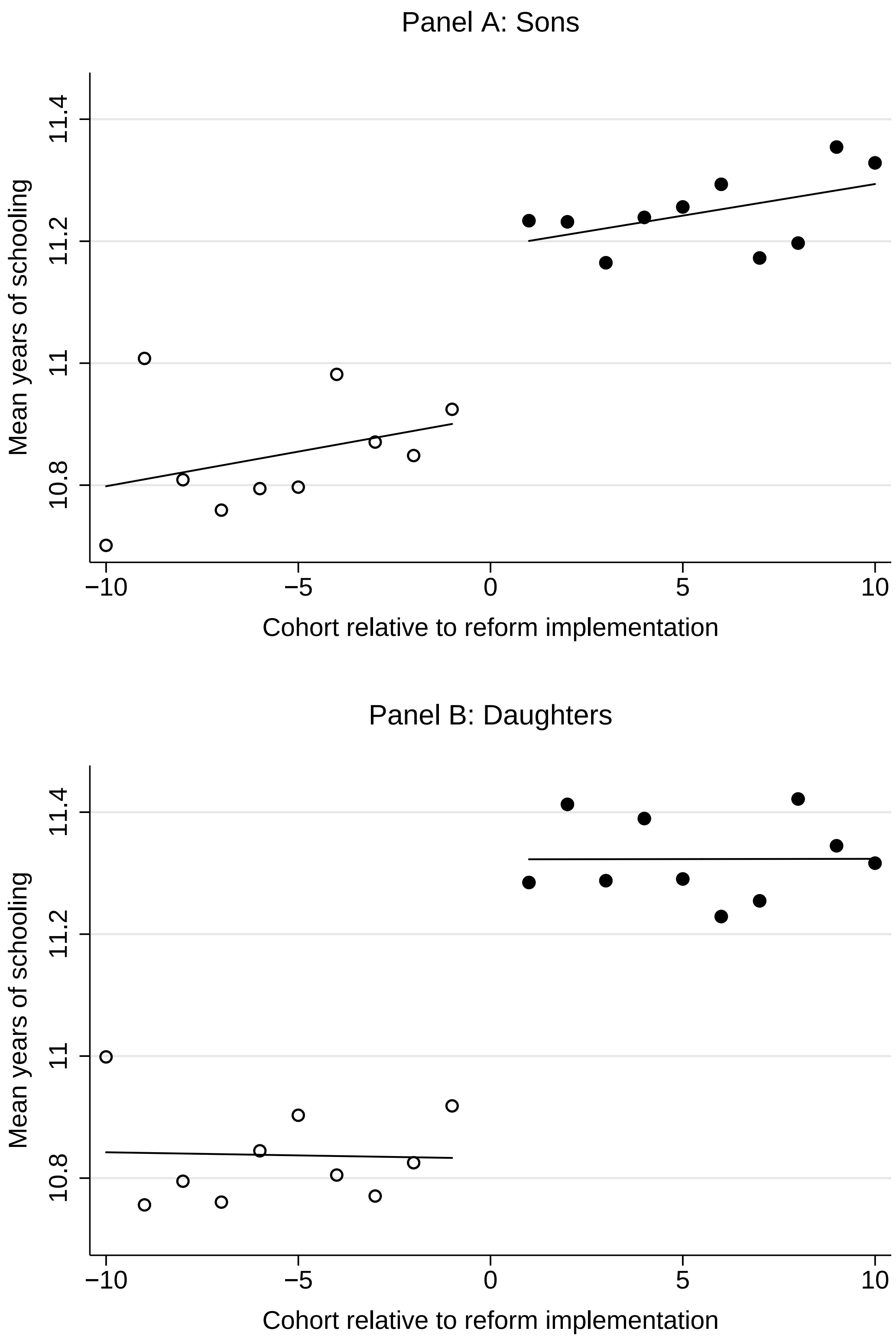

Figure 2: Compulsory schooling reforms and years of schooling by children's gender.
Note: The figure displays the mean years of schooling of cohorts of male children (Panel A) and female children (Panel B) born within ten years before and after the implementation of a compulsory schooling reform.

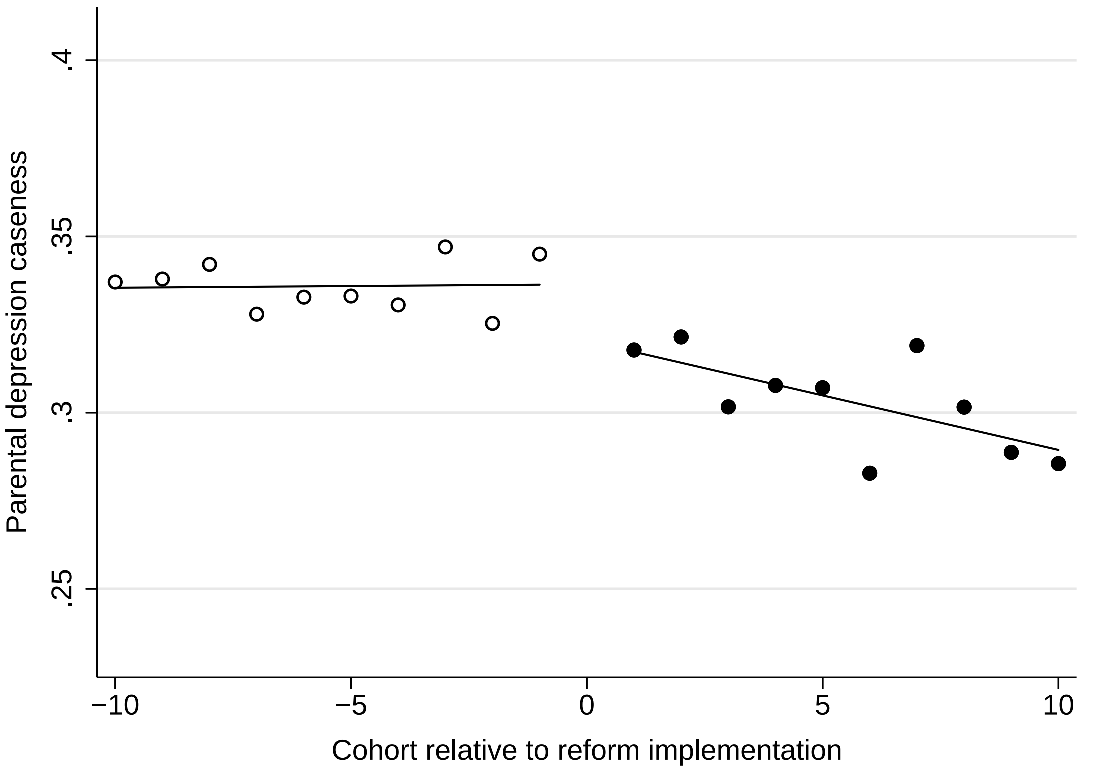

Figure 3: Compulsory schooling reforms and parental depression caseness.
Note: The figure displays the share of depression caseness among parents of children born within ten years before and after the implementation of a compulsory schooling reform.

Tables

Table 1: Schooling reforms by country.

Country	Reform implemented	First affected cohort	Compulsory years of education		
			Before reform	After reform	Difference
Austria	1962	1951	8	9	+1
Belgium	1983	1969	8	12	+4
Czech Republic	1960	1947	8	9	+1
Denmark	1971	1957	7	9	+2
France	1959	1953	8	10	+2
Greece	1976	1963	6	9	+3
Italy	1962	1949	5	8	+3
Netherlands	1973	1959	9	10	+1
Poland	1961	1952	7	8	+1
Portugal	1986	1981	6	9	+3
Spain	1970	1957	6	8	+2

Sources: Brunello et al. (2009); Garrouste (2010); Sousa et al. (2015); d'Hombres and Nunziata (2016); Mocan and Pogorelova (2017).

Table 2: Descriptive statistics.

	All		Pre-reform		Post-reform		Difference
	Mean	SD	Mean	SD	Mean	SD	in means
Child							
Years of schooling	11.15	2.04	10.85	2.41	11.29	1.83	0.44***
Age	46.72	8.60	50.67	8.09	44.92	8.21	−5.75***
Female+	49.66	50.00	50.17	50.00	49.44	50.00	−0.73
Parent							
Years of schooling	9.14	4.40	8.62	4.31	9.37	4.42	
Age	73.81	9.10	77.50	8.44	72.14	8.89	
Female+	61.86	48.57	64.03	47.99	60.87	48.81	
Number of children	3.03	1.52	3.12	1.56	2.99	1.50	
EURO-D depression caseness+	31.25	46.35	33.60	47.23	30.19	45.91	
EURO-D score (0-12)	2.71	2.42	2.89	2.44	2.63	2.41	
Felt depressed in last month+	41.44	49.26	42.82	49.48	40.82	49.15	
Euro-D score ≥ 3+	44.25	49.67	47.49	49.94	42.78	49.48	
Country and interview information							
Austria+	4.91	21.61	1.96	13.88	6.25	24.20	
Belgium+	24.27	42.87	35.39	47.82	19.22	39.41	
Czech Republic+	3.60	18.62	0.81	8.95	4.86	21.51	
Denmark+	11.46	31.86	10.53	30.70	11.88	32.36	
France+	11.26	31.62	9.87	29.83	11.90	32.38	
Greece+	9.31	29.05	11.41	31.79	8.36	27.67	
Italy+	4.52	20.78	2.09	14.29	5.63	23.04	
Netherlands+	12.68	33.27	10.81	31.05	13.52	34.20	
Poland+	3.10	17.32	1.79	13.26	3.69	18.85	
Portugal+	3.02	17.12	6.25	24.21	1.56	12.38	
Spain+	11.87	32.35	9.09	28.75	13.14	33.78	
Year 2004+	16.18	36.83	16.62	37.22	15.99	36.65	
Year 2005+	7.71	26.67	11.25	31.61	6.10	23.93	
Year 2006+	4.95	21.69	5.27	22.34	4.80	21.39	
Year 2007+	20.32	40.23	20.62	40.46	20.18	40.13	
Year 2011+	22.56	41.80	24.25	42.86	21.79	41.28	
Year 2012+	0.83	9.09	0.44	6.63	1.01	10.01	
Year 2013+	14.08	34.79	10.76	30.99	15.59	36.28	
Year 2015+	13.37	34.03	10.79	31.03	14.54	35.25	
Observations	42,082		13,132		28,950		

Note: The first six columns of the table display the mean and standard deviation (SD) for children and parents for the whole sample and by children's schooling reform exposure status, respectively. The difference in means by reform status for children's characteristics is in the last column.

+ Mean represents a percentage share.

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3: OLS results: Relationship between children's education and parental mental health.

	Parental depression caseness				
	(1)	(2)	(3)	(4)	(5)
Panel A: Pooled sample					
Child's years of schooling	-0.021***	-0.013***	-0.013***	-0.013***	-0.009**
	(0.006)	(0.004)	(0.004)	(0.004)	(0.003)
Observations	42,082	42,082	42,082	42,082	42,082
Panel B: Fathers					
Child's years of schooling	-0.012**	-0.009**	-0.009**	-0.009**	-0.006*
	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	16,052	16,052	16,052	16,052	16,052
Panel C: Mothers					
Child's years of schooling	-0.022**	-0.015***	-0.014***	-0.015***	-0.010**
	(0.007)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	26,030	26,030	26,030	26,030	26,030
Panel D: Sons					
Child's years of schooling	-0.020**	-0.013**	-0.013**	-0.012**	-0.008*
	(0.006)	(0.005)	(0.005)	(0.004)	(0.004)
Observations	21,182	21,182	21,182	21,182	21,182
Panel E: Daughters					
Child's years of schooling	-0.023**	-0.014***	-0.014***	-0.014***	-0.010***
	(0.007)	(0.004)	(0.004)	(0.003)	(0.003)
Observations	20,900	20,900	20,900	20,900	20,900
Country fixed effects	no	yes	yes	yes	yes
Cohort fixed effects	no	yes	yes	yes	yes
Country-spec. quadr. trends	no	no	yes	yes	yes
Additional controls	no	no	no	yes	yes
Parental schooling	no	no	no	no	yes

Note: The table shows the results from OLS regressions of parents' probability of developing depression on children's years of schooling. Additional control variables included in column (4) are parental age and gender, child's gender, and interview year. Column (5) displays the results for the preferred specification, which also includes a control variable for parental schooling. Standard errors clustered at the country level are in parentheses.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: First stage results: Compulsory schooling reforms and years of schooling.

	Child's years of schooling				
	(1)	(2)	(3)	(4)	(5)
Panel A: Pooled sample					
Schooling reform	0.161	0.159***	0.164***	0.167***	0.155***
	(0.091)	(0.038)	(0.040)	(0.041)	(0.039)
Observations	42,082	42,082	42,082	42,082	42,082
F-statistic	3.11	17.94	16.58	16.35	16.06
Panel B: Sons					
Schooling reform	0.149	0.143***	0.132***	0.139***	0.121***
	(0.108)	(0.027)	(0.035)	(0.037)	(0.037)
Observations	21,182	21,182	21,182	21,182	21,182
F-statistic	1.91	27.66	14.16	14.01	10.88
Panel C: Daughters					
Schooling reform	0.173*	0.170***	0.193***	0.192***	0.188***
	(0.078)	(0.052)	(0.050)	(0.049)	(0.045)
Observations	20,900	20,900	20,900	20,900	20,900
F-statistic	4.93	10.73	14.96	15.59	17.58
Country fixed effects	no	yes	yes	yes	yes
Cohort fixed effects	no	yes	yes	yes	yes
Country-spec. quadr. trends	no	no	yes	yes	yes
Additional controls	no	no	no	yes	yes
Parental schooling	no	no	no	no	yes

Note: The table shows results from first stage regressions of children's years of schooling on the change in compulsory years of schooling induced by the reforms. Additional control variables included in column (4) are parental age and gender, child's gender, and interview year. Column (5) displays the results for the preferred specification, which also includes a control variable for parental schooling. Standard errors clustered at the country level are in parentheses.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Reduced-form results: The intention-to-treat effect of compulsory schooling reforms on parental mental health.

	Parental depression caseness				
	(1)	(2)	(3)	(4)	(5)
Panel A: Pooled sample					
Compulsory schooling reform	-0.012	-0.004	-0.008**	-0.009**	-0.008**
	(0.009)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	42,082	42,082	42,082	42,082	42,082
Panel B: Fathers					
Compulsory schooling reform	-0.010*	-0.010***	-0.012**	-0.013**	-0.012**
	(0.006)	(0.003)	(0.006)	(0.006)	(0.006)
Observations	16,052	16,052	16,052	16,052	16,052
Panel C: Mothers					
Compulsory schooling reform	-0.006	-0.002	-0.009**	-0.008**	-0.007**
	(0.010)	(0.005)	(0.003)	(0.003)	(0.003)
Observations	26,030	26,030	26,030	26,030	26,030
Panel D: Sons					
Compulsory schooling reform	-0.015*	-0.006**	-0.009***	-0.012***	-0.010***
	(0.009)	(0.003)	(0.002)	(0.002)	(0.003)
Observations	21,182	21,182	21,182	21,182	21,182
Panel E: Daughters					
Compulsory schooling reform	-0.009	-0.002	-0.007	-0.006	-0.006
	(0.009)	(0.008)	(0.007)	(0.007)	(0.007)
Observations	20,900	20,900	20,900	20,900	20,900
Country fixed effects	no	yes	yes	yes	yes
Cohort fixed effects	no	yes	yes	yes	yes
Country-spec. quadr. trends	no	no	yes	yes	yes
Additional controls	no	no	no	yes	yes
Parental schooling	no	no	no	no	yes

Note: The table shows results from reduced-form regressions of parents' probability of developing depression on children's years of schooling. Additional control variables included in column (4) are parental age and gender, child's gender, and interview year. Column (5) displays the results for the preferred specification, which also includes a control variable for parental schooling. Standard errors clustered at the country level are in parentheses.

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6: IV results: The effect of children's education on parental mental health.

	Parental depression caseness				
	(1)	(2)	(3)	(4)	(5)
Panel A: Pooled sample					
Child's years of schooling	-0.075*	-0.025	-0.047**	-0.055**	-0.052**
	(0.041)	(0.020)	(0.024)	(0.024)	(0.026)
Observations	42,082	42,082	42,082	42,082	42,082
Panel B: Fathers					
Child's years of schooling	-0.091	-0.075***	-0.080**	-0.081**	-0.082*
	(0.066)	(0.022)	(0.040)	(0.040)	(0.043)
Observations	16,052	16,052	16,052	16,052	16,052
Panel C: Mothers					
Child's years of schooling	-0.033	-0.011	-0.049**	-0.046**	-0.040*
	(0.043)	(0.024)	(0.023)	(0.021)	(0.021)
Observations	26,030	26,030	26,030	26,030	26,030
Panel D: Sons					
Child's years of schooling	-0.099*	-0.039**	-0.069***	-0.086**	-0.085**
	(0.051)	(0.019)	(0.026)	(0.033)	(0.037)
Observations	21,182	21,182	21,182	21,182	21,182
Panel E: Daughters					
Child's years of schooling	-0.054	-0.013	-0.034	-0.034	-0.032
	(0.042)	(0.044)	(0.034)	(0.035)	(0.035)
Observations	20,900	20,900	20,900	20,900	20,900
Country fixed effects	no	yes	yes	yes	yes
Cohort fixed effects	no	yes	yes	yes	yes
Country-spec. quadr. trends	no	no	yes	yes	yes
Additional controls	no	no	no	yes	yes
Parental schooling	no	no	no	no	yes

Note: The table shows results from IV regressions of parents' probability of developing depression on children's years of schooling. Additional control variables included in column (4) are parental age and gender, child's gender, and interview year. Column (5) displays the results for the preferred specification, which also includes a control variable for parental schooling. Standard errors clustered at the country level are in parentheses.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7: Mechanisms: Child outcomes.

	Full-time employed (1)	Unemployed (2)	Married (3)	Number of children (4)	Lives close to parents (5)	Regular contact with parents (6)
Panel A: Pooled sample						
Years of schooling	0.022	-0.014*	0.100***	0.217***	0.006	0.116***
	(0.054)	(0.007)	(0.035)	(0.074)	(0.039)	(0.036)
Observations	42,080	42,080	42,071	41,994	42,082	42,082
Panel B: Sons						
Years of schooling	0.071	-0.006	0.141*	0.581**	-0.052	0.129*
	(0.056)	(0.017)	(0.083)	(0.229)	(0.067)	(0.077)
Observations	21,181	21,181	21,177	21,130	21,182	21,182
Panel C: Daughters						
Years of schooling	-0.007	-0.020	0.077**	-0.034	0.048**	0.112***
	(0.068)	(0.014)	(0.038)	(0.083)	(0.022)	(0.032)
Observations	20,899	20,899	20,894	20,864	20,900	20,900

Note: The table shows results from IV regressions of different child outcomes on children's years of schooling. All regressions include country fixed effects, child birth cohort fixed effects, and country-specific quadratic trends. Further control variables are parental education, age, and gender, child's gender, and interview year. Standard errors clustered at the country level are in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 8: Mechanisms: Parental outcomes.

	Easily make ends meet (1)	Retired early (2)	Retired due to own ill health (3)	Married (4)	Childcare (5)
Panel A: Pooled sample					
Child's years of schooling	0.009	-0.006	0.014	-0.002	0.030
	(0.019)	(0.011)	(0.017)	(0.047)	(0.039)
Observations	42,082	29,834	29,834	42,082	18,202
Panel B: Fathers					
Child's years of schooling	0.046*	0.035**	0.004	0.066	-0.066
	(0.027)	(0.014)	(0.021)	(0.062)	(0.059)
Observations	16,052	14,021	14,021	16,052	7,203
Panel C: Mothers					
Child's years of schooling	-0.012	-0.031	0.011	-0.019	0.111*
	(0.025)	(0.019)	(0.016)	(0.040)	(0.064)
Observations	26,030	15,813	15,813	26,030	10,999

Note: The table shows results from IV regressions of different parental outcomes on children's years of schooling. All regressions include country fixed effects, child birth cohort fixed effects, and country-specific quadratic trends. Further control variables are parental education, age, and gender, child's gender, and interview year. Standard errors clustered at the country level are in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 9: Instrument exogeneity: Effect of parental education on reform exposure.

	Child exposed to schooling reform			
	(1)	(2)	(3)	(4)
Panel A: Pooled sample				
Parental schooling	0.011*	0.000	0.000	0.000
	(0.005)	(0.000)	(0.000)	(0.000)
Observations	42,082	42,082	42,082	42,082
Adjusted R^2	0.01	0.81	0.83	0.83
Panel B: Fathers				
Paternal schooling	0.005	-0.000	0.000	0.000
	(0.005)	(0.000)	(0.000)	(0.000)
Observations	16,052	16,052	16,052	16,052
Adjusted R^2	0.00	0.82	0.85	0.85
Panel C: Mothers				
Maternal schooling	0.014**	0.001	0.001	0.001
	(0.005)	(0.001)	(0.001)	(0.001)
Observations	26,030	26,030	26,030	26,030
Adjusted R^2	0.02	0.80	0.83	0.83
Country fixed effects	no	yes	yes	yes
Cohort fixed effects	no	yes	yes	yes
Country-spec. quadr. trends	no	no	yes	yes
Additional controls	no	no	no	yes

Note: The table displays the predictive power of parental education for children's exposure to compulsory schooling reforms. Additional control variables included in column (4) are parental age, gender, and interview year. Standard errors clustered at the country level are in parentheses.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 10: Sensitivity analyses: Pooled sample.

	Parental depression caseness				
	(1)	(2)	(3)	(4)	(5)
Panel A: Placebo reforms, 3 years prior to implementation					
Child's years of schooling	-0.067	0.112	-0.039	0.002	0.004
	(0.056)	(0.089)	(0.094)	(0.092)	(0.101)
Observations	42,082	42,082	42,082	42,082	42,082
Panel B: Only first observation used					
Child's years of schooling	-0.092**	-0.034	-0.048	-0.058**	-0.056*
	(0.046)	(0.023)	(0.033)	(0.029)	(0.033)
Observations	22,980	22,980	22,980	22,980	22,980
Panel C: Bandwidth ± 1-7 years					
Child's years of schooling	-0.084**	-0.036**	-0.035	-0.046*	-0.045*
	(0.034)	(0.018)	(0.028)	(0.024)	(0.025)
Observations	28,955	28,955	28,955	28,955	28,955
Panel D: Bandwidth ± 2-10 years					
Child's years of schooling	-0.073*	-0.034	-0.069***	-0.078***	-0.077***
	(0.040)	(0.026)	(0.022)	(0.026)	(0.027)
Observations	38,018	38,018	38,018	38,018	38,018
Panel E: Linear trends					
Child's years of schooling	-0.075*	-0.025	-0.044*	-0.052**	-0.049*
	(0.041)	(0.020)	(0.025)	(0.025)	(0.026)
Observations	42,082	42,082	42,082	42,082	42,082
Panel F: Cubic trends					
Child's years of schooling	-0.075*	-0.025	-0.049**	-0.057**	-0.054**
	(0.041)	(0.020)	(0.023)	(0.023)	(0.025)
Observations	42,082	42,082	42,082	42,082	42,082
Panel G: Outcome: Parent felt depressed					
Child's years of schooling	-0.064	-0.024	-0.058***	-0.068***	-0.071***
	(0.040)	(0.023)	(0.022)	(0.023)	(0.025)
Observations	42,082	42,082	42,082	42,082	42,082
Panel H: Outcome: EURO-D score ≥ 3					
Child's years of schooling	-0.097**	-0.044	-0.077***	-0.085***	-0.085***
	(0.047)	(0.028)	(0.029)	(0.029)	(0.032)
Observations	42,082	42,082	42,082	42,082	42,082
Country fixed effects	no	yes	yes	yes	yes
Cohort fixed effects	no	yes	yes	yes	yes
Country-specific trends	no	no	yes	yes	yes
Additional controls	no	no	no	yes	yes
Parental schooling	no	no	no	no	yes

Note: The table shows sensitivity analyses for the pooled sample and the effect of children's years of schooling on parents' probability of developing depression. Additional control variables included in column (4) are parental age and gender, child's gender, and interview year. Column (5) displays the results for the preferred specification, which also includes a control variable for parental schooling. Regressions in Panels E and F include country-specific linear and cubic trends, respectively, and country-specific quadratic trends in all other Panels. Panels G and H offer alternative measures of parental depression. Standard errors clustered at the country level are in parentheses.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix

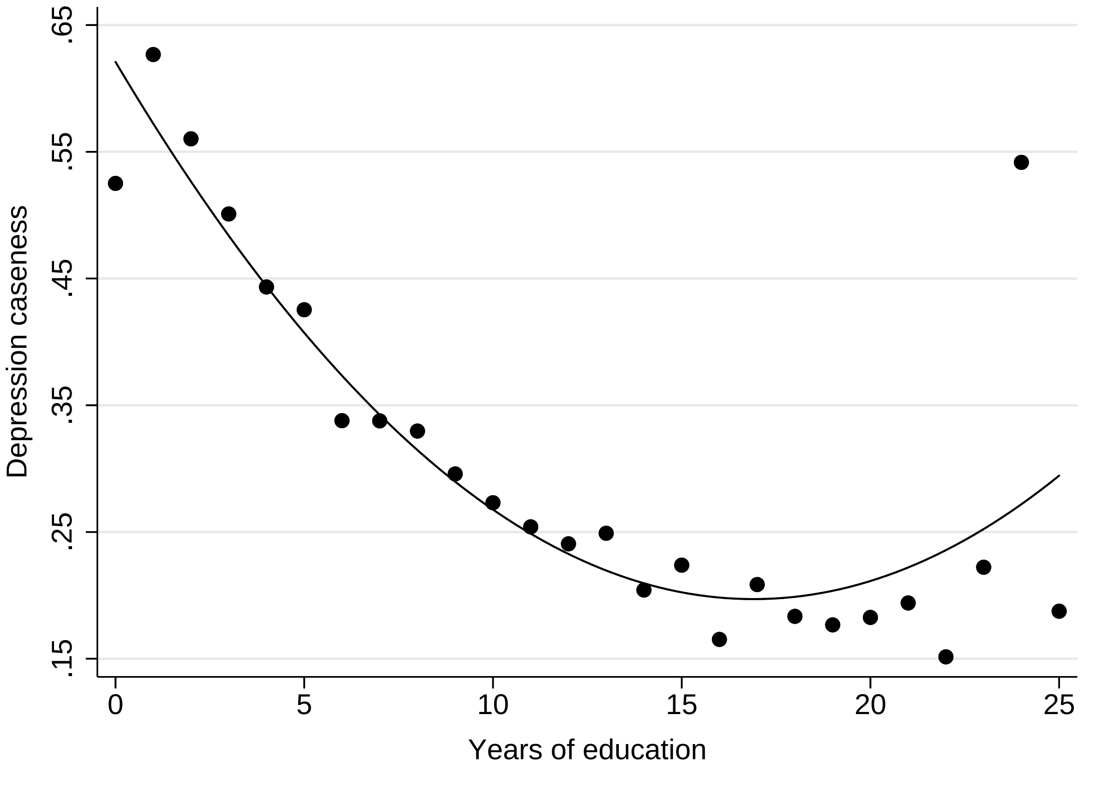

Figure A1: Education and depression caseness.

Note: The figure displays the descriptive relationship between the education of parents and the share of parental depression caseness.

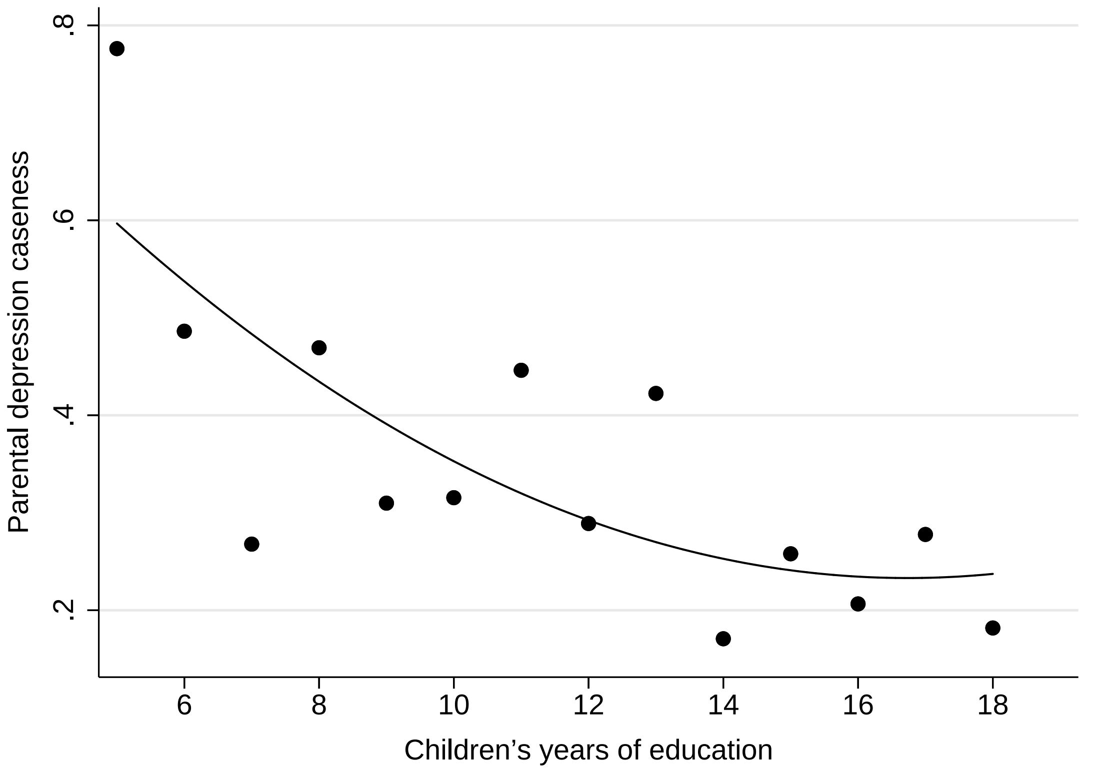

Figure A2: Children's education and parental depression caseness.
Note: The figure displays the descriptive relationship between children's education and the share of parental depression caseness.

Table A1: Treatment effect heterogeneity: By living situation and relative education level.

	Parental depression caseness	
	(1)	(2)
Panel A: Parent not cohabiting		
Child's years of schooling	-0.050*	-0.035
	(0.029)	(0.037)
Observations	19,407	22,675
Panel B: Parent lives alone		
Child's years of schooling	-0.082*	-0.020
	(0.045)	(0.033)
Observations	14,920	27,162
Panel C: Parent more educated than child		
Child's years of schooling	0.079	-0.109**
	(0.086)	(0.053)
Observations	10,754	31,328
Panel D: Child more educated than siblings		
Child's years of schooling	0.131	-0.038*
	(0.093)	(0.020)
Observations	7,021	26,864
Criterion (Panel-specific)	yes	no

Note: The table shows treatment effect heterogeneity analyses by the parent's living situation and the child's education relative to other family members for the pooled sample, and the effect of children's years of schooling on parents' probability of developing depression. Regressions in columns (1) and (2) comprise the subsamples which do and do not fulfill the sample splitting criterion described in the header of each Panel, respectively. Standard errors clustered at the country level are in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A2: Sensitivity analyses: By gender of parents and children.

	Parental depression caseness			
	Fathers (1)	Mothers (2)	Sons (3)	Daughters (4)
Panel A: Placebo reforms, 3 years prior to implementation				
Child's years of schooling	0.157	-0.125	-0.011	0.029
	(0.166)	(0.138)	(0.072)	(0.218)
Observations	16,052	26,030	21,182	20,900
Panel B: Only first observation used				
Child's years of schooling	-0.086*	-0.039	-0.113*	-0.020
	(0.046)	(0.030)	(0.066)	(0.053)
Observations	8,838	14,142	11,540	11,440
Panel C: Bandwidth ± 1-7 years				
Child's years of schooling	-0.062	-0.016	-0.072	-0.024
	(0.044)	(0.028)	(0.075)	(0.043)
Observations	10,855	18,100	14,480	14,475
Panel D: Bandwidth ± 2-10 years				
Child's years of schooling	-0.172**	-0.062***	-0.108	-0.066**
	(0.073)	(0.022)	(0.078)	(0.031)
Observations	14,576	23,442	19,139	18,879
Panel E: Linear trends				
Child's years of schooling	-0.083*	-0.035*	-0.088**	-0.025
	(0.044)	(0.021)	(0.038)	(0.034)
Observations	16,052	26,030	21,182	20,900
Panel F: Cubic trends				
Child's years of schooling	-0.080*	-0.043**	-0.081**	-0.038
	(0.042)	(0.021)	(0.035)	(0.035)
Observations	16,052	26,030	21,182	20,900
Panel G: Outcome: Parent felt depressed				
Child's years of schooling	-0.200***	-0.008	-0.182***	0.001
	(0.047)	(0.027)	(0.042)	(0.026)
Observations	16,052	26,030	21,182	20,900
Panel H: Outcome: EURO-D score $\geq$ 3				
Child's years of schooling	-0.117**	-0.066**	-0.120***	-0.062
	(0.053)	(0.028)	(0.027)	(0.041)
Observations	16,052	26,030	21,182	20,900

Note: The table shows sensitivity analyses for the gender-specific effects of children's years of schooling on parents' probability of developing depression. All regressions include country fixed effects, child birth cohort fixed effects, and country-specific trends. Further control variables are parental education, age, and gender, child's gender, and interview year. Regressions in Panels E and F include country-specific linear and cubic trends, respectively, and country-specific quadratic trends in all other Panels. Panels G and H offer alternative measures of parental depression. Standard errors clustered at the country level are in parentheses.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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